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Special Issue

Leviathan and the Air-Pump

After 40 Years: Reception, Criticisms and Impacts

Constructivism and Historiography of Science in Argentina: A Critical Perspective Regarding the 20th Anniversary of the Translation of *Leviathan and the Air-Pump*

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Abstract:

The article focuses on the networks involved in the Spanish translation, in 2005, of the book *Leviathan and the Air-Pump*, by Steven Shapin and Simon Schaffer. It shows that the translation by the publishing house of the National University of Quilmes was linked to prior and subsequent tensions that arose within the framework of the local institutionalization of the STS (Science, Technology and Society) field and overlapped with concerns specific to the historiography of science in Argentina. Focusing on multidisciplinary publications and local institutional developments, it is shown how the constructivist approach adopted in the book received criticism from other currents such as the neo-institutional, Marxism, or historical epistemology. The final reflections show some of the derivations of these debates in relation to the decisions of science policies.

Keywords: Constructivism; Historiography; Science; Argentina; STS Field

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Introduction

The book *Leviathan and the Air-Pump Hobbes, Boyle and the Experimental Life* by Steven Shapin and Simon Schaffer, first published in 1985, multiplied in the historiography of science some constructivist considerations that prevailed in the STS (Science, Technology and Society) field. The new sociology of knowledge, in which David Bloor had outlined the *Strong Programme* of constructivism in 1976, not only influenced the authors of the book in question, but also by then had various appropriations. Bruno Latour's work with his *laboratory studies* was one of the most influential. The STS field had been developing

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internationally since the 1970s, consolidating research groups, academic meetings, institutions, and publications that participated in the discussion of these perspectives. In Argentina, the book was translated in 2005 by the publishing house of the National University of Quilmes (UNQ). The local STS field was developing at that time, also involving scholars of the historiography of science. The volume was part of a collection directed by Pablo Kreimer, titled, precisely, *Science, Technology and Society*. The translation was done by Alfonso Buch, and he was assisted by Alberto Lalouf.

Around this publication in Argentina, this article proposes a historical overview of the critique of the heterogeneous current that posited the social construction of scientific knowledge in the 1970s. The following pages will show how constructivism and its re-significations influenced, or not, the ways of researching of some historians of Argentine scientific practices. In particular, attention will be paid to those whose academic dynamics were intertwined with those of the team that undertook the translation of the book. It will be shown how the translation was linked to the continuity of some multidisciplinary debates that had been taking place in Argentina as part of the increasing institutionalization of the STS field.

It should be taken into account that so far the most extensive reflection on this translation is the study included in the book itself, carried out by Alfonso Buch. In the following section, the problem will be traced in some Argentine publications linked to the joint dynamics of historians of science and scholars of the STS field, from the decade prior to the translation of Shapin and Schaffer's book. Some criticisms as they developed in Argentina around the perspectives opened by constructivism will be synthesized. The third section will address the topic of the evolution of these debates, as well as their impact on the historiography of science since the publication of the translation. Finally, the conclusions will provide elements from a critical theoretical framework of the historiography of science in Argentina, considering how academic debates on constructivism were intertwined with academic, but also political, technological, and scientific developments.

Before the Translation of the Book: The STS Field in Argentina in the 1990s and its Relationship with the Historiography of Science

The *Science, Technology and Society* collection of the UNQ, in which the translation of Shapin and Schaffer's book was published, had originally been directed by Mario Albornoz. In 1996, Albornoz, Kreimer, and Eduardo Glavich were in charge of editing the volume that inaugurated the series.² The book was titled *Science and Society in Latin America*. The chapters had originally been papers from the first *Latin American Conference on Social Studies of Science and Technology*, which marked the beginning of meetings subsequently known as ESOCITE (promoted by the *Latin American Association for Social Studies of Science and Technology*). Although none of the three editors of this first volume were historians, they influenced the training of researchers who dedicated themselves to the historiography of science in Argentina. Furthermore, several of the contributors to the book were already important figures for the historiography of science in Latin America. The organization of the book's index serves as an example of the topics gathered in these networks. In its pages, themes frequently addressed by the STS field were combined with those belonging to the historiography of science in Argentina. The Scientific and Technological Policy section was organized by Hernán Thomas and Renato Dagnino. The

² I am grateful to Eduardo Glavich for his valuable comments on the first version of this article.

Sociology of Science section was coordinated by Hebe Vessuri. Judith Sultz centralized the chapters on University, Science and Technology. The Division of Scientific and Technological Information and Dissemination was directed by Enrique Oteiza. Finally, Juan José Saldaña coordinated the Social History of Science section. Among the historiographical contributions in this section were those of Ariel Barrios Medina, María Elina Estébanez, Cristina Mantegari, Marcelo Montserrat, and Carlos Prego.

In the first pages of the book, the three editors clearly stated the political objectives of the collection, which aimed to strengthen the STS field in Argentina. This effort was also accompanied by other initiatives such as the publication of the *Redes* journal and the creation of an STS Master's program, both at the UNQ. Albornoz had previously directed another Master's program in Policy and Management of Science and Technology, created in 1988 at the Center for Advanced Studies (CEA-UBA)³ and many of its professors would also teach in the UNQ Master's program. The teaching staff of both Master's programs included recognized Argentine historians and thus offered training that complemented that of other professors who were important figures in discussions about science policy since the 1970s and who held positions in management institutions of national scientific and technological development policies. In the STS Master's program, associated with the publication of the book, classes were taught on problems of innovation economics, science policies, sociology and history of technologies and sciences. As part of this institutional dynamic, the editors of the book commented that they were trying to revitalize concerns that had unfolded in industrialized countries and were dormant in Latin American countries. In the countries of the North, the STS field had developed after WWII, in parallel with the evolution of public policies related to scientific and technological activity. Its developments were also associated with the growing awareness of the possibilities and dangers involved in the evolution of that activity during the 20th century. As the editors recalled, in Argentina in the 1970s, several of the central issues of the STS field had been formulated with great impetus by the representatives of *Latin American Thought in Science and Technology*. In fact, the book brought together scholars who participated in the late debates of those heterogeneous currents of thought from the 1970s. Some of them, in the 1990s, had continued publishing. This was the case, for example, with the publication of the *Centro Editor de America Latina*, edited by Oteiza in 1992, titled *The Politics of Research in Science and Technology. Histories and Perspectives*. A year later, a book written by Oteiza together with Vessuri, titled *Social Studies of Science and Technology in Latin America*, had also been published.

The editors of the 1996 volume also recognized as antecedents the historical studies on science by José Babini, Julio Rey Pastor, and Desiderio Papp, who in Argentina had promoted the academic institutionalization of the historiography of science. This was a line indebted to the approaches of George Sarton and had developed hand in hand with Aldo Mieli in Argentina. However, their attempts had been cut short after an incipient creation of journals and academic institutions. Saldaña in his introduction to the *Social History of Science* section also mentioned these trajectories. He considered them as part of his analysis of the Historiography of Science in Latin America since its birth, which he understood to be tied to the modernizing projects of the bourgeoisies under the influence of liberal doctrines and the pressure of foreign capital. Saldaña pointed out some of the more or less truncated developments of these attempts until their consolidation as an academic professional discipline in the 1980s.

³ The CEA master's degree was conceived as part of a series of initiatives that included the creation of the Secretariat of Science and Technology of the UBA (University of Buenos Aires) and the creation of the UBACYT scholarships

As the editors pointed out, unlike the attempts at historiography to which Saldaña referred, which were very fragile in Argentina, the currents that could be considered local precursors of the STS field in the 1970s, those of *Latin American Thought in Science and Technology*, had not been configured as an academic undertaking. They had been constituted around a series of normative concerns regarding science and technology policy, within the framework of the development theories prevalent at the time. For this reason, the editors attributed the lethargy of the 1980s and 1990s to the reflection, on the cognitive level, of the bewilderment that the failure of development models had produced in the region (Albornoz, Kreimer, and Glavich 1996, 15, 19, 20). Thus, the collection proposed to put the discussion about the problems emerging from scientific research and technological development within our societies back at the center of the scene. They invited facing an old problem with new elements, asking what kind of science is needed and why do science in Latin America (Albornoz, Kreimer, and Glavich 1996, 23).

The majority of the historiographical chapters in the volume were focused on cases of experimental medicine in Argentina, and few of them explicitly mentioned the developments of the new sociology of knowledge, except for Prego's chapter, which mentioned *laboratory studies*. However, in the sociology of science section, two chapters written by the editors themselves accounted for central debates in the STS field regarding constructivism and its derivations. Glavich wrote a chapter in the book that was specifically dedicated to the postmodern strand of constructivism. There, he criticized the idea of the new principle of symmetry proposed by Latour, extending the designs of the *Strong Programme*. It was a framework that, according to its mentor, was “destined to explain both nature and society (Boyle and Hobbes) at the same time, that is, a world in which the representation of things in the laboratory is not dissociated from the representation of citizens in the social contract” (Glavich 1996, 162). Glavich was explicit in that chapter regarding how he believed Latour's political positions had shaped each of his sociological concepts. Eliminating the analysis of production relations, the postmodern had accentuated symbolic relations to the point of conceiving that democracy could include objects. Avoiding in his analysis belonging to social classes and reference to their irreconcilable interests, the Frenchman had elaborated the idea of human and non-human actors who negotiated. Discarding socialism and clinging to the idea of ecologizing capitalism, Latour had proposed a participatory democracy centered on the representation of networks of human and non-human hybrids. For Glavich, Latour's humanization of objects, to the point of boasting about promoting a parliament of things, in reference to giving votes to non-humans, was the other side of the coin of the fetishization of social relations. That is, it was an ideological masking, dressed in democracy, that hid the true relations of production. Glavich ended his chapter saying that the confusion of Latour's theoretical framework, at best, showed his manifest inability to understand the essential elements of the functioning of capitalist society and, at worst, involved an attempt to ideologically justify the established social order.

In the year 2000, Glavich would complete his critique by extending it not only to postmodern derivations but also to constructivist currents and the STS field as a whole, in an article titled *A Marxist Critique of Constructivists and Critics* (Glavich, 2000). There, he interpreted both the relativism of constructivism and the options that granted autonomy to technological developments as approaches typical of the strength that pessimistic currents had acquired after WWII. It was a disenchantment also marked by the birth of explicit postwar science and technology policies. From Glavich's perspective, these approaches considered social determinations on scientific and technological development too vaguely, especially because they did not incorporate political economy into the much-touted interdisciplinary framework they advocated. For decades, Glavich accompanied one of the leading figures of *Latin American Thought*, Oteiza, in a seminar on *Social Studies of Science and Technology* at the School of Sociology of the UBA. In those classes and in

those he taught at the School of History at the same university, he emphasized that it was important to integrate political economy into social studies of science.

Kreimer's chapter in the 1996 volume included his own research advances in laboratory studies, an area that would be fundamental for constructivist approaches. Having carried out his research work at the *Pasteur Institute* and *Birkbeck College* in London, the author knew in detail not only the conceptual tools but also the internal workings of the new sociology of knowledge since was enunciated by Bloor in 1976. Kreimer partly took up Hebe Vesuri's argument that a crisis was already running through the *Strong Programme*. The chapter mentioned, in addition to Latour himself, other referents of the discussions within the area as Harry Collins, Karin Knorr Cetina, and Michael Callon. The author considered that *laboratory studies* derived from constructivist positions had stimulating contributions for anyone wishing to penetrate the so-called *black box* of scientific research. However, he proposed reversing the impetus of the derivations of the new sociology of knowledge that had led to discarding all the classical theoretical instruments of sociology and anthropology. Likewise, he critically considered the most extreme positions of constructivist derivations, such as those that had led Latour or Callon to challenge the very specificity of the social sciences. Specifically, Kreimer criticized the idea of dissolving the differences between the human and the non-human in the famous category of actant, of actor-network theory. Kreimer criticized it above all in relation to the attribution of intentionality to the non-human (Kreimer 1996). For years, Kreimer would teach in the STS Master's program at the UNQ, offering a detailed overview of the evolution of constructivist currents and the theoretical nuances of laboratory studies.

In the year 2000, another book published in Argentina accounted for a professional sociability partly coinciding with that of the group gathered for the 1996 volume, although all its chapters would already be oriented towards historiography. It was compiled by Montserrat, who had also written in the volume edited by Albornoz, Kreimer, and Glavich. The compiler also worked with them at the UNQ and vindicated the tasks of the STS Master's program at that University, as well as its publication organs, in relation to the consolidation of multidisciplinary issues. The chapters of this volume had been presented at a meeting of the Department of Humanities of the University of San Andrés convened as *The History of Science in Argentina between the XIX and XX Centuries*. The book was titled *Science in Argentina Between Centuries. Texts, Contexts and Institutions* (Montserrat 2000) and brought together a series of authors whose names partly repeated those of the previously mentioned volume. Thus, in addition to Montserrat's own name, those of sociologists who conducted historical studies of science in Argentina, such as Buch and Kreimer, were repeated. Furthermore, other historians of local scientific practices were added, such as Irina Podgorny, Diego Hurtado de Mendoza, Cristina Mantegari, Dora Barrancos, Miguel de Asúa, and Luis Tognetti. The book's compiler said that he intended to affirm a clearly multidisciplinary proposal for the historiography of science, which he considered a discipline too spoiled by chronicle. For this reason, he claimed to have brought together historians of science concerned with discourses and institutions, linguists, sociologists, and anthropologists under the siege of other aspects of that multiple activity that science was.

In this book, Lewis Pyenson, a Canadian historian of science who, based on his interest in what he called cultural imperialism, ended up studying the exact sciences practiced in Argentina since the late 19th century, wrote a chapter. His work was very influential and was mentioned in several of the chapters of the volume in question. Among the authors of the book, Pyenson was the most cited within the compilation itself. In his chapter, he pointed to the contradictions of postmodern rhetoric and did not hide his frank discomfort with what he called "Latour's iconoclasm" (Pyenson, 2000, 89). On the other hand, Shapin himself was characterized as a postmodern by the Canadian. Pyenson

also retorted that Shapin's constructivist book, titled *The Scientific Revolution*, was "brief and confusing" (Pyenson 2000, 90). Having been interested in the history of scientific disciplines in Argentina, Pyenson thought that postmodernism swept away the tools of historians of science that allowed studying the ubiquitous and national differences without losing sight of universal aspects of scientific practices. Furthermore, in the book compiled by Montserrat, the author expressed his hope to see the end of postmodernism in the academic generations that would follow. In that sense, he celebrated that in the then brand-new Parisian journal *Cahiers de science et vie*, on whose editorial committee sat "spokespersons of postmodernism, including Bruno Latour and Simon Shaffer," the universal character of the results of the exact sciences during the 19th century was not discussed. Pyenson affirmed with jubilation that in the journal there was "(...) a profuse debate about the context of justification and the inspiration to undertake a measurement or an observation, but a general agreement prevails in that astronomers and physicists fulfilled the details of their work thanks to the norms of their disciplines" (Pyenson 2000, 97).

In his chapter, Kreimer polemicized with the Canadian's general approach. He pointed out that although studies of the diffusion of knowledge from the metropolis, in Pyenson's style, could account for some issues related to the universal character of scientific practices, they did not take into account some local dimensions of science that were relevant to analyzing research traditions in scientific communities that were not located in the metropolis. For the latter, Kreimer vindicated constructivist sociology, which presented science as something that was no different from "other alternative forms of social and cognitive endeavors" (Kreimer, 2000, 189). However, Kreimer did not ignore, nor did he fail to criticize, the biases of constructivists. For example, he highlighted the problem implied by their consideration that research findings and the physical environment were simply subordinate to the ability of practitioners to mobilize resources and impose their point of view on other actors. As an alternative, Kreimer underlined the proposals of a current that had been developing in parallel to, but differentiating itself from, the constructivists and that was called "neo-institutional" (Kreimer 2000, 191, 192). This strand, which had Terry Shinn as one of its exponents, considered both the socio-strategic and the cognitive conditionings associated with the achievement of intellectual trajectories, including forms of reasoning, work practices, evidence, evaluation, and publication criteria.

Another of the chapters in this second book we are analyzing here was written by a historian who is responsible for one of the most prolific works on the practices of science in Argentina. Podgorny, who writes a chapter on the circulation of fossil mammals in Parisian institutions, over the years become the director of several scholars of the history of local scientific practices. Her writings on museums, natural history, and paleontology could undoubtedly be partly related to the historiography of Shapin and Schaffer. This is clear, for example, in the way Podgorny conceives the existence of the practices of testimony as fundamental to the development of science. Her studies analyze how only certain witnesses who followed established protocols could guarantee the reality of the facts they sought to demonstrate. Like Shapin and Schaffer, Podgorny emphasized analyses that show how modes of writing helped in that constitution of witnesses at a distance from the evidence offered as part of scientific practices. This is a common imprint to other authors who, through Historical Epistemology, have been influenced by constructivism. However, through that epistemology, and also relying on a vein coming from German media theory, Podgorny emphasized in her work the strong materiality of the analysis of scientific practices, in contrast to the interpretations of Shapin and Schaffer. Not surprisingly, as we will see in the following section, one of the most prominent representatives of historical epistemology, in which Podgorny converges, opposed the so-called sociologizing positions characteristic of the *Strong Programme's*

constructivism (Rheinberger 2005). Asúa, Hurtado, and Busala (Asúa 2000, Hurtado and Busala 2000), in their respective chapters of this 2000 book, wrote retrospectives on publications of history and philosophies of science in the region. They inquired into the level of connection of those editions with the research done from Argentina at the behest of the initial impulses given by Mieli and Babini in relation to the institutionalization of the area. Although in this book Hurtado did not yet deal with constructivism, as will be seen in the following section, it would not take long for him to also focus his criticism on those positions. Both Podgorny and Hurtado would also teach in the History of Science and Technology subject of the UNQ's STS Master's program.

The generation of professionals involved and the references appealed to in these two books would feed the training of other researchers in the history of scientific practices and policies in Argentina, such as Máximo Farro, Susana García, Marina Rieznik, Andrea Pegoraro, Alejandra Pupio, Lucía Romero, Juan Pablo Zabala, José Buschini, Cecilia Gárgano, and Adriana Feld, among others. Here we have focused on some of the interstices of the debates that are glimpsed and that drive these two books because they are the ones that most intertwine with the STS dynamics and issues in which the work teams linked to the translation of Shapin and Schaffer's book were interested. However, we are aware that scholars of the history of scientific practices in Argentina, outside this temporal or spatial scope, also shared various multidisciplinary publications, spaces, and trajectories with other academics that accompanied the consolidation of the historiography of science in Argentina.⁴

Debates since the translation of *Leviathan and the Air-Pump* in Argentina

In 2005, within an academic context partly shaped by the debates summarized in the previous section, the UNQ collection, by then directed by Kreimer, published the translation of Shapin and Schaffer's book.⁵ Buch, who was in charge of the translation, wrote a lengthy introduction to this work, which he considered a contribution to both the history of science and political history. He characterized the book as a central reference, a classic of the new sociology of knowledge that allowed for closely linking the analysis of the sphere of scientific representation of nature and the examination of the sphere of political interests. Buch summarized the book's central thesis: solutions to the problem of

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⁴ Perhaps one of the publications that best shows the breadth of these exchanges is the publication of the selection of papers from the *Conferences on Epistemology and History of Science* convened annually since 1994. This periodic event still brings together a large contingent of researchers. It is organized by the Logical-Epistemological Area of the School of Philosophy of the National University of Córdoba. On the other hand, since 2003, the *Conferences on the History of Argentine Science*, convened by the National University of Tres de Febrero and the *Argentine Group for the History of Science*, also published a selection of their papers for some years. This group traced its affiliation to the impulses of Rey Pastor and Babini in the first decades of the XX century and encouraged the consolidation of a community dedicated to the history of science in our country. We are also leaving out of this scope the very varied publications, associations, and other institutional spaces dedicated to the history of science in the Latin American or South American sphere, which also contributed to the development of the historiography of science in Argentina (Saldaña, 1996, cf. Podgorny, 2023).

⁵ By then, the group gathered in the STS Master's program at the UNQ had split, and Albornoz would form another one, the *Redes Group*, with its own publications, meetings, institutional spaces, and organizations that would be relevant in the consolidation of the STS field in Argentina, as well as in its international insertion and the development of regional institutions.

knowledge were embedded in practical solutions to the problem of social order, and different practical solutions to the problem of social order involved different practical solutions to the problem of knowledge. Buch argued that this conclusion was a generalization, perhaps excessive, derived from a specific historical case study. He then went on to summarize the book's case, which concerned the historical disputes about the nature of the vacuum and the instruments used to understand it. The historical analysis of the different interpretations of the experiments also involved the analysis of political and religious conflicts. Thus, Buch presented the book's central argument, that is, how the classic discussion between Robert Boyle and Thomas Hobbes in 17th-century England not only involved a debate about what was happening in nature but also controversies about how natural philosophers should organize themselves to understand it.

As Buch pointed out, although the history of material technology was involved in the experiments being carried out in the book, what was less evident from this analysis, and therefore should be highlighted in this work, was how the history of social and textual technology unfolded. What was unique about this work was how it posited the existence of a practice and a theory of testimony that guaranteed the reality of the facts adduced by naturalists. In this historical analysis, the modes of writing that appealed to the possibility of the reader becoming a virtual witness of the experiments became relevant (Buch 2005, 13). As Buch shows, this path follows a classic route of constructivist sociology, which, as seen in the previous section, was already debated in Argentina. Buch concluded his introduction to the book's translation by highlighting the importance of reflexivity in the proposal of David Bloor's *Strong Programme*.

As a mark of the uneven development of the fields of knowledge and the internal debates within constructivism that Kreimer and Vessuri had opportunely pointed out, criticisms of the constructivist currents and its derivations intensified internationally. In the same year the book was translated, an former researcher at the History of Science section of the Max Planck Institute for Berlin, Wolfgang Lefèvre, published an article titled "Science as labor" in the journal *Perspectives on Science* (Lefèvre, 2005; cf. Perret, Ugartemendía y Rieznik, 2009). It was an accusation against the hegemony of constructivism in the reconstructions of the histories of science. Lefèvre argued that in the last thirty years constructivism had been hegemonic and had placed at the center of its concerns a notion of practice as a concept reducible to pure interaction. Thus, the radical constructivist perspective had highlighted the priority of social interaction over the complex material and natural determinations of social relations.

Lefèvre criticized the constructivist position and insisted that part of the problems were linked to the fact that science did not function "as if" it were work, but that it was, in the literal sense of the term, a work process: "...scientific production [is] [...] work strictu sensu" (Lefèvre, 2005, 211). By emphasizing all the material constraints that this implied, he was criticizing positions such as those of Latour, when he stated that "matter is not something given, it is a recent historical creation" (Latour, 2001, 247). Or when he argued that the "conditions of happiness for political life" could advance without being interrupted by the *inhuman laws of nature*" (Latour, 200, 356). The fact of considering science and its practice as work processes, said Lefèvre, might appear obvious to other areas of reflection and research in the social sciences, where no one disputed that the concept "work" was central to account for human activity. However, this was not the case in the field of social studies of science and technology, where, not by chance, radical constructivism had prevailed (Lefèvre, 2005, cf. Perret, Ugartemendía y Rieznik, 2009). As in the case of Glavich in Argentina a few years earlier, the German author was pointing out a weakness stemming from the limited consideration of political economy as a tool for analysis.

In the same issue of the journal in which Lefèvre published, Hans-Jörg Rheinberger, a leading figure in the Historical Epistemology current that had some

influence in Argentina, wrote a harsh critique of Bloor. Bloor had attempted to ingratiate himself by writing a review of a book written by Rheinberger in which he called for uniting interpretative efforts (Rheinberger, 2005). Rheinberger, however, clearly emphasized the aspects that distinguished his own approach from constructivist interpretations. Thus, he highlighted the material aspects of scientific practices, pointing out the weaknesses of an approach too focused on intersubjectivity, such as that of constructivists.

Years later, these two articles would be part of the subject History of Science and Technology of the UNQ STS Master's program taught by Irina Podgorny, accompanied by Susana García and Marina Rieznik. On the other hand, a chapter by José López Piñero offered by the same subject is pertinent at this point. The author, already cited by Podgorny from the book compiled by Montserrat, questioned a good part of the historical reconstruction that the historiography of science made of its own field. The disciplinary history, criticized by Piñero for being unreflective, coincided with the narrative about the inaugural milestones of the constructivist theoretical framework to which we have been referring. For, as Buch presents the problem in the introduction of Shapin and Schaffer's book, they, like other constructivists before them, presented themselves as those who had managed to move away from the simplifying debates between externalists and internalists. Influenced by the Kuhnian framework, they would have adopted the idea that the scientific method constituted a practical discipline, involving a language whose rigor was but the result of a consensus reached in practice, which was not given as a starting point. Thus, truths were conceived as relative to systems of conventions, that is, to paradigms (Buch, 2005, 17, 18). Indeed, in Bloor's book in which the *Strong Programme* was postulated, a vision of the program's history was proposed, in which Kuhn was placed as an important predecessor in relation to the role played by the social factor in the interpretation of the history of science. It is precisely this attribution of importance to Kuhn's theories that was searingly criticized by López Piñero.

Contrary to the interpretation offered by the *Strong Programme*, which was that of a large part of the historiography of science, Piñero questioned the existence of an Anglo-American revolution represented by figures such as Kuhn, Feyerabend, and Hanson. Not only that, he asserted that those who considered these productions relevant based themselves on the consideration of reiterations, local versions, and imitations disguised with speculations under the guise of novelty. For the author, many of the supposed novelties that Kuhn contributed regarding the importance of the social in the history of science had already been addressed by previous historiographies of science, which, moreover, had already intertwined with diverse social studies since the first decades of the 20th century (López Piñero, 1992).

In line with the criticism made by López Piñero, which Podgorny so assiduously recalls (Podgorny, 2023), we can add the consideration that the constructivists' elaboration of the Kuhnian root as an antecedent of their own program can be seen in light of a specific interest. It was about leaving out of the STS field, as had been done in the history of science, other perspectives that had been developing before and beyond Kuhn. The very development of the STS field in 1970 owed its impetus to the need for an alternative to Marxist theories. In the historiography of science, those theories had been influential against internalism since the famous 1931 London congress (Huerfano Melcón, 2004). The denial of Marxist perspectives by constructivists was related to and reinforced by the need to establish Kuhn as the most important antecedent of their own viewpoints.

Furthermore, in a work I wrote in the same year as the publication of the translation of Shapin and Schaffer's book, I pointed out some historical aspects of what constructivism had claimed to combat, that is, the defense of a kind of immaculate, objective, and autonomous science, in the style of the Vienna Circle's 1929 manifesto. In Vienna, it was an attempt to defend itself against the first Nazi persecutions faced by its members, which culminated in their subsequent exile in the United States. But this kind of

celebration and defense of a science independent of political pressures was also wielded as a reaction against the possibility of state planning. This was manifesting as a developing reality since the Russian Revolution of 1917, and was posed as an overcoming of capitalist anarchy, in circumstances in which the latter was manifesting as a global crisis of unprecedented scope. For this reason, over the years the invocation of neutral science also took the form of a questioning of what was happening in the USSR at the time. This happens, whether against the very foundations of its radical social transformation (abolition of private ownership of the means of production and its centralization in the state apparatus), or alternatively, in the more specific terms of a critique of the bureaucratization of the ruling elite in the old country of the Tsars, then under the criminal management of Stalin. When constructivism emerged in the 1970s, in a context of new capitalist crisis and the defeat of the largest imperialist power in Southeast Asia, the discourse of the neutrality of science no longer seemed to suffice as an alternative to the planning proposals of Marxists. Especially considering that by then no one disputed the need for states to promote their scientific policies. It is in this context of decadence that, at the antipodes of the Vienna's Circle, when the so-called constructivist currents ended up renouncing science, which came to be considered a kind of mere social convention, without its own physiognomy or identity (Rieznik, 2005).

In that context, in which constructivism presented itself as the new alternative, many of the novelties it offered had also already been extensively discussed not only by Marxism, but also by more traditional epistemologies, for example, by the epistemology view that constructivism claimed to combat. In fact, concepts such as the "theory-ladenness of observation" or the "underdetermination of theory by experience" had extensive discussions behind them. In this sense, an observation made by Buch in the presentation of the book he translates is interesting. When he subscribes to one of the central theses of Shapin and Schaffer's work, namely, that matters of fact were, strictly speaking, constructions mediated by social technologies, Buch argues that thanks to these social technologies "the discourse of men, which was expressed as theory, was naturalized and taken as a *datum* of reality. The conclusion is that a fact depends in its character on a convention that takes it as such (...)" and then, he adds "(...) philosophers of science, when speaking of the theoretical load of observation, have accustomed us to this notion" (Buch, 2005, 13). Indeed, that science could not invoke what is called value neutrality was not new. It had already been raised many decades ago, even by members of the Vienna Circle, in the first decades of the 20th century. Thus, Otto Neurath, for example, had raised questions that would later be taken up by postmodern detractors. For Neurath, it was not possible to speak of value neutrality, in the sense that one must account for the validation process as a social act, historically determined. Science should be conceived as a boat at sea and scientists as sailors who revise the boat without ever having the possibility of repairing that boat out of the water. Clearly for Neurath both hypotheses, principles, and protocol sentences were conventional, something that applied to social sciences as well as natural sciences, to the question of the choice between theories, to the acceptance or rejection of hypotheses, or the choice of alternative hypotheses with the same empirical basis. Hypotheses, principles, and sentences that, on the other hand, he considered historically determined by the traditional forms of cognitive cooperation, that is, by the forms of social practices of science. When choosing between theories or hypotheses, said Neurath, one is thinking about which is the most adequate to achieve certain objectives, and in such a choice, the logic of science is not enough, but one must also use evaluative elements only comprehensible within a specific social historical framework (Uebel, 2000, Rieznik, 2005).

In short, beyond how novel the proposals of constructivism were, it is undeniable that the current had a much greater local and international influence in the STS field than those of its critics. Thus, in the same year that the translation of Shapin and Schaffer's

book into Spanish was published, the collection led by Kreimer also published the translation of another book that was a reference for constructivism in the STS field. It was Karin Knorr Cetina's (2005) book, titled *Manufacturing Knowledge: An Essay on the Constructivist and Contextual Nature of Science*. Kreimer continued to the present day using diverse methodologies, combining elements of actor-network theory and constructivism, but emphasizing the importance of analyzing specific historical cases without taking the constructivist theoretical framework as a whole. In a recent book, he argues that the main analyses of the STS field are dominated by European and American practices and fail to answer crucial questions about the capacity of local scientists to influence international megaprojects or about the adaptability of technology from central countries to the needs of peripheral countries (Kreimer, 2019). Based on the concept of "subordinated integration," he described how in the current stage peripheral researchers join projects coordinated by a center, performing specialized tasks without being able to influence the general formulation or theoretical perspective of the project.

The collection in which this translation was included would later, and to the present, be directed by Hernán Thomas, who for years would also be in charge of the UNQ STS master's program. Subsequent volumes reflect the concerns of the team he directs, consisting of historians and sociologists of technology especially focused on the sociotechnical framework that adopts several of the fundamental notions of constructivism reviewed here.

Conclusions and Perspectives. The Critique of the Critique

In 2010, Hurtado de Mendoza published a book. He had previously written the chapter in the volume compiled by Montserrat and had also taught History of Science and Technology for five years in the STS Master's program. Hurtado commented, in the introduction to his book, on some considerations regarding Shapin and Schaffer's book. He pointed out, in defense of a history of science from the perspective of political and institutional history, that the absence of studies on the history of science with this perspective or with the integration of economic history, with the exception of the book compiled by Oteiza in 1992, was not accidental. He argued that this was due to political decisions aligning foreign interests, inherent to processes of expansion and domination, with the interests of the local intelligentsia in accessing coveted international consultancies. He stated that the same occurred with academic fashions. Explicitly alluding to Shapin and Schaffer's book, he questioned whether it was possible to be a constructivist from Latin America. Although he did not answer the question, he suggested that the authors' constructivism was only possible because it was based on a significant body of concrete historical studies that preceded it and were not yet found in the Argentine historiographical tradition. He associated the lack of such research with the impossibility of a common basis for dialogue, debate, and collective production, characteristic of the fragmentation of the field of the history of science and technology at that time.

In recent years, many reflections by historians of science who have focused on the development of Argentine scientific institutions have questioned the inaugural discourses of elites and founding decrees in light of the activities of scientists within the created institutions. To do this, they have had to analyze social networks, economic circulation, and the materiality of practices in the field, in the laboratory, and in the spaces of circulation of scientific objects. These studies have partially adopted some constructivist notions in an attempt to move beyond traditional narratives that conceived scientific development as a linear process of diffusion of ideas driven by the country's political elites. On the other hand, in some studies, the materialities of local scientific practices have been approached from notions related to labor processes with perspectives

converging with those of political economy. Overall, instead of focusing on the development of concepts, disciplines, institutions, or individual researchers, many researchers have followed the path of epistemic objects, but considered in the strict sense of material objects of work and research. Thus, in studying scientific practices and their products, some scholars have focused in recent years on the diverse activities of the human labor process that mediated the appropriation and transformation of the natural and social environment. They developed lines of research related to the history of instruments and machines for recording and observation, the evolution of the characteristics of the labor force involved in the construction of the scientific objects focused on, and the spaces, organization, and results of this scientific work. Historians of science working from Argentina have published with increasing frequency in international journals, but also in local journals such as the aforementioned *Revista Redes* of the UNQ, *Saber y Tiempo*, published for a time by the team led by Hurtado at the Universidad de San Martín, or the *Revista Iberoamericana CTS* of the *Redes Group* directed by Alborno. The articles on the historiography of science in Argentina published in the last 40 years reflect the diverse conceptual framework of recent decades with which contributions have been made to the critique of previous institutional histories.

Recently, Podgorny, in reconsidering the current situation of the historiography of science in the region, has proposed that there is a certain decoupling between this boom in historiographical publications and local initiatives such as those summarized here, and even with respect to Latin American associations of the history of science that have had variable durations over the last decades. For her point of view, the increasing visibility of historians of science in Argentina is rather linked to the dynamics and new parameters of international academic excellence. That is, to the need for individual careers to publish as a requirement for obtaining a doctorate and to the needs of new journals to fill their pages with articles. This trend would also be reinforced by institutional requirements regarding the representation of regional and gender diversity. Furthermore, Podgorny argues that our present shows that, in the field of the historiography of science, as in many other fields of knowledge, theories, concepts, and words, far from being replaced one by one another according to either the accumulation of knowledge and experiences or paradigmatic changes, rather pile up and function in synchrony with different degrees of conflict. Moreover, in this accumulation, she points out a notable and general characteristic of contemporary academic practices: the marked preference for concealing conflict by hiding it behind silence, in a climate in which nothing is discussed, not even the veracity of the data offered.

Certainly, the uncritical adoption of the constructivist theoretical framework would only help to legitimize the accumulation and confusion noted by Podgorny. As an antidote to this tendency, relevant debates for historiographical reflections have been recovered here, developed by those who promoted the current consolidation of the STS field in Argentina, regarding the relevance or not of the constructivism in which Shapin and Schaffer's book is rooted for the analysis of our own history.

I have already pointed out in another publication, in convergence with Glavich's critique in 2005, the context in which social reality itself was diluted into a kind of diffuse magma of men and objects, or of humans and non-humans, as Latour liked to call them, randomly arranged in events that did not register historical laws or regularities in their development. In these perspectives, science and technology, like society and its history, were increasingly presented as a "contingent succession of opportunities and circumstances" according to Trevor Pinch and Wiebe Bijker, two of the conspicuous representatives of constructivism in the history of technology. Behind the imprecise embellishment of the idea of heterogeneous networks postulated by postmodernism, Sussan Cozzens is an author who followed many of Latour's own ideas. She posited the need to "establish a close friendship between science and the powerful institutions (...)

that can build the 'strong and broad networks' that make scientific activity more stable (...) the most successful strategy that science should adopt is to ally itself with the groups that have already managed to subject a large part of the world to their control (...) for whom money is not a problem". In this way, increasing privatization and commodification of science were openly proclaimed as a goal, albeit under the pathetic excuse of "transforming the modern enterprise into an increasingly public and responsible organization" (Cozzens, 1996). Behind Latour's proposals, we then find the advance of large corporations over scientific and educational institutions.

In 2004, Latour himself initiated a shift in the derivations of the constructivism in his own work. Thus, he wrote a note in which he expressed his concern about the political uses of the conceptual tools he had helped to build, which seemed to be collaborating, for example, with climate change deniers. Latour then asked, from the title of his article, "Why Has Critique Run out of Steam? From Matters of Fact to Matters of Concern". The shift would culminate in 2017 when the headline of one of the interviews he gave to Science magazine stated: "Bruno Latour, a veteran of the 'science wars,' has a new mission. He has long been a thorn in scientists' sides. Today, Latour wants to help rebuild trust in science". Despite this shift – which is the extreme case of the crisis of the Strong Programme – in the years following the translation of Shapin and Schaffer's book in Argentina, and partly thanks to the book, constructivism has had a considerable influence on the historiography of science, complementing other critical views of the traditional narrative.

Forty years after *Leviathan and the Air-Pump*, and twenty years after its translation in Argentina, the challenge for the historiography of science is to continue exploring perspectives that allow understanding the complexity of local knowledge production processes in relation to the international framework, in a context marked by capitalism. In this article, we have underlined, in the debates before and after the publication of the book's translation, how the need to go beyond constructivism or frankly to move in another direction was also proposed. Those tensions implied incorporating into the analysis of the historiography of the history of science relevant aspects of the materiality of labor processes and tools of political economy that allow us to more fully understand the historical development of scientific practices in Argentina.

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