# PIERRE DUHEM ON INDUCTION AND INFERENCE IN SCIENTIFIC DISCOVERY[1]

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**Abstract:** This paper analyzes Pierre Duhem's position on the role of simple induction in scientific research. Duhem has been understood as a critic of induction in science but that is not entirely accurate. Duhem holds a position similar to one that might be recognized in traditional philosophy of science (according to which, although theories are not the result of inductions, some physical laws can be understood as inductive generalizations). Nonetheless, Duhem differs from that position in ways that have not been clearly recognized in secondary literature. The most important difference is that Duhem's distinction between inductive laws and theoretical hypotheses is not based on the kind of concepts we use but supported by a (mostly implicit) conception of scientific discovery. The idea is that there are two different processes of discovery distinguished by the use of different types of inference, of which induction is only one. Clarifying these distinctions can contribute to recent debates on the interpretation of Duhem's philosophical work in recent literature.

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## 1. Introduction

At the beginning of the 20th century, Pierre Duhem (1861-1916) criticized (what he called) the *Newtonian method*. This method consisted in the use of induction for the formulation of theories in physics. Duhem's rejection of the Newtonian method strongly contrasted with the position accepted by previous generations who even referred to physics as an inductive science. However, Duhem did not explicitly clarify what, if any, he considered the role of induction in scientific research to be. If it is not through induction that new theories are discovered, according to Duhem, does this mean that induction has no role in scientific research? Or rather, does it mean that it has a different role? At the outset, a careful reading of Duhem's texts shows that he repeatedly uses the term induction when talking about physics methodology. So, it seems he still considered induction important, although it is not entirely clear what he considered its role to be.[2]

Some historians and philosophers of science have noted the ambiguity of Duhem's position on induction and have tried to explain it. In this text we will look at their explanations in more detail, but it must be said from the outset that these philosophers have

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[2] There are various types of inductive inferences, however, Duhem seems to refer exclusively to *simple or enumerative induction*, i. e. to infer a general statement based on a series of observations and for which no counterexample has been found. In section 2, I will elaborate Duhem's conception of induction in more detail.

reached very different conclusions. Even worse, their conclusions seem insufficient. Anastasios Brenner (1990), for example, says that Duhem accepted induction in an early phase of his career but that he rejected it later. Nonetheless, there are books written by Duhem in his last years which still refer now and then to induction. Jules Vuillemin (1991), in contrast, argues that, for Duhem, only the laws of physics which had not been reduced to a more fundamental thermodynamic theory were inductive conclusions. Nonetheless, there are instances in which Duhem refers to laws of thermodynamics as inductive laws. More recent literature seems to avoid the issue entirely.[3]

The aim of this text is to answer the question: *What, according to Duhem, is the function of induction, if there is one, in scientific research?* Although this question may seem of limited interest, since it only seeks to clarify an apparently minor aspect of Duhem's philosophy, the answer is more far-reaching than it might seem. Among other things, the answer contributes to debates about other aspects of Duhem's philosophy that have been more discussed in recent literature (such as the interpretation of his concept of *good sense*). Furthermore, the issue is relevant to understanding Duhem's position on *scientific discovery*.

However, Duhem does not explicitly (or systematically) answer the question about the role of induction raised in the previous paragraph. Because of this, I will carefully analyze his comments on induction to get a conclusion about Duhem's position. So, first, section 2 analyzes Duhem's conception of induction and (what he called) the Newtonian method that he associated with it. Then, section 3 points out Duhem's arguments and quotes which

[3] Milena Ivanova's 2021 text, *Duhem and Holism*, for example, doesn't even mention the word 'induction.' The closest reference to induction is only one phrase which says "Duhem…is firmly opposed to inductivist schools.." (Ivanova 2021, 12).

reveal an ambivalent position on induction. The same section considers possible explanations for this double attitude that can be found in secondary literature and their shortcomings. Section 4 analyzes a plausible, but ultimately inadequate, explanation of Duhem's position that relies on the distinctions he drew between terms used in common sense and those terms used in physics. Then, in section 5, I will argue that a better way to understand Duhem is to consider the similarities and influences from other physics of the time, specifically those of Hermann von Helmholtz. Section 6, also considers the influences of William John Macquorn Rankine. These last two sections will allow to, then, make the case in favor of a particular interpretation of Duhem which provides a coherent account of his comments on induction. Under this interpretation Duhem distinguishes two levels of abstraction in physics and induction plays a role in only one of them. The distinction between these two levels of abstraction will be further elaborated in section 7. Finally, section 8 will highlight some parallelism between Duhem's ideas (as reconstructed in previous sections) and those of the philosopher Charles Sanders Peirce (1839-1914). The comparison will help clarify what Duhem was probably trying to do by limiting the role of induction. It will also show how the conclusions might contribute to current debates on interpreting Duhem's concept of *good sense*.

## 2. Duhem's understanding of induction and the Newtonian method

Before trying to explain Duhem's position it is important to clarify what he understood by 'induction.' Although he did not explain his definition in a systematic and detailed manner, we can discern it through his comments dispersed in several of his texts.

First, and foremost, Duhem associates induction with, what he calls, the *Newtonian method*. In *The Aim and Structure of Physical Theory* (hereinafter *Aim and Structure*), Duhem describes that method as stating that all the principles of a theory should be either "drawn from observation by the sole use of those two intellectual operations called induction and generalization, or else a corollary mathematically deduced from such laws" (Duhem 1906/1954, 190-191). He also states that the "General Scholium" of Newton's *Principia* prescribes this, therefore the name Newtonian method.[4]

In the "General Scholium" Newton describes induction as an operation that begins with phenomena and ends with propositions, which he distinguishes from hypothesis. He states that: "...I frame no hypotheses; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction" (Newton 1846, 506-507). Therefore, the Newtonian method rejects the use of hypotheses in physics, characterizing them as conceptions that are not inferred from phenomena and generalized through induction.

In contrast to Newton, Duhem (1906/1954, 20) does consider that theories are based on hypotheses. Duhem conceived scientific theories as *deductive* systems and claimed, contrary to what the Newtonian method holds, that the principles of deduction in these systems are *not* arrived at by induction: "These principles may be called 'hypotheses' in the etymological sense of the word for they are truly the grounds on which the theory will be built…" (Duhem 1906/1954, 20). The only requisites that these hypotheses must comply with, according to Duhem (1906/1954, 220), are logical consistency and making it possible to deduce correct *experimental laws* from them.

[4] There has been some criticism of Duhem's reading of Newton. A critical review of this criticism can be found, for instance, in Eduardo Salles O. Barra and Ricardo Batista dos Santos (2017). In any case, what matters here is not Duhem's accuracy in reading Newton but what his interpretation tells us about his conception of induction.

Newton referred to ideas that were not derived from phenomena through induction as hypothesis; however, Duhem (1906/1954, 20) understood hypotheses in their 'etymological sense,' that is, as the foundational principles from which theories are deduced. In Duhem's definition, there is no necessary contrast between hypotheses and propositions inferred through induction. Hypotheses could be the result of induction or some other process; what defines them is their function as the foundation of theories. Yet, Duhem does reject the Newtonian method arguing that "...it is impossible to construct a theory by a purely inductive [i.e. Newtonian] method." (Duhem 1906/1954, 219).

Duhem takes care to define his concept of 'hypothesis', possibly because he himself recognizes the difference between his usage and Newton's. However, the fact that he does not dwell on the term 'induction' suggests that he accepts the way in which he understands Newton to use that term. That is, Duhem shares the idea that induction is an operation that starts with phenomena and ends with a general proposition. This conclusion can be supported by Duhem's own comments. When commenting on the 'General Scholium', Duhem notes that... "he [Newton] rejected so vigorously as outside of natural philosophy any hypothesis that induction did not extract from experiment; when he asserted that in a sound physics every proposition should be drawn from phenomena and generalized by induction" (Duhem 1906/1954, 191).[5]

In short, Duhem understands induction as a method of generalization that begins with the study of 'phenomena' or 'experiments' and culminates in a general proposition.

[5] In some cases, Duhem seems to distinguish between generalization and induction. "It was also induction and generalization that led James Thomson to conceive the idea of the theoretical isotherm" (Duhem 1906/1954, 94-95). However, in other instances, as in the quote at the end of the previous paragraph, Duhem speaks of 'generalizing by induction.' The difference between these quotes seems to be merely a stylistic variation; in any case, induction is part of a generalization process.

This is most clearly seen in the first part of *Aim and Structure*, where Duhem states that, according to the Newtonian method "The attentive study of phenomena and their laws permits the physicist to discover by the inductive method appropriate to his science some of the very general principles from which experimental laws may be deduced" (Duhem 1906/1954, 47).

Regarding how generalization occurs in inductive reasoning, Duhem states that it requires logical consistency between the observed phenomena and the general proposition derived from them. Part of his argument against the Newtonian method rests on this principle of logical consistency. Duhem argues that Newton claimed to have derived the law of universal gravitation from Kepler's laws through induction, but that this claim is false. "The principle of universal gravity, very far from being derivable by generalization and induction from the observational laws of Kepler, formally contradicts these laws" (1906/1954, 193). In other words, the evidence that Duhem presents to refute the idea that the principle of universal gravitation is the result of induction from Kepler's laws is precisely the fact that there are contradictions. For Duhem, an inductive argument requires logical consistency between the general statement derived and the statements describing the specific phenomena that lead to that general statement.

However, the requirement of logical consistency between the observed phenomena and the resulting proposition tells us relatively little about how to perform an inductive reasoning process. There are several ways to formulate general propositions that are consistent with particular observations. Furthermore, with this requirement alone, one could argue that hypotheses, as understood by Duhem, are also forms of induction, since, according to him (1906/1954, 219-220), they too must be logically consistent, and more specific consequences must be deducible from them. Nevertheless, in rejecting the idea that the principle of universal gravitation can be derived inductively from Kepler's laws, Duhem also points to another reason: "Can induction derive it [the principle of universal gravitation] from these two statements [Kepler's laws]? Not at all. In fact, *not only is it more general than these two statements and unlike them*, but it contradicts them" (Duhem 1906/1954, 193, emphasis added). In addition to the contradiction, the principle of universal gravitation cannot be an inductive conclusion derived from Kepler's laws because it is more general. In other words, Duhem seems to believe that, in an inductive argument, the resulting proposition should be of the same level of generality as the instances upon which it is based.

It is difficult to understand Duhem's requirement regarding the level of generality in induction, since generalization is precisely the goal of this process. How then can we understand Duhem's criticism that the principle of gravitation is more general than Kepler's laws and, therefore, is not the result of induction, if induction itself is a means of generalization? Duhem's requirement only makes sense if we assume that the general proposition obtained through induction should only encompass the specific type of instances from which it was formulated. In other words, for Duhem, a genuine induction based on Kepler's laws would result in a proposition that could *only* be applied to planetary orbits and not to other phenomena. The principle of universal gravitation, however, can be applied to all kinds of objects with mass, not just planetary orbits, and consequently, Duhem considers it to be something more than a mere induction. As Eduardo Salles O. Barra and Ricardo Batista dos Santos point out: "For Duhem, the result of mutual attraction between *all bodies* can definitely not be derived from the Kepler's laws" (O. Barra and Batista dos Santos 2017, 9, emphasis added).

That Duhem believed that induction does not add new information is evident in his description of the Newtonian method; he points out that this method is often considered free from speculation: "A theory based on such [inductive] hypotheses would then not present anything arbitrary or doubtful; it would deserve all the confidence merited by the faculties which serve us in formulating natural laws. It was this sort of physical theory that Newton had in mind…" (Duhem 1906/1954, 190-191). By limiting itself to expressing only what is already known, induction preserves the certainty and confidence we already have in our observations, but, for the same reason, it is too limited and rigid for the development of a physical theory. Nonetheless, as we will see, Duhem still attributes an important function to induction, although it is not easy to identify which.

**3. Duhem's ambivalent position on induction**

In *Aim and Structure*, Duhem argues against the *Newtonian method* stating that it is impracticable: "That is because *two inevitable rocky reefs make the purely inductive course impracticable for the physicist*. In the first place, no *experimental law* can serve the theorist before it has undergone an interpretation transforming it into a *symbolic law*; and this interpretation implies adherence to a whole set of theories. In the second place, no *experimental law* is exact but only approximate, and is therefore susceptible to an infinity of distinct *symbolic translations*; and among all these *translations* the physicist has to choose one which will provide him with a fruitful *hypothesis* without his choice being guided by experiment at all" (Duhem 1906/1954, 199, emphasis added).

To understand his argument, it is important to consider that Duhem conceived scientific theories as *deductive* systems. He claimed, contrary to what the Newtonian method holds, that the principles of deduction in these systems are *not* arrived at by induction. Duhem rejects the Newtonian method concluding that "...it is impossible to construct a theory by a purely inductive [i.e. Newtonian] method." (Duhem 1906/1954, 219).

Duhem's criticism of the *Newtonian method* suggests that he completely rejects the use of induction in physics. Larry Laudan (1981) even associates this rejection of induction with the abandonment of the search for a *logic of scientific discovery*. Laudan claims that, until the mid-nineteenth century, it was thought possible to formulate clear rules for the inductive processes leading to the discovery of new theories, i.e. a *logic of scientific discovery*. Nevertheless, "by the last half of the 19th century, this enterprise was dead, unambiguously repudiated by such philosophers of science as Peirce, Jevons, Mach, and Duhem" (Laudan 1981, 183). However, more recent authors have shown that, in fact, the late nineteenth-century changes in how to understand scientific logic were more complex than Laudan suggests. Krist Vaesen (2021), for example, points out that several late-nineteenth-century French authors remained deeply interested in discovery processes

Moreover, in the same book in which Duhem rejects the Newtonian method, *Aim and Structure*, he explicitly points out several scientific conclusions as inductions. For example, when mentioning the studies on osmotic pressure, he claims that "…experimental *induction* furnished Raoult with the laws necessary for the progress of the new doctrine." (Duhem 1906/1954, 95, emphasis added). These kinds of statements show that Duhem still considered that there was an important role in physics for induction.

Anastasios Brenner (1990) explains Duhem's references to induction by arguing that Duhem accepted the inductive (Newtonian) method early in his career but later abandoned it. In an early text (from 1892), entitled "Quelques réflexions au sujet des théories physiques," Duhem says that "The mind arrives at the understanding of *experimental laws* through induction, transforming the facts it has come to understand" (Duhem 1892/1996, 1, italics in the original). In this 1892 text, Duhem also argues that some experimental laws can be used as principles of deduction for theories. However, even here he considered building theories by induction to be an impracticable ideal: "Physics presents to us several theories that approach this ideal more or less. It does not offer us any theories that realize it completely" (Duhem 1892/1996, 6).

Brenner (1990) claims that, when Duhem wrote the 1892 text, he had not yet developed his critique of the inductive method. Brenner interprets Duhem's comment that no theory is completely inductive as an assertion that only the ideal theory would be inductive, although not the theories we actually have. Brenner claims also that it was only when Duhem developed his other theses (the critique of the idea of crucial experiments and that of the holistic nature of confirmation) that he completely rejected the inductive method. This would have been around the time of the publication of *Aim and Structure* (1906). However, Duhem's theses that supposedly led him to reject the inductive method, the holistic nature of confirmation and the critique of crucial experiments, are all present in an 1894 text.[6] And even in *Aim and Structure*, as we have seen, Duhem still gives examples of results in physics that he considers inductive conclusions. It may be suggested that such comments in *Aim and Structure* are a consequence of Duhem reusing previous texts when writing his book.[7] But he continued to describe some laws of physics as inductive conclusions even in later works. In *Le système du monde* (1913), for instance, Duhem expresses admiration for the 14th-century philosopher Jean Buridan: "Avec une grande netteté, avec une grande précision, Buridan nous a décrit sa méthode philosophique…elle se bornera donc à procéder a posteriori, à établir *par induction des lois d'origine expérimentale*, à combiner des hypothèses dont nous nous déclarerons satisfaits lorsqu'elles auront, le plus simplement possible, sauvé toutes les apparences." ["With great clarity, with

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6 This text is listed in the references as Duhem (1894/1996).

7 In fact, Martin (1991, 104) points out that *Aim and Structure* was written, precisely, from previous material. Specifically, Duhem (1893b/1996) and (1894/1996) are the articles that already contain a good part of the arguments of *Aim and Structure*, in some cases, almost word for word.

great precision, Buridan has described his philosophical method to us...he will therefore limit himself to proceeding a posteriori, to establishing *by induction laws of experimental origin*, to combining hypotheses with which we will declare ourselves satisfied when they have saved, as simply as possible, all appearances”] (Duhem, 1913/1954, 728-729, emphasis added). Explicitly, Duhem continues to maintain that it is correct to establish laws by induction.

Jules Vuillemin (1991, XXII-XXIII), on the other hand, notes that Duhem refers to Kepler’s laws as the result of inductions and argues that, for him, only some laws of celestial mechanics are initially the result of inductions. His argument is that, since Duhem supported a position known as *energetics*, according to which the most fundamental area of physics is thermodynamics, all the laws can be reconstructed as derived from thermodynamics. Thus, laws such as Kepler’s would only be seen as the result of inductions before being formally reconstructed as consequences of non-inductive thermodynamic theories of physics.

However, Duhem gives examples of thermodynamic laws that he describes as inductive conclusions, which make Vuillemin’s argument questionable. This is seen when Duhem comments on discoveries made in thermodynamics: “The idea of a continuity between the liquid and gaseous states was presented to the mind of Andrews by experimental *induction*. It was also *induction* and generalization that led James Thomson to conceive the idea of the *theoretical isotherm*” (Duhem 1906/1954, 94-95, emphasis added). Both the work of Thomas Andrews (1813-1885) and that of James Thomson (1822-1892) had been in thermodynamics. Furthermore, Duhem’s *energetics* did not involve the reductionist project that Vuillemin attributes to him. As Paul Needham (2002, XX) points out, Duhem held that both the concepts of mechanics and those of thermodynamics should be taken as fundamental, without attempting to reduce one to the other.

Finally, Roger Ariew (1984 and 2022) notes that “Duhem clearly indicates that some sciences can use the Newtonian method, derive their principles by induction from observation and disconfirm them as tests warrant. What Duhem thinks is wrong with inductivism is that it cannot be applied to all sciences” (Ariew 1984, 320). This clearly cannot explain Duhem’s comments that we have seen. In them, Duhem refers to several results *in physics* as inductions.

## 4. Scientific language and Duhem’s distinction between experimental and symbolic laws

Duhem rejects the Newtonian method arguing that “no experimental law can serve the theorist before it has undergone an interpretation transforming it into a symbolic law; and this interpretation implies adherence to a whole set of theories” (Duhem 1906/1954, 199). This suggests that experimental laws are actually inductive conclusions, but that they cannot be used in a theory, only symbolic laws. So, the first question here is: what does he mean exactly by experimental laws and symbolic laws? Duhem does not explicitly define these concepts.

One possible way to understand Duhem’s distinction between laws, which seems natural at first glance nowadays but (as we will see) incorrect, is related to scientific language. The idea is that the distinction between experimental and symbolic laws derives from another distinction that Duhem maintains between the language of physics (which he calls symbolic) and the language of common sense (what we today might call natural language). Duhem considered that the terms of common-sense language refer to objects that we recognize through our senses. In contrast, the terms of the symbolic language of physics are magnitudes defined with the help of theories and measuring instruments.

Duhem's language distinction has been understood in philosophy of science as the distinction between 'observable terms' versus 'theoretical/unobservable terms.' However, the names of these categories might lend themselves to confusion, since it is suggested that theoretical terms represent hidden entities (such as atoms). Nonetheless, the distinction, as proposed by Duhem, is not related to what we can see and what we cannot. Duhem (1906/1954, 146) considers, for example, the volume of a macroscopic object as a theoretical term. The distinction is more related to the way we define and use the terms. When attributing volume to a gas, for example, we use measuring instruments and theoretical assumptions about how these instruments work. In contrast, Duhem considers that we do not need elaborate theories or precise instruments to recognize referents of terms such as 'horse.' "Just as the laws of common sense are based on the observation of facts by natural means to man, so the laws of physics are based on the results of physical experiments" (Duehm 1906/1954, 165). Theoretical (symbolic) terms do not refer to unobservable entities, but to precise measurable magnitudes.

In *Aim and Structure*, Duhem speaks of common-sense laws that are stated in terms of common-sense language. An example of a common-sense law is: 'all men are mortal'. This law associates two abstract terms: 'man' and 'mortality'. "But these abstractions are in no way theoretical symbols, for they merely extract what is universal in each of the particular cases to which the law applies" (Duhem, 1906/1954, 165). We use these terms to refer to objects that we recognize with our senses: "The abstract terms referred to in a common-sense law being no more than whatever is general in the concretely observed objects, the transition from the concrete to the abstract is made in such a necessary and spontaneous operation that it remains unconscious" (Duhem 1906 /1954, 166). This spontaneous transition from concrete facts to abstract terms might be understood as an induction. Aristotle, whom Duhem had studied carefully, refers to induction precisely as a process in which multiple perceptions of something eventually give rise to the perception of a universal in the mind.[8]

In contrast to common sense terms, Duhem considers the symbolic terms of physics to be abstractions that relate to facts only indirectly, through measurement operations. Such terms are symbolic because they require theoretical interpretation: "The symbolic terms connected by a law of physics are, on the other hand, not the sort of abstractions that emerge spontaneously from concrete reality; they are abstractions produced by slow, complicated, and conscious work, i.e., the secular labor which has elaborated physical theories" (Duhem 1906/1954, 167). The process of formulating symbolic terms is different from that of formulating common-sense terms; unlike the latter, the former is consciously controlled.

As we saw, Duhem argues that the Newtonian method is impracticable because "no experimental law can serve the theorist before it has undergone an interpretation transforming it into a symbolic law; and this interpretation implies adherence to a whole set of theories" (Duhem 1906/1954, 199). This suggests that experimental laws are indeed the result of inductions, but that only symbolic laws can be part of the theory. And, since Duhem contrasts experimental law with symbolic law, we could assume that the former is a common-sense law, that is, stated in common-sense terms. This would explain why Duhem considers experimental laws to be the result of inductions but that they cannot be used in physical theory. At least, the common sense terms in an experimental law would have to be replaced with theoretical (symbolic) terms. However, *this does not seem to be what Duhem maintains*. It is implausible that he equated *experimental* law and common-sense law, since Duhem himself (1906/1954, 182-183) emphasizes that experiments involve the use of a

8 Aristotle, *Posterior Analytics*, trans. and ed. Jonathan Barnes, 2nd ed. (Oxford: Oxford University Press, 1984), II.19, 100a15–100b5.

variety of theories.[9] Duhem (1906/1954, 165-168) describes common-sense laws as imprecise expressions used in everyday situations, whereas experimental laws are established in precise experiments. Duhem also argues that symbolic laws cannot be the product of inductions because they result from theoretical 'translations.' Induction allows for generalization, but it does not select the correct theoretical translation; thus "...it is impossible to construct a theory by a purely inductive method." (Duhem 1906/1954, 219).

**5. Experimental laws in historical context: the influence of Helmholtz**

As we saw, Duhem distinguishes between experimental and symbolic laws, such that only the former are inductive conclusions. He suggests that if a law is the result of induction, then it has not received a theoretical interpretation. But he also mentions laws obtained by induction formulated in theoretical terms. For example, Duhem (1906/1954, 94-95) mentions that experimental *induction* led James Thomson to conceive the "*theoretical* isotherm." In other words, there seems to be a problematic ambiguity in what Duhem calls experimental law. On the one hand, experimental laws seem to be laws obtained by experiments and formulated in theoretical terms and, on the other hand, Duhem says that such laws cannot be used in a theory because they have not received a "theoretical interpretation."

Part of the difficulty in understanding Duhem's concept of experimental law derives from the association that has traditionally been maintained between the law/theory distinction and the observable/unobservable distinction. Authors such as Ernest Nagel (1961, 79-80) claimed that the distinction between laws consists in the type of terms used; Laws state relationships between observable terms, whereas laws that use unobservable

9 This is also emphasized in Duhem (1894/1996).

terms are "'theoretical laws' (or simply 'theories')."[10] In Duhem's terminology, Nagel's description would imply that experimental laws are common-sense laws. However, as we saw at the end of the previous section, that is hardly Duhem's position.

A better way to make sense of Duhem's position is to consider the historical influence of other authors on his work. In particular, Duhem took strong interest in the work of Hermann von Helmholtz (1821-1894). The importance of Helmholtz for Duhem is shown, for instance, by the utmost respect Duhem expresses for Helmholtz, even though Duhem was a harsh critic of German science.[11] Moreover, Duhem (1902/2015) defended Helmholtz's version of electromagnetic theory against that of James Clerk Maxwell (1831-1879). According to Alan Aversa, "One reason Duhem preferred Helmholtz's electromagnetic theory over that of others…is because…Helmholtz's approach to electromagnetism was to classify the unique interaction energies between the various combinations of charged and current-carrying 'laboratory objects.' Thus, Helmholtz explicitly classified experimental laws, forming a true theory in the Duhemian sense" (Aversa 2015, ix). In other words, it seems that Duhem considered Helmholtz's approach as particularly representing key characteristics of his own position regarding experimental laws.

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[10] The distinction Nagel was trying to explain was between laws and theories, since what he calls "theoretical laws" are just theories. Some philosophers, such as Craig Dilworth, have noted that "The question of the difference between scientific laws and theories has received remarkably little attention in the philosophy of science" (Dilworth 2007, 176). The distinction, however, seems to have been more important to Duhem than has traditionally been considered.

[11] Duhem's book *German Science* contains strong criticism of German scientists but even there he says: "…which of the reproaches that we have addressed to German science could be applied to a Clausius or a Helmholtz? There is a school among the masters beyond the Rhine in which you can place your complete confidence" (Duhem 1915/1991, 68).

Jed Z. Buchwald (1993, 1994) analyzes Helmholtz's approach to electromagnetism and argues that, in contrast with other contemporary approaches, Helmholtz looked to establish a 'taxonomy of interactions' between laboratory objects. Other physicists of the time sought to explain electromagnetic phenomena by the action of abstract entities to which the interactions of laboratory objects were reduced. While Wilhelm Weber (1804-1891) and Gustav Fechner (1801-1887), for example, explained electromagnetic phenomena through the action of hypothetical electric particles, Michael Faraday (1791-1867) and Maxwell explained them through the states of the space (or *field*). However: "According to Helmholtz, objects in the laboratory remained entities in their own right. A 'charged' object differed from an 'uncharged' one by acquiring a condition or state that it did not previously have in relation to other objects that were also charged. Helmholtz viewed electromagnetic interactions–indeed, all interactions–as instantaneous and bipartite, and he held the nature of the interaction to depend upon the simultaneous states of the interacting objects, which themselves were given directly in the laboratory" (Buchwald 1993, 339).

According to Buchwald, for Helmholtz, the objects in the laboratory and their interactions are considered primary, without speculating about any underlying entities that might govern these interactions. Helmholtz focuses solely on understanding the different ways in which these objects can interact. "Thus for Helmholtz the goal of physics (and not just electrodynamics) was to discover what states objects could have and what the possible forms of interaction energies could be–to establish, that is, a taxonomy of interactions" (Buchwald 1993, 341). This taxonomy of interactions is what Aversa (2015) identifies as the concept that Duhem would have considered a true theory, that is, as a classification of experimental laws. The first part of *Aim and Structure* aims precisely to establish that the goal of physical theory is to classify experimental laws.

Naturally, Helmholtz's approach was not limited to simply reporting data without any theoretical speculation, nor did he intend to do so. His taxonomy of interactions explicitly incorporated abstract principles, such as the principle of conservation of energy. Nevertheless, a key aspect of Helmholtz's approach was his rejection of models that attempted to explain phenomena through speculative mechanisms. "The Helmholtzian scheme, which places strictures on the relationships between objects and which suggests how to probe those relationships, is extraordinarily weak if it is considered to be a theory in the usual sense…On the other hand, Helmholtzians benefited enormously from the comparative freedom from constraint that this very abjuration of models gave them, for they were at liberty to conceive new states and new interaction energies when dealing with novel, unfamiliar laboratory situations" (Buchwald 1993, 341).[12] Abstract theoretical principles were used to classify the interactions observed in the laboratory and to suggest possible relationships that had not yet been confirmed.[13]

Duhem's acceptance and defense of Helmholtz's electrodynamics suggests that his conception of experimental laws was similar. Experimental laws consist of establishing relationships between objects in the laboratory. As we have already seen, Duhem suggests that experimental laws are inductive, which is also consistent with Helmholtz's position, who considered that: "All knowledge of natural laws is inductive" (Helmholtz 1892/1995,

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[12] The rejection of models was consistent with Duhem's own philosophy, as can be seen in Chapter IV of the first part of *Aim and Structure*.

[13] Even though Buchwald (1993) does not explicitly talk about Duhem when explaining Helmholtz's approach to electrodynamics, it is notable that he and Duhem refer to the same Helmholtz's articles. Buchwald (1993, 339) points out that Helmholtz position can be reconstructed from articles like "Ueber die Bewegungsgleichungen der Elektrodynamik für ruhende leitende Körper." Duhem (1902/2015, 173), for his part, mentions "the most beautiful works of Helmholtz," specifically referring, in a footnote, to the same article.

409). Moreover, the fact that, as we saw early, Buchwald refers to the 'Helmholtzian scheme' as 'extraordinarily weak if it is considered to be a theory in the usual sense' fits well with Duhem's narrower definition of induction (as we saw in section 2). However, for Duhem, there is an important distinction between experimental laws and theory. This is particularly evident in his critique of one of Helmholtz's most important followers: Heinrich Hertz (1857-1894).

Like Helmholtz, Hertz carefully studied electrodynamic theory, especially the different versions of Maxwell's theory. Hertz believed that different conceptions of Maxwell theory share some content in common and that this content was the definition of the theory. "To the question, 'What is Maxwell's theory ?' I know of no shorter or more definite answer than the following: Maxwell's theory is Maxwell's system of equations" (Hertz 1893, 21). Duhem strongly criticized this idea, pointing out that two theories can share the same equations and yet still be different theories: "...it is not sufficient that the equations that they propose be literally identical; it is also necessary that the letters contained in these equations represent quantities related in the same way to measurable quantities, and to ensure this last characteristic, it does not suffice to compare the equations. We must compare the reasonings and hypotheses that constitute both theories" (Duhem 1902/2015, 172).

For Duhem, we need the hypotheses that constitute a theory in order to connect concepts with measurable quantities. However, as we have seen, he clearly rejects the idea that these hypotheses are the result of inductive reasoning. Nevertheless, if experimental laws merely describe interactions between objects, they can be inductive, even if the concepts or measurable quantities they use are not themselves derived inductively. When doing an experiment, we can observe dependency relations between magnitudes, such as changes in the volume of a gas due to variations in pressure. We use the theoretical terms *volume* and *pressure*, as well as experimental observations, to state a law that indicates how changes in one theoretical term affect the other. Yet, when a law is discovered in this way, although it is formulated in theoretical terms, it is not initially part of a defined deductive system based on hypotheses (which Duhem calls a theory). To achieve this requires another stage of development.

**6. The influence of Rankine and the 'theoretical interpretation'**

According to Duhem, the construction of a physical theory requires (at least) two stages. This can also be established considering the historical influence of other authors on his work. Duhem himself refers to William John Macquorn Rankine (1820-1872) when explaining his conception of physical theory. In *Aim and Structure*, Duhem quotes Rankine (1855/1881, 209)'s distinction between what the later called 'abstractive theory' and 'hypothetical theory.' Both kinds of theories were supposed, by Rankine, to be representations of natural phenomena, and that would be archived in two stages:

> …the first stage consists in observing the relations of phenomena, whether of such as occur in the ordinary course of nature, or of such as are artificially produced in experimental investigations, and in expressing the relations so observed by propositions called formal laws. The second stage consists in reducing the formal laws of an entire class of phenomena to the form of a science; that is to say, in discovering the most simple system of principles, from which all the formal laws of the class of phenomena can be deduced as consequences. Such a system of principles, with its consequences methodically deduced, constitutes the *physical theory* of a class of phenomena.(Rankine 1855/1881, 209, emphasis in the original)

The distinction between 'abstractive theory' and 'hypothetical theory' refers to the different ways in which the second stage, the 'discovering of simple systems of principles,' is achieved. 'Hypothetical theories' use arbitrary assumptions as principles. Rankine criticized this type of theory as inadequate or, at most, provisional. Instead, he argued that it was best to develop theories solely through ‘abstractions’ of formal laws: “So shall we arrive at a body of principles, applicable to physical phenomena in general, and which, being framed by *induction* from facts alone, will be free from the uncertainty which must always attach, even to those mechanical hypotheses whose consequences are most fully confirmed by experiment” (Rankine 1855/1881, 213, emphasis added).

Chapter III (first part) of *Aim and Structure* ends with Rankine distinctions and Duhem’s comments about the proliferation of ‘hypothetical theories’ at the end of the nineteenth century. Given Duhem’s criticism of the use of induction to formulate hypotheses, we would expect a harsh criticism of Rankine. However, far from dismissing Rankine’s position, Duhem takes up his ideas in Chapter IV. Duhem begins that chapter stating that:

> The constitution of any physical theory results from the two-fold work of abstraction and generalization. In the first place, the mind analyzes an enormous number of concrete, diverse, complicated, particular facts, and summarizes what is common and essential to them in a law...In the second place, the mind contemplates a whole group of laws; for this group it substitutes a very small number of extremely general judgments, referring to some very abstract ideas; it chooses these primary properties and formulates these fundamental hypotheses in such a way that all the laws belonging to the group studied can be derived by deduction that is very lengthy perhaps, but very sure. This system of hypotheses and deducible consequences, a work of abstraction, generalization, and deduction, constitutes a physical theory in our definition; it surely merits the epithet Rankine used to designate it: abstractive theory. (Duhem 1906/1954, 55)

The same distinction between two stages that Rankine points out is restated by Duhem. However, there is at least a key difference between Duhem and Rankine. Duhem seems to erase Rankine’s distinction between abstractive and hypothetical theories. Duhem says that, in the second stage, we formulate a system of hypotheses which constitutes a physical theory and, nonetheless, it is also an abstractive theory. Later, in *Aim and Structure*, Duhem argues that the hypotheses in which the physical theory is based are not reached by induction. But the same criticism is not evident regarding laws.

As we saw at the beginning of the last section there seems to be an ambiguity regarding Duhem's experimental laws. On the one hand, experimental laws seem to be laws obtained by experiments and formulated in theoretical terms and, on the other hand, Duhem says that such laws cannot be used in a theory because they have not received a "theoretical interpretation." Also, it is suggested that experimental laws are the result of inductions. *I contend that, for Duhem, the theoretical interpretation of a law does not consist in replacing the terms in which it is formulated, rather it corresponds to the achievement of the second stage of theory development, i. e, in incorporating the law into a deductive system (i.e. into a theory).*

By "incorporating an experimental law into a deductive system" I mean showing that the law can be deduced from a defined set of hypotheses. In case the hypotheses available to us are insufficient to produce such a deduction, the incorporation can be achieved by modifying the experimental law in question. This necessity to modify the law in order to make it deducible from a given set of hypotheses would explain Duhem's use of the term 'translation.' In his discussion of the concept of 'free fall', for instance, Duhem (1906/1954, 208-212) states that the modifications must be such that either they do not introduce alterations greater than the limits of precision available, or the theory itself explains why these alterations are observed (e.g. by pointing out sources of error).

For Duhem, then, the distinction between experimental and symbolic law does not depend on the kind of terms they use. The distinction between common-sense and theoretical terms only distinguishes everyday statements from statements of physics. But that distinction between terms is no longer relevant to distinguish between experimental and symbolic laws. The experimental law is not a common-sense law, since it is formulated by experiments and theoretical terms; but it is not a symbolic law either, since it has not been incorporated into a defined deductive system. Translating an experimental law into a symbolic law would mean, for Duhem, its incorporation into a particular deductive system (or theory).

The interpretation proposed here is consistent with Duhem's article entitled "Some Reflections on the Subject of Experimental Physics" (1894). The first part of this text, explicitly devoted to experimental physics, analyzes experimental physics and the laws it produces, contrasting them with those of common sense. The second part of the same text, devoted to theoretical physics, analyzes the laws of physics in contrast, also with those of common sense. This already suggests that Duhem distinguished both experimental laws and those of physics from the laws of common sense. At the same time, it implies that he considered there to be a difference between the laws produced by experiments and those of theoretical physics properly speaking.

However, there are some apparent difficulties with interpreting Duhem's concept of experimental law in this way, as a 'law established by experiments and theoretical terms, but not yet incorporated into a deductive system'. First, it now seems less clear why an experimental law cannot be used directly in a theoretical system. If experimental laws were formulated in common-sense terms, it would be obvious that they cannot be used in theory until they are replaced by theoretical terms. But if experimental laws are already formulated in theoretical terms, it does not seem clear what prevents us from using them. I will address this issue in the next section. Second, the relevance of the distinction between experimental and symbolic law seems questionable, since it is only a matter of time before the former is converted (translated) into the latter. With experimental induction we formulate experimental laws, but then we incorporate them into a theory, so that we end up with only symbolic laws. Nevertheless, I consider Duhem's distinction to be relevant to understanding *scientific discovery*, rather than theories as a final product.

The interpretation of Duhem's comments on laws and induction that I propose is to consider that, once the theoretical terms have been formulated, the process for formulating an experimental law is inductive; it consists of establishing, through a series of experiments, what relationship of dependence exists between theoretical terms (magnitudes). We can infer by induction that two variables are related and, in general, it is relatively easy to indicate which mathematical formulation of said relation is simpler. Duhem (1906/1954, 171) states that a physicist chooses between possible formulations of a physical law based on simplicity. What is not inductive, according to Duhem, is the formulation of hypotheses.

**7. Physics different levels of abstraction according to Duhem**

Why exactly does Duhem reject the idea of using an experimental law in a theory directly, without a theoretical translation? As we saw, Duhem conceives physical theories as *deductive* systems and there are two ways in which an experimental law could be made part of one of those systems. The experimental law could either be a conclusion deductively derived from the first principles of the theory, or it could be itself one of the principles. In the former case, it might be difficult to come up with principles (which Duhem calls hypotheses) that allow an exact derivation of the law in question. It would be more practical to derive another new law resembling the original one and explain the deviation by other means (e.g. invoking sources of error). The law so reconstructed would then be a "theoretical translation" of the original experimental law. In the second case, using the experimental law as one of the principles of the theory might seem easier. However, Duhem considered that the principles of a theory (or hypotheses) must predict, i.e. they must be able to be used to deduce new laws from them, not only those already known. To do so, the principles (hypotheses) would have to be formulated with a higher level of abstraction and generality than experimental laws.

As we saw in the last section, Duhem's distinction between laws and principles or hypotheses seems to be associated with two levels of abstraction in physics; what he describes as 'double intellectual economy'. According to Duhem, scientists formulate experimental laws to condense multiple results of concrete experiments. "The experimental law itself already represented *a first intellectual economy*. The human mind had been facing an enormous number of concrete facts, each complicated by a multitude of details of all sorts…Abstraction entered the scene. It brought about the removal of everything private or individual from these facts, extracting from their total only what was general in them or common to them, and in place of this cumbersome mass of facts it has substituted a single proposition, occupying little of one's memory and easy to convey through instruction: it has formulated a physical law" (Duhem 1906/1954, 22, emphasis added).

Duhem takes up Ernst Mach's concept of intellectual economy, as well as the example he proposes: the law of refraction (Snell's law).[14] Duhem points out that Snell's law allows us to foresee innumerable refraction phenomena. According to him, this law and the optics that represents it "experiment, induction, and generalization have alone produced them" (Duhem 1906/1954, 34). In other words, experimental laws represent a variety of concrete experimental facts.

[14] The affinity between Mach and Duhem was explicitly acknowledged by both, as can be seen in several of their texts, as well as in their correspondence. In *Knowledge and Error*, for example, Mach even claims (in the prologue to the second edition) that Duhem basically reached the same conclusions as he did. Part of their correspondence and academic comments about it can be found in Klaus Hentschel (1988). More recently, Anastasios Brenner (2019) reviewed the relation between Duhem and Mach as well as their agreements.

Hypotheses have a different function; they form a theory that constitutes a second intellectual economy. "The economy achieved by the substitution of the law for the concrete facts is *redoubled* by the mind when it condenses experimental laws into theories. What the law of refraction is to the innumerable facts of refraction, optical theory is to the infinitely varied laws of light phenomena" (Duhem 1906/1954, 22, emphasis added).

Duhem considers that at the first level of abstraction (the first intellectual economy) we elaborate experimental laws by *induction*. However, at the second level (the second intellectual economy) we elaborate theories, but *not by induction*. This second level requires sets of hypotheses that represent *laws* and not concrete experimental facts directly.[15] According to Duhem, hypotheses must be the foundation of a deductive system (theory) that is general enough to comprise multiple experimental laws. "These fundamental principles or *hypotheses* (in the etymological sense of the word) are not *axioms*, self-evident truths. Nor are they laws, that is, general propositions reached directly by induction from teachings of experience" (Quoted in Garrigou-Lagrange 1955, 450, italics in the original).

The distinction between the two levels of abstraction looks similar to the one we saw in Rankine, but an important difference is the idea that the second level cannot be inductive. Since, for Duhem, experimental laws are formulated in a sufficiently concrete way to represent facts from particular experiments, they typically do not have a sufficient degree of generality to lead to the deduction of other distinct laws. This is consistent with

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15 Some contemporaries of Duhem recognized the distinction between the two levels of abstraction that Duhem points out, as can be seen in the comments of Abel Rey: "Les théories physiques sont pour lui [Duhem] une nécessité, et il existe une physique théorique, à côté de la science empirique [Physical theories are a necessity for him [Duhem], and there is theoretical physics alongside empirical science]" (Rey 1904, 704). I am grateful to one of the reviewers for reminding me of the importance of Abel Rey's work.

the narrow conception of induction we saw in section 2. Consequently, Duhem points out that experimental laws would have to be modified to be used as hypotheses and there is no unique way to make that modification. Induction does not indicate the set of hypotheses capable of deducing various experimental laws. It is not a matter of observing a set of laws and inferring a general statement. If this were the case, there would be no reason to expect that new laws (predictions) could be deduced from the set of hypotheses. *Duhem distinguishes experimental laws from hypotheses by the process of discovery: the former is inductive; the latter is not*. At the same time, these different processes of discovery are associated with different levels of abstraction.

Although it seems certain that Duhem distinguishes different levels of abstraction, the number of these levels might not be so clear. In *Aim and Structure*, as we saw, Duhem mentions two levels of "intellectual economy" in physics. In another earlier text, "Physics and Metaphysics," Duhem says that "All experimental science is composed of at least two phases: the observation of facts and their reduction to laws. But in those that, like physics, have arrived at a sufficient degree of development, a third phase is conjoined to the two others. This is the theoretical phase" (Duhem 1893a/1996, 35-36). In other words, the observation of facts and formulation of laws is described as one level of abstraction in *Aim and Structure*, while they are two phases in "Physics and Metaphysics."[16] In any case, the level of the theories is different from that of experimental laws. Note, in addition, that the idea that physics is different from other sciences because of its "theoretical phase" is consistent with what Roger Ariew (1984, 320) points out: that, for Duhem, sciences other

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16 Rogelio Miranda Vilchis (2018) argues, contrary to what is commonly thought, that metaphysics has a place in Duhem's philosophy of science, although at a higher level of abstraction and generalization than even theories.

than physics do use the Newtonian method. This is because the level of the theories (theoretical phase) is the one that, according to Duhem, cannot be reached by induction.

It may seem that Duhem's way of distinguishing experimental laws and hypotheses differs very little from the one that has traditionally been maintained in philosophy of science between laws and theories. Rudolf Carnap (1966, 225-231), for example, distinguishes 'theoretical laws' from 'empirical laws' pointing out that only the latter can be considered inductive generalizations. However, this traditional distinction differs from Duhem's in some relevant respects. First, the traditional distinction seems to be aimed at distinguishing unobservable microscopic entities (such as atoms) from macroscopic observable entities. Duhem does not seem to be concerned with this issue, since he was skeptical of atomistic physics anyway.[17] Indeed, Carnap acknowledges that physicists commonly use the term 'observable' in a different way than philosophers.[18] Second, and more importantly, Carnap and Nagel's traditional distinction is defined by the difference

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[17] On Duhem's rejection of atomism see, for example, Klodian Coko (2015).

[18] It is also important to consider that there were relevant differences between Duhem's scientific context and Carnap's. While atomic theories were considered problematic by many physicists for most of Duhem's scientific career, the acceptance and importance of such theories had greatly increased by Carnap's time

between different *types of terms*, in contrast to Duhem's distinction defined by different *types of inference*.[19] [20]

## 8. Inferences in Physics: Induction, hypotheses and *Good Sense*

I argue that, for Duhem, inductive inferences are means of elaborating experimental laws that constitute a first level of abstraction in physics. At the second level, that of theories, it requires the formulation of hypotheses, which, Duhem believes, are not formulated by induction.

[19] Also, Nancy Cartwright (1983) points out that physicists, such as Duhem, distinguish laws in a different way than philosophers. According to Cartwright, philosophers often speak of phenomenal laws and theoretical laws, but: "For the physicist, unlike the philosopher, the distinction between theoretical and *phenomenological has nothing to do with what is observable and what is unobservable*. Instead the terms separate laws which are fundamental and explanatory from those that merely describe." (Cartwright 1981, 2, emphasis added). Cartwright takes the physicists' distinction to be between (fundamental) laws that are used to explain phenomena and (non-fundamental) laws that merely describe what happens in experiments. Cartwright repeatedly quotes Duhem to support this view. However, Duhem took neither laws nor theories to be explanations, i.e., he does not seem to recognize anything like an "explanatory law." Yet Cartwright does entertain a notion of "explanation" in which "to explain a collection of phenomenological laws is to give a physical theory of them, a physical theory in Duhem's sense, one that summarizes the laws and logically classifies them" (Cartwright 1981, 96). In that sense, Cartwright's distinction between explanatory laws and phenomenal laws does seem consistent with the distinction I take Duhem to hold between theoretical laws and experimental laws; except that Cartwright (1981) also uses a stronger notion of "explanation" (different from Duhem's) when talking about "theoretical entities."

[20] Carnap and Nagel are mentioned here with the intention to make a contrast and to point out what I take to be distinctive features of Duhem's position. However, there are reasons to suspect an important influence of Duhem in, at least, Carnap. As one reviewer pointed out, Carnap called himself a conventionalist in his early works and included Duhem's book, for instance, in the reference of his *Physikalische Begriffsbildung* (1926). Carnap also refers explicitly to Duhem in *Logical syntax of Language* (1937, 318). Even so, I believe that to explore in detail this influence falls outside the scope of the present paper.

Duhem (1906/1954, 220) points out that the only requirements that hypotheses must meet are those of being logically consistent and that the greatest possible number of experimental laws can be deducted from them. However, he recognizes that these requirements give physicists too much freedom, i.e. they leave them without guidance. "Can such unlimited freedom be useful to a man? Is his mind powerful enough to create a physical theory all out of one piece? Surely no." (Duhem 1906/1954, 221). Something else is needed to formulate the hypotheses that constitute new theories.

In Chapter VII of *Aim and Structure*, Duhem argues that there are historical processes by which hypotheses are formulated. In those processes the hypotheses are found first with the use of *analogies* and then they are gradually modified in response to new observations. Lothar Schäfer (2006) views analogies as the foundation of Duhem's method of scientific discovery. However, there are many ways in which one can make analogies when investigating physical phenomena. Schäfer (2006) indicates that Duhem's method of discovery requires what Duhem calls *good sense* [*bon sens*]. By means of *good sense*, physicists select suitable analogies to formulate hypotheses and thus discover new theories.

Duhem did not clearly define his concept of *good sense,* but it has been understood in recent literature as a judgment based on moral and intellectual considerations. Duhem (1906/1954, 216-218) introduces *good sense* in his analysis of test experiments that yield results against a hypothesis. The physicist may either modify the hypothesis or modify the set of assumptions that, according to Duhem, are always involved in experimental tests. Both solutions may be valid, so Duhem (1906/1954, 217) argues that one chooses between them by using *good sense*. Since Duhem (1906/1954, 218) emphasizes that the use of *good sense* requires honesty and impartiality, it has been argued that he was signaling a judgment based on considerations about epistemic agents.[21] Just as virtue epistemologists define knowledge as the belief that an intellectually virtuous person accepts, Duhem would be arguing that the correct conclusion is the one that the physicist with certain virtues accepts.

However, there seems to be no complete consensus in secondary literature regarding the interpretation of Duhem's *good sense*. Milena Ivanova and Cedric Paternotte (2013) pointed out that many of the interpretations of *good sense* made in the direction just described are unsatisfactory. Among other things, they show that such interpretations are not entirely consistent with Duhem's comments on *good sense*. Also, Jamie Shaw (2020) criticized many aspects of recent interpretations.

Recalling what was said in the previous session, we can conclude, at least, that Duhem's *good sense* is a resource that serves a function at the second level of abstraction. Duhem is clear in arguing that *good sense* is required to formulate new hypotheses: "When the factual proof has turned against the preconceived idea, it is not enough simply to reject it. One must substitute for it a new supposition which has the possibility of standing up better to experimental testing…Truly, in order to perform this well, it is necessary that good sense should transcend itself, that is, push its strength and its suppleness to their very limits…" (Duhem, 1915/1991, 24-25). In other words, *good sense* is necessary to produce new hypotheses and, consequently, new theories. In this sense, *good sense* seems to be the resource that replaces *induction* at the second level of abstraction in physics. *This suggests that good sense implies the use of a different kind of inference*.

A contemporary of Duhem also recognized that reasoning in scientific research requires more than *inductive* and *deductive* inferences: Charles Sanders Peirce (1839-1914).

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[21] For example: David Stump (2007), Milena Ivanova (2010), Abrol Fairweather (2012), Sindhuja Bhakthavatsalam (2017) and Jamie Shaw (2020), among others. It has also been said (Martin 1991) that Duhem's concept of *good sense* should not be confused with that of *common sense*.

Even though Duhem and Peirce didn't quote each other (as far as I know) and there were differences in the intellectual climate of France and the United States, there are reasons to expect some significant similarities between them. Duhem himself (1913/1996, 237) came to recognize that many of his ideas could be understood as related to American pragmatism (although he expressed some reservations). Moreover, he felt the need to defend himself against some of the association made in this sense by his contemporaries such as the philosopher Abel Ray (1873-1940).[22] Besides, an early review of Duhem's book in the United States (from 1906) also made that association between him and pragmatism. The review written by Edward G. Spaulding states that:

> The general position which M. Duhem takes seems to me to be of special opportuneness and interest in connection with current agitation as to radical empiricism and pragmatism… the pragmatist might, perhaps, point with satisfaction at finding a position, sympathetic to his own, yet taken by an active physicist. But for M. Duhem this is methodological pragmatism. With the position which goes farther than this and finds in this pragmatism the implications of an ontological doctrine, one with whose outlines we have all been made familiar recently, doubtless much to their disappointment some of our pragmatists would not find him to be in agreement…" (Spaulding 1906, 610).

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[22] That defense can be found in the article "The Value of Physical Theory," (published in 1908 initially in the *Revue générale des Sciences pures et appliquées* and later as an Appendix to *Aim and Structure*). There, Duhem argues about Abel Rey's book *La théorie de la physique chez les physiciens contemporains* (1907). This supports the idea that Duhem had at least some knowledge of pragmatism. According to Duhem, Rey points out that several important physicists of the time (including Duhem himself) held a pragmatic, utilitarian view of scientific theories, but that they also hoped that science would eventually lead to a theory that genuinely expressed the ontological nature of the world. Duhem points out that such a hope cannot be justified logically, but only by *good sense*. So, Duhem's main disagreement with pragmatism has to do with the end point of scientific inquiry but not with scientific methodology. I am grateful to one of the reviewers for reminding me of the importance of Abel Rey's work.

The reservation that Spaulding finally expresses regarding Duhem's identification with pragmatism (which is similar to the ones that Duhem himself expresses) seems related to the position that the latter takes regarding ontological and metaphysical questions. However, it is clear that, when it comes to scientific methodology, Spaulding recognizes Duhem's affinity with pragmatism.

Regarding the issue of inferences in scientific inquiry, Peirce had pointed out in "Deduction, Induction, and Hypothesis" the importance of a third kind of inference (apart of induction and deduction). He referred to this third kind of inference as "hypothesis" and, in other texts, as "abduction": "By induction, we conclude that facts, similar to observed facts, are true in cases not examined. By hypothesis, we conclude the existence of a fact quite different from anything observed, from which, according to known laws, something observed would necessarily result. The former, is reasoning from particulars to the general law; the latter, from effect to cause. The former classifies, the latter explains" (Peirce 1931–1958, 2.636).

According to Peirce, hypotheses explain in the sense that, with them, we can deduce consequences that we observe.[23] In an induction, on the contrary, observations lead to a conclusion as a consequence.

Peirce was not sufficiently clear about how he understood hypothesis/abduction and there is a wide debate in recent literature regarding how to understand some of its aspects. It has been said, for example, that Peirce's "abduction" refers to "inference to the best

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[23] At first glance, there would seem to be a disagreement between Duhem (1906/1954, 19) and Peirce, since the former emphasizes that theories (and, consequently, the hypotheses that form them) are not explanations. However, Duhem clarifies that theories do not explain in the sense of "bring us into immediate contact with reality." Rather, Duhem says that the hypotheses are such that experimental laws can be deduced from them, which is the same as what Peirce says in the text in question

explanation." However, authors such as Mousa Mohammadian (2021), have argued that this is incorrect because Peirce would see hypothesis/abduction (also) as a means of *discovery*. Peirce seems to have had difficulty making explicit, in a systematic way, the characteristics of the type of inference he sought to emphasize, but he seems to be looking for something similar to what Duhem was looking for with his concept of *good sense*.

Like Duhem's *good sense*, Peirce's hypothesis/abduction is required to formulate theories but this does not prevent Peirce from understanding some scientific results (such as what he calls 'empirical formulas') as inductive conclusions: "These simple formulæ are not usually, if ever, exactly true, but they are none the less important for that; and the great triumph of the hypothesis comes when it explains not only the formula, but also the deviations from the formula. In the current language of the physicists, an hypothesis of this importance is called a theory…" (Peirce 1931–1958, 2.638). Furthermore, according to Peirce, hypothesis/abduction is presented as a response to an unexpected event: "Hypothesis is where we find some very curious circumstance, which would be explained by the supposition that it was a case of a certain general rule, and thereupon adopt that supposition…" (Peirce 1931–1958, 2.624). This is the same type of case in which we saw Duhem resort to *good sense*: the unexpected encounter with the negative result in test experiments. Additionally, like Duhem when speaking of *good sense*, Peirce emphasizes moral virtues when speaking of hypothesis/abduction: "...in order that the process of making a hypothesis should lead to a probable result, the following rules must be followed…The failures as well as the successes of the predictions must be honestly noted. The whole proceeding must be fair and unbiased" (Peirce 1931–1958, 2.634). Succinctly put, I take Duhem's *good sense* to be his version of Peirce's hypothesis/abduction, i.e. it is an attempt to identify a kind of inference, distinct from induction and deduction, that is necessary for the discovery of new theories.

Peirce (Peirce 1931–1958, 2.625) gives examples of the use of hypothesis/abduction from both science and everyday life, and if Duhem believed that the kind of inference he sought to identify could be recognized in everyday reasoning, it would make sense that he chose the term *good sense* because of its closeness to that of *common sense*. Duhem's emphasis on moral virtues is due to the absence of explicit schemes and rules for this kind of inference. That absence makes the kind of inference in question difficult to distinguish objectively from a conclusion based on mere preferences, which is mitigated by the imperative to maintain honesty and impartiality.

I am not trying to suggest here, though, that there was a direct influence between Peirce and Duhem, nor that Peirce's concept of hypothesis/abduction is completely identical to Duhem's *good sense*. Rather, I am seeking to highlight that both share a conception of theories that distinguish levels of abstraction, which constitutes a background that frames their reflections. In particular, the discussion on *scientific discovery* is marked by this distinction. The process that allows the discovery of laws is not the same as the process that allows the discovery of hypotheses, since different types of inference are required.

To show in detail that this interpretation of Duhem's *good sense* (as a kind of inference similar to Peirce's abduction) is more adequate than other proposals would require further elaboration. Nevertheless, I consider that this section sufficiently shows how clarifying the role of induction in Duhem's philosophy can contribute to other recent debates, such as attempts to interpret his concept of *good sense*.

**9. Conclusion**

As said in the introduction, the aim of this paper is to answer the question: *What, according to Duhem, is the function of induction, if there is one, in scientific research?* We have seen that Duhem believed that induction allows for the formulation of experimental laws as part of the first level of abstraction in scientific knowledge. Duhem distinguishes two levels of abstraction in physics, each associated with discovery processes based on different inferences. The distinction between the levels of abstraction in physics does not depend on the type of terms used; rather, it depends on the distinction between the types of inferences that the discovery processes at each level require. At the first level of abstraction, the discovery process requires experimental induction, which produces experimental laws. At the second level, that of hypothesis, it requires a different type of inference (which Duhem called *good sense* and which seems similar to what Peirce called abduction).

In sum, we can draw the following lessons from the present study: (1) contrary to what authors such as Brenner, Vuillemin and Ariew have pointed out, Duhem recognized an important function for inductive inferences in physics: to assist the formulation of experimental laws, although not theories. (2) the excessive emphasis on the distinction between observable and theoretical terms can cause us, like Nagel and Carnap, to lose sight of other important ways in which scientific statements differ from each other. Not all results of scientific research are supported by evidence in the same way because they can result from different types of inference. (3) taking into account the distinction between levels of abstraction in Duhem's philosophy, as well as the functions of different types of inference in them, contributes to attempts to interpret his concept of *good sense* by locating the level of abstraction at which it should fulfill its function. Finally, (4) rather than the complete abandonment of the inductive method that Laudan describes, what we find in Duhem is an attempt to broaden the conception of scientific research. The limitations of inductive inferences are emphasized in order to point out the need for other types of inferences. This was an attempt to broaden the conception of the logic of research in physics.

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