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The Writing of the History of Science from the Notion of Scientific Field

Ivã Gurgel[1]
Graciella Watanabe[2]

Abstract:

Does science depend on its historical context? Does understanding science as a social construction demand us to abandon rationalist perspectives of knowledge? Based on these issues, this article aims to discuss epistemological questions concerning the problem of the historicity of sciences. In first part, we analyze how different philosophical systems conceptualize this problem and point out to tensions that emerge when one tries to reconcile a rationalist with a historicist perspective of knowledge. Then, we discuss the sociological epistemology of Pierre Bourdieu arguing that the field autonomy is a key concept to understand what the author denominates the "social conditions of the progress of reason". Finally, we present criteria to delimit the most relevant contexts in a case study on the history of science.

Keywords: Pierre Bourdieu; Scientific Field; Epistemology; Historiography of Science

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Introduction

Whenever science is affirmed as a historical construction, it is assumed that this is a precise and non-controversial definition. The defense of the historicity of science and its social character is opposed to positivist and empiricist views, denying science as a cumulative process of objective statements about the world. Constructivist views of the sciences comprehend that "scientific knowledge is a human creation, made with available material and cultural resources, rather than simply the revelation of a natural order that is pre-given and independent of human action" (Golinski 2005 [1998], 6). However, to assert that sciences happen within the historical time, bearing the marks of the places where they are produced, that they are diverse and are not reducible to a method exercised by a genius, is

[1] Ivã Gurgel [Orcid: 0000-0003-4968-6907] is a Professor in the Institute of Physics at the University of São Paulo – USP. Address: Rua do Matão, 1371 – Cidade Universitária, São Paulo – SP 05508-090 – Brazil. E-mail: gurgel@usp.br
[2] Graciella Watanabe [Orcid: 0000-0001-6710-0194] is a Professor in the Centre of Natural and Human Sciences at the Federal University of ABC. Address: Avenida dos Estados, 5001 – Santa Terezinha, Santo André, SP 09210580 – Brasil. E-mail: graciella.watanabe@ufabc.edu.br

related to the claim that the "Truth (in any precise philosophical sense) is not a product of science" (Shapin 2010, 5).

One may consider beneficial avoiding scientificist postures and to think science as regarding all of its human restraints – individuals and collectives. Nonetheless, epistemological questions whose focus is the validity of the scientific knowledge may be raised. Does abandon the "Truth" mean to renounce the differentiations regarding the limit of validity of knowledge? Is there something that qualifies the truths in the sciences and distinguishes them from other cultural manifestations? Does historicize science comprise it as resembling all other practices inserted in human history? Thereby, considering science as a historical and social construction does not cease a debate, but, on the contrary, urge for questions to be answered.

Borrowing a term that gained identity in the work of François Hartog (2015 [2004]), queries related to the previous ones involve inquiring what are the possible *regimes of historicity* admitted by each one of the different areas that composes the sciences. It is necessary to question if there is a unique dynamics that allow us to understand the changes within time. This implies doubting both the possibility of having a single way to interpret how the macrosocial changes occur (that is, the patterns that guide the "great flow of history"), as well as the existence of unitary theoretical matrices for specific social microcosms. We can ask ourselves if the history of sciences always develops by the same general standards. Would it be possible to make an abstraction exercise concerning concrete situations – namely, that took place in history, relativizing the characteristic and social positions of the involved characters – in which one could verify the most general way that sciences are constituted in time?

To think about how science is inserted in history is also a reflection about the necessary condition for the production of knowledge and its limits of validity. Would specific social environments, which are constituted in history, admit their own rationality scheme? Discussions of historiography involve philosophical questions, and one finds unavoidable not to transit between both fields. As pointed out by Mauro Condé:

> historiography of science (...) places itself among history and philosophy of science, as it is never a mere photograph of the different manners of how science was written by historians, but invariably presupposes an epistemological conception behind its models, goals, limitations, possibilities, etc. (2017, 19)

This article aims to discuss intrinsic epistemological questions to the problem of the historicity of sciences. In particular, we seek to analyse the validity of correlating historicist views of sciences with relativist perspectives. Does understanding science as a social-historical construction demand us to abandon rationalist perspectives of knowledge? As a complement to this question, we shall discuss some views of how the relationship between science and its context is given. To what extent does science depend on its social-historical context?

Clearly, we do not hope to exhaust such questions. In the same way, we do not intend to deny the importance of contributions made to them in the last few years. Contextualizing this own work, it is important to mention that sciences are going through a singular moment. Despite the existence of anti-science movements throughout different epochs, their political strength has never been so strong (Thompson and Smulewicz-Zucker 2018; Bensaude-Vincent 2003 [1999]). This compels us to think about how to bring forth a speech about sciences that at the same time do not mask and hide their nature, but also that do not relativize them to the point of withdrawing its social role.

In the next section, we revisit some philosophers of knowledge in order to reconceptualize further addressed issues. Through the analysis of how such philosophers considered the historicity of science, we will seek to identify tensions that emerge when one

tries to reconcile a rationalist understanding with a historicist perspective of knowledge. Then, we make a brief plunge in the work of Pierre Bourdieu in order to characterize, from his epistemology and sociology, what the author denominates *the social conditions of the progress of reason* (1975). Finally, we resume the historiographical debate to ponder how Bourdieu's work helps us to think researches in the history of the sciences. In particular, we discuss how to delimitate the most relevant contexts in a case study on the history of science.

The Validity of Knowledge and the Problem of Historicity of Science

The thinking about how science is constituted in time is not something new. As much as the answers that several thinkers had presented over time do not contemplate problems that are currently imposed, the examination of their works helps us to think about ways that lead us to face them; either by updating their projects or by consciously denying them.

It is common to present positivism as a *non-historical* philosophy. However, in the work of Auguste Comte (1798-1857), in particular, in his *Course of Positive Philosophy*, published between 1830 and 1842, we can find a project concerned with the history of knowledge. The starting point of his positive philosophy is the study of the "progressive march of the human spirit, considered in his literary ensemble, given that it is only possible to properly know any concept through its history" (2020 [1842], 60). One of the foundations of Comte's philosophy is his Law of Three Stages, in which, "each of our main conceptions, each branch of our knowledge, goes through three different theoretical states successively: the theological or fictitious stage, the metaphysical or abstract stage, and the scientific or positive stage" (2020 [1842], 61). The latter, in contraposition to the formers, is accomplished by renouncing the attempt of characterizing the innermost causes of the phenomena. It seeks, through reasoning and observation, the discovery of "the invariant relations of succession and similitude" (2020 [1842], 62).

Comte marks the beginning of positive science in the works of Francis Bacon, René Descartes, and Galileo Galilei, the same period established as the beginning of modern science. He recognizes the "impossibility to strictly determine the origin of this revolution" (2020 [1842], 73), but balances that in these authors a project to develop the true knowledge is evident. For a certain knowledge reaching its positive stage, its development would follow the guiding principles of this philosophy, with no further epistemological reviews. Comte also questions whether all categories of phenomena had already been subjected to a positive understanding. In his judgement, the study of social phenomena would be lacking, leading the author to propose a *social physics*.

What reigns in Comte's work is a progressive view of the history of science. The stages of knowledge would evolve naturally, as much from the point of view of one's thinking, as from the point of view of a collective society. Here, history refers to the necessary antecedents to reach a certain maturity, with no room to think about the historicity of science in terms of its contextual elements. In other words, knowledge would not carry characters that demonstrate its link with the social forms of organization of an epoch. What interests Comte is to characterize the progress of knowledge so that it serves as a model for the evolution of societies. The opposite path, that is, the characterization of social conditions for the progress of knowledge is absent in his work. The vector of science to society is imposed.

Philosophical movements from the 20th century, close to Comte's positivism, go through similar paths. Hans Reichenbach (1891-1953), who became known for the distinction between the contexts of discovery and justification (Reichenbach 1938), elaborates a narrative in which science is viewed as the accumulation of observations and experimental results that would allow the construction of theories increasingly abstract. By discussing the

historical process of the construction of space and time concepts, the author comments that both have "little connection with economical needs; always dealing with abstract things, far from our daily life, without direct influence on our everyday activities" (Reichenbach 1980 [1942], 11). Reaffirming his logical empiricism, Reichenbach situates in Michelson-Morley experiment the origin of the special theory of relativity (Reichenbach 1980 [1942]).

It is not about denying the relevance of experiments in the sciences but to comprehend its meaning on an epistemology. Empirical-positivists traditions narrow the knowledge to the observable and measurable by considering them as objectives. Nonetheless, different authors, of which we highlight Norwood Hanson (1958), argue about the inseparability between observation and interpretation. Current studies still stress that the meaning of an outcome of an experiment goes through the process of peer negotiation. As pointed out by Peter Galison, "experimental physics cannot be rewritten as a logical fantasy in which all theorizing is forbidden until 'facts' clinch the argument. Nor can experimentation be parodied as if it were no more grounded in reason than negotiations over the price of a street fair antique" (2018 [1987], 277). Hence, it is not appropriate to attribute to an experiment itself the validity of knowledge. It is necessary to understand how this validity constitutes itself and builds the place of legitimacy in the speeches and actions of agents who play specific roles in the production of knowledge.

Throughout the 20th century, many epistemologies will oppose themselves not only to positivism but also to different foundationalists philosophical systems. What is broader in this movement is the attempt to build an epistemology that would not establish a prior set of norms for knowledge. Thereby, authors come to value history as a basis to comprehend knowledge.

Gaston Bachelard (1884-1962) elaborates an epistemology in which knowledge is analyzed depending on the individual, that is, in his work knowledge and thought are inseparable. One of the questions that pervade his work is the one about the conditions of possibility of knowledge, forcing us to question the scientific (spirit) mindset. Bachelard develops a philosophy where science is the result of a process on which thinking itself needs to be constantly surpassed. The production of knowledge by a determined way of thinking is exhausted with time, becoming an obstacle to new ideas (Bachelard 1993 [1938]). Thus, the development of science would not be reduced to a cumulative process of new concepts, theories, etc; it would be founded over a continuous practice of a same philosophical programme, as pursued by the positivists. The scientific knowledge would come from epistemological breaks, from a process of denying previous knowledge (Bachelard 2012 [1940]).

If Comte considered science as the culmination of a process, from which an epistemological pattern would remain constant, Bachelard historicizes scientific knowledge differently, by placing the inherently human thought in focus. However, Bachelard's reduction of the individual to its cognitive dimension is criticized, for example, by Marxist philosophers (Lecourt 1975) who see lacking of historicizing in his approach. The reason progresses according to the internal problems of science, without it being characterized by the context or the social dynamics in which it is inserted.

Bachelard's epistemology is composed of texts in which the history of science is under analysis. After having written his main works in 1951, the philosopher gives a conference at the Palais de la Decouvert, in Paris, in which he reflects on the writing of history. Entitled *The Present of the History of Sciences*, the author considers the relationship between the present and the study of the past. Aware of being against the current thinking among historians, Bachelard says that the historian must judge the past. To understand the history of the sciences is to explain the value of each new thought for its development. In his own words:

> the history of sciences is essentially a judged history, judged on the details of its plot, with a meaning that must be permanently tuned by the values of truth. The history of

> sciences cannot simply be a history of records. The minutes of the academies naturally contain numerous documents for the history of sciences. But these minutes do not really constitute a history of sciences. The historian of sciences must draw, from them, lines of progress. (2010 [1951], 209)

The development of science is given through the progress of reason. Despite that, Bachelard does not look for an ultimate definition of rationality, given its constant changing and, consequently, historical nature. Its evolution is associated with the problems on which it is applied, avoiding it from relapsing in empty and idealistic reasoning (Bachelard, 1966 [1949]). Nevertheless, its rational materialism does not go beyond the dimension of the individual, which makes reason its immanent capacity. Thus, there is no foundation of knowledge that is at the social level of the sciences. In summary, the history of science in Bachelard's work is a history of how the rational thinking has changed when facing problems whose solutions would demand to go beyond the first impressions we may have about the world.

In the third quarter of the 20th century, the problem of the scientific change and the possibilities of a model for scientific development became central themes in the epistemological debate. In this context, one finds Thomas Kuhn and his *The Structure of Scientific Revolutions* (2012 [1962]). The normal science is the research firmly based on one or more past scientific achievements (2012 [1962], 19). In it, scientists generate knowledge from the same theoretical remarks, experimental procedures, data interpretation systems, etc. Aspects often less conscious on a scientist's mind are also shared, as the epistemological assumptions guiding their work.

When a paradigm starts to present many anomalies – problems it is unable to solve – a revolutionary process begins. The same is completed when a new paradigm begins to hold in the scientific community. Thus, Kuhn's historical epistemology is also based on ruptures in the process of knowing. Still, his view differs from that of Bachelard since the former does not look at science focusing on thought. One of Kuhn's key aspects, which influenced subsequent generations, was to discuss science from its collective character, that is, from the scientific community.

Kuhn does not quite elaborate a theory of history, leaving open questions raised from his epistemology. On the one hand, the emphasis given on the scientific community means that the epistemology developed in the *Structure* does not reveal the importance of contextual aspects in science, that is, which bring elements external to the sciences. On the other hand, this same focus is associated with relativist assumptions, particularly as the author claims the immeasurability of paradigms and their non-progressive character. Therefore, scientific change is guided by a social process that is not necessarily restricted to epistemic values.

One of the main epistemological confrontations on the problem of the scientific change was realized by Imre Lakatos (1922-1974). A rationalist philosopher inspired in Karl Popper's work, Lakatos seeks to elaborate an interpretation of history where scientific revolutions are seen as rational progress of the sciences. His main concern is precisely to avoid relativist theses which might lead to an understanding of the scientific change as a process in which members of the scientific community are converted to a new way of thinking.

Lakatos's concerns led him to develop the methodology of scientific research programmes. These can be characterized by their hardcore, which cannot be falsifiable and "is conventionally accepted" (Lakatos, 1980a [1970], 46). It is complemented by the protective belt that forms a positive heuristics "consisting in a partially articulated set of suggestions, or cues, on how to change and develop the refutable variants of the research programme, and how to modify and sophisticate the refutable protective belt" (1980a [1970], 51). If the adjustments on the belt broaden the theory and accommodate new

experimental data, the research programme can be considered progressive. Still, if "its theoretical growth is delayed" (1980a [1970], 77), in other words, the explanation of new facts demands *ad hoc* hypotheses, then the programme has degenerated. Within such perspective, a scientist's adhesion to a research programme becomes something based on a rational criterion, opting for the most progressive one, with the greatest scientific legitimacy at that moment.

One of the greatest difficulties in Lakatos' epistemology is determining the dimensions, including historical ones, of research programmes. If we think about a "longue durée" history, we can define science as a vast research programme. In this case, it would never be abandoned, as it would be constantly in progress. If, as opposed to that, we opt for a micro-history, an anomaly can be already interpreted as a decline symbol, that is, the programme is degenerating and must be forsaken. What would then be the intermediate historical dimension allowing a fair evaluation? Lakatos partially answer this question in his essay *History of Science and its Rational Reconstructions* (1980 [1971]).

Lakatos comprehends that science develops under the most diverse contextual influences; however, it is possible to separate between internal history from the external one. The former is attentive to (1) the successes and failures of the research programmes that were important for long periods of time; (2) to their progressive and degenerative changes; (3) to their rivalries and (4) to the *slow emergence of the victory of one programme over the other*. The external history would be composed by the non-rational factors that may (or may not) have influenced the development of science.

> The history of science is always richer than its rational reconstruction. But rational reconstruction or internal history is primary, external history only secondary since the most important problems of external history are defined by internal history. External history either provides a non-rational explanation of the speed, locality, selectiveness, etc., of historic events as interpreted in terms of internal history; or, when history differs from its rational reconstruction, it provides an empirical explanation of why it differs. But the rational aspect of scientific growth is fully accounted for by one's logic of scientific discovery. (Lakatos 1980a [1971], 118)

The separation, as done by Lakatos, puts back, in a new way, the problem of demarcation in the history of sciences. The appraisal of the scientificity of knowledge or the judgement of the rationality of a decision is accomplished from a historical perspective, that is, in relation to the development in the time of a research programme. It is necessary to "draw a line" in history, separating the internal from the external, ascribing secondary importance to the later. Lakatos' work is relevant for setting in the collective and historical plane the exercise of reason. Nevertheless, this is done for the price of decontextualizing science, making abstract the scientific practice.

Internalism and externalism are two historiographical approaches that marked the debates on the history of sciences in the middle of the 20th century, especially in relation to the debates about the emergence of modern science in the 16th and 17th centuries. The main representative of internalism is Alexandre Koyré (1892 - 1964), a historian with a philosophical training who follows the tradition of French rationalism. Science consists of the production of rational theories about the world, following the Galilean understanding which comprehends that the "book of nature" is written in a mathematical language. For Koyré, the mathematical rationality is not just an epistemological characteristic of the physical sciences, but also its foundation and what legitimizes them (Koyré 1985 [1966]).

Similarly to Bachelard's understanding, Koyré's history of science is the history of scientific thought. The narratives produced by the author seek to reveal, as much as possible, the reasoning behind the production of the ideas that founded mechanics and gravitation. The explanation of its emergence is limited to the philosophical assumptions that underpin

scientific thinking. Factors linked to the social changes that occurred in the Renaissance would be secondary to the understanding of the origins of modern science.

Externalism brings together authors from different times. In his work, *The Social and Economic Roots of Newton's 'Principia'* (1931), Boris Hessen (1893-1936) presents a Marxist reading of history of science, in which the work of the English scientist would be due to the structural changes that led to the advent of capitalism. Some years later, Edgar Zilsel (1891 - 1944) elaborates the thesis that will carry his name. Modern science is the result of economic changes associated with the origins of capitalism and the technologies and practical knowledge involved in the material production process. This context, associated with the increasing population of cities and their new modes of organization, "enabled the junction between the theoretical knowledge of the philosophical tradition and the practical one, of artisans and artisan-engineers, hence establishing modern science" (Condé 2017, 32).

The externalist view is, sometimes, still related to the Marxist philosophy. Underneath it prevails the critique that the modes of economic production deterministically impose themselves on the plan of the cultural productions. However, in the second half of the 20th century, works concerned with the influence of external factors were developed based on other theoretical foundations. By analyzing them in a quite broad manner, one verifies a marked presence of the *cultural studies*, and these authors can be grouped into what has come to be known as *science studies*. This area gathers studies with very different philosophical characteristics, but which are grouped by sharing some similar principles.

The *science studies* can be considered an "anti-philosophical system", or even an "anti-epistemology". This does not mean that its studies do not address philosophical and epistemological questions. Its research seeks to avoid essentialist perspectives on science, as the latter would establish a unifying answer to queries about what science is. For example, it refuses to elaborate models for scientific development, which to some extent end up having normative pretenses, as did Kuhn and Lakatos (Pestre 2006). This withdrawal from the philosophical tradition is the result of an attempt to understand in detail the conditions of the scientific practice, in order to reveal all the human dimensions involved. Accordingly, the focus of analysis is no longer theories, models, and scientific concepts, but its practice, the science in action, as defined by Bruno Latour (2005 [1987]).

The sciences seen as a practice involves valuing the most different dimensions of knowledge production. Therefore, the study of a historical episode may involve the theoretical and experimental procedures employed in the construction of knowledge; the material culture involved, that is, the nature of the instruments used and their own manufacturing processes; the forms of publication and reading, taking into account the processes of circulation of ideas and the disputes, litigations and controversies involved in their validation; the cultural and ideological determinations of the scientific practice; the institutions, their modes of organization and their funding; the policies that govern the field and determine its forms of dispute; among others. To sum up:

> The studies on science and science practices that have made history in the last decades have denaturalized the object "science", they have de-essentialized it, de-idealized it. It is postulated that there is no evidence that the science object exists identical to itself over time, that its identity is unproblematic. (Pestre 2006, 6)

One of the most influential works in the sciences studies is *Knowledge and Social Imagery*, by David Bloor. This work founded the so-called Strong Programme in the Sociology of Scientific Knowledge, which claims the place of the analysis of knowledge until then occupied primarily by epistemology. Distancing from the definition of knowledge as a justified true belief, "the sociologist will be concerned with beliefs which are taken for granted or institutionalised, or invested with authority by groups of people" (Bloor 1991 [1976], 5).

Bloor elaborates four tenets that should govern the explanations about knowledge. "It would be causal, that is, concerned with the conditions which bring about belief or states of knowledge (...); impartial with respect to truth and falsity, rationality or irrationality, success or failure (...); symmetrical (...) The same types of cause would explain, say, true and false beliefs; (...) [and] reflexive. In principle, its patterns of explanation would have to be applicable to sociology itself" (1991, [1976], 7).

With the strong programme, Bloor hopes to achieve a radical historicization of knowledge. The social context would not only affect the institutional daily life and the ways of organizing science; the cognitive content itself should be explained by it. The epistemological characteristics of knowledge would be explained by the constraints of an epoch. What Bachelard called judged history should also be avoided, as the scientific achievements of an epoch should not be judged by current epistemological evaluations. This is particularly important because many episodes show that what we could consider as right or wrong occur simultaneously in the same practice by the same agent. Explicitly disrupting with Lakatos, Bloor rejects the possibility of drawing a line separating internal and external history. Thus, research in history must be able to show that the same influences that led the scientist to make a mistake must explain his successes.

Bloor's work is not an isolated example of a project for understanding science. The strong programme was influenced by several types of research, especially in the history of sciences, and influenced many others. Besides, other proposals appeared at the same time and in later periods, such as Bruno Latour's actor-network theory. However, at the same time that constructivist perspectives, developed since the 1970s, allowed to humanize and put science in context, they raised questions that remain open.

For the current work, two issues are central. The first consists of the relationship between historicity and relativism. Does understanding science as a social construction imply adopting a relativist position? In other words, does presenting science with its human restraints make it necessary to abstain from epistemological questions that qualify knowledge? The most common positions have responded positively to such questions. This position has played an important role in the last decades for having allowed, as previously mentioned, to denaturalize, desentialize and deidealize the sciences. However, they left open epistemological issues that are important when one considers the role of sciences in democratic societies.

Secondly, it is necessary to question what are the dynamics that drive the history of sciences. That is, is it possible to ponder the different influences present in scientific practice? These questions reflect the first inquiry raised by Peter Galison in his "Ten Problems in History and Philosophy of Science". By retaking the "intellectual civil war" (2008, 112) between internalism and externalism, and reviewing it for our times, the author comments on two main ways of considering the context of a work in its analysis. Would the context be the intellectual production of authors around the analyzed work, as philosophers prefer, or rather, as historians indicate, the "non-textual" factors, that is, political, institutional, industrial or ideological?

> What kind of thing is a candidate for context? Further: Is a contextual explanation as strong as a causal account (…)? Is a contextual explanation as weak as saying that the surround offers "resources" taken up by the scientists we are studying? (…) In short: What is context, and how does a contextual explanation work? (2008, 113)

The same work, character, or collective scientific practice is immersed in different contexts. The scientific context is delimited by agents who work with similar theoretical and methodological assumptions, who develop practices of the same nature, and who collaborate or compete with each other. There is a common microcosm that unites different actors. One can also think of a broader intellectual environment, where there are mutual

influences among scientists from different areas, but also between scientists and artists, or, for example, the inspirations of a physicist that occurred when reading a philosophical work. This context can be extended to a wider and ethereal cultural plan, in which the context is formed by social imagery as well as social behaviours. In addition to these, it is possible to think about the macro contexts, which involve different organizations and social hierarchies, different forms of government and political agents and, finally, modes of production and circulation of goods. Then, the following questions arise: are these different contexts – scientific, intellectual, cultural, social, political, and economic – present in the scientific practice likewise?

As already mentioned, questions like these do not wear out at work. Nevertheless, it is possible to point paths from the analysis of the work of different authors. In the next section, we aim at treating those from the perspective of Pierre Bourdieu's relational sociology. An author with philosophical training, he became a sociologist in practice but never waiving concerns regarding the foundations of knowledge. The search to explore this dimension in his work is still overlooked, which makes it relevant to reposition it in relation to the question here presented.

Pierre Bourdieu's Sociological Epistemology

In 1975, Bourdieu publishes the article "La spécificité du champ scientifique et les conditions sociales du progrès de la raison", which is reprinted one year later with few modifications and a reduced title, "Le champ scientifique". In these papers, we already find the complete project of what we may call his sociological epistemology of science,[3] which would be detailed in his posterior works (1991, 2001), in particular in his last course at the *Collège France*, published in the book entitled *Science de la science et la réflexivité* (2004). His works are part of the movement to renew the sociology of science, although it builds up a research agenda that seeks distinct and critical paths regarding his contemporaries.

According to Bourdieu. science, in relation to other fields of intellectual production, is the one who most claimed the distinction between internal and external. The internalist analysis "views the scientific practice as a pure activity completely independent of any economic or social determination; in contrast, external analysis views science as a direct reflection of economic and social structures" (Bourdieu 1991, 4). Overcoming this polarization involves thinking about how reason is historically constituted, since it would avoid both the *absolutist realism*, which considers that science represents reality as it is (or provides the closest representation of it), and the *historicist relativism*, which takes science as a conventional social construction, "reflecting the objective structures and the typical beliefs of a particular social universe" (Bourdieu 1991, 4). The epistemological project requires a different approach from the one favoured by philosophers, which focuses on the analysis of the internal coherence of knowledge. At the same time, we need a new sociology of science that focuses on the analysis of the social conditions allowing the development of knowledge with specific features.

Bourdieu produced analyses on different fields and his work may be defined as relational sociology. According to Vandenberghe (1999), it is an attempt to convert Bachelard's applied rationalism of the natural sciences to the human sciences. The author presented it as a theory of practice whose goal would be to overcome false oppositions in the social sciences, embodied by the objectivism-subjectivism antagonism (Lentacker 2010). The *habitus*, field and capital are the key concepts of Bourdieu's sociology (Tampakis 2016).

[3] With this denomination, we highlight that Bourdieu's works, as they seek to characterize and analyse the validity of the scientific knowledge, constitute an epistemology. At the same time, the author locates its foundations in the social conditions of its production.

Bourdieu's field theory seeks to comprehend the dynamics governing the social microcosms which are not reducible to the objective aspects of the functioning of the social space. In this context, understanding science means to understand how this field works explicitly and implicitly, and how its rules lead to the creation of specific symbolic productions. In particular, one must understand the elaboration of scientific truths, these "social products relatively independent of their social conditions of production" (Bourdieu 1976, 88). Thus, it is necessary to unveil the rules of the social game of science in order to specify how this dynamics allows the production of a specific truth. Bourdieu's interest is to provide the basis for a *truly historical approach to the constitution of the scientific field* (Gingras, 2009, 283).

> The scientific field is a separate world, apart, where a most specific social logic is at work, affirming itself more and more to the degree that symbolic power imposes themselves that are irreducible to those that are currently in the political field as well as to those instituted in the legal or theological field. (…) The scientific field is a field of forces whose structure is defined by the continuous distribution of the specific capital possessed, at the given moment, by various agents or institutions operative in the field. It is also a field of struggles or a space of competition where agents or institutions who work at valorizing their own capital – by means of strategies of accumulation imposed by the competition and appropriate for determining the preservation or transformation of the structure confront one another. (Bourdieu 1991, 6-7)

Analyzes of the scientific field reveal a complex social game formed by struggles for the scientific authority and search of the symbolic recognition of peers who, in turn, rule and limit participants through specific practices and knowledge. Therefore, those who are able to deal with the problems considered as real in their fields are accepted in the game of sciences. The scientific *habitus* is initially acquired by exposure to the *modus operandi* of science present in pre-determined social spaces, such as schools. These ways of thinking, perceiving and acting are consolidated by experiencing different places where science takes place, such as laboratories, seminar rooms, public congresses, etc. Therefore, one acquires corporeal and sensitive pre-dispositions for the practice of science.

Every agent will accumulate, throughout its historical trajectory, capitals of different nature (social, cultural, etc), which will lead him to acquire a position in the scientific field. The higher the position in the field, the grater the possibilities for intervention in the "rules of the game". Bourdieu (1991, 1976) insists on the inseparability between what is scientific and what is social, but recognizes two components in the scientific capitals. One would be the symbolic recognition of scientific authority (pure scientific capital) and the other would be the recognition of social authority (political or temporal scientific capital) (Bourdieu, 1991; 2004; 2019 [1998]). The first involves the recognition by its "peer-competitors" of a distinct competence in solving legitimate problems of the field. This is done, for example, in publications in prestigious journals. The second, a less representative component of the autonomy of the field, may be exemplified through management positions in different institutions or actions in the public sphere. These agents who are in privileged positions have great power in defining what is and what is not scientific, which implies intervening in the very own rules of the game of science.

A determining factor in the Bourdieusian theory, that aims to constitute a counterpoint to Robert Merton's idea of "scientific community", refers to the autonomy of the field and its conditions of entry. For Bourdieu (2004) the Mertonian idea of community needs to be surpassed, thus, "to speak of a field is to break with the idea that scientists form a unified, even homogeneous group" (Bourdieu, 2004, 52). Scientists do not follow the same scientific *ethos* but dispute the rules that regulate the production of knowledge. For the author, the

Mertonian idea of a scientific community would lead to the understanding that entering the field and playing its game would not have any kind of criteria regulated through the recognition of its peers. This view would lead to mask all types of scientific practices and struggles based on an opposition process between consensus and conflict (Lombardo, Sabetta 2018). The permanent struggles in the field allow it to acquire a structure to the dynamics of operation that makes the actions within it highly regulated. You cannot "win" a dispute by acting in a way that has not been previously validated. The sciences only recognize what is produced by means of their criteria of validity, making it an autonomous microcosm in relation to the other sectors of society.

> Autonomy is not a given, but a historical conquest, endlessly having to be undertaken anew. This is easily forgotten in the case of the natural sciences, because their autonomy is inscribed both in the objectivity of the structures of the field and also in scientists' minds, in the form of theories and methods, incorporated and returned to the practical state. (Bourdieu 2004, 47)

The autonomy in the field of physical sciences appears, in particular, through a historical process that mobilizes the very way in which scientists built their theories. Evermore committed to the mathematical logic and with the interpretation given to the data obtained by technically sophisticated experiments, they preclude those outside the field to opine on scientific problems, as they are deprived of the symbolic resources (cognitive predispositions) necessary to the exercise of the scientific authority. These are the specificities that transformed the field of science, which ceased to be heteronymous, as in the Copernico-Galilean period when it was strongly influenced by the Catholic Church.

> First, competence: this means not only mastery of existing knowledge, of the resources accumulated in the field (mathematics in particular), but also the fact of having incorporated all the theoretical-experimental (that is to say, cognitive and material) resources resulting from previous research, transforming them into a practical sense of the game, converting them into reflexes. (Bourdieu 2004, 51)

This process leads to a complex acquisition of capital which defines the positions and practices (*habitus*) that place and legitimate the taking of positions and influence of the field of science. The scientific capital is a symbolic one, whose long-term goal is to promote the visibility of those who own it, that is, the weight of their symbolic capital in the power game (Bourdieu 2016). This type of capital is legitimated by the knowledge and recognition by peers, exclusively validated by the scientific field. The weight of this symbolic capital varies according to the distinctive value of the contributions and originalities that each scientist acquires and that can only be recognized and validated by his peers in the scientific field (Bourdieu 2004). Therefore, the relationship between capital and field is characterized by the way these instruments of symbolic recognition and validation act in the relationship web between agents that define, in a more straightforward manner, the positions in the scientific hierarchy. The more a field is autonomous, the more the capital is unevenly distributed, ensuring that those operating in positions with greater scientific prestige are also those who accumulate more capital which, in turn, generate profits in the field of forces.

> It follows that the more autonomous a field is, the more the hierarchy according to the distribution of scientific capital is differentiated, event to the extent of taking an opposite form to that of the hierarchy by temporal capital (...) Judgements of scientific works are contaminated by knowledge of the position of the authors in the social hierarchies (and the more heteronomous the field is, the more this is the case). Thus, Cole and Cole show that, among physicists, frequency of citation depends on the

> university to which a scientist is attached, and it is known that a researcher's symbolic capital, and therefore the reception of his work, depends in part on the symbolic capital of his laboratory. (Bourdieu 2004, 57)

The scientists themselves are who defines such struggles, in the face of conflicts and interests in the field, due to their high autonomy in the natural sciences. For this reason, participation in this field can only be evaluated and legitimated by peers, and, consequently, analyzed by the possession of a capital acquired historically in the trajectory of this field that is constituted in the long-term learning of methods, techniques, *habitus* and thoughts which define the specialized knowledge of those who own it. The scientist does not present himself only as a scientist, but also as the representation of the field in its objectified universe of the relationships that regulate the productions and their agents (Bourdieu, 2004).

Unlike some social studies of science, Bourdieu's sociological epistemology is not limited to recognizing that the "scientific fact is won, constructed, observed" (Bourdieu, 2004, 72), but understands the scientific knowledge as the result of a regulated process, established among social agents (scientists) in relation to the object in question (scientific knowledge) (Bourdieu, 2004). There will always be a validation process of knowledge that cannot be treated only between the scientist and the object, but between scientists, their peers and the object. Recognizing this trend of the scientific process thus requires rethinking the state of the field, the choices, and the role that each agent plays in the scientific field and their ability to mobilize it (Bourdieu, 2019 [1998]). The memory and historicity of science can be represented either in the order of the symbolic or in the culture objectified in books, articles, documents, instruments, laboratories. Both produce a certain historical action by scientists that aim at representing laws and theories – which transcend the history and the individual experiences imposed in the trajectories of the social agents – that were developed through "the space of possibles (and of impossibles) that confronts any competent researcher" (Bourdieu, 1991, 12).

Bourdieu's sociology can be characterized as a theory of practice, in which one seeks to understand how agents act in the field. Thus, the scientist is someone who works in the scientific field, and his actions are modulated and guided by what he can obtain in this social microcosm. Just like in a chess game, the scientist's thinking operates in such a way as to anticipate which actions are able to bring the best outcomes. This explains the tendency of researchers to focus on problems considered to be of great importance by agents with a "high degree of legitimacy" (Bourdieu 1976, 90). By establishing dialogue with the work of Thomas Kuhn, Bourdieu will point out that an autonomous scientific field establishes, moreover, the problems that may be considered revolutionary. Even if they disrupt, in different aspects, with the science established up to that moment, the need for them to be submitted to the criteria of the legitimacy of the field makes revolutions limited to their possibilities of transformation. Science itself provides the "institutional conditions for rupture" (Bourdieu, 1976, 98).

> A decisive change occurs when censorship of those social drives that are not scientifically sublimated has been progressively incorporated in the structure of the field and in the mechanisms that control entry in it, and also, most importantly, when it has been implanted in specific resources that are more and more completely objectified in *formalized* (notably mathematical) procedures. Under these circumstances, a revolution against established science is carried out with the help of an institution that provides the instruments of rupture with that establishment: the field thus becomes the site of a *permanent revolution*, but one that is increasingly stripped of political effects. (Bourdieu 1991, 18)

Pierre Bourdieu's position, when analyzing the scientific field, is opposed to the studies of David Bloor and Robert Merton. If, on the one hand, it deviates from the structural-functionalist studies of Robert Merton, whose scientific community, its norms and values are aimed at the first end, which is scientific development (Merton 1988), on the other, it also criticizes the Strong Programme for its vision with a strong appeal to the cognitive context and little emphasis on the scientific one.

For the author, the criticism to the structural-functionalism is based on the idea that the recognition acquired in and by the scientific community cannot be reduced to an effort of social practices to reinforce, or to fight for, an imposition to a certain theory, but in the constitution of a symbolic capital that does not consist only of individual recognition, but in the relationship between the position of the laboratory and the scientist in the scientific field (Bourdieu, 2012). Likewise, the Mertonian approach does not refer to the way scientific conflicts are resolved. Although, for Merton, the Mathew Effect[4] could be an important constitution for improving the idea of recognition in the scientific community, as part of the "economic" practices of scientists, it does little help when it is recognized that the position in the social structure of those who have the knowledge, the theory or scientific data end up being decisive in the process of struggles for scientific authority (Bourdieu 2004).

On the other hand, the ways in which he understands the limits of the Strong Programme are centered in the critique of the interactionist views that comprise the relationships between agents as the principle of the scientists' actions. They ignore the structure of objective relationships that are historically established and, thus, prior to agents themselves, who become constituted by it. The scientific field, therefore, would be that place where the specific knowledge becomes fundamental for the aggregation of new players to play the social game of science, which limits the actions and the ways in which interactions can be part of many cultural and political interests, at the same time that they need to have the scientific rationality as the basis for structuring the relationships between these agents.

> But the limits of his work result from the fact that he remains enclosed within an interactions vision which seeks the principle of agents actions in the interactions between them and ignores the structures (or objective relationships) and the dispositions (generally correlated with the position occupied within these structures) that are the real principle of actions and, among other things, of the interactions themselves (which may be the mediation between structures and action. (Bourdieu 2004, 20)

Given the complexity of thinking about such practices, associated with the contemporary science studies, Bourdieu (2004) questions the logic of the scientific field, and how to deal with increasingly complex and expensive laboratories, which are only sustained through collective work (Bourdieu 2004). Such inference also seeks to build a direct relationship between the technical objects and the power they are supposed to have in the production of knowledge. If the scientist is the scientific field, represented by the cognitive structure that is homologous to the structure of the field and the expectations inscribed in that field (Bourdieu 2004), it is worth questioning how it would be to consider the logic between field, goals and agent. In that sense, an important part of this articulated process of thought, to respond to Pierre Bourdieu's research agenda on the social conditions of the progress of reason, that does not seem to have ended yet.

[4] Mathew Effect consists in a "cumulative advantage directs our attention to the ways in which initial comparative advantage of trained capacity, structural location, and available resources make for successive increments of advantage such that the gaps between the haves and the have-nots in science (as in other domains of social life) wide until dampened by countervailing processes" (Merton 1988, 606).

Bourdieu and the Writing of the History of Science

Bourdieu proposes a research agenda that allows the double truth of the sociologist's work – and, for the questions addressed by this work, of the historian's – whose methodological value is made in its reflexivity (Bourdieu 2004; 1991; 1975; 1997). Critical to different epistemologists and historians of science, he recognizes the difficulties in doing the "science of science" (Bourdieu 2004).

In an attempt to build a proposal of reading the world of science and, in a more general manner, of a society that is not static, the author seeks to depart from the structure-action dichotomy of social agents. The sociologist produces a relation between social space, power place and thought-action-practice that operate jointly to elaborate the understanding of the world and the positions taken by the agents in the social game (of the natural sciences). Therefore, the *habitus* takes action in the field through the mobilization, acquisition and capital value that each agent has in the social game under analysis (Vandenberghe 1999). Starting from these central concepts (yet many others, such as the *illusion* and the transubstantiation, are still necessary) is possible to recognize that, in his theory, nothing can be thought, act and built in the absence of a complex game of meanings and practices that are imbricated by the social field in which those who play the game are placed.

Bourdieu's interest in the scientific field has always sought to weight both sides of the balance between the heteronomy and autonomy of the sciences. His interests in the natural sciences were marked by Gaston Bachelard's epistemology as a consequence of his training at the Sorbonne with Georges Canguilhem (Gattinara 2018). The challenge of comprehending the scientificity of Physics as an articulated autonomy process of the field, which he imposed to himself, has rendered him reflections capable of helping the understanding of social aspects of the production of the social sciences. To Bourdieu, the mathematical thinking had an important role for the autonomy of the Physics' microcosm, whose scientists sought, throughout the history of their knowledge, a scientific authority that had influenced the struggles within the field, protecting it from interferences and external interests. This lesson has been learned in face of the impacts the Catholic Church had in the Copernican and Galilean studies, and that today are strongly ressurging in the studies of quantum physics and genetics (Supiot 2014).

Analyses of the development of the field enable the understanding of what is recognized as a problem and what is accessible by the intellectual conditions that define what is a true problem. This positioning in the place at which the scientist acts requires playing the game whose academic rules, strongly guided by the scientific authority, will never be deprived of value in face of the position occupied by the one who produces the knowledge. To move away from common sense to produce the scientific knowledge, it would require, then, an epistemological vigilance along the lines defended by Gaston Bachelard. In a way, such a proposal explores the limits of the scientist of nature in seeking to understand his/her own social practice, that is, the limits of scientists in making "science of sciences".

The autonomy of the field is what best represents Bourdieu's contribution to the study (social or historical) of sciences, as it is a primary piece to comprehend that the social game of Physics is a game with rules, based on a process of acquiring scientific authority (strict or social) (Bourdieu 1991), that cannot be coerced by the speech of servitude to the economic or political world. In its turn, this interpretation is not naïve, but canny when the author imposes the limits of the field to the scientific knowledge. It recognizes that the produced knowledge is not exclusively an epistemological construct, but an interested social practice, whose knowledge and recognition by the peers have a strong influence by the social position of those who speak or the laboratories from where they speak. Although the scientific authority has an expiration date, the field guarantees to its holder a substantial period of

time between a failure and the resumption of the scientific authority, almost always less impacting to those who have it at the expense of those who pursue it.

The scientific field changes over time. At the same time that previous achievements configure the distribution of the capitals at a given time, new ways of validating knowledge may be admitted. Bourdieu understands that priority disputes are often based on the search for the legitimacy of a certain way of doing science. Hence, these are at the same time "scientific debates about the meaning of what is discovered and epistemological discussions on the nature of the scientific practice" (Bourdieu 1991, 13). The main struggles in the scientific field are epistemological, involving the criteria that validate the knowledge. Thus, as much as the most diverse motivations may lead scientists to engage in disputes, in practice science requires that the actions and speeches carried out focus on gaining the recognition of peer-competitors. Although the field involves agents in unequal positions, the reason is constituted by the establishment of clear rules which allow the judgement of their work.

> Thus, it is in history that we find the reason for the advances of a reason that is thoroughly historical and yet irreducible to history. Scientific reason realizes itself only when it is inscribed, not in the ethical norms of a practical reason or the technical rules of a scientific methodology, but in the social mechanisms of an apparently anarchic competition between strategies armed with instruments of action and of thought capable of regulating the very conditions of their use as well as in the durable dispositions inculcated by the school and reinforced by the very functioning of the field. (…) Against all those who see no possibility of "grounding/founding" reason other than ascribing it to a transhistorical "human nature" independent of social conditionings, we must admit that reason realizes itself in history only to the degree that it inscribes itself in the objective mechanisms of a regulated competition capable of compelling interested claims to monopoly to convert free to say anything at all imprison themselves, provided that they say nothing about anything essential or that they say it in such a form that nothing will escape from the closed circle of the initiated. (Bourdieu 1991, 21-22).

Different sciences, practiced at different times, have a variety of degrees of autonomy. As previously mentioned, Copernican astronomy, developed in a scientific field that was still heteronomous, has been constituting, in its historical trajectory, certain degrees of autonomy until the present day, in which admission to the field does not require highly specialized knowledge. Studies on thermal physics in heat engines during the industrial revolution were of equal interest to artisans, engineers, industrialists, and those who would later be called physicists. In this context, this branch of knowledge is not restricted to a field that is already properly formed, with its well-established social rules. After James C. Maxwell and Ludwig Boltzmann, many of the problems related to heat and temperature came to be understood as problems of statistical mechanics, restricting them to the specialized knowledge of Physics. In the same period, we see a reverse process, the electrodynamics being born with André-Marie Ampère, with a work of selected interest from the men of scientific academies, little by little becoming the property of engineers and industrialists who will promote the second industrial revolution.

When we study a historical episode and ask ourselves how its historical-social context has influenced it, the answer to this question goes through assessing how much this episode involves themes and social agents that are part of a specific field. In cases where the field is heteronymous, the production of knowledge will be under the direct influence of external interests, both economic and worldviews based on assumptions that refer us to the most diverse cultures. However, when studying a case inserted in a very limited way to the production of an autonomous field, the "external" influences act indirectly, often playing a secondary role in the episode.

Conclusion

Science is a historical construction. At the beginning of this article, we tried to problematize this statement, indicating that within it an epistemological tension between rationalist and relativistic views of the scientific knowledge is present. Thus, the challenge raised was to trace paths that would allow reflecting on the possibilities of overcoming this tension, discussing its implications for the writing of the history of sciences.

The most different epistemologies have tried, over time, to deal with the problem of the historicity of sciences. Positivist perspectives, in their legitimate project that sought to constrain – perhaps excessively – what scientific knowledge would be, ended up limiting the history of sciences to the progress of observation-based theorizing. Thus, it would be the story of the triumph of an epistemology. Bachelard conceives reason in such a way to distinguish it from logic by attributing to its historicity. In his view, science goes through epistemological ruptures and, during this process, new rationality is built. It is interesting to note that the plurality of reason occurs because it applies to the world in search of answers to the questions asked. However, Bachelard reduces the history of sciences to the achievements of the human thought, which rectifies its errors.

In the third quarter of the 20th century, the historical progress of science becomes central to epistemology. Kuhn's work allows us to emphasize science as the making of a community, but the notions that allow us to see it in this perspective keep us away from rationalist projects. This question is retaken by Lakatos, who develops the methodology of research programmes by seeking to establish a model in which the decision between rival programmes obeys a rational criterion, choosing the progressive programme. However, the ways in which scientific practices are historically constituted are hidden in his model. The author ends up dealing with this problem by separating the history of science into an internal and an external part.

The science studies and the new historiography of science that is born at the same time reveals the entire contingency of sciences. In a kind of anti-epistemology, science is studied as a situated practice, in which all human restraints are present. The demarcation problem is abandoned, and, with that, the sciences are matched with other cultural manifestations. However, scientific knowledge no longer has a specific social role, which leads us to political issues, including the possibility of having democratic societies.

Bourdieu presents his "science of science" as a way to understand *the social conditions of the progress of reason* (1975). The social rules that limit the modes of action of agents in the scientific field are different from other social spaces. They are historically constituted through struggles that seek to give legitimacy to the knowledge, and to those who produce it. Scientific disputes are epistemological quarrels in which the validity of knowledge is in question. Thus, the result of each struggle, far beyond a "winner", is the refinement of the rules to be followed in the production of knowledge.

The autonomy of sciences is the result of a specialization process in which the field itself defines the legitimate problems to be addressed, and the valid procedures for solving them. This process of "internalizing" the field – in the sense of independence from external factors (political, economic, theological, etc.) – occurs because the agents of the field internalize its structures, acquiring their own *scientific habitus*. Thus, the cognitive and the social are merged in the elaboration of rational knowledge.

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