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# "Being in the True?" Science and Truth in the Philosophy of Georges Canguilhem

## **Cover Page Footnote**

Presentation given at the colloquium *Georges Canguilhem, Philosophe, historien des sciences*, Collège International de philosophie, Paris, from October 6-8 1990, *Actes du Colloque*, Albin Michel, Paris 1993. Reprinted in E.B., *Lieux et noms de la vérité*, Editions de l'Aube, 1994.

In 1964-1965, an educational program devoted to the theme "Philosophy and Science" appeared on television.<sup>1</sup> In one broadcast, Georges Canguilhem was interviewed by Alain Badiou, leading to the following exchange:

Question: Must we continue radically to oppose scientific knowledge to common knowledge?

Answer: Yes, and increasingly so. There is no scientific knowledge without, on one hand, highly-developed mathematical theories, and without, on the other, the use of increasingly complex instruments. I would even venture to say that there is no such thing as common knowledge.

Question: Do you mean by that that the expression "scientific knowledge" is a pleonasm?

Answer: You have understood me perfectly. That is what I mean. Knowledge that is not scientific is not knowledge. I maintain that "true knowledge" is a pleonasm, as are "scientific knowledge" and "science and truth," and that they all express the same thing. This does not mean that for the human mind there is no goal or value outside of truth, but it does mean that you cannot call knowledge what is not knowledge, and cannot apply this term to a way of living that has nothing to do with truth, that is, with rigor.

These incisive formulations— let us not forget, originally spoken rather than written —have always made me uncomfortable. We can attribute two meanings or uses to them, just as we do every time we confront a tautological equation (a "pleonasm" as Canguilhem says) in philosophy whose terms connote, whether we like it or not, the transcendental or the absolute: *Deus sive Natura, Scientia sive Veritas*. Must we understand Canguilhem's formulations as proposing a critical, even positivist, restriction of the empire of truth to the domains delimited by scientific activity and objectivity? Or instead as a hyperbolic extension of science, or the sciences, to the totality of the field of truth, even if this field is in motion —not bordered once and for all by some constitutive limit, but open, according to its own ongoing history? It goes without saying that, depending on the orientation adopted, the meaning of this specification or precaution ("this does not mean that for the human mind, there is no goal or value outside of truth") would be very different. In the one case, it might refer to the place occupied by philosophy alongside science (if not above it), whereas, in the other, it would instead signal what, in any event, escapes it and prevents it from establishing itself as the Court of Final Appeal of our existence.

Thus, our perplexity increases through the two elements that figure in the same context. Canguilhem has vigorously denied to philosophy—or should it be renamed epistemology?—the capacity to "determine the extent of the concept of science" and, in consequence, to "define the comprehensiveness" of philosophy, except through a

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<sup>1</sup> Text of the program published in the *Revue de l'Enseignement philosophique*, 15th year, n° 2, December 1964—January 1965, pp. 10-17.

simple reference to the field of human culture, in which science is distinguished from other activities (in particular from industrial activities) on the basis of its theoretical finality. But this does not prevent Canguilhem from proposing an epistemological, and thus philosophical, thesis on truth: "There is truth either in the formal sense or in the sense of the coherence of the interpretation of phenomena. There is no other." To which he added, that "the difficulty here is that the formal serves the experimental at a given moment in order itself to advance, and that the experimental more often advances through the formal than by the experimental itself." In other words, Canguilhem has outlined a general epistemology, a rather rare thing for him (I will return to this), but has done so practically, in the form of a denial.

Might it be said that I have subjected formulations that the author would probably not have included in his oeuvre, however he defined it, to abusive treatment? Perhaps. In this case, we may consider the discussion a pretext for the elaboration of a question whose real answers we will seek in the texts themselves. The fact remains that, several years after the televised interview, Canguilhem laid claim to these same words, even as he further specified them. This time it was in the course of a discussion that took place on February 27th, 1968 at the Sorbonne as part of the conference series, "Les Structures et les Hommes," organized by the review *Raison présente* and the *Union rationaliste*.<sup>2</sup> There, Canguilhem remarked that

One day, it seems that I scandalized all the philosophy students who watched a particular television program. The students, as well as many of their professors, because I said this: there is only scientific truth, there is no such thing as philosophical truth. I am ready to take responsibility here for what I said elsewhere. But to say that there is only scientific truth, or that there is only objectivity in scientific knowledge does not mean that philosophy is without object (...). There is no philosophical object in the sense that there is a scientific object that science constitutes theoretically and experimentally ... but finally I do not mean that there is no object of philosophy.

No philosophical object, but one or more objects of philosophy? Let us hazard the following paraphrase: no *constituted* philosophical object, in the sense that there are constituted scientific objects, but one or more objects, that is, *questions*, for philosophy: and Canguilhem cited as an example the question of the political uses of neurophysiology.

The context of this new intervention brings with it an interesting specification: for any science, unlike the non-sciences or the pseudo-sciences that are immediately distinguishable by their historicity or their repetitiveness, its specific *history* is constitutive of true scientificity. More precisely, what makes it constitutive are the successive historical forms within which conditions of objectivity, inseparably

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<sup>2</sup> Text published in *Structuralisme et marxisme*, U.G.E. 10/18, Paris 1970, pp. 205-265.

theoretical and experimental, conceptual and instrumental, are organized within progressive systems, indefinitely substitutable in an order of increasing objectivity.

From this point on, the preceding reference to two types, or modes of scientific truth—the only truth there is—may be reinterpreted. It is less a matter of outlining a classification of the sciences according to knowledge [connaissance] than of designating *this identity of objectivity and of historicity* as the same field of truth, the content of which, in every region of knowledge [savoir], at every moment, is a defined and coherent combination of formalism and instrumentation. This thesis is clearly Bachelardian in inspiration, although it does not appear explicitly anywhere in Bachelard's work; based on it, we may now turn to Canguilhem's oeuvre properly speaking. Can we say that, for Canguilhem, at least at some point in his reflection, science and truth became identified, insofar as they are both concerned with a more essential identity, that of objectivity and historicity?

It is here, however, that a complication awaits us. Earlier, I hazarded the phrase "general epistemology"—and we might just as easily say philosophy, or philosophy of knowledge. But, we know full well that it is neither by chance nor from a lack of time or opportunity that such an "epistemology" is precisely what Canguilhem always refused to develop as a *separate* discourse. There is clearly, for him, an intrinsic connection between the fact of postulating, or, indeed of simply suggesting, this essential equation, and the fact of going to the things themselves, while setting aside any general or generic discourse that would have had "science" as its object, beyond the theoretical minimum required to take up the problems of history and of philosophy *through* a critique of their traditional presentation. It is easy to see how, for Canguilhem, such a meta-scientific discourse would have exactly the same characteristics as the discourse of "scientific method" or "experimental method," whose essential connection to positivist philosophy produced a normative interpretation of the *fait accompli* and a denial of the historicity of knowledge [savoir].<sup>3</sup> At the same time, it was a matter of proving that the real alternative for Canguilhem is *not* the choice between the renunciation of philosophy and the construction of a methodology, of a meta-language—contrary to what positivism suggests. Unfortunately for us, this also means that Canguilhem's *philosophical* utterances (about knowledge, life, history, or technique), and they are not rare, are always *interwoven* in a highly specified critical and historical context, and consequently lose their significance as soon as we try to separate them from this context.

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<sup>3</sup> Notably the texts on Claude Bernard assembled in the *Etudes d'histoire et de philosophie des sciences*, 1st ed., Vrin, Paris 1968, pp. 127—71, as well as the educational television program on "La recherche expérimentale" (avec C. Mazières), transcribed in the *Revue de l'Enseignement philosophique*, 18th year, n° 2, December 1967—January 1968, p. 58ff.

There are, however, some exceptions to this situation, the conditions of possibility of which arise from polemical conjunctures or by commemorations. I am thinking primarily of the texts written for the purpose of presenting and analyzing the work and thought of Gaston Bachelard.<sup>4</sup> But here we encounter yet another difficulty. Canguilhem persistently adhered to the thesis, which he attributes to Bachelard, according to which a *critical* history of the sciences, a non-naturalist history of the sciences, does not endow itself with the ability to construct a fictional record of the *facts* of knowledge, but situates itself either in the perspective of an evaluation, or analysis of the *problems* that the scientist seeks to resolve, or the perspective of the search for truth that is by definition an axiological procedure—and a history of the sciences of this type must necessarily be *founded on an epistemology*. An epistemology, like Bachelard's, which would be a non-positivist philosophy of theoretical discontinuities, and intellectual innovations. Taking these texts literally, at the limit we would have done nothing more than substitute a reflection on Bachelard for a reflection on Canguilhem. But this is not at all what we want to do because we are convinced, in re-reading Canguilhem's historical and epistemological work, that, while clearly not anti-Bachelardian, it is profoundly original even in its use of concepts borrowed from Bachelard. There remains, however, another category of texts: those in which Canguilhem was led for his own purposes to think the category of the "true" *by means* of a reflection on and a discussion of *the history of the sciences*. I will thus focus on the three of these texts that I regard as crucial.

The first is exactly contemporary with the remarks to which I referred a moment ago: the lecture, "Galilée, la signification de l'oeuvre et la leçon de l'homme," delivered in 1964 and re-published in the *Etudes d'Histoire et de Philosophie des Sciences*. In this simple, but extraordinarily concentrated, text whose assumptions are borrowed from Koyré, Santillana and Clavelin, Canguilhem reconstitutes the epistemological dilemma that underlies the ethical problem posed by Galileo's refusal to accept the theoretical and as well as political compromise proposed by the Church (the doctrine of the astronomical "Equivalence of Hypotheses"). Galileo's work developed simultaneously in two principal directions: first, laying down the foundations of a revolutionary dynamics on the basis of the articulation of the first invariant in physics to be expressed mathematically (which renders this thesis incompatible with the ancient perception of nature: movement is a *state* of things that conserves itself indefinitely), and second, providing a body of evidence to support the Copernican thesis, some observational (with the scientific use of lenses transformed into the telescope), others physical and thereby demonstrative. I quote Canguilhem:

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<sup>4</sup> The principal texts are collected in a section of the *Etudes d'Histoire et de Philosophie des Sciences*, 1st edition, Vrin, Paris 1968, pp. 173—207.

Galileo rejected Osiander's interpretation of Copernicus, which accommodated the Aristotelian philosophers and the Catholic theologians. Faithful to Copernicus, he set himself the goal of establishing that Heliocentrism is true in the sense of a physical truth. But his peculiar genius lies in having perceived that the new theory of movement, Galilean dynamics, furnished a model of physical truths still to be advanced, truths that would define Copernican astronomy as a radical and complete refutation of Aristotelian physics and philosophy. It was in his pursuit of this mission that Galileo compelled the Church to condemn Copernicus by condemning him.

But Canguilhem continues:

We concede to those who have pointed out that the physical arguments made by Galileo ... did not have the evidentiary value that he attributed to them, and in particular that Galileo did not manage to produce the evidence demanded by Tycho Brahé in support of terrestrial movement (...). None of Galileo's experiments ... succeeded in confirming the predictions of calculus, none succeeded in convincing even scientists as non-Aristotelian as he was (...). On the other hand, the physical evidence that should have established calculus, the measure of the parallaxes of the fixed stars ... was only partially furnished by Bradley in 1728 and was not complete until the 19th century.

And yet, we will say, along with Alexandre Koyré, that it is Galileo who is in the true.

Being in the true does not mean always saying the true.<sup>5</sup>

*Being in the true*: a remarkable and remarked upon formula. Does it imply, to return to our initial question, that being "in the true" is to be "in science?" And, once again, according to what orientation is this formula to be understood? But first, how do we interpret this "in" that suggests, at least metaphorically, a space, a domain, or perhaps borders? In "The Order of Discourse," Michel Foucault in 1970 proposed an interpretation, citing and taking inspiration from Canguilhem:

Within its own limits, each discipline recognizes true and false propositions; but it pushes back a whole teratology of knowledge beyond its margins. The exterior of a science is both more and less populated than is often believed: there is of course immediate experience, the imaginary themes which endlessly carry and renew immemorial beliefs; but perhaps there are no errors in the strict sense, for error can only arise and be decided inside a definite practice; on the other hand, there are monsters on the prowl whose form changes with the history of knowledge. In short, a proposition must fulfill complex and heavy requirements to be able to belong to the grouping of a discipline; before it can be called true or false, it must be 'in the true', As Canguilhem would say.

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<sup>5</sup> G. Canguilhem, *Etudes d'Histoire et de Philosophie des Sciences*, pp. 44-46. The passage from Koyré that inspired Canguilhem is found in the *Etudes galiléennes*, vol. 2, Hermann 1939 (reprinted 1966), p. 155.

In order to maintain the sense of the discussion, it is necessary to cite yet the following page:

People have often wondered how the botanists or biologists of the nineteenth century managed not to see that what Mendel was saying was true. But it was because Mendel was speaking of objects, applying methods, and placing himself on a theoretical horizon which were alien to the biology of his time. Naudin, before him, had of course proposed the thesis that hereditary traits are discrete; yet, no matter how new or strange this principle was, it was able to fit into the discourse of biology, at least as an enigma. What Mendel did was to constitute the hereditary trait as an absolutely new biological object, thanks to a kind of filtering which had never been used before; he detached the trait from the species, and from the sex which transmits it; the field in which he observed it being the infinitely open series of the generations, where it appears and disappears according to statistical regularities. This was a new object which called for new conceptual instruments and new theoretical foundations...

And Foucault's conclusion:

Mendel spoke the truth, but he was not 'within the true' of the biological discourse of his time: it was not according to such rules that biological objects and concepts were formed. It needed a complete change of scale, the deployment of a whole new range of objects in biology for Mendel to enter into the true and for his propositions to appear (in large measure) correct. Mendel was a true monster, which meant that science could not speak of him; whereas about thirty years earlier, at the height of the nineteenth century, Scheiden, for example, who denied plant sexuality, but in accordance with the rules of biological discourse, was merely formulating a disciplined error.

It is always possible that one might speak the truth in the space of a wild exteriority, but one is 'in the true' only by obeying the rules of a discursive 'policing' which one has to reactivate in each of one's discourses.

The discipline is a principle of control over the production of discourse. The discipline fixes limits for discourse by the action of an identity which takes the form of a permanent re-actuation of the rules.<sup>6</sup> [cited from "The Order of Discourse" in *Untying the Text: A Post-Structuralist Reader*, translated by Ian McLeod]

Although this analysis without a doubt merits its own discussion, this is not my object. But it seems clear to me that it attempts precisely to reverse the sense of Canguilhem's phrasing. In effect, what Canguilhem said is not that Galileo already found himself—unlike his adversaries—within the limits of a constituted discipline,

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<sup>6</sup> Michel Foucault, *L'ordre du discours, Leçon inaugurale au Collège de France prononcée le 2 décembre 1970*, Gallimard, Paris, pp. 35-38. This analysis of the "Mendel case" may be compared to those that Canguilhem himself proposes in "Sur l'histoire des sciences de la vie depuis Darwin" (1971), collected in the anthology *Ideologie et rationalité dans l'histoire des sciences de la vie*, Vrin, Paris 1977, as well as in J. Piquemal, "Aspects de la pensée de Mendel" (1965), reprinted in J. Piquemal, *Essais et leçons d'histoire de la médecine et de la biologie*, P.U.F. 1993.



that he submitted himself to the norms and to the “discursive policing” of certain rules authorizing the validation of statements, and therefore a certain mode of division between the true and the false—in short, what he said and meant is not that the truth of Galileo is *relative* to certain theoretical and institutional conditions, retrospectively discovered as necessary, but on contrary that Galileo *anticipated* ahead of time and in the absence of rules, a regime of the universality of a truth that will be sanctioned after the fact. A regime absolutely incompatible with the systematic error of Ptolemaïcism, of Aristotelianism, and their union, under the seal of Catholic theology. It is in this real anticipation—totally distinct, however, from the fiction of the “precursor”—wherein, for Galileo, the fact of being “in the true” resides.

And if we reflect on this difficulty, beginning with Canguilhem’s previous work, from *Le normal et le pathologique* to *La formation du concept de réflexe aux XVIIe et XVIIIe siècles* and the essay on the history of cellular theory in *La connaissance de la vie*, we clearly see that any other interpretation would have led to one or another variant of the concept of “normal science,” against which Canguilhem constantly argued (even before it was identified as such) by portraying science as an adventure of intelligence at the heart of life itself which, borne by that part of life that is human,, must distinguish itself from life in order to allow the resolution of problems that life poses to the living: what we might call theory, not the equivalent of a *normality*, but of a *normativity*. But we see that above all the epistemological “frame” produced by Canguilhem’s description is not synchronic nor is it spatially metaphorisable: it is only conceivable as a temporal modality, and the problem it represents is entirely contained in the question of knowing what content must be assigned to this “gap” between what Galileo is certain of—the objective or real truth of Copernicanism—and what he is capable of demonstrating.

What does Canguilhem tell us in this regard? Two things that are distinct, even if they are connected. First, that Galileo was aware of being able to bring *to a conclusion* a proof founded on “the power of calculus that permitted the formulation of the first law of mathematical physics,” that is, the constitution of a complete mathematical physics “of universal dimensions.” And this is what *we* know will effectively be produced, but, and I am still citing Canguilhem, in his activity, Galileo “assumed for himself, in his human existence, the infinite task of measuring and coordinating experiments that require the time of humanity as the infinite subject of knowledge.” In other words, he *imagined it*, and imagined himself as the subject of science. In other words, he *imagined it*, and imagined himself as the subject of science. But herein lies the second aspect: Galileo imagined this infinite task as *finite*: in other words, at the same time that he is “in the true” he is also *in error*, notably because he maintained a “circularist” representation of the cosmos (which is one of the reasons why he did not pay attention to what Kepler proposed to him, without which he could not know the concepts that completed his own and furnished an essential part of the required “proof”).

In sum, and this is without a doubt Canguilhem's most profound thesis, "being in the true" is being *in disequilibrium* in relation to the time of the true: it is not being the contemporary of the true or being present to the true (to the "presence" of the true), but being ahead of and, simultaneously, behind it. And in consequence, being in the true is also to be in the *non-true*: between the two formulations with which Canguilhem describes Galileo's situation ("not always saying the true," "to be in the true"), we discover not a restrictive relation or a contingent juxtaposition, but a strict implication. In order to "be in the true," far from remaining within the limits of a domain that would be, even virtually, the empire of the true (with its "police"), or one of the regions of the empire of the true (one of the established scientific disciplines), one must *also*, in an unstable and polemical —presumptuous, as Canguilhem will say elsewhere—way, be able to remain in the non-true or in error. A certain type of error. Passing over to the other side of our equation (science = truth), would we then say that being "in science" is also being in non-science, in a determinate *ideology*?

Why use this terminology, which until now has played no role in our discussion and seems to have been imported from a foreign philosophy? It is suggested by a reading of a second text, by means of which I would like to continue the discussion. The text in question is the 1969 article, "*Qu'est-ce qu'une idéologie scientifique?*," collected in the volume *Idéologie et rationalité dans l'histoire des sciences de la vie*, whose title refers to the article.<sup>7</sup> It is theoretically the center of the volume, around which its diverse illustrations and elaborations are arranged. And it represents in particular the end point of a long series of indications dispersed throughout the work, all of which converge around the idea that there cannot be a history of truth that is *only* the history of truth, nor a history of science that is *only* the history of science.<sup>8</sup>

These two formulations are not completely equivalent. The first designates an internal contradiction: "Those who seek to write the history of truth alone, will produce an illusory history. M. Suchodolski is correct on this point, the history of the truth alone is a contradictory notion." The only means by which history can be made a non-contradictory project is thus to *bring contradiction itself* into history ["...*faire entrer la contradiction dans l'histoire...*"], and even into truth; "error" and "truth" in this sense are not juxtaposed, but, as Canguilhem says a little earlier, using Bachelardian terms of "lapsed history" and "sanctioned history," are "at once separated and interlaced." Our second formulation—the history of science cannot *only* be the history of science—designates an exterior condition, and even a double exterior condition:" on the one

<sup>7</sup> Op. cit., pp. 33-45.

<sup>8</sup> In Foucault's *Histoire de la folie à l'âge classique* (Paris, Plon, 1961, p. 456). Canguilhem was able to find this astonishing formula taken from "a contemporary of Claude Bernard:" "The history of madness is the response of the history of reason" (Michea, *Démonomanie* article from the *Dictionnaire* de Jaccoud). The Pascalian and Nietzschean project of a "history of truth" was only appropriated by Foucault, retrospectively and prospectively, much later (cf. *La volonté de savoir*, 1976; *L'usage des plaisirs*, 1984).

hand, practices, experiments and institutions, among which science itself figures as a practice, an experiment and an institution; and, on the other, an “unconscious need for direct access to the totality,” an extraordinary formula in which, if we allow this terminology, we will recognize the subject of the desire of knowledge—that *is not* the universal and impersonal subject of science, but is, however, never *separable* from it.

At the point that these diverse determinations, internal and external, meet, what Canguilhem calls “scientific ideologies,” emerge, marking the conclusion of a meticulous differentiation of Marx, Althusser, Foucault. Of these scientific ideologies, irreducible to political ideologies of class, distinct from the false science of anti-science (religion), and equally distinct from the ideologies of scientists (or scholars), he gives several examples: Atomism, Heredity, and Evolutionism.<sup>9</sup> [Is not vitalism, or at least the aspect of vitalism (Organicism) with which Canguilhem was occupied in the early part of his oeuvre, in this sense, equally a “scientific ideology?” And how is it possible not to try to think *pre-Copernican* astronomical and cosmological *geocentrism* in these terms?] He shows that they occupy a necessary, although paradoxical, place not outside but *in* “the space of knowledge.” Further, in the guise of a conclusion, he states three *theses* on scientific ideologies, destined simultaneously to illuminate their constitution and their function:

- a) The scientific ideologies are explanatory systems whose object is hyperbolic relative to the borrowed norm of scientificity that is applied to it.
- b) There always is a scientific ideology before the emergence of a science in the field where the science will be established; there is always a science before an ideology in the lateral field at which this ideology obliquely aims.
- c) Scientific ideology should not be confused with false sciences, magic or religion. It is, like them, moved by an unconscious need for direct access to the totality, but it is a belief that watches from the sidelines of an already established science whose prestige it recognizes and whose style it imitates.<sup>10</sup>

Thus, scientific ideologies are “presumptuous” (hyperbolic) extensions of a model of scientificity: they transpose a norm of truth beyond conditions of the application of the concepts that support this model, and that allow this norm to exist (thus, in the examples analyzed here, the concepts of “natural selection,” of “correspondence between ontogenesis and phylogenesis,” etc.). Through this extension, objectivity is lost, and one moves in a certain sense from the virtuality of truth to the virtuality of error.

It is nonetheless presented as the decisive moment in the *history* of truth, and thereby in the history of scientific knowledge [connaissance]. In effect, without this

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<sup>9</sup> This last example is fully developed in the collective work of G. Canguilhem, G. Lapassade, J. Piquemal, J. Ullmann, *Du développement à l'évolution au XIXe siècle*, Thalès 1960, reprinted Paris, P.U.F., 1985.

<sup>10</sup> *Ibid.* p. 44

extension there would never be migrations or exportations of concepts from one domain, or even one discipline, to another, for Canguilhem, the general form, or at least the presupposition, of any progress in explanation. This coincides with the fundamental idea according to which the typical unities of knowledge [savoir] are not “theories” but “concepts.” Or, to put it another way, the idea according to which the strategic element in theories around which the “possibilities of truth” are in play, the element that also enters into a process, in practice infinite, of circulation, “naturalization” and “transformation, is the *concept*. Not only is the epistemology of Canguilhem, after those of Bachelard and Cavallès, an epistemology of the concept *par excellence* (and not an epistemology of “theories”), but Canguilhem is one of the very rare contemporary philosophers who works on the question: “What is a concept?” or who seeks to construct a concept of the “concept.”<sup>11</sup>

It is in their circulation (that is, translation, transposition, generalization) that the application or “labor” of concepts takes place, which in turn makes it possible to test them and to establish their truth. However, we can equally assume—taking up some of Canguilhem’s earlier suggestions<sup>12</sup>—that “presumptuous” extension is also a correlative of the *dogmatization* of concepts in their original domain. That is, of the—provisional—erasure of the equivocations, the possibilities of divergent interpretation that they entail: on this point, we might think of the striking example of post-Newtonian mechanical philosophy in which the causal thought of the *Principia* and of the *Opticks* retreats into the univocity of a “determinist” doctrine of “central forces.” In order hyperbolically to “extend” the use or application of a concept beyond an established epistemological border (and any extension of this kind is first supported by an analogy, whether formal, imaginary or pragmatic), it is in effect necessary *to choose* among its theoretical virtualities. It is thus necessary to re-transform the “concept-problem” into a “concept-solution.”<sup>13</sup> The contradiction is immediate.

But Canguilhem goes even further: he suggests that scientific ideologies not only *follow* conceptual creation, or the “fact of truth,” but that they always *precede* scientific creation, that is, epistemological ruptures or breaks. It is not with just any

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<sup>11</sup> See, among others, “Le vivant et son milieu,” in *La Connaissance de la Vie*, 2<sup>nd</sup> editions, Paris, Vrin 1965, pp. 129 and on, as well as the conference “Du concept scientifique à la réflexion philosophique,” *Cahiers de philosophie*, published by the Group d’étude de philosophie de l’Université de Paris, UNEF-FGEL, n° 1, January 1967. This question was the subject, in its time, of commentaries by P. Macherey commentaries (“La philosophie de la science de G. Canguilhem,” *La Pensée*, n° 113, January—February 1964) and D. Lecourt (“L’histoire épistémologique de Georges Canguilhem,” in *Pour une critique de l’épistémologie*, F. Maspero, Paris 1972).

<sup>12</sup> Cf. “La théorie cellulaire,” in *La Connaissance de la Vie*, op. cit., pp. 43 and on; “Le vivant et son milieu,” *ibid.*, pp. 129 and on. And naturally *Du développement à l’évolution...*, op. cit.

<sup>13</sup> Thus Canguilhem seeks to find this “philosophical pluralism” in the *past* itself, by virtue of the virtual polyvalence of concepts that Bachelard considered necessary to an analysis of the present, that is, the activity of *modern* science: the reason perhaps being that, for him, any reason which explores and works is always already “dialectical.”

error, web of errors, or even theory with which a science must break in order to establish itself: it must break with an ideology that is already itself the result of the ideologization of a science. I would add: it breaks with an ideology that is already the ideologization of *a* scientific concept, or, as Spinoza said, of "a true idea." Thus Darwin and Mendel broke with concepts of milieu or heredity that rested on, at least in part, an ideological extension of mechanistic science. Galileo broke with a dynamics whose concepts (above all that of "natural place") accompanied the ideologization of the first geometrization of the universe.

This is an apparently strange, perhaps even contradictory, idea, in that it suggests that scientificity as such *has no beginning*, but that there is always already a dialectic of scientificity and ideologization, or better yet, *of the ideologization and de-ideologization* of the concept, that is constitutive of knowledge [connaissance]. But we can also interpret it by saying that Canguilhem's propositions (profoundly Spinozist in this regard) do not permit a thinking of the *absolute* beginning of scientificity, but only its infinite process, its re-commencement or its development. Is this the weak point of his propositions or precisely their strength? With Canguilhem epistemology truly takes leave of the problem of "origins," whether the origins of science or the origins of positivity, that continues to haunt the problematics of "demarcation" and the "break." Epistemology is coextensive with the recognition of the historicity of knowledge [savoir], although the recognition is not a "historicism," since such a historicity absolutely excludes any relativization of knowledge [savoir]. I do not believe that I am wrong to see in this the importance of certain of Auguste Comte's theses for Canguilhem, especially the idea that the hold of the "theological" had never been total.

To clarify this point, let us try to better comprehend what is at play in the relationship between knowledge [connaissance] and ideology by invoking a third text: the article "Life," published in 1974 in the *Encyclopaedia Universalis*.<sup>14</sup> This synthetic text (where Canguilhem assembles the results of a great number of inquiries and readings) allows us to comprehend how the Bachelardian notion of the "epistemological obstacle" was finally reconceptualized as a necessarily related to the question of scientific ideologies. Posing the question of "the obstacles to a scientific knowledge [connaissance] of life," he recalls that "it is to the work of Gaston Bachelard that contemporary French epistemology owes its interest in the origin and function of the obstacles to knowledge [connaissance]." By working on the Bachelardian idea of a "psychoanalysis of objective knowledge [connaissance]," according to a perspective both closer to Freud and directly appropriate to the problems of biological knowledge [connaissance], he organized his reflections on the recurrent conflict between the objectivity of knowledge [savoir] and the values of human life around the description

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<sup>14</sup> Encyclopaedia Universalis, Vol. 16, Paris 1974, pp. 764-769.

of three major “objects of complexes (*objets de complexes*):” the desire for metamorphosis, the myth of spontaneous generation, and the technical interest in the use of the living animal by the living human. Each of these complexes explains in its way that “the extension of the methods of the knowledge [connaissance] of matter to life had, until our time, encountered persistent resistance, which was not only an expression of an affective repugnance, but sometimes of a deliberate refusal a paradoxical hope of explaining a power by means of concepts and of laws initially formed from hypotheses that deny it.”

In other words, theory in biology never escapes the conflict between an analytical explanation that brings the living thing into the universality of natural phenomena, and a singular experiment that perceives it as an exception to nature (and that, when all is said and done, will present itself as the “privilege of death”).

The study that is then undertaken of the great theoretical conceptions of life—at once enacted in time and recurrent in the history of ideas, life as animation, life as mechanism, life as organization, life as information or communication<sup>15</sup>—will show that such “complexes” are, each time, present as a pre-existing foundation in the construction of a definition of life. This follows from the fact that there is no conception or conceptualization of life as such, distinct from a simple description or classification of living things, which is not also a worldview. And reciprocally, any conception of the world, any “extension to the totality of the human experience,”<sup>16</sup> will probably only find what allows it to ground the illusion of its simplicity and its absolute necessity only in the unconscious force that communicates to it certain complexes of birth, life and death, as well as the transgression of the limits of the individual or the species. Yet the “definitions of life” (that are precisely Ideas: the Idea of the soul, the Idea of the machine, the Idea of the organized body, etc.) *are not fundamentally different from what Canguilhem previously called “scientific ideologies.”*<sup>17</sup> They are at least historically inseparable: because in any “scientific ideology,” and especially in those that truly mark the epoch, a “definition of life”—for example, individuality in itself, organization endowed with auto-plasticity—is present, whether as its condition, as a source of conceptual generalization, or as its aim, its by-product (in this regard the case of Evolutionism, studied in all of its details by Canguilhem and his collaborators, is absolutely convincing).<sup>18</sup> And this is hardly surprising given that the “unconscious need for direct access to the totality” does not express itself in the *theoretical* element, without a schema of life or of the living thing intervening to homogenize, analogically at least, the representation of the individual, subject of

<sup>15</sup> It is astonishing that, in this series, Canguilhem did not include a specific place for the “definition” of life as evolution or transformation.

<sup>16</sup> *Idéologie et rationalité...*, pp. 43.

<sup>17</sup> Canguilhem is here speaking of “medico-philosophical ideologies.”

<sup>18</sup> *Du développement à l'évolution au XIXe siècle*, *ibid.*

knowledge [connaissance], and of the universe. Any definition of "life," as Canguilhem often showed,<sup>19</sup> however positive and positivist, is "ideological" at least in the sense that, in order to enunciate its specificity in a given state of knowledges [connaissances] and the corresponding means of language [langage], it must necessarily aim for *more* than life, in any case *beyond* the universality of living things. And in consequence *alongside* life, insofar as it is a "property" common to living things.

But the same analyses also illuminate the "scientific ideologies," whose place the 1969 article had already marked by indicating the link between the discourses of heredity in the 18th century and "the juridical problems of the subordination of the sexes, of paternity, of the purity of lineages, of legitimacy of the aristocracy," or between Spencerian Evolutionism and "an engineer's project in the English industrial society of the 19th century: the legitimation of free enterprise, the corresponding political individualism and competition." Without doubt scientific ideologies *are not* openly "class ideologies" or more generally socio-political ideologies, whether in the mode of "false consciousness," or the discourse of legitimation. There is, however, a question of whether they are not in *all* the cases, overdetermined by a representation of society, of its conflicts of power and of its history, the best example of which is the interpretation of the organism in terms of a division of labor between the organs or in terms of a society of cells, which in turn permits the thinking of society as an organism.

From the moment consensus is identified with solidarity, we no longer know which, organism or society, is the model, or at least the metaphor, of the other.<sup>20</sup>

The necessary link between scientific ideologies and socio-political and theologico-political ideologies from which emerges another tendency to the totalization of experience, is not presented by Canguilhem as an unconscious link between desire and resistance, as had been the case in his discussion of complexes, the definitions of life and of worldviews, but instead as a link between implicit and teleological presupposition. "The law of differentiation ends with the support given to the individual against the State. But, if it ends that way explicitly, it is perhaps because it had begun that way implicitly." This is another way of "misrecognizing [*méconnaître*] its real relationship to the real," which can be known only by being detached from the real, given that "ideology is knowledge [*connaissance*] that is the more separated from its given object the more it believes itself to be connected to it."<sup>21</sup> Thus the epistemological obstacle's multi-dimensional structure, such as it is re-thought by

<sup>19</sup> See the discussion with F. Dagognet, "Le vivant," *Revue de l'Enseignement philosophique*, 18th year, n 2, December 1967-January 1968, pp. 55 and on).

<sup>20</sup> See "Vie," p. 768. See also "Le tout et la partie dans la pensée biologique," in *Etudes d'histoire et de philosophie des sciences*, pp. 319ff.

<sup>21</sup> *Idéologie et rationality*, pp. 36, 42, 45.

Canguilhem, is completed. It is an intellectual and historical formation whose labor of knowledge permits us, in a recurrent way, to identify a triple relationship: to the extension of concepts (and therefore to explication, and analytical discursivity), to the imaginary and the practical aims of Man in society, to the desire for knowledge [savoir] (or of non-knowledge [non savoir]) proper to the living human.

In recalling these propositions, and by attempting, for better or for worse, to inscribe them in the same progression, have I not lost view of the problem I initially posed? I do not think so and we may now have the means of providing some of the elements of a response. For these propositions contain implicitly both a conceptualization of the history of knowledge [connaissance] and a thesis on truth (or on its production, from which it appears indissociable).

I earlier proposed the pair “ideologization/de-ideologization” to characterize the labor of the concept. We may understand by this the incessant movement of thought that, at the very moment it advances within the element of language [langage] towards the unknown, it exposes the unknown to the grasp of the imaginary (of the species, the individual, the institution), but precisely to offer up this imaginary to conceptual critique and elaboration. Science in its history is thus the infinite process that, escaping repetition, but without assignable end, projects the “internal” conditions of thought (whether unconscious or implicit) into exteriority and discursivity, in order to be able to free itself from them through objectivity.

Hence, the “developed” formula through which I will attempt to express the dialectic immanent in the equation *science = truth*, that Canguilhem, without necessarily stating it, put into practice:

Science = (historicity = (ideologization/de-ideologization) = objectivity) = truth

The unity and division of contraries (ideologization / de-ideologization) is at the very center of this equation: this is why I speak of “dialectic,” a word that Canguilhem (unlike Bachelard) uses rarely, but which he does not reject.<sup>22</sup> This is the *limit* of intellectual labor, which marks the impossibility of being “in the true” without be exposed to the risk of error and thereby to its own rectification. It is also, reciprocally, the mark of the fact that it is impossible for ideology to remain identical to itself, or for thought to remain at rest in ideology, that is, *not to know*. From this

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<sup>22</sup> Discussing the relationship between the conception of the implied dialectic here, and other conceptions proposed in the history of philosophy, would demand another work. Let us be content with evoking a text where, surprisingly, Canguilhem comes to speak the language of the “negation of the negation” starting from an analysis of Nietzsche: “De la science et de la contre-science,” in *Hommage à Jean Hyppolite*, collected works, P.U.F. 1971.



point on, is not the statement "science and truth are the same thing" the most adequate way of expressing the fact that neither of these two terms can ever conceal an immutable essence?

If, to be "in the true," is necessarily to be in science, its labor and its risks, would this mean that science is the only thought that thinks itself, the only "thought of thought"—even if there always remains something new for it to discover on its own basis by profiting from its errors? "Project, error, marks of thought," wrote Canguilhem.<sup>23</sup> Science is in any case the only thought whose internal obstacles may finally become its conditions of possibility. It is also the only thought that may hope to find its own external and contingent conditions of possibility *elsewhere*, having displaced them, as "objects" of necessary thought. This is why, *if science is not everything*, or is *not the whole* (of experience, of life, of thought), it may nevertheless be said that virtually *nothing is external to it*, insofar as it can *exteriorize everything*, including its own activity — not all at once, but in the "infinity of knowledge."

*Translated by Andrew Fan*

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<sup>23</sup> "Le cerveau et la pensée," conference at the Sorbonne on the 20th February 1980 (part of the M.U.R.S. conference series) re-edited in *Georges Canguilhem, Philosophe, historien des sciences*, Actes du Colloque du Collège International de philosophie, Albin Michel, Paris 1993.