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Larry Laudan's Critiques Regarding Social Constructivism

As críticas de Laudan ao socioconstrutivismo

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Abstract

Social constructivism can be taken as a broad sociological-philosophical approach of science which is introduced as follows: i) an explanation of the acceptance of successful scientific achievements ought to be done through the use of epistemological criteria and social community factors; ii) the knowledge produced by successful scientific achievements cannot be taken as a representation of reality. The social constructivist approach has always been strongly criticized by traditional philosophical conceptions of science because 1) an explanation of the success of successful scientific theories should be made only through the use of epistemological criteria, and 2) the knowledge produced by successful scientific achievements matchs to reality. This paper deals with the first criticism, by means of an expanded analysis of the debate between a representative of social constructivism (David Bloor) and a critic (Larry Laudan), based on the hypothesis that, although Laudan has pointed out some important limits to the scope of the social constructivist approach, the argumentative core of his critique does not hold itself, except as a prescription about the tasks that a philosopher of science must do.

Keywords: Social Constructivism. David Bloor. Larry Laudan. Symmetry.

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Resumo

O socioconstrutivismo é uma ampla concepção sociológico-filosófica da ciência que pode ser assim apresentado: i) uma explicação da aceitação de realizações científicas bem-sucedidas deve ser feita por meio do emprego de critérios epistemológicos e de fatores sociocomunitários; ii) o conhecimento produzido pelas realizações científicas bem-sucedidas não é uma representação da realidade. A concepção socioconstrutivista sempre foi fortemente criticada pelas concepções filosóficas tradicional de ciência pois 1) uma explicação do sucesso das teorias científicas bem-sucedidas deve ser feita por meio apenas do emprego de critérios epistemológicos; e 2) o conhecimento produzido pelas realizações científicas bem-sucedidas, para ser considerado efetivamente um conhecimento, deve ser uma representação da realidade. Este artigo trata da primeira crítica, por meio de uma análise ampliada do debate entre um representante do socioconstrutivismo (David Bloor) e um crítico (Larry Laudan) a partir da hipótese de que, embora Laudan tenha apontado alguns limites importantes do alcance da concepção socioconstrutivista, o núcleo argumentativo de sua crítica não se sustenta, exceto como uma prescrição sobre as tarefas que devem ser realizadas por um filósofo da ciência.

Palavras-chave: *Socioconstrutivismo. David Bloor. Larry Laudan. Princípio da simetria.*

Introduction

Social constructivism has arisen at philosophical community in 1976, originally named “Strong Program in Sociology of Knowledge”; it is a broad sociological-philosophical approach of science that, despite its breadth, can schematically be introduced as supporting the following theses: i) an account about why some successful scientific achievements are accepted must employ both epistemological criteria (coherence, relationship with background knowledge, empirical adequacy and so on) and social community factors; ii) the knowledge manufactured by successful scientific achievements is not a representation of reality at all – rather, it should be seen as a construction which is accountable for epistemological criteria and social community occurrences.

The social constructivist approach has always been strongly criticized by traditional philosophical conceptions of science[1]. Following what I said above, the criticisms are as follows: 1) an explanation of the success of some scientific achievements must be given only through the use of epistemological criteria; 2) for something deserving to be considered a knowledge it must be a representation of reality.

The second criticism aims to show that social constructivism is either a theoretical impossibility or highly implausible. It would be *theoretically impossible* because it would have a logically unacceptable conception of construction: if scientific claims are constructed, then social constructivist claims are also constructed; thus, social constructivism, as a conception of science, is nothing more than a theoretical impossibility (Leplin, 1997, p. 5; Readhed, 1997). On the other hand, it would be an *implausible* approach from its very conception of construction: if scientific statements are constructed, then the reality would play no role in accounting for the success of scientific theories. In this paper I will not discuss that second criticism.

This paper concerns the first critique (an explanation of the success of some scientific achievements must be given only through the use of epistemological criteria). Larry Laudan’s “The Pseudo-Science of Science?”, from 1981, is still both classic and unbeatable paper about this issue. This paper is a specific critique of David Bloor’s book *Knowledge and Social Imagery*, from 1976, which has introduced, in a groundbreaking way, general guidelines of the social constructivist approach (which Bloor has named as “Strong Programme in Sociology of Knowledge”[2]). Laudan's paper is both classic and unbeatable because, despite all the later development of social constructivism, his criticisms reamis as a fundamental starting point for other critics of social constructivism. In other words: Laudan still leads some dimensions of the debate between traditional philosophers of science and social constructivists.

The first criticism – an explanation of the success of some scientific achievements must be given only through the use of epistemological criteria – is the argumentative core of Laudan's paper; however, this core entails other critical thoughts on social constructivism.

Laudan's paper was replied by David Bloor himself in 1984, and, here, I deal at the first section with Laudan's criticisms, Bloor's responses, and my own assessment of the arguments. I would like to make it clear to the reader that this section introduces Laudan's and Bloor's arguments by a list; that is: an explanatory synthesis that unifies these arguments will not be presented. The explanation for this

[1] By “traditional philosophical conceptions of science” I mean an affiliation to classical philosophical trends in the philosophy of science. The core idea is that an analysis of scientific knowledge should take place only using epistemological criteria: the relationship between scientific productions (theories, hypotheses and so on) and reality.

[2] Over time, there was a replacement of the phrase “Strong Program in Sociology of Knowledge” by the more current name “social constructivism”.

strategy is quite plain: the theoretical unification of the whole Laudan/Bloor discussion is located exactly in the first criticism, which will be widely addressed in this paper.

An evaluating of the Laudan/Bloor debate can remain in the debate itself; however, from the point of view of an understanding of social constructivism, the debate is worth of exceeding the pioneering Laudan/Bloor considerations (in other words: the debate is still a current one). Therefore, my strategy, at the second section, is to emply the Laudan/Bloor debate as a starting point to understand similar debates that are more current but which nevertheless have an affiliation with the Laudan/Bloor debate itself. For doing that I will examine the Laudan-Bloor debate from the following hypothesis (using some inputs from more current social constructivist philosophers): although Laudan actually has pointed out some essencial limits to the reach of the social constructivist approach, the argumentative core of his critique does not hold up, except as a prescription (completely disregarded by social constructivists) concerning the tasks that a philosopher of science must perform.

However, even I will conclude Laudan's argumentative core is not able for disturbing social constructivism, my aim is less to criticize Laudan (and the critics of social constructivism grounded on Laudan's thoughts) than to show that the social constructivism, by responding to first critique, shows its conceptual virtues – which, even if they do not compel anyone to support the ideia that the social constructivism is the best approach of science, they are an interesting contribution to the philosophy of science, despite the existence of a problem that will have been pointed out in the "Concluding remarks" of this paper.

Before starting, it is important to note that this paper is not intended as a defense of social constructivism, but an effort to clarify a significant part of the debate on social constructivism and the relevance (albeit not the superiority) of this approach for an understanding of science.

Laudan's cricitisms and Bloor's reply

Laudan's main criticism of social constructivism is that an explanation of the acceptance of some successful scientific achievements must be given only through the use of epistemological criteria. I will go back to that criticism throughout the paper. Before, however, evaluating Laudan's main criticism, I need to show other significant points of his paper. I will present Laudan's criticisms in the form of a list (all points at the list will be discussed in the next section, even the ones I consider Laudan's criticism are propor ones).

a) Social constructivism does not make clear its causal explanatory mechanisms, in the usual way we see in traditional philosophical accounts of science (Laudan, 1981, p. 174, p. 185). Bloor replies that the absence of explanatory causal mechanisms is accounted by the fact that traditional philosophical categories (in his own example, simplicity) are controversial ones (Bloor, 1984, pp. 77-78).

Laudan's criticism is appropriate. If a philosopher of science employs any philosophical concept – for instance: "background knowledge" –, this is done by showing how the concept itself would causally explain its relevance to the acceptance of some knowledge. Social constructivists effectively do not make clear their causal explanatory mechanisms.

Furthermore, whether controversial or not, philosophical categories were (and still are) quite fundamental for understanding scientific dynamics. What is more: social constructivists, even when do not refer themselves philosophical categories in their narratives, actually stand them in their works.

For instance: by means of Paul Thagard's concept of simplicity (1978, pp. 86-89) – given that simplicity is measured by the smallest number of *ad hoc* hypotheses introduced by a theory -, let us take the example of the rise of classical genetics in a social constructivist narrative (which was made by Peter Bowler). The assumption, by early geneticists, that genetics ought to be a discipline that would only deal with the transmission of characters (and not with the development of organisms), has allowed geneticists themselves to avoid facing issues which would require several *ad hoc* hypotheses, which in turn would make the genetics much more complex (Bowler, 1989, p. 131). That is: the social constructivist (Peter Bowler) has made use of the concept of simplicity, but did not inform us about that (This example can be found at several writings of social constructivists.).

b) Social constructivism is not even a sociological approach, but just a range of some principles; even worse: as a genuine sociological approach, it does not explain social processes (Laudan, 1981, pp. 174-175). Bloor replies to Laudan's criticism saying that social constructivism explains social processes; he mentions social constructivist analisys of the controversy between Louis Pasteur and Félix Pouchet (Bloor, 1984, p. 79).

From a descriptive point a view Laudan is again absolutely right; Bloor's sociology of knowledge does not have an auxiliary theory that deals with society. But the example given by Bloor indeed shows social constructivists’ concerns with social processes. Pasteur was privileged by social relations that were favorable to him, such as being sympathetic to the current political regime and belonging to the ruling scientific elite (Collins; Pinch, 2000, p. 87).

Bloor said nothing concerning Laudan's criticism that social constructivists do not make clear their explanatory mechanisms. But I am able to provide an answer. Social constructivist really relies on its narratives and not on an exposition of its concepts; however, this strategy derives from social constructivist style, not from its content, even because, as I already pointed out in (a), social constructivists do need concepts from the traditional philosophy of science in order to explain science.

c) Social constructivistism is a conception that deals only with scientific mistakes and not with success; acknowledging success would amount to take science as a sacred culture, and this is not accepted by social constructivists (Laudan, 1981, p. 175, p. 177, p. 178, p. 182); moreover, Laudan adds: if science is not a sacred culture, how could social constructivist principles be? (Laudan, 1981, p. 182). For Bloor, Laudan misses the point of the discussion (Bloor, 1984, p. 83).

At this stage, the debate becomes confusing itself. Bloor just does not explain why Laudan misses the point. Nonetheless, it is possible understanding Bloor: he has voiced four major tenets of social constructivism – the fourth one is called “reflexivity”: the standards of explanation used by sociologists of science to evaluate science also should be applied to the sociology of science itself (Bloor, 1976, pp. 4-5). So, applying the tenet of reflexivity, neither science is sacred, nor are social constructivist studies.

d) Social constructivism grounds itself on an old “skeptical” strategy – the thesis of underdetermination of theory by data: if there are at least two differents theories which explain the same evidence, the choice for one of them would not made by epistemological criteria, but instead by social factors (Laudan, 1981, p. 196). This issue will be addressed at the next section; for now, it is sufficient to say Bloor has realized that Laudan assumed that underdetermined theories are constructed outside of a social environment (Bloor, 1984, pp. 78-79). That is: Bloor rightly points out the interrelationship between theoretical construction and social factors; they are not, as Laudan thinks, independent between themselves. It is important to quote Bloor here (1984, p.79):

> It is necessary to introduce some process such as socialization into a tradition of normal science in order to explain the constraints which limit the acceptable interpretations which can be put on the facts of experience. And If this applies to the circumstances which lead up to a crucial experiment, it applies equally to the decisions that are made about its outcome.

e) Laudan grants the very existence of social factors (Laudan, 1981, p. 189). Bloor's answer is that very existence is not just an existence, but a way of community interaction among scientists in order to expand their scientific knowledge, their "techniques and methods" (Bloor, 1984, p. 80).

At this stage, Laudan splits the issues. He admits that scientists "are educated and socialized into a certain community and they address their publications to their peers" (Laudan, 1981, p. 196); but this socialization has nothing to do with the knowledge which is produced by science. But I ask, from Bloor: how could someone submit publications to their peers if there was no journal at all? It is not the very existence of a journal a part of science as a whole? (The question about journals could either be applied to other items or be generalized: how would there be science without scientific institutions?)

f) Laudan points out what he calls "fallacy of partial description" (Laudan, 1981, p. 194): science has several dimensions (psychological, economic and so on), but a psychology or economics of science does not follow from this (there is only philosophy of science). The fallacy of partial description is answered by Bloor: an understanding of science cannot dispense any available theoretical resource (Bloor, 1984, p. 75).

That issue is a historical-institutional one. The sociology of science has been able to understand science – economy and psychology have not (although this does not mean that insights from both areas sometimes cannot be useful). Thus, Laudan is not formally (and only formally) right in introducing his analogical argument: as other (than philosophical) forms of knowledge cannot help, neither can the sociology of science; on the other hand, Bloor uses innapropriate examples (biology and psychology). Be that as it may, and against Laudan, the sociology of science effectively has enabled itself to be a way of understanding science[3]. But, against Bloor, we can noitice that, even if science is set by several dimensions (psychological, economic and so forth), a psychology or economics of science really did not arise (instead, a philosophy of science did).

g) Finally, we face the first criticism – an explanation of the success of some scientific achievements must be given only through the use of epistemological criteria; Laudan states that, for sociologists of science, "only" (Laudan, 1981, p. 173) through sociology of science an understanding of science would be possible. Bloor's answer is that there is a transmission from what is observed in the behavior of scientists to a conception of science (Bloor, 1984, p. 83). I will return to this issue at the next section.

Evaluating the debate from the development of social constructivism

An evaluating of the Laudan/Bloor debate can remain inside the debate itself; however, from the point of view of an understanding of social constructivism, the debate is worth of exceeding the pioneering Laudan/Bloor considerations (in other words: the debate is still a current one). Therefore, my strategy, from now on, is to emply the Laudan/Bloor debate as a starting point to understand similar debates that are more current but which nevertheless have an affiliation with the Laudan/Bloor debate itself. And, from now on, I will focus on an evaluation of the debate, following the list already presented at the previou section, but also from conceptual contribution given by other philosophers who have developed the social constructivist program.

a) Social constructivism does not make clear its causal explanatory mechanisms, in the usual way we see in traditional philosophical accounts of science (Laudan, 1981, p. 174, p. 185).

As I have already pointed out, Laudan's criticism is a meaningful one and, what is more: it is still current. Social constructivism does not lack concepts: "black box" (Latour, 1987, p. 2), "stabilization" (Lenoir, 1997, p. 47; Latour, 1987, p. 42); "reciprocal coordination" (Smith, 1997, chap. 8); "scientific

[3] But it must be noted: at the time Laudan critized sociology of science (1981), that approach was not well established.

community” (Knorr-Cetina, 1981, pp. 68-69), “modality” (Latour, 1987, p. 22). Be that as it may, such concepts are not articulated in the way they are into traditional approaches of science.

b) Social constructivism is not even a sociological approach, but just a range of some principles; even worse: as a genuine sociological approach, it does not explain social processes (Laudan, 1981, pp. 174-175).

The development of social constructivism has made it clear that it is not a theory only about social processes; rather, it concerns an explanation of scientific success *also* through the use of social factors. That is: it is not about construct a social theory and then understanding what kind of science could be produced within some society. As Bruno Latour has persuasively argued, if it is true that scientists do not know nature in detail (to use the vocabulary of classical philosophy since Hume), it is also true that scientists also do not know society in detail (Latour, 1987, p. 142). Thus, for a social constructivist, the knowledge we can have of society does not precede the knowledge we can have about nature.

c) Social constructivistism is a conception that deals only with scientific mistakes and not with success (Laudan, 1981, p. 175, p. 177, p. 178, p. 182).

Bloor remains very helpful in responding to Laudan here. The most famous of Bloor's four tenets of social constructivism is called the “principle of symmetry”: the same kind of causes explain both success and failure (Bloor, 1976, p. 5). I need to separate Laudan's objection into two parts.

Firstly, Laudan, in a previous text, from 1977, had granted the relevance of social constructivism, but only as an analysis of social and economic factors that there are in included on science (Laudan, 1977, p. 198, p. 208); however, these factors should not be taken to an account for an understanding of the cognitive content of science. Laudan’s thought is like that: when a theory is a successful one, and it can be explained only in terms of epistemological criteria, why would we need to worry about social and economic factors? But this point brings us to the second part of the issue – which, in turn, needs to be subdivided into two points.

The first point concerns Laudan’s mistake about categories: saying that the same kind of causes explain both success and failure entails only *sameness of kinds* and not *sameness of causes*. As the non-constructivist Tim Lewens had said by (2005, p. 563:

> Engineers do not give just the same explanations for why bridges stay up as for why they fall down, and machines that work may need different explanations from machines that fail. So it doesn’t follow that the same explanations [...] for true beliefs [and] for false ones [...]. But Bloor is mak[ing] the point that the same family of explanatory concepts should work to explain all kinds of belief formation. Roughly the same kinds of concepts—concepts of force, for example—explain why bridges stand up and why they fail, but characteristic patterns of force may explain failure.

Indeed, the second point is more relevant to the debate. There is (albeit less and less), at dealing of a scientific controversy, a historiographical habit of lauching social and economic factors for accounting for why the winner has won the controvery – but by the example of the spontaneous generation controversy we clearly see that this is not the case. As the historian Gerald Geison reminds us, if it is true that Pasteur was effectively privileged from a sociopolitical point of view, it is also true that i) such privilege did not come only through a raw process of “negotiation”, but by Pasteur’s performance in the fields of industry and agriculture (Latour, 1999, p. 169), and ii) the defeated, Félix Pouchet, also sought to establish alliances which would reach people beyond community science (GEISON, 1995, p. 112). The only difference between Pasteur and Pouchet is that the former has succeeded in his associations, while the latter has not. The processes, however, were the same.

d) Social constructivism grounds itself on an old "skeptical" strategy – the thesis of underdetermination of theory by data: if there are at least two differents theories which explain the same evidence, the choice for one of them would not made by epistemological criteria, but instead by means of social factors (Laudan, 1981, p. 196).

When it is assumed the thesis of underdetermination of theory by data, social factors would be like tiebreakers for defining a winner in some controversy. The controversy between Pasteur and Pouchet has concerned a specific point: either are the microorganisms that emerge from apparently sterile cultures explained by some physical event or are they the output of any spontaneous generation (Debré, 1994, p. 158)? From an empirical point of view, in fact, the dispute was not solved. However, Pasteur widely changed the terms of the dispute. At the begin, he has shown that cultures are not always completely sterile when temperature conditions are alterd. Second, when Pasteur himself entered into the dispute, his studies on fermentation had already been at an advanced stage, and he related such studies to the controversy over spontaneous generation (Geison, 1995, p. 108), since, if what he discovered about fermentation were valid, there could be no spontaneous generation (Latour, 1999, pp. 163-164). Thus, even if one accepts the thesis of the underdetermination of theory by the data, it is an artificial thesis which does not grasp actual science. What is more: social constructivists do not use social factors as tiebrakers.

e) Laudan grants the very existence of social factors (Laudan, 1981, p. 189). For Laudan, the sociology of science has a merely descriptive role – and this would be its disciplinary function. His point is normative-disciplinary: he dictates what an analyst of science can do: philosophers of science explain the cognitive content of science; sociologists of science explain the social factors that there are in a scientific achievement. What is more: for Laudan, the two tasks are independent between themselves and should not be connected. For answering Laudan, it is enough to rewrite Bloor's answer I put at previous section: the existence of social factors is not just an existence, but a way of community interaction among scientists in order to expand their scientific knowledge, their "techniques and methods" (Bloor, 1984, p. 80).

f) Laudan points out what he calls "fallacy of partial description" (Laudan, 1981, p. 194): science has several dimensions (psychological, economic and so on), but a psychology or economics of science does not follow from this (there is only philosophy of science). Again, it is enough using Bloor to answer Laudan: an understanding of science cannot dispense any available theoretical resource (Bloor, 1984, p. 75). Nonetheless I allow myself to develop Bloor's answer.

Why would we not have, in the future, a biology or economics of science? Scientific disciplines, like the philosophy of science itself, are historical constructions and not institutional resolutions. The philosophy of science, although it has been based on several studies of science over the centuries, only became an independent discipline in philosophy from the Vienna Circle. By the way, it is quite striking that Laudan himself, having argued that all scientific theories are historical products (Laudan, 1977, ch. 1, 2 and 3), did not extend his historical thesis to philosophy itself.

g) Finally, we face the first criticism – an explanation of the success of some scientific achievements must be given only through the use of epistemological criteria. Laudan states that, for sociologists of science, "only" (Laudan, 1981, p. 173) through sociology of science an understanding of science would be possible. Actually, (g) is the key point of Laudan's criticism, the key point of current criticisms, and also the key point for the purpose of this paper.

Laudan's argument roots itself from a false disjunction, and a one which appears in several analysis of social constructivism: either science is explained only by epistemological criteria or it is explained only by social factors; science cannot be explained by social factors alone; therefore, it must be explained only by epistemological criteria (Nola, 2014, pp. 297-298; Niiniluoto, 1991, p. 152; Nelson, 1994, p. 541). I can give three replies to Laudan.

The first answer emerges from social constructivist literature itself: social constructivist statements are assertive in regard the importance of both epistemological criteria and social factors (Bloor, 2009, pp. 4-5). Bloor, for example, states: "[...] does the acceptance of a theory by a social group make it true? The only answer that can be given is that it does not" (1976, p. 38); Isabelle Stengers argues that political, economic and industrial factors are insufficient to support a hypothesis that claims to be scientific (Stengers, 2000, p. 104).

Second, beyond of social constructivists statements, it is important to look at the historical explanations given by social constructivists for historical episodes – and, inside those explanations, we again will find a blend of epistemological criteria and social factors (Pickering, 1990; Bowler, 1989; Berry, 2014).

Finally, a fine analysis of the three above works shows that there is not even a hierarchy whereby social factors would be more important than epistemological criteria. As an example, we can see Peter Bowler, explaining the rise of classical genetics by means of both epistemological criteria and social factors (Bowler, 1989, p. 8):

> Good PR work may not be able to self a bad theory, but a potentially good one may find its acceptance blocked if its proponents cannot play the game of scientific politics. They must adopt a workable strategy for converting others, undermining the influence of opponents, gaining access to journals and research grants, and all the other activities required to ensure an expanding role within the scientific community.

Thus, social constructivists do not explain science only by an appeal to social factors. First criticism – an explanation of the success of some scientific achievements must be given only through the use of epistemological criteria – just does not hold itself.

Concluding remarks

Socioconstructivism is one approach (among many ones, such as Laudan's) that can help us to understand science. It is not the best approach, but just an alternative. Be that as it may, Laudan's criticism of the fallacy of partial description (f) from my list of criticisms) deserves further attention.

Taking for granted the existence of other factors beyond epistemological criteria, which factors would these be? Socioconstructivist are not able to build a general conception along the lines of traditional conceptions of the philosophy of science. The social fact that Gregor Mendel was born in Brno in no way changes the reception of his work of pea breedings; the community scientific fact that there was not a genetic approach to evaluate Mendel's experiments did. Therefore, there is no general rule.

And the lack of a general rule is indeed a philosophical problem for social constructivists. It is arguable whether they themselves see that as a problem. I think they should consider the lack of a general rule as a genuine philosophical problem.

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