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Bas van Fraassen's Constructive Empiricism and Bruno Latour's Constructivism: The André Kukla's Approach

Marcos Rodrigues da Silva[1] – https://orcid.org/0000-0003-3388-6381

Abstract:

André Kukla has argued that both Bas van Fraassen's constructive empiricism and Bruno Latour's constructivism should be regarded as anti-realist approaches. In order to support his own claim, Kukla brought forward three parallels between them, namely: i) the observable/unobservable distinction in Bas van Fraassen's constructive empiricism may be found at Bruno Latour's constructivism, too; ii) the constructivist's emphasis over the social construction of facts could be (although it was not) acknowledged by van Fraassen; iii) both constructive empiricism and constructivism are approaches about scientific practice. The aims of this paper are, firstly, to introduce and criticize the support gave by Kukla regarding his own conception, and, secondly, to argue for that there is, instead, a stronger relationship between constructivism and realism than constructive empiricism and constructivism.

Keywords: Constructivism; Andre Kukla; Scientific realism; Bruno Latour, Bas van Fraassen

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Introduction

Since its emergence, the concept of constructivism, introduced by the sociologist of science David Bloor in 1976,[2] has been strongly criticized by philosophers who support scientific realism.[3] The argumentative core of these criticisms is underlain from an interpretation about Bloor's constructivism (and other constructivists, as well): for realists, Bloor suggested that science ought to be explained only by social factors. After they had put this interpretation, realists right

[1] Marcos Rodrigues da Silva is a Professor of Philosophy at the State University of Londrina (Universidade Estadual de Londrina). Address: Rodovia Celso Garcia Cid, PR-445, Km 380 – Campus Universitário, Londrina - PR, CEP: 86057-970. Email: mrs.marcos@uel.br

[2] Although there is a tradition of constructivism approaches starting from authors like Fleck and Mannheim, for instance, Bloor's Strong Program undoubtedly brought about many reactions.

[3] The scientific realism is understood in this paper in its well-known standard way: the concept that the success of scientific theories ought to be understood through the convergence between a successful theory and reality since i) scientific terms have reference (even the terms that refers to unobservable entities) and ii) a successful theory provides quite strong reasons for our belief in its scientific propositions, which must be counted as true ones.

away took a second step: the critique of constructivism, which can be introduced by the following argument:[4] *either science is explained only by epistemological criteria or is explained only by social factors; it cannot be explained only by social factors, since, if science is explained only by social factors, then our scientific productions are not convergences to reality, but merely constructions made by groups of people who rule the scientific world; therefore, science ought to be explained only by epistemological criteria.*[5]

There are many problems concerning this argument – but I will not debate them in this paper. My primary purpose (section 1) is stressing its original source: how did this interpretation arise? For realists, constructivism is both a philosophical and sociological view that put epistemological criteria of assessment of science aside (or at least regards such epistemological criteria much less significant than social factors). However, as far as the debate had been developed (and, also, from the conceptual development of constructivist framework itself), it became clear that this interpretation hardly matches to constructivist literature.[6] In the long run constructivists have shown that their approach neither suppresses epistemological criteria nor even places social factors in a higher position than epistemological criteria.

Happily, this new way of grasping constructivist ideas has not been neglected by philosophers who, although cannot be called "constructivists", do not evaluate constructivism as realists did. Several recent accounts by non-constructivist philosophers have supported a new reading of constructivism, a new one that makes possible that constructivism can be understood in the light of those new remarks. Among these studies, stands out the book *Social Constructivism and the Philosophy of Science*, by André Kukla (2000).

At chapter 9 of his book, Kukla seeks to show the relationship between constructivism and a doctrine, for Kukla, seemingly near to it: Bas van Fraassen's constructive empiricism. At this challenging chapter, Kukla points out convergences between Bas van Fraassen's constructive empiricism and constructivism, especially with Bruno Latour's constructivism. Kukla sees that these two approaches are anti-realists.

Despite, to Kukla, van Fraassen to be careful and to have many points of disagreement with regard the constructivists, Kukla argues that such differences are not greater than those that can be found between constructivists themselves (2000, 57). Furthermore, Kukla signals meaningful differences between constructive empiricism and constructivism. However, for him, it is possible to get parallels between them, namely: i) van Fraassen's observable/unobservable distinction may be found in Latour, too; ii) the constructivists' emphasis on the social construction of facts could be (although it was not) acknowledged by van Fraassen; iii) both constructive empiricism and constructivism are approaches about scientific practice.

In the second section of this paper, I introduce Kukla's claims and then I propose an evaluation of its. I am going to suggest that the parallels pointed by Kukla do not support the claims because a) when Latour talks about observables, this talk concerns an issue about methodology of the constructivist approach itself instead about scientific propositions; b.1) van Fraassen might get along with constructivism by means of his theory of scientific explanation,

[4] This way of driving the debate was remarkable described by James Ladyman in his *Understanding Philosophy of Science*, by means of an imaginary dialogue between a realist and an anti-realist (2002, 11-13).

[5] One of the clearest expressions of this interpretation can be found in Kitcher (1993, 162): "The deep point of the sociological critique is that the social forces (…) may be sufficiently powerful that the effects of nature are negligible". Other similar statements can be found in Nola (2014, 297-298), Niiniluoto (1991, 152), Nelson (1994, 541), Laudan (1981, 194), Leplin (1997, 4) and Siegel (1986, 241).

[6] For developments on David Bloor's initial proposal, see Latour (1987, 142), Stengers (2000, 104), Herrstein Smith (1997, chapter 8), Millstone (1978, 120). However, it is significant to emphasize David Bloor's own initial proposal made already clear that epistemological criteria are as very useful as social factors (Bloor 1977, 4-5).

however, van Fraassen himself did not suggest such a convergence; b.2) although van Fraassen could be seen as a philosopher who does not deny the significance of social factors, only this lack of opposition (for social factors) without any further statement in favour of social factors is understood in this paper as a weak evidence for supporting the relationship between constructive empiricism and constructivism; c) unlike constructivism, constructive empiricism cannot, in a broad sense, be understood as a conception about scientific practice (although it may be in a weak sense – but again it is also a weak evidence for establishing the relationship between the two philosophies).

In the third section, I argue that, although Kukla is not thoroughly wrong in placing van Fraassen and Latour in the same anti-realist tradition, sometimes it seems quite the opposite: Latour and realists can share some theses about science.

At "concluding remarks", I shall develop the third section and try to show that Latour is closer to realism than to anti-realism.

André Kukla and the Relationship between Constructivism and Constructive Empiricism

The first relationship put by Kukla is that van Fraassen's observable/unobservable distinction may be found in Latour, too (in a paper of him with Michel Callon) (Kukla 2000, 60-61).

Among other reasons, van Fraassen's constructive empiricism is well-known because his deeply explored distinction between observable and unobservable entities (van Fraassen 1980, p. 15). This distinction, however, does not have a pragmatic dimension – since, for a scientific explanation, theories (when necessary) must use such unobservable entities (van Fraassen 1980, 53-54) –, but only an epistemological one: unlike propositions concerning to observable entities, we cannot believe in the truth of propositions regarding unobservable entities (1980, 53-54). Thus, from an epistemological point of view, propositions about observables are privileged over those about unobservable.

According to Kukla (2000, 60-61), Latour, although had not argued for a distinction between more privileged and less privileged statements in a scientific theory, actually suggested a distinction between observables and unobservable which can be found in a short statement in a paper with Michel Callon. Kukla, however, did not make clear to the reader the context in which this distinction emerged in that paper.

The paper referred by Kukla is a reply from Latour and Callon to Harry Collins and Steve Yearley about a metamethodological issue on the sociology of science's methodology. According to Latour and Callon (1992, 350-351), Collins and Yearley's model determines that both nature and society are causes of the stabilization of a scientific production. However, according to Latour and Callon, natural objects (nature) and social movements (society) are unobservable. To Latour and Callon when it comes to observable these are "(…) traces left by objects, arguments, skills, and tokens circulating through the collective" (Latour e Callon 1992, 351). Then we see, by this quote, that indeed there is a distinction between observable and unobservable.

Nonetheless, the distinction promoted by van Fraassen concerns scientific statements like these ones: electrons are unobservable; the volume of a gas is observable, and so forth. Thus, van Fraassen deals with scientific statements. Latour and Callon, on the other hand, are not dealing (in the context of the referred paper) with scientific statements but with statements that compose the material of analysis of science that ought to be taken into account by sociologists of science. Therefore, the Latour/Callons's distinction has no bearing on any relationship with the one promoted by van Fraassen. Furthermore, it ought to keep in mind that, when Latour deals with scientific statements (and not with statements of sociological studies about science), the distinction just do not arise.

The second argument by Kukla is that the constructivist's emphasis on the social construction of facts might be (although it was not) undertaken by (and with no loss for) van Fraassen. Van Fraassen has devised a theory of explanation whose emphasis was on what he called "why-questions". Typically, a scientist asks why a particular phenomenon comes about (van Fraassen 1980, 134). Van Fraassen fairly sees that a scientist can ask a quite lot of questions about a phenomenon. However, this scientist chooses something particular for guiding investigation. This choice, according to van Fraassen, is set up by the researcher's context (van Fraassen 1980, 112). When a scientist is answering a why-question, he assumes that what ought to be explained is a genuine scientific issue. However, such an entitlement occurs also due to scientist's "desire" (van Fraassen 1980, 156) of explaining such issue. Thus, for example, explaining why metals gain weight in combustion used to be a legitimate issue while Antoine Lavoisier had been developing his chemistry system, but it was not scientifically sound in the qualitative chemistry of phlogiston; so, there were two different traditions who held likewise different "desires" for different kinds of explanation.

As Kukla points out, on Bruno Latour's constructivism there is a defence that the context of the production of scientific knowledge appoints (to some extent, at least) the very existence of what scientists assume to. Therefore, both van Fraassen and Latour understand that human action is fundamental to define a subject matter for scientific theories, because to explain is "something quite pragmatic" (van Fraassen 1980, p. 100): it is not sufficient only to take into account the relationship between theory and facts, but also the relationship between theory and facts and the context: "related to the concerns of the user of the theory and not something new about the correspondence between theory and fact" (van Fraassen 1980, 100).

It turns out, however, that "context" is a word that suggests something broader than meant by van Fraassen. Let us look at a hypothetical example (given by van Fraassen) and then I will give an example from the history of science.

Let us suppose the following question (van Fraassen 1980, 127): "why did Adam eat the apple?". To van Fraassen, different demands stem themselves from this issue. One can, from the original statement, asks the following three questions: Why did *Adam* eat the apple / Why did Adam *eat* the apple? / Why did Adam eat *the apple*? The use of italic types in these statements makes clear the precise shape of the demand of the question. At the first version, we wish to know why Adam (and not another person) ate the apple; so, an answer such as "because Adam was hungry" would not be a good answer. At the second version, Adam's own act (eating) is asked, and that possible answer now may be sound: "because he was starving". Finally, at the third, we want to understand why Adam ate that apple and not another fruit or food; so, this last answer must take into account, for example, Adam's taste for apples and not for another fruit or food.

It becomes clear that the first problem we face is to know which of the three versions of the issue we should choose. To van Fraassen, the answer is pretty clear: the context will determine the choice. Standing still in his example, Eve would choose the first version if she told Adam to keep the fruit for herself; she would choose the second one if she told Adam to store the fruit for dinner; and she would choose the third if she wanted to tell Adam that apples are damaging for him, and therefore it was better for him eating grapes. So, in a context of an everyday conversation like this one, when Eve asks Adam – "why did you eat the apple?" –, he must give the kind of answer expected by her.

Van Fraassen's suggestion is that the same is true, of course, in scientific practice. A scientific issue opens itself for so many possible answers, too – a myriad of investigative lines, as it were. Let us look at an example from the history of biology. Until near to the end of the 19th century, the scientific problem of explaining the cause of resemblance between parents and their offspring took into account issues linked to the relationship between parents and the environment (Stanford 2006, 62; Bowler 1989, 23, 58; Olby 1963, 251); that is: what parents did

during their life could get in on the explanation of why their offspring are as they are (soft heredity). Nevertheless, since the beginning of the 20th century the scientific problem had changed and it was assimilated in another context, the context of classical genetics. And, in this new context, it does not matter what parents do throughout their lives, but only their genetic heritage and what they can transmit of such fixed heritage to offspring (Bowler 1989, 3) (hard heredity). In other words, as van Fraassen put it, the context entails the direction of the research.

The problem, however, is that at nowhere van Fraassen links his concept of "context" to scientific contexts. Van Fraassen, as it were, had restricted himself to asserting the existence of a context, and he did not give any examples like this of the history of genetics, or, at least, examples taking in account a wider context.

A second problem is that an ordinary assertion of the existence of a context is highly weak to support Kukla's thesis. Actually, no one believes that any philosopher of science denies the very existence of a context. Larry Laudan (1977, 198) and Paul Thagard (1978, 92), for example, are assertive about the existence of a context that guides research. But, like van Fraassen, none of them (unlike Latour) explores historiographically the causal meaning of this context for the establishment of a scientific production. Thus, Kukla's claim that van Fraassen could accept the concept of social construction of facts is a weak thesis (and this could even be applied to many other philosophers).

Finally, the third relationship established by Kukla: both constructive empiricism and constructivism ought to be considered approaches about scientific practice (Kukla 2000, 63).

Whoever be the philosopher of science, the more abstract his approach may be, he or she thinks that it does not (albeit roughly) have a relationship with scientific practice. But what does "relationship with scientific practice" mean for a philosophical approach to science? It is not enough, for a philosophical approach, to bring forward historical examples; a philosophical approach that really deals with scientific practice ought to bravely interplay with historical records and to comprehend philosophical categories in order to explain these historical records. In other words: scientific practice must intervene in the philosopher's approach. Thus, a philosopher who deals with scientific practice and sustains the thesis (T) about a scientific theory should modify T if her approach concerning this theory does not match with the historical record. In this case, contrary to what Kukla argues, van Fraassen did exactly the opposite. Let us see how this happened.

According to many scientific realists, scientists proceed through what is called inference to the best explanation: in face of one phenomenon, and assuming the existence of some rival alternatives, the hypothesis that best explains this phenomenon gives a good support for the belief in truth of its own explanation (Harman 1965, 89). Van Fraassen has pinpointed several challenges to the inference to the best explanation, and the most famous is called "the argument from the bad lot" (Psillos 1996, 36-37) (also called "underconsideration argument" (Lipton 1993, 89-90)): did the lot of rival hypotheses include all possible hypotheses for the explanation of the phenomenon? We cannot know that it included; so, we can ask: the hypotheses chosen as the best explanation would not be out of the lot? Van Fraassen said: "it is possible"; so, it is not impossible that the hypothesis chosen as the best one could be picked out from a bad lot (van Fraassen 1989, 142-143).

Trying to rejoinder to van Fraassen it is a hopeless task. His framework is quite abstract and impose itself to whichever episode in the history of science. No matter how much scientific practice reveals through historical records, van Fraassen can still argue from the argument from the bad lot; even we would change the lot and would include new hypotheses, the lot still could be a bad one. This means that scientific practice, as prescribed in this paper, does not affect van Fraassen's philosophical approach.

Once introduced the three components of the relationship set forth by Kukla between van Fraassen and Latour, I understand that this relationship does not take place –at least not by means of the reasons put forward by Kukla.

It is useful to make clear Kukla is trying to set up such a relationship in order to place van Fraassen and Latour in the same anti-realist tradition. I believe that the relationship can be supported, although not by means of Kukla's conditions. However, it seems the opposite comes about: Latour and the realists can share some theses about science. The next section of this paper deals with this point.

Latour and Scientific Realism

After van Fraassen had launched his argument from the bad lot, realists replied to the argument. According to Psillos (1996, 39) and Lipton (1993, 99),[7] it is possible to rebut the argument from the following counterargument: as far as the scientists, while are formulating their hypotheses, usually take into account the background knowledge, then, the lot would not be bad, since it already excluded hypotheses which had not relate to some background knowledge.

It is not necessary to feel that scientific realism has a strong relationship with scientific practice in order to understand that its appeal to background knowledge is a bit closer to scientific practice than the argument from the bad lot: a scientist sets out hypotheses from their relationship with some background knowledge, which provides a constrain for the proliferation of hypotheses (a constrain which, as it has been seen, was not found in van Fraassen's critique of the inference to the best explanation). Furthermore, as Peter Lipton argues, background knowledge, for realists, ought to connect realism itself with scientific practice (Lipton 1993, 96-97).

This very idea of proliferation of new hypotheses due to background knowledge is quite homely to Latour. For him, scientists, when propose novelties, do not hesitate to connect such novelties with some consolidated knowledge, knowledge that as whole he encapsulates in the phrase "black box". Such a connection provides reliability and plausibility to the novelty that will be submitted to scientific community. Also, to Latour, as scientific statements do not stand on their own – but need the help of other statements –, the meaning of the concept of background knowledge is pretty clear: "A sentence may be made more of a fact or more of an artefact depending on how it is inserted into other sentences. By itself a given sentence is neither a fact nor a fiction: it is made so by others, later on" (Latour 1987, 24). So, in this case – the relationship of a philosophical approach to science with the scientific practice –, Latour seems quite fitted in a realist environment.

Moreover, Latour stresses the relationship with consolidated knowledge for the same reason above: statements are not analysed alone; for working out, they need a broader context: "By themselves, a statement, a piece of machinery, a process are lost. By looking only at them and at their internal properties, you cannot decide if they are true or false, efficient or wasteful, costly or cheap, strong or frail. These characteristics are only gained through incorporation into other statements, processes and pieces of machinery. These incorporations are decided by each of us (…)" (Latour 1987, 29).

Winding up this section, let us recall the realists' interpretation of constructivism: either science is explained only by epistemological criteria or is explained only by social factors; it cannot be explained only by social factors, since, if science is explained only by social factors, then our scientific productions are not convergences to reality, but merely constructions made by groups

[7] See also Boyd (1985, 6-7; 1991, 206), Leplin (1997, 116) and Giere (1999, 193).

of people who rule the scientific world; therefore, science ought to be explained only by epistemological criteria.

In his book *Science in Action*, Latour has addressed the methodological issue of how about understanding the emergence of a scientific production at the end of some controversy. From the historical record of science, Latour, after had developed each stage of his argument, launched one methodological rule for the understanding of scientific productions. Rule 3 lay down the following guideline: after a controversy had been closed, it could not be said that nature is the cause of such closure; in fact, what is called "nature" is the consequence of a closure of the controversy.

Well, if nature is not the cause, who might play its role? In the realist interpretation about constructivism, society replaces nature. However, after had introduced Rule 3, Latour proposed another line of argument, this time to show, first, that nature is not the cause, and, second, by means for another rule, Rule 4, that society cannot play the role of nature as the cause. Rule 4, in fact, has the same words as Rule 3, except that, at Rule 3, Latour uses "nature"; instead, at Rule 4, Latour uses "society". The upshot is quite clear: neither society is the cause, nor nature is the cause (of the closure of a controversy), as long as scientists do not know deeply nature, but they also do not know deeply social context. So, one cannot speak of only social factors as causes of scientific productions (Latour, 1987, 142).

That is why Latour triggers Rule 5: the actual understanding of the closure of a controversy – a closure that peaks with the "stabilization" of some knowledge[8] (Latour, 1987, p. 42) – comes about only if we follow what scientists actually do: "Our fifth rule of method will thus be the following: (…) we should follow the two sides [of controversy] simultaneously, making up a list, no matter how long and heterogeneous, of all those who do the work" (Latour 1987, 176). And, as far as we follow two rivals in a controversy, we will see them both performing politically and socially and working in their labs.

Closing Remarks

Perhaps due the label "constructivist", it hardly ever takes up in the literature that Latour sometimes argues for a kind of scientific realism in his analysis of science.[9]

According to Latour, there are two stances about science that can be assumed by the philosopher/sociologist of science: either one can look at "science in action" (Latour 1987, 15) or can look to "final products" (Latour 1987, 21). On science under construction, scientists, according to Latour, joined themselves in order to put their knowledge beyond any doubts (Latour 1987, 142). At this moment, the philosopher/sociologist of science should be relativist, or else there not will be an understanding the complexities tackled by scientists. However, once some knowledge has consolidated itself (that is, it has become itself a "black box") the philosopher/sociologist of science must be realist, or else there not will be the acceptance the outcomes of science: "This division between relativists and realist interpretation of science has caused analysts of science to be put off balance. Either they went on being relativists even about the settled parts of science – which made them look ludicrous; or they continued being realists even about the warm uncertain parts - and they made fools of themselves. (…) We do not try to undermine the solidity of the

[8] On this concept, see also Pickering (1990), especially page 222, and Lenoir (1999), especially the chapter "The Discipline of Nature and Nature of Disciplines" and specifically page 47.

[9] There is a paper by Tim Lewens (2005) (although he has not used Latour as a reference) that is very close of direction which I am suggesting in these concluding remarks. Lewens argues that the truth of scientific theories could be explained both epistemologically and by social and political factors (Lewens 2005, 562), since fulfilled some conditions (Lewens 2005, 567-568).

accepted parts of science. We are realists (…). But as soon as a controversy starts we become as relativist (…)" (Latour 1987, 100).

Thus, I finish this paper with two conclusions. First, the relationship argued by Kukla between van Fraassen and Latour – a relationship introduced to place both in the anti-realist tradition – does not take place, or at least not for the reasons provided by Kukla. Second, conversely, there is a relationship between Latour and the realists.

As I have put at the abstract, Kukla tried to find three parallels between van Fraassen and Latour: i) the observable/unobservable distinction in Bas van Fraassen's constructive empiricism may be found at Bruno Latour's constructivism, too; ii) the constructivist's emphasis over the social construction of facts could be (although it was not) acknowledged by van Fraassen; iii) both constructive empiricism and constructivism are approaches about scientific practice.

I hope I have established Kukla's claims do not support those parallels, since i) the term "observable" is employed in one sense by van Fraassen and another one by Latour; ii) even tough van Fraassen does not prevent a talk of social factors, he has never carried on such a talk; iii) unless we accept that every philosopher of science deals with scientific practice (because they analyse science), so van Fraassen would be a philosopher who would deal with scientific practice; but, as we have seen, handling with scientific practice demands one approach which we cannot find in van Fraassen's work

But even tough, in this paper, I have been criticizing Kukla's approach, I would like to highlight I recognize that, in the first place, his book is a remarkable contribution both to change the traditional realist interpretation about constructivism and to weak this traditional but wrong interpretation. Also, in the second place, Kukla, at several parts of his book, cleared up other subject matters concerning constructivism.

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