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## Aimless science

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## **Aimless Science**

This paper argues that talk of ‘the aim of science’ should be avoided in the philosophy of science, with special reference to the way that van Fraassen sets up the difference between scientific realism and constructive empiricism. It also argues that talking instead of ‘what counts as success in science as such’ is unsatisfactory. The paper concludes by showing what this talk may be profitably replaced with, namely specific claims concerning science that fall into the following categories: descriptive, evaluative, normative, and definitional. There are two key advantages to this proposal. First, realism and its competitors may be understood to consist of highly nuanced variants. Second, scientific realism and its competitors may be understood as something other than ‘all or nothing’ theses about science. More particularly, one may accept that there are general claims concerning science in some of the identified categories, but deny that there are such claims in the others.

Keywords: aim of science, scientific realism, scientific progress, van Fraassen, constructive empiricism

### **1. Introduction**

Talk of ‘the aim of science’ is widespread in the literature on general philosophy of science, and commonplace, in particular, in discussions of scientific method and scientific realism; see, for example, van Fraassen (1980: 8–9), Newton-Smith (1981: ch. 3), Popper (1983), Laudan (1984), and Sankey (2000). But should this talk be taken literally? And if not, does the metaphor serve any useful purpose?

I hope to convince you that the answers to both these questions lie in negative, and also that the issues at stake in the general philosophy of science become clearer when we adopt a non-teleological mode of discourse. In fact, talk of aims in the philosophy of science is the source of considerable confusion, among professional philosophers as well as students, as I have experienced first-hand on numerous occasions.<sup>1</sup> One such recent occasion, a research seminar, prompted me to write this paper. In particular, the notion that ‘the aim of science’ is some kind of function from the aims of individual scientists, in doing science, was assumed by many of the audience. As we will see, however, this is not what is typically intended by the phrase in the philosophy of science. And in any event, even if there is such a sense in which one may legitimately speak of ‘the aim of science’, this is a matter for empirical sociological (or socio-historical) study. (It should also be carefully defined, e.g. in terms of a clearly specified aggregation function of individual aims, if it is to be rendered an object of that kind of empirical study. But to the best of my knowledge, no such definition exists.)

I will proceed by discussing the most highly-influential discussion of the aim of science in recent decades, namely that provided by van Fraassen (1980), which has had a considerable impact on subsequent work in general philosophy of science and beyond.<sup>2</sup> But before I continue, I should like to note that van Fraassen is, in one sense,

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<sup>1</sup> I have also used such talk, albeit somewhat unwillingly, in Rowbottom (2010). Interestingly, concerns involving the use of ‘the aim of science’ were responsible for major revisions of the initial version of that paper being requested. The second section, which comprises around one thousand words, was produced in response.

<sup>2</sup> Google scholar helps to give a rough measure; the book has been cited almost 3000 times. Compare Popper (1983), which has only been cited 600 times, and Laudan (1984), which has only been cited 300 times.

an undeserving target; many other authors, beforehand and thereafter, have used the phrase without any serious discussion, and/or in confusing fashions.

Here are some brief examples. First, Cooper (1964), in a paper on ‘The Aims of Science’, simply fails to discuss what the phrase is supposed to mean. Second, Popper (1983: 132), in a book with ‘Aim of Science’ in the title, writes only:

To speak of ‘the aim’ of scientific activity may perhaps sound a little naïve; for clearly, different scientists have different aims, and science itself (whatever that may mean) has no aims. I admit all this. Yet when we speak of science, we do seem to feel, more or less clearly, that there is something characteristic of scientific activity, and since scientific activity looks pretty much like a rational activity, and since a rational activity must have some aim, the attempt to describe the aim of science may not be entirely futile.

A more recent introductory discussion by Newton-Smith fares no better. The term is introduced as follows:

At what is science especially successful? ... Are theories just tools for predicting and manipulating the world? ... This [is] controversy about the aim of science. (Newton-Smith 2000: 3)

Just two pages later, Newton-Smith (2000: 5) writes: ‘Much philosophy of science concerns issues of aims and methods. In the course of pursuing our aim (whatever it may be) using our rich range of methods, we have crafted descriptive and explanatory

tools...’ But science can be ‘especially successful’ at something without us *pursuing* that end; and similarly, theories can be ‘just tools for predicting and manipulating the world’ even if all scientists think otherwise, and *pursue* a different end. It is easy to see how a student—and even a professional philosopher—reading this introduction may emerge confused. A simple and effective solution, I will urge, is to ditch talk of ‘the aim(s) of science’.<sup>3</sup>

## 2. Van Fraassen on the Aim of Science: A Critique

Van Fraassen (1980: 8, 12) characterizes scientific realism and his alternative, constructive empiricism, as involving two competing claims about the aim of science:

*Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true.*

*Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate.*

Now if the aim of science were understood to be determinable by looking to the aims of individual scientists in doing science—what they hoped to achieve, be it fame, fortune, finding the truth, or finding empirically adequate theories—then the difference of opinion would be easily settled by sociological study. The appropriate procedure is simple. State what has to be true of the aims of individual scientists in order for the overarching ‘aim of science’ to be *x*, and then study the aims of

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<sup>3</sup> Note also that science may be ‘especially successful’ at achieving some end even when achieving said end is not *constitutive* of the activity. The significance of this will become apparent in due course.

individual scientists. For example, one might (rather crudely) take some  $x$  to be *an* aim of science if at least 50% of scientists hope to achieve (or get closer towards)  $x$  by doing science. And one might take some  $y$  to be *the* aim of science if it is the single most prevalent aim of scientists in doing science.<sup>4</sup> Then simple quantitative investigations, e.g. appropriate survey work, would bring to an end the interminable debate between scientific realists and constructive empiricists. (At least, that's for the time being. On such views, the aim of science could change over time.) In the words of Rosen (1994: 144):

A literal interpretation... would... have it that SR [scientific realism] and CE [constructive empiricism] are opposing proposals about what scientists actually think.

We would also take a dim view of claims about the aim(s) of science based on limited personal experience, or on armchair speculation (and hence of van Fraassen 1980, and the resultant philosophical exchanges on the aim of science). One can also find many claims that appear remarkable on this literal view, such as: 'The establishing of general laws is an invariant and focal aim of all science' (Cooper 1964: 328). It is highly dubious that this goes for biology, for example; see Keller (2007) and Author [2011c].

This said, it should be of little surprise that van Fraassen (1980) does *not* take 'the aim of science' to reflect the aims of scientists; that the literal interpretation 'just can't be right' (Rosen 1994: 144). Van Fraassen (1980: 8) explains this by using chess as an

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<sup>4</sup> Much more refined aggregation functions may be possible, e.g. after those used in social epistemological contexts; see List and Puppe (2007). But this will not change the underlying point here; the result of applying such a function would have to be determined by spade work.

analogy for science. The aim of chess, we are told, is to give checkmate. The aims of individual chess players may be quite different: to become famous, rich, or simply to have some mental stimulation. And even if all chess players just so happened to share the same aim, this would not be sufficient for it to constitute an aim of chess. Thus as he explains elsewhere, constructive empiricism should not be associated with the claim that:

(all or most) real scientists aim to construct empirically adequate theories, and believe the theories they accept to be empirically adequate... (Van Fraassen 1994: 180)

So how should talk of the aim of science be understood? Van Fraassen (1980: 8) adds only that:

What the aim is determines what counts as success in the enterprise as such; and this aim may be pursued for any number of reasons. Also, in calling something *the* aim, I do not deny that there are other subsidiary aims which may or may not be means to that end.

Note that van Fraassen's claim, here, is that *the aim determines what counts as success*. It is not that *what counts as success determines the aim*. But in the case of chess, this appears backwards. On the contrary, *just because* giving checkmate to one's opponent counts as success—i.e. winning—in chess, it is the aim of chess to give checkmate to one's opponent. In any event—and here is the fundamental point—we may describe chess quite adequately without saying that it has an aim. Instead, we

may state the rules of the game, and what it takes to win (or to succeed). No mention of any aim of the game is required. Success suffices.

But how about pulling the trick of saying that chess players, like scientists, have *the shared personal aim of succeeding*? This is precisely what van Fraassen (1994: 182) appears to do at one stage:

[S]cientists with their very different motives and convictions participate in a common enterprise, defined by its own internal criteria of success, and this success is their common aim ‘inside’ this cluster of diverging personal aim. How else could they be said to be collaborating in a common enterprise? The question is only what that defining criterion of success is.

At best, however, this trick suggests that all philosophers of science should *agree* on what the aim of science is, namely to succeed at science. But if that is so, then there is no genuine dispute over *aims* at all, and it is a mistake—or at least, highly misleading—to express the difference between scientific realism and constructive empiricism in those terms. It would again be better, surely, to refer only to different accounts of *what constitutes success in science*.

Besides, the claim that many (or most) people who play chess do so with the aim of winning *is* sociological. (If you doubt this, then you must think that this claim is not open to empirical refutation. It is. For example, I have played many games in chess tournaments in which I have aimed for an early draw, e.g. by complicating the



position in the middle game and unnerving the opponent; and I am happy with the draws I have achieved against superior players.<sup>5</sup>)

But might we not stick with the notion of success, as a replacement for the talk of aims? Not on the strength of van Fraassen's initial analogy, in so far as science bears little significant resemblance to chess.<sup>6</sup> First and foremost, in chess, what it takes to win is stipulated as *part of the game design*. This is not so in science. Second, it is dubious that the rules of science are enough to *define* it, or indeed play a definitional role at all, in the way that the rules of chess serve to define that game. In chess, to fail to know any of the rules is to be unable to play the game; to fail to know some of them means (at least) that one is unable to play the game properly. Similarly, to know the rules but intentionally violate them is to fail to play the game, but instead to adopt the semblance of doing so (e.g. to cheat).<sup>7</sup> Third, and finally, science involves groups and communities; so at best, it may only be understood as a team game.<sup>8</sup>

In relation to the second and third of the points above, consider that there are several possible answers to van Fraassen's rhetorical question, "How else could they be said to be collaborating in a common enterprise [unless there were a common criterion of success]?" One short answer, for instance, is that many scientists simply obey instructions to perform particular local tasks (and collaborate in the same way a private does with an officer); and while they may wish to *succeed* in following those

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<sup>5</sup> It may be objected that I would have taken a win if the opportunity arose, e.g. if I spotted a way to force mate in a small number of moves. I accept this. But I was still playing *with the aim of not losing*, rather than the aim of winning.

<sup>6</sup> Kuhn (1996) infamously used a chess analogy too, in order to illustrate how normal science is a puzzle solving process. However, he elsewhere argued that normal scientists rely on pattern recognition, rather than rule following. See Bird (2000: 71–75) and Rowbottom (2011a).

<sup>7</sup> See also Resnik (1993).

<sup>8</sup> This final element of disanalogy is perhaps not as troublesome as the others; see Rowbottom (2010: 211).

instructions and performing those tasks, this hardly suggests that they want to succeed (or contribute to success) in some further global sense. Another reaction to van Fraassen's question is to simply deny that all scientists are *collaborating* in a common enterprise; on the contrary, many of them are *competing*. My overarching point, here, is that there is a chasm between what counts as 'success' for any given individual or research group and what counts as 'success' for the whole; and in fact, some will legitimately question the very idea that there *is* such as thing as 'success' for the whole of science, above and beyond different local criteria for 'success' in different areas of (what we just so happen to classify as) science.<sup>9</sup>

The only obvious way to rejoin, and urge that there genuinely is something philosophically distinct that counts as success in science, is show how to determine what this is. Perhaps rather than asking scientists what their own aims are, i.e. going into sociology, we might look to other empirical data? In fact, van Fraassen (1994: 186) notes this kind of possibility by considering a different analogy, between science and war, which is rather better than his earlier one between science and chess:

Clausewitz' doctrine of war: [the aim of] war is the continuation of diplomacy by other means. This does imply:

the soldier's aim, the criterion of his [sic] success, is the continuation of [his/her country's] diplomacy.

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<sup>9</sup> This is closely related to the distinction, made by Magnus and Callender (2004: 321), between *retail* and *wholesale* approaches: 'Wholesale realism seeks to explain the success of science in general; wholesale anti-realism seeks to explain the history of science in general. Dissolving the debate... involves attending to the retail arguments without trying to settle the debate in an all-or-nothing, wholesale manner.' I disagree, however, with the suggestion that wholesale anti-realism concerns history, rather than success; and I believe that this is illustrated by van Fraassen's emphasis on success.

Van Fraassen continues by asking us to imagine that we can find no soldier who says that her aim is to continue her country's diplomacy. What then? Is Clausewitz refuted? Van Fraassen (1994: 186–187) thinks not, because some of the soldiers, at least, understand what war is really all about:

I do not mean to rely on this as an example of false consciousness. What happens in war flows from the conscious self-understanding of the participants. It also flows from the aims, intentions, and beliefs of the actual participants. But the two are not the same. I think that the soldiers or at least the generals understand the business of war very well. If they are sincere in their statement of their own aims and intentions, then it follows, I think, that they are convinced that pursuing those is compatible (and perhaps best combined) with the continuation of their country's diplomacy by force of arms.

So one might think that we can sociologically probe scientists' understandings of science without asking them about their own aims in doing science. For example:

Approach some scientists you know and mention some of their most valued scientific colleagues. Then tell them... that as a matter of fact those colleagues are not pursuing the aim of finding true theories, but are privately concerned only to construct empirically adequate ones. Now ask them whether, with this new information in hand, they still regard those men and women as real scientists? (Van Fraassen 1994: 187)

Several obvious variations on this experiment are possible; one may ask the interviewees to think of figures from the history of science, or even from future science, in place of colleagues. But this is not the way that van Fraassen (1994: 188) wants to go: ‘I... do not equate the aim of science... even with all or most scientists’ understanding of what science is.’

So how about answering the question with reference to publications, citations, and other relevant awards? Or one could look a little deeper; one could look for patterns in the kind of papers that are (or have been) well-cited or otherwise lauded. And one might gain, as a result, an insight into what kind of work *is (or has been) actually successful* in science (above and beyond what kind of work is thought, by a majority of scientists, to be successful). This may be understood as a kind of systematic approach, which bears similarities to the ‘aims as characteristics’ option which Resnik (1993: 229–230) considers to be the best available.<sup>10</sup> The underlying idea is that the system favours (or has favoured) some kinds of activity, but not others. Yet there would be serious problems in conducting this kind of research. For example, why should we expect the relevant differences to show up in the papers, without an interpretative spin provided by realism or one of its competitors? Perhaps papers that posit new theoretical entities are (or have been) popular, for instance. But does this show that the discovery of such entities is (or was) a scientific priority? Or does it just happen to be the case that such theoretical posits are usually bound up with significant

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<sup>10</sup> Resnik (1993: 230) is mainly concerned with how aims might justify methodological rules. And he notes, correctly, that this view will not do: ‘If we think of aims as characteristics which describe scientific conduct, then aims cannot justify methodological rules because these characteristics are too general and abstract to offer genuine guidance.’ Via this different route, he also comes to the conclusion that (1993: 231): ‘philosophers of science might achieve more useful results by shifting their attention away from the aims of science... [which] seem to have little... effect on working scientists’.

new empirical predictions? And in any event, even disregarding such difficulties, one would end up with the question: is (or was) the way the system set up *good* or *bad*? The worry is that the realist will say one thing, and the anti-realist another, under many key circumstances (e.g. if we somehow learned that saving the phenomena was what was most rewarded). So we would not have penetrated into the real differences that divide the two camps.<sup>11</sup>

Let me put the point more bluntly. The worry is that talk of some kind of essential ‘success in science’ papers over the genuine division between many realists and anti-realists. In particular, a difference in value judgements, concerning what kinds of inquiry are worthwhile, is (arguably) instead treated as if it were a disagreement on a matter of fact about science. For van Fraassen, for instance, the valuable product is empirically adequate theories. For many realists who disagree, the valuable product is true (and empirically adequate) theories. And rather than cast the difference as being ‘For van Fraassen, science would be finished when it had empirically adequate theories of everything, although it might not be finished for the realist’, we can instead say ‘For van Fraassen, there would be no value in continuing to inquire in some domain when one had empirically adequate theories, although there might be for the realist (if, for example, those empirically adequate theories were known not to be true)’. So the presumption of ‘success in the enterprise as such’ (like that of ‘the aim of science’) sets the debate up in a way that is potentially highly misleading.

I therefore suggest not only that talk of ‘the aim of science’ should be ditched, but also that ‘what counts as success in science as such’ is an inadequate replacement. I

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<sup>11</sup> This is the kind of problem that van Fraassen (1994) grapples with in closing. It is always open to a philosopher to maintain that some activity is not science because it does not have the correct features.

will now turn my attention to what to put in their place, and will propose a roomier framework in which the finer distinctions between specific realists and anti-realists can be made more readily apparent.

### 3. Aims and Success: Lost in Translation

So when a philosopher of science says ‘the aim of science is  $x$ ’, what on earth *might* she mean? Like Bacon (1620: bk. II, §2) in the face of scholastic philosophy, I propose that we can profitably dispense with such teleological discourse, which ‘rather corrupts than advances the sciences except such as have to do with human actions’. There are four distinct kinds of theses, each decidedly philosophical (rather than sociological) in character, which we may instead discuss:

*Descriptive—‘What Science Can Be Expected To Do’—Theses*

For example:

- (a) Science can reliably achieve  $x$ .
- (b) Science can make reliable progress towards  $x$ .
- (c) Science will achieve  $x$ .
- (d) Science has a higher objective probability of achieving  $x$  than any other of a peculiar class of alternatives,  $\{x_1, \dots, x_n\}$ .

*Evaluative—‘Why It’s Worth Doing Science’—Theses*

For example:

- (e) Science would not be worth doing if it could not reliably achieve  $x$ .
- (f) Science is only worth doing if it can reliably achieve  $x$ .

- (g) Science is only worth doing if it will achieve  $x$ .
- (h) Science would no longer be worth doing if it achieved  $x$ .
- (i) Science is worth doing more if it can reliably achieve  $x$  than it would be if it could only reliably achieve any (or all) of a peculiar class of alternatives,  $\{x_1, \dots, x_n\}$ .

*Normative—‘What Scientists Should Do’—Theses*

For example:

- (j) Scientists should (as a community) strive to achieve  $x$  if they are able.
- (k) Each and every scientist should strive to make progress towards achieving  $x$  if he/she is able.

*Definitional—‘What Science Is’—Theses*

For example:

- (l) An activity counts as a science if it can reliably achieve  $x$ .
- (m) A branch of inquiry is not scientific if it cannot make reliable progress towards  $x$ .

These lists of possible theses are just indicative; they are by no means exhaustive. Some of these types may also be connected; and there will be differences of opinion about how it is appropriate to connect them (especially when it comes to the definitional theses and the others). This is not the place to discuss such connections; instead, I want to suggest that this framework may be used to delineate, and clarify, the available spectrum of positions in what are normally called the realist and anti-realist camps.

How about mapping the view of van Fraassen in this way?<sup>12</sup> He denies (a)–(c) for  $x$  as truth or approximate truth; and I think, in so far as he is an anti-inductivist, that he *should* also deny these claims when  $x$  is empirical adequacy.<sup>13</sup> I suspect, however, that he *does* endorse (b) for  $x$  as empirical adequacy; and I am also reasonably confident that he would endorse (d), when  $n$  is two, for  $x$  as empirical adequacy and  $x_1$  and  $x_2$  as truth and approximate truth. In the evaluative domain, I take it that he would reject theses (e)–(g) for any of the standard options for  $x$ , such as truth, approximate truth, structural adequacy, and ‘empirical adequacy’. I believe, however, that he would endorse (h) for  $x$  as empirical adequacy. I think he would also endorse a further thesis (f\*) for  $x$  as empirical adequacy (if he does indeed endorse (b) for the same  $x$ ): ‘Science is only worth doing if it can make reliable progress towards  $x$ ’.

In the normative dimension, I believe he says that (j) is true for empirical adequacy but not truth or approximate truth. And finally, on the definitional theses, I am not entirely sure where he stands; I suspect, however, that he would agree with me that we should not define science in terms of what it can achieve.<sup>14</sup>

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<sup>12</sup> Otávio Bueno has suggested to me that his position might be best understood as concerning what’s *constitutive* of science. I need not deny this, however. Rather, I think that constitutive claims can be broken down into claims falling into the aforementioned categories (especially descriptive, evaluative, and definitional).

<sup>13</sup> In Rowbottom (2010, 2011b), I argue that this goes through from the point of view of evolutionary epistemology.

<sup>14</sup> Here’s my own view. I deny all of the first three descriptive theses, (a)–(c), where  $x$  is truth or approximate truth. I am also more of an anti-realist than most, in so far as I also deny these theses when  $x$  is structural adequacy, empirical adequacy, or even approximate empirical adequacy. (I am instead inclined toward thinking that (a) is true for  $x$  as ‘the elimination of empirically inadequate theories’; see Rowbottom (2010, 2011b).) Furthermore, I deny all of the first three evaluative theses, (e)–(g), when  $x$  takes *any* of the aforementioned values. On (h), however, I think that ‘empirical adequacy’, or even something less, will do the trick; for example, I see no value in discovering which of two completely empirically adequate theories is true (although I understand why others might see value here, at least in so far as they wish to satisfy their curiosity about how the world is). In the normative dimension, I deny (j) and (k) when  $x$  takes *any* of the aforementioned values except ‘the elimination of empirically inadequate theories’. In addition, if  $x$  is instead something like ‘save the phenomena relevant to our practical concerns in an economical fashion’ then I am inclined to endorse both. Finally, I reject (l) and



Realists, by way of contrast, are more inclined to think that true statements may be formed by letting  $x$  be ‘truth’ or ‘approximate truth’ in (a)–(c), (e)–(h), and (j)–(m). To be describable as a realist about science at all, it is plausible that a philosopher must think that *at least one* of these statements is true when this is done. And strong realists will think that many of these statements are true when this is done. As such, we have a rough and ready framework for measuring the strength of realist (or anti-realist) commitment. (For example, some are descriptive realists only. Some are normative realists but not descriptive realists.) And all this without talking of the aims of science, or indeed of ‘what counts as success in science as such’.<sup>15</sup>

It is also worth noting the extra freedom that the proposed framework provides, with respect to the difference between local and global issues—or what Magnus and Callender (2004) call *retail* and *wholesale* claims—which was raised earlier. For example, one might think that all of the descriptive questions can legitimately be answered globally (or *wholesale*), whereas none of the evaluative ones can. Or one might think that some descriptive questions can legitimately be answered globally, whereas others cannot. It is therefore possible to chart positions that are far more nuanced than those that can readily be appreciated by talking in terms of ‘aims’ and/or ‘success’.

## Acknowledgements

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(m) in so far as I do not think we should *define* science in terms of what it can (or could conceivably) achieve. (I do not discuss (d) or (i) because this would require an extensive digression.)

<sup>15</sup> In the normative dimension, naturally, we may be said to be discussing what we think scientists should aim for. But it is explicit, in this context, that we are discussing *scientists*.

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