Instrumentalism

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1. Introduction

'Instrumentalism' – a term that was probably coined by Dewey[1] – has meant several different things to different philosophers of science, and there is no standard definition thereof. It is best to think of instrumentalism as a philosophical movement, with a historical basis.[2] Scientific realism is no different in this respect. As Hacking (1983: 26) puts it:

> Definitions of 'scientific realism' merely point the way. It is more an attitude than a clearly stated doctrine… Scientific realism and anti-realism are … movements. We can enter their discussions armed with a pair of one-paragraph definitions, but once inside we shall encounter any number of competing and divergent positions …

And as Chakravartty (2011) adds:

[1] I will not be able to explore how Dewey's notion relates to the history of philosophy of science. But there are some interesting parallels, and being a pragmatist aligns well with being an instrumentalist about science, as will emerge in what follows. Indeed, *some* forms of instrumentalism about science may be construed as local forms of pragmatism.

[2] It might be best construed as a kind of stance. Stances are discussed in §III.7.

> It is perhaps only a slight exaggeration to say that scientific realism is characterized differently by every author who discusses it, and this presents a challenge to anyone hoping to learn what it is.

So what *can* we say about the instrumentalist movement?[3] It has two key components. First, as one might expect, it involves a cluster of views – both normative and descriptive – on which science, or a significant part thereof, is construed as an instrument. Second, it involves characterizing the positive role of said instrument solely, or centrally, in terms of observable things (or phenomena). Thus instrumentalism is closely aligned with, and might even be understood to be a subspecies of, empiricism about science.

Here are some specific examples of theses that are instrumentalist in character, according to the characterization above: science is valuable primarily in so far as it is an instrument for making predictions about the observable; science *is* merely an instrument for making predictions about the observable; and scientific discourse about the unobservable is merely an instrument for making predictions concerning the observable. One might think of such theses as falling into categories such as 'axiological', 'epistemic', and 'semantic', if this helps to compare them with realist alternatives. It's also worth bearing in mind that each thesis might be weakened somewhat and still retain an instrumentalist character. For example, the final thesis

[3] The following is a *characterization* in so far as it presents individually necessary, but not jointly sufficient, conditions for a position to count as instrumentalist. One advantage in this characterization, as we will see, is that it avoids the common error of understanding instrumentalism too narrowly.

might be weakened to 'scientific discourse about the unobservable is *typically* no more than an instrument for making predictions concerning the observable'. Appeals to typicality feature in some characterizations of realism, such as Boyd's (1980), too.

For illustrative purposes, let's consider constructive empiricism. Van Fraassen (1980: 12) defines this position as follows: 'Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate.' Empirical adequacy is in turn defined in terms of observability (ibid.): 'a theory is empirically adequate exactly if what it says about the observable things and events in this world is true'.[4]

So constructive empiricism satisfies one of the two criteria to be instrumentalist, in so far as it characterizes the positive role of science in terms of observable things. But does it fulfill the other? This depends on how one construes the talk of 'the aim of science'.[5] But suffice it to say two things about this. First, rephrasing the definition of constructive empiricism such that it begins 'Science is an instrument for giving us theories…' doesn't, *prima facie*, do any violence to it. Second, as we'll see below, Mach wrote of 'the task of science', which is plausibly interchangeable with 'the aim

[4] In fact, observability is more central to constructive empiricism than this rough definition of empirical adequacy makes apparent. To see this, consider the statement 'The fire emits phlogiston'. This is about an observable thing, namely a fire, but is (presumably) false. Therefore, on van Fraassen's definition, it couldn't be a part of (or a consequence of) an empirically adequate theory. But such a claim should be able to be. Thus, van Fraassen intended to convey something like 'a theory is empirically adequate exactly if what it says about observable aspects of observable things and observable events is true'.

[5] See Rosen (1994), van Fraassen (1994) and Rowbottom (2010; 2014) on this matter.

of science', in a highly similar way. And Mach is widely acknowledged to have been an instrumentalist.

2. How Do Instrumentalist Theses Interrelate? Nineteenth-Century Lessons

A good way to penetrate to the core of instrumentalism, and to get an understanding of how the theses associated with it are connected, is to look at the work of the key historical figures associated with the movement. Let's begin by considering Mach's views on science, and distilling the core instrumentalist component of these. We can then consider how Mach's views relate to those of several other philosophers and scientists of the time who shared his anti-realist inclinations.

Wherein did Mach take science's primary value to lie, if not in finding the truth about the unobservable world? The answer is apparent from the following passage:

> The biological task of science is to provide the fully developed human with as perfect a means of orientating himself as possible. (Mach 1984: 37)

But why did Mach think this? In part, the answer is that he took physical objects to be bundles of sensations. In his own words: 'Properly speaking the world is not composed of "things" as its elements, but of colors, tones, pressures, spaces, times, in short what we ordinarily call individual sensations.' (Mach 1960: 579). So Mach held that there are no unobservable physical things for us to describe. It is a short step to thinking that '[i]t is the object of science to replace, or *save*, experiences, by the reproduction and anticipation of facts in thought' (Mach 1960: 577).

Mach also strongly emphasized the importance of economy, and declared, as a result, that much talk about unobservable entities and processes should be eliminated. (He did allow that *some* talk of unobservable things is useful.[6]) In his words: 'all metaphysical elements are to be eliminated as superfluous and as destructive of the economy of science' (Mach 1984: xxxviii). As Pojman (2009) explains:

> Mach's reason for insisting that economy must be a guiding principle in accepting or rejecting a theory is that uneconomical theories cannot fulfill their biological function, which … he insists is (in a descriptive sense) the

[6] For example, concerning striking an elastic rod held in a vice, he wrote:

> Even when the sound has reached so high a pitch and the vibrations have become so small that the previous means of observation are not of avail, we still *advantageously* imagine the sounding rod to perform vibrations... [T]his is exactly what we do when we imagine a moving body which has just disappeared behind a pillar, or a comet at the moment invisible, as continuing its motion and retaining its previously observed properties... We fill out the gaps in experience by the ideas that experience suggests … (Mach 1960: 588)

He thought differently about atoms, partly because of the properties ascribed to them:

> But the mental artifice atom was not formed by the principle of continuity … Atoms cannot be perceived by the senses; like all substances, they are things of thought. Furthermore, the atoms are invested with properties that absolutely contradict the attributes hitherto observed in bodies. However well fitted atomic theories may be to reproduce certain groups of facts, the physical inquirer who has laid to heart Newton's rules will only admit those theories as provisional helps, and will strive to attain, in some more natural way, a satisfactory substitute. (Mach 1960: 589)

> function of science. The biological purpose of science is the improvement or the better adaptation of memory in service of the organism's development.

However, Mach's view was more extreme than it needed to be, given his view on the biological purpose, or task, of science. That's because 'orienting' oneself may involve more than acquiring predictive (and retrodictive) power (in, if desired, an economical way). Gaining an understanding of the phenomena and how they interrelate may also be necessary for a genuine orientation, or at least for achieving 'as perfect a means of orienting [oneself] as possible'. (For instance, having an understanding of why something occurs may improve one's memory that it occurs.) And a tale concerning unobservable things – especially one involving analogies with observable things – might furnish one with such an understanding.

Now if understanding is factive – that is to say, if any proposition expressing an understanding is true – then this is no real alternative for Mach. But philosophers such as Elgin (2007) and Rancourt (2015) argue that understanding is not factive.[7] What's more, a non-factive (and not even quasi-factive) view of understanding was popular among many of Mach's contemporaries who took a more positive view towards discourse about unobservables, while nonetheless taking said discourse to be largely,

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[7] This is an ongoing matter of dispute. Another recent paper that takes a more realist-friendly view is Rice (2016). This is not the place to develop a full anti-realist account of understanding. But suffice it to say this. An instrumentalist might grant that 'understanding *that*' is factive (or involves, at least, approximately true propositions). But she might deny that 'understanding *why*' and 'understanding *how*' are factive (or need to involve even approximately true propositions). See Rowbottom (Manuscript) for more on this.

or completely, non-literal in character.[8] For example, here are two key passages in which Poincaré approvingly discusses non-literal scientific discourse:

> [Some hypotheses have] only a metaphorical sense. The scientist should no more banish them than a poet banishes metaphor; but he ought to know what they are worth. They may be useful to give satisfaction to the mind, and they will do no harm as long as they are only indifferent hypotheses. (Poincaré 1905: 182)

> [I]ndifferent hypotheses are never dangerous provided their characters are not misunderstood. They may be useful, either as artifices for calculation, or to assist our understanding by concrete images, to fix the ideas, as we say. They need not therefore be rejected. (Poincaré 1905: 170–171)

Poincaré's mention of 'satisfaction to the mind' stands in contrast to Mach's (1911: 49) suggestion that '[w]hat we represent to ourselves behind the appearances … has for us only the value of a *memoria technica* or formula'. It also gels with the stance that many (Cambridge educated) British physicists in the Victorian era took on mechanical models, as explained by Heilbron (1977: 41–43):

[8] Another way of thinking about 'understanding' in this context is as non-factive *explanation*. As Niiniluoto (2002: 167) puts it: 'for the realist, the truth of a theory is a precondition for the adequacy of scientific explanations'. Instrumentalists may disagree, as, for instance, van Fraassen (1980) does.

> [T]he representations were not meant or taken literally … The same physicist might on different occasions use different and even conflicting pictures of the same phenomena … piecemeal analogies or provisional illustrative models.

Heilbron (1977: 42) adds: 'Such pictures, they believed, fixed ideas, trained the imagination, and suggested further applications of the theory.' As Lodge (1892: 13) put it:

> [I]f we resist the help of an analogy … there are only two courses open to us: either we must become first-rate mathematicians, able to live wholly among symbols, dispensing with pictorial images and such adventitious aid; or we must remain in hazy ignorance of the stages which have been reached, and of the present knowledge ...

Indeed, Kelvin (1883: 270) went so far as to declare that having a model is necessary for having understanding:

> If I can make a mechanical model, I understand it. As long as I cannot make a model all the way through I cannot understand … I want to understand light as well as I can without introducing things that we understand even less of.[9]

[9] The use of 'mechanical' is noteworthy; at the time of writing, it was typically considered important that: 'only those classes of forces with which physicist had become familiar since the time of Newton should be admitted … the resultant description had to be continuous in space and time.' (Heilbron 1977: 41). However, this demand may be relaxed, and was profitably relaxed, for example, by Bohr. For more on this, see Bailer-Jones (2009: 41–42) and Rowbottom (Manuscript: §IV.2 & §IV.4).

Mach disapproved of piecemeal modelling in the name of economy.[10] In short, having to remember lots of different models (or theories) for different occasions seemed to him to be bad, reasonably enough, in so far as memory space is concerned. However, Mach was considering economy at a global, rather than a local, level. And a theory (or theoretical framework or model) that lacks economy at the global level may have economy in considerable measure – even when it comes merely to considering ease of calculation – at the local level. (It may also, as intimated above, be easier to remember than any more economical competitor.) Think of classical mechanics. We could dispense with it entirely, in principle. But we don't because it's such a useful tool in some circumstances. It's simpler than the alternatives in the straightforward sense that it involves fewer variables and constants; it doesn't require reference to the speed of light, or a wavefunction, or a quantum potential. Of course, Mach could reply to this example that classical mechanics is simply a 'special case' approximation to the other forms of mechanics we have. But this route isn't always open; in the case of models, as distinct from theories, it typically isn't. To this we may add that if we're interested in understanding, and not just easy prediction, then simple models for local contexts can be worthwhile parts of our intellectual arsenal for independent reasons. For navigation at sea, for example, it's convenient to think in terms of a central Earth and a sphere of stars revolving around it. This doesn't mean we should disregard global economy. It's important. But it should not be maximized irrespective of local considerations.

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[10] It was similarly condemned by Duhem (1954: §IV.5), who remarked:

> In the treatises on physics published in England, there is always one element which greatly astonishes the French student; that element, which nearly invariably accompanies the exposition of a theory, is the model …

Let's take stock. We've seen that Mach's instrumentalism involved evaluative and descriptive components. Descriptively, for example, Mach thought that scientists (and hence science) could not find the truth about the unobservable world. He thought this on ontological grounds, although modern instrumentalists typically argue for it on epistemological grounds. Evaluatively, he thought that science progresses when its ability to orient us increases. We have also seen that Mach *took* these views to have methodological significance. But we have also seen that their genuine methodological significance is open to dispute. So Mach might have been persuaded by the considerations concerning understanding above, and would have been no less an instrumentalist as a result.

For more on the period I've briefly discussed here and its relevance for instrumentalism, with special reference to understanding (which is a key component of my own instrumentalism), see Rowbottom (Manuscript: Ch. IV).

3. What Is Instrumentalism Not?

We're now in a position to dispel some of the common confusions concerning instrumentalism. One kind of error involves conflating, or inappropriately connecting, descriptive and evaluative instrumentalist theses. Consider, for example, the following *Encyclopaedia Britannica* entry (which *is* by an eminent academic):

> Instrumentalism, in the philosophy of science, [sic] the view that the value of scientific concepts and theories is determined not by whether they are literally

> true or correspond to reality in some sense but by the extent to which they help to make accurate empirical predictions or to resolve conceptual problems. Instrumentalism is thus the view that scientific theories should be thought of primarily as tools for solving practical problems rather than as meaningful descriptions of the natural world. (de Neufville 2015)

The second sentence does not follow from the first. To see this, let's imagine we know that contemporary scientific theories are not only meaningful, but also highly accurate, descriptions of the natural world. It follows that we should think of those theories in just such a way, i.e. as approximately true descriptions, on standard accounts of knowledge. But does this mean that their value consists, wholly or even partly, in the fact that they are such descriptions? No. It's possible to maintain that the primary value of scientific theories is practical, e.g. in providing us with predictive power concerning observable things. And this might hold even if the predictive power of theories is closely correlated with their truthlikeness. Finding truthlike theories could merely be a necessary means to an end. Analogously, the value of shopping doesn't consist in the loss of money it inevitably involves.

I chose this entry because it provides a quick and easy way to illustrate the point; but this kind of misrepresentation appears in journal articles and monographs too. This claim is supported by the next example, where instrumentalism is instead mischaracterized as centrally involving a methodological thesis, in one of the most influential books on scientific realism in recent decades:

> Syntactic instrumentalism comes in two variant forms: eliminative and non-eliminative. The *non-eliminative* variant (a kind of which can be associated with Duhem) is that one *need not* assume that there is an unobservable reality behind the phenomena, nor that science aims to describe it, in order to do science and to do it successfully… *Eliminative instrumentalism* takes a stronger view: theories should not aim to represent anything 'deeper' than experience, because, ultimately, there is nothing deeper than experience to represent. However it will typically resist the project of translating away *t*-discourse [i.e., discourse about unobservables]… (Psillos 1999: 17)

Let's begin by considering the way that non-eliminative instrumentalism is represented as a thesis concerning what it takes – or more appositely, doesn't take – for a person, or perhaps a group of people, to be able to do science successfully. Here's why this is a serious mischaracterization of instrumentalism. Imagine an ardent realist who holds that all contemporary scientific theories are almost entirely true, when taken at face value. She believes in black holes, DNA, atoms, electrons, photons, quarks, the Higgs boson, and even superstrings. She also believes that these things have the properties standardly ascribed to them, and behave in the ways they are standardly said to behave, e.g. that black holes emit Hawking radiation, that atoms have nuclei, and that electrons have an intrinsic property of spin. Must she also, on pain of inconsistency, commit to the falsity of 'non-eliminative instrumentalism', as Psillos defines it? On the contrary, she might wholeheartedly endorse it.

First, she might hold that scientists have simply learned, and never needed to *assume*, that there is an 'unobservable reality behind the phenomena'. She might think that they tried talking about unobservable things, found this was useful, and hence acquired good evidence for the existence of such things. (After all, realists tend to think that empirical success is indicative of probable truth-likeness.) She might also think, more importantly, that successful science was done *before* any good evidence for unobservable things was acquired, e.g. by ancient and medieval astronomers.

Second, more incisively, she might think that it's possible to do science well while *mistakenly* thinking that all talk about unobservable things is just for convenience. For example, two scientists might select theories (or models) on the basis of exactly the same criteria – simplicity, consistency, and so forth – and weight those criteria in precisely the same way, yet *disagree* about the ultimate purpose of the theory-selection (or model-selection) process. They could agree on what the best theories (or models) were, but *disagree* about what those best theories (or models) achieve.

The nub of my argument in the last three paragraphs, in summary, is simple. Instrumentalism – whatever it is – is a form of anti-realism. But the position that Psillos calls 'instrumentalism', in the passage cited, is compatible with (ardent) realism (in at least one variant). Therefore, the position that Psillos calls 'instrumentalism' is not instrumentalism.

Let's now briefly consider whether 'eliminative instrumentalism' is also characterized in a methodological way in the passage quoted above. Unfortunately, the prose concerning this is awkward to interpret because it has a highly metaphorical, and hence undesirably vague, character. For example, what does it mean for theories to aim, and to say that theories are under an obligation (not to aim at one thing in particular)? Given the methodological character of the previous part of the passage, however, it seems fair to interpret the claims about 'theories' as claims about *scientists*, e.g. about what scientists should do *with* theories. (Note that 'science aims' is used in the definition for the 'non-eliminative' variant, too. There is a regrettable precedent for talking about 'science aims' in different ways – see Rowbottom 2014 – but one reading is 'scientists aim'.) It therefore appears reasonable to suspect that Psillos's 'eliminative instrumentalism', as defined in this passage, is not a form of instrumentalism either.

What explains Psillos's mischaracterization of instrumentalism as methodological in character? The answer, I suspect, is that several early instrumentalists were practicing scientists, who took their views on the value and purpose of science, and indeed their views on fundamental ontology, to be more closely linked to scientific practice than was strictly necessary. That's what we saw with Mach, who was plausibly one of the philosophers that Psillos had in mind in writing of 'eliminative instrumentalism'. As we've seen, Mach's view was that there are no ('physical') entities beyond the phenomena, and hence that there are no unobservable ('physical') entities. It follows that theories *cannot* – not that they 'should not' (whatever that means) – 'represent anything deeper than experience' (Psillos *ibid.*). But strictly speaking, as we saw in the previous section, nothing of substantial methodological import follows. In short, one might agree with Mach on these matters, as well as on his thought that science is valuable (and/or progresses) primarily in so far as it saves the phenomena, etc., yet hold that realist scientists are, or that a realist approach to science is, best. For example, one might think that realist scientists are typically better motivated because of their (delusional) beliefs that they're uncovering deep mysteries concerning the world, and one might think that developing theories positing unobservable entities is typically indispensable for achieving a superior understanding of how phenomena interrelate. One might even go so far as to think that realist scientists are typically better at developing theories positing unobservable entities than anti-realists. An instrumentalist certainly needn't agree with the remarkably strong descriptive view attributed to her by Sorensen (2013: 30), namely: 'the scientist merely aims at the prediction and control of the phenomena … scientists are indifferent to the truth'.

Rigid presentations of instrumentalism are convenient for realist authors, or other opponents of the movement, in so far as they present clear targets for assault. But the targets are straw. And were the roles to be reversed in this process, realists would object. If a modern anti-realist were to declare that scientific realism involves the claim that 'contemporary scientific theories are approximately true', for instance, then a moderate realist might respond by saying that this is only a rough characterization of the epistemic element of scientific realism, and that scientific realism survives when it's recast as involving a weaker claim such as 'well-confirmed theories in mature sciences are typically approximately true'. But what's sauce for the goose is sauce for the gander. There has been considerable movement towards the centre, away from the extremes, in the scientific realism debate *as a whole* over the past century. Yet identification with traditions and movements continues.

Nevertheless, some authors *sympathetic* to instrumentalism also characterize it in ways that are unnecessarily strong. For instance, Sober (1999: 5) claims that:

> Instrumentalism does not deny that theories are and ought to be judged by their simplicity, their ability to unify disparate phenomena, and so on. However, instrumentalism regards these considerations as relevant only in so far as they reflect on a theory's predictive accuracy. If two theories are predictively equivalent, then a difference in simplicity or unification makes no difference, as far as instrumentalism is concerned.

We've already seen two reasons why this is too strong. First, the more economical of two empirically equivalent theories may be the more valuable, for an instrumentalist, because of the greater ease of using it or remembering it. Second, gaining an understanding how the phenomena relate, in addition to 'saving' them, may be of value to some instrumentalists.

4. Objections to Instrumentalism

Since instrumentalism is a movement, few instrumentalists defend precisely the same theses. Hence, a reasonable objection to any given form of instrumentalism is unlikely to be a reasonable objection to most other forms. Nevertheless, it's possible to point to classes of objections which are similar in character in some significant respects, and which are pertinent to all – or, failing that, almost all – forms of instrumentalism. Those covered here all involve the concept of observability.

One type of argument is that the set of unobservable entities has shrunk in the course of past science, and should be expected to shrink considerably further in future science. To understand how this type of argument works, it's important to note that 'unobservable' can legitimately be understood in different ways. Like other notions of considerable historical significance in the philosophy of science, such as 'verifiable' and 'falsifiable', it's modal. To be specific, 'unobservable' means 'impossible to observe'. But the impossibility might be either *in practice* or *in principle*. (Or to put it differently, the 'impossibility' may be context bound, rather than fully general.) In practice, we cannot build a spacecraft capable of travelling from Earth to Mars in under four months. However, it's plausible that we can do so in principle, in so far as it's plausible we will be able to do so in practice at a later time.

So the set of the observable in principle is plausibly much larger than the set of what's observable in practice at present. A maximally strong realist claim is that *everything* is observable in principle. Weaker assertions, which are correspondingly more plausible, involve more restricted claims, e.g. that relatively few things are unobservable in principle.

But how may what's observable in practice increase, over time? Maxwell (1962) argues that there are two main mechanisms. First, on the assumption that observations are theory-laden (or even theory infected), we may discover new theories. Second, we may devise and build new instruments, via the use of which we may extend our sensory range (in some sense).[11]

[11] Another possibility is that the community of scientists might change, e.g. mutant humans (Maxwell 1962) or alien species (Churchland 1985) may be introduced. However, this may not be so different from our instruments changing. Indeed, our eyes and ears may be viewed as instruments of a natural variety.)

Let's think about theory change first. Let's grant, for the sake of argument, that all observations – or, if preferred, many observations relevant to science – are theory-laden in a relatively uncontroversial sense. Let's say that (sincere) *observation statements*, rather than observations simpliciter, are theory-laden. Let's also say that such statements express beliefs. Hence what a person comes to believe, on the basis of their sensory inputs in some specific situation, depends on the theories that they (explicitly or implicitly) believe to be true (or approximately true). Here's an example. If my nine-year old daughter Clara and I were walking in the fields near our home and came upon a beast, she might believe 'I am seeing a rabbit', whereas I might believe 'I am seeing a hare'. What's more, I might believe 'I am seeing a lagomorph'. She wouldn't, as she doesn't yet possess the concept 'lagomorph'.[12]

The thesis that observations are theory-laden has its roots in instrumentalist thinkers. Duhem (1954) is often cited in connection with the claim, and Poincaré (1905: 159–160) agreed:

> It is often said that experiments should be made without preconceived ideas. That is impossible. Not only would it make every experiment fruitless, but even if we wished to do so, it could not be done. Every man has his own conception of the world, and this he cannot so easily lay aside. We must, for example, use language, and our language is necessarily steeped in

[12] (One can push deeper, and think about differential knowledge of taxonomy, of the Ancient Greek roots of 'lagomorph' – i.e., λαγώς and μορφή – and so forth. How one conceives of the differences will depend on whether one adopts an atomistic or a holistic view of the content of beliefs. On this, see Schwitzgebel (2015: §3.2).)

> preconceived ideas. Only they are unconscious preconceived ideas, which are a thousand times the most dangerous of all.

Indeed, if a realist holds that observations are theory-laden to a high degree – following Feyerabend (1958) – then it will be tricky for her to defend claims that she is inclined to defend, such as that we have the ability to latch onto natural kinds, conceptually, on the basis of experience. Why might our observations not be easier to save and comprehend when we employ non-natural category schemes, for example? And wouldn't successful actors outcompete deep knowers, from an evolutionary perspective?

Moreover, there is no need for the instrumentalist to deny that observations are theory-laden in order to maintain that there's a significant distinction between the observable and the unobservable. For example, the distinction might be relativised to a privileged theory set that tends to remain stable over time. One option is to appeal to innate theories of some form or another, as some philosophers have done when it comes to our linguistic ability. Another option, which I articulate and defend (Rowbottom Manuscript: Ch. V), is to look to the set of folk theories central in our upbringings and daily lives. What counted as a table or chair for Newton counts as a table and chair for you, and vice versa. The same may be said for many property ascriptions, like 'spherical', 'blue', 'hard', and so on. (Your 'know how', concerning when such terms apply to things in everyday life, is no different.) And one could understand scientific progress to centrally involve improving our predictive ability and/or understanding as *expressed in that restricted, relatively stable, language*.

One natural realist response is that stability isn't absolute; there are folk theories now about computers, for example, that there weren't in the past. However, the instrumentalist may rejoin that these theories tend to remain stable once they're introduced, and also that they have a large degree of autonomy from the content of science. Indeed, technological changes often occur without significant theoretical changes. A stronger realist objection might be that the folk theoretical set isn't, or shouldn't be, privileged. And here, some of the deeper considerations that split realists and instrumentalists will come to the fore. Different values – and even perhaps stances (§III.7) – are at play. Instrumentalists tend to be more pragmatic in orientation than realists, and to see little or no intrinsic value in science. Facts about the unobservable world, even if they can be known, may be seen to be as trivial as facts about numbers – e.g., that the lowest prime number is two – unless they have some 'cash value' in the world of experience.

A different instrumentalist strategy, proposed by Stanford (2006), is also worthy of mention. This is to deny that there is any 'privileged or foundational role for the hypothesis of the bodies of common sense or any specific hypothesis *built into* the instrumentalist's account of the world and our knowledge of it' (Stanford 2006: 205). Why does he say this? Stanford's basic idea is that both realists and instrumentalists take some theories to be true (or highly truth-like) and others to be merely instrumentally useful. In particular, scientific realists continue to think it's reasonable to use discredited theories – like the ideal gas law, classical mechanics, and so forth – purely as means to predictive ends in particular contexts.[13] And 'the instrumentalist

[13] A potential differentiator not mentioned by Stanford, however, is that realists may be much more inclined to think that such theories (and/or associated models) aren't vehicles for providing genuine

simply assigns a much larger set of theories we have to [that] category' (Stanford 2006: 205). Stanford thinks instrumentalists can be justified in so doing on the basis of the argument from unconceived alternatives; see §III.6 and Rowbottom (In Press; Manuscript, Ch. III).

Let's now think about whether we can use instruments to extend the range of our observations. Carnap (1966: Ch. 23) suggests the answer is "No" only when we consider *direct* observation, which, on Carnap's view, must be technologically and inferentially unaided. Moreover, Carnap allowed that 'observable' and 'unobservable' are vague (and van Fraassen 1980 follows suit on this):

> There is a continuum which starts with direct sensory observations and proceeds to enormously complex, indirect methods of observation. Obviously no sharp line can be drawn across this continuum; it is a matter of degree. (Carnap 1966: 226)

This makes it dubious that the *directly* observable is of special significance in ontological, epistemological, axiological, or methodological senses. For example, I can see it's snowing outside from where I'm sitting. But should I treat my observation through the double-glazed window – through the panes of glass – as less reliable than the one I'd make if I were outside in the cold? The instrumentalist need not answer in the positive. Rather, there is considerable leeway for an instrumentalist to give ground

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understanding. Hence, here is an interesting contact point between Stanford's version of instrumentalism and my own version. For example, Stanford's account might be strengthened by incorporating an anti-realist conception of understanding.

on the sense of 'observable' pertinent to her position, while maintaining that many of our modern instruments do not enable observation. Such an instrumentalist might concede that optical microscopes enable the observation of cells (in line with Hacking 1985). But she might deny that scanning-force microscopy enables the observation of atoms, or that the so-called 'Sudbury neutrino observatory' was *really* an observatory.[14] It might be added that some realists just concede that the observable is limited, and argue directly that detection has a similar status to observation in the context under discussion. See Contessa (2006), for example, on how detection procedures can provide good evidence for existential claims.

This brings us onto a final argument type. Rather than arguing that the set of the unobservable will shrink over time, and so forth, a realist may instead deny that there's a *significant* distinction between the observable and the unobservable. The sense of 'significant' will be determined by the context, namely the specific kind of claims – semantic, epistemic, etc. – under dispute. For example, in response to the claim that science *is* merely an instrument for predicting how observable things behave, one might conceivably object that it's not possible to predict how observable things behave without knowing about and predicting how (at least some) unobservable things behave, in some parts of contemporary science. One might suggest that we couldn't genetically engineer *drosophila melanogaster* variants without knowing some things about DNA, such as that it contains 'base pairs' of

[14] One important possible view, suggested by van Fraassen (2001: 155), is that: 'The instruments used in science can be understood as not revealing what exists behind the observable phenomena, but as creating new observable phenomena to be saved.' As intimated in the main body of text, one might weaken this view, e.g. by introducing 'typically' or 'often' before 'can'.

purines and pyrimidines, even granting that DNA is unobservable. The basic idea behind this objection is that the empirical success of a theory is linked with how truthlike it is. (Note that in this example, we're able to intervene; the 'empirical success' at stake doesn't merely concern passive observations.) Realists tend to think that the empirical success of a theory indicates its truthlikeness, whereas instrumentalists (like many other empiricists) tend to deny this. The classic argument for the instrumentalist view is the so-called pessimistic induction, presented by Laudan (1981) and (plausibly much earlier by) Poincaré (1902), which is discussed in more detail in §II.2. The basic idea behind this argument is that past theories have been responsible for great predictive successes in spite of 'centrally' positing unobservable things that we now take not to exist, such as caloric and phlogiston.

5. Conclusion

In this chapter, we have seen how instrumentalism is best characterized relatively broadly, with reference to the work of those involved in the movement's genesis. We have also seen how it is often mischaracterized, even in recent work. Finally, we have seen that many of the central arguments concerning the viability of instrumentalism concern the nature and significance of observability.

If there is a single 'take home' message, then it is as follows. Instrumentalism is dead only to the extent that one understands it in a highly restrictive, and correspondingly uncharitable and implausible, fashion. In the words of Stanford (2006: 192): 'instrumentalist sympathies have produced a wide variety of importantly divergent attitudes (sometimes within the works of a single author) toward the cognitive, semantic, and epistemic status of theories…' Modern instrumentalists continue to develop, explore, and defend new views on the science (and parts thereof). Typically, these new views are more sophisticated, and hence defensible, than their earlier counterparts.

Related Entries

Empiricism; Epistemic Stances and Voluntarism; Modelling, Idealisations and Fictions; Realism and Logical Empiricism; Scientific Progress; Structural Realism

Biographical Note

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