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May the Reinforcement Be with You:

On the Reconstruction of Scientific Episodes

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ABSTRACT: Like theories, reconstructions of episodes in the history of science can possess, or lack, certain virtues such that, when we face two or more different reconstructions of the same episode, we assume that we should choose the most virtuous one". However, we will argue that, with dissimilar reconstructions of the same episode, it is not always necessary to separate the "good ones" from the "wrong ones", and that, as a matter of fact, each reconstruction could provide different but perhaps equally relevant data about the episode, about science in general, and about particular philosophical theses. In order to help us to identify these benefits, we will present a criterion that guides the search for historiographical reinforcement of philosophical theses and we will use it to evaluate three different reconstructions of the same scientific episode.

1. Introduction

What can the history of science say about our philosophical theses? What does the philosophy of science want from the history of science? What are the philosophical goals of using historical evidence in the analysis of science? All these are fundamental questions regarding the nature of the relationship between philosophy and the history of science, and so the importance of these questions has not gone unnoticed by philosophers and historians of science.

During the last century, many different responses have been offered to such questions: on the one hand, the recurring view in the traditional literature of philosophy of science has been very optimistic about

the results of combining history and philosophy of science.[1] On the other hand, a more recent group of philosophers and historians of science have endorsed a more skeptical attitude concerning the philosophical benefits of such a combination.[2] One of the strongest arguments presented in favor of such skepticism could be summarized as follows:

(1) A historical reconstruction of a particular scientific episode constitutes an attempt to provide a rational understanding of a specific historical moment. Historical reconstructions are not only historical data, but also a particular way to put such data together.

(2) In the philosophy of science, historical reconstructions are often used as *case studies*[3].

(3) It is assumed that the use of case studies (from the history of science) can play a central role in the philosophical analyses of science.

(4) When a particular case study is selected because it exemplifies a particular philosophical point, it is not clear that the historical data has not been manipulated to fit the point.[4]

(5) When a philosopher of science starts with analyzing a particular case study, it is not clear where to go from there–for it is unreasonable to generalize from one case or even two or three.

(Conclusion) "Even very good case studies do no philosophical work. . . . They are at best heuristics. At worst, they give the false impression that history is on our side, sort of the history and philosophy of science version of Manifest Destiny . . . it is not clear what philosophical work is being done".[5]

Call this *the dilemma of case studies*.

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[1] Cf. Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (University of Chicago Press, 1977); Imre Lakatos, "History of science and its rational reconstructions", in *The Methodology of Research Programmes* (Cambridge University Press 1978), 102—138; and Larry Laudan, *Progress and its Problems: Towards a Theory of Scientific Growth* (University of California Press, 1977).

[2] Cf. Joseph C. Pitt, "The dilemma of case studies: toward a Heraclitian philosophy of science", *Perspectives on Science 9* (4) 2001, 373—382; Jutta Schickore, "More thoughts on HPS: Another 20 Years Later", *Perspectives on Science 19* (4) 2011, 453—481.

[3] We are fully aware of the fact that there is an ongoing philosophical debate about the status of the different characterizations of case studies, however, we believe this will suffice for the purposes of the paper.

[4] Joseph C. Pitt, "The dilemma of case studies: toward a Heraclitian philosophy of science", *Perspectives on Science 9* (4) 2001, 373.

[5] Idem.

This dilemma is problematic not only because it suggests that, very often, history of science is not able to carry out the functions that philosophers of science expect it to perform, but also because it introduces a scenario in which philosophers of science seldom tell us anything about actual science. If that is actually the case, how can we make things right – that is, how can we use the history of science appropriately as a source of philosophical science, not as mere testing of philosophical theses largely independent of actual history – without losing a considerable amount of our philosophical achievements regarding the understanding of science?

In what follows, we will try to answer only one particular segment of that question. We will focus exclusively on the relationship between history and philosophy of science in reconstructing particular scientific episodes, and we will ask ourselves how we can use and understand particular historical reconstructions and their philosophical interpretations. That is, we will claim that a philosophically fruitful question to ask is: *Under what circumstances and for what purposes is one reconstruction more philosophically virtuous than another?* We will propose a criterion, the *Historiographical Reinforcement Criterion*, which allows us to compare and evaluate dissimilar historical reconstructions by considering the fulfillment of a philosophical goal.

The plan of the paper is as follows. As a starting point, in Section 2, we will briefly discuss some of the implications of examining what makes a particular reconstruction of a scientific episode the best one. In Section 3, we will argue in favor of shifting that enterprise to a more modest one, namely, to answering the question *Under what circumstances and for what purposes is one reconstruction more philosophically virtuous than another?* Moreover, we will present our criterion, arguing that it allows us to evaluate historical reconstructions within a philosophical framework. In Section 4, we will introduce a case study from biology: the debate between the theory of heterogenesis and the theory of biogenesis from the seventeenth to the nineteenth century. In Section 5, we will present three different reconstructions of the same episode that have the intention of supporting different philosophical theses. Finally, in Section 6 we will evaluate such reconstructions by employing the criterion that we presented in Section 3 and draw some conclusions.

2. On *the best reconstruction* plan

The aim of this section is to argue that the project of discovering *what makes a reconstruction of a scientific episode the best* is, at best, highly problematic, and at worst, hopeless.

First of all, a historical reconstruction of a particular scientific episode constitutes an attempt to provide a rational understanding of a certain historical moment. Any such historical reconstruction is supplemented and constrained by an *empirical external history*[6], which is in turn shaped by the historical records and ordered according to a specific historiographical methodology. Therefore, historical reconstructions depend on historical records and specific methodologies, namely, they are not only historical data but also a particular way to put such data together.

Now, if the main task of a historical reconstruction is epistemic, the values that function as regulative ideals for the evaluation of such reconstructions might be plausibly epistemic. That is, if we aim to increase our understanding (or our knowledge) through the use of reconstructions, they should satisfy as many epistemic values as possible. Such values are the ones "that are usually regarded as capable of furthering our knowledge, like 'understanding' or 'explaining'"[7], i.e., values such as simplicity, scope, fruitfulness, consistency, among others.[8]

However, reconstructions are not mere theories, and thus might be evaluated not only according to their possession or lack of some epistemic values, but also according to some methodological criteria as well. In fact, when trying to evaluate historical reconstructions, the *external history* must also be weighed. A standard way to do so is to discover how many methodological virtues the reconstruction possesses, such as historical relevance, historical accuracy, and normative accuracy[9], among others.

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[6] Imre Lakatos, "History of science and its rational reconstructions", in *The Methodology of Research Programmes* (Cambridge University Press 1978), 102.

[7] Mauro Doratto, "Epistemic and nonepistemic values in science", in *Science, Values, and Objectivity* (University of Pittsburgh Press, 2004), 53.

[8] Cf. Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (University of Chicago Press, 1977).

[9] For instance, depending on the studied subject, its nature and the discipline from which it is being analyzed, the standards and demands about generality, precision, and validity, among others, could vary in a non-arbitrary way. Cf.

Thus, it seems that if one wonders what makes one reconstruction of a scientific episode the best, one may answer straightforwardly that the secret lies in the satisfaction of as many epistemic values and the possession of as many methodological virtues as possible. Nevertheless, this response is rather naïve. For instance, as has been shown in the literature, there are too many different approaches as to what counts as an epistemic virtue[10] and different views about which ones should be considered relevant for the evaluation of historical reconstructions. The same goes for methodological virtues. Therefore, it seems plausible to suppose, given the widespread disagreement among experts, that it is very hard, if not impossible, to provide a complete and well-defined set of values and methodological virtues that will allow us to univocally identify the best historical reconstruction at our disposal.

Furthermore, even if it were possible to establish a well-defined set of values and virtues that unequivocally good historical reconstructions ought to fulfill, this information does not provide, absent other facts, a procedure for elaborating a hierarchy of such values and virtues, nor will it necessarily tell us how to choose between two rival[11], (allegedly) equally successful reconstructions of the same episode. If that happens to be the case, we will have a scenario where univocally identifying the best historical reconstructions will still be at best problematic, and at worst, impossible.

In addition, even if by some chance it becomes possible to unequivocally select the *best* historical reconstruction, what should we do with the others? And more importantly, what should we do with the philosophical and historiographical achievements made possible by the use of these reconstructions? It seems that the options are: either we, at least, weaken our confidence in such philosophical and

John Gerring, *Case Study Research: Principles and Practices* (Cambridge University Press, 2007), and Bent Flyvbjerg, "Five misunderstandings about case-study research", *Qualitative Inquiry 12* (2) 2006, 219—245.

[10] Cf. Carl G. Hempel, "Science and human values", in *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science* (New York, Free Press, 1965), 81—96; Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (University of Chicago Press, 1977); Helen Longino, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry* (Princeton University Press, 1990); Hugh Lacey, "Is there a distinction between cognitive and social Values?", in *Science, Values, and Objectivity* (University of Pittsburgh Press 2004), 24—51.

[11] In what follows we will assume that there is rivalry between two reconstructions if (a) the reconstructions emphasize different elements while aiming to recuperate the same historical episode, (b) they seem not to be mutually compatible, and (c) it is expected to choose no more than one reconstruction as the *correct* one for describing the historical episode.

historiographical work, or we embrace the possibility of nevertheless obtaining some kind of knowledge from the use of these not-best historical reconstructions of scientific episodes.

3. Purpose, Understanding and the Quest for Reinforcement

As we argued in the previous section, seeking for a recipe to identify the best historical reconstruction forces us into very difficult scenarios. In response to this fact, different stories have been told about why the integration of history and philosophy of science often ends up being problematic. As an interesting remark, many of such stories emphasize that, most of the time, historians' research goals and commitments are very distinct from the ones of philosophers of science.[12] If that is the case and historians' interests do not always overlap with philosophers' interests, then one should consider changing the question from *Which are the benefits of integrating philosophy and history of science?* to two more modest ones, meaning: *Which are the benefits that philosophers would find through the connections between philosophy and history of science?* and *Which are the profits that historians would get from such acquaintances?* In what follows, we will only focus on the first question.

From a philosophical framework, it has been famously said by Lakatos that "philosophy of science without history of science is empty; history of science without philosophy of science is blind"[13]. It has also been claimed that one of the main roles of the history of science is to provide data that could be used to falsify or support specific methodological and philosophical claims.[14] Additionally, a common philosophical use for historical reconstructions consists in letting the historical data illuminate the rationale

[12] Cf. Jutta Schickore, "More thoughts on HPS: Another 20 Years Later", *Perspectives on Science 19* (4) 2011, 453—481; and Jouni-Matti Kuukkanen, "Historicism and the failure of HPS", *Studies in History and Philosophy of Science*, Part A (55) 2016, 3—11.

[13] Imre Lakatos, "History of science and its rational reconstructions", in *The Methodology of Research Programmes* (Cambridge University Press 1978), 102.

[14] Cf. Karl Popper, *The Logic of Scientific Discovery* (New York: Science Editions, 1995); Thomas Nickles, "Remarks on the use of history as evidence", *Synthese* 69 (*Testing Theories of Scientific Change* Special Issue) 69 (2) 1986, 253—266.

of given scientific decisions and thus enlighten our philosophical understanding of scientific practice.[15] In general terms:

> [T]he intuition behind philosophical use of case studies is often inductivist. More than merely exemplifying philosophical theses, the use of case studies conveys the expectation that there will be more cases similar to the one described, and that therefore the actual episode under study reveals some general or at least typical features of the scientific endeavor.[16]

Taking into consideration all these desired uses of historical data for philosophical purposes, many philosophers and historians of science have provided different treatments to the relation between history and philosophy. Some of them have been very skeptical about the possibility of satisfying such expectations[17], while others have suggested more modest ways to understand and achieve such uses of historical evidence in favor of the philosophical work.[18] However, one may ask if these are the only uses of historical data that can benefit our philosophical work. In what follows, we will claim that an additional goal, namely, the enhancement of our philosophical understanding, should be added to the list.

3.1. Understanding our philosophical theses

There is a common agreement that one of the main purposes of historical reconstructions consists of providing a philosophical or historiographical understanding of certain historical moments, and that one of the main philosophical purposes of such reconstructions is to guide the construction of accurate descriptions and explanations for highlighted scientific phenomena, without allowing historical generalizations to be tagged as "historiographically justified philosophical theses". Moreover, and perhaps as important as the above, reconstructions might reinforce philosophical theses, whether strongly by providing a rationale for

[15] Cf. Thomas Nickles, "Philosophy of Science and History of Science", *Osiris 10* (1995), 139—163.
[16] Katherina Kinzel, "Narrative and evidence. How can case studies from the history of science support claims in the philosophy of science?", *Studies in History and Philosophy of Science*, Part A (49) 2015, 49.

[17] For instance, Joseph C. Pitt, "The dilemma of case studies: toward a Heraclitian philosophy of science" *Perspectives on Science 9* (4) 2001, 373—382.
[18] Cf. Richard M. Burian, "The dilemma of case studies resolved: The virtues of using case studies in the history and philosophy of science", *Perspectives on Science 9* (4) 2001, 383—404; Jouni-Matti Kuukkanen, "Historicism and the failure of HPS", *Studies in History and Philosophy of Science*, Part A (55) 2016, 3—11.

them, or weakly through the idea that reconstructions increase our knowledge of philosophical theses not necessarily by supporting them, but by enhancing our understanding of them by clarifying some of their concepts or applications.

What we mean by enhancing philosophical understanding in relation to historical cases is the possibility of illustrating, clarifying or conveying philosophical concepts through their exemplification in some historical case, either because some set of relevant properties are instantiated in the case or because the case embodies relevant concepts and distinctions in a concrete way, etc. There is literature on understanding[19] which affords evidence that toy models and simplified representations are important tools when it comes to understanding abstract concepts.[20] These features are certainly present even in historical case studies which are lacking in detail or precision and in this respect these case studies are not much different from frictionless planes or a one particle system. Because philosophical theses in the philosophy of science are concerned with the characteristics of actual science, historical case studies, broadly described, similarly enhance our understanding of these theses by exemplifying some of their features and readily bringing them to mind.[21] Taking all this into consideration, we believe a different question might shed light on how to appreciate and evaluate some of the epistemic benefits of historical reconstructions, sometimes, even despite their methodological flaws.

We therefore suggest "the very question at issue" should be *Under what circumstances and for what purposes is one reconstruction more philosophically virtuous than another?* In order to tackle this new question, we must first say something about the philosophical purpose of the use of historical reconstructions.

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[19] "An understanding, on this conception, is an epistemic commitment to a comprehensive, systematically connected body of information that is grounded in fact, is duly responsive to evidence, and enables non-trivial inference, argument and perhaps action pertaining to the phenomena the information is about." Catherine Z. Elgin, "Exemplification in Understanding", in *Explaining Understanding: New Perspectives from Epistemology and Philosophy of Science* (Routledge, 2017), 82.

[20] Cf. Catherine Z. Elgin, "Making manifest: Exemplification in the sciences and the arts", *Principia* 15 (2011), 399—413; Catherine Z. Elgin, "Exemplification in understanding", in *Explaining Understanding: New Perspectives from Epistemology and Philosophy of Science* (Routledge, 2017), 76—91.

[21] Moisés Macías-Bustos helped us to give a better phrasing of our ideas on this point.

3.2. *HRC* and the Philosophical Purpose of Historical Reconstructions

We will now introduce a criterion that allows us to weigh rival historical reconstructions by evaluating how they satisfy the goal of reinforcement; let's call this criterion the *Historiographical Reinforcement Criterion* (henceforth *HRC*).

First, *HRC* sustains that a historical reconstruction could be *philosophically virtuous* with respect to a particular philosophical thesis *X* if it reinforces *X* in some way.[22] Secondly, if one distinguishes between different levels of historiographical reinforcement, it can be seen that historical evidence might reinforce a philosophical thesis in at least two degrees:

> **Strong Reinforcement:** This level of reinforcement is achieved when, given a philosophical thesis (T) and a specific, relevant historical reconstruction (H'), H' provides a rationale for (a significant part of) T.
>
> **Weak Reinforcement:** This level of reinforcement is achieved if, given a philosophical thesis (T) a specific relevant historical reconstruction (H'), H' supports the basic assumptions of T, contributes to a better understanding of T, illustrates mechanisms relevant for the understanding of T, or clarifies some of the concepts of the theory and their applications.

However, we also consider that sometimes, for very diverse reasons, a given philosophical thesis may not find reinforcement in specific historical evidence (which does not necessarily mean that the thesis is mistaken or that the lack of evidence constitutes a refutation of said thesis, but that this particular historical data cannot rightfully illustrate an instance of the philosophical claim). Taking these circumstances into account, we suggest another degree of reinforcement:

[22] It could be argued that this characterization of *philosophically virtuous historical reconstruction* makes every historical reconstruction trivially virtuous (any historical reconstruction will reinforce some philosophical claim). However, we assume that the type of virtue that we have in mind is contextual. Given certain constraints, such as: a specific philosophical thesis, some designated epistemic values, and specific methodological virtues, a historical reconstruction will be philosophically virtuous with respect to such constraints if it somehow enhances our philosophical understanding of the initially chosen philosophical standpoint.

No Reinforcement: The absolute lack of reinforcement occurs when, given a philosophical thesis (T) and a specific, relevant historical reconstruction (H'), H' does not instantiate any elements of T, nor does it contribute to a better understanding of the philosophical thesis in question.

Related notions of *reinforcement* in Laudan's and Elsamahi's works.[23]

Now, we believe that, assuming that one of the main philosophical purposes of historical reconstructions is to serve as a guide for evaluation in philosophical contexts, *HRC* will not only be helpful when evaluating rival reconstructions of the same scientific episode, but will also help to clarify what philosophers mean when claiming that particular historical evidence reinforces a philosophical thesis. In addition, we believe that *Weak Reinforcement* will help us to retain some historical reconstructions that have been shown to lack methodological virtues – for instance, to not be historically accurate – appealing to the fact that they enhance our understanding of philosophical claims. Finally, we consider that the third degree of reinforcement, *No Reinforcement*, allows us to dismiss non-philosophically virtuous historical reconstructions, thus preventing historical evidence from being arbitrarily used to support philosophical theses.

In the next section, we will introduce a scientific episode with at least three different historical reconstructions that aim at supporting quite different philosophical theses.

4. Some History from Biology and Parasitology

The aim of this section is to present a scientific episode from biology, which, in Section 5, will be used to illustrate the application of the *HRC* when evaluating different reconstructions of the same scientific episode.

During the late seventeenth and early eighteenth centuries, symptoms such as nausea, indigestion, vomiting, flatulence, lack of appetite, diarrhea, colored stool, skin eruption, pustules, disturbed sleep, and

[23] Cf. Larry Laudan, *Progress and its Problems: Towards a Theory of Scientific Growth* (University of California Press, 1977); Mohamed Elsamahi, "Coherence between theories", *Canadian Journal of Philosophy 35* (2) 2005, 331—352.

rapid pulse, among others, were assumed to be strongly related to the presence of worms in the human body.[24] More importantly, worms in the human body were assumed to be an important cause of childhood mortality. All this made scientists, doctors, and academic institutions concerned about determining the effects and causes of parasitic worms. At the time, Carl Linnaeus had already listed lumbricius, ascaris, taenia and 25 species of intestinal parasites in the tenth edition of his famous *Systema Naturae* (1758).[25]

To answer questions regarding the origin of life (and more particularly, the origin of parasitic worms), two different biological theories were employed at the time: first, was the new theory of biogenesis, and second was the theory of spontaneous generation: (in the form of abiogenesis as well as in the form of heterogenesis). Both theories were scientifically successful in different senses.

On the one hand, the theory of biogenesis posits that life can only arise from living biologically-related matter, meaning that, in this particular case, parasitic worms could only come from parasitic worms. This explanation was compatible with ontological theories of disease[26] (and particularly incompatible with the humoral theory); it was defended by ontological theorists such as John Arbuthnot (1667–1735) and George Armstrong (1719–1789), who argued that "the only reasonable explanation of abdominal worms was that they were ingested, nourished, and reproduced within the body".[27]

It has to be stated that the theory of biogenesis was strongly supported at the time by experimental success, particularly that of experiments dealing with infusorians. However, in the case of parasitic worms, there was no experimental tradition dealing with the problem during this period, when spontaneous

[24] Cf. Laurie E. Aronstein, *Parasitic Worms: Their Role in Medicine and Science in Modern Europe*, BA project, (Oregon State University), June, 2013; William Black, *An Arithmetical and medical analysis of the diseases and mortality of the human species*, 1788; Daniel Lysons, *An Essay on the Effects of Camphire and Calomel in Continual Fevers* (1772).

[25] See John Farley, "The spontaneous generation controversy (1700–1860): The origin of parasitic worms", *Journal of the History of Biology 5* (1) 1972, 95—125.

[26] "Ontological theorists believed that the source of disease came from outside of the body. Sources of disease included chemical, filth and dirt." Laurie E. Aronstein, *Parasitic Worms: Their Role in Medicine and Science in Modern Europe*, BA Project (Oregon State University), June, 2013, 11.

[27] Laurie E. Aronstein, *Parasitic Worms: Their Role in Medicine and Science in Modern Europe*, BA project, (Oregon State University), June, 2013, 18.

generation was regarded as a possible explanation for the origins of parasitic worms.[28] It also needs to be said that, in 1713, Carlo F. Cogrossi (1682–1769) had already explained the origin of the itch mite (which was believed to be a type of parasite) through the theory of biogenesis and had suggested that any treatment for scabies (which was caused by the itch mite) had to be an ontological one.

On the other hand, the main thesis of the theory of spontaneous generation (in its form of the theory of heterogenesis) was that organic matter was capable of generating living organisms that did not necessarily need to be biologically related to each other. Thus, when trying to explain the origin of parasitic worms, there were diverse hypotheses. Nevertheless, all advocates of heterogenesis agreed that the problem had to be tackled with the help of a physiological theory of disease[29], specifically with the help of the humoral theory.[30] The explanation for the origin of parasitic worms that appealed to the theory of heterogenesis was strongly supported by the success of treatments for symptoms related to the presence of parasitic worms that were based on the humoral theory, as well as by the general acceptance of this explanation.[31]

[28] For an account of this stage of parasitary studies, see John Farley, "The spontaneous generation controversy (1700–1860): The origin of parasitic worms", *Journal of the History of Biology 5* (1) 1972, 95—125.

[29] "Supporters of physiological causes theorized that disease stemmed from a state of being. One does not "catch" an illness, rather the body cycles through an ill state of being. The most prominent physiological theory was the humoral theory. Physicians used the humoral theory to both diagnose and treat illnesses well into the nineteenth century". Laurie E. Aronstein, *Parasitic Worms: Their Role in Medicine and Science in Modern Europe*, BA Project (Oregon State University), June, 2013, 11.

[30] "Humoralists believed that changing the balance of blood in the body cured blood ailments, and realigned the body's humors to a natural state. Purging treatments expelled harmful excesses of whatever humor was causing a disease. Herbal drugs were of particular significance to humoralists, as some herbs induced vomiting, excretion, sweating and fever as means to rid the body of disease-causing fluid imbalances." Laurie E. Aronstein, *Parasitic Worms: Their Role in Medicine and Science in Modern Europe*, BA Project (Oregon State University), June, 2013, 11.

[31] "In 1780, the Royal Academy of Science in Copenhagen held an essay competition on the subject of the origin of parasitic worms. The first two prizes were taken by Marcus Bloch, a medical doctor from Berlin, and Johann Goeze, a pastor and gentleman scientist from Quedlinburg, both whom argued for the spontaneous origin of parasitic worms. The significance of their work was not so much in that both men accepted spontaneous generation, but that for the first time, arguments were put forward which referred to the question of the origin to parasites themselves, rather than to general ideas on the process of generation, on the structure of matter and the prerequisites of theological belief—arguments which had dominated the discussion in the earlier part of the eighteenth century." John Farley, "The spontaneous generation controversy (1700–1860): The origin of parasitic worms", *Journal of the History of Biology 5* (1) 1972, 98f.

Both theories continued to be disputed until the nineteenth century when the French Academy of Sciences ended the debate by favoring the theory of heterogenesis and its main defender at the time, Louis Pasteur.

5. Three Stories, Three Arguments, Three Theses

The aim of this section is to provide three different reconstructions of the same historical episode and to present the arguments that have (or could have) been put forward to argue for the impact of this particular scientific episode in reinforcing specific philosophical theses.

5.1. Story A

Taking into account some of the historical data presented above, the following thesis has been defended by John Farley: Sometimes, theory choice depends only on social aspects.[32] (Farley 1972 and Farley and Geison 1974). More specifically, the Pasteur–Pouchet Debate (1859–1864) was resolved only by considering the French Academy of Sciences' political commitments and by ignoring the explanatory power that the theory of heterogenesis still had at the time.[33] The reconstruction goes as follows.

First of all, the theory of abiogenesis (which claims that living organisms arise from inorganic matter)

> was never an issue in the eighteenth and early parts of the nineteenth centuries, probably as a result of the microscopical discoveries of lower organisms (. . .) the issue at stake was the doctrine of heterogenesis. (. . .) In most accounts of spontaneous generation, there are two assumptions which I regard as invalid: the first is that the controversy raged mainly over the origin of microscopic organisms; the second is the belief that spontaneous generation was disproven by experimentation. (. . .) The fact was, however, that spontaneous generation was

[32] John Farley, "The spontaneous generation controversy (1700–1860): The origin of parasitic worms", *Journal of the History of Biology 5* (1) 1972, 95—125. See also

[33] This thesis assumes that theory choice took place considering exclusively non-epistemic aspects and that criterions such as empirical adequacy did not play any role in the theory choice phenomenon.

> never a doctrine destined for inevitable decay, but on the contrary, held fluctuating degrees of support during the eighteenth and first half of the nineteenth centuries. It was utilized to explain the origin of parasitic worms as much as microscopic organisms, and as far as parasites were concerned, both the quantity and quality of support for spontaneous generation reached its zenith in the first three decades of the nineteenth century".[34]

In addition, in 1780, Marcus Bloch and Johann Goeze won the Royal Danish Academy of Sciences prize and both argued for the spontaneous origin of such parasitic entities. The debate on parasitic worms and the different ways of experimenting with them continued for many more years. Even in the early nineteenth century, from 1815 to 1849, there was still not enough evidence to refute the theory of heterogenesis.[35]

In 1858, Félix-Archimède Pouchet presented a series of experiments to the French Academy of Sciences, arguing to have proved the theory of spontaneous generation. In 1859, he published "Heterogenie ou traite de la generation spontanée", in which he described a sort of plastic force (*force plastique*) found in organic matter that is required to give rise to a spontaneous generational process under certain specific situations. For Pouchet, if water, air, and heat are present, then new larvae of particular organisms can later emerge. However, as soon as Louis Pasteur heard of Pouchet's experiments and conclusions, he immediately argued that they were biased and spent the following years trying to prove them wrong.

The debate lasted for many more years. However, because there was still no conclusive evidence in favor of the theory of biogenesis when the debate between Pasteur and Pouchet took place, and because some of the results of "Pouchet's experiments at higher temperatures would have provided support (at least temporarily) for his theory of spontaneous generation and forced Pasteur to give an explanation"[36], the French Academy of Sciences did not take them as support for the theory of heterogenesis.[37]

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[34] John Farley, "The spontaneous generation controversy (1700–1860): The origin of parasitic worms", *Journal of the History of Biology 5* (1) 1972, 96f.
[35] John Farley, "The spontaneous generation controversy (1700–1860): The origin of parasitic worms", *Journal of the History of Biology 5* (1) 1972, 112.
[36] Antonio Gálvez, The role of the French Academy of Sciences in the clarification of the issue of spontaneous generation in the mid-nineteenth century"; *Annals of Science 45* (4), 1988, 347.
[37] Cf. John Farley and G.L. Geison, "Science, politics and Spontaneous Generation in Nineteenth-Century France: the Pasteur-Pouchet Debate", *Bulletin of the History of Medicine* 48, 1974, 161—198.

5.2. Argument A

The argument in favor of theory choice depending only on social aspects goes as follows.[38] As it has been shown by the historical data, during the seventeenth and early eighteenth centuries, the theory of heterogenesis could be confirmed by empirical evidence but could never be experimentally falsified. In addition, it has also been shown by the historical record that, at the time, the theory of biogenesis could not be proven experimentally. This puts both theories at the same level. No overwhelming evidence was put forward against either of the two theories in dispute. More importantly, with the emergence of *the origin of parasitic worms* as one of the main inquiries of biological sciences at the time, and with the ability of the theory of heterogenesis to explain that particular phenomenon, it is plausible to claim that the theory of heterogenesis was, at least explanatorily, more powerful than its rivals. And because of the lack of conclusive confirmation of either of the two theories at the time (especially because of the lack of overwhelming evidence in favor of the theory of biogenesis), the French Academy of Sciences did not have irrefutable evidence for justifying its final decision in 1864. Thus, this decision was not motivated by any kind of epistemological criteria, but by religious and political factors.

5.3. Story B

Taking into account some of the historical data presented in Section 4, one could suggest the following general thesis: Sometimes, intertheoretic inconsistency toleration takes place in the sciences. More particularly, assuming that no theory choice was necessary at the time one can claim that during the late seventeenth and early eighteenth centuries, a significant inconsistency was tolerated between the theory of biogenesis and the theory of heterogenesis. The corresponding reconstruction goes as follows.

First of all, during the late seventeenth and early eighteenth centuries, scientists knew that the domain of parasitic entities included, among others, the itch mite as well as the 25 species of intestinal

[38] See John Farley, "The spontaneous generation controversy (1700–1860): The origin of parasitic worms", *Journal of the History of Biology 5* (1) 1972, 95—125.

parasites classified by Linnaeus in his famous *Systema Naturae* (1758). Furthermore, even though it was possible to explain the origin of some parasitic entities using one single theory, namely the theory of heterogenesis, and it was likewise possible to explain the origin of some other parasitic entities using only the theory of biogenesis, the explainable domain was nonetheless broader when combining both theories.

Additionally, for doctors and entomologists, it was clear which species were explained by the theory of heterogenesis and which by the theory of biogenesis. For instance, while Cogrossi's explanation of the origin of the itch mite (at the time considered to be a parasite) was accepted through the theory of biogenesis in the early eighteenth century, parasitic worms were satisfactorily explained and treated at the same time through the theory of heterogenesis and its medical applications.

5.4. Argument B

The argument in favor of the inconsistency toleration scenario goes as follows. First of all, there is an inconsistency between two theories if they have mutually inconsistent assumptions or mutually inconsistent consequences. Furthermore, an inconsistency between theories is *significant* if and only if there is a relevant domain that somehow requires the conjunction of both theories.[39] Finally, an inconsistency is tolerated if triviality is not reached, i.e. if scientists are aware of the inconsistency and still do not reach any arbitrary conclusions from the inconsistent union of the theories in question.

Secondly, it is correct to say that in the case of the biogenesis–heterogenesis debate, the basic assumption that one of the two theories was in clear conflict with the basic assumption of the other theory; while the theory of biogenesis said that every living entity arose from another biologically related living entity, the theory of heterogenesis said that some living entities arose from non-biologically-related living matter. This is a clear logical contradiction.

[39] This has been stressed in Kevin Davey, "Can good science be logically inconsistent?", *Synthese* (*Is Science Inconsistent?* Special Issue) 191 2014, 3009—3026.

Thirdly, thanks to then-recent advances in entomology (especially the works of Linnaeus, Vallisneri, and Cogrossi), it was possible to identify an empirical domain that contained many varied entities that were considered to be parasites, including parasitic worms and the itch mite. Thus, the conjunction of the explanations for the origin of parasitic worms and the itch mite was justified if the empirical domain that was being studied was the whole set of parasitic entities. Scientists, therefore, did not have any serious justification for the separation between worms and the itch mite.

Finally, the propositions involved in this contradiction could be pointedly grouped while answering questions about the origin of parasitic entities. The conjunction between them, could therefore be claimed to be justified. As this inconsistent union did not lead to a crisis, the inconsistency was tolerated.

5.5. Story C

Taking into account the historical data presented in Section 4, one could suggest the following general thesis: Theory choice is often guided by the comparison of the problem-solving efficiency of each theory, as suggested by Laudan.[40] More particularly, one can claim that the rivalry between the theory of heterogenesis and the theory of biogenesis illustrates how one theory can be recognized as more adequate (and thus, more acceptable) than its rival when the former exhibits a greater problem-solving effectiveness than the latter.

The corresponding reconstruction goes as follows:

From the late eighteenth to the nineteenth century, academies of science fostered research through prize questions. In 1780, the Royal Danish Academy of Sciences and Letters organized an essay prize on the subject of the origin of parasitic worms; Marcus Bloch and Johann Goeze won this prize by arguing for the spontaneous origin of parasitic worms. Nevertheless, the empirical support for the theory of heterogenesis applied to parasitic worms depended largely on the medical applications of the theory rather than on the explanatory power of the theory as such.

[40] Larry Laudan, *Progress and its Problems: Towards a Theory of Scientific Growth* (University of California Press, 1977).

In addition,

> heterogeneists were convinced that it was the materials ingested by men and animals that produced worms in their intestines. In 1851 the Academy chose a prize-question on the development of intestinal worms. One of the main motives for the selection was the elucidation of the issue of spontaneous generation (. . .) Research on intestinal worms carried out in German universities and in Belgium in the 1830s and 1840s had contributed to weaken the theory of spontaneous generation in different ways.[41]

As a matter of fact, in 1849, E. Van Beneden and G.F.H. Küchenmeister responded to a prize question on the origin of two genera of parasitic worms (cestodes and trematodes); their responses were based on the theory of biogenesis and their descriptions were accurate enough for them to win the *grand prix* and the *encouragement prize*, respectively. Moreover, from 1837 to 1862, thirteen prize questions relating to spontaneous generation and biogenesis were selected by the French Academy of Sciences, and all of them were satisfactorily answered through the theory of biogenesis. All this evidence radically reduced the feasibility of any kind of support for the theory of spontaneous generation. A few years later, the French Academy of Sciences endorsed Pasteur's experimental results in favor of the theory of biogenesis as being conclusive.

5.6. Argument C

The argument in favor of the problem-solving guidance for theory change goes as follows: first of all,

> A theory solves an *empirical* problem when it entails, along with appropriate initial and boundary conditions, a statement of the problem. A theory solves or eliminates a conceptual problem when it fails to exhibit a conceptual difficulty of its predecessor. (. . .) The worth of a theory will depend *inter alia* on how many problems it solves.[42]

[41] Antonio Gálvez, The role of the French Academy of Sciences in the clarification of the issue of spontaneous generation in the mid-nineteenth century"; *Annals of Science 45* (4), 1988, 345—50.

[42] Laudan, Larry (1981), "A Problem-Solving approach to scientific Progress," in I. Hacking (ed.) *Scientific Revolutions.* Oxford: Oxford University Press, p. 148.

And so, theory choice is often guided by the comparison of the problem solving efficiency of each theory.[43]

Now, if one takes the Academy's perspective as a good candidate for illuminating the commitments that scientists had at the time and the results of prize question contests as highlighted data for understanding the cutting edge of the debate between the theory of heterogenesis and the theory of biogenesis. I; and if one also tracks the scope of both theories through the historical records, one will notice that the theory of heterogenesis stopped providing novel predictions in the very late eighteenth century and started explaining phenomena *post hoc* at almost the exact same time. "It was therefore a clear case of a research programme which was not 'progressive' (to use the terminology of I. Lakatos) and did not deserve to succeed"[44].

In contrast, the theory of biogenesis generated scientific novelties at different levels: it helped to discover the very existence of microorganisms, it led to the design of several new experiments and the instruments involved, it provided satisfactory new explanations for phenomena that the rival program could not account for, it helped link different fields of knowledge thanks to its explanatory power, among others. It was a research program that possessed a high degree of problem-solving effectiveness.

Finally, considering that "one theory is more adequate (i.e. more acceptable) than a rival just in case the former has exhibited a greater problem-solving effectiveness than the latter"[45], it could be argued that the comparison of the problem solving efficiency of each theory guided the theory choice made at the moment, and made it clear that the theory of biogenesis was more adequate and acceptable than the theory of heterogenesis.

6. *HRC* and Three Stories

The aim of this section is to show how *HRC* could help us evaluate these three stories in terms of their arguments and philosophical theses.

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[43] See Larry Laudan, *Progress and its Problems: Towards a Theory of Scientific Growth* (University of California Press, 1977), and Laudan, Larry (1981), "A Problem-Solving Approach to Scientific Progress," in I. Hacking (ed.) *Scientific Revolutions.* Oxford: Oxford University Press, pp. 144-155.
[44] Ibid 345.
[45] Larry Laudan, *Progress and its Problems: Towards a Theory of Scientific Growth* (University of California Press, 1977), 153.

First of all, when a historical reconstruction is evaluated, at least some epistemic values and methodological virtues should play a role in its analysis. Secondly, when a relationship between a historical reconstruction and a philosophical thesis is brought to the table, this relation should be weighed as well; while doing so, philosophers may also be guided by the pursuit of some epistemic values and some methodological virtues. As it was introduced in Section 3, *HRC* is a methodological criterion that allows philosophers to analyze the strength of the relationship between the history of science and philosophical theses derived from particular cases.

HRC aims at helping philosophers of science to both evaluate rival historical reconstructions from a philosophical framework, and to clarify the kind of philosophical success that is achieved through the use of specific historical reconstructions. In addition, *HRC* says that a philosophically virtuous historical reconstruction is one that reinforces a philosophical claim in some way. There are three degrees of reinforcement that *HRC* considers: *strong reinforcement*, *weak reinforcement* and *no reinforcement*. Now let us put *HRC* to use.

6.1. Story A and the No-Reinforcement Situation

The first story aimed at showing that theory choice depends only on social elements: In particular that the Pasteur–Pouchet debate (1859–1864) was resolved only by considering the French Academy of Sciences' political commitments, and by ignoring the actual epistemic virtues of the theory of heterogenesis.

First, if Story A were evaluated considering the methodological virtue of historical accuracy, one will notice that its reconstruction is deficient, at least, in one sense: the story leaves out many other instances of the success of the theory of biogenesis, which may have played an important role in the theory choice. The story relies on the relationship between Bloch's and Goeze's success in 1780 and the final decision of the French Academy of Sciences almost one century later. With that enormous gap in the middle, it does not seem that Story A is historically accurate or that its elements were brought together with concern for historical relevance.

Furthermore, in Argument A, the relationship between Story A and the philosophical thesis is very deficient (if not hopeless). On the one hand, the "argument implies a kind of conspiracy theory against Pouchet"[46], but no evidence of this is provided. On the other hand, the argument rests on the assumption of success achieved through prizes being a sort of privileged evidence in favor of a particular theory, but, if that were the case, the theory of biogenesis would have been at least twelve times stronger than its rival because of the thirteen prizes related to biogenesis that were granted during the nineteenth century. However, if such data were taken into account, it would look like the French Academy of Science was justified in giving the victory to Pasteur.

Thus, because of its historiographical flaws, and because of the deficiency of Argument A, this reconstruction cannot provide any type of reinforcement to the philosophical thesis that Farley[47] and Farley and Geison[48] have put forward. In general terms, one can say that, from a philosophical framework, Story A does not reinforce the thesis that says that theory choice sometimes depends solely on social aspects. This is a case of no reinforcement.

6.2. Story B and the Weak Reinforcement Situation

The second story aimed to support the thesis of intertheoretic inconsistency toleration. More specifically, it aimed at providing evidence in favor of the recognition of a significant, tolerated inconsistency between the theory of biogenesis and the theory of heterogenesis.

First of all, Story B seems to be much more precise, as it focuses on a short period and a limited empirical domain. At the same time, Story B takes into account more elements than just Bloch and Goeze's 1870 prize-winning essay; its selected historical data comes from general biology, entomology, medicine

[46] Antonio Gálvez, "The role of the French Academy of Sciences in the clarification of the issue of spontaneous generation in the mid-nineteenth century", *Annals of Science 45* (4) 1988, 346.

[47] John Farley, "The spontaneous generation controversy (1700–1860): The origin of parasitic worms", *Journal of the History of Biology 5* (1) 1972, 95—125.

[48] John Farley and G.L. Geison, "Science, politics and Spontaneous Generation in Nineteenth-Century France: the Pasteur-Pouchet Debate", *Bulletin of the History of Medicine* 48, 1974, 161—198.

and early parasitology, all of which areas of knowledge relevant for the analysis of the parasitic entities. In addition, it seems that the historical data is grouped under a criterion of historical relevance, there are no suspicious information gaps nor shady connections. Nonetheless, we believe there is a problem with the relationship between Story B and the philosophical thesis it supports, namely the historical evidence does not show that scientists were in any way aware of this inconsistency and thus it does not provide any evidence in favor of the toleration of such inconsistency. So, it seems that Story B cannot provide a rationale for the inconsistency toleration thesis.

However, something peculiar has happened. In the literature of inconsistent science, the standard characterization of a significant intertheoretic inconsistency requires three things:

(a) given two theories, they have mutually inconsistent assumptions or consequences,

(b) there is a particular domain that requires the union of the two conflicting theories for scientific purposes, and

(c) *Explosion*, the inference form a contradiction to any proposition whatsoever, is somehow avoided.

Nevertheless, despite the many attempts to find case studies that clearly illustrate the intertheoretic inconsistency scenario, philosophers of science have almost never succeeded at it.[49] Sometimes the two theories do not contradict each other and sometimes the domain that motivates the conjunction of the two theories is missing, among other complications. Yet Story B illustrates the satisfaction of the (a), (b), and (c) conditions very well.

Perhaps Story B does not count as a rationale for the philosophical thesis it intends to support, but it does help clarify what an intertheoretic inconsistency scenario should look like, and so it therefore enhances our understanding of a way in which intertheoretic inconsistencies could take place in empirical sciences. The Story B therefore weakly reinforces the inconsistency toleration thesis.

[49] Cf. Kevin Davey, "Can good science be logically inconsistent?", *Synthese* (*Is Science Inconsistent?* Special Issue) 191 2014, 3009—3026; Peter Vickers, *Understanding Inconsistent Science* (Oxford University Press, 2013).

6.3. Story C and the Strong Reinforcement Situation

The third story aimed at supporting the idea that the rivalry between the theory of heterogenesis and the theory of biogenesis illustrates the way in which the comparison of the problem-solving efficiency of two theories can guide theory choice processes. First of all, we believe that not only the historical data taken into account is adequate for telling a coherent story, but that it is also historically accurate. Additionally, it seems that the assembled historical evidence satisfactorily incorporates the accurate historical data presented by the other two stories without losing clarity or simplicity.

The link between Story C and the philosophical thesis it supports does not seem artificial in any sense. As a matter of fact, it looks like all the historical phenomena that are portrayed in the story are also well described by the philosophical thesis. Story C, therefore, not only enables us to understand the content and the structure of Laudan's problem-solving approach, but also provides a rationale for at least part of the philosophical thesis in question. The relationship between Story C and the Laudan thesis therefore illustrates a case of strong reinforcement.

7. Final Remarks

We claim that dissimilar historical reconstructions of the same historical episode can provide different degrees of reinforcement to philosophical theses.

We also defend that, from a philosophical framework, one can evaluate different reconstructions of the same scientific episode by considering the degree of reinforcement that they can offer to our philosophical claims. In order to allow for such evaluation, we propose a criterion that can help us to identify philosophically virtuous historical reconstructions according to the level of reinforcement that they can provide. This criterion is what we call *Historiographical Reinforcement Criterion* (*HRC*).

HRC assumes that a historical reconstruction could be philosophically virtuous with respect to a particular philosophical thesis *X* if, given certain constraints, it reinforces *X* in some way. HRC allows us to distinguish between different levels of historiographical reinforcement, similar to Laudan's and Elsamahi's notions of (theoretical) reinforcement:

Strong Reinforcement: This level of reinforcement is achieved when, given a philosophical thesis (T) and a specific relevant historical reconstruction (H'), H' provides a rationale for (a great part of) T.

Weak Reinforcement: This level of reinforcement is achieved when, given a philosophical thesis (T) a specific relevant historical reconstruction (H'), H' supports the basic assumptions of T, contributes to a better understanding of T, illustrates mechanisms relevant for the understanding of T or clarifies some of the concepts of the theory and their applications.

No Reinforcement: The absolute lack of reinforcement is achieved, when given a philosophical thesis (T) and a specific relevant historical reconstruction (H'), H' does not instantiate any elements of T, nor does it contribute to a better understanding of the philosophical thesis in question.

We also provide three different historical reconstructions of the same scientific episode and claim that each one of them offers a different degree of historiographical reinforcement to specific philosophical claims.

We want to warn the reader that the different degrees of historiographical reinforcement defined herein could serve as a good guideline when evaluating the relationship between historical reconstructions and philosophical theses, but are not enough to determine the best historical reconstructions in general.

We have suggested that if one wants to evaluate historical reconstructions from a philosophical framework and for philosophical purposes, the *HRC* is a valid criterion. In addition, we have shown how to use this criterion when evaluating different reconstructions of the same episode. We hope to have shown that there is still much to explore regarding the different degrees of reinforcement that historical evidence can provide when evaluating philosophical theses, and that this will stimulate further research in this field.

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