Duhem, Quine, Wittgenstein and the Sociology of scientific knowledge: continuity of self-legitimation?

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Duhem, Quine, Wittgenstein and the sociology of scientific knowledge: continuity or self-legitimating?

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Summary: Contemporary sociology of scientific knowledge (SSK) is defined by its relativist trend. Its programme often calls for the support of philosophers, such as Duhem, Quine, and Wittgenstein. A critical rereading of key texts shows that the main principles of relativism are only derivable with difficulty. The thesis of the underdetermination of theory doesn't forbid that Duhem, in many places, validates a correspondence-consistency theory of truth. Thus he never says that social beliefs and interests fill the lack of underdetermination. Quine's view on the underdetermination of theory by data leads to Duhem's view. But, to take some examples, his idea of a selective revision of hypotheses, as well as the neat incompatibility between holism and conventionalism, openly challenges the principles of relativism. When reading Wittgenstein's work, which is not presented in book-form but rather as a tree, we have first to avoid aphoristic choices that credit any text-excising. This remark allows us to tackle the passages that sociological relativism is based on. According to Wittgenstein, mathematical conventions seem not to be anthropological objects. Moreover, when Wittgenstein examines the famous "language-games," he only speaks of the functioning of natural language, not to be confused with scientific formal languages. We then should render the formula "language-game" by "well-defined, explicit and compulsory rules of communication", this is a much less attractive formula for relativism. Consequently, in terms of contents, there doesn't exist a real continuity between the epistemologies of Duhem, Quine and Wittgenstein, and the recent works of the SSK. Thus we are entitled to wonder whether such references don't simply further the purpose of self-legitimising the programme.

Riassunto: Un orientamento relativista si esprime oggi nella sociologia della conoscenza scientifica. Il suo programma invoca spesso la cauzione di certi epistemologi come Duhem, Quine e Wittgenstein. Una rilettura critica dei testi chiave di questi filosofi prova però che i principi essenziali del relativismo si possono ricavare da essi solo con molta difficoltà. La tesi della sottodeterminazione delle teorie non toglie che Duhem, in molte occasioni, aderisca a una teoria della verità basata sulla corrispondenza e la coerenza logica: egli non ha mai affermato che gli interessi e le credenze sociali vengano a colmare le lacune della sottodeterminazione. La concezione di Quine circa la sottodeterminazione della teoria rispetto ai dati confluisce verso la concezione di Duhem. Ma, ad esempio, le sue idee sulla revisione selettiva delle ipotesi, così come la incompatibilità da lui affermata fra convenzionalismo e olismo, ricusa apertamente i principi relativisti. Nel leggere le opere di Wittgenstein, che non sono libri, ma piuttosto alberi logici, dobbiamo evitare di scegliere aforismi che possono accreditare qualsiasi taglio del testo. Questa osservazione permete di concentrarsi sui passi in cui si fonda più spesso la sociologia relativista. Secondo Wittgenstein, le convenzioni matematiche non sembrano affatto ogetti antropologici. Quando Wittgenstein esamina i famosi "giochi linguistici", parla soltanto del funzionamento del linguaggio naturale, da non confondere con i linguaggi formalizzati della scienza. Dobbiamo poi tradurre la formula "gioco linguistico" con "regole di comunicazione definite, esplicite e cogenti", e ciò sembra una formulazione molto meno attraente per il relativismo. Consequentemente, non esiste, in termini di contenuti, una vera continuità fra le epistemologie di Duhem, Quine e Wittgenstein e i recenti lavori della sociologia della conoscenza scientifica. Siamo quindi autorizzati a chiederci se quei riferimenti non servono piuttosto il progetto di una autolegittimazione di questo programma.

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Introduction

The SSK's[2] constant difficulties[3] in establishing the dependence of scientific knowledge on the social context of production is often due to an insufficient clarification of concepts[4] that allows some observers to read systematic and necessary relations where others would only perceive personal and contingent connections.

This seems to be the case as regards the continuity between eugenics and statistics that Norton (1978) infers from a detailed study of Karl Pearson. Everyone will admit that Pearson's contribution to statistical methods is closely linked to his medical doctrine, because statistics can indeed further the aims of the study of genetic selection.

Nevertheless, the role of eugenics in the development of statistics is rather postulated than demonstrated. In order to make this hypothesis acceptable we need not look for *one* eugenistic statistician, but prove—at least over a well defined period—that a *significant portion* of the statisticians were in favour of eugenics (this of course is not true of Bernoulli, Laplace, Gauss or Kolmogorov). And vice versa, we need another demonstration that a *significant portion* of the eugenists actually contributed to the advancement of statistics (this is not true of Darwin, Galton, Vacher de Lapouge or Carrel). So, as we extend the investigation to the two communities of statisticians and eugenists, the personal correlation, vouched for in the case of Pearson and some others, disappears. Let's suppose now this correlation to be testified to for a whole generation of scholars: should we speak in terms of "eugenic statistics"? Nothing is less certain, because we have good reason to use the terms "zero" and "compass" instead of "In-

2. Sociology of Scientific Knowledge, hereafter SSK.

3. It is not the purpose of this article to remind the reader of these difficulties; see: Freudenthal (1984), Isambert (1985), Matalon (1986), Ben-David (1991), Bunge (1991-92), Boudon and Clavelin (1994), Cole (1996), Raynaud (1998ab, 1999) and many others.

4. In his time, Robert K. Merton made a similar criticism of functionalism: " *Too often we have used, either a single word to represent different concepts, or various words to translate the same concept.* Clarity of analysis and accuracy of expression have suffered from this ill-considered use of words" (1953: 68, my italics). Bunge expresses more radical doubts about the new sociology of science: "An ideological program is a confession of faith and a plan to reinforce and propagate the faith. A scientific program is a research project that starts with problems, not principles other than the general philosophical principles underlying all scientific research—for example, that the external world is real, lawful, and knowable" (1991: 537).

dian zero" and "Chinese compass": these instruments presuppose universalising knowledge that can easily become emancipated from the social context of discovery.

It is not uncommon that relativist sociology, caught in such an argument, ends up by justifying its theories by reference to views in philosophy and the philosophy of science. It is a way of extending the debate. The present article aims to propose a re-reading of this continual coming and going, and tries to understand what the SSK intends to take from the philosophical texts, and to what extent it is within its rights to do so. Let us begin with a definition of the relativist position:

R1 The objects of the natural world that scientific statements are related to are nothing other than "textual constructions" (Woolgar, Latour).

R2 The natural world plays a negligible role in the construction of scientific statements (Collins).

R3 The social context, local as well as global, plays a decisive role in the construction of scientific statements (Mulkay).

R4 Scientific knowledge is "conventional" (Bloor) and its reasoning is built on some "informal social negotiation" (Mulkay).

Duhem, Quine and Wittgenstein enjoy a very great reputation among the instigators of relativism. This mark of esteem is obvious in the works of Collins (1974), Bloor (1973, 1983a, 1983b), Barnes (1977, 1983), Cartwright (1983), Latour (1984, 1987), Callon and Latour (1991), Shapin and Schaffer (1993), Fourez (1996) and others. Let us take only one example of the connection usually described between sociology and philosophy of science. In a textbook devoted to the sociology of science, Vinck (1995) suggests that these philosophers have, by a kind of internal shaking, "opened the door to a sociological analysis" of the contents of science, that remained in an embryonic state in the sociology of Merton (1937, 1938, 1973). The question still remains as to what extent the SSK can support its own programme with such references. It is actually an important question for the coming development of the sociology of knowledge.

1. Duhem

Nowadays, Duhem arouses a new interest that explains the reissuing of his books (Duhem, 1981, 1990). His theses hold the attention of sociologists on the question of "epistemic holism,"[5] which means that "The physicist can never subject to experiment a single hypothesis, [6] but only a whole set of hypotheses; when experiment diverges from prediction, it tells him that at least one hypothesis of the set is unacceptable and needs to be changed; but it doesn't tell which one" (1981: 284). From this, Duhem derives a criticism of inductive method and a profound revision of Bacon's concept of "crucial experiment." Holism thus leads to a weakening of the determination of scientific contents by experiments on the natural world. *Prima facie*, this seems to lend weight to the relativist stand and to the idea of the dependence of knowledge on social beliefs. This connection however clashes with two main arguments.

First argument. In the actual texts concerning holism, Duhem never puts forward the chain R2-R3 imagined by the SSK. He is categorically opposed to such an attempt. *First*, Duhem's analyses rest solely on physics (1981: xv). In many places, he shows that the other sciences, such as mathematics or physiology, are completely outside his attention. Thus we cannot jump to conclusions on scientific activity in general. *Second*, Duhem condemns keenly the idea of a metaphysical (or religious) foundation of this science: "[Physical theories] are autonomous and independent of any metaphysical system" (1981: 334). He shows, for instance, that Descartes, Huygens and Fresnel's contributions to optics weren't deduced from the explanatory principles of light these scientists were most attached to, but from their results as experimenters. Duhem developed this point in 1905, in answer to Abel Rey, who criticised him for having affected the study of the methodology of physics by catholic creeds. In this text, Duhem

5. As Brenner (1990: 36-43) noticed, Duhem's works offer, on that point, a very striking constancy. This conception appears in the 1894 articles: "Some reflections about experimental physics" and "Theories in optics." Duhem writes: "In physics, an experiment can never condemn an isolated hypothesis, but only a whole theoretical set" (1894a: 187); "It is never possible to subject to experimental control a single hypothesis, but only a set of hypotheses" (1984b: 112).

6. In several places, Duhem gives a definition of this word: "The various kinds of magnitude that we have introduced in this way are linked by a small number of propositions that will be used as the principles of our deductions; these principles may be called *hypotheses*, in an etymological sense, because they are the very foundation the theory is built on" (1981: 25). See also: "If, in common language, the word *hypothesis* took the meaning of some dubious supposition, philosophers and astronomers retained its etymological meaning, that of a fundamental proposition a theory is built upon" (1990: 121).

maintains the separation of physical and metaphysical questions: "Having no common term, these two kinds of judgement can neither agree with, nor contradict each other" (1981: 431). *Third*, nor does Duhem admit the transfer of certain hypotheses from "common sense" to physics. He explains himself, showing that "The bottom of common sense is not a treasure buried in the ground […]; it is the resources of an immense and prodigiously active society"—this is the only occurrence of the word "society" in his text—before concluding that, if a physicist believes himself to be using an hypothesis of common sense, "he would have taken back in the fund of common knowledge […], the pieces that the theoretical science itself left in this treasure" (1981: 397). These three reasons, expressed without equivocation by Duhem, invalidate the principle R3 of relativism, that society plays a major role in the construction of scientific statements. Nothing, to tell the truth, is more removed from his view.

Second argument. Duhem's epistemic holism paves the way for a question which Vuillemin (1986) and Brenner (1990) have echoed. When we say that the hypotheses of a theory make up a whole, what should we understand by "whole"? Is it the most limited part of a scientific subject, a specialised field, physics as a whole, or the whole of human knowledge? In reading Duhem, one has the feeling that epistemic holism is limited to one discipline (physics) and that it resembles the holism of physical bodies subjected to gravitation. Indeed, in order to test certain hypotheses of mechanics "it would be necessary, says Duhem, that there exist closed systems; but such systems don't exist: *the only closed system is the Universe as a whole*" (1981: 325, my italics). However, a systematic search of passages that mention the "whole" shows Duhem's hesitancy. Sometimes, it means "the whole system of physical theory" (1981: 304); sometimes it represents only the identifiable and dissoluble *elements* entering into a scientific statement (1981: 321). Vuillemin (1986) and Brenner (1990) have interpreted this hesitancy in opposite ways.

Vuillemin tries to limit the impact of Duhem's epistemic holism by supporting the idea of a "compartmentalisation" of the sciences: "I understand by 'compartments' the existence of autonomous and almost closed systems that closely resemble the ideal of a system independent of any external intrusion. The history of taxonomy, astronomy and dynamics shows that science has been made possible because some of the compartments were frequent and basic enough […] to become easy subjects for theoretical reconstruction" (1986: 19). So Vuillemin's restriction consists in saying that the testing of a theory is performed on the basis of the propositions of the field to which the theory belongs. Given: a theory T entering the compartment C of a science S; p a proposition of T, p' a proposition of C − T, p'' a proposition belonging to S − C. The relation Fpp' (p' grounds p) is false if there exists a p'' such that Fpp''. And yet we can find many examples of this type. Let us take the example given by Duhem that consists in the calculation of the position of the sun (1981: 254). Several hypotheses must be advanced: geometrical (reduction of the solar globe to a sphere, reduction of the centre of gravity to a geometrical centre), optical (the constancy of the speed of light, the law of atmospheric refraction), temporal (knowledge of solar time and sidereal time), geographical (determination of latitude and longitude), and mathematical (algebra, sexagesimal arithmetic and trigonometry). Thus it appears that the hypotheses are external only to the compartment C of mechanics.

Does the previous example then justify the interpretation of Brenner (1990), that the testing of a theory implies the handling of *every* proposition of physics? Given the same writing conventions (let T be a theory of the compartment C of a science S; p a proposition of T, p' a proposition of C − T, p'' a proposition of S − C*)*, then Brenner's reading corresponds to Fpp''. This relation is false if a p'' exists, so that $\neg Fpp''$. Again, in the same example, it is clear that if external hypotheses are necessary to determine the position of the sun, no part of relativity, electrostatics, or thermodynamics is useful for this calculation. Many compartments of physics don't contribute at all to the setting of T. So much so that the hypotheses the physicist relies on are not taken from the field S of physics.

Those two readings of Duhem's epistemic holism raise difficulties. A new interpretation has to be launched. If we go back to the example of the calculation of the position of the sun, it is clear that, given C the compartment of mechanics not every proposition of C grounds T, and that the propositions which really ground T don't necessarily belong to C. Equally, not every proposition of S grounds T. What Duhem meant by "holism" was perhaps that *certain theoretical elements* coming into the picture of a physical theory belong to the whole field of a science, not that *every* theoretical element of this science contributes to the construction of a specific theory. Vuillemin's compartmentalism can be retained if we carefully use a negative: "Some compartments of a science *do not* contain any propositions founding a theory." Then Duhem's epistemic holism is not a holism *stricto sensu*. Indeed, it loses its nature of being an organic whole and expresses no more than the need for grounding a theory upon something *external*.

Whatever ones favourite interpretation, it appears that none can make Duhem's ideas agree with the principles of relativism. In particular, principle R3 is the exact opposite of the Duhemian view, which would never admit a continuity between social beliefs and physical theory.

Duhem is also much quoted with regard to his thesis concerning the underdetermination of theory, afterwards developed by Quine, that several rival theories can pass the same experimental tests. "Abstract and mathematical propositions that theories link to observed facts are not […] fully determined; an infinity of various propositions can match the same facts" (1981: 245). The same physical phenomenon is describable by several incompatible theories. In *Sôzein ta phainomena*, Duhem says: "The hypotheses of physics are nothing but mathematical tricks designed to save the phenomena" (1990: 140). This passage has an unquestionable overtone of relativism.[7] But, is this thesis a guarantee for the stance of the SSK? in particular for the principles R2 (the world plays a negligible role in the construction of scientific statements) and R4 (every theory is conventional and based on fragile consensus)? The idea that these principles can be deduced from Duhem's texts clashes with new sound objections.

First argument. Duhem admits the arbitrariness of physical theory, but indeed confines this arbitrariness to its stage of development. Admittedly, we read: "A physical theory is free […] to take no account of experimental facts" (1981: 313). But we should read this sentence in its context. Duhem states that freedom only lasts during the time of conception: "It does not apply when *the theory has reached its complete development*" and when it is subjected to experimental

7. On this point, it is perhaps unfounded to see a radical break between *La théorie physique* and *Sôzein ta phainomena*, as is supported by Petroni (1994: 114n). The developments of 1908 are in perfect agreement with the thesis of the representation of the real that appears in his first book. Compare for instance: "So Astronomy doesn't seize the essence of celestial things; it only gives an image of them; this image is not correct but only approximate" (1990: 23) with the passage where Duhem confronts explanation and representation (1981: 26). The only gap concerns the formula "save the phenomena." Note that the Greek σωζειν means equally: "save, *keep in memory*, observe," and that an equivalent word from Plato is: διασωθηναι, which also means "*keep faithfully*." Therefore, the title *Sôzein ta phainomena* could have been understood in less catchy terms. Duhem simply uses this expression to mean that astronomers have often preferred to retain phenomena rather than theories.

tests (that will decide whether or not it has to be rejected). In other places, Duhem explains that an array of rival theories may be reducible one to the other, *before* they pass experimental tests, depending on the degree to which the deductions we can obtain from them are useful (1981: 205). In addition, the fact that two rival theories are in the position to give an account of the same facts doesn't prevent our preferring one to the other, from a rational choice based on the logical aspects of the two theories[8] (the one may require some *ad hoc* inconsistent hypotheses, whereas the other offers an obvious simplicity). Pierre Duhem mentions the example of Biot, who refused to support Newton's corpuscular theory of light once it had been contradicted by Foucault's experiments (1981: 331). This development shows that we cannot deduce from Duhem's texts the principles R2 and R4 of relativism, other than at the risk of a forced reading. The misunderstanding stems from reasoning *pars pro toto*. The fact that the development of hypotheses could be vitiated by arbitrariness doesn't mean that experimental tests and the deduction of consequences have to be so vitiated. Remember that, in Duhem's view, such operations are designed to limit the arbitrariness of initial hypotheses.

Second argument. In relation to the conventional nature of knowledge that is often ascribed to him, Duhem doesn't support a hard-line position, because he explicitly subscribes to both theories of consistency (formal rationality) and correspondence (objective rationality). *First*, the physicist is free to build any theory he likes as long as it fulfils the rules of logical consistency. Duhem often comes back to this aspect of physical theory. He writes: "[Physics] has to build a logical structure; it is thus compelled, by planning this structure, to respect scrupulously the laws that logic imposes on all deductive reasoning" (1981: 312). The philosopher then applies these characteristics to the hypotheses: "First, an hypothesis shouldn't be a self-contradictory proposition, for the physicist intends to exclude all nonsense. Second, the various hypotheses that have to carry physics shouldn't contradict each other" (1981: 335). The arbitrariness of a

8. Duhem writes: "It may be that *common sense* allows us to decide on our two physicists. It may be that we don't consider the haste of the second physicist to disrupt the principles of a wide-ranging and harmonious theory to be sensible, when a detail modification, a slight correction, would be enough to bring these theories into agreement with the facts. But it may be, on the contrary, that we think childish and unreasonable the obstinacy of the first physicist to maintain at all costs, by continually repairing and constructing a jumble of muddled props, the worm-eaten columns of a building that wobbles on both sides, when, by knocking down those columns, it would be possible to build on new hypotheses a simple, clear and solid system" (1981: 330).

theory is thus conditioned in two ways: by time (its stage of development) and by an external norm (logical consistency). *Second*, Duhem shows that, in fact, the confinement of arbitrariness is not twofold but threefold. An hypothesis is indeed subjected to a rule of correspondence with the real, which Duhem calls "representation" or "likeness" (1981: 24, 313). When speaking about the experimental consequences that derive from hypotheses, he writes: "These judgements are compared to experimental laws that theory intends to represent; if they agree with laws […] the theory has reached its goal we declare it valid; otherwise it is wrong, it needs to be changed or rejected […]. *For a physical theory, agreement with experiment is the only criterion of truth*" (1981: 26). However, this adequacy is never perfect, for it is based on a translation of natural phenomena into a symbolic language that smuggles in approximations. So it becomes clear that: "Every physical law is necessarily an approximate law" (1981: 259). This is why a physical law is temporary and revisable; not only because of the freedom of construction that theoretical activity presupposes, but also because of the advances in measurement and observation (1981: 261). The growing accuracy of physical measurements then commands a revision of hypotheses, thereby determining a growing resemblance between theory and phenomena (1981: 311). An example illustrates this progress quite well: "When we see […] the immense field of optics, until then so dense and confused, being put in order and organised, we cannot believe that this order and this organisation shouldn't be the image of real order and organisation […]. The more the [physical theory] improves, the more we believe that the logical order, in which it arranges the experimental laws, is the reflection of an ontological order" (1981: 35). So Duhem doesn't simply recognise the existence of the real; he admits that, by running trials, physics manages to build up a set of statements that corresponds to it with an ever-increasing accuracy: the final goal of the physical theory is to become a "natural classification." This clear-cut position evidently conflicts with principle R1 that the world doesn't exist outside textual constructions, with principle R3 that plays down the function of the natural world in the construction of theories, and with principle R4 that expounds the conventional nature—in a non-Duhemian sense—of scientific theories. Whatever we think of Duhem's idea of physics, we should recognise that it doesn't lend any weight to the stand taken by the SSK. In Duhem's view, the preference between rival theories is not at all determined by social beliefs, but only by considerations of adequacy, simplicity and mathematical elegance.

2. Quine

The works of Willard van Orman Quine offer an interesting expression of the thesis of the "underdetermination of theory" that is often called on in support of the SSK. Resulting both from analytic philosophy and from a criticism of logical empiricism, this development is accompanied by subtle differences that partly invalidate the name "the Duhem-Quine thesis" used by certain scholars (cf. Quine, 1963: 41, 1977: 17).

Quine's holism returns to the main argument of *La Théorie physique*, so much so that we notice, in some places, an almost identical position: "Our statements about external reality confront the judgement of sensitive experience, not individually, but as an organised body" (1972: 12). According to Boyer (1978), Quine has seen yet further: 1) he converts Duhem's neutrality into an "ontological engagement"; 2) he deserts the dualism of scientific and common-sense knowledge in favour of there being a continuity between them; 3) his holism concerns not only the interpretation of experiments, but also the meaning[9] of theory (semantic holism)—thus Lakatos distinguished a weak version (Duhem) and a strong version (Quine) of the famous "Duhem-Quine thesis" (Lakatos, 1994: 138-139).

First argument. Let us start from epistemic holism, one of the many Quinean wordings of which could be: "Experiences call for changing a theory, but do not indicate just where and how" (1977: 106). Quine spontaneously qualifies his holistic thesis.[10] The philosopher assumes that we are not compelled to revise all the propositions that found a theory, but only those most directly tied, by relations of affinity, to the object of investigation. "Affinity" means a "soft association, reflecting the [theory's] relative probability, so that we choose, in practice, to revise such a statement rather than another, in case of a recalcitrant experiment" (1980: 118). Two classes of statement enjoy a greater stability: observational statements and logico-mathematical statements. Quine himself explains the immunity he grants to the second sort of

9. Let us add that, in the context of the verificationism admitted by Quine, the meaning of a theory only lies in its procedure of verification.

10. Quine considers this kind of holism as unjustified. He writes: "This point has been lost sight of, I think, by some who have objected to an excessive holism espoused in occasional brief passages of mine" (1977: 40n).

statement:[11] "The more a law is fundamental for our conceptual organisation, the less likely it is that we choose it for revision […]. Vast domains of laws can easily be considered as a rule to be immune from revision […]. Mathematics and logic, located as they are at the centre of our conceptual organisation, tend to be granted such an immunity, as a consequence of our conservative preference for the revisions that disturb the system as little as possible" (1972: 12). The idea of a selective revision of hypotheses—sometimes called the standard of "least action" (1977: 50)—connects either with Vuillemin's compartmentalism or with his restriction, as suggested above. But it openly challenges the principles R1 and R3 of relativism.

Second argument. There seems to exist a serious incompatibility between Quine's holistic view and the conventionalism we have identified as principle R4 of relativism. Indeed, epistemic holism intends to retain the criterion of verification, otherwise we couldn't even say that the statements confront experimental tests (as an organised body), because there wouldn't exist any verification procedure allowing us to confront them with reality. Therefore, holism and verificationism are interdependent. If the meaning of a theory is given by procedural rules, it needn't be founded on a priori conventions. This result bears unsuspected consequences. As Quine (1980) questions the gap between synthetic and analytic statements, the previous conclusion applies both to the experimental sciences and to the logico-mathematical field, where the notion of convention is equally suspended. So Seymour says: "Logic appears no more as a set of conventional rules, but definitely as an effective inferential practice, and the rules of calculation stand here only to model this practice" (2000: 136). It is then Quine's commitment to pragmatism that invalidates the deduction of principle R4 from his texts.

Third argument. Finally, Quine observes an incompatibility between semantic holism and the thesis of underdetermination of theory (and its corollary of the indeterminacy of translation). Indeed, semantic holism is closely connected to the verificationism of the thirties. The *Wiener Kreis* placed the meaning of a statement in its truth-conditions (or, what comes to the same thing, in its verification procedure). It follows that, if two theoretical statements have the

11. As for the first (observational statements), they are evidently revisable, but only within certain limits. On this point, Quine says: "We cannot, at the risk of a nonsense, reappraise the reality of the external world, nor deny that our senses testify in favour of the existence of external objects" (1980: 220), as clearly invalidates principle R1 of relativism.

same truth-conditions, they are *synonymous*, and they falsify the thesis of the underdetermination of theory (Gochet, 1978: 40). As Quine wants to preserve this, he moves away from holism towards a sort of semantic atomism. In that line, he writes: "Translation proceeds little by little and sentences are thought of as conveying meaning severally" (1977: 125). In a way, Quine is forced either to abandon the thesis of the underdetermination of theories in favour of holism (that confronts previous arguments), or to abandon holism in favour of the thesis of underdetermination, as we shall now consider.

Quine's version of the underdetermination of theory by data implies that various rival theories are in the position to pass tests *ex aequo*. "In general the simplest possible theory for a given purpose needs not be unique […]. Scientific method is the way to truth, but it affords even in principle no unique definition of truth" (1977: 54). This standpoint questions the idea—admitted by Duhem—that science may approach the truth in an asymptotic way. It seems that we have here a congruent point with principle R4 of relativism, that every truth is of a conventional nature and lends itself to negotiation. This reading is subject to debate.

First argument. The difficulty of deciding between various theories is the result of an equivalence of rival systems—from the point of view of experiment, logic, or scientific interest. In such a case, scientists can agree to adopt one such theory, but there is very little chance that they do so if the theories present *exactly* the same degree of correctness or usefulness. The history of science contains many examples of two rival theories both being used for a long period before a new experiment or a logical revision came along and proved the superiority of one of them. See, for instance, the lengthy debate (1815-1911) between Stas and Prout's theories about the atomic weight of chlorine. On a small scale, every controversy is a proof of this. The postulate that all sets of good theories should be conventionally reduced to but one theory is obviously wrong, because no rule and no behavioural norm allows a scientist to choose between several theories that are *exactly* equivalent. It's got to be one thing or the other: either there is no choice because the theories are strictly equivalent (the conventional *choice* is groundless); or there is a choice that is made on the basis of scientific criteria (the *conventional* choice is groundless). The conversion of the thesis of the underdetermination of theory into that of the determination of theory by social factors (principle R3) has no regularity, and that is probably why this conversion is nowhere clarified.

Second argument. We don't have to examine Quine a great deal in order to see how the thesis of the underdetermination of theory correlates with relativism. He asks: "Have we now lowered our sights so far as to settle for a relativistic doctrine of truth? […] Not so" (1977: 56). Such a negative answer needs an explanation. Let us take the example of two rival theories proceeding from a rewording of the laws of mechanics. The relation linking force, mass and acceleration admits two equivalent wordings: 1) a differential equation; and 2) a discrete equation. Some suppose this rewording to be a case where two theories, having exactly the same empirical content, are logically incompatible—in the first equation, time is continuous; in the second, discontinuous. Although being clear as to the goal, the choice of the example is questionable. *First*, it doesn't match Quine's exact purpose: "The indeterminacy that I mean is more radical. It is that rival systems of analytical hypotheses can conform to all speech dispositions […] and yet dictate, in countless cases, utterly disparate translations; not mere mutual paraphrases, but translations each of which would be excluded by the other system of translation" (1977: 119). A discrete equation may be changed into a differential equation and vice versa. Therefore, they are two "paraphrases." *Second*, if we assume that they are not, their difference according to the model of time removes any strict equivalence. A choice is then possible between the two translations, depending on a correspondence norm (the model of continuous time agrees better with experience). It seems difficult to find an indisputable case of two statements having exactly the same empirical content.

Third argument. Let us admit, nevertheless, that Quine comes close to adopting a relativist idea of truth, as some of the instigators of the SSK seem to think. It is right that Quine (1993: 116-117) admits a deflationist theory of truth such that:

"*p*" is true if *p*.

Thus, "the table is round" is true if, and only if, the table is round. This rule—called dequotation—raises at least two problems. *First*, as Engel observed, it seems difficult "to avoid reintroducing here our intuitions about correspondence and consistency" (1998: 38). For instance, are we entitled to dequote the sentence in all cases? A detailed examination shows, on the contrary, that we can dequote it only if there is a way of subjecting the roundness of the table to verification. Otherwise, it seems impossible to know what "the table is round" means.

In this context, the dequotation theory of truth is, no more and no less, reduced to the classical correspondence thesis. *Second*, the dequotation theory conflicts with the pragmatist definition of meaning Quine adheres to. It stipulates that the meaning of a statement is determined by its truth-conditions, while, according to dequotation theory, the examination of the possible truth of a statement presupposes its meaning. It requires either withdrawing from verificationism—as Quine refused to do in other circumstances—or revising the theory of truth in favour of the correspondence thesis—as seems possible.[12] But the fact remains that both possibilities weaken principles R1 and R2 of relativism.

Each conclusion requires giving up one or several points of the programme of relativism; so it's not possible to endorse the whole Quinean view without refuting, as a result, the principles the SSK is based on.

3. Wittgenstein

Wittgenstein's texts are often referred to by the instigators of relativism in the field of the SSK, in particular by Bloor (1973, 1983b). It is not the early Wittgenstein that holds their attention, but the author of the works undertaken at Cambridge in 1929: those that were to lead to the writing of the *Philosophische Untersuchungen*. The question still remains whether we may correctly conceive a division between an "early" and a "later" Wittgenstein. The two groups of texts—dating from 1921 and 1936-1949 respectively—mark an unquestionable evolution in the thought of the Viennese philosopher; but they are not divorced from one another, as one might think from reading certain commentaries. Those writing such commentaries are aware of the aphoristic appearance of Wittgenstein's writings. Commentators have often put emphasis on the sense of formula in Wittgenstein. The idea that we have to do with aphorisms is based on two elements: the strength of the ideas, and the literary purity of some passages. Shwayder (1969: 66) thus observes that the text works as "flashes of lightning." Nevertheless, the term "aphorism" presents a danger: it argues in favour of an autonomous reading of the

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12. More accurate propositions are put forward by Wright (1992) and Engel (1998). In the context of this minimal conception, the truth would be, as Engel explains, an *over-assertibility* norm, "because it records that our assertions are justified in the way that is most stable, absolute, and shielded against revision. Even if we are never certain to have reached this, perhaps mythical, ideal limit, it is what we aim for" (1998: 72).

paragraphs, and so justifies any arbitrary extractions. "*Apophthegma*"—Greek for "memorable words"—would fit better, because it leaves to the commentator the responsibility of the excising.

If Wittgenstein's *Tractatus* and *Philosophische Untersuchungen* both appear as structured successions of paragraphs, the method of reading that consists in extracting one of them in order to synthesise a view is not a proceeding in keeping with the author's project. Wittgenstein refused to condense his researches (1961: 111). It is always attractive to make such a synthesis, but it can lead to as many different results as we have possible choices of excising. There exists a visible order in the *Philosophische Untersuchungen* that follows the hierarchical organisation of the *Tractatus*. In general, the extraction of a remark involves the deletion of the ordering number which is seldom used as a reference,[13] though this number indicates precisely which propositions the remark has to be linked to. The *Tractatus* is not a book nor an album. It is a *tree*, the reader being supposed to make a clear distinction between two types of progression: a "horizontal" reading that links coextensive propositions (e.g. 3.11, 3.12, 3.13, 3.14); and a "vertical" reading that follows the predecessors explaining the proposition in question (e.g. 2, 2.1, 2.13, 2.131) (Granger 1969, 1990, Table 1). Granger then summarises the chain of basic propositions approximately in these terms: "The world is all that is the case; A logical picture of facts [a proposition] is a thought; Truth-possibilities of elementary propositions are the conditions of the truth and falsity of propositions; What we cannot speak about we must pass over in silence" (Granger, 1969: 22-25).

On Wittgenstein's own admission, the softening that we see when moving from the *Tractatus* to the *Philosophische Untersuchungen* is a mark of a change in the treatment of the problem of meaning. But Wittgenstein's suggestion of presenting the two groups of apophthegms together also vouches for a certain continuity (his endless questioning about activity and use). As far as Wittgenstein is concerned, the *Tractatus* and the *Philosophische Untersuchungen* are interdependent. We may read the passages of the latter, referring to their origin. Thus, paragraph 282

13. Here we may regret the little care taken in the French Pierre Klossowski's edition. The most obvious mistakes of numbering are the following: 2.051 (= 2.0251), 2.063 (= 2.062), 3.31 (= 3.031), 521 (= 5.521), etc. They make the perception of chaining very difficult.

seems to be a result of the *Tractatus'* fourth vertical chain (4, 4.4, 4.46, 4.461, 4.4611).

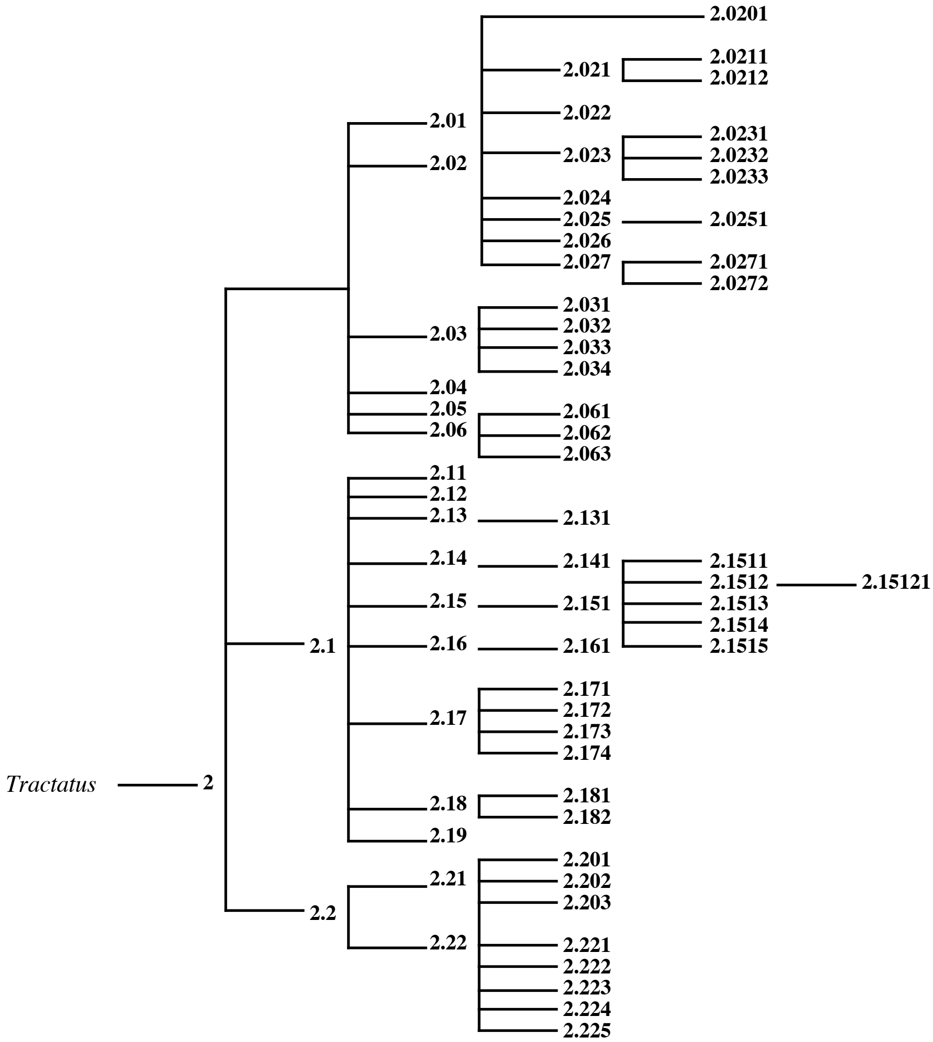

Table 1: The tree of the Tractatus, proposition 2

The remarks concerning the method of reading required by the apophthegms now allow us to tackle the passages on which sociological relativism is based.

Let us start with the conventional nature of knowledge. Bloor (1983a: 34), and Barnes (1983: 33) and Shapin and Schaffer (1993: 152), think Wittgenstein to be a source supporting their programme of studying the *social foundations* of (conventional) scientific knowledge. In so thinking, sociological relativists ignore the fact that Wittgenstein himself answered a very similar question: "Are mathematical propositions anthropological propositions telling how we, mankind, infer and calculate? Is a legal code a book of anthropology that tells us how men of that people treat a thief? […] But the judge *doesn't use* the code as a textbook in anthropology" (1983: 174). Mathematical conventions are not anthropological objects either. But let us assume that Wittgenstein said nothing about the *extra-social* nature of conventions, and go on following the view of relativism. The passage that fits best with this view is:

4.002. The tacit conventions on which the understanding of everyday language depends are enormously complicated.

Separately, it is true that this "aphorism" has an overtone of relativism. But the fact is that the previous remarks forbid any aphoristic reading; this proposition must first be referred to its source-proposition:

4. A thought is a proposition with a sense.

What is thought? Wittgenstein answers: "3. The logical picture of facts is thought." What is a picture? Wittgenstein says:

2.1511. *That* is how a picture is attached to reality; it reaches right out to it.

2.1512. [The picture] is laid against reality like a measure.

Thinking thus requires a correspondence between thought and the world, as 2.1511 and 2.1512 show. No nihilism and no scepticism appear here. The exercise of thinking doesn't mean that man is condemned to manipulate arbitrary pictures (because the tacit conventions of understanding the world would be unattainable), nor that the world doesn't exist: "1. The world is all that is the case."

In 4.002, Wittgenstein only speaks of the difficulty of describing the functioning of *natural language*, not to be confused with *formal language* (besides, this is precisely the problem that will encourage him to set about describing natural language from 1929 on). The Viennese philosopher makes a distinction between philosophy (that uses natural language) and science (that may use a formal language). This cut is established by the following paragraphs:

4.11. The totality of true propositions is the whole of natural science (or the whole corpus of the natural sciences).

4.111. Philosophy is not one of the natural sciences.

Therefore, paragraph 4.002 cannot act as part of the foundation of the SSK programme, because Wittgenstein assumes that the scientist's goal is to describe the world, and that he achieves this end by making a logical picture; he can determine whether or not it corresponds to the real. The relativist reading is improper, insofar as it attaches no importance to Wittgenstein's clear distinction between statements of common language and scientific statements.

Let us now focus on one of the most famous passage of the *Philosophische Untersuchungen*, in which Wittgenstein introduces "language-games." According to the sociologists Shapin and Schaffer, Wittgenstein would like to "highlight the fact that speaking a certain language is akin to some activity or life-form" (1993: 21). Thus, they suggest that scientific controversies have to be studied as "language-games." Others, such as Vinck, have learnt that Wittgenstein put natural language in the very centre, and that he "refutes any preferential place for logic" (1995: 85). See now the arguments that can be directed at those attempts at connection.

First argument. Apart from any consideration regarding its form of expression, a language is structured by its function. The goal of a game is to play; the goal of science is to describe as far as possible natural phenomena. To draw a parallel is to admit either that the correspondence norm applies to the game, or that it doesn't apply to science. The first hypothesis is really unconvincing, for no game of chess can exist *before* the players know the rules of the game. Such rules don't tend to give an account of preceding experimental data; they aim at fixing a priori conditions for the possibility of a game that is never given in advance. The second hypothesis is no better. It means that science is a game the rules of which are laid down a priori, which then fix the appearance of natural phenomena. An experiment determining the presence of gravity, for instance, doesn't follow but *precedes* the description of natural phenomenon. Wittgenstein—who sometimes nourished doubt about the analogy between game and language—would probably not have admitted this annexation of science by games. He speaks, in explicit terms, of the "a priori order of the world [which] stands before any experience" (1961: 161). Scientific work is not to be considered as a game, owing to the irreconcilable natures of the two.

Second argument. Suppose now—we are forced to make this assumption to go on—that

Wittgenstein never speaks about the difference between mathematics, logic and natural language (cf. 1961: *passim* chain 4, 1965: 28). Thus we should wonder, with certain other scholars, whether all his reflection on language can lend any weight to the SSK. First notice that, in Wittgenstein's view, the formula "language-game" doesn't emphasise any absurd aspect of language, but the existence of *rules*, as they exist in games. This development follows paragraph 3.326 of the *Tractatus*, that founds the recognition of symbols on their use. The word "language-game" only aims to clarify our use of words, applying the formula: "How do we use…" But what is that use? Wittgenstein suggests an analogy: "Let us say that the meaning of a piece is its role in the game. […] The game must be determined only by rules!" (1961: 281-282). And he always takes for a model games where the rules are *well-defined, explicit and compulsory*. "If a rule doesn't compel you, then you are not following any rule" (1983: 329). In addition, he declares language to be a human system of communication (1965: 81). When connecting these two developments, it is clear that we should now render "language-game" by: "explicit and compulsory rules of communication." Were we to do so, I would guess that the instigators of the SSK would be less interested in it! Indeed, this rendering forbids any relativist account. Let us also add that every time Wittgenstein admits the existence of "vagueness" in the rules, he only mentions natural language, and carefully excludes the formal languages of science (1961: 162).

Third argument. Return now to the study of scientific conventions. In paragraph 199 of the *Philosophische Untersuchungen*, Wittgenstein asks a genuine question of sociology of science that allows us to catch another glimpse of language-games. "Is what we call 'obey a rule' something that a *single* man could do, and only *once* in his life?" The answer is: "To obey a rule […] to play chess, is a *habit* (use, institution)" (1961: 202). Even if Wittgenstein limits his comments to natural language, we may think that this passage implies a small gap between logic and common language. What does the equivalence between uses, habits, customs and institutions mean? Perhaps, that sociology might have something to tell us, not about the *rule*, but about the *obeying* of the rule.[14] Scientific theories not being rules scientists have to obey—their work consists in testing them—the question of obedience doesn't reach scientific knowledge, but

14. When asking questions such as: On what basis does the respect for rules stand? Does the breaking of rules involve a penalty? Are rules learned? etc., we rediscover large parts of Mertonian sociology of science.

only the methods and the general norms making up experimental tests. If convention may concern the *obedience relation* to certain principles or methods, we must keep in mind that, according to Wittgenstein, it doesn't concern theories or scientific results. This conclusion opposes Bloor's view, that "Scientific theories, methods and justified results are social conventions" (1983a: 48).

Only we hardly see—even in the context of Wittgenstein's conventionalism—how theories and scientific results may proceed from social conventions. But it is still possible to justify Bloor's view by declaring that such conventions proceed indirectly from theories and results that are always supposed to have recourse to methods.[15] So we need now to decide whether or not methods have a conventional nature. Although this conception is often maintained, it again confronts an old but ever-decisive paradox. It is due to Lewis Carroll (1895), and is expressed in the form of a *modus ponens*. What do we do to infer the conclusion "q" from "p" and "p implies q"? From a strictly conventionalist standpoint, we can make such an inference only because of the conventional nature of *modus ponens*: "if p, and p implies q, then q." But nothing guarantees that this rule should apply to the special case we are focusing on. Therefore, we must admit by convention that: "if p, p implies q, if p and p implies q, then q." So, the way is open to an infinite regression (Table 2).

premises	conclusion
1. "p", "p implies q"	"q"
2. [1] + "p and p implies q, then q"	"q"
3. [2] + "p, p implies q, if p and p implies q, then q"	"q"
4. [3] + etc.	"q"

Table 2: Lewis Carroll's paradox

15. The distinction proposed here between methods and results is one of major importance, because the question of the conventional nature of a method bears no relation to that of the conventional nature of the results reached by that method. There is a distinction between the two. A good example is to be found in the *abacus*, with the different types of multiplication invented, and practised, in relative independence: the Persian form (with the deletion of intermediary results), the Indian (with the retention of intermediary results), the Arab (by the *gelosia* method), the Italian (by the reduced *gelosia* method), etc. The *operans* and *operandum* being fixed, all *different* methods evidently give the *same* results.

A strict conventionalist point of view should then claim that the most elementary logical deduction needs a infinite number of premises. This result is clearly counter-intuitive for, in a logical deduction, it's not possible to survey an infinite series of premises. If *p*, and *p* implies *q*, we immediately deduce *q*, without calling on any other premise. As Seymour says: "It is hard to see how the conventionalist conception can appropriately solve Lewis Carroll's paradox" (2000: 130).

In sum, when ruling out aphoristic readings that employ text-excising, we observe that Wittgenstein's ideas can only with difficulty be considered compatible with principles R1 and R4 of contemporary sociology.

Conclusion

It is no small paradox that Duhem's, Quine's and Wittgenstein's texts—so frequently called upon by the SSK—cannot actively help the programme that the SSK has tried to base itself upon for about thirty years. It follows that philosophical references to their works secure no real continuity between philosophy and sociology of science. They rather constitute a kind of self-legitimating. Recalling glorious ancestors ever strengthens the belief in the existence of a genuine tradition of research! Now we can formulate some concluding remarks. *First*, the genealogy of relativism is probably to be found more in hidden readings than in those of Duhem, Quine and Wittgenstein.[16] *Second*, the frequent recourse to "authority" is perhaps a mark of the weakness, not to say failure, of the relativist programme.[17] *Third*, we may suggest that

16. Nietzsche, for instance. As far as France is concerned, a study of most influential scholars' careers in the humanities reveals the appearance of a whole generation marked by Nietzsche and his French commentators. Some have later denied this influence, whereas others have reinforced sceptical and nihilist leanings (Dosse, 1997: 419). In the field of the SSK, Latour openly admits the mark of Nietzsche on his conception of science (1997: 125), and Fourez refers to Nietzsche in various passages (1996: 35-37, 363-366). Regarding Nietzsche's reception in France, cf. Staszak (1994). We can find in Bouveresse (1973, 1984) many elements that throw light on the contemporary diffusion of the relativist doctrine.

17. Remember the criticism of Siegel (1987), who puts forward two main arguments. The first is the argument—formerly called "the argument of retorsion"—UVNR [relativism undermines the very notion of rightness]: "Relativism is inconsistent because, if it is right, then the very concept of truth is undetermined, in which

the SSK should rediscover the way to a rational analysis of scientific knowledge.[18]

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case relativism cannot be true." Second is the argument NSBF [necessarily some beliefs are false]: "Relativism is inconsistent because it claims that all beliefs and opinions are true; but given that opinions conflict, some are necessarily wrong, in which case relativism cannot be true" (1987: 4-6). Siegel then shows that all the arguments for relativism fall within the province of one or the other of the two arguments. The concept of "relative truth" is thus self-refuting.

18. As well as studying styles of thinking, the SSK has also something to say about statements that are wrong (logical lacunae, observational errors) and untestable statements (axioms, postulates, *ad hoc* statements). The untestable statements are only marginally parts of a scientific theory, for their truth-values cannot be determined. Strictly speaking, they are "a-scientific statements." This is why the SSK can look to see whether social interests or beliefs lead to their adoption or rejection. On this point, cf. Raynaud (1998ab, 1999).

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