

Transversal: International Journal for the Historiography of Science, 4 (2018) 27-46  
ISSN 2526-2270  
www.historiographyofscience.org  
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## Dossier Georges Canguilhem

### Canguilhem's Concepts

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#### Abstract:

In the 1950s, George Canguilhem became known in France as a vocal exponent of the philosophy of the concept, an approach to epistemology that treated science as the highest expression of human rationality and scientific concepts as the necessary preconditions for the manifestation of scientific truth. Philosophers of the concept, Canguilhem included, viewed concepts as the key to the study of science; and science, in turn, as the key to a substantive theory of reason. This article explains what concepts are for Canguilhem, how they are extracted from the history of the sciences, and why they continue to matter for contemporary debates in the History and Philosophy of Science (HPS).

#### Keywords:

Georges Canguilhem, History and philosophy of science, Historical epistemology, Concepts, Philosophy of the concept

Received: 29 December 2017. Reviewed 13 April 2018. Accepted: 24 April 2018.  
DOI: <http://dx.doi.org/10.24117/2526-2270.2018.i4.05>

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One of history's cruelest tendencies may be that it tends to bury its disciples faster and more unceremoniously than its adversaries. Perhaps not without a profound sense of irony, history appears to forget those who remember it and to remember those who boast about having forgotten it. Georges Canguilhem's legacy, sadly, illustrates this irony well.<sup>2</sup> Even though he was an almost perfect embodiment of what Lopes, Gonçalves, and Salles (2015) call a "disciple of history" (seeing as he spent the better part of his life championing the value of history for the philosophy of science), his contributions to the philosophy of science seem to have been left behind by the callous march of time. While there has been renewed interest

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<sup>2</sup> Georges Canguilhem (1904-1995) was a historian and philosopher of science who played an important role in the French education system in the middle of the twentieth. He was Inspector General of Secondary Education from 1948 to 1955 (a position that allowed him to exert remarkable influence over the teaching of philosophy in France in the postwar period) and President of the committee d'Agrégation in philosophy (a position that allowed him to influence the upper echelons of French thought). See Talcott (2017) and Schrift (2008). In this function, he influenced a number of famous figures associated with 1960s French thought, such as Louis Althusser, Maurice Merleau-Ponty, and Michel Foucault.

in Canguilhem's work in some corners of Europe and the Americas in the last decades, this micro-renaissance has been concentrated in Paris and mostly limited to a small group of philosophers trained in continental philosophy. As Pierre Méthot has observed, "contributions assessing the influence of Canguilhem's philosophy both in France and overseas are almost non-existent" (Méthot 2009, 39). All things considered, Canguilhem's body of work remains virtually unknown, especially among the very expert communities that would arguably benefit most from it: professional scientists, historians, and, of course, philosophers of science.<sup>3</sup>

One can make sense of this unhappy state of affairs in a number of ways. One can point, for example, to the interdisciplinary character of Canguilhem's work, which put him at odds with the very disciplines he sought to reconcile. His emphasis on the importance of the history of science pitted him against professional philosophers of science, who often abide by an ahistorical understanding of science. His incessant references to the history of Western metaphysics and epistemology, from Plato to Hegel, made his work suspect in the eyes of professional scientists, who frequently see his writings as unnecessarily abstract and speculative, as 'too philosophical' to be useful to scientific practitioners. Meanwhile, his belief in the objectivity and rationality of science stultified professional historians who, given the politics of their discipline, have historically defined 'history' as 'political and military history' and looked at scientific history with suspicion, if not disdain. For most of his life, Canguilhem found himself trapped in the negative space between these disciplines: too historical for the philosophers, too philosophical for the scientists, and too scientific for the historians.

Aside from this disciplinary maneuvering, many of Canguilhem's theoretical commitments cut against the *Zeitgeist*, making him appear out of touch with the times. His belief in the autonomy of the life sciences typifies this mismatch between his thought and his broader intellectual milieu. From the 1920s to the 1970s, most philosophers of science viewed physics as a model science and felt comfortable equating the 'philosophy of science' with the 'philosophy of physics.'<sup>4</sup> Collectively, they believed that *whatever is true of physics must necessarily be true (in one form or another) of all other sciences because physics is 'the' scientific endeavor par excellence*. In their eyes, it was not Man but Physics that was the measure of all things.<sup>5</sup> In this physics-centric atmosphere, Canguilhem's demand that philosophers of

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<sup>3</sup> A number of experts have, however, stressed the relevance of Canguilhem's thought. They include Jean-Jacques Salomon, Kevin Thompson, Anton Vydra, Larry Shiner, among others

<sup>4</sup> The philosophy of biology split off from the philosophy of physics and became established as an autonomous a sub-branch of the philosophy of science in the 1980s. By this time, however, Canguilhem's most important works had already been published and would not be incorporated into the incipient 'canon' of this new field.

<sup>5</sup> At the level of philosophical theory, this belief had a number of implications about the source of epistemic legitimacy in science. It meant, for instance, that a discursive field could be 'scientific' only to the extent that it approximated physics in the epistemologically relevant ways (object, method, lawfulness, etc.). It also meant that, at least on some interpretations, the truths of every science could be expressed in physical descriptions of varying degrees of complexity and that, therefore, the language of physics could be reasonably construed as the language of science. At the level of philosophical practice, however, this belief accustomed philosophers of science to philosophizing *centrifugally* (from the inside out) rather than *centripetally* (from the outside in). Those who professed their faith the exceptional epistemological status of physics would often declare physics the 'ground-zero' of science, furiously work on the aspects of physics they deemed thought-provoking, extract general principles from their inquiries, and then, without a moment's hesitation, use these principles as yardsticks to assess the epistemic worth of the other sciences (biology, chemistry, psychology, sociology, economics, linguistics, etc.) without ever seriously considering the possibility that the objects, methods, and laws of these other sciences may not revolve around, or 'reduce' to, those of physics and hence may require investigation on their own terms.

science confront the life sciences without filtering them through the screen of physics was genuinely counter-cultural. This demand expressed the radical thesis that the life sciences are irreducible to the physical sciences,<sup>6</sup> a thesis that, if accepted, would have forced philosophers of science to see science, as Proust once said, “with new eyes” – not as a universe with many worlds that all revolve around a clear center (physics), but as a universe with many worlds and no absolute center. Without a doubt, Canguilhem's contemporaries would have viewed this thesis as a monstrous aberration, as an unwelcome departure from what was considered normal and acceptable for philosophers of science to do.<sup>7</sup>

Fortunately, a number of developments since the 1980s have transformed the public face of the philosophy of science: first, the positivist picture of science (which was responsible for the glorification of physics and the marginalization of historical considerations from philosophical discourse) has, by and large, lost its original luster as post-positivist theories of varied orientations (Marxist, feminist, poststructuralist, etc.) have gained steam; second, the philosophy of biology has emancipated itself from the philosophy of physics and established itself as a self-sufficient branch of the philosophy of science with its own apprehensions, puzzles, and mysteries; third, the history of science has asserted itself as something that even the most prominent philosophers of science cannot dismiss as nonchalantly as perhaps their predecessors would have; fourth and finally, an entire discourse of ‘interdisciplinarity’ has spread over the academy and encouraged the kind of disciplinary border-crossing for which Canguilhem was reprimanded in his time.<sup>8</sup> These developments have altered the landscape in which philosophical discussions of science take place, and they have made the field of the philosophy of science more open to ideas it previously found intolerable, including ideas concerning the irreducibility of disciplines and the historicity of knowledge. By widening the Overton window of philosophical discourse, moreover, these developments have made Canguilhem's writings (in another paradoxical turn of fate, no doubt) seem more ‘contemporary’ now than when they were first published, more applicable to our scientific culture than his. It is almost as if Canguilhem wrote for a time to come – for a time that, as luck would have it, looks uncannily like the present. By delving into the foundations of Canguilhem's philosophy, this article sheds light on what made Canguilhem's works so radical in their time and now makes them so poignantly relevant to ours.

The argument is divided into four sections. Section one situates Canguilhem's philosophy historically and clarifies its relationship to logical positivism, the philosophical school of thought that single-handedly dictated the terms in which most twentieth century debates in the philosophy of science were couched. Section two explains how Canguilhem separated himself from the positivist school by making *concepts* the core preoccupation of the philosophy of science. Concepts, for Canguilhem, are normative categories that condition scientific perception, scientific discourse, and scientific thought. Section two offers an account of Canguilhem's theory of conceptuality that turns on the six chief properties of

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<sup>6</sup> “Today, one would have to be quite uninformed of the methodological tendencies of biologists [...] to believe that anyone can honestly boast of having discovered, by physico-chemical methods, anything more than the physico-chemical content of phenomena, whose biological meaning escapes all techniques of reduction” (Canguilhem 2008, 16).

<sup>7</sup> Another plausible explanation for the relative obscurity of Canguilhem's writings is simply that his work was quite simply overshadowed—for whatever reason—by the work of other French philosophers who gained international notoriety after WWII and became emblematic of the French spirit of ‘68, such as Louis Althusser, Simone de Beauvoir, Maurice Merleau-Ponty, and Michel Foucault, among others.

<sup>8</sup> In the 1940s and ‘50s, when Canguilhem's career was budding, this discourse did not exist and most intellectuals conceived of what we today would call interdisciplinarity principally in terms of *privation*, as a lack of intra-disciplinary mastery rather than as evidence of inter-disciplinary agility, as a sign of intellectual deficiency rather than proficiency.

scientific concepts. Then, section three investigates how these concepts, which hold the key to the Canguilhemian philosophy, are extracted from the history of the sciences, which, according to Canguilhem, involves a deliberate act of *suspension* whereby epistemologists momentarily suspend their philosophical convictions and beliefs. Finally, section four reflects on the importance of overcoming what Brenner (2015) calls “the cultural barrier” that still exists between the history and the philosophy of the sciences. Overcoming this barrier, I argue, requires nothing short of the *becoming-genealogical* of the philosophy of science itself.

## Historical Background: The Dominance of Positivism in the Twentieth Century

To appreciate Canguilhem's contribution to philosophy of science we need to understand the discourse he fought against. Canguilhem's most critical works were written between the 1940s and the 1970s, when logical positivism reigned supreme in the philosophy of science on both sides of the Atlantic. Born in Austria and Germany in “the exuberant ‘modernism’ of the Weimar period” (Friedman 1999, xi),<sup>9</sup> logical positivism viewed itself as a corrective to German idealism. Positivists worried that the German idealist tradition was too speculative to provide a sound basis for rigorous philosophical thinking, especially when it came to the empirical methods of the natural sciences. Inspired by the positivist philosophy of August Comte, logical positivists sought to render philosophy itself ‘positive’, i.e., they sought to model philosophical inquiry after scientific investigation by importing into philosophical discourse the language, methods, and standards of the positive sciences. “It is necessary for the philosopher,” says A. J. Ayer in *Language, Truth, and Logic*, “to become a scientist [...] if he [sic] is to make any substantial contribution towards the growth of human knowledge” (Ayer 1952, 153).

Central to the project of positivism was a linguistic conception of science. The heralds of the positivist creed viewed science as a body of natural-language sentences and maintained that philosophers could triumph in their search of a ‘theory of science’ – a theory that would capture the essence of science itself – by subjecting these natural-language sentences to logical and conceptual analysis using the tools of classical logic and the latest developments in the philosophy of language. For first-generation logical positivists, such as Rudolf Carnap, philosophers familiar with the content of contemporary science and trained in the methods of logical analysis could build a philosophical empire by capturing the logical essence of the first-order language of science in the second-order language of propositional logic.

This search for a philosophical meta-language had two metaphilosophical implications that marked the philosophy of science for most of the twentieth century. First, it reduced the philosophy of science to a philosophy of language insofar as it convinced followers of the positivist tradition that all philosophically interesting problems related to science were, at their root, linguistic problems. The implication here was that philosophers could, in theory, solve any philosophical problems posed by science solely through conceptual clarification and logical correction. Marjorie Grene, a student of Carnap at the University of Chicago during the 1940s, has pointed out that debates among early logical positivists were so mired in linguistic concerns that they inevitably turned into linguistic squabbles, including squabbles about how to best formalize the natural-language sentences and speak about science without raising any linguistic objections. She writes: “I remember an adjuration by Carnap: we must not say ‘This book is about Africa,’ but ‘This book contained the word Africa’” (Grene 2000, 49). For the great masters of the Vienna School and their trainees, the *summum bonum* of philosophical life was a combination of linguistic clarity and logical consistency. For them,

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<sup>9</sup> For a discussion of the politics of logical positivism, see Galison (1990).

the philosophy of science should be clear and internally consistent because a philosophy that was not both was not a philosophy worthy of its name. This implicitly set up the philosophy of science as a 'language game' that was a strictly logical rather than historical in nature. On this view, logic is not a department of philosophy; rather, as A. J. Ayer put it, "philosophy is a department of logic" (Ayer 1952, 57).

Various scholars have observed that the linguistic concerns of logical positivism concealed a drastic philosophical ambition: to turn philosophy into the handmaiden of science. Philosophers, positivists believed, had historically misunderstood the nature of their calling. From the Ionian *physiologoi*<sup>10</sup> of the pre-Socratic period to the German metaphysicians of the eighteenth and nineteenth centuries, philosophers (with a few exceptions) assumed that their job was to unravel the mysteries of nature and make truth claims about ultimate reality, even if those claims were ultimately unfalsifiable. In reality, this job belonged to the scientists whose empirical methods (a) yielded more reliable knowledge about nature than the transcendental methods of the philosophers, which yielded only wild speculation and (b) kept scientists 'in check' by limiting their claims to phenomenal rather than ultimate nature, which restricted the scope of what they could claim to what was in principle empirically falsifiable. But if philosophers were not to make pronouncements about ultimate reality, what were they to do? For positivists, the answer was simple. Philosophers existed to lend a logical hand to scientists in the latter's journey to gain positive knowledge of nature. The function of philosophy was to help science avoid logical contradictions and linguistic ambiguities. For this, however, philosophy had to give up metaphysics.

This is why Grene characterizes logical positivism as a rebellion against German metaphysics, "a rebellion against German (or Germanic) *tiefere Bedeutungen* [deep meanings]." German metaphysics, which was typified in the minds of the young positivist rebels by the systematic philosophy of G. W. F. Hegel, suffered from two irremediable faults. First, it was speculative rather than empirical. It made truth-claims that could not be proven true or false by empirical means and thus were meaningless. Second, it attributed cognitive content to form and style. It held that the form in which philosophical ideas were expressed and the style of that expression were themselves legitimate objects of philosophical investigation, which logical positivists flatly denied.

Although logical positivists zoomed in on the language of science, they adopted a one-dimensional understanding of language. In *Truth and Justification*, Jürgen Habermas (2003), drawing on Wilhelm von Humboldt's linguistics, identifies three functions of language: a *cognitive* function (forming thoughts and representing facts), an *expressive* function (conveying emotions and stirring feelings), and a *communicative* function (dialoguing with others in private or public settings). When I speak in a natural language, such as Spanish or German, I can use the language to achieve different objectives. I can certainly use it to make a factual claim, to assert that something is either true or false. But I can also use it to create a bond with my interlocutor, to discover who my interlocutor is, explore who I am, express disagreement, or even deliberate about what we have in common. Indeed, there is a whole philosophical tradition – running, according to Habermas, from Dilthey to Heidegger – that views language not simply as a tool for representing facts about the external world but as the very element in which all aspects of human life and existence unfold and in which they are immersed. Logical positivists, however, rejected this tradition and collapsed language solely to its cognitive function. While they viewed the language of science as the sole object of the philosophy of science, they did not believe this language served any purpose beyond acting as a mirror of nature, which is to say, beyond reflecting facts so we can make truth

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<sup>10</sup> The term given to pre-Socratic thinkers such as Thales, Heraclitus, and Anaxagoras, *physiologoi* (Greek, *φυσιολόγοι*) meant "those who discourse on nature".

claims about the world.<sup>11</sup> They viewed their object as one-dimensional frame precisely because they conveniently dismissed as irrelevant the aspects of this object that most clearly resisted the advances of logical analysis. What could not be 'logicized' was, quite simply, written off as insignificant.

Here, the difference between positivists and idealists could not be starker. Idealists had a more holistic understanding of language, including the language of science. For them, for example, form and style were not external ornaments devoid of philosophical significance or accouterments that philosophers could either take or leave without consequence. The form in which philosophical thoughts were expressed and the stylistic characteristics of that expression were themselves philosophical choices that contributed to the meaning of philosophical works, and maybe even entire philosophical systems. From the standpoint of at least some idealist philosophers, one could not understand the philosophy of Plato without thinking about its dialogic form; neither could one understand the philosophy of Nietzsche without tarrying with its aphoristic form. This is why when German idealists reflected on the writings of their predecessors, they reflected on the relationship between form and content. This is also why when they themselves put pen to paper, they invested so much time stylizing their own writing and giving it the right form, the form that could achieve the specific effects they were after – effects that were not always tied to the specifically cognitive function of language (as described by Habermas).<sup>12</sup> Not surprisingly, one of the most common criticisms logical positivists hurled at idealists was that, aside from not being empirically-minded enough, they played with form and style so much that their writings crossed over into poetry and literature and thus ceased being properly 'philosophical'.

It is significant that, in theory, logical positivists could have achieved their revolutionary objectives by simply calling for philosophy to shed its idealist skin and trade metaphysical speculation for logical analysis. But as the revolution gained speed, the German and Austrian rebels came to believe that abandoning speculative methods was not enough to purify philosophy from idealist contamination. For that, philosophy had to surrender all aspects of the idealist program that could be linked to its speculative excesses, including (i) idealism's stylistic exorbitance, and (ii) idealism's obsession with history, which positivists considered irrelevant for the philosophical study of science.<sup>13</sup> Even if stylistic adornments and historical excursions were not necessarily implicated in speculative depravity, the worry was that they were symptoms of it, if not causes. By the time logical positivism was in full swing in Europe and North America in the 1950s, positivists had declared metaphysics dead. Gone, they said, were the unreal worlds idealists loved speculating into existence. Gone was the ridiculous idea that form and style are carriers of philosophical meaning. And gone were the days when philosophy was so mired in the past that it was incapable of confronting the present or anticipating the future. Gone, in other words, were all the underhanded affiliates of metaphysics. And good riddance! Metaphysics was dead, and logical positivists gladly claimed responsibility for the kill.

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<sup>11</sup> This explains their dogged insistence that the speculative impulses of philosophy be subordinated to what is scientifically known (or at least knowable), and that philosophical statements be as clear and precise as possible, maximally free of linguistic opacity and logical confusion (Galison 1990, 734).

<sup>12</sup> It is interesting that scholars of German idealism tend to come from English and comparative literature departments as frequently as from philosophy departments. The same cannot be said of logical positivism.

<sup>13</sup> Grene and Depew explain the positivist approach as follows: "Taking fundamental physics, or a caricature of it, as its model, it separated the process of discovery (which it ignored) from the context of justification. Within the latter context, it aimed at a logical reconstruction of sciences, a science that rigorously followed a single hypothetico-deductive method, and that was to issue in the utopian structure of a unified science" (Grene and Depew 2004, 348).

This brings us back to Canguilhem. Grene has claimed that because of its denunciation of form and style and its abjuration of history, the positivist revolt against idealism bred “a singularly dry kind of literal-mindedness, detached from any broader, or deeper, intellectual tradition” (2000, 46). Form and style, for the positivists, could only mystify; history could only obscure.<sup>14</sup> Against this background, Canguilhem’s writings on the life sciences, with their unique style and unapologetic affinity for history, could only appear to positivist eyes as obscurantist mystifications, as archenemies of rigorous philosophical thinking. In spite of its obviously rationalist inclinations, Grene says, positivist philosophers were convinced that Canguilhem’s philosophy was a French celebration of the irrational, a mad bacchanalia of unreason. To them, “[Canguilhem was] just another of those wild continental irrationalists, who, as my colleagues liked (still like?) to say, never produced a single argument” (2000, 50). Not a single one.

Canguilhem’s works provoked this reaction because they grew out of an altogether different intellectual background. They grew out of a number of intellectual traditions, some native to France, some foreign to it.<sup>15</sup> But above all, they grew out of the uniquely French tradition of ‘the philosophy of the concept’, which materialized in France in the early twentieth century. This tradition differed from its Viennese counterpart in that it: (a) did not look down on the more personal, elegant and poetic style characteristic of the French (especially the French *normalien*) and (b) did not abscond from interest in history and deep meanings (*tiefere Bedeutungen*) that motivated so many of the old Germans. Inspired by this tradition, Canguilhem rushed toward everything that positivism regarded with horror. It rushed toward history insofar as it affirmed the history of the sciences as the ground from which the philosophy of science must draw its life and energy, and it rushed toward the *tiefere Bedeutungen* of the Germans, except that instead of defining them as ‘essences’ that are somehow sequestered in the depths of the philosopher’s own mind, it redefined them as ‘concepts’ that inhere in scientific history.

As positivists rebelled against German idealism, Canguilhem rebelled against their rebellion. Yet, he never, for that reason, sided with idealism. He was an enemy of the enemies of idealism; and an enemy of idealism, too. For reasons I explore in below, he refused to shed philosophy’s Germanic past, choosing instead to don it in his very own (and, yes, very French) way. Positivists, of course, could only interpret this move on the part of Canguilhem as capitulation. They interpreted his talk of ‘concepts’ as a relapse into metaphysics and as incongruent with the demands of the rising dogma. So, they kept him at bay. They refused to read him, let alone teach him. They refused even to engage him lest their engagement accidentally dignify him. They never understood that Canguilhem’s philosophy of the concept put as much distance between him and idealists as it did between him and the positivists

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<sup>14</sup> The rejection of style was a consequence of positivism’s emphasis on logical analysis, which is not equipped to deal with aspects of language other than its apophantic function. The rejection of history, by contrast, was a function of positivism’s theory of meaning, which stipulated that only things that can be verified or falsified by experience have cognitive content. Since the past cannot be re-lived or experienced, claims about it can never be either confirmed or falsified and are therefore entirely meaningless. So, without taking stock of the implications of this move, positivists relinquished all history, including the history of philosophy, the history of language, and the history of science.

<sup>15</sup> Numerous currents of French thought influenced Canguilhem, including the idealism of Émile Boutroux, the positivism of Auguste Comte, the historicism of Raymond Aron, the post-positivism of Gaston Bachelard, the mathematism of Jules Vuillemin and Jean Cavailles, not to mention the historiographical interventions of *Annales* historians such as Lucien Febvre, March Bloch, and Fernand Braudel. Traditions that were imported into France but originally took root elsewhere in Europe — such as the phenomenology of Edmund Husserl, the dialectical materialism of Marx, Freud’s theory of the unconscious, and even the logical positivism of the ‘Vienna School’ — also shaped his approach to the history of science.

themselves. They never realized, in other words, that they shared something essential with Canguilhem. They shared idealism as enemy!

The positivist program forced the philosophy of science into an exceedingly narrow frame in which the only thing that mattered was the logical investigation of the language of science, where the language of science was conceived in strictly ahistorical terms. This narrowness is the reason why Canguilhem's philosophy barely registered outside a limited French sphere. Yet, it is also the reason why Canguilhem rejected positivism with as much ferocity as positivism had rejected idealism – because he thought positivism set up nothing but pitfalls for understanding and theorizing science. An alternative to the positivist approach, Canguilhem's philosophy of the concept was designed precisely to avoid these pitfalls.

## The Philosophy of the Concept

From the moment he entered the elite *Ecole Normale Supérieure* in 1924 (“where his first writings were on the positivist August Comte”) to the moment he retired from the Sorbonne in 1971 (“where he developed a reputation as a terrifying examiner”), Canguilhem's main theoretical interest was the evolution of scientific knowledge (Horton 1995, 316). Like Kant, he was fascinated by the categories that condition the production of synthetic judgments (i.e. judgments that give us knowledge about the external world). Unlike the architectonic philosopher, however, he did not see these categories as unchanging terms that lie forever petrified in a transcendental table; rather, he saw them as historically contingent terms that scientists inherit from the past. He called these categories ‘concepts.’

At the most basic level, concepts are schemas of perception, discourse, and thought that affect how and what scientists *see*, *say*, and *think*. Typically, they denote the entities and processes that make up the world of a particular science. The world of the mathematician, for example, is made up of ‘numbers’, ‘sets’, ‘functions’, ‘exponents’, and ‘logarithms’. The world of the chemist is filled with ‘elements’, ‘compounds’, ‘bonds’, ‘moles’, ‘intramolecular forces’, and so on. Meanwhile, the world of the psychologist is a world of ‘impulses’, ‘instincts’, ‘repressions’, ‘action potentials’, ‘cognitive modules’, ‘mental states’, and more. Each of these worlds has its own reality because each operates according to its own concepts. Each concept, in turn, captures something that counts as real – i.e., something that can be meaningfully talked about and plugged into scientific practices and inferences – in each of these worlds.

Canguilhemian concepts exhibit six important properties.<sup>16</sup> First, they are theoretically polyvalent, meaning that their meaning depends on the specific scientific theory in which they appear. In the opening pages of *La formation du concept de réflexe aux XVIIe et XVIIIe siècles*, Canguilhem makes a critical distinction between a ‘concept’ and a ‘theory.’<sup>17</sup> A theory is an axiomatic deductive system with a number of lawful premises that, when combined with statements of facts, can furnish a properly scientific explanation. A concept, by contrast, is simply a term that captures an object that the scientific mind has imbued with a sense of reality and considers a possible candidate for scientific investigation. A concept can appear in a theory (say, as a term in one of its premises), but it is not itself a theory. A theory can incorporate a concept into its explanatory space, but it is not itself a concept. Concepts denote. Theories explain. It follows from this that a *history of scientific concepts* will differ from a *history of scientific theories* because a single concept can appear in more than one theory. Canguilhem draws our attention to this by describing concepts as “theoretically

<sup>16</sup> These are properties I compiled from reading Canguilhem's works, not properties that Canguilhem himself itemizes in any particular place.

<sup>17</sup> Gutting calls this distinction “Canguilhem's most important methodological contribution” (Gutting 2003 52) to the philosophy of science.

polyvalent” (Canguilhem 1955). For him, concepts do not hew to any one theoretical interpretation.<sup>18</sup> And since concepts are parts of theories but theories are not parts of concepts (since concepts do not have parts), the study of concepts is prior, logically, to the study of theories. One cannot make sense of a scientific theory before investigating the concepts it contains. “Before we relate theories in terms of logical content and origin, we must ask how contemporaries interpreted the concepts of which those theories were composed,” he says (Canguilhem 1994, 180). Conceptually, concepts come first; theories, second.

Second, they are historical. Let us consider only one of the many concepts whose histories Canguilhem documented: the biological concept of life. Today, we think of life in terms of DNA. But this is, of course, a recent invention. In an entry for the *Encyclopædia Universalis* written between 1974 and 1975, Canguilhem argues that there have been other conceptions of life in the history of Western thought:

- I. The concept of *life as animation* that dominated antiquity and was most clearly expressed in Aristotle's *De Anima*
- II. The concept of *life as mechanism* that appeared in the seventeenth century, especially in the rationalist philosophy of Descartes
- III. The concept of *life as organization* that emerged sometime in the late eighteenth and early nineteenth centuries, especially in the works of Immanuel Kant and later German romantics such as F.W.J. Schelling.
- IV. The concept of *life as information* that was put into place by the emergence of cybernetics and modern genetics in the middle of the twentieth century.

Canguilhem's point is not that there have been four unrelated conceptions of life in the Occident. His point is that these four conceptions are linked by an assortment of historical and conceptual connections that together make them relevant for the present. His point, in other words, is that our concept of life, which we treat as objective and self-evident, did not drop out of the sky fully formed. It took form in time. It is historical in the sense that it changes over time, but also in the sense that it is, as he argues in *Knowledge of Life*, “constructed” (Canguilhem 2008, 60). This historico-constructivist understanding of concepts led Lecourt (1975) to argue that Canguilhem's philosophy of the concept “belongs in principle to [...] ‘historical materialism,’” (Lecourt 1975, 126) because Canguilhem essentially recapitulates, in the arena of the philosophy of science, the critique Marx leveled against Hegel in the realm of epistemology (19). For Canguilhem, as for Marx, concepts are *products* of a particular kind of work – i.e. scientific work – that unfolds under determinate historical conditions. Concepts are never found. They are formed.

Third, even though concepts are products of history and, as Ian Hacking says, “have memories”, they often suffer from amnesia and forget their own historicity. Concepts often portray themselves – or are portrayed by those who use them – as contemporaries without history, much like Brontë's *Jane Eyre* or Coetzee's *Cruso*. They present themselves, in other words, as ahistorical principles that have always been what they are today. Sadly, this ruse fools only too many scientists and philosophers who, under its spell, see the meaning of a concept as delimited by present-day usage, by how scientists operationalize it in modern-day settings of feedback-controlled action. The view that the meaning of a concept depends solely on its insertion into present-day tactics, institutions, and discourses, explains the “contempt for history” we find even today among so many professional scientists and philosophers of science, who dismiss as epistemologically trivial everything but the most recent scientific theories and discoveries (Canguilhem 1983, 155).

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<sup>18</sup> Canguilhem dismisses as prejudicial the idea that concepts “can originate only within the framework of a theory” (Canguilhem 1994, 179).

Fourth, concepts are behaviorally dynamic. To be sure, no two concepts follow the same historical path. Each concept's history is singular and unique. Still, there are at least four 'trajectory-types' that recur in the history of the sciences: stable, intermittent, branched, and curled.

- ❖ Concepts with a *stable trajectory* appear in a domain at a given moment, play a role in its discourse for a period of time, and subsequently fall out of epistemic grace, thus disappearing from scientific discourse altogether.
- ❖ Concepts with an *intermittent trajectory* are born in a particular domain, operate in it, and eventually disappear, but later re-appear in the same domain like a corpse that has come back to life.<sup>19</sup>
- ❖ Concepts with a *branched trajectory* are born in one domain but then leap to another. After their leap, they either lead two separate lives (one in each domain) or move to the adoptive domain after disappearing from their native one.
- ❖ Concepts with a *curled trajectory* do not re-appear after a period of absence. Nor do they leap from one domain to another. They simply stay put in one domain but undergo such radical transformations over time that at some point they actually transmogrify into what appears to be their very own opposite. They go from 'A' to 'not-A'.<sup>20</sup>

Although these trajectories are only heuristics, they help us visualize the different ways in which concepts can move in and through time. They also emphasize the point that the evolution of concepts, much like that of biological systems, is a protean process that follows no predetermined path. There is no logic to scientific history and there is no 'direction' in which the history of any science is heading. As such, all attempts to detect laws of historical change in history are a dead end, and that there is no room in Canguilhem's philosophy of the concept for a philosophy of history.

Fifth, concepts are autopoietic (from the Greek *autos* meaning 'self' and *poiein* meaning 'to generate' or 'to produce'), meaning they are self-generative or auto-reproductive. As concepts gain traction within an economy of thought, they enter into all kinds of relations with other concepts. Sometimes these relations are 'generative' in the sense that they culminate in the creation of new concepts (Méthot 2013, 119). Like cells, concepts re-produce. *Omnis conceptu e conceptu*. In "La constitution de la physiologie comme science," for example, Canguilhem argues that some of the concepts that helped secure physiology's place as an autonomous discipline in the late nineteenth century came into being through the fusion of other concepts. The concept of the 'internal regulator' came from the generative combination of two other concepts: the concept of 'inner milieu' (that grew out of Harvey's theory of the circulation of the blood) and the concept of 'energy conservation' (that was a cornerstone of early thermodynamics). In this sense, an internal regulator simply is an energy-conserving feature of the internal milieu. Similarly, the concept of 'conditioning' that became the pillar of behaviorism in the twentieth century was born from the co-absorption of the concepts of 'reflex' and 'localization', each of which has a

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<sup>19</sup> The concept of 'preformation' largely dropped out of use in biological discourse by the early nineteenth century. It suddenly returned in the late nineteenth century with developments in cell theory, especially the discovery of chromosomal formations in the cell nucleus (Morris and Bowler 2010).

<sup>20</sup> Émile Bréhier points to Eugène Dupréel's notion of the "anti-concept" to stress this point. Anti-concepts are concepts that negate previous iterations of themselves. An example is the concept of 'negative mass', which evolved directly from the concept of mass yet is fundamentally opposed to it. The concept of negative mass has zero of the substantialist commitments of the classical concept of mass (Bréhier 1964).

history of its own (we shall return to the history of the reflex in section three). A conditioned response simply is a localized reflex.<sup>21</sup> These examples demonstrate that the playful movement of concepts in the discursive space of a science can generate new concepts that can then re-organize in minor or major ways the space of that same science. Indeed, when discussing the history of bacteriology, Canguilhem makes the stronger claim that the capacity to generate new concepts “confirms” the scientific status of a domain. It is almost as if a domain becomes ‘scientific’ only when its concepts start multiplying, which is to say, the moment it “makes unanticipated discoveries and [...] incorporates new concepts” (Canguilhem 1994, 152).

Sixth and finally, concepts are capable of multi-modal signification. They can mean (i.e. convey meaning) in more than one way. Most commonly, concepts mean by denoting specific objects. They help scientists pick out, or refer to, particular objects across dissimilar contexts. But denotation is only the most common mechanism of signification. Concepts also convey meaning by *association*, *resemblance*, or *suggestion*. Imagine that at a certain point in time concept ‘A’ gets associated with concepts ‘B’ and ‘C’ (say, because they co-appear in a popular theory). If the association is strong, it may be difficult for the scientific mind to dissociate these concepts at a later time, even after one or more of them are no longer part of the conceptual arsenal of the relevant science. In these cases, scientists may verbally invoke ‘A’, but what they really invoked is the associated cluster ‘A-B-C’ as a whole.<sup>22</sup> Similarly, concept ‘A’ may resemble concept ‘B’ (say, because they are phonetically or grammatically similar). If they become indistinguishable, the scientific mind will oscillate unconsciously between them even if they denote different things. Finally, concepts also mean by suggestion. Of the different mechanisms of conceptual signification, suggestion is the most difficult one to grasp because suggestions are context-dependent, non-literal, and elusive, and because philosophers of language have systematically neglected the suggestive function of language.<sup>23</sup> Still, concepts may explicitly declare one thing but suggest another, and what they suggest can be, epistemologically speaking, as momentous as what they declare.<sup>24</sup>

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<sup>21</sup>Canguilhem uses this understanding of the concept of ‘conditioning’, for example, to argue that Pavlov may have more in common with distant thinkers who worked on the theory of reflex and the conservation of energy than with many of his contemporaries. These examples suggest that conceptual innovation in science is not the work of ‘genius’ but the almost inevitable consequence of the ‘play’ of concepts in a wider economy of thought (Canguilhem 1983, 226-73).

<sup>22</sup>This is Canguilhem’s reading of the concept of the normal and the normative. These terms have been paired for so long in the history of the life sciences (especially medicine) that scientist often fail to recognize the critical difference between them, assuming that what deviates from the normal (conceived as a statistical average) is also abnormal (conceived in terms of a value judgment) (Canguilhem 1991). It is also Canguilhem’s reading of the concept of the cell, which has historically been difficult to disambiguate from the concept of ‘the individual’. “The history of the concept of the cell is inseparable from the history of the concept of the individual. This has already allowed us to maintain that social and affective values hover above the development of cell theory” (Canguilhem 2008, 42).

<sup>23</sup>The suggestive function of language has been systematically neglected in the philosophy of language and the philosophy of science, perhaps because it is impossible to impute a suggestion to any of the manifest ingredients of a speech act or perhaps because it is hard to differentiate suggestion from other modes of cover signification (such as hinting, innuendo, insinuation, and implication). While some philosophers of language have begun theorizing about linguistic implicature, the philosophical tradition in which suggestion has been most regularly debated is Indian philosophy, which recognizes numerous varieties of suggestion. See Kunjunni (1977), Chari (1977), and Keating (2016).

<sup>24</sup>Concepts rooted in metaphors often operate in this way. Consider Darwin’s concept of ‘the struggle for existence’, which became a key component of his theory of evolution via natural selection in 1859. Explicitly, the concept simply stated that some organisms inevitably perish before reproducing and thus fail to pass on their traits to the next generation. Implicitly, however, this concept (which was

These properties – theoretical polyvalence, historicity, forgetfulness, behavioral dynamism, autopoiesis, and multi-modal signification – give us an outline of the theory of conceptuality that underpins Canguilhem's philosophy, even if he never formulated this theory explicitly in any of his works. This theory allowed Canguilhem to bypass the ahistorical and logical philosophy of the positivists and reclaim the *tiefere Bedeutungen* of the Germans *without thereby resuming the idealist project of metaphysics*. Recall that the two core ideas that define Canguilhem's philosophy are: (a) that epistemologists are more likely to make headway in the study of science if they focus on studying the concepts that govern scientific perception, discourse, and thought rather than the syntax of scientific language (Carnap 1938), the morphology of the scientific method (Popper 2005), or the social determinants of scientific practice (Bloor 1984), and (b) that the best way to probe what Henning Schmidgen calls "the life of [scientific] concepts"<sup>25</sup> is historically, by looking at moments of genesis, patterns of change, and evolutionary arches; by asking when they were born, why they mutated, how they evolved, and whether or not they still have a presence in modern scientific practice. These ideas led Canguilhem to the philosophical conclusion that *there is no straight road from philosophy to epistemology*. To become epistemological, philosophy must take a detour through the history of the sciences. As we shall see in the next section, however, this detour cannot succeed unless philosophers suspend their philosophical convictions before embarking on it, unless they leave some cherished possessions behind.

## Canguilhem's Method and Suspended Judgment

As a disciple of history, Canguilhem always stressed that concepts can only be studied historically, through rigorous archival research.<sup>26</sup> Yet, an aspect of his method that has not received attention in the literature is his worry about the risks of historical research. Historical research is not fundamentally good or bad, but it is fundamentally risky. One of its dangers is that it can fool researchers into thinking they have found what they have in fact fabricated, that they have reached a new destination when they never even left their point of departure. More specifically, Canguilhem worried about projection. He worried that researchers might have no way of knowing whether the concepts they "see" in the history of science are really concepts that emerge *from* this history or concepts the researchers have projected *onto* it. To avoid this problem, Canguilhem concluded, researchers should undergo a unique form of self-analysis before (and during) archival research whereby they reflect on their own preconceptions, biases, and assumptions, identify them, and then actively suspend them. In this section, I look at two concrete historical cases that highlight the dangers of *not* suspending judgment when constructing historical narratives: (i) Henri-Marie Ducrotay de

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taken from political economy) led natural scientists to view nature as a Hobbesian war of all against all. This led natural scientists to think of the 'struggle' in question almost exclusively as a struggle among organisms (rather than between them and their environment), which prevented them from considering the possibility that animals may cooperate in nature in order to survive the elements. The metaphor of the struggle for existence closed the door on discussions of altruism and affected the direction of biological and ethological research for much of the twentieth century. See Gould (1988).

<sup>25</sup> Canguilhem once joked that his interest in concepts may earn him a reputation as a "conceptualist fossil" (1988, x). For a discussion of the Canguilhemian theory of concepts, see Schmidgen (2014).

<sup>26</sup> In *Knowledge of Life*, Canguilhem cites the work of Paul Langevin, who speaks of the value of the history of science for scientific pedagogy. According to Langevin, a close attention to historical sources is likely to give science students a better image of what scientific practice is all about than an ahistorical education that merely teaches contemporary knowledge in a vacuum. A historical methodological will teach students that the scientists whose theories we now categorically reject were acutely aware of the limits and shortcomings of their theoretical propositions, and it will educate them about the dangers of dogmatically attaching oneself to the theories that one has been reared into simply because we feel certain that those theories are true (Canguilhem 2008, 27).

Blainville and François-Louis-Michel Maupied's 1847 *Histoire de sciences de l'organisation et de leurs progrès comme base de la philosophie* and (ii) Emil Du Bois-Reymond's 1858 history of reflex. Canguilhem critiques both works for failing to bracket second-order judgments and argues that this failure prevents them from tapping into the conceptual dimension of the history of science that alone can give history epistemological relevance.

In a 1979 article published in *Revue d'histoire des sciences* under the title "L'Histoire des sciences de l'organisation de Blainville et l'abbé Maupied," Canguilhem chastises Blainville and Maupied for failing to shake off second-order judgments in their interpretation of French scientific history in their 1847 book, *Histoire de sciences: de l'organisation et de leurs progrès comme base de la philosophie*. Pieced together from lectures Blainville delivered at the Sorbonne between 1839 and 1841 and presented as a survey of the evolution of the organic sciences in Europe from antiquity to Napoleon, this text claims to prove: the scientific character of scripture and the linear and progressive character of scientific history. And it claims to prove this based solely on the historical record.

But Canguilhem argues that, in reality, Blainville and Maupied assume these conclusions from the start. Prior to any investigation of the history of French science, Blainville and Maupied were already convinced that all knowledge originates in divine revelation and that all nomological knowledge of nature is procured by reading the mind of God through his works.<sup>27</sup> As Enlightenment thinkers, they also believed history follows a rectilinear and progressive trajectory and that the history of science best illustrates this linearity. These convictions pre-determined the outcome of their investigation and explain some of the more puzzling features of their book, such as its countless references to Aristotle. If the reader of the *Histoire* gets the uncanny feeling that every other sentence ends with a bizarre reference to Aristotle, this is because the authors embrace a progressivist philosophy of history according to which all later historical events are embryonically contained in earlier ones. For Blainville and Maupied, "Descartes, Bacon and all the others [...] are merely the logical consequence, the elaboration, of Aristotle" (Canguilhem 1991, 59) – the one philosopher among the gentiles.<sup>28</sup> They saw no meaningful difference between the teleological worldview of Aristotle and the mechanistic one of Descartes. Their belief that history forms an unbroken line of progress primed them to see continuity where there is discontinuity, to assert similitude where it makes more sense to assert difference.

Blainville and Maupied also applied a questionable 'principle of selection' to their subject matter. Because they believed in the necessary compatibility of science and scripture and in the linearity of all history, they include in their historical narrative only evidence that coheres with their philosophical beliefs while excluding any events that, as they themselves put it, "[did not] push science in the right direction" (Canguilhem 1991, 61). The notion that there is a 'right direction' in history is, of course, a philosophical judgment about the nature of history that precedes the investigation and impacts what, for the authors, counts as historical evidence. Toby Appel has observed that Blainville and Maupied deliberately selected out of their analysis everything that undermined their principle aim, which was "to

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<sup>27</sup> Blainville, in the "Introduction," writes: "I conceived and carried out my *Histoire de l'organisation* as a possible foundation for philosophy, while at the same time demonstrating that philosophy is one and the same thing as the Christian religion, which is so to speak only an a priori, revealed to man by God himself when the state of society required it" (quoted in Canguilhem 1991, 60). Some have speculated, however, that this religious orientation was intensified by the edits of Maupied, who was a priest.

<sup>28</sup> The periodic references to Aristotle are themselves revealing since Blainville and Maupied present Aristotle as the first philosopher to have truly understood that knowledge is a 'reading' of God's works rather than a 'creation' of the human mind (as stipulated by the Catechism of the Dioceses of Paris). In tracing modern scientific developments back to the Aristotelian philosophy, the authors simultaneously 'prove' their thesis of historical continuity and lend 'support' to their belief that all knowledge comes from revelation.

support Catholicism with biology” (1987, 287). As such, the *Histoire* relies on a principle of selection that combines a theory of knowledge rooted in Judeo-Christian hermeneutics with a run-of-the-mill, Enlightenment-style adoration of progress for progress's sake that render it epistemologically suspect.<sup>29</sup>

Another example of what can go wrong when scholars working on the history of the sciences fail to suspend prior beliefs is Emil Du Bois-Reymond's 1858 history of the reflex, which Canguilhem's *La formation du concept de réflexe aux XVIIe et XVIIIe siècles* directly rebuts. Du Bois-Reymond's 1858 thesis is straightforward: the concept of the reflex, which appeared in the discourse of physiology in the nineteenth century, originated in the mechanical writings of René Descartes, especially *The Passions of the Soul*, and not in the vitalist writings of either the English doctor Thomas Willis (a Professor of natural philosophy at Oxford) or the Czech-Austrian physiologist Georg Prochaska (a professor of anatomy at the University of Prague). As Canguilhem says: “[Du Bois-Reymond] refers to Descartes the honor of having ingeniously anticipated, in matters of the reflex, the word and the concept” (Canguilhem 1955, 139).

Yet, according to Canguilhem, this narrative is doused in philosophical valorizations, including a dogmatic commitment to the supremacy of the mechanistic philosophy and an unswerving devotion to German nationalism. Well before he spilled a single drop of ink on the history of the reflex, Du Bois-Reymond had spilled many defending the value of mechanistic explanations of natural phenomena and discrediting all strands of non-mechanistic thought, especially the vitalist school comprised by Georg Stahl, Xavier Bichat, and Arthur Schopenhauer, among others. There is a real sense, then, in which Du Bois-Reymond was fated to consign to the history of mechanistic thought the origins of concept of the reflex not because this is what the historical record shows but because he was already convinced that the concept of reflex is thoroughly mechanistic and that the mechanistic philosophy is the archetype, if not the very locus, of all epistemic legitimacy. Rather than tracing the history of the concept of the reflex wherever it led, the 1858 history names as the founder of the concept of the reflex the man who fathered and emblemized the mechanistic paradigm so as to condemn as “guilty of metaphysical sin” the school of *Naturphilosophie* to which vitalists like Prochaska, and before him Willis, belonged (1955, 140). “It was not so much for reasons of pure physiology as for reasons of philosophy,” Canguilhem concludes, “that Descartes was anointed a great physiologist and illustrious precursor” (Canguilhem 1988, 56).

In exalting Descartes and “putting down” Willis and Prochaska, Du Bois-Reymond made more than a philosophical point about the right way to think about natural phenomena (Canguilhem 1955, 140). He also made a political point about the ascendancy of German culture and the supremacy of German science. While Du Bois-Reymond honors Descartes (a Frenchman) as the founder of the concept of reflex, he presents the German physiologist Johannes Müller – whose experiments on reflex-reactions helped disseminate a mechanistic theory of life in Germany in the mid-to-late nineteenth century – as the European heir to the Cartesian empire. For Du Bois-Reymond, Müller was a German Descartes. The 1858 history, therefore, is really a story about the rise of Germany's scientific *Kultur* and a testament to “the nationalist supremacy of a ‘strong’ [German] science over the science of a dominated nationality embodied in this case by Prochaska” (Lecourt 1975, 177). It is also an intellectual autobiography of sorts. Du Bois-Reymond himself succeeded Müller as Chair of Physiology at the University of Berlin in 1858 – the same year his history of the reflex was published. It is no

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<sup>29</sup> After reading Blainville and Maupied's work, August Comte protested that their work offered “only an irrational succession of biographic and bibliographic notices” (Pickering 1993, 492). It didn't pay attention to the aspects of scientific history that might actually interest practitioners and historians of science. It lacks, for example, any attempt to explain “the connection between biological theories and discoveries” (ibid).

surprise that in it, Du Bois-Reymond draws a direct line between himself and Müller (the father of modern philosophy who, by then, was a household name in German science), and between himself, Müller, and Descartes (the father of the mechanistic philosophy Du Bois-Reymond himself worshipped). At once metaphysical treatise, nationalist manifesto, and intellectual autobiography, the 1858 history of the reflex is a hodgepodge of valorizations masquerading as history.

Of course, the real danger of second-order philosophical judgments is not that they yield self-aggrandizing histories awash with petty nationalism and glib philosophizing. Their real danger is that they can take command of the research process and make it functionally impossible for researchers to see any 'forms' or 'patterns' in history other than the ideological arabesques that we project onto it. If left unchecked, these judgments infiltrate historical research and jam it from within. Once this happens, we are more likely to miss important links, hints, and traces that might prove essential for understanding the historical origins or trajectory of a particular scientific concept. It is in relation to this concern that we must interpret a work such as *La formation du concept de réflexe aux XVIIe et XVIIIe siècles*, where Canguilhem sets out to 'set the record straight' by showing that an altogether different understanding of the history the reflex is possible when we hold second-order judgments in abeyance.

*La formation* offers an alternative account of the history of the reflex to the one Du Bois-Reymond puts on the map by arguing that Descartes did not invent the reflex concept. Willis and Prochaska did. It was the vitalist writings of Willis and Prochaska (who conceived of life as light) and not in rationalist works of Descartes (who conceived of life as mechanism) that rendered the reflex concept thinkable for the first time in scientific history. By using light as a model and metaphor for life, Willis and Prochaska envisioned certain bodily movements that do not seem to be under the control of an organism's executive function as 'reflections' that bounce off the nervous-motoric system like light off the surface of a white wall. In thinking of bodily movements in this way – i.e. as autonomous reflections – these vitalist thinkers invented the concept of the reflex and paved the way for its introduction into physiological discourse. The fact that Descartes, as Du Bois-Reymond observes, made a passing reference to 'reflected spirits' in *The Passions of the Soul* is irrelevant since the concept of the reflex that was incorporated into physiology starting in the 1850s had much more in common with the vitalist notion articulated by Willis than with the mechanistic one formulated by Descartes. Descartes's theory, for example, posited that all reflexes involve a 'pathway' that leads from sensory stimulus to motoric reaction *through the brain*. By contrast, Willis's theory de-centralized reflexes by suggesting that their 'pathways' are localized and do not pass through brain at all. This second conception was closer, epistemologically, to the concept of the reflex that was eventually woven into the conceptual fabric of physiology in the late 1800s as most physiologists defined reflexes as behaviors that are not under executive control, i.e., as behaviors not mediated by the brain.<sup>30</sup>

In an impressive turnabout of Du Bois-Reymond's thesis, Canguilhem shows that the origins of the modern concept of the reflex are found precisely in the tradition of

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<sup>30</sup> Descartes's theory explained reflexes vis-à-vis a mechanistic theory of strings and pulleys modeled after Harvey's theory of the circulation of the blood, whereas Willis's explained reflexes vis-à-vis a vitalist theory of combustion. Gary Gutting writes: "Willis explain bodily proceses in terms of the chemistry of combustion and a vitalistic notion of the animal soul. His accounts were therefore far removed from the Cartesian mechanism that seems the natural locus of reflex movement. Nonetheless, Willis was led, as Descartes was not, to the two central elements of the concept of the reflex. First, he clearly distinguished the cerebellum, as the center of involuntary movement, from the cerebrum (or brain proper), as the center of rational thought and voluntary action. This effected the crucial decentering of reflex movement. Second, he conceived of reflex movement as a genuinely symmetrical process of back-and-forth motion by the animal spirits, explicitly employing the term *motus reflexus* in analogy to the reflection of light" (Gutting 1989, 36-7).

*Naturphilosophie* that Du Bois-Reymond denigrates. No doubt, Du Bois-Reymond is right that *The Passions of the Soul* contain the word 'reflex,' but he is wrong to claim that this work is also the birthplace of the concept expressed by this word. It is only in the thinking and writings of Willis and Prochaska, and their disciples, that we find the word *and* concept the side-by-side. "Concerning the reflex, we find in Willis, the thing, the word, and the notion" (Canguilhem 1955, 68). What led Du Bois-Reymond astray was his top-down approach to the history of the sciences, which produced a history from pre-existing beliefs when it should have formed beliefs from a pre-existing history. Du Bois-Reymond's failure was a failure of suspension.

Although Canguilhem never claimed that it would be possible for him – or any other epistemologist, for that matter – to reach a pure and objective understanding of the history of the sciences, he believed it was paramount for practitioners to bracket philosophical and theoretical prejudices. History writing may not be a science. But neither should it be a free-for-all in which all claims are equal. There are better and worse explanations, there are histories we can embrace as elucidating and histories we can dismiss as misguided or mistaken. Indeed, this is what Cristina Chimisso calls the "negative part" of historical epistemology, the side that enables epistemologists to evaluate and possibly reject historical explanations that are either unsubstantiated or injudicious. We see this negative part at work clearly in Canguilhem's history of the reflex, which "establish[es] that certain continuities were delusions created by [problematic] reading of past texts" (Chimisso 2003, 312). The mere presence of this negative dimension suggests that, far from being a rash celebration of irrationalism, Canguilhem's philosophy of the concept is committed to norms of evidence and objectivity in history writing.

## **Becoming-Genealogical: On the Cultural Barrier Between Philosophy and History**

Canguilhem devoted his life to interrogating the concepts that breathe life into science. And he was firm in his conviction that these concepts are not, as the early logical positivists maintained, merely "abstractions from our use of words" (Glock 2008, 42). They are historical schemas that condition what epistemic agents can perceive, say, think, and know in scientific spaces. One of the most drastic implications of this historicist position is that philosophers who fail to engage the history of the sciences simply fail to engage the concepts that animate scientific rationality and, consequently, fail to do epistemology. To avoid this failure, philosophers must develop a historical conscience and come to see the philosophy of science not as a department of logic, as A. J. Ayer wished, but as a department of genealogy.

But what does it take for the philosophy of science to become genealogical? Merely recruiting a handful of historians who dabble in philosophy or a handful of philosophers who dabble in history will not suffice. This transformation will require something more drastic and, also, more exciting: the birth of an altogether new character, "the epistemologist", who is fluent in the languages of historians, scientists, and philosophers but is not identical to any one of them. A synthesis of these other figures, the epistemologist will have expertise in the history of science (like the historian), in present-day scientific knowledge (like the scientist), and in the history of epistemology and metaphysics (like the philosopher); but, because of her unique perspective, she will also pose questions and venture answers that neither of these other figures can – questions and answers that may occasionally call into question the mission and self-understanding of these other figures and their respective disciplines.

For this new character to emerge, however, the material conditions under which the philosophy of science unfolds need to change and the "cultural barrier" (Brenner 2015) that currently keeps the analytic philosophy of science and the history of science apart must be dismantled. This barrier has ravaged the history of these disciplines and produced a situation

in which philosophy and history cannot see their interests reflected in those of one another and consequently cannot imagine a shared future. While the recent emergence of the field of 'HPS' (The History and Philosophy of Science) has indeed chipped away at this barrier, the situation has not substantively changed. Today, the history of science is still conducted almost exclusively by a small cohort of historians who happen to be interested in the scientific past but who have no real interest in the epistemological difficulties raised by the contents of scientific history. The philosophy of science, on the other hand, still falls within the ambit of analytically trained philosophers who, as Thomas Uebel claims, "profess to care little for history" (2010, 13). In this environment, it is not uncommon for professional historians to decry philosophical theory as aloof and for philosophers to dismiss the history of science as nothing but the "cold-case squad" of the philosophy of science (French and Saatsi 2014, 363). Converting the philosophy of science into a subdivision of genealogy, then, requires re-educating the philosophy and the history of science about the power of each other's tutelage and the value of studying concepts genealogically, i.e., in terms of the epistemic functions they have served in scientific settings at different points in time.

Queloz (2017) notes that analytic philosophers of science steer clear of genealogical investigations because such investigations are thought to lack normative power. Studies of origins might be interesting and educational, the argument goes, but they are irrelevant to "the space of reasons" because origins are not justificatory. Queloz counters, however, that genealogies *can* justify conceptual practices. It is just that mainstream philosophers of science have misunderstood how they do so. Genealogies do not justify contemporary conceptual practices by identifying a point of origin and imputing normative power to it. Rather, they do so by identifying a point of origin *and then tracing* the various reasons epistemic agents had for continuing to embrace said practices, even when there were viable alternatives. By both showing that the choice to continue a practice was rational given local conditions and also that our present-day commitment to such practice is the direct result of these choices, a genealogy can lay bare the normative dimensions that connect past and present and thus offer us a justification of the present rooted in the past. This, I argue, is how Canguilhem's histories should be understood, i.e., as genealogies whose objective is not simply to describe the evolution of scientific concepts but also to justify the epistemic value of concepts that continue to play a role in contemporary science. His histories are "normative histories" that judge (Chimisso 2013).

Genealogies, however, do more than recognize the normative potential of history. Especially in the wake of Nietzsche and Foucault, genealogies recognize history as what de Certeau (1994) calls a "polemological space," i.e., a space in which a number of strategies and tactics constantly collide with one another. Genealogy, as Foucault observes in the opening sentence of "Nietzsche, Genealogy, History," "operates on a field of entangled and confused parchments" (Foucault 1984, 76). It recognizes that its object – be it Christian moralism (as in the case of Nietzsche) or the experience of madness (as in the case of Foucault) – is constructed in an environment that is permeated by power relations in which a large number of actors, discourses, disciplines, and institutions are involved. In the case of science, Foucault says, genealogy recognizes that science is not a value-neutral activity that is magically immune to power relations and ideology. Genealogy understands that scientific knowledge and scientific practice involve human actors, with all their human limitations, contradictions, and flaws; it understands that "the precision of scientific methods arose from the passion of scholars, their reciprocal hatred, their fanatical and unending discussions, and their spirit of competition – the personal conflicts that slowly forged the weapons of reason" (Foucault 1984, 78). Foucault learned this lesson from his teacher, Canguilhem.

That science can be justified with the aid of genealogy does not mean science is not drenched in the social, cultural, and political strife that envelops it. Under a genealogical model, if anything, this strife is essential to the process of justification since it is in the whirlwind of social, cultural, and political life that concepts vie for epistemic legitimacy and

recognition, since it is in the midst of strife that concepts rise or fall, succeed or fail. This is why Canguilhem suggested as early as the 1940s that the philosophy of science needs to engage not only the history of science, but also the history of ideas more generally – because the history of ideas gives the epistemologist access into that broader milieu in which scientific knowledge is constructed and operationalized. As he states in a key passage from *The Normal and the Pathological*: “The history of ideas cannot be superimposed perforce on the history of science. But as scientists lead their lives as men in an environment and social setting that is not exclusively scientific, the history of science cannot neglect the history of ideas” (Canguilhem 1991, 46).

Unfortunately, we are yet to grasp the full significance of Canguilhem's genealogical approach for contemporary debates in the philosophy of science, including debates about realism and anti-realism,<sup>31</sup> about the relationship between science and ideology, and about the link between the descriptive and normative dimensions of genealogical thought.<sup>32</sup> This, as I argued above, is because the philosophy of science continues to be trapped in an echo chamber of its own creation. To break out of this chamber, we (philosophers of science) need to work on enriching “our panoply of methods” (Brenner 2015) and on curbing philosophical desire. By this, I mean that we can no longer lose ourselves in the dream of crafting a universal theory of science that conclusively captures the essence of all science, the soul of scientificity itself. This desire for totality, which can easily be subjected to psychoanalytic investigation, motivated early defenders of logical positivism and, not without a sense of irony, brought them into the company of the German idealists they so deeply and intensely abhorred. Like idealists, positivists fetishized totality, even if the totality they yearned for took the form of a comprehensive theory of science rather than a complete philosophical system.

Canguilhem rejected this fetishistic approach to science because he rejected the desire that nourishes it. On his view, science does not yield to totalities because science is not One. It is not one domain, one practice, one language, or one method. Science is ever plural and ever diverse. Science is Many. It is many practices across many domains, each of which champions a shifting assortment of norms, vernaculars, and methods without a common blueprint or archetype. In affirming this plurality and diversity, Canguilhem invited us to recognize that only a regional approach that encourages investigations of individual concepts and their local developments can live up to the name “epistemology.”

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<sup>31</sup> A broad question that continues to intrigue philosophers of science is whether the objects that our scientific theories are about — atoms, genes, species, etc. — are metaphysically real or only heuristics that help us formulate research questions and intelligently navigate the world. As noted above, Canguilhem argues that concepts are “constructed”. Yet, he believes concepts are constructed non-arbitrarily by scientists operating under settings of feedback-controlled action where the freedom to construct is limited by the theoretical and technical armature of the field.

<sup>32</sup> Interestingly, most post-positivist philosophers of science of the twentieth century never read works by French epistemologists. “A survey of the works of Kuhn, Lakatos, Feyerabend, and Hilary Putnam reveals a paucity of references to historical epistemology, and these are often superficial or derogatory. Post-positivists never engaged in serious debate with French philosophers of science” (Brenner 2015, 210).

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