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Social Constructivism and Methodology of Science

Abstract

Scientific practice is a type of social practice, and every enterprise of knowledge in general exhibits important social dimensions. But should the fact that scientific practice is born out of and tied to the collaborative efforts of the members of a social group be taken to affect the products of these practices as well? In this paper, I will try in to give an affirmative answer to this question. My strategy will be to argue that the aim of science is partially determined by a socio-historical context and that this aim, together with the available background knowledge, stands behind a methodology that is responsible for empirically and aim-adquate theoretical results.

Keywords

social constructivism, the aim of science, scientific realism, the commercialization of research, scientific methodology

1. Values, interests and social constructions

Scientific practice is a social practice, and every enterprise of knowledge in general exhibits important social dimensions. But should the fact that the scientific practice is born out of and tied to the collaborative efforts of the members of a social group be taken to affect the products of these practices as well? Should we understand the content of science as being dependent and tied to the contingent facts about the social group in which it is produced? Those who believe that we should do this usually opt for replacing

“... the view that observation and experiment play the dominant role in the specification of scientific facts with the view that these processes involve collective negotiations, interests and the infusion of experimental outcomes with contingent features of situations.” (Knorr-Cetina 1993, p. 556)

Thus, they opt for a social account of the content of science. These are the social constructivists.

1.1. A social constructivism interlude

According to social constructivists, science should be understood as embedded within a certain socio-cultural context that determines its features. Regarding science this way usually involves the following considerations:

- A. *Metaphysical considerations* – the objects of science are not to be found in nature, but are built by the scientific community (i.e. by a group of people organized in a certain way and pursuing a certain practice), and exhibit particular values, interests and needs that their

creators had at the moment of their creation. This means that “if scientists had chosen to confer facthood otherwise than they actually did, then subsequent history would reflect this in a world-view consistent with the choice they counterfactually made” (Nelson 1994, p. 541).

- B. *Epistemological considerations* – the abovementioned metaphysical position goes well with a social dependence conception of knowledge, according to which knowledge is relative and should be analyzed in terms of the historical and cultural contexts that the knowledge producer occupies. Since there are no context-free or super-cultural vantage points from which to evaluate different ways of knowing the world, we cannot do better but to take them as equally valid. “First-world science is one science among many; by claiming to be more it chases to be an instrument of research and turns into a (political) pressure group.” (Feyerabend 1993, p. 3)

The idea behind these two types of considerations is that the connection between society and our knowledge of the world should not be treated lightly. Social and cultural contexts are considerably more important than it was previously presupposed. Social constructivists push this relation beyond, by now trivial, common acceptance that science has a social dimension, i.e. that it is a social practice pursued collaboratively by members of a social group which can be influenced in their work by certain economic, political or social values. This influence can explain, for example, why a certain scientific community is more interested in certain problems than others or why a scientist formulated a certain theoretical conjecture. What the constructivists are claiming is not that these values influence the practice of science, but that they are affecting (to be read constructing) the very content of science.

Writing about the instrumental manufacture of knowledge in the lab, K. Knorr-Cetina, for example, argues that

“... the products of science are contextually specific instructions which bear the mark of the situational contingency and interest structure of the processes by which they are generated, and which cannot be adequately understood without an analysis of their construction.” (Knorr-Cetina 1981, p. 5)

Thus, scientific objects and/or scientific knowledge are constructed. But what does it mean to say that something is socially constructed? What do the constructivists have in mind when they are claiming that science is socially constructed? The first answer that comes to our mind is that science is the product of social practices. This can be understood in two ways. First, we can understand this dependence in terms of social requirements necessary for science. Science cannot obtain in isolation. It needs collaborative efforts of the members of a social group living in a society with a certain structure, and in the context of certain political and economic conditions. Since these are necessary conditions for the appearance of science, we can say that science is a social construction. But this is trivial and in agreement with the classical view about science: it amounts to nothing more than saying about scientific practice that it is socially constituted.

Second of all, we can understand this claim in contrast with the idea that something is discovered. When the social constructivists are saying that science is a constructive activity, what they have in mind is that the work of scientists actually brings into existence their domain of study and that their creative activity is guided and determined by the social, economic, political values, needs, and interests.

To better understand what the constructivists have in mind when they speak of social constructions, let's look at the following example: gendered individuals are social constructions. To say that gender is a social construction is to say that our society constructed the concept of gender and made gender classifications based on this concept. Subsequently, those to whom this classification was forced upon started to behave in accordance with the classification, exhibiting the very properties that the classification attributed them. This results in a type of thing that wouldn't have existed in the absence of certain social creation processes, or if the values or interests that stood behind these processes were different. Two things should be noted about these products of social practices: they are context dependent and value-interest¹ impregnated. They are context dependent because they would not have existed under different (social) circumstances. They are value-interest impregnated in the sense that they are analyzable in terms of the specific values and interests that stood behind and determined their apparition.

As our example shows, it is relatively easy to see how this view about social construction could work in theories about gender, race, sexuality, emotions, class, etc. Things get really complicated, though, when we try to apply this view to science, and say that the scientific objects are social constructions.

We will not pursue further the issue of the way we can reconceptualize the objects of science as social constructions because this will distract us too much from our aim. Suffice to say that there are several accounts of the construction processes in the literature and that what they have in common is that they attribute to the objects resulted as products of the construction the two characteristics noted above: context dependence and value-interest impregnation.

The common picture is that the processes in which scientific objects are generated can be spelled out in terms of negotiations and decisions, and behind this we can find social values, interests and needs. Products of science are decision-laden and these decisions are in turn value/interest-laden. This brings to our attention the following problem: the role of values in science. An interesting way to look at the social-constructivism issue is to approach it by looking to see if values play such a crucial role in science.

1.2. The role of values in science

Classical view about the role of values in science is categorical: science is value-free. This can be found already in Galileo's discussion about "the facts of Nature, which remain death and inexorable to our wishes" (Galileo 1957, p. 270), but it is mastered by logical empiricists who contrasted scientific judgments with value judgments which represent subjective phenomena not open to rational appraisal and thus lacking truth value, and, moreso, are not assertable in a meaningful statement at all (Carnap 1959, p. 77). Our scientific decisions are not value-laden from this perspective:

"The grounds on which scientific hypothesis are accepted or rejected are provided by empirical evidence, which may include observational findings as well as previously established laws and theories, but surely no value-judgments." (Hempel 1965, p. 91)

¹ I use value and interest here somewhat interchangeably. This will allow me to recast, in the next section, at least a part of the social constructivism issue in terms of the problem of values in science. The idea here is that we

value what we are interested in and we are interested in those things that we value the most. So, we can take interests as being nothing more than a way of thinking about the way we pursue values.

Values can figure only in the context of discovery, but the part of scientific practice that is epistemologically relevant is the context of justification. From this perspective, science is a neutral, impartial and autonomous discipline whose aim is to represent the world as it is without contaminating it with our preferences, needs and interests. As it is well known, this view came under a powerful attack. First, the distinction between fact and value was rejected starting from Quine's work (in particular his holism).² Secondly, Kuhn emphasized the pragmatic factors in theory choice.

How does this relate to our previous discussion about social constructivism? In two ways: one in favor of social constructivism, and one against it.

First, Thomas Kuhn is usually pointed out as the main source of inspiration for constructivism. If theory choice is not determined by rational considerations, what else determines it? A choice will be the values, interests and needs that drive any other social decision. This opened the door for a social account of the content of science. The second way this discussion bears on our previous discussion is that it is pointing us in the direction of an argument against social constructivism. The idea here is that of distinguishing between epistemic values and social values and to take scientific decision making as based on the first kind of values.

If we interpret their position in terms of values, social constructivists seem to be committed to the following thesis: the scientific practice is not fully and essentially determined by its own values (we can name this the flow of values thesis)³. This can happen for two reasons: either there are no values specific to the scientific practice (the strong thesis), or there are such values but they are overridden by social values, needs and interests (the weak thesis). Do these theses make sense?

In its strong form, the thesis seems to imply that we don't have different social practices with their own values and goals. What it is at work in science, as it is in other social practices, are economic, political and social interests. But this seems at least implausible, and not just for the case of science.

The first thing we can observe about the weak form of the flow of values thesis is that it can be easily transformed into its strong counterpart. All we need is to deny that social values can enter scientific decision making without affecting the values that define science as an independent social practice. But let's say that this is possible – what the weak thesis implies is that social values contaminate scientific decision making and so change the whole outcome of scientific practice. This is highly controversial, though. We normally see science as comprising those social practices aimed at the knowledge of the world, hence at truth, and we take the values that shape these practices as securing the chances of attaining this goal. Of course that the members of the scientific community bring with them certain political, economic, and social values that influence in certain respects how they pursue their work but, normally, these are either overridden by the epistemic values of the community or they don't interfere with the latter (for example if the social values manifest themselves in the choice of scientific problems and the direction of scientific research). This doesn't mean that the path to truth, which is supposedly secured by the epistemic values found in science, is not easily distortable. It means only that there are such values and they shape the scientific practice.

There is another way, though, for the social constructivists to argue for their doctrine, which may not seem to be so problematic. This time, their target is not the values but the goal that those values are supposed to improve the

chances of attaining. Is the goal of science really knowledge? What I will try to do⁴ in the next sections is to show that this view about science's aim is problematic.

2. Science and society

“Science does not stand outside of society dispensing its gifts of knowledge and wisdom; neither is it an autonomous enclave that is now being crushed under the weight of narrowly commercial or political interests.” (Gibbons et al. 1994, p. 22)

Science is resource dependent. Besides the time and energy of researchers, it needs a large amount of finances to cover all the costs generated by the research processes: starting from laboratory equipment to the salaries. Now, where does science gets this money from? This is an important question because it is not hard to realize that, whoever controls resources, controls science;⁵ and through money are injected into the system the funders' values: their tastes, needs, ethics, etc. The biggest and oldest sponsor is society as a whole, but recently there are other parties joining in. There are interest groups that invest in science through their own research foundations and there are firms that develop in-house research programs or provide funds for external ones.

Is scientific knowledge affected by this resource dependency of science? There is no straightforward answer to this question. What we can say with certainty is only that the production of scientific knowledge is sensitive to this dependency. Actually, there are recent sociological studies that draw our attention to the recent transformations of the nature of the research process that takes place in our society as a result of the commercialization of research.

2.1. The commercialization of research

In recent sociology studies⁶ we can notice a growing interest towards a very interesting phenomenon which seems to take the aspect of a true revolution: the transformation of the old paradigm of scientific research into a new mode of knowledge production. According to the main researchers⁷ who have drawn the attention upon this phenomenon, the revolution consists in passing from

2

See for example Putnam: “without values we don't have a world” (Putnam 1990, p. 141).

3

If everything revolves around social interests and is oriented to social goals, then the social values flow throughout every human practice.

4

Actually, I'm sketching only the outlines of an argument because filling in the details (examples, details of different positions, extended counterargument and historical support) would have inflated the paper to undesired proportions.

5

In the words of an electrical engineering department's chair: “You have to accept the fact

that it [research] is going to be driven by the people who give you the money. [If] the state gives us money, they tell us what to do. [If] NSF gives us the money, they tell us what research they want done. [If] DoD gives us the money, [its] the government ... Why is it any different with industry? I see no difference whatsoever.” (in Slaughter et al., 2004, p. 135)

6

See for example Scott (1999), Finnegan (2005), Sörlin and Vessuri (2007), Viale and Roselli (2010).

7

Gibbons et al. (1994); Nowotny et al. (2001).

Mode 1 science – homogeneous, built on theory, autonomous and disinterested – to Mode 2 knowledge production – trans-disciplinary, contextualized, oriented towards applications and receptive to certain external factors such as sociological problems and industrial priorities.

In this context we can notice a breaking down of the boundaries between university, industry and governments.⁸

“The university takes the role of industry by stimulating the development of new firms from research, introducing ‘the capitalization of knowledge’ as an academic goal. Firms develop training to ever higher levels and share knowledge through joint ventures, acting a bit like universities. Governments act as public venture capitalists while continuing their regulatory activities.” (Etzkowitz 2008, p. 1)

The new entrepreneurial science assumes as a core value the participation in the process of economic development and becomes a veritable economic actor committed to making money.

The outcome of these transformations is that knowledge becomes valued in terms of the profit-potential that it has on the market. It is no longer a public good, but an “intellectual property” which can be commercialized as any other goods and services. This changes completely not only the way scientists view the results of their activity, but also the very nature of this activity as they tend and are stimulated to become more focused on the socio-economic aspect of their research: scientific research has become – under these circumstances – a socio-economic enterprise.⁹

How does this recently assumed function affects the other roles of scientific research? What we know for sure is that it can change completely the choice of scientific problems. But does it interfere with the epistemological function of science?

2.2. *Truth, adequacy, profit...*

The new process of knowledge production depicted in the last section suggests an interesting way to argue for a social constructivist view on science. In order to see this, we need to make a short detour to look at the dispute between scientific realists and their opponents.

What is scientific realism? This is a difficult question since the philosophical literature comprises a whole galaxy of doctrines that can be placed under this label. A way to order this variety of realist views is on the basis of the theses that are taken to best characterize them. We can distinguish between the following realist theses:

- A. *The ontological thesis* – expresses the scientific realist’s belief that most of the unobservable entities postulated in a (mature) scientific context populate the world as well as observable objects do.
- B. *The semantic thesis* – takes the theoretical claims about the unobservables as being, as well as those about observables, truth-conditioned descriptions of an independent reality.
- C. *The epistemological thesis* – is making explicit the trust that scientific theories are our best sources of knowledge about the whole world: observable and unobservable.
- D. *The axiological thesis* – presents science as aiming at true theories.

Most scientific realists prefer to present their position as a research program comprising several of these theses (usually the first three), but there are phi-

losophers who choose only one of them as constitutive for the central aspects of their view.

Let's focus on the realist view expressed with the help of the axiological thesis. Between those who choose to see realism in an axiological light is Bas van Fraassen. He takes the following to be the correct statement of scientific realism:

S.R.: "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." (van Fraassen 1980, p. 8)

To this, he opposes the following anti-realist position:

C.E.: "Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate." (van Fraassen 1980, p. 12)

Does what we have said above about the production of scientific knowledge influence in any way the debate between scientific realists and anti-realists as it is cast in van Fraassen's terms? It surely does seem so. To see this, let's reformulate the above realist axiological thesis by taking into account the discussion from our previous section about the capitalization of knowledge. We have the following thesis:

C.K._R: *Science aims to give us market competitive and social relevant results, and acceptance of a scientific theory involves the belief that it is true.*

At first sight, the first part doesn't seem to be compatible with what follows. After all, what has market competitiveness to do with truth? If anything, it seems to stand in the way of uninterested, autonomous, and objective truth seeking activities. Besides, if you don't search for something, what chances do you have of finding it? If our current science turned its interest away from truth, can we still find it between the scientific results? An affirmative answer to this question would presuppose an exaggerated view about the constraints that the world imposes upon our theoretic activities. Should we then reformulate the second part of this thesis along the lines traced by the above anti-realist position? Does the following thesis make more sense?

C.K._E: *Science aims to give us market competitive and social relevant results; and acceptance of a scientific theory involves as belief only that it is empirically adequate.*

8

This phenomenon is usually referred to as the triple helix: "As these three institutional sectors (public, private and academic) interact, a spiral pattern of linkages emerges at various stages of the innovation process" (Etzkowitz 2002, p. 139).

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Not everyone sees these things in the same colors. Etzkowitz, for example, takes the universities' assumption of economic and social missions as an extension and normal continuation of its previous functions of teaching and research: "The first academic mission of education implies a second mission of research that in turn propels a third mission of eco-

nomical and social development" (Etzkowitz 2008, p. 30). See also Viale and Rosselli: "Academic communities are fearful that capitalization will diminish the university goal of knowledge production *per se*. This fear seems to be linked to a traditional image of the division of labor in universities. Curiosity-driven research is separated from technology-driven research. Therefore, if a university focuses on the latter, it handicaps and weakens the former. On the contrary, in our opinion, in many technological fields knowledge production simultaneously encompasses various aspects of research" (Viale and Rosselli 2010, p. 3). My reasons for not adopting this approach will be exposed in the next section.

Settling for this second position only on the basis of what we have said so far would be, of course, too hasty. Someone can object, for example, that $C.K._R$ would make more sense if we add, as we should have done in the first place, the search for knowledge as the primary aim of science. The capitalization of knowledge doesn't replace the original aim of science, but adds to it, transforms it. After all, what gets used as a marketable product or a means to solve social problems are the results obtained by science from its knowledge seeking activities. The old (main) aim of science is retained, what changes is the way this aim is taken. Knowledge no longer represents a goal in itself, but a resource valued for its utility in and relevance for extra-scientific contexts. But putting scientific knowledge to work, exploiting it for its socio-economic implications doesn't mean abandoning it as an aim. Thus, this thesis should look like this:

*C.K.*_R: Science aims [mostly] at market competitive and social relevant knowledge of the world; and acceptance of a scientific theory involves the belief that it is true.*

As in the realist position (S.R. above), the aim here is truth, and we no longer have any compatibility issues between the first part and the rest of this thesis. What is different in $C.K.*_R$ is the type of knowledge science is after: instead of knowledge for knowledge's sake, we have knowledge for benefit. The capitalization of knowledge makes science more sensitive to, and more concerned with what can be done with its results, but it doesn't take it out of the truth seeking business, so we don't need to abandon a realist position and adopt some sort of empiricism ($C.K._E$ thesis above) just because of it. Or do we?

Unfortunately for the scientific realist, things are not that simple. If truth-seeking would have not been such an opaque and mysterious process, so that we could say easily what counts as a good method for finding it and what not, the realist would have been in a much better position, but things are far from simple when we take into consideration scientific truth (the anti-realists offer us plenty of reasons for doubting that we can struck truth in our scientific endeavors).

2.3. *Methods and aims*

Can we say with certainty that the scientific methodology represents a reliable tool for arriving at approximately true theories about the world, or do we have to accept that all we can get from it is empirically adequate theories? Richard Boyd offers a realist answer to this question. He takes as a starting point in his argument the instrumental reliability of scientific methodology and points to a realistic account of scientific theories as being the only scientifically plausible explanation for it. We can reconstruct his argument this way:

- (i) Nobody denies the fact that we have instrumentally reliable theories, i.e. theories that are empirically adequate (or that save the phenomena in van Fraassen's words).
- (ii) We arrived at these theories by using a certain scientific methodology.
- (iii) But the principles that stand behind this methodology are theory-laden.
- (iv) We know that, so far, this scientific methodology is very successful in providing us with increasingly good predictive theories.

- (v) From (i) and (iv) we can say that our methodology is instrumentally reliable.
- (vi) But, in order to have an explanation for (v), keeping in mind what we have said at (iii), we have to accept that our scientific theories – the same ones that are behind and lead to improvements in methodology – are more than empirically adequate, that they are approximately true. In Boyd’s words: “A satisfactory naturalistic answer regarding the instrumental reliability of each of these methodological principles¹⁰ is available if one assumes that they apply in a situation in which the relevant background theories are approximately true [as well as instrumentally reliable].” (Boyd 1980, p. 621)

C: Our scientific theories are not only empirically adequate; they are approximately true. Thus, what we have in science, in Boyd’s view, is a sort of dialectical process:

“Our methodology, based on approximately true theories, would be a reliable guide to the discovery of new results and the improvement of older theories. The resulting improvements in our knowledge of the world would result in a still more reliable methodology leading to still more accurate theories, and so on.” (Boyd 1983, p. 65)

Can we recast Boyd’s argument in a context similar to that of the capitalization of knowledge? I think not. If we take into consideration the change of goal that accompanies the new mode of knowledge production, we have good reasons to suspect that the dialectical process, that represents the core of science in Boyd’s view, fails. In this context, we can no longer speak about new theoretical knowledge leading to improvements in methodology which, in turn, leads to better (in terms of approximating truth) theories, because the methodology is no longer affected only by or shaped according to new theoretical knowledge. There are other things sneaking in.

To better understand what we are dealing with here, let’s take a look at scientific methodology. What do we have in mind when we speak of a method? Usually, a method is a prescription telling how to do something in a certain way or by a special procedure. Thus, we can have methods for almost every activity, ranging from the simplest like drinking tea to the most complex like building space shuttles. The relation between methods and these activities is complex: methods can play different roles, from improving the outcome of an activity (either by making it socially acceptable, e.g. drinking tea or eating in a certain way, or by bringing the result closer to one’s wishes) to making possible a certain activity (there are complex and complicated activities that are impossible without a method that tells exactly what to do at each step of the process).

A thing that all methods have in common is the strong bond to some ends. Every method is specially designed for the realization of some goal, and it consists of the set of rules one has to follow in order to reach that goal. From this perspective, one can represent methods as some sort of imperatives:

I. M.: You ought to do X, if you aim at Y [where X can be taken as a set of rules].

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The principles Boyd is referring to here are the extra-experimental principles (i.e. those

different from consistency with observational data) at work in theory choice.

The problem with this representation is that it is too restrictive. Not every method can be thought of as a sort of imperative because not every aim is so straightforward realizable or attainable with the help of a method – either because the aim is too vague¹¹ (and not knowing exactly what you want prevents you from finding a method that can deliver) or it is too complicated (and it is no simple business to find out what you have to do in order to attain it). This leaves room for methods that only improve the chances of reaching a certain goal. These methods are more like guidelines that can't do more than set one on what seems to be the right path towards the desired aim. Instead of a set of rules, these methods consist of a set of values, i.e. things that is desirable to pursue given a certain aim. We can understand these methods as having this form:

G. M.: If you want Y, then you should want X also, because, from what we know, doing X improves the chances of attaining Y [where X can be taken as a set of values].

This formulation (especially the “from what we know” part) brings to the forefront another characteristic shared by all methods: the dependence of some background knowledge. The means one finds for realizing certain aims are strongly dependent on the context in which they are looked for, and this because, first of all, methods can be found (are possible) only if the right context presents itself (e.g. building a space shuttle would have not been possible outside the context of the 20th century science), and second, different context can lead to different methods or to improvements in existing ones.

Now it is a good time to return to Boyd's argument. His view on the dialectical relationship between scientific methodology and scientific theories corresponds broadly to what we have just said about the context dependency of methods in general. What is missing from his picture is the importance goals play in shaping a certain methodology: a methodology is not only theory-dependent but it is aim-sensitive as well and the slightest change in aim can have considerable effects on the associated method. So why did Boyd disregard the importance of aims in the shaping of the scientific methodology? I don't think he did. He took the aim of science as constant throughout history (as fixed) – as most realists do – and so it was just uninteresting to bring it into discussion: what changes is only the theoretical context in which scientific methods are forged in the struggle to strip the world out of its truth. But this impression, i.e. that the same struggle (in terms of aim) dates back to the beginning of science, is mistaken – it originates in an idealized view on science that doesn't take into consideration its socio-historical evolution. Compare, for example, Newton's or Boyle's semi-theological aims with the present-day view about what science aims and should aim at. Do we have the same thing? Some will say we do: in both cases we have as the main aim the search for true theories about the world – everything else is unimportant. I disagree, but I am not going to take the historical path here and try to provide examples in support of my position. Actually, that won't work very well, because showing that throughout history scientists had different views about the goal of science doesn't do much in terms of assessing the impact (if any) these differences had on the content of science. More than that, someone can easily reply to such an undertaking that, what the historian identifies as different goals is actually the same goal (search for true theories about the world) cosmeticized with all kind of things that are not concerned with the content of science, and are best treated under the heading “extrinsic values”.

A better way to understand the importance aims play (especially aim differences – even the small and seemingly unimportant ones) in the shaping of the content of science is to look at their relation with the means we normally use for attaining them. Returning to our previous talk, we can say about methods that they are doubly dependent: on the knowledge context they are created in and on the aim they are meant to fulfill. Depending on the aim we can distinguish between *imperative methods* and *guideline methods*. Those methods that are known to deliver the aim every time they are (correctly) used can be dubbed imperative methods, because the steps one takes or the rules one follows in accordance with such methods can be formulated as some sort of imperatives. The most important prerequisite for such methods is that the aim that they are linked to is clear (we have to know exactly what we want if we are to find ways that can deliver). But not every aim is this way. We sometimes have vague goals, i.e. we want things we don't really know much about. This brings into attention the second type of methods, because the most we can do when we are on the path towards an unknown, obscure and elusive end is to rely on something that seems to capture all the good characteristics of what we feel is the right direction.

Now, doesn't this talk about vague, obscure, unknown, and elusive ends ring a bell? Doesn't it remind us about the seemingly unchanged goal of science: truth about the world – this beast that the methods of science are supposed to tame? For those inclined to answer negatively to this question, I must specify that I'm not saying that there isn't such a thing or that it is in principle an end we cannot reach. Actually, I am making the most modest of claims: we are far from having a clear picture about what "truth about the world" amounts to. As evidence for this stand all the "good" past scientific theories that are lying now in the scientific mausoleum. But there is nothing here that a scientific realist will not agree with. Every realist nowadays will avoid taking scientific theories as true descriptions of something (no matter how much they want to do that), because we can no longer say about a theory that it offers us a true story about the world without sounding ridiculous. Claiming that implies a complete ignorance towards the history of science, i.e. to the fact that all our past scientific theories have turned out to be false, and to Popper's teachings about the fact that we can never know if a theory is true. Instead of truth, they use concepts as Popper's verisimilitude or the concept of approximate truth and interpret past theories as approximately true or see truth as being approached by the increasing verisimilitude of such theories.

Science aims at true theories about the world, but we have no idea what that is. Or do we? Most realists will disagree with this claim: by true theories about the world, we mean theories that give us accurate accounts about the causal structure of the world. A reply might be that they are more than that; they are theories that give us knowledge of the intrinsic natures of physical (observable and unobservable) objects. To counterpoint, one might say that they are about the natural kinds of objects, properties and processes and their real essences. But wait, there are no objects or essences or any kind of individual things, so they can only be about the nomological structure of the world. Thus, which one is it? What can we replace "the world" with in the above formulation of

the aim of science? Is it the objects, the essences and their kinds, the fundamental laws, the causal structure or the nomological structure? Of course, it would be pure madness to try to answer this question. The point here is that, even though, at first sight, “about the world” seems to convey a precise and clear meaning, is actually as vague and obscure as it gets.

We will say it again then: science aims at true theories about the world, but we have no idea what that is. We have no idea what a true theory will look like, and we have no hope of finding such a thing. The most we can hope for is truth-approximations or truth-likeness or verisimilitude or whatever other substitutes for truth we prefer in order to deal with all the idealizations, approximations, simplifications used in scientific theories and all the errors present in the scientific practice. But truth-substitutes do come with their own problems which don’t do us any good when it comes to having a clear picture about what we are looking for in science. But this is not all. In addition, we have no idea what the “world”, that the scientific theories presumably are about, amounts to in this context.

Thus, what science aims at after all? Well, we could say that science aims at something that we know we cannot have about something we know nothing about. I take this to be the real meaning of the following phrase: science aims to reveal the secrets of nature.

Let’s get back now to our previous discussion about methodology and ask ourselves: what does all this say about what happens in science? What kind of methods do we find there? Well, if I’m right and science has one of the vaguest aims you can find, then the only kind of methods we will find in science are guideline methods and so the activity of science would be a lax sort of undertaking. But this strikes us as a completely mistaken picture. Scientific practice seems to be replete with rules that are part of methods for specific activities. Scientists don’t seem to rely only on (or follow just) some guidelines when conducting an experiment for testing the outcome predicted by some theory, for example, or for measuring a certain quantity – they are following strict rules that guarantee that the outcome of their activity is accepted by their (scientific) community and considered scientific. But there is no way these rules could have been forged in the context of a practice with such a vague aim that science presumably has, right? Wrong. If all that matters is what the scientific community accepts, i.e. if we translate this in terms of some sort of social acceptability, then everything is possible. I am not willing to take this path, though, that is why I will reformulate the last phrase so that it won’t lead us in that direction: when calculating, making observations, conducting experiments or pursuing whatever other activities they are involved in, scientists are obeying rules designed to ensure the success of their practice. A good question here would be: what kind of success? If not social success, then what? A simple (and neutral) answer would be success in interacting in certain ways with certain specific aspects or our surroundings. But where do these rules for interacting with our surroundings come from? They form imperative methods that seem unlikely to be generated under the auspices of the aim of science as described above.

An obvious way out here would be to take this problem as evidence that science has another aim. “The search for true theories about the world” as the aim of science is a product of the realist’s wishful thinking which doesn’t come even close to what really happens in science (as the above discussion seems to suggest). Science doesn’t aim at truth, it doesn’t struggle to reveal

the secrets of nature, but it's searching for better ways of dealing (in terms of controlling, transforming, predicting its behavior) with our surroundings. This is a way more tractable goal that fits better with the image projected by the scientific practice (i.e. with the imperative methods that govern what seems to be the main scientific activity: interacting in certain ways with our surroundings).

I don't think this is a good escaping route from the above difficulty, though. Actually, I don't think we have a difficulty at all. The apparent tension between what the goal of science (the realist version) implies (in terms of methodology), and what the practice of science presents us with was generated by the fact that we looked in the wrong place for an assessment. In order to diffuse the tension, it is sufficient to acknowledge the existence of the following rough divide between two types of scientific practices: *interactionist practices* (making observations, experimenting or applying theories), and *theoretic practices* (theory creation and theory assessment). The interactionist practices are based on and derive from the theoretical context generated by the theoretical practices, but the methods that shape these practices shouldn't be related to the overarching goal of science but to the theoretical constraints acting on them. The overarching aim has a direct relation only to the theoretic part of the scientific practice. But it is well known that this part is methodologically flawed. Thus, the fact that the goal of science is so vague, obscure or elusive is perfectly compatible with the scientific practice. No tension here and no need to search for a more tractable goal.

Let's return to Boyd one final time. Why is all this discussion about the aim of science and its relation with scientific methodology and scientific practice relevant for our main objective in this section (i.e. to show that Boyd's argument fails in a context similar to that of the capitalization of knowledge)? As we said earlier, in Boyd's case, we have a fixed, clear, objective, not socially-related, realist aim of science¹² and some background knowledge that together represent the context in which our scientific methodology is generated; methodology that leads to increasingly accurate theories about the world which, in turn, become background knowledge for a new cycle that will get us even closer to the truth (see fig. 1). But as our subsequent discussion tried to show, the aim of science is far from being clear and fixed. If we can show that this aim is socially related as well, we have a strong case against Boyd, because we can argue that the above dialectical process doesn't actually get us close to the truth.

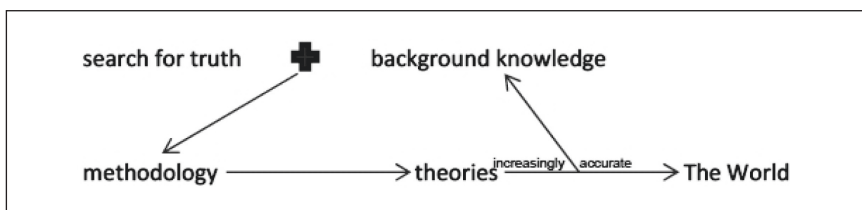


Fig. 1

Is the goal of science socially related? Not really, no. Even though it doesn't have the most fortunate of aims, it does have a stable one: science tries to

¹²

Boyd doesn't mention such a thing in his argument, but I take it as a hidden presupposition of it.

reveal the secrets of nature no matter what social setting is enveloping its practice. But, as our previous discussion about the capitalization of knowledge showed, this aim is sometimes coupled with other, socially determined/relevant, goals. In this context, instead of aiming just at revealing the secrets of nature, science has a more focalized aim: not every secret of nature will do. What our society is requesting from science is to unveil more of those secrets that are useful in solving social problems or that have industry/economy boosting potential. Now, the effects that this coupling has on science are not content-related, and thus they shouldn't be given too much epistemological attention. If we look at it from the perspective of our previously sketched relationship between aims and methods, we can capture this remark in terms of the following dissociation between the values that the guideline methods consist in: *intrinsic values* and *extrinsic values*. We call intrinsic those values that are directly related (in terms of contributing to attaining it) to the desired aim. Extrinsic values, on the other hand, are those values that do not affect the path towards the aim, but that are telling us how we should take it and what to do with the results. In our case, market competitiveness and social relevance function as extrinsic values determinators that don't interfere with more important aspects of our scientific methodology.

Unfortunately, things are far from being that simple. There is a problem that prevents us from following the above reasoning about the epistemological attention that socially determined scientific goal alterations should enjoy: the fact that the goal of science is so vague puts us in difficulty when it comes to distinguishing between intrinsic and extrinsic values. Remember that the role of values in the guideline methods case was to capture all the good characteristics of what we feel is the right direction towards a desired aim. But finding a good path towards a vague aim is not an easy job. That is why when we have a compound goal, vagueness can be taken as weakness: the less vague part of the compound will have priority over the rest in determining the values that the methods should consist in. We will always be tempted to go (value-wise) towards the better known end even though is not our main aim (especially in a context in which, due to the vagueness of our main aim, it is hard to tell what counts as a deviation from the path towards it).

We can illustrate this point by looking at the following example: *If you want market competitive and social relevant knowledge of the world, then you should prefer scientific theories that exhibit the following virtues..., because, from what we know, this kind of values improves the chances of attaining your goal.* The dots here should be replaced with a set of extrinsic and intrinsic values that compose our method in this case. The extrinsic values will have to do with those things that ensure that our theoretic results have economic potential and social relevancy, and the intrinsic part with knowledge. Consider now the following two situations:

- (a) our scientific practice comes up with a theoretic result that satisfies in an evident, undeniable fashion the extrinsic values set up for science by this social setting, but doesn't excel when it comes to the intrinsic part (it doesn't conflict with any value, but is also hard to tell if it satisfies them);
- (b) we have a theory that doesn't satisfy classical intrinsic methodological value, say simplicity, but is particularly fruitful in technological results (i.e. we have a complex but technologically / economically fruitful theory).

What kind of decision will presumably be taken in these cases? I have strong doubts that our scientific community would decide to abandon either one of the above theoretical results. What would determine such a rejection? In both cases we have strong (social) reasons to accept the theories and we are uncertain about the reasons to dislike them (we are far from being sure that simplicity, for example, is such a good right path indicator).

What this suggests is that extrinsic (socially related) values are far from being epistemologically irrelevant: they affect in a straightforward way scientific decision making, and so they affect the very content of science. This doesn't amount to saying that epistemic values (truth / justification related values) are not the main players in scientific decisions, but that they can easily become compatible with other (possibly not epistemologically innocent) factors.

Thus, the goal of science is partially but significantly socially related. Why is this a problem for Boyd's argument? Because it suggests replacing his picture about the scientific dialectical process with the following one: we have a social-historical context which partially determines the aim of science, which in turn, together with the available background knowledge, stand behind a methodology that is responsible for empirically and aim-adequate theoretical results (see fig. 2). Progress towards truth is out of the question here because the methodology is no longer trustworthy in this respect. This, of course, leaves us with the problem of explaining the instrumental reliability of this methodology. But this shouldn't bother us too much and this for two reasons. First, there are alternative anti-realist explanations to choose from. Second, if we are right about these methods and the scientific decision making, a truth-involving realist explanation (a la Boyd) will not do anyway (it would be crazy to say that, even though socially contaminated as they are, these methods can still contribute to "the overall reliability of the scientific practice with respect to the acceptance of theoretical principles and laws which are not only predictively reliable but approximately true as well" (Boyd 1980, p. 621)).

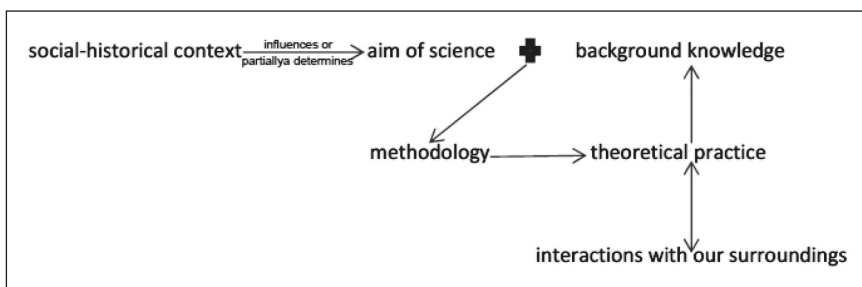


Fig. 2

2.4. Social constructivism... again

After our long detour has ended, we can return to our previous aim-related position formulation game. What would be a proper thesis in this context? I think we should go with the following one:

C. K.: Science aims at market competitive and social relevant knowledge of the world, and acceptance of its theories involves the belief that they are good [in terms of satisfying the aim].

As it stands, this thesis is, of course, not satisfactory. We don't want to (wrongly) imply that social constructivism (in the clothes it wears here) is a

view suggested and supported only by this recent phenomenon (the capitalization of knowledge). What I have in mind is that the type of social influence on science that this phenomenon exhibits accompanied science throughout its history. In Steven Shapin's words:

“Throughout history, all sorts of universities have ‘served society’ in all sorts of ways, and, while market opportunities are relatively novel, they do not compromise academic freedom in a way that is qualitatively distinct from the religious and political obligations that the ivory tower universities of the past owed to the powers in their societies.” (Shapin 2003, p. 19)

The above thesis is only a related to the present social and scientific contexts expression of our view. A complete picture will consist in a list with all such theses that can be obtained from the history of science by replacing what comes after “aims at” in C.K. above. We can resume this list with the help of the following thesis which can be taken as the essential formulation of our position:

S.C.: Science aims at some socio-cultural contextual knowledge involving aim, and acceptance of its theories can't [shouldn't] imply more than the belief that they are adequate for [in light of] the contextualized aim.

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Gabriel Târziu

Socijalni konstruktivizam i metodologija znanosti

Sažetak

Znanstvena praksa tip je društvene prakse i svako područje znanje uopćeno manifestira važnu društvenu dimenziju. No treba li se činjenica da znanstvena praksa nastaje suradničkim trudom članova društvene grupe biti povezati s proizvodima takve prakse? U ovom radu nastojim na to pitanje odgovoriti afirmativno. Moja će strategija biti argumentirati da cilj znanosti djelomično određen društveno-povijesnim uvjetima i da taj cilj, uključujući dostupno pozadinsko znanje, stoji iza metodologije odgovorne za empirijski adekvatne na cilj orijentirane teorijske rezultate.

Ključne riječi

društveni konstruktivizam, cilj znanosti, znanstveni realizam, komercijalizacija istraživanja, znanstvena metodologija

Gabriel Târziu

Sozialer Konstruktivismus und Methodologie der Wissenschaft

Zusammenfassung

Wissenschaftliche Praxis ist eine Art sozialer Praxis, und jedes Wissensgebiet manifestiert verallgemeinert eine wichtige gesellschaftliche Dimension. Soll jedoch die Tatsache, dass die wissenschaftliche Praxis durch mitarbeiterisches Bemühen der Mitglieder einer gesellschaftlichen Gruppe entsteht, mit den Produkten einer solchen Praxis in Verbindung stehen? In dieser Arbeit bin ich bestrebt, diese Frage affirmativ zu beantworten. Meine Strategie ist das Argument, dass das Ziel der Wissenschaft partiell von gesellschaftlich-historischen Bedingungen bestimmt wird, und dass dieses Ziel, einschließlich des verfügbaren Hintergrundwissens, hinter der Methodologie steht, die für empirisch adäquate zielorientierte theoretische Ergebnisse verantwortlich ist.

Schlüsselwörter

gesellschaftlicher Konstruktivismus, Ziel der Wissenschaft, wissenschaftlicher Realismus, Kommerzialisierung der Forschung, wissenschaftliche Methodologie

Gabriel Târziu

Constructivisme sociale et méthodologie de la science

Résumé

La pratique scientifique est un genre de pratique sociale, chaque domaine de la connaissance manifestant de manière générale une importante dimension sociale. Au vue du fait que la pratique scientifique apparaît sur la base d'un commun effort des membres du groupe social, faut-il pour autant l'associer avec les productions de telles pratiques ? Dans ce travail, je m'attache à répondre à cette question par l'affirmative. Ma stratégie s'appuie sur l'argument selon lequel le but de la science, étant partiellement déterminé par des conditions historico-sociales, inclut un savoir accessible en arrière-plan, et est motivé par une méthodologie qui est responsable des résultats empiriques adéquats et de la théorie qui en résulte.

Mots-clés

constructivisme sociale, but de la science, réalisme scientifique, commercialisation de la recherche, méthodologie scientifique