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A demarcation between good and bad constructivism: the case of chemical substances as artifactual materials.

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One can understand only what one has created.
Giambattista Vico, 1725

*In turning toward the craftsmen, the ingenious engineers who actually build engines and machines, we shall be able to clarify the strange notion of construction to which "**constructivism**" does not seem to be particularly faithful.*
Bruno Latour, 2013

Resumo: Este artigo pretende mostrar que sem a influência de uma filosofia construtivista que eu denomino boa, representada principalmente por Bruno Latour, a elucidação das substâncias químicas teria sido virtualmente impossível. Sem a noção de materiais "artefatuais" cunhada por eles (artefatual não é o mesmo que artificial, uma palavra com conotações de engano ou falsidade que não se aplicam a artefatual; este neologismo procura evitar essas conotações e vem da palavra artefato), a Química Moderna seria impensável a partir dos metaparadigmas em uso no campo atual da história e da filosofia da ciência. A tese central que defendo aqui é a de que o construtivismo, tal como definido pelos antropológos da ciência, é uma maneira disfarçada de colocar a histórica filosofia materialista na trilha da discussão histórica e filosófica das ciências e tecnologias e que isso restaura o papel desempenhado pelos materiais usados pelos cientistas para que sejam exatamente o que são.
Palavras-chave: construtivismo; materiais artefatuais; substâncias químicas

Abstract: This paper aims to show that without the influence of a constructivist philosophy I call good, mainly represented by Bruno Latour, the ontological elucidation of the chemical substances would have been virtually impossible. Without the notion of artifactual materials given by them (artifactual is not the same as artificial, a word with connotations of deceit or falsehood that do not apply to artifactual; this neologism attempts to avoid these connotations, and comes from the word artifact), would be Modern Chemistry unthinkable from the metaparadigms in use in the present field of history and philosophy of science. The central thesis I defended here is that constructivism as defined by the anthropologists of science is a disguised way of sliding historical materialist philosophy into the mainline of philosophical and historical discussion of the sciences and technologies and that those restore the role played by the materials used by sciences to be exactly what they are.
Keywords: constructivism; artifactual materials; chemical substances.

1. INTRODUCTION[1]

Whenever we approach philosophical thinking, or a variety of philosophies (realism, relativism, constructivism, materialism[2], conventionalism, instrumentalism, operationalism, and so forth), as we get closer to the field these philosophies multiplies themselves exponentially and we can find ourselves with more than one thousand, for instance, scientific realisms, as Carman argued in his doctoral dissertation

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(Cf. CARMAN, 2008). We can indeed live among a plurality of ideas and even of philosophies, but there comes a time when specificity becomes important. It is the moment of reflection, the moment when similarity and difference are of consequence. It is the moment in which some applications of these ways or varieties of philosophies must serve to study a defined object, substance and/or process.

My aim in this paper is to discuss the notion of materiality or rather of materials as *artifactual materials*. It is clear that not any philosophy will allow me to do this. Realism would be *useless*, and so, alas, would be relativism, whereas operationalism in Chang's (CHANG, 2009) version would seem inevitable, and so forth.

In fact, there is a philosophical way of thinking that since its inception around 1978 has facilitated thinking about materials and the materiality of objects and processes studied by science and technology which is basically what interests me here. This philosophy is a very specific constructivism that on this occasion I will summarize, differentiate and determine once again (LEWOWICZ, 2003). I will also try to show very briefly *what is good* about this constructivism for the history and philosophy of Chemistry and, reciprocally, what it contributes to the notion of chemical substance or chemical material. In the Introduction of Ursula Klein & E.C. Spary's book we read:

> The dramatis personae of this book are materials such as metals, gunpowder, pigments, and foods. Materials and Expertise in Early Modern Europe take useful materials substances, "materials" as a route into mixed artisanal and learned practices, which contributed to artisanal innovation, the development of the consumer market, and the formation of the observational and experimental sciences of the early modern period.(KLEIN & SPARY, 2010, p.1)

This is the definition of material I will use here, and at the same time I will consider that chemical substances *qua* chemical substances are the same as materials or artifactual materials. And that the terms that refer us to these types of substances are all mass terms: "metal", "gunpowder", "pigment", "meat extract". In other works (LEWOWICZ, LOMBARDI, 2013a; 2013b) we have shown the difficult situation of the reference of mass terms. The basic idea we defended was that chemical substances and materials are not individuals (but technical and epistemological *individualizations* -now I add) and therefore our historical and scientific languages have several, and in some cases severe difficulties to refer to the above-mentioned materials. These difficulties are clear although they may not be exhaustive. For the problem of reference to be such and to make sense, it cannot be conceived as, curiously, Bloor does in several of his texts (BLOOR, 1999; 1999a; 2005) as a synonym of *speaking about something*. It is necessary to mention this problem here because Chemistry is a very genuine place to talk about scientific language and its problems. And we will find the tematization of this problem only in what I call *good* constructivism.

Moreover, I want to make it clear from the start my ontological position with regards to "material" or "chemical substance" or artifactual material. All of them exist, very few of them independently from our ways of thinking and working, others not at all. In other words, the independence of materials or chemical substances are in themselves cases for ontological study. They are not, and nor can they be *real* beforehand, unless realism be considered differently, and I am not sure I want to increase Carman's list. The program of Organic Chemistry, for instance, is the production of new chemical substances thanks to the overwhelming productive ductility of carbon, all of them are clearly artifactual materials. All these substances exist and become *autonomous* from the social, historic, technical and scientific contexts where they were produced, but they are clearly constructed and new with respect of nature.

This line of reasoning approaches the uselessness of the ontological theses of scientific realism when dealing with chemical substances. Too many chemical substances exist but are not independent from their conditions of production or invention. And what scientific realism posits is basically that the objects, substances and processes that science study are, in fact, existent and independent from scientific theories and conjectures, to speak only of the merely ontological theses of scientific realism. In order to speak about its epistemological theses, we must address (again) the problem of the reference of scientific terms first, the existence of natural kinds secondly, and the non-arbitrary existence of artifactual kinds, thirdly. Roberto Torretti has rightly insisted that realism is a *chosisme*[3] and that it should never have been called realism. Indeed, there are few if any things in macrochemistry, the chemistry of human dimension; there are parts and pieces, few and many, non-countable objects. If we wonder what is *meat extract*? The answer involves necessarily the historical conditions of its production, the way in which it is elaborated and the

kind of material produced: a very sticky substance of a dark brown used as a base to produce different dishes. I can point to the object that contains it, but in no way can I point to the meat extract without this condition. This is to say, the ostension function in the realist ways of referring does not work here.

Apart from the fact that I do not find the philosophical taste of relativism pleasing, I have stated until now that chemical substances or artifactual materials become autonomous from their initial processes of production. Actually, they manage at some point in their history to become independent from the model or models, theories or conceptual frameworks that took part in their production in such a way that the common sense believes that *plutonium* is a radioactive chemical substance extracted from *nature*, that is to say a natural substance, wholly forgetting the production process of this artifactual material which simultaneously "produced" Madame Curie's Nobel Prize and what in fact killed her, that is to say, completely avoiding the history of chemical process that produced this wholly artifactual radioactive material which, however, was easily accommodated in the Periodic Table in use in the times of this great female scientist. And it remained there. Relativism demands that these things should not happen. On the contrary, it posits that each entity or process produced under a conceptual framework – and moreover, without a conceptual framework no entity or substance or process would have been produced—should not surpass conceptual change. And, of course, that it should turn the problem of reference into a solution. Each entity, substance or process produced within the conceptual framework is already *predefined* in it, albeit not in its entirety.

2. WHAT CONSTRUCTIVISM IS NOT

Since the 80s, the expression "social constructivism" has been uttered with increasing reiteration and on such occasions just like one would repeat a pejorative, degrading and mainly accusatory label of a certain unknown philosophical crime. In general, this *status quaestionis* is due to the gratuitously negative effect of Alan Sokal (SOKAL, 1997) and some of his scientist colleagues who tend to understand too little of philosophy and a little more of the anachronistic philosophies of science that survived until 1962.[4]

One of the wrongly dubbed social constructivists is the *Strong Program in Sociology of Scientific Knowledge* or the so-called Edinburgh School, or simply, the sociologists of science or *bad* constructivism, as I shall call them here. David Bloor, Barry Barnes, Steven Shapin, John Henry, are some of its numerous representatives. In a post-Mertonian environment of laboratory studies the Strong Program is part and sometimes a synonym of a hybrid interdisciplinary field now known as Social Studies of Science and/or Technology, which separates the old and the new sociology of science, even though its philosophical tenets coincide. They have the common assumption that science and technology, just as them are, works.

And the basic purpose of both is to describe how science works and explain why it works that way. It must be clear that the purpose of these sociologies of science is not to analyse, criticise or challenge modern science and technology – be them as a value system or a world-view, or as a way of life and work.

In *Knowledge and the Social Imagery* (1976) David Bloor, the official voice of the Strong Program, remarks that sociology of science must not deal exclusively with its institutional character but also with its *content and nature*. This task used to be assigned to Philosophy of Science but today this has changed somehow. Unfortunately I cannot discuss this matter here. The general idea is that the so called sociologist of science (a further competitor of Philosophy of Science) must study the content and the nature of the sciences in the light of the scientific institutions where it is produced. This apparently social constructivist proposal of analysis of science will soon become disturbed when Bloor remarks that "the sociologist is concerned with knowledge, including scientific knowledge, purely as a *natural* phenomenon" (BLOOR, 1976, p. 2; my emphasis). Instead of defining scientific knowledge as *true* belief (not materials, substances, objects, processes or artifacts), scientific knowledge will be for the sociologists of the Strong Program that which people consider knowledge. "In particular the sociologist will be concerned with beliefs which are taken for granted or institutionalized" (BLOOR, 1976, pp. 2-3).

However, the sociologist's concern will be to locate those regularities and general principles or processes that seem to work well, and their goal will be the construction of *theories* that explain these regularities. If these theories satisfy the requirements of a maximum generality they will be applied both to false and to true beliefs and will offer the same kind of explanation for both. Bloor's example to illustrate what he is defending is the following: "the aim of physiology is to explain the organism in health and disease; the

aim of mechanics is to understand machines whish work and machines which fail" (BLOOR, 1976, p.3).

Likewise, for Bloor the sociologists look for theories that might explain *the beliefs* – again, not materials, objects or processes - that actually exist, regardless of how the researcher evaluates them. So far, it seems obvious that sociologists of knowledge adopt a naturalistic thesis similar to Quine's thesis (QUINE, 1960) and hence they must adhere to the four principles and in this way embody, according to Bloor, the same values that are taken for granted in other sciences (BLOOR, p. 106).

Before enumerating the four principles of the Strong Program I must be allowed a brief digression, and reflect upon some of the consequences of what Bloor has said so far. It is a naturalist and descriptivist sociology that does not intend to make value judgments on the contents and the nature of the sciences, but which will study what people think science is. It emulates the natural sciences when it pretends or looks for the warrant of the assumed *neutrality* of the natural sciences, and to this end it must reduce sociology of science to a conventional and consensual description of the sciences: sciences are what scientists and the layman *believe* sciences are, hence substituting any normative feature of philosophy for an apparently unstoppable descriptivism. The principles will now make what I am saying more evident:

> *a. Principle of causality*: the sociologist of the Strong Program will deal with the conditions of beliefs, those beliefs emerging from the very content and nature of sciences.
>
> *b. Principle of impartiality*: the sociologist will have to account for both the truth and the falseness of beliefs, of their rationality or irrationality, success or failure, etc.
>
> *c. Principle of Symmetry*: the same kinds of causes must explain beliefs judged by others as true or false, etc.
>
> *d. Principle of Reflexivity*: it should be possible on principle to apply these patterns of explanation to sociology itself. This principle answers the need to look for general explanations. An obvious requirement, since otherwise sociology would be a living refutation of its own methodology.

The so-called *principle of reflexivity* does not seem to have anything at all to do with the natural sciences. These do not require such a logical and methodological principle to avoid inconsistency, but it still appears in the four principles of the Strong Program in the sociology of scientific knowledge. And needless to say it cannot be included in a set of theses that presuppose Quine's naturalism (QUINE, 1960), according to which *grosso modo* there exists a continuity between *the perceptual stimulus, the sciences and the philosophy*. Consequently, it would also fall into descriptivism. The *principle of reflexivity* on the contrary is entirely prescriptivist; it must be applied in order to the Program should not fall into contradiction with itself.

So far, what would be constructivist about the Strong Program in the sociology of scientific knowledge? Constructivism or the so called social constructivism, accepts Kant and Kuhn's "constructivist" patterns (other, *avant la lettre*, with the same erroneus name). Namely: that there is a substantial difference between "being constructed by" and "being constituted by". The construction of something is entirely contingent until what has been constructed is conventionally accepted by the community within which it was constructed. Conversely, the constitution or constitutivism, is essential. A phenomenon is constituted by the forms of sensibility and the categories of understanding in kantian terms. Moreover, something is constituted by the paradigm if, when the paradigm changes, those what moves across paradigms change worlds (LEWOWICZ, 2005). Until now the Strong Program is a kind of weak constitutivism, in no way a kind of constructivism, if we accept this distinction beyond conceiving that perception has a theoretical, or theoretical-practical touch – a thesis rather more relativistic than constructivist, or to make it more clear, a constitutivist touch – nothing clearly constructive is at work in the process of meta-knowledge that the sociologists of science produce.

The so-called social constructivism seems to emerge not from those principles defined in the Strong Program but rather from the empirical case studies they research. In essence, these empirical cases have to do with the analysis of scientific polemics that emerged throughout the history of science. And after a detailed and thorough analysis - something that is always useful - it is always concluded, litany-wise, that scientific debates are won if one of the rivals manages to *negotiate* in better way which are true beliefs

and which are not. Or what is the same, those who have more powerful social interests to impose: the confrontation of scientific beliefs is always empirically and referentially inscrutable (QUINE, 1969) - which always forces us to appeal to more or less powerful social conventions. Now, are social conventions constructed? *Grosso modo* it is very hard to deny this possibility. Why or who is not known. Neither is what for, and moreover, these conventions are always ready made at the moment of confronting a dissident colleague or group of colleagues, always handy as historical or social and theoretical - albeit not ontological - material. But conventions are in there origins contingent, as the *good* constructivism requires.

2.1. CONSTRUCTIVISM AND MATERIALISM

In 2003 I published a paper where I linked this *good* constructivism - also called *in situ laboratory studies*, or anthropology of science or ethnography of science, or reflexive anthropology - to an attempt to establish the logic and the nature of these meta-scientific studies and their link with a materialism more modern than Boyle's and other's "materialisms". The merit of these studies does not lie exclusively in the evident fact that leads us directly to the practices of scientific laboratories, but which contributes - albeit insufficiently - to revealing the social nature of the sciences: the attention these scholars of scientific knowledge pay to the manipulation of experimental processes and experimentation in general, and the path they open to reach a clear knowledge, for example, of what Marx wanted with regards to scientific knowledge and in particular to those objects he called commodities: artifacts (as, for instance, fertilizers) produced in any process of human construction or in any process of human production and *appropriation*.

In order to offer a well-organised exposition of what these scholars do theoretically I will have to make my way through much more traditional areas of history and philosophy of the sciences. For some scholars of science and practicing scientists, the scientist is a dispassionate, objective, disinterested, rational person who has been educated for the natural and immediate encounter with the things that can be found in "the world", and turn them into "terms that refer". This character is usually called a naive realist, to the point of turning this expression into a simple, fast and comfortable, but polite, insult. Mary Hesse (HESSE, 1980) remarked that this "scientist" believed in the existence of an external world that in principle could be explained exhaustively by means of scientific language. The scientist can then capture the external objects of the world and lay them out in propositions that will be candidates to be considered true or false.

In a somewhat idealist way, science presented itself as a linguistic system in which a true statement corresponded to the facts in such a way that, even if they were not observed they can be observable at some point in time. These obscure objects were described in scientific theories and they could be inferred from their empirical bases. In few words, it was possible to infer from the objects that could be observed those objects of the world outside our "weak" powers of observation.

The naive realist is "cut off from the world" and above other poor mortals. Here I will only underscore that the great problem of this vision of the scientist and of science itself is that it does not give us an idea or empirical image of either. Empirical studies of science demand from the philosopher that he should leave his comfortable armchairs and allow scientific practice to be a mundane character, subject to cognitive and social interests.

The antecedents of these empirical studies of science can be traced back to Marx, Nietzsche, Durkheim, Spengler, Fleck, and the early Kuhn. Towards the end of 1970, the criticism of these conceptions gave a clear idea of the importance of studying the sciences empirically and generating an *empirical philosophy of scientific practices*.

Therefore, I will speak here about the findings of *in situ* laboratory studies, or, what is the same, of the so called ethnographer and anthropologist of the sciences and the conclusion that their theoretical program allowed them to reach, such as the *artifactual nature* of the objects, substances, processes and artifacts of the sciences and technologies they were studying. Constructivsm links itself to the ontology of sciences and technologies.

If we set out to explore scientists' laboratories with theoretical perspectives and with the instruments of ethnography, it will be of use to keep two things in mind: the anthropology of science tries to pay attention to those parts of scientific knowledge that were *neglected*, and in this sense it can be considered an empirical epistemology of the sciences. Secondly, the ethnography of science is not necessarily a relativism, even though as Latour (1999) has repeatedly pointed out, it cannot but be a *relationalism*.

In other words, empirical studies of the sciences will always establish a link between a large number of scientific and technological aspects, among them the role of *agent* of the objects, substances, processes and artifacts of the sciences and technologies.

This good Constructivism pays special attention to the way in which scientific objects are produced and reproduced in the laboratory. This constructivism can be characterized by means of five distinctive features:

1. The reality with which scientists work is of an *artifactual nature*. This is to say, scientists manufactured the reality (again, objects, substances, processes) with which they work. They do not and cannot see the bare reality directly. They create conditions, chose materials and trust the theoretical and instrumental tools they inherit from other scientists and technologists to "observe and analyses" the objects they produced.

2. The activity of the laboratory is imbued by decisions. Scientists constantly select courses of action, instruments, chemical compounds and so forth (HESSE, 1980, p. 32). But moreover, previous choices reappear afterwards under the guise of rules, methods and interpretations. In consequence, scientific objects are imbued by decisions - not of theories, but of practical acts - and they also *impregnate* new decisions.

3. The third aspect of the production of the sciences and technologies is the transmutation of knowledge statements into facts. This is clearly illustrated, for instance, in statements like those discussed by Latour and Woolgar (1979). The advocates of scientific *in situ* studies posit that the laboratory changes statements throughout all 5 stages.

> Type 1. Statements are "linguistically market conjectures" of an author.
> Type 2. Statements.
> Type 3. Statements are "qualified general assumptions" ("x is generally assumed to be...")
> Type 4. Statements are "incontrovertible", facts still associated to an author (KUHN, 1970)
> Type 5. Statement stands as an unqualified "fact."

Zenzen and Restivo also show how various drafts of a report on a set of experiments progressively incorporate statements that are more technical, more general, more conclusive, and less controversial.

> In brief, scientific rhetoric becomes progressively *objectified* as we move from shop talk to published papers. Scientific work involves transforming selections into non-selections, the subjective into the objective, and 'the fabricated' into the 'the found'. Scientists separate the 'natural' from the 'social' by (temporarily) ruling out certain selection and choosing others. (ZENZEN & RESTIVO, 1978, pp- 447-73)

4. The fourth aspect of the construction of the sciences and technologies is that social and cognitive factors are inextricably interconnected. The closer we are to the cognitive core of the sciences, the clearer its social nature will become. The weakest form of this thesis is, of course, the strong program, since it posits that the kind of social group that produces a science will affect the kind of science produced. And it is good to point out that within this group of sociologists of scientific knowledge Latour is nowhere to be found. But we do find for instance Lynch or Woolgar.

5. Some sciences are self-referential. This is to say, scientists are committed to *reproducing* these conditions of construction of the sciences, particularly scientific practice. Besides achieving or constructing their findings (facts or artifacts) they also produce the epistemological meanings of their findings. Technology, regretfully, lacks this point.

The constructivist underscores that the primacy of social practice is grounded on the presupposition that facts and artifacts, facts and truths and the philosophical reconstructions of scientific work, of their objects, etc., are, all of them, social achievements. Consequently the constructivism I present here is not subjectivist (it does not emphasize psychological, individual, selfish or idiosyncratic aspects) nor is it

naturalistic, in the sense that it does not turn the sciences into natural phenomena accessible exclusively through the methods of the natural sciences (QUINE, 1960), nor is it relativist in the sense that it does not make of the construction of scientific and technological objects an entirely independent project from the external world; this constructivism can be considered an empirical epistemology. As Restivo put it:

> Indexicality encompasses a variety of ideas that tie facts to social settings. The decisions (selections) scientists make are conditioned and constrained by social contexts, available resources, opportunities that present themselves ("opportunistic logic"), the circumstances and occasions of scientific work variations in the criteria used to select methods and materials, and the negotiations leading up to the identification of a scientific finding (Collins, R, 1989). Knowledge, according to the constructivist interpretation, is then the sum of these decisions, selections, and conditions. (RESTIVO, 1994, p. 129

3. SOME OBSERVATIONS

A number of additional conjectures have been formulated by ethnographers of science:

1. Metaphor does not play a key role in the origin of ideas in the laboratory; *analogical transfer* is much more important (MILLS, 1963, pp. 229-30, 417)

2. The dependence on local opportunities makes laboratory work very much a *tinkering* exercise.

3. Interests and success (as opposed to "truth") are key driving forces in scientific practice. The scientist and the technitian as well starts from a solution or an opportunity for success, and moves on from there with the objective of "making things work". Perceived solutions push the research forward, in whatever direction opportunities for success may lie. Thus, the *logic* of science rests on what scientists *do* with reality.

4. There are no rationalities unique to science; just like everyday reasoning, scientific reasoning is practical, indexical, analogical, socially-situated, literary, and symbolic. In Latour's words:

> Scientific fact is the product of average ordinary people and settings, linked to one another by no special norms or communications forms... What *does* distinguish scientists from other people is their reliance on inscription devices. The inscription devices modify the scale of the things scientists want to talk about; complex and unwieldy phenomena become transformed into "the inscription on a flat surface written in simple forms and letters. (LATOUR, 1979)

5. Scientific and technological work is a process of *simplification* in the presence of constraints; certain things that have been done are *ignored*, and certain things are just *not done*.

6. Contingencies (social, *material*, and symbolic) are not merely "externalities"; they are "*constitutive of*" scientific facts. That is, scientific facts are actually "made up of," or manufactured out of contingencies.

In general, I find it hard to see laboratory studies and some related constructivisms (Woolgar, Knorr-Cetina, Pickering) as proposals that attempt to weaken or even undermine materialist theses; they seem to me instead roads that explore all the implications of the idea that a scientific fact is a social fact and an artifact, instead of being something one stumbles upon when one goes on a walk. In other words, we need to develop ways of studying science and technology, in a *critical* way, and not always and insistently in a legitimising way – or, to follow Nietzsche:

> We are not thinking frogs, nor objectifying and registering mechanisms with their innards removed: constantly, we have to give birth to our thoughts out of our pain and, like mothers, endow them with all we have of blood, heart, fire, pleasure, passion, agony, conscience, fate, and catastrophe (*in* ZENZEN & RESTIVO 1982, p.447).

4. CONSTRUCTIVISM AND ARTIFACTUAL MATERIALS

It would be fortunate to be able to conclude here that without the *good* constructivism I define in this paper as it appears in mainstream history and philosophy of science it would be impossible to deal with earthly aspects of Chemistry in a philosophical way. But this would be simply false. Instead, we can only point out that the influence of constructivism is such that it re-values artifactual materials as a necessary component of chemical research and at the same time it devalues the importance of theory as the main object of any epistemological reflection on the sciences, without these two posits the philosophical approach to Chemistry would have been extremely complex. The language of Chemistry is firmly and in its greatest part made up by mass terms. These terms have been received little attention not merely by linguistic philosophy of science but by philosophy of language in general. Only isolated and obscure cases can be enumerated, the case of Pelletier (PELLETIER, 1979) is beyond any doubt the most remarkable. To access Chemistry via experimentation is also difficult: chemical experimentation is not easily reproducible; it shares instead an innovative and emergent character that other sciences do not have in this level and degree; they seem to have it more on the level of theories. And finally the role of models in Chemistry is much more iconic than in other scientific disciplines. Models in Chemistry occupy the place of substances that are mixed both to produce a bond and to produce a new substance.

A model in Physics requires basically an interpretation of the axioms or theorems of the theory, which allows for a vision of the world. Chemistry works on a level with the "world". There is no way to interpret the world for Chemistry in the absence of the material substances or artifactual materials it studies. In this sense, its relation with the so-called pre-Socratic naturalist philosophers is truly refreshing in the world of contemporary history and philosophy of the sciences: the question Galileo rejected when he founded modern science, "what" is the world, and which he turned into "how" is the world, was dealt with always in connection to visual perception. Chemistry has asked for a much longer time: what is the world made of. Of water, answered Thales; of four elements replied Empedocles, and modern science, consistently, replies with a diversity that grows in geometric proportion by the constructive power of chemical experimentation, particularly organic chemistry, and because of the receptivity that every chemical substance has always enjoyed. It is obvious that Chemistry works with substances isolated from nature and it obviously work with artifactual materials. The property these substances share is precisely that they have been constructed or isolated in a laboratory. This feature, possibly trivial for chemists, is however very problematic for philosophers, particularly for those who detest materialism or who consider that materialism is a form of realism and not the other way around. This is to say that materialism implies accepting that there exists a logical (or necessary) connection between materialism and realism (or *chosisme*): the assumption of independence. We owe this logical mistake in the new (historical) materialism tradition to the duo Engels-Lenin[5]. In fact materialism is applied to both what is independent and what is dependent on something human, the important feature for materialism is not the independence from that which is an object of study for science but instead *its power of autonomization from the conditions under which a chemical substance, for instance, is produced in a laboratory*. And this can be abundantly observed in Chemistry.

But needless to say the philosopher and historian of Chemistry, a materialist (she could hardly not be one) must face like Diderot and Bachelard, or like Marx and Engels in their *Thesis XI on Feuerbach*, a much harder task: to turn scientific and technological practices into objects, etc., that can be worthy of philosophical analysis:

> [...] we can see that the idea that there are two kinds of science - theoretical and practical - is nothing new. Indeed, Diderot explicitly favored empirical sciences that relied on the work of the hand over pure theory, condemning the construction of theoretical systems as ultimately fruitless. Nevertheless, in the course of the last two centuries, the rise of modern physics has promoted pure theory over other forms of science, making it natural to characterize those that rest at the level of practice as impure if not degenerate. Of course, considering chemistry as impure is ironic in light of the fact that one of the central goals, if not the major obsession of Chemistry, has been to purify substances. (BENSAUDE-VINCENT & SIMON, 2008, p.5)

Finally, the central thesis I defended here is that constructivism as defined by the anthropologists of science is a disguised way of sliding historical materialist philosophy into the mainline of philosophical and historical discussion of the sciences and technologies and that those restore the role played by the materials used by sciences to be exactly what they are.

On the other hand, the notion of chemical substance vindicates the priority of materials *vis à vis* individuals, and underscores the difficulty faced by historical and specialized languages to deal with materials that might not be countable unless they be separated in parts - with the usual risk of not making the sum of the parts coincide experimentally with the whole. A clear redemption of potency before act and of material before form (SCHUMMER, 2008).

NOTAS

1. For the purposes of this article, I must offer almost caricaturesque summaries of the philosophical positions I present. In fact, with the exception of Chapter 2, I include this overview of some of the ways of philosophizing in history and philosophy of science and technology for methodological rather than epistemological reasons.

2. By *materialism* I understand, following the definition offered by Ajdukiewicz: "historical materialism posits that matter, during its historical development, changed form, and became richer as it adopted qualities that are irreducible to the qualities it originally possessed. Original inert matter, in which only chemical and physical processes would occur, gains in some of its parts, in which these processes acquired a high degree of complexity, a new quality, irreducible to the chemical-physical qualities, the quality we call life" (AJDUKIEWICZ, 2002pp 120-121).

3. "What Bachelard calls the substantialist or *chosiste* obstacle to science is rooted in man's spontaneous tendency to conceive any process as the gradual manifestation of the attributes of one or more lasting and independent things taking part in it. It is conceivable that the stability and comparative hardness of the solid objects that primitives encounter as stumbling blocks, or wield as tools, or capture or collect as food, have contributed to entrench this tendency in human thinking, language and behavior. However, it is likely that its source lies deeper, that its very usefulness in the struggle for life gave it a hereditary advantage. Christian Aristotelianism, which assimilates the universe to an aggregate of substances, any one of which can be annihilated by God without altering the others, is, so to speak, the climax and the paragon of *chosisme.* This explains the dire straits to which churchmen educated in this philosophy have been reduced, since the 17th century, by the advent of modern science. I believe that the most extreme expression of this philosophical idea is contained in a famous passage by Leibniz: "Si j'étois capable de considerer distinctement tout ce qui m'arrive ou paroist a cette heure, j'y pourrois voir tout ce qui m'arrivera, ou qui me paroistra a tout jamais; ce qui ne manqueroit pas, et m'arriveroit tout de meme, quand tout ce qui est hors de moy seroit detruit, pourveu qu'il ne restat que Dieu et moy (*Discours de métaphysique*,§ 14; Leibniz, GP, 4: 440)." (TORRETTI, 2013 – free English translation provided by the author)

4. The year of publication of the first, and so far the only best seller of Philosophy of Science: *The structure of scientific revolutions*, of Thomas S. Kuhn.

5. I am only referring to the most classical philosophical texts of both authors: *Materialism and empirio-criticism* in the case of Lenin and *The dialectic of nature* in the case of Engels.

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