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Special Issue
Leviathan and the Air-Pump
After 40 Years: Reception, Criticisms and Impacts

The Symmetrical Leviathan

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Abstract:
Steven Shapin and Simon Schaffer's main contribution has been to show how science can be understood as a complex activity. This perspective encourages a view of scientific endeavour not only in terms of its outcomes, but as a more holistic process that includes experiments, instruments, and discourses. From this perspective, the principle of symmetry established by the Sociology of Scientific Knowledge (SSK) programme plays a central role. In this paper we would like to analyse three aspects of the historical research carried out by the SSK authors: first, the tensions between historical and sociometric modes of inquiry; second, the emergence of the symmetry principle as a founding criterion of the SSK programme and its implication in the internal/external debate; and finally, a critical suggestion about the type and self of the historian who conducts the history of science on the basis of the SSK.

Keywords: Sociology of Scientific Knowledge; Symmetry Principle; History of Science; Steven Shapin; Simon Schaffer

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Sociometric and Historical Modes of Inquiry

In the introduction to the 2011 edition of *Leviathan and the Pump-Air*, Steven Shapin and Simon Schaffer decided to reflect on the legacy of their book, first published in 1985. One of the most interesting quotes in this text refers to a polemical article published by Harry Collins in 1981, which serves as an apt introduction to the kind of problems that were part of the debates between historians, philosophers, and sociologists of science.

The article, entitled Understanding science, referred to the scope and limitations of sociological and historical methodologies in achieving a better understanding of scientific

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activity. The main argument of Collins' paper is as follows: understanding science and the possibility of being a true *verstehende* could be persuaded by two modes. The task of understanding is always an activity carried out by a researcher, but two modes are possible for them. The first mode is usually defined by the historical method, that is, understanding science by recovering the scientific activities and achievements of the past. On the other hand, we could understand science through a sociometric axis, i.e., by taking into account scientific activity carried out in contexts that are socially and culturally different from that of the researcher. Both ways of understanding science could be mixed up, for example, for a European researcher trying to study older forms of mathematics in ancient Chinese dynasties; but the main difference between them is that they are defined in terms of the past: the historical approach is always about the past, and the sociometric approach is mainly about the present of a different culture.

The adoption of a sociometric mode requires a methodology of the anthropological/ethnographic type, in the meaning of immersion in another culture. The possibility of a real understanding of science is based on the crucial activity of working within scientific groups, where the main task of the researcher is to understand how science is developed by observing and being part of the research process. It is not the interest of this article to go into the problems of this kind of methodology, but to note that in the early eighties, this kind of anthropological account represented a strong vision of how new disciplines could overcome the problems of the historiographical tradition. It is no coincidence that it was during these years that Collins developed his first steps in the sociology of scientific knowledge, immersing himself in the North American scientific community of physics, particularly those involved in TEA laser research and the detection of gravitational waves (Collins 2019). As a sociologist, he 'lurked in the shadows' of these physics communities in order to understand and reconstruct the main controversies surrounding these phenomena. In the same vein, in 1979, Latour and Woolgar published the controversial book *Laboratory Life: The Social Construction of Scientific Facts*, which, beyond its impact, became a symbol of a sociometric approach that could finally solve the classical historical deficiencies (Latour & Woolgar 2013).

What was wrong with the history of science for these sociologists? As Collins recalled in the 1981 article, the historical mode adopts the methodological tools provided by the discipline of history for the main purpose of reconstructing some aspects and periods of the past. In other words, the historian of science provides an interpretation of past science. How can this kind of research be carried out? It can be done in two ways. The first assumes that we can only reconstruct the past on its own terms, i.e., explain it without trying to describe past episodes as part of the developing society that continues into the present; the second constructs the past to explain the science of the present, allowing the complex idea of progress to enter. Collins pointed out that the historical mode exposes the classic problems of scope and limits in history. The historian can't immerse himself in the past; he can only reconstruct it, but this reconstruction – to be meaningful – needs an architecture that connects that past to our present. Otherwise, historical reconstruction seems as eccentric as a distant culture, like a tribe, which we could only understand if we could immerse ourselves in that culture, but with the impossibility of doing so because it belongs to the past. This sort of thing is well known to anyone involved in historical research, and the historiographical debate was also known as the 'inside-outside' debate: the assertion or denial of a kind of 'insider knowledge' in science. As far as we have seen, behind the methodological separation between historical and sociometric modes of inquiry lies the whole problem of a more precise access to the internal knowledge produced by scientists. The focus seems to be on the sociometric approach because it is possible, at least in theory, to know how knowledge is produced within controversies by allowing the researcher to immerse himself in scientific communities.

Shapin and Schaffer were well aware of these debates. Indeed, they argued at the end of their book that they had "transcended" the categories of internal and external factors in science:

> We find ourselves standing against much current sentiment in the history of science that holds that we should have less talk of the "insides" and "outsides" of science, that we have transcended such outmoded categories. Far from it; we have not yet begun to understand the issues involved. We still need to understand how such boundary conventions developed: how, as a matter of historical record, scientific actors allocated items with respect to their boundaries (*not ours*), and how, as a matter of record, they behaved with respect to the items thus allocated. Nor should we take any one system of boundaries as belonging self-evidently to the thing that is called "science". (Schaffer & Shapin 2011, 342, italics are mine)

Shapin and Schaffer described their attitude to the internal/external problem as "outmoded categories" that they decided to transcend. But this attitude doesn't solve the problem or make it disappear. In fact, a large part of *Leviathan...* is devoted to showing how boundaries were made in the period in question and how they should be explained. At the time when experimental science was being developed in the eyes of Boyle and Hobbes, boundaries were being constructed around an idea of science and experimentation. So, at the end of this or any history of science, we could say that there are internal and external factors, although there could be no agreement to define both sides in a definite and conclusive way. That is, the question of internal/external factors always remains because science, as a particular kind of knowledge, requires a historical understanding of what should be considered inside the box. In this respect, Collins tends to neglect the capacity of history to fully explain the causes that produced such boundaries in any episode of the history of science. And I suspect that Shapin and Schaffer, although they have written a classic historical book in the history of science, are more likely to provoke the idea that *Leviathan...* should be read as a sociometric investigation into the creation of pump air. Indeed, they reiterate this idea in the introduction to 2011:

> *Leviathan and the Pump Air* was inter alia an instantiation of a research programme in the sociology of scientific knowledge. That is to say, it was *case study.* It *was* meant to illuminate important features of seventeenth-century natural philosophy, but it was, at the same time, intended to frame an agenda for the sociological and historical study of knowledge generally. It was never just the one thing or the other. (Schaffer & Shapin 2011, xli -italics in the original-)

In the further course of this article, I would like to provoke a different approach to the idea that *Leviathan...* is just a case study. If we take the methodological insights presented by Collins in 1981 and the quotations presented by Shapin and Schaffer in the 2011 edition of the book so far, *Leviathan...* seems to deal with historical episodes, but at a sociometric distance. Looking back at the first quotation in this article, it is important to note the clarification - in parentheses – that might go unnoticed in a quick reading: "how, as a matter of historical record, scientific actors have allocated items in terms of their (not ours) boundaries". Who is this "ours"? If *Leviathan...* is a case study in the science of the past, but treated as not our past, then an exploration of the sociologist's self must be undertaken. Who participates in the definition of "ours"? I would like to propose an answer at the end of this text, but first, we need to talk about the principle of symmetry in the SSK programme.

The History of Symmetry in the Sociological Approach

The search for symmetry has been identified by members of the sociology of scientific knowledge as one of the major concerns of their programme (Bloor 1991, 5). Symmetric explanation operates as a justification of knowledge as a natural phenomenon and imposes on the investigator the requirement to search for causes in order to construct these explanations. It assumes that all explanations are not self-evident, which in the jargon of the discussion of the 1970s and 1980s meant that rationality was not an adjective to be applied in cases of obvious facts and universal forms of reason, because such things simply don't exist. So, symmetry of explanation implied that knowledge, as we could learn from the history of science, could be explained in the same way when we talked about errors or true beliefs. It doesn't matter whether it's a false or unjustified belief, or what we take to be true; the SSK mandates the search for causes of how beliefs are justified and distributed in the cultural context of science, creating and granting legitimacy and authority. In a sense, it doesn't matter whether it was a theory or an experiment that led to new knowledge in a particular period of time; what interested the members of the first movement of the SSK most was the idea of understanding how scientific controversies were resolved, what strategies and methods were used, and what criteria prevailed over others.

It is interesting, forty years after the publication of *Leviathan,* why this kind of principle became so important for the new sociology of science that emerged in Edinburgh and Bath in the early 1970s. The first thing was to offer, in those years, what they defined as a strong programme in the sociology of science, mainly and directly opposed to the functionalist American school led by Merton since the forties. The main task was to construct a sociology that attacked the "black boxism" of the sciences, something that the old sociology, in their eyes, not only didn't do, but helped to establish.

> (…) sociologists of science are preoccupied with the producers in a way that takes little account of what is being produced (…). The internal organization of science, science as a social institution, as a profession and as a communication system are the chief areas of study. (…) These approaches, which are characteristic of North American work, exclude any discussion of the subject matter of science. Ignoring the cognitive aspect of scientists' activities, they restrict sociology to discussion of social relations and processes. Ideas are taken as given, they are objectified as citation or paper counts where each paper is taken to be of equal importance.
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> By assuming that the cognitive aspect is non-problematic for sociology, the sociologists of science have implicitly adopted a view of scientific knowledge. If all ideas and discoveries are seen as basically the same, if social processes are assumed to have no bearing on what is discovered or how it is discovered, then scientists are assumed to be perfectly rational in their cognitive activity. This perfect rationality assumes that there is but one way of understanding the world and that every scientist knows this method and can apply it. Application of this method necessarily results in scientific knowledge and so a scientist is not defined by his production of a particular kind of cultural artifact but by his practice of *the* scientific method. Scientific knowledge is thus demarcated from non-scientific knowledge by the type of activity which produced it. The Sociology of Science, in this view, is the study of who practices *the* scientific method, how they learn it and what rewards they receive. (Whitley 1970, 61-62)

The article by Richard Whitley, Professor of Sociology at the University of Manchester since 1968, became central to the subsequent debate on science. The main idea of science as a 'black box' became popular with the publication of this article. And Whitley became a mentor to some new English sociologists interested in science and technology, such as Trevor Pinch. But why was science seen as a 'black box' in need of a process of translucency? For the main protagonists of the time: Collins and Pinch in England (and later Pickering); Shapin, Barnes and Bloor in Scotland; Latour in France, it was quite obvious that the description given by historians, philosophers, and sociologists of science at the time was far from satisfactory:

> That is an interesting question: why were people so obsessed with opening the black box? Actually, I do not know, but it seemed the right thing to do. Many of the people coming into this had some background in the sciences. I know Collins had some background in physics; I had an undergraduate degree in physics; later Pickering came in the field, he had a PhD in physics. Barnes had some technical background. Many of these people were familiar with science, and I think they just found Merton's approach dissatisfying. They knew enough about science to know that there had to be more to say—more interesting than just the normative or institutional structures of science, which Merton was doing, which was of course a fine sociological approach. But if you have done some science, you know for example about these entities called neutrinos. For me, it's dissatisfactory just to talk about the physics community or the career structure of physicists: we wanted, somehow, to be more evocative. (Tosoni & Pinch 2017, 9)

In the case of the Edinburgh School, a new element was crucial in transforming this kind of concern into a research programme, namely the institutional conditions created by the Unit of Science Studies, established in 1966. Indeed, the term 'science studies' has become an academic label since the formalisation of the Unit in the sixties. It is of the utmost interest why and how such a new and original institutional arrangement was created and became the main reference for the future sociology of science.

> The Science Studies Unit was established in 1966 as part of an initiative instigated by the renowned evolutionary biologist and geneticist C. H. Waddington (1905–75) to diminish, from the science side, the separation between the 'two cultures' (of the arts and the sciences), which had recently been highlighted by the novelist and prominent intellectual C. P. Snow. The man charged with setting up the Unit was David Edge (1932–2003), who had trained as a radio astronomer under Martin Ryle at the Cavendish Laboratory but who was then working for the Science Unit at the BBC. David brought together Barry Barnes, a molecular biologist turned sociologist of science, David Bloor, an experimental psychologist, and Gary Werskey, a radical socialist historian of science, who was later replaced by Steven Shapin, whose doctoral dissertation, at the University of Pennsylvania (1971), had focused on the Royal Society of Edinburgh. (Henry 2008, 224)

This interdisciplinary institutional arrangement led, in my view, to the need to construct a new methodological approach to the sociology of science that could identify the unit's main mission. If this mission were to combat all black boxism in science, a principle of empirical action was needed: the principle of symmetry. This methodological principle for empirical research was the main characteristic of the SSK programme and was conceived as a central principle for one reason in particular: the first members of the new sociology of science were trained as scientists at a time when science was not yet the specialised practice it is today, but was becoming so. However, the Science Studies Unit was also a project committed to bringing real science into the sociological academic atmosphere, including teaching by eminent scientists such as the physicist Peter Higgs and the biologist Aubrey Manning (Bloor 2003). Far from the generally ridiculous image that some critics tried to paint of this new sociology of science, the SSK programme was fully committed to the science of the day.

Indeed, it was the scientist who was increasingly ahead of the philosophical implications of science. If C.P. Snow laid the burden of proof for the gulf between the two cultures on the shoulders of the literary and humanistic tradition, for not being interested or not making enough effort to understand the scientific level, the Science Studies Unit sought to attack the same gulf by adopting an attitude towards the specialised scientist that could not recognise the philosophical implications of her work. The geneticist Waddington, the intellectual creator of the Unit, described this situation in his book *Tool for Thought*:

> The only people who, to some extent, escape from the domination of a philosophy which they consciously or unconsciously believe in are those few who devote themselves so wholeheartedly to researching into new understandings of human or inanimate nature that the sheer brute facts they come across impose themselves regardless of the philosophical system with which they were approached (Waddington 1977, 15)

It is possible to think that symmetry was the strategy to undermine the gap between the two cultures. Just as it is possible to explain a novel by understanding the cultural context in which it was written, the same is true of the knowledge produced by science on the basis of "sheer brute facts". All knowledge is produced in a cultural milieu that influences that production. If it is possible to trace the causes of all knowledge, the main division into two cultures becomes obsolete.

The symmetry debate was popular in those years. Not only Merton, but also Karl Mannheim or E.E. Evans-Pritchard were references for the new sociology to show how not to do things. It was possible for the old school of sociologists to understand the religious roots that make the Azande believe in the "chicken oracle", but it is not possible at the same level of explanation to find any cultural or social reasons that explain why we believe that space-time curves. For the old sociology, we believe it because it simply is. The scientific truths are not, and could not be, part of the sociological enterprise of cultural and human explanation. The response to this kind of restriction, for example, in Pinch and Collins, has been to try to overcome the idea that scientific knowledge cannot be treated like any other kind of knowledge:

> One handicap to argument in the rationality debate is the inaccessibility of examples of self-contained systems of rationality. The ways of thought pertaining to cultures are difficult to grasp for mundane, as well as philosophical, reasons. Firstly, modern cultures are large, diffuse things which are hard to pin down. Those which are less large and diffuse present difficulties of access because they are historically distant or simply because they are inaccessible for logistical reasons. In recent years, the debate has taken as its exemplary case the rationality of the Azande oracle, yet it is doubtful whether any of the recent debaters have ever visited the Azande, or poisoned a chicken. This is not only a problem of geographical distance, it is also a problem of language, and the very strangeness of the culture which is under debate. A field trip of several years would seem to be a minimum requirement for anyone who wanted to get first-hand experience of what the poisoned oracle means to the Azande. (Collins & Pinch 2013, 3)

As could be surreptitiously seen, in order to rely on the empirical validation of any knowledge, beyond its final state of being true or false, the sociometric mode of inquiry seemed more faithful to the principle of symmetry. As noted above, the main authors of the SSK movement believed that there was a difference between the sociometric and the historical approach: the former was likely to provide more accurate symmetrical explanations than the latter. At any given point in time, the researcher cannot be sure why and for what reasons certain knowledge is considered true or false for a given community at a given time. We could only interpret and reconstruct the plausible causes available to scientists, but no more. With the sociometric approach, we could not only treat scientific knowledge sociologically, but also confirm the seemingly invisible cultural roots hidden in it.

> The recommendation that springs from this analysis is to do contemporary work on areas of scientific controversy if one wants to understand, without problem, and then explain the way that scientific knowledge is socially constructed. I do not want to say that a symmetrical history of science is impossible. Certain work on historical periods gives every appearance of being as good or better than work on contemporary periods as regard its understanding of what is now taken to be spurious science (I think immediately of Shapin) but I hope that the questions raised here will give rise to a fruitful methodological debate. Historians might offer details on the sources of their confidence in their interpretation of documents pertaining to spurious ideas which have passed from the scientific scene. (Collins 1981, 380)

As I see it, the whole principle of symmetry makes it almost impossible (except in some cases) to pursue a proper reconstruction of the past in science. It is almost impossible for historians to "offer" all the details about the sources that are needed to establish an irrefutable interpretation of the documents that will allow them to explain not only the victorious science, but also the spurious one.

The symmetry principle had a purpose. It was intended to encourage historians, philosophers, and sociologists to broaden their understanding of science beyond the reductionist programme that conceives of science as merely the results of discoveries (names, theories, laws, or theorems) that remain historical. If it is possible to develop such a perspective, then the division of cultures within a society will be overcome. If we are to explain all achievements and false science in the same way, in a causal form of explanation, this means that both successes and failures are contingent on the same level. This idea was hotly contested by scientists and philosophers in the decades when *Leviathan and the Pumped Air* were published, and for many years afterwards. This debate came to be known as the Science Wars, and, like the war itself, the end result was that the good and the bad sides in the battle could not be clearly demarcated.

What is more important, I think, is the way in which symmetry-causal explanations were conceived in historical investigations of science in the past, which were too difficult to achieve, or at least not in the same way as they could be achieved in a sociometric approach. So, symmetry as a principle was not always applied in the same way. The researcher could more easily find the causes of successful or unsuccessful types of knowledge if he were able to immerse himself in a scientific community of his own time, but it will be harder to achieve the same weight of evidence in some past controversies to maintain symmetry, as proposed by the SSK programme. History may not have been strong enough.

Internal and External Reloaded

This specific problem of the history of science is, at a certain point, the problem of history - as a discipline- itself. To rephrase this problem as a question, I would say: Is it possible, using the methods of history, to satisfy the requirement of symmetry in causal explanations of successful and unsuccessful knowledge? The English sociologist Paul Rock had defined why it is almost impossible for historians to succeed in a phenomenological reconstruction of the past:

> As mediator and creator of order, the historian produces a particular kind of description whose coherence and plausibility flow from his techniques of reconstructing that everyday reality. He can 'know' the past, but the content and form

> of any knowledge he acquires are finally shaped by his existential relationship with the dead. The dead are not available to him as his contemporaries are; he cannot bodily enter their environment; he cannot converse with them; and he can know them only through fragmented and problematic records. He cannot survey them as he would a contemporary observing the detail of gesture, tone, expression and position. He cannot question them. He cannot assume any community of context or experience to unite him with the dead. He shares no intersubjective world which transcends his existence and theirs. He cannot properly probe his own past for stocks of knowledge which he might possess in common with them. In short, a reconstituted past is phenomenologically impoverished and unsure. (Rock 1976, 354)

Because of these questions, which have reached the limits of historical knowledge, the history of science represents a special task within general history. If an important object of research for the new sociology of science has been defined as the search for scientific controversies in order to know how scientific communities solve their own problems, then not only the cultural environment but also the internal scientific discussion of problems has to be treated. In other words, the internal history of science is needed, although we cannot reduce science to this historical approach alone.

I believe that some of this insight was among the critical responses that the SSK movement received in those decades of academic debate on the history and sociology of science. I would like to recall again Shapin and Schaffer's quote about rejecting all controversies about internal or external history; because -I believe- one of the main philosophical concerns about the construction of the history of science requires a position on this particular task.

As is well known, the dichotomy about internal or external elements in the history of science was given the particular label of Whiggish vs. Tory history of science by an essay published in 1931 by the English historian Herbert Butterfield. The term used to define the particular kind of historiography denounced by Butterfield was not conceived for the particular case of the sciences. In fact, it was part of a new controversy that he was starting with some authors of the English historical tradition, although this tradition could only be vaguely defined.

> There are close to no normal supports for what he says—no footnotes, almost no quotations or examples, and very few references to historians who were attached to the political alliances that in the late seventeenth, eighteenth, and early nineteenth centuries went by the title Whig, in contrast with those called Tories (McIntire 2008, 57).

As E. H. Carr pointed out a few decades later, there were several problems with the label of Whiggish history. First, it wasn't clear to whom the label should be applied other than the historian Lord Acton (Carr 1990, 41). Second, it wasn't clear what kind of new proposal Butterfield had in mind. What's more, Butterfield's essay was not intended as a book on the philosophy of history (namely historiography), but somehow it became a crucial discussion in the philosophy and history of science for several reasons. Butterfield's main concern was not the impossibility of true historical knowledge, but the tendency to judge historical episodes from the point of view of the present. Again, more than a methodological discussion, Butterfield asserted the moral dimension that he perceived in the tradition of English historians. This moral dimension, which led to judgment, could be replaced by a more specialised practice of history. It is time to say that Butterfield was not only speaking against academic historical production, but he also had in mind the products of the literary world, the historical novel. A few years before the publication of his most famous book, the young Butterfield wrote an essay on the relationship between historical books and historical novels. Thus, the main insights he provided in *The Whing Interpretation of History* were part of a long self-reflection on the whole historian production in England, not only in the academic one, but also in his first teaching experiences in general European history courses (Mc Intire 2008, 52). This is an important aspect of the way in which this label, which is so popular in the historiography of the sciences debate, was not in fact a clear label from the outset, not a label thought out for the specificity of the sciences, and above all a defence of a particular way of doing history.

In one respect, however, the problem of the internal/external history of science remains equally relevant to these discussions in general history. Butterfield seemed concerned about a problem in the practice of making the past when he noted that the historian approaches the past not through raw facts but primarily through 'our idea of the past' (Mc Intire 2008, 30). This means that we have a cultural background that gives us a first impression, and this will be a problem if our research into the past is guided and structured acritically by these impressions. But what does critical mean? In his book *The Idea of History*, R. G. Collingwood devotes a chapter to the history of English historians and defines the critical function of history, which was established by the nineteenth-century historian F. H. Bradley and his treatise *The Presuppositions of Critical History*:

> He [Bradley] starts with the fact that critical history exists, and that all history is to some extent critical, since no historian copies out the statements of his authorities just as he finds them. 'Critical history', then, 'must have a criterion'; and it is clear that the criterion can only be the historian himself. (…) What the critical historian has to do is to decide whether the persons whose testimony he is using were, on this or that occasion, judging correctly or erroneously. This decision must be made on the basis of his own experience. This experience tells him what kind of things can happen; and this is the canon by which he-criticizes testimony. (Collingwood 1956, 136-137)

Bradley (through Collingwood), without using the term, speaks of symmetry in historical research. If symmetry requires a central axe to divide two images, that axe is the historian himself, his experience and the canon he uses. The question is how to deal with those testimonies which, in the light of present knowledge, are regarded as false, although they were obtained as genuine knowledge in the past. In this respect, Collingwood advances Bradley's vision:

> Where he [Bradley] goes wrong, I think, is in his conception of the relation between the historian's criterion and that to which he applies it. His view is that the historian brings to his work a ready-made body of experience by which he judges the statements contained in his authorities. Because this body of experience is conceived as ready-made, it cannot be modified by the historian's own work *as* an historian: it has to be there, complete, before he begins his historical work. Consequently, this experience is regarded not as consisting of historical knowledge but as knowledge of some other kind, and Bradley in fact conceives it as scientific knowledge, knowledge of the laws of nature. (Collingwood 1956, 136-137)

So, to put it in more contemporary terms, if Bradley believed that some testimonies of the past are proved false on the basis of present science, no matter how true they were in the past, then for Collingwood the historian should treat all kinds of knowledge as historical knowledge, because history is ultimately called upon to remove those preconceptions of the historian's own experience. Collingwood establishes a deep distance from the positivist claims he sees in Bradley. The latter entertains the idea that current accepted science could serve as a criterion for delimiting true knowledge about past societies. This is not possible for Collingwood, whose approach resembles the symmetry principle of the SSK. It could be said that when the internal/external dichotomy is defended, the historical criterion gives up its place to the scientific criterion of the laws of nature accepted in the present, but when this dichotomy is left behind, there is only the historical criterion, which is the criterion of the historian himself.

For example, we might see a defender of the internal/external division in the Nobel laureate in physics, Steven Weinberg. Weinberg points to another, by no means unimportant, question about the place of the practice of the history of science. If, like Bradley and Collingwood, we accept the idea that the criterion for history is no more than the criterion of the historian, why should we not imagine a history made by the scientist with his own criteria? If we do not want to conceive of a 'historian' in the exclusively limited form of a person trained and graduated in history, then a historian is no more than the person who researches the past. For Weinberg, the Whiggish history of science is a kind of history made for those who practice science, and it is as important and necessary as a history made by historians, sociologists, and philosophers.

> Though the history of science thus has special features that make a whig interpretation useful, it has another aspect that makes the idea of keeping an eye on the present troublesome to some professional historians. Historians who have not themselves worked as scientists may feel that they cannot match the working scientist's understanding of present science. On the other hand, it must be admitted that a scientist like myself cannot match the professional historian's mastery of source material. So who should write the history of science, historians or scientists? The answer seems to me obvious: both. (…)
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> I think that this is because scientific history with an eye to present knowledge is needed by scientists. We don't see our work as merely an expression of the culture of our time and place, like parliamentary democracy or Morris dancing. We see it as the latest stage in a process, extending back over millennia, of explaining the world. We derive perspective and motivation from the story of how we reached our present understanding, imperfect as that understanding remains.
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> Certainly history should not ignore those influential past figures who turned out to be wrong. Otherwise we would never be able to understand what it took to get things right. But the story makes no sense unless we recognize that some were wrong and some right, and this can only be done from the perspective of our present knowledge. Right and wrong about what? A whig history of science that amounts just to a totting up of plus and minus scores for whatever facts a past scientist has gotten right or wrong would not be very interesting. Much more important, it seems to me, is to trace out the slow and difficult progress that has been made over the centuries in learning how to learn about the world: What sort of questions can we hope to answer? What sort of notions help us to these answers? How can we tell when an answer is correct? We can identify which historical practices set future scientists on the right path, and which old questions and methodologies had to be unlearned. This can't be done without taking account of our present understandings, so painfully learned. (Weinberg 2018, 59-60)

The question of the internal/external perspective is reduced to the question of whether scientific knowledge is and should be treated as a different kind of knowledge in modern societies. The main argument for treating scientific knowledge as a different kind of knowledge is that scientific knowledge is generally about nature, and nature is a term used to refer to the world that exists and evolves beyond human action. Collingwood rejects this idea. He recognised that our idea of nature depends on detailed descriptions of some aspects of that nature, which are provided by the natural sciences through detailed descriptions of some particular aspects. Our more general idea of nature is structured for these detailed descriptions, but includes more than that. Nature is thus, in a sense, a general and idealistic concept that is constantly being redefined by the particular and detailed discoveries of the natural sciences in every age. When a scientific fact takes place in a certain period of time, for example the effects of sunlight passing through a prism, this fact is more historical than scientific because: "The fact that the event has happened is a phrase in the vocabulary of natural science which means 'the fact that the event has been observed'. That is, it has been observed by someone at some time under some conditions; the observer must be a trustworthy observer, and the conditions must be such as to permit trustworthy observations. Finally, the observer must have recorded his observations in such a way that the knowledge of what he has observed becomes public property" (Collingwood 1945, 176). So when a scientist tries to reproduce this knowledge, he is not taking a scientific fact, but a historical one. In other words, Collingwood was pointing out that the sciences, despite their apparent autonomy, are only possible because they depend on another body of knowledge: history.

Symmetry for Whom?

In this original conception of what it really means to regard a fact as scientific knowledge, Collingwood introduced the seeds of the new sociology of science. Every fact is a historical fact because it has been produced in a cultural setting that deserves a historical interpretation in order to truly understand that fact. This is, as I see it, a revival version of Goethe's famous statement that prays: "The history of science is science itself", but the anticipation of the main argument of the SSK programme. The second chapter of *Leviathan...* is devoted to making clear how it was possible for Boyle to produce the experimental facts of pneumatic air, and Shapin and Schaffer tried to reverse the internal history that conceived the facts of matter as universal and irrevocable - as Weinberg intended to remark:

> There is nothing so given as a matter of fact. In common speech, as in the philosophy of science, the solidity and permanence of matters of fact reside in the abscence of human agency in their coming to be. Human agents make theories and interpretations, and human agents therefore may unmake them. But matters of fact are regarded as the very "mirror of nature". (…) What men make, men may unmake; but what nature makes no man may dispute. To identify the role of human agency in the making of an item of knowledge is to identify the possibility of its being otherwise. To shift the agency onto natural reality is to stipulate the grounds for universal and irrevocable assent. (Shapin & Schaffer 2011, 23)

This passage strikes at the very heart of the problem of the internal/external distinction in the history of science, and thus at the possibility of conceiving the methodological tool of symmetry in historical modes of inquiry. The authors of *Leviathan...* suggest that, in this respect, Boyle pioneered the foundations of modern science, but not by adopting the mathematical or theoretical steps to which the handbook of the history of science always resorts. For them, it was the experimental investigations (of pneumatic phenomena) that first constructed the idea of a fact of matter, and then the communitarian legitimation needed to accept that these facts were facts of matter. A fact of matter, in Boyle's experimental science, was the result of the experience and interpretation of a phenomena; and the testimony of the persons who will legitimise and give authority to that phenomena as produced by the agency of nature.

> (…) the matter of fact is to be seen as both an epistemological and a social category. The foundational item of experimental knowledge, and of what counted as properly grounded knowledge generally, was an artifact of communication and whatever social forms were deemed necessary to sustain and enhance communication. (Shapin & Schaffer 2011, 25)

This last quote shows the way in which a sociological explanation of scientific knowledge should be constructed: a matter of fact must be conceived as an epistemological and social category; this is only the way in which Shapin and Schaffer have chosen to begin their narrative of the case study of Boyle, Hobbes and the air-pump machine. But it is possible that a scientific historian of the same episode would focus on the phenomenological results of the experience, such as the removal of atmospheric air from the receiving part of the mechanism, and how these experiments (from Otto von Guericke, Galileo and Torricelli to Boyle's *machina boyleana*) helped to prove the existence and utility of managing and producing vacuum. In this respect, Shapin and Schaffer's research could be seen as complementary to those internal narratives about the discussion of the possibility of vacuum that have been going on since Aristotle.

If the history of science is written according to the criterion of historians, the main question, as I see it, is: Who are these historians? I think that in Shapin's and Schaffer's perspective, and also in Steven Weiberg's, no one explains their history as one position among others. Is this question the hidden reason for what the science wars were about? A kind of academic and elite dispute over the power to define science by its past. In a provocative way, the science wars were nothing more than the tensions between different traditions over how to validate the position of the ideal historian of science (with its criterion).

Since we are celebrating the fortieth anniversary of *Leviathan…*, I will limit myself at the end of this article to the principle of symmetry in the light of a historian's sociological commitment to the SSK programme. For the authors of the Edinburgh and Bath Schools, as for Collingwood before them, a scientific fact is first and foremost a historical fact. Recognition of this fundamental task makes it possible to treat each individual fact with the methodological tool of symmetry. But symmetry depends on who the historian is. Even more, it depends on the criterion of that historian, and that criterion is always part of a culturally indexed framework.

The etymological definition of symmetry, going back to its Greek origins, indicates the proper proportion of the different parts of something, material or immaterial (Fré 2018, 9), and it is possible to find it in Plato's expressions about the exact proportion of night to day. Fré reconstructs the use of symmetry along Western history, showing how the roots of the current geometrical and mathematical idea of symmetry carry some of the moral and aesthetic from its origins. One of the most characteristic features of symmetry is its predictive power. If you stand in front of a symmetric image, say a pattern design in a carpet or a wall, it is not necessary for you to recognise the symmetry after every part of the image; it is more like your brain completes the idea of symmetry (Fré 2018, 13). It is difficult to trace a clear analogy of this etimological and definitional symmetry with the symmetry principle of the SSK. However, two aspects of the use of symmetry in both traditions are particularly problematic. First, when the SSK recalls the principle of symmetry in order to evaluate truth and false belief with the same standard of analysis, it forgets the property of prediction that makes symmetry so important in a cognitive sense. Prediction has been one of the main critical aspects pointed out by writers defending the internal aspects of science, the idea that some facts of nature repeat their behaviour beyond human interaction. Secondly, the idea of symmetry needs a point or a line to divide the proportion between two different parts. It is clear how this works in the geometrical dimension, but when we are talking about historical research, does this mean that the historian acts as an abstract point or line, acting in a pure standard to separate equally true from false beliefs?

The European history of science of the seventies and eighties of the twentieth century focused its attention on the question of who, among the disciplines, were the men most capable of reconstructing the great history of the one science. The work of Shapin and Schaffer was intended to lower the tone (Shapin 2010) for the old idea, characteristic of the first attempts to reconstruct the sciences at the beginning of the first century, that only the successful science was part of the narrative. But in a way they perpetuated a problem that is becoming increasingly evident for those historians of science who are not based and working in the European sphere: they acted as if the historian's criterion were a universal one (like a point or a line in geometric symmetry), and the principle of symmetry could be extended in all directions as a methodological tool. One might ask why Steven Shapin - a North American graduated geneticist who later was trained in the history of science - and Simon Schaffer - educated as a child in Australia and trained in the history of science in England - felt and believed that the case study represented in *Leviathan...* was part of a narrative that belongs to their tradition. When the historian is called upon to be the archimedean point or the dividing line to find causal explanations in the past, it is of the utmost interest who he is, how he was formed and educated, and which tradition he perceives as his own.

If the SSK programme was as fruitful as I think it was and is, it is because it was largely based on the official tradition of modern European (and, from the second half of the twentieth century, American) science. It certainly gave a more complex and richer idea of how science was produced, developed, and extended, but it was based on the main assumption that the true definition of science was a European project. Recent developments in the history of science in Latin America, to take the example with which I am most familiar, question and contest this main assumption, offering and suggesting that other ways of conceiving science are more accurate for these contexts. And in doing so, I think they have broken the main force of symmetry.

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