

## **Are Realism and Instrumentalism Methodologically Indifferent?**

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## **Are Realism and Instrumentalism Methodologically Indifferent?**

**Abstract:** Arthur Fine and André Kukla have argued that realism and instrumentalism are indifferent with respect to scientific practice. I argue that this claim is ambiguous. One interpretation is that for any practice, the fact that that practice yields predictively successful theories is evidentially indifferent between scientific realism and instrumentalism. On the second construal, the claim is that for any practice, adoption of that practice by a scientist is indifferent between their being a realist or instrumentalist. I argue that there are no good arguments for the indifference claim under the second interpretation, and good reasons to think that it is false.

**1. Introduction.** Realists argue for scientific realism by pointing to its ability to explain the predictive success of scientific theories: the success would, they claim, be miraculous were the theories not at least approximately true. Instrumentalists typically reject the call for explanation, impugn the realist explanation, and offer alternatives. The trump card is the instrumentalists' scepticism towards inference to the best explanation. Even if realism has all the best explanations, explanatory power is no indicator of truth.

Richard Boyd has presented a more intricate version of the realist explanationist argument (Boyd 1981, 1984). Boyd's strategy is to note that certain scientific practices of theory construction and choice yield theories that are more likely to be predictively successful than mere chance would allow, and that these practices are available only to the realist (see Hendry 1995 for a critical discussion of this methodological argument). The implication is two-fold: firstly that, in so far as the practices are widespread or indispensable, instrumentalism must be correspondingly rare among scientists; secondly that only realism can explain the fact that these practices yield theories that are more likely to be predictively successful than mere chance would allow. Against these implications, Arthur Fine (1986b) presents an algorithm that turns a realist explanation of a scientific practice into an instrumentalist explanation of it. Hence there are no aspects of scientific practice that the realist can explain and the instrumentalist cannot. Although he rejects Fine's general algorithm, André Kukla (1998) argues that no scientific practices have yet been established to be incompatible with either realism *or* instrumentalism. Either way, realism and instrumentalism are indifferent with respect to the practice of science. It is the aim of this paper to explicate and assess this claim of indifference.

**2. Realism and Scientific Practice.** Scientific realism gives an account of the aims, semantics and epistemology of science. Firstly, science aims to provide true explanatory accounts of the phenomena that interest us. Secondly, theoretical discourse is to be construed literally: uses of theoretical terms are putative references to theory-independent entities, be they observable or not. Thus construed, theoretical claims can be true or false independently of our interests and commitments. Thirdly, given a predictively successful, explanatory theory, we have reason to believe that it is (approximately) true and that its central theoretical terms refer. The different components work together: in accepting a theory, we take it that it advances our scientific aims, so acceptance of a theory must involve the (tentative and qualified) belief that it provides true explanatory accounts of the phenomena that interest us. Since realist aims include explanation, the realist's acceptance of a theory results involves (what amounts to) inference to the best explanation. By 'instrumentalism' I mean an account of science which is analogous to the realist's, except that in describing the aim of science, empirical adequacy is substituted for truth, and in describing the proper epistemic stance towards predictively successful theories, belief in their empirical adequacy is substituted for belief in their truth. On this construal there need be no implication of the thesis, about the content of scientific theories, which has historically been associated with instrumentalism.

A standard and important kind of historical project consists of relating the actions of historical scientists to their broader metaphysical and epistemological views, a classic example being Kuhn's account of the mystical, neo-Platonist background to Kepler's astronomical work (Kuhn 1957, 209–19). Now scientific realism, no less than neo-Platonism, is a cluster of metaphysical (and in the case of realism, epistemological) views which, common sense dictates, can affect a scientist's practice. During the nineteenth century, chemists debated the epistemic status and import of chemical formulae (see for instance Nye 1989). The issues are familiar: should molecular models be taken seriously on the basis of their ability to explain? Is chemical affinity a genuine property of atoms, or 'a definition, a convenient figure of speech for talking about chemical change and its properties' (Nye 1989, 176)? Sir Benjamin Brodie was so exercised by hostility to the speculative excesses of the atomists' models as to develop an abstract calculus of chemical combination (for the negative phase see Brodie 1867, 296–7). Jumping forward to the twentieth century, Einstein's rejection of quantum mechanics has often been explained by that theory's failure to display internal theoretical virtues associated with realism (although see Fine 1986a, Chapter 6).

Leaving aside the context and rhetoric that complicates the interpretation of historical scientists' utterances, I would extract the following historiographical intuition. Some scientists have sometimes displayed recognisably realist (and indeed instrumentalist)

commitments, and their realism (instrumentalism) motivated them to engage in certain projects of theory construction, emendation and selection. This need not imply that scientists' beliefs are especially 'transparent' with respect to their practice: scientists—like anyone else—will often fail to reason in accordance with their considered epistemological views. The historiographical intuition implies only that realism (or instrumentalism) was what motivated some scientists to engage in the practices they did engage in, in the sense that they would not have been motivated to engage in those practices had they not been realists (or instrumentalists). Therefore their stated positions (whether realist or instrumentalist) should be an important part of the historical explanation of why they did what they did. But this intuition is, on the face of it, incompatible with Fine's and Kukla's claims that realism and instrumentalism are indifferent with respect to the practice of science.

**3. Fine and Kukla on Realism and Scientific Practice.** Arthur Fine (1986b) has claimed that arguments based on the ability of scientific realism to explain certain aspects of scientific practice can provide no evidential support to realism over instrumentalism. Fine offers two 'metatheorems' in support of the claim. The second of these (Metatheorem 2: see Fine 1986b, 162) notes that instrumentalists refuse to acknowledge the explanatory power of realism as a reason to believe it. Metatheorem 1, meanwhile claims that

If the phenomena to be explained are not realist-laden, then to every good realist explanation there corresponds a better instrumentalist one (1986b, 154).

Metatheorem 1 is an important part of Fine's diagnosis of the dialectical bind in which realists are caught. Either the premises of realist explanationist arguments for realism presuppose realism itself, in which case they will be rejected by the instrumentalist, or the instrumentalist can offer 'better' instrumentalist explanations, in which case inference to the best explanation would favour instrumentalism, not realism. Theorems, even metatheorems, demand proofs, and Fine offers the following to establish Metatheorem 1:

*Proof:* In the proffered realist explanation, replace the realist conception of truth by the pragmatic conception. The result, framed in terms of reliability, will be the better instrumentalist explanation. (NB, the antecedent of this theorem can be discharged, for otherwise the realist begs the question.) (1986b, 154)

Now there are two ways to read Metatheorem 1, which are importantly different, because one is compatible with the intuitions about historical explanation outlined in the last section, while the other is not. The ambiguity turns on three connected questions of interpretation:

what kind of thing is referred to by the word ‘phenomena,’ what it is for the phenomena to be ‘realist–laden’ and what it is to explain them.

*3.1 Explanatory Indifference.* On the first interpretation, the phenomena to be explained involve the *success* of certain scientific theories, or perhaps the success of science itself. On the methodological variant stressed by Boyd, the phenomena to be explained concern the reliability of certain scientific practices, measured in the contribution they systematically make to the likelihood that theories that are constructed or chosen with their help will make successful predictions. Correspondingly, the realist’s explanation will be an account of how that success is achieved, appealing to scientific realism itself (in so far as it is a *realist* explanation). A phenomenon is ‘realist–laden’ when its description presupposes scientific realism itself, or is otherwise such as to be rejected by the instrumentalist, which is why the proviso is relevant to the explanatory indifference of realism and instrumentalism. This first interpretation is, I think, the most natural one, firstly because it fits Fine’s own proof of Metatheorem 1, and secondly because Metatheorem 1 appears as part of Fine’s general discussion of explanationist arguments for realism. To give an example, Aronson, Harré and Way (1995, Chapter 9) cite manipulability as grounds for realist commitment to the existence of theoretical entities. However, instrumentalists will admit only that there is an empirically adequate theory according to which we can manipulate unobservable entities. Before they are forced to go any further, they need to be convinced that the observable events that, according to the theory, are manipulations of unobservable entities *really are* manipulations of unobservable entities.

*3.2 Motivational Indifference.* The second, more problematic construal of Metatheorem 1 is as follows. The phenomena to be explained are scientific practices themselves. Correspondingly, a realist explanation of the phenomena will consist of a rational reconstruction of the relevant practices based on the inferences and cognitive aims that are characteristic of realism, rather than explanations of the fact *that* certain scientific practices yield successful theories, when they do.

This interpretation of Metatheorem 1 is offered by Kukla (1998, Section 3.2). On Kukla’s construal, Metatheorem 1 implies what Kukla calls ‘Metalemma 1,’ according to which

from every realist account of a *scientific practice*, we can construct a corresponding antirealist account of the same practice by substituting “empirical adequacy” for “truth”

(Kukla 1998, 32, italics in original).

Kukla goes on to argue, quite rightly, that this algorithm does not always work, because some scientific practices involve theoretical derivations—or choices among theories—which have no bearing on the empirical adequacy of those theories. Hence the instrumentalist reconstruction of these practices cannot proceed via the algorithm encapsulated in Metalemma 1. However, the antirealist may always appeal to *pragmatic* elements in scientific practice. This, for instance, is how van Fraassen (1980, 87–9) has interpreted the practice of choosing theories that offer certain kinds of explanation for relevant phenomena. Kukla notes that the appeal to pragmatic elements in science seems to be open to the instrumentalist regardless of the nature of the scientific practice, since the instrumentalist may call any theoretical virtue that transcends empirical adequacy ‘pragmatic.’ There is in principle ‘no end to the pragmatic virtues that may motivate a piece of scientific work’ (Kukla 1998, 33). However, even this strategy is not always applicable, because some scientific practices (Kukla calls them ‘epistemic practices’) may involve the formation of *beliefs*, and it is highly controversial whether the formation of beliefs for non-evidential reasons is even a coherent possibility. Unless they are allowed to presume an extreme position on the vexed question of doxastic voluntarism, it has not yet been shown that instrumentalists can offer rational reconstructions of every scientific practice. Kukla takes the ‘realist-laden’ phenomena of Fine’s Metatheorem 1 to be just such epistemic practices: scientific practices involving the formation of beliefs which only scientific realists can coherently form, beliefs, for instance, in the existence of theoretical entities (Kukla 1998, 34). Of course, there might also be ‘instrumentalist-laden’ epistemic practices, in which only the instrumentalist can consistently engage, such as refusing to believe in the existence of the central theoretical entities of well-confirmed theories. Criticising Fine’s argument for Metatheorem 1, Kukla notes that it does not apply to ‘realist-laden’ practices. If there are realist-laden practices in science, Fine has not yet established that scientific practice fails to ground a good argument for realism. Kukla concludes, however, that the intuitions behind Fine’s use of Metatheorem 1 are right, since ‘realists have searched far and wide’ for realist-laden practices and ‘come up empty-handed’ (1998, 34). I will return to this claim in the next section.

Comparing Kukla’s Metalemma 1 to Fine’s proof of Metatheorem 1, it is clear that Kukla takes the word ‘phenomena’ in Metatheorem 1 to refer to scientific practices, and ‘explanations’ to be rational reconstructions of those practices. But this yields a metatheorem concerning the motivational indifference of realism and instrumentalism, which is importantly different from the explanatory indifference yielded under the first interpretation. Metalemma 1 (at least as Kukla goes on to use it) is not a straightforward application of

Fine's proof to the special case of scientific practices, for the relevant sense of 'explanation' is different, as I will now argue.

Metatheorem 1 arises as part of Fine's attempt to show that the debate between realists and instrumentalists is irreconcilable (as Kukla himself (1998, Chapter 12) elegantly puts it), but that the situation is nevertheless asymmetrical. Metatheorem 1 expresses Fine's claim that even if instrumentalists can be persuaded to set aside their scepticism regarding inference to the best explanation, reason still need not force them to be realists. For according to Fine, the instrumentalist account of science provides explanations of whatever phenomena both realists and instrumentalists agree on—for instance, the existence of predictively successful theories—explanations that are at least as good as those provided by the realist. But realist and instrumentalist explanations of the kind that are central to Kukla's interpretation of Metatheorem 1 are not obviously relevant to the relative epistemic standing of realism and instrumentalism as explanatory accounts of science. To know that realists (or instrumentalists) can rationally reconstruct certain scientific practices does not itself indicate whether *I* should accept realism (or instrumentalism): at best it implies that a scientist who is a realist (or an instrumentalist) would be motivated to engage in those practices.

I am not saying that there is no connection between explanations of the success of particular scientific practices, and rational reconstructions of them. A case of the former will often yield a case of the latter. Suppose we have a partial probabilistic explanation of the predictive success of theories that are constructed or selected according to some scientific practice, *modulo* the assumption that its conditions of application were met. Conditioning the explanation on this kind of assumption is one way in which phenomena can be 'realist-laden' in the sense of the first construal of Metatheorem 1. For instance, the practice may involve taking an approximately true theory and minimally amending it, yielding a successor theory that preserves its core of approximate truth. This explanation of the predictive success of the successor theory clearly would be 'realist-laden', and would beg the question in favour of realism (see for instance Laudan 1984, Chapter 5; Fine 1984). That explanation may entail that correctly following the practice (i.e. within its conditions of application) is more likely than not (or more likely than alternative strategies) to yield predictively successful theories. For those who seek predictively successful theories, this would provide a motivation for adopting that practice. Hence it also explains why someone who seeks predictively successful theories engages in the practice, *modulo* the attribution of relevant beliefs, for instance that the conditions on the application of the method were met. Note that this realist motivation for the practice begs no question against instrumentalism, for it appeals only to the scientist's *belief* that the original theory is approximately true, a point that has been made by Leplin (1986, 40). But there are two different explanations at work here, because their explananda

are different: The first kind of explanation is relevant to the classic explanationist argument for realism, the second is not (not directly, at any rate). It may well be that scientific practice is such as to be 'realist-laden' to the core, in that science is suffused with practices which only realist inferences can motivate, while instrumentalists have the best explanation of the success of those 'realist-laden' practices.

Even if it were true that every explanation of the success of a scientific practice may also provide a rationale for engaging in that practice, the converse surely does not hold. The reason for this is simple: the motivation for a practice need not imply that it is likely to realise its aim. To give an example, a realist is motivated to *refrain* from accepting extant empirically adequate theories that fail to display what the realist regards as crucial internal virtues (explanatory power, or unity, for instance), preferring to withhold commitment until a more internally plausible alternative comes along (think of Einstein on quantum mechanics). Like David Bohm, the realist scientist may even be motivated to embark on a programme of theory construction. But the motivation for this theoretical work—dissatisfaction with extant theories—is compatible with pessimism about its prospects for issuing in a new empirically adequate theory that does possess the required internal virtues. If the required theory does emerge, realist and instrumentalist explanations of its predictive success will be different, and may be entirely independent of the motivation for the practice that produced the theory.

What is the relationship between the two claims? The motivational indifference claim implies that for every realist reconstruction of a scientific practice there corresponds an instrumentalist reconstruction. The explanatory indifference claim implies only that for every realist explanation of the success of a scientific practice there is an instrumentalist explanation of its success. In the preceding two paragraphs, I have discussed two claims, that (i) to every explanation of the success of a scientific practice there corresponds a possible rational reconstruction for that practice, and (ii) not every rational reconstruction of a scientific practice yields an explanation of its success (even if it is successful). If (i) and (ii) are accepted, it follows that explanatory indifference implies motivational indifference, but not vice versa. If so, then to argue against motivational indifference, as I will do so in the next section, is also to argue against explanatory indifference. However, if (i) is false, then explanatory and motivational indifference turn out to be independent. Given that only the first interpretation of Metatheorem 1 is relevant to Fine's stated aim of showing that no argument from scientific practice can favour realism over instrumentalism, motivational indifference could play no direct role in Fine's argument. Independence is not crucial to my central argument however, which is that there are no good arguments for the motivational indifference claim, but it does help to bracket the consideration of explanatory indifference, and the related question of the irreconcilability of realism and instrumentalism.



**4. Reconstructing Scientific Practices.** In this section, I will argue that no convincing argument has been offered for the motivational indifference claim. The key to this part of the argument is that there is a difference between a practice being coherent for a scientist to engage in, given some specified background belief, and their being motivated to engage in that practice, given that belief. Scientific practices, after all, can be arduous and time-consuming things to engage in, and can direct one's efforts away from other important activities like childcare, getting rich and engaging in other scientific practices. Hence rational reconstruction properly so-called requires more than mere consistency: positive motivation is required. I conclude the argument by contrasting the motivational indifference claim with the historiographical intuitions of Section 2.

First recall that Kukla has shown that Fine's proof of Metatheorem 1 fails to establish Kukla's Metalemma 1, which Kukla takes to be both a consequence of Metatheorem 1 and a statement of the motivational indifference of realism and instrumentalism with respect to scientific practice. However, Kukla (1998, 34) goes on to argue that no one has yet identified practices which are either realist-laden (i.e. motivationally compatible only with realism) or instrumentalist-laden (i.e. motivationally compatible only with instrumentalism). Although Kukla does not offer a survey, he does consider two candidate practices, grounding realist and instrumentalist arguments respectively: conjoining theories, and the use of multiple incompatible models. He concludes that neither practice is incompatible with either realism or instrumentalism. I will now consider Kukla's claims with respect to these two practices.

*4.1 Conjoining Theories.* Long ago, Putnam argued that it is common in science to conjoin accepted theories and look to their joint consequences for novel predictions (1973, 210–11). The realist—for whom acceptance means *acceptance-as-true*—finds this easy to rationalise: if  $T_1$  and  $T_2$  are true, then so must be  $T_1 \& T_2$ . For the instrumentalist, however, acceptance means only *acceptance-as-empirically-adequate*, and the empirical adequacy of  $T_1$  and  $T_2$  does not imply the empirical adequacy of  $T_1 \& T_2$ . So the pooling of explanatory and predictive power—central to the very cumulativeness of science—seems inexplicable on any instrumentalist account of acceptance. Unlike van Fraassen (1980, 83–7), Kukla (1998, 29) accepts that simple conjunction is a genuine phenomenon, citing the 'mercenary' use of auxiliary hypotheses. However, Kukla offers a broadly instrumentalist motivation for conjoining theories, based on what he calls 'conjunctive empiricism'. Unlike constructive empiricists, who limit their beliefs to the truth of the empirical consequences of theories, conjunctive empiricists are willing to believe the empirical consequences of theories in

conjunction with any other theories they accept (as empirically adequate). Because the conjunctive empiricist is recognisable as a species of instrumentalist, Kukla considers himself to have provided an instrumentalist motivation for conjoining theories.

But that is too quick. Kukla considers the objection that the truth of the empirical consequences of a theory *T* in conjunction with any other empirically adequate theories is ‘utterly fantastic’ (1998, 30), unless *T* is true. The truth of the empirical consequences of a theory *T* in conjunction with other empirically adequate theories demands further explanation, by the truth of *T* (for an argument that conjunctive empiricism is parasitic on realism, see also Psillos 1999, 208–210). However, Kukla retorts that this objection is a question–begging example of the ‘no miracles’ argument, since the instrumentalist will refuse to believe the realist’s proffered explanation on the basis of its explanatory power, or indeed reject the demand for explanation. In the context of a discussion of the explanatory indifference of realism and instrumentalism, Kukla’s retort is to the point. However, in the context of the motivational indifference claim, we can ask whether conjunctive empiricism is a well–motivated epistemological stance. Why should anyone be a conjunctive empiricist? It would be circular to say that the motivation is to help ourselves to the practice of conjoining theories, for that is the practice for which we sought motivation in the first place. Contrast conjunctive empiricism with constructive empiricism: one of the major tasks van Fraassen has faced in defending his position is in showing that the distinctions on which it depends (for instance, that between the observable and the unobservable), are epistemologically well–motivated. This involves showing that a reasonable person could have genuine reasons to limit their beliefs to the empirical adequacy of predictively successful theories. It does not seem out of place to ask for something similar on behalf of conjunctive empiricism. If there is no rationale for being a conjunctive empiricist, then conjunctive empiricism can offer no rationale for the practice of conjoining theories.

Van Fraassen’s original response to the conjunction argument is that simple conjunction doesn’t occur. Rather, the conjuncts typically undergo a process of correction. Margaret Morrison develops this line of defence, arguing that instrumentalism ‘explains the practice of conjunction as one that is crucial in the search for theories that are equipped to explain and predict a variety of theories’ (1990, 312). In conjoining theories we prompt ourselves to notice and remove the empirical inadequacies in the conjunctions, thus yielding empirically adequate theories of broader scope. Presumably what is invoked here is the theoretical virtue of scope, which on the instrumentalist account could be a pragmatic criterion of theory–choice. But why should an instrumentalist value scope? If the answer is simply that the instrumentalist may prefer theories of wider scope, then we have no more motivated the practice of conjunction–correction than if we appeal to some particular instrumentalist’s

desire for theories that employ the word ‘purple’. On the other hand, it is not clear that the instrumentalist motivation can appeal to some deeper instrumentalist value, such as utility, for the simple fact is that theories of broader scope tend to be *more* difficult, not *less* difficult to apply in the service of making predictions. In any case, there would remain the problem of the cases in which no correction is likely, such as the mercenary use of auxiliaries from different domains.\*<sup>1</sup>

*4.2 Multiple Models.* In something close to the converse of the conjunction argument, Morrison (1990, Section 4.2) argues that the use of multiple incompatible molecular models to generate predictions of the behaviour of gases is incompatible with realism. However, Kukla responds that ‘realists can also make empirical predictions from multiple incompatible models without being irrational’ (1998, 31). The realist supposes that there is a (so far undiscovered) true theory of the relevant phenomena, but uses the various incompatible theories on the basis of their empirical adequacy within specific domains. However, a realist who adopts Kukla’s strategy is not acting *qua* realist, for realism posits a general explanatory connection between predictive success and truth: that, remember, is how the argument for realism begins. To apply that general connection in this particular case will, of course, yield a contradiction, so given the existence of multiple incompatible models, realists must locally suspend it. Now Kukla is right that there is nothing logically incoherent in this local suspension, but that is insufficient to make the acceptance of multiple incompatible models motivationally indifferent between realism and instrumentalism. A realist who is a scientist has a *prima facie* motivation to generate alternative theories. The instrumentalist, in contrast, has no such motivation, and may acquiesce in the existence of multiple models *qua* instrumentalist, although as we saw in the last section, other ‘pragmatic’ interests may intervene. I conclude that no realist motivation for accepting multiple models has been offered, only local recourse to instrumentalism.

**5. Conclusion.** In reviewing the conjunction and multiple–model arguments, it should be remembered that these two practices are not universal or indispensable elements of science. Kukla (1998, 28) worries that if realist and instrumentalist–laden practices coexist in science,

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<sup>1</sup>\* It is worth noting here that the conjunction objection as formulated applies specifically to instrumentalist positions, like van Fraassen’s, in which scientific aims and inferences are characterised in terms of empirical adequacy. Instrumentalism characterised in terms of reliability, where reliability is preserved under conjunction, would avoid this kind of objection. I’d like to thank Arthur Fine for pointing that out.

science must be irrational. But scientific practice is not monolithic (witness the existential quantification in the historiographical intuition of Section 2). Different interests dominate in different phases of theory–construction. To borrow an example from Nancy Cartwright (1999, 192–7), when applying quantum mechanics to superconductors, numerical accuracy and detailed causal knowledge are important: by the nature of the activity, model building will be a messy and pragmatic business. However, contrast that with the initial introduction of the *same* theory in 1925. For a brief period in the twilight of the old quantum theory, Wolfgang Pauli suspended his work on atomic spectroscopy, dissatisfied with the incompatible assumptions required to make quantum–theoretic atomic models fit different spectroscopic situations (see Hendry 1984, 43). Pauli’s worries were part of a wider recognition that something fundamental would have to change in quantum theory. Would we really have counseled Pauli to be content with the old quantum theory? There is nothing incoherent in the simultaneous truth of the opposing counterfactuals: without interests in unification, we might not have had matrix mechanics, or heliocentrism; but if we insisted on it, we might not have understood lasers, or superconductivity.

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