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PIERRE DUHEM'S *THE AIM AND STRUCTURE OF PHYSICAL THEORY*: A BOOK AGAINST CONVENTIONALISM

ABSTRACT. I reject the widely held view that Duhem's 1906 book *La Théorie physique* is a statement of instrumentalistic conventionalism, motivated by the scientific crisis at the end of the nineteenth century. By considering Duhem's historical context I show that his epistemological views were already formed before the crisis occured; that he consistently supported general thermodynamics against the new atomism; and that he rejected the epistemological views of the latter's philosophical supporters. In particular I show that Duhem rejected Poincaré's account of scientific language, Le Roy's view that laws are definitions, and the conventionalist's use of simplicity as the criterion of theory choice. Duhem regarded most theory choices as decidable on empirical grounds, but made historical context the main determining factor in scientific change.

Duhem's famous book *La Théorie physique* is almost universally considered one of the most significant documents of that cultural movement addressed against positivist optimism. Reflecting on the crisis of nineteenth-century mechanism, at the beginning of our century, this movement generated an instrumentalistic conception of scientific knowledge. Duhem's text has always been considered one of the most brilliant and vital – perhaps the most vital – of the conventionalist movement, the skeptical, philosophical answer to the difficulties of classical science.

The study of Duhem's intellectual biography (Maiocchi 1985) has led me to reach conclusions in many ways diametrically opposed to traditional judgments. These may be synthesized in a formula which is only apparently paradoxical: the main intent of the *Théorie physique* was to oppose instrumentalism, subjectivism, and the devaluation of the cognitive power of science.

The first observation to be made, apparently a point of chronology, but of decisive importance, is that the epistemological theses contained in the *Théorie physique* of 1906 were clearly and fully expressed by Duhem in a series of articles written between 1892 and 1894 (Duhem 1892a, 1892b, 1892c, 1893a, 1893b, 1893c, and 1894a). Thus Duhem's epistemology predates the discovery of radioactive phenomena, Gouy's experiments on Brownian motion, and Kaufmann's experiments on the variable mass of the electron, as well as the introduction of quantum

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hypothesis and the first relativistic hypotheses. In short, Duhem presented his epistemological theses before the "undoing of all principles", to use Poincaré's ill-timed expression, and before the explosion of the 'crisis' of the sciences.

In fact, the thematics of crisis are totally absent in Duhem. On the contrary, all of Duhem's historical and epistemological reflections, all of his scientific work, as a researcher reveals the conviction that science was not only undergoing a period of great splendor during the late nineteenth century, but was getting rid of the errors that had accompanied it through the last three centuries! Duhem's criticism of mechanism never attacks the trust in mechanics as a theory of mathematical physics, but always and only rejects attempts to extend mechanics into a nonscientific, metaphysical sphere. Above all, his criticism of mechanism is based upon the fundamental assumption that there is a better theory than rational mechanics, i.e., generalized thermodynamics. It is the success of thermodynamics that imposes the necessity of constructing a new mechanics, not the failure of the old one. Thanks to the new generalized mechanics, the dreams of the boldest mechanists, such as Berthollet, seemed to be on the verge of coming true.

The new mechanics did not reject classical mechanics, but enlarged and generalized it. Classical mechanics became the model for rigor as well as for method (Newton's, obviously, not Descarte's), and for the form given to one's own principles, which were required to maintain the closest possible analogy with the classical ones. The new mechanics stayed close to its classical model, rather than opposing it, following the original program of energetics formulated by Rankine and carried on by the mechanist William Thomson, before the latter became what Duhem called a "modelist" (see Duhem 1893d).

In Duhem's opinion, the developments in nineteenth century science confirm the positivist belief in a continuous progress of scientific knowledge from other methodological bases: "In our days, many are being swept by a wave of skepticism", but those who force themselves to find in science "the continuation of a tradition of a slow but steady progress", will see "that a theory that disappears, never disappears completely" (Duhem 1894b, p. 124). It is not the crisis of science, but its successes which impose upon Duhem the necessity of epistemological reflection.

Duhem's interpretation of scientific theories as simple instruments of classification does not appear as an answer to a supposed crisis of

mechanism. Not only is his epistemology free of such a 'crisis', but such instrumentalistic conceptions had already been present in the French milieu for several decades. Diffidence toward hypotheses, a phenomenalist view of science, and an instrumentalistic, manipulative interpretation of theories were widely diffused ideas, and dominant among the French scientists of the positivist age. The French scientists' ideal was personified by the likes of Regnault, Bertin, Berthelot, Sainte-Claire Deville, Jamin, Cornu, Violle, and Le Chatelier – all fundamentally experimenters. They sporadically showed an ideological belief in the supreme value of mechanics, although in fact they produced 'anti-modelist' physics. In many cases, their work was mathematically poor, deaf to the calls of theoretical physics and insensitive to nuances of experiment. Even more clearly than contemporary physics, the milieu of chemistry, in which Duhem was trained, showed a general mistrust of the idea that scientific theories might yield explanations, in the sense of revealing hidden truths behind phenomena. These objections appeared most clearly in the case of the atomic theory. Atomism was interpreted in the first place as a classifying tool, even by its supporters like Wurtz. Duhem's understanding of science and epistemology was fundamentally influenced by Henri Sainte-Claire Deville. In the work of this French chemist-physicist, Duhem found, even before Mach, the very clearly stated idea that every scientific theory is simply a classifying tool (see Sainte-Claire Deville 1866).

Unlike Mach in Germany, Duhem did not have to fight against dogmatic belief in the nascent cognitive power of mechanics or against a tendency to objectify models. The cognitive devaluation of theories and models was already extensively employed as a criticism by French positivism. As criticism it was not particularly discerning, but certainly historically effective. Duhem had to fight a battle in exactly the opposite direction: contrary to the flattening out imposed by the empiricist method of his predecessors, he had to avenge the rights of theory, showing how the ineliminable theoretical components present in every observation gave meaning to the scientist's experimental work. Duhem's epistemology was a defense of theories against positivist pretenses to eliminate them by strictly reducing science to pure experience. The positivists considered theories as secondary tools when compared to experience, even as superfluous and therefore, eliminable. Duhem endeavored to show that theories are the heart of a scientific venture.

His was a radically anti-inductivist, antiempiricistic epistemology, a 'praise of theories', and in that sense it was opposed to Mach's.

Even Duhem's most renowned battle against Anglo-Saxon physics cannot be understood as a fight against the identification of models and reality, denying cognitive value to the models. Duhem never identified, but rather, he always underscored the distinction between Cartesian mechanism and that of Kelvin and Maxwell: in British mechanism, Duhem immediately recognized the model had only heuristic functions; it was a working tool for the physicist who needed to 'illustrate', satisfying the need of concrete interpretation (see Duhem 1893b). The fundamental charge against the British was not the fact that they used models, but their use of models in an incoherent way, conceiving them precisely as simple instruments. Even Laplacian physics was 'modelist', and yet, on repeated occasions, Duhem referred to it as one of the great examples of a theory in physics. Anglo-Saxon 'modelism', propagated in France by Poincaré, seemed to Duhem the most dangerous variant of instrumentalism, and he fought it by taking an explicitly anti-instrumentalistic position: if theories were simple classifying tools, it would be perfectly normal to adopt various criteria for different classifications, introducing incoherence in physics by using different models to represent the same object, as the British did. Incoherence, (i.e., British physics as supported by Poincaré) can be fought only by admitting that theories are classifying tools, being neither arbitrary nor subjective, but leaning rather not toward the construction of a 'natural classification', namely, one having objective significance.

Theoretical coherence obsessed Duhem's research. He sought the rigorous structuring of scientific terms in a deductive, hypothetical system which conceded nothing, in matters of rigor, to intuition or common sense. Generalized thermodynamics was the perfect answer to these requirements (and where it fell short, Duhem took great pains to make the necessary corrections). Anglo-Saxon 'modelism' instead proposed an uncoordinated physics, a gallery of images that, due to lack of coherence, could not be judged a theory. But coherence was sustainable and justifiable only by admitting that theories, inasmuch as they are constructed to organize mathematically the world of phenomena, are also capable of reflecting an ever-perfectible and always 'more perfect' real arrangement, rather than a subjective one. We know with certainty – according to Duhem in 1893 – that relations among material

substances are "neither undetermined nor contradictory". Therefore, when faced with physics proposing two irreconcilable theories:

> We are certain that the classification proposed by such physics is not in conformity with the natural order of 'the laws . . . making the incoherence disappear, we will have some probability of bringing it closer to that order to make it more natural, thus, more perfect . . . . (Duhem 1893c, pp. 369–70)

The idea of 'natural classification' was judged an extrinsic ideological addition, and even contradictory with Duhem's epistemology. Yet this idea was enunciated from 1893 as the methodological axis carrying the fight against British physics and in favor of generalized thermodynamics. Without it, all of Duhem's scientific work would be meaningless. Not only that, but the whole of his epistemology and his historical work was an effort to sustain this notion. The pivotal problem around which all of the *Théorie physique* hinges is just that: how to reconcile an unprejudiced, pitiless and extremely acute critique of the scientist's work with the idea of a science that has cognitive value. How does one criticize the dogmatic empiricism of positivism without falling into the subjectivity of instrumentalism? In order to understand why this problem had become so important in Duhem's eyes during the first few years of the new century, we should remember the genesis of the *Théorie*, and the framework within which it was generated.

During the years 1892–94, Duhem took up the fight against the basic positivist empirical notion of science and against Anglo-Saxon 'modelism', which was still encountering noticeable diffidence among the French. From those years to the year 1906, the year of publication of the *Théorie*, a number of riotous overlapping events considerably changed the French scientific and cultural scene. A series of upsetting experimental discoveries and an equally surprising sequence of theoretical elaborations (especially tied to Lorentz's theories) imposed a realistically interpreted atomic theory, together with Maxwell's electromagnetism, upon the younger French scientists such as Perrin and Langevin. These events diffused the theme of scientific crisis.

The victory of atomism and electromagnetism meant victory – or at least seemed to – for that modelism Duhem thought already defeated. These scientific events accompanied and even favored changes of great importance in the French philosophical panorama, which was characterized by the ever-increasing success of instrumentalistic, anti-intellectual,

and subjective concepts. Beginning in the 1890s, with the explosion of the celebrated debate on the 'bankruptcy of science', French philosophy was deeply marked by an impetuous blossoming of anti-intellectual currents, such as Bergsonianism and modernism, radical conventionalism, and varied forms of spiritualism. To use Fouillée's famous words, this period saw the "revolt of the heart against the intellect". Modelism and instrumentalism, English physics, atomism and exasperated conventionalism, the crisis of science, anti-materialism and spiritualistic skepticism seemed to form a thick web destined to surround and suffocate the model of scientific rationalism elaborated by Duhem in the 1890s.

To fight these foes, Duhem published a series of works in the early 1900s. The *Théorie* represents the ultimate battle of this campaign. In 1902, he criticized electromagnetic theory very harshly in *Les Théories électriques de J. Maxwell*. During the same year he attacked atomism with *Le mixte et la combinaison chimique*. In 1903, with *L'évolution de la mécanique*, he confronted the more generalized critique of the 'modelist' approach in its diverse historical variants. And finally, in 1904, Duhem started publishing for the *Revue de Philosophie* a series of articles which were eventually collected (with some additions) in 1906 to form *La Théorie physique*. Here he fought the conventionalism then in style on the epistemological level. The *Théorie physique* was, therefore, not at all a book opposing the positivism of the 1800s, in the name of the new century's revolution in physics. It was a work against the emerging novelties intended to demonstrate that the criticisms brought a decade earlier against the positivist conception of science need not give way to the skeptical conclusions that seemed to follow directly from these novelties.

An analysis of the text of the *Théorie* confirms the interpretation which has led me to give the history of this work's genesis. For reasons of space, it is impossible to carry out a detailed analysis here, but some indications may be given: on all the key problems of epistemology (what is a scientific fact? what is a law? how does one choose theories?), Duhem clearly takes a position critical of the main conventionalists, primarily Poincaré and Le Roy, and fights against their supposed solutions. I will briefly consider some examples.

Le Roy had given a rather strong subjectivist interpretation of the 'scientific fact', starting with the analyses made by Duhem in the 1890's and maintaining that, due to the ineliminable theoretical components

present in every experience, the 'scientific fact' is to a certain extent 'created' by the subject (Le Roy 1899, p. 516). Poincaré had retorted by trying to subdue this radical subjectivism, maintaining that what the scientist creates is the language with which we ask nature questions, and it is then nature's task to give the answers (Poincaré 1905, p. 266 et seq.).

Duhem argues at length even against Poincaré's mitigated version of conventionalism. The theme of science understood as a well-made language is certainly not new: from Condillac and Lavoisier, through the *Ideologues*, it had gone through positivism and had almost become commonplace. It was directly connected to a depreciation of theories, reducing them to the role of dictionaries which, through obviously conventional rules, allowed the scientist to translate experience into language. The view had, in fact, been emphasized by radical conventionalists like D'Adhémar (D'Adhémar 1904). Against these general positions Duhem emphasizes (Duhem 1906, p. 266) that science differs from other languages as to its terms, just because they are defined within a theoretical context, stabilizing multiple interconnections in a network of relationships between term and term, concept and concept, not to mention relationships among some terms and groups of phenomena. A scientific fact is not differentiated from a nonscientific fact only because it is expressed in a language resulting from customs known only by a small group of people (Poincaré's thesis). Its main characteristic is that of belonging, by virtue of the theories that we use to express it, to an intricate network of relationships with theoretical terms and with a multitude of other scientific facts. When we translate a raw fact into a scientific fact, we do not simply construct a proposition using the expressions of a language provided with conventional rules known by a small group of people (the scientists), we do much more. We insert that fact in a sequential scheme, including other facts, and we recognize relationships among phenomena. However, the linguistic translation of the raw fact to the scientific fact is not simply made by choosing the rules of translation freely and conventionally. It is guided by the theories allowed at a given historical moment, and the result of the translation work is, therefore, not invented by the scientist; it is the result of history. It depends upon the level that science has reached at a given historical moment. Science as a means of human expression is, in fact, a language, but a language radically different from all others.

Duhem contributed more than anybody else to the criticism of positivism's dogmatic concept of science's empirical basis. His analysis of the impossibility of crucial experiments is famous, but his criticism is not limited to the denial of the notion of the empirical basis of science. The distinction between the theoretical and the observational is unsustainable for Duhem from the logical point of view, because, in the mature sciences, every observation is impregnated by theories. Positivism taught (just as neopositivism would in the future) that such a distinction was logical, and therefore absolute. For an absolute distinction between theoretical physics and experimental physics, Duhem substituted an historical distinction: there exists at every historical moment the heritage of previous history; a body of 'trusted' theories which guarantees the experimental physicist the possibility of making 'observations' without having to doubt every concept used. Thus science may progress, constantly increasing the theories it trusts (the 'background knowledge' in modern terms). It makes statements which, from the logical point of view, are unavoidably 'theoretical'. They are theoretical at the moment they are proposed, but they become increasingly more 'experimental' as they are provided with increasingly more guarantees of their validity.

Even on the notion of scientific law Duhem's views are opposed to Poincaré, Milhaud, and especially Le Roy. His critique centered upon the typically conventionalist affirmation that laws are used as definitions: in the presence of an experiment which seems to falsify the law, we do not reject it, but we say that the present case does not fit those for which the law was defined, that there are upsetting causes that the conditions of the applicability of the law did not foresee. Experiments, therefore, will never be able to force us to reject the laws; they are not falsifiable (Le Roy 1899, p. 523). Evidently, this approach empties scientific laws of any empirical content. It makes the rigorously required comparison with experience useless for the development of science, since any problem may be resolved by exercising an inventive activity which saves a law by placing it beyond experience. For Duhem, the stern comparison, the refutation of experience, has a result which is only apparently identical to Le Roy's rescue of law thanks to the addition of new perturbing causes, but actually has diametrically opposed objectives to that of instrumentalism. For Duhem, in fact, when confronted with a denial of experience we save a law by specifying its conditions of validity, and that constitutes cognitive

progress. It is true, we use the law as a criterion to establish whether or not the conditions under which it is considered valid are respected; but it is experience which allows us to establish the conditions of validity. Not every refutation falsifies the law. Some give us information about its limits of validity, and this constitutes progress. Difficulties are not resolved in a clever, intellectual game of rescue. They are resolved in a symbolic representation by adhering more closely to reality. Each one of our laws is necessarily a poorer scheme than the reality it wants to represent. The failures, the falsifications, force us to refine the theoretical scheme and to complete it in order to "make it more suitable to represent reality" in an unending process of perfection (Duhem 1906, p. 285; 1954, p. 174). In this process it is the falsifying experience that teaches us the conditions of the law's validity and the restrictions to which the primitive terms of law are submitted. Experience is not an enemy from which, with more or less astute devices, one must seek protection, it is the source of the perfecting process of the theoretical scheme: "The necessity of these restrictions didn't appear at all in the beginning, it was imposed by experience." (Duhem 1906, p. 287; 1954, p. 176). The work of continuous minor repairs, through which the laws of physics avoid the denials of experience, does not have the function of saving a law by petrifying it into the limbo of conventionalism, but it plays an "essential role in the development of science" (Duhem 1906, p. 288; 1954, p. 176). What is important from Duhem's point of view is not the rescue of the law, but the progress of the theoretical scheme, which is realized in the attempt to resolve the issues raised by a falsifying experience:

> It is through the unending struggle of this work which continually completes the laws to the end of including exceptions, that physics is able to progress . . . it progresses because without interruption, experience is forever causing the explosion of new contradictions among the laws and the facts and without interruption, physicists rectify the laws so that they may represent facts more accurately. (Duhem 1906, pp. 289–90; 1954, p. 177)

Even the problem of choosing the basic hypotheses of a theory, which Poincaré and Le Roy had resolved in terms of a conventional choice, receives in the *Théorie* a solution that decidedly finds fault with the main lines of subjectivism. Moreover, it is just in this respect that the most important and significant variation between the articles of the 1890s and the book of 1906 should be considered. The problem of the choice of hypotheses had been dealt with in *Quelques réflexions au suiet*

des théories physiques (1892), in criticisms of the positivist dogmatism extolling the creative freedom of the researcher. In this article, Duhem had maintained that hypotheses are chosen freely and that choices are guided by subjective criteria, mostly that of simplicity. The *Théorie physique* repeats almost to the letter all of that article's criticisms of positivism, but the paragraph dedicated to the choice of hypotheses is completely ignored, together with all those passages containing rather excessive conventionalism!

In the *Théorie*, Duhem shifts the problem from the field of logic, of metahistorical methodological criteria, to the field of history. The subjective criteria of choice so dear to instrumentalistic conventionalism are no longer given space because Duhem is convinced of the fact that even if they are possible from the abstract point of view, the scientist, in reality, does not use them to make his own choices. He does not use them because in the concrete cases of historical evolution, the scientist does not make choices of any kind. The theory or hypothesis germinates within him without his concurrence. This means, in a less paradoxical form, that logical criteria are altogether insufficient to guide theory choice and, relying only upon them, the scientist would remain paralyzed in his progress by excessive freedom. This had been the objection which, in a ferocious attack, the neo-Thomist Vicaire had addressed to Duhem's article of 1892 (Vicaire 1893, p. 79). Duhem, in the following year had rectified his own position, maintaining in *L'école anglaise et les théories physiques* that in the choice of hypotheses, a scientist is never guided by logic alone:

> The particular inclination of his spirit, his prevailing faculties, the diffused doctrines in his environment, the tradition of his predecessors, the habits he has adopted, the education he has received, will be his guide, and all of these influences will be found again in the form of the theory he will conceive. (Duhem 1893b, p. 377)

In the *Théorie*, this idea is extensively developed and represents one of the basic theses of the whole work: the historical context, in which every scientist moves, guides the choice of hypotheses; these are the concrete influences that every stage of development of the historically determined scientific thought exerts upon the researcher, resulting in the generation of new ideas. These ideas are the product of all of the foregoing evolution, without which they could not be created; they are 'the last stage of a long development'. Since whoever contributes to scientific progress is so immersed in his contemporary historical context

that he cannot move freely, every new hypothesis can only be a modification of already-stated hypotheses. The history of science must be a continuous development (Duhem 1906, pp. 364 *et seq.* and 416 *et seq.*).

The thesis of historical continuity is one part of his epistemology with which Duhem attempts to resolve the problem of the choice of hypotheses. And it is a thesis which has a very important result: if in 1892, relying upon logical criteria of choice, such as simplicity, Duhem had not been able to avoid the acceptance of an instrumentalistic vision of science, now, leaving the problem of choice to the thesis of historical continuity he can sustain a realistic and cognitive vision of the scientific enterprise.

> Showing the physicist the continuing tradition through which science of every age is nourished by the previous century's systems, through which it is pregnant with the physics of the future, citing to the physicist the prophecies that theory has formulated and that experience has confirmed; it creates and strengthens in him the conviction that a physical theory is not at all a purely artificial system, useful today and useless tomorrow, that it is an always more natural classification; an always clearer reflection of reality that the experimental method could never bear in a face-to-face contemplation. (Duhem 1906, p. 445; 1954, p. 270)

What can be said then about the historiographical scheme, that makes Duhem (and conventionalism in general) the advocate of a vision of science proceeding on the basis of choice and decided by a criterion of simplicity? In my opinion, the only possible answer is that the scheme is completely wrong. Except for the paper of 1892, Duhem never admitted that simplicity could be a sufficient criterion for choice. Obviously, like every reasonable person, he considered the simplicity of a theory part of its merit, but he certainly didn't use it as a guideline.

Duhem's vast historiographical work clearly shows that the case of empirically equivalent rival theories is in his opinion extremely rare, and that for the vast majority of cases it is empirical factors that supply a clear criterion of nonsubjective choice. Great theoretical disputes were resolved, according to Duhem, by the superiority of one theory over a rival one in 'saving the phenomena', without any necessity at all to resort to criteria such as simplicity. For example, this is the case of the clash between Ptolemy and Copernicus. It is a very wretched historiographical thesis that, according to conventionalism, the heliocentric system won over the geocentric system because it was simpler. In *To Save the Phenomena*, where Duhem confronts the problem, the

sixteenth-century victory of the Copernican system, intended as a calculating instrument, is always explained on the basis of the greater precision that the system allowed in the construction of astronomical tables. There are times (although few) that simplicity is also mentioned along with precision, but this attribute never appears by itself. It is always an additional quality which certainly does no harm to the Copernican system, but that certainly can not explain its victory just for its sake. When commenting on sixteenth-century astronomers following Copernicus, from Reinhold to Peucer, from Schreckenfuchs and Piccolomini to Giuntini, Duhem uses the term of simplicity along with 'precision' only once, when referring to Reinhold. In all other cases, what is always and only considered is the calculating precision obtainable starting from the Copernican hypotheses. Duhem concludes his analysis of the victory of Copernicus's theory with the astronomers thus:

> The spirit of the greater part of the astronomers, during the 20 or 30 years following the publication of Copernicus' book is very clear. The work of the astronomer from Thorn attracts their attention very strongly because it appears to be suited for the construction of *precise* astronomical tables. (Duhem 1908, p. 509; 1969, p. 8)

In the first place, therefore, the criterion of simplicity turns out to be irrelevant and superfluous for the vast majority of theoretical disputes. But even when facing those cases where we are in the presence of equivalent empirical ranges in two rival theories, Duhem never considers simplicity a decisive element, capable of generating a choice endowed with any solidity. The only example of empirically equivalent theories contained in *Le système du monde* is made up of two different astronomical representations. Appolonius of Perga had proved epicycles and eccentrics to be equivalent with respect to observational effects; in 244 B.C., Hipparchus, when faced with this surprising discovery, refused to make a decision in favor of either representation. Now, in Duhem's opinion, this attitude was not due to the astronomer's uncertainties or inability but, on the contrary, it was the attitude of one who follows the correct scientific method; when confronted with theories equivalent from the observational point of view and in the absence of other references capable of guiding the choice (which could be other already accepted theories), he refuses to choose. Although using one theory because it is judged simpler, the astronomer does not condemn the other and does not discard it as a possible alternate tool (Duhem 1913–59, Vol. I, pp. 455–60). It is clear in this case that Duhem

does not consider simplicity the only criterion sufficient in founding a definitive theoretical choice. In other cases of empirically equivalent theories examined in detail by Duhem in his various works, the notion of simplicity is always relegated to the background. That is the case, for example, with the contrast between Lagrange's *Mécanique analytique* and Poisson's *Mécanique physique* where the fundamental criterion for evaluating the superiority of one over the other is seen in the relationship between the type of mathematics used and the model upon which such mathematics are applied (Duhem 1905, p. 83 *et seq.*).

The fact that the criterion of simplicity takes on a completely secondary historical role in Duhem should not surprise us, if we remember its previously stated position in the historical dynamics of the relationship between abstract and concrete, which is apparent in scientific laws. According to Duhem, the symbolic schemes produced by the scientist are always impoverished when compared to the reality they are supposed to symbolize. The modification of the theoretical scheme always happens through its increasing complication as it attempts to represent all the richness of the experience. This is also the case for theories: in Duhem's opinion historical progress generally creates an increasing complexity of theoretical physics and only in some particular cases (for example with Copernicus's theory) do we have a simplification which, in any case, cannot by itself justify the acceptance of a theory. At this point, consider once more *Le système du monde*. Here all of the ancient and medieval history of astronomy is recounted as a progressive increase of complexity in function, to create a better adaptation to observational data (Duhem 1913–59, vol. I, pp. 129 and 201). And in *To Save the Phenomena* Duhem advises that the complication of a theory cannot be considered the sole motive for rejecting it:

> The exact representation of celestial movements may force the astronomer to gradually complicate his hypotheses, but the complexity of the system where he will have stopped cannot be a reason to reject such system if it is in full accord with the observations. (Duhem 1908, p. 129; 1969, p. 17)

The most conclusive demonstration of how much Duhem considered scientific progress to be substantially characterized by an increase of complexity is found in considerations regarding the relationship between mechanics and general thermodynamics. Just because it is a more limited theory, capable of covering a lesser number of observational data compared to general thermodynamics, mechanics is much simpler.

Conversely, because it is a better-suited to phenomena, general thermodynamics or energetics is even more complex than the theory it wants to replace.

> The new mechanics founded on thermodynamics has not at all imposed upon its essential hypotheses the exaggerated simplicity required by the old mechanics: it has tolerated their being more numerous and more varied allowing them to express themselves with more complex formula. This greater amplitude left to the choice of principles proved to be a happy and fruitful one. (Duhem 1905, p. 343)

Here then, is a rather difficult historiographical problem for the supporters of Duhemiam simplicity: the new mechanics, that to which Duhem dedicated all of his work as a researcher, is considered more complex than the old mechanics. The revolution brought about to thermodynamics in the chemical and physical sciences proceeds from the simpler to the more complex!

In the *Théorie* Duhem intended to show how to avoid skepticism without abandoning any of the criticisms of dogmatic positivist empiricism he had made over a decade before. The attempt is rather risky: it is a question of constantly maintaining a balance on a metaphorical thread with the recurring risk of falling on the one side into dogmatism, on the other side into skepticism. It is clear that it is this second danger that Duhem fears most because, as a matter of fact, his juvenile theses had been interpreted as skeptical. And it is here that Duhem develops a constant and tight polemic against Mach, against Poincaré and against Le Roy. It is astounding how the critics have not taken into account these extremely clear Duhemian passages which represent the most lucid, articulate and effective polemic against conventionalism with a skeptical note. On all fundamental epistemological questions (what is experience? what is a law? what is a theory? what is the nature of science?), Duhem both reproposes and confirms his own juvenile theses; moreover, he is concerned to show how from those theses one doesn't necessarily have to reach the depreciative conclusions of his false friends. If all its pages are to be taken seriously, and not just those on the critique on positivist dogmatism which in 1906 were the most worn-out for Duhem's readers, the total vision of science that emerges from the *Théorie* is evidently a vision that is rather far from any trust in science void of criticism. But it is also a realistic conception of a science in constant movement, a science made by human beings, and as such always revisable. It proceeds by continuously retouching its

own conceptual schemes, modifying them, and generally complicating them, in view of an ever better adjustment between the scientific image of reality and reality itself, a reality that has certainly not been completely reached but that becomes always more approachable.

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