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THE DUHEM-QUINE THESIS RECONSIDERED

Abstract. The high point of the falsification of physical theories in a standard view of the philosophy of science is the so-called crucial experiment. This experiment is a kind of manipulated empirical test, which provides the criterion for distinguishing between two rival hypotheses, where one is an acceptable theory due to passing the test, and the other turns out to be an unacceptable theory as it does not pass the test. The crucial experiment was supposed to play a significant role because, in virtue of an empirical disconfirmation of one theory, the experiment was assumed to confirm the other as true. However, in 1906, in *La théorie physique, son object et la structure* (hereafter quoted in English translation as *The Aim and Structure of Physical Theory* (1906/1954)), Pierre Duhem famously argued against this view and stated that crucial experiments in physics are impossible as they are necessarily ambiguous and logically incomplete. His contention rested on the claim that, "[a] physical theory is not an explanation [of true reality in itself in virtue of some broad metaphysical ramification of physics]. It is a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as accurately as possible a set of experimental laws" (*ibid.*, p. 19). Furthermore, different theories could be equally suitable to represent a given group of experimental laws. And, assuming holism, no hypothesis could be tested in isolation, but merely as a part of a set of an entire scientific theory. The problem which Duhem identified in 1906 was slightly overshadowed and neglected in mainstream philosophy of science until the appearance of a challenging paper by Willard Van Orman Quine published in 1951 and entitled "Two Dogmas of Empiricism". Quine's paper caused a revival of interest in Duhem's original formulation and gave a new impulse towards the problem in the form of the so-called Duhem-Quine thesis. The aim of this paper is to reconsider whether Duhem was right to argue that there are no crucial experiments in physics. In order to assess the validity of the thesis, first, this paper makes an exposition of Duhem's arguments in their favour, and analyses the major criticisms of this position offered in the subject-literature of Adolf Grünbaum, who explicitly attacked the arguments for the thesis as inconclusive and false. Then, this paper presents possible modes of defence of the Duhem-Quine thesis and argues that the original formulation of the thesis is well qualified and plausible. Finally, this paper offers a pragmatic interpretation of the theory choice.

Keywords: Duhem, Quine, Crucial Experiments, Holism, Theory Choice, Virtue Epistemology, Pragmatism.

1. Introduction

Piere Duhem (1861–1916) is regarded as one of the most original philosophers of science of the last century. Although he was primarily a practicing physicist, the important part of his scientific activity also concerned the history and philosophy of science. A distinctive mark of his philosophical writing is a combination of historical research and philosophical analysis.[1] Duhemian thought had an immediate influence and was the subject of discussions of such philosophers as Ernst Mach, Henry Poincaré, the members of Vienna Circle, and Karl Popper (see Ariew and Barker, 1990, p. 179). Recent interest in Duhem's ideas has a lot to do with Wilfried V. Quine's (1951) reference to Duhem's thesis that a conclusive falsification of a hypotheses is not possible in virtue of experimental evidence alone. The Duhemian and Quinean holistic approach (i.e. a conviction that an experimental test concerns a whole group of hypotheses, not an isolated hypothesis) is labelled in the contemporary philosophy of science as the Duhem-Quine thesis and caused extensive discussion over its implications for physical science (see Harding, 1976; Weber, 2009; Fairweather, 2012).

By the time of Duhem, one of the most widely accepted methodological positions, especially within nineteenth-century scientism, was that there is some kind of "controlled experience which provides the basis for distinguishing between acceptable and unacceptable theories in science: acceptable theories are those which can pass empirical tests" (Harding, 1976, p. IX). The so-called "crucial experiment" was thought to play this role. In virtue of the empirical disconfirmation of one theory, the crucial experiment was supposed to confirm the other as true.

However, in *La théorie physique, son object et la structure* (hereafter quoted in English translation as *The Aim and Structure of Physical Theory*, 1906/1954), Duhem argued against this view and stated that crucial experiments in physics are impossible as they are necessarily ambiguous and logically inexhaustive. Duhem's contention rested on the claim that, "[a] physical theory is not an explanation [of the true reality in itself in virtue of some broad metaphysical ramification of physics]. It is a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as accurately as possible a set of experimental laws" (Duhem, 1906/1954, p. 19). Furthermore, different theories could be equally suitable to represent a given group of experimental laws. And, assuming holism, no hypothesis could be tested in isolation, but merely as a part of an entire set of scientific theories.

The aim of this paper is to provide an answer to the question regarding the validity of the Duhem-Quine thesis or – in other words – whether Duhem was right to argue that there are no crucial experiments in physics. In order to assess the validity of the thesis, I make a favourable exposition of Duhem's arguments (section 2), and I analyse the major criticism of Duhem's position offered in the subject-literature of Adolf Grünbaum (section 3). Grünbaum explicitly attacked the arguments of Duhem as being inconclusive and false. Finally, I present possible ways one can defend the Duhemian position, and I argue that Duhem's methodological view concerning the crucial experiment is well qualified and plausible (section 4).

In the course of the discussion of the Duhem-Quine thesis, this paper proposes a new interpretation of theory choice, with the theory choice constituting the major puzzle for the thesis (section 5). This puzzle concerns the lack of criterion for choosing one of the competing scientific theories if there are no crucial experiments guiding such a choice. The proposed solution to this puzzle indicates that a correlate to the Duhem-Quine thesis, as formulated by Duhem, should be perceived as a concept of the specific scientific skills that guide the aforementioned choice. These skills are labelled by Duhem as "good sense." This paper reads them as a combination of virtue epistemology and the pragmatic process of the formation of such virtues analogous to Aristotelian character formation as given in Aristotle's virtue ethics.

2. Duhem's Arguments

In *Aim and Structure*, Duhem argued against the nineteenth century scientists and thinkers who were sustaining "naïve realism," i.e. a standpoint that claims that physical theories serve as an explanation of true reality and that science is supposed to resemble the proper reality of nature (see Duhem, 1906/1954, pp. 7–19; cf. Laudan, 1965/1976, p. 156). In other words, scientific theories could be proven as being natural truths. A probe for finding a true theory seemed to be the so-called "crucial experiment."[2] This being an experiment in virtue of which it was possible to refute one theory (or hypothesis) and simultaneously verify a competing theory (or hypothesis) as the only logical explanatory alternative to a target theory (or hypothesis). By eliminating rival theories, science was supposed to reveal a true picture of nature and make a steady progress. Even if the theories in question would not be proven through direct verification, they still could be proven indirectly by a falsification of rival theories.

According to Duhem, this is improper to the characteristics of the scientific research procedure. Firstly, we should distinguish between a metaphysical description of the world in terms of its true nature, and a scientific description in terms of a representation of empirical evidence or the appearance of the world (see Duhem, 1906/1954, pp. 7–19). The latter, scientific description exhibits distinctively epistemological assumptions. Secondly, all empirical material is already theoretically-involved; empirical material (data) is always seen as providing some ramification upon interpretation. The second point reveals a crucial methodological assumption of Duhemian holism (see Duhem, 1906/1954, pp. 144–165). In the sixth chapter of *Aim and Structure*, Duhem offered two arguments against the notion of crucial experiments. Namely, that these experiments emerge from (1) an ambiguity of the falsification procedure, and (2) the logical non-exhaustiveness of experiments.

2.1. The Argument from the Ambiguity of the Falsification Procedure in Physics

In order to demonstrate the ambiguity of the falsification procedure, Duhem analysed two examples of experiments from optics that were regarded as crucial in the nineteenth century. The first one, Wienner's experiment, concerned the direction of the vibration of a polarized light (Duhem, 1906/1954, p. 185). The second, Foucault's experiment, regarded the velocity of light in air and water as a crucial test between two rival theories of light: the wave theory and the particle theory (Duhem, 1906/1954, pp. 186–187, 189). Let us take a brief look at the Duhemian analyses of the second experiment.

The particle theory (which assumes that light consists of small particles) had predicted that the velocity of a light refracted in water, with water being a medium with a higher density than a vacuum, should be greater than that of a light refracted in air (in virtue of the speeding up of particles by a short-range force in the denser medium). The opposing wave theory (which assumes that light is a crossways pulsation in a medium, the ether, that permeates material bodies) makes a completely opposite prediction. Foucault's method of measurement in virtue of rotating mirrors demonstrated that the velocity of the propagation of light in water is less than in the air. This was commonly thought of as a crucial experiment in favour of the wave theory: the particle theory was falsified and simultaneously the wave theory was confirmed and verified.

However, according to Duhem, both theories were not a single hypothesis but a whole group of hypotheses from which one could deduce the rela-

tionship between the index of a refraction (a propagation) and the velocity of light in different environments (Duhem, 1906/1954, p. 187). In other words, what is tested here is not a separate empirical prediction of the hypothesis in question, but the entire system of hypotheses and the auxiliary assumptions (e.g. the instruments and circumstances of an experiment) that we need to test for these same predictions. This theoretical setup contains, at least, a tested target theory, but also auxiliary theories concerning an experiment's use of instruments, the genuine factors of an experiment's environment, and the background physical theories used to interpret observational data. This is a key point of Duhemian holism and its subsequent interpretation of Quine. Therefore, Foucault's experiment shows us merely that at least one of the hypotheses or assumptions is false, but the experiment did not determine which of these hypotheses or assumptions is false. Assuming this holistic approach, we can state that it is not possible to conclusively falsify a physical theory and, *a fortiori*, that there are no crucial experiments in virtue of the ambiguity of the falsification procedure itself.

Schematically, the Duhemian argument could be expressed in the following way:[3] In order to formulate an unambiguous (i.e. conclusive) experimental procedure, a hypothesis must take the form of an inference in terms of the basic logic law of *modus tollens*. That is, if from a target hypothesis (say, H) observable evidence is predicted (say, O) [i.e. the premise 1], and the prediction turns out to be false (say, $\sim O$) [i.e. the premise 2], then, it would be implied that the target hypothesis deduced from the prediction is false (say, $\sim H$) [i.e. the conclusion]. This could be put on as follows:

$$[(H \rightarrow O) \,\&\, \sim O] \rightarrow \sim H$$

by virtue of the law of *modus tollens*:

$$[(p \rightarrow q) \,\&\, \sim q] \rightarrow \sim p$$

However, as Duhem argued, this is not a factual form of the scientific reasoning found in physics. Instead, when a theory is tested it is not the case that there is only the theory alone as a single hypothesis. In fact, a theory rests on many assumptions (auxiliary hypotheses) and rules of inferences (deduction in physics is rarely, if ever, possible). Hereafter, if $A_1, A_2, \ldots, A_n$ would stand for an auxiliary hypothesis, it could be stressed that as follows:

$$[(H \,\&\, A_1 \,\&\, A_2 \,\&\, \ldots \,\&\, A_n \rightarrow O) \,\&\, \sim O] \rightarrow \sim(H \,\&\, A_1 \,\&\, A_2 \,\&\, \ldots \,\&\, A_n)$$

Then, due to the fact that a falsified prediction (O) is a consequence of the conjunction of the set of hypotheses contained in the target hypothesis (H)

and the connected group of auxiliary hypotheses ($A_1 \& A_2 \& \ldots \& A_n$), we are not able to rule that one of these hypotheses is a false one. We can only assert that the whole set of these hypotheses (i.e. $H \,\&\, A_1 \,\&\, A_2 \,\&\, \ldots \,\&\, A_n$) is false. In other words, merely the whole body of hypotheses could be disconfirmed, but not any of the individual hypothesis. This holistic claim is valid *a fortiori* in the case of a crucial experiment. Because of the ambiguity of the falsification procedure itself, we are not able to isolate a target hypothesis as refuted and simultaneously confirm its rival (Duhem, 1906/1954, pp. 185–187).

2.2. The Logical Inexhaustiveness of a Crucial Experiment

In order to establish the second, decisive argument, Duhem contrasts the usage of a crucial experiment method in physics with how the reasoning *reductio ad absurdum* is used in mathematics (Duhem, 1906/1954, pp. 188–190):

> Reduction to absurdity seems to be merely a means of refutation, but it may become a method of demonstration: in order to demonstrate the truth of a proposition it suffices to corner anyone who would admit the contradictory of the given proposition into admitting an absurd consequence. We know to what extent the Greek geometers drew heavily on this mode of demonstration (*ibid.*, p. 188).

In mathematical scientific practice, we can prove some theorem in virtue of the method of indirect proof. Namely, assuming its falsity and then deducting from its falsity a contradiction. To obtain this conclusion, we need just logical truths as additional premises. The key point of this reasoning is that a theorem is logically exhaustive by reason of using merely necessary truths (axioms or rules of references) as its premises. In effect, if the negation of the theorem in question is a contradiction (whereas all auxiliary premises are true), then the theorem in question is conclusively true (Curt and Cover, 1998, p. 360). As Duhem rightly noted in a comparison to this, scientific practice in physics is different. A physicist by the means of a crucial experiment does not engage himself with necessary truths as the premises of his reasoning, he simply operates with empirical assumptions and a number of competing hypotheses that could be equally true and false. With this, a crucial experiment is then logically inexhaustive.

If a crucial experiment would be formulated in terms of the reasoning concerning *reductio ad absurdum*, then it would be possible to conclusively falsify a target hypothesis and simultaneously confirm as true an alternative one. But this is not the case:

> Unlike the reduction to absurdity employed by geometers, experimental contradiction does not have the power to transform a physical hypothesis into an indisputable truth; in order to confer this power on it, it would be necessary to enumerate completely the various hypotheses which may cover a determinate group of phenomena; but the physicist is never sure he has exhausted all the imaginable assumptions. The truth of a physical theory is not decided by heads or tails (Duhem, 1906/1954, p. 190).

In physics, unlike in the mathematical *reduction ad absurdum* method, an alternative theory does not imply itself to be the only logical alternative. An alternative theory merely implies itself to be one possible rival hypothesis. Schematically, it is not $\sim H \rightarrow H$, but rather $\sim H \rightarrow H^*$, where H^* stands for one of the set possible, disjunctive hypotheses. It is always possible (1) to falsify a rival hypothesis (as not necessarily true), and (2) to establish (discover) a new alternative hypothesis (unknown at the moment) as a more plausible and satisfactory one. Therefore, in order to fulfill the logical exhaustiveness requirement such as is in the reasoning of *ad absurdum*, the crucial experiment should reflect a complete list of various hypotheses suitable in determining observational data. In fact, crucial experiments do not achieve this goal. The crucial experiment is then logically inexhaustive: "while two rivals cannot both be true, they both might be false; thus, we cannot infer the truth of the one from the falsity of the other" (Curd and Cover, 1998, p. 356).

3. Criticism of Duhem's Arguments

In order to assess the validity of the Duhemian arguments, I consider the major criticism of Duhem's position offered by Adolf Grünbaum.[4] In the years of 1960 and 1970, in several publications, Grünbaum attacked Duhem's thesis as (1) inconclusive and (2) false.

3.1. The Charge of the Inconclusiveness of Duhem's Arguments

Grünbaum (1960/1976; 1974/1976) argues that the Duhemian thesis is *non-sequitur*, i.e. that a conclusion concerning the impossibility of crucial experiments does not follow logically from its own premises. Thus, the whole argument of Duhem is not conclusive.

A target of the Grünbaum's criticism is Duhem's claim that the, "*falsifiability* of an isolated empirical hypothesis H as an *explanans* is *unavoidably inconclusive*" (Grünbaum, 1960/1976, p. 116). In other words, there is no possibility of falsifying a separate hypothesis, because a target hypothesis is

always tested with auxiliary hypotheses as part of a whole set of hypotheses (as is presupposed by the holistic character of the thesis of Duhem). It is impossible to determine, which of the aforementioned theses is in fact disconfirmed. According to Grünbaum, the Duhemian "schema of unavoidably inconclusive falsifiability consists" of the two following parts:

> (a) an elementary fact of *deductive* logic is that if certain observational consequences O are entailed by the *conjunction* of H and a set A of auxiliary assumptions, then the *failure* of O to materialize entails *not* the falsity of H by itself but only a weaker conclusion that H and A cannot *both* be true; the falsifiability of H is therefore *inconclusive* in the sense that the *falsity* of H is not *deductively inferable* from the premises $[(H \,\&\, A) \rightarrow O] \,\&\, {\sim}O$,

and

> (b) the actual observational findings O', which are incompatible with O, *allow* H to be true while A is false, because they always permit the theorist to preserve H with impunity as a part of the *explanans* of O' by so modifying A that the *conjunction* of H and the *revised* version A' of A does explain (entail) O'. This preservability of H is to be understood as a retainability *in principle* and does *not* depend on the ability of scientists to propound the required set A' of collateral assumptions at any given time. Thus, there is an ingression of a kind of *a priori* choice into physical theory [...] (*ibid.*, p. 117).[5]

However, according to Grünbaum, Duhem's contention is that "the falsification of part of an *explanans* is always unavoidably inconclusive, is untenable" (*ibid.*, p. 116). In order to sustain this claim, Duhem should offer some assurance that "there exists such a *non-trivial* set A' [i.e. an auxiliary assumption] for any component hypothesis H [i.e. a target hypothesis] *independently* of the domain of empirical science to which H pertains [...]" (*ibid.*, p. 118). But, as Grünbaum holds, "such assurance *cannot* be given on general logical grounds at all but that the existence of the required set A' needs a *separate* and *concrete* demonstration for each particular case" (*ibid.*, p. 118). Schematically, a key point of Grünbaum's objection is as follows: from premises $[(H \,\&\, A) \rightarrow O] \,\&\, {\sim}O$ it is not possible to follow logically to the conclusion $(\exists A')[(H \,\&\, A') \rightarrow O']$.

In other words, Grünbaum disagrees with the range of the validity of Duhem's holism in respect to a falsification procedure. That is, the target hypothesis (H) cannot be conclusively falsified by the reason that (H) is always a part of the whole theoretical setup. All empirical evidence is considered in the wide range of a set of the target hypothesis and its auxiliary assumptions. Then, if this evidence is not compatible with these target

hypotheses and auxiliary assumptions, in order to save the target hypothesis, it is possible to try to adequately change the auxiliary assumptions. However, for Grünbaum, in order to guarantee the general validity of this maneuver (not just in this particular case), Duhem should establish some logical (universal) grounds for the following extrapolation: In every case for any empirical evidence (confirming or not) of a tested theory, there would exist some auxiliary assumption, not merely the target hypothesis. "Unless we have a proof that an appropriate non-trivial A' [saving an auxiliary assumption] exists for every H [a target hypothesis] and O [an observational prediction of the hypothesis], then we need not believe that every falsification is inconclusive" (Laudan, 1965/1976, pp. 155–156). Then, there is no logical foundation to sustain the position that crucial experiments are impossible in physics.

3.2. The Charge of the Falsity of Duhem's Thesis

Moreover, according to Grünbaum, Duhem's thesis is not only *non-seqitur*, but it is also false (Grünbaum, 1960/1976, pp. 119–127). In order to prove this claim, Grünbaum offers some examples of a theory from physical geometry as conclusively falsified. The arrow of his attack is directed precisely to Duhem's claim that, "[*no*] *one* constituent *hypothesis H* can ever be *extricated* from the ever-present web of collateral assumptions so as to be *open* to *decisive refutation* by the evidence as part of an *explanans* of that evidence, just as no such isolation is achievable for purposes of verification" (*ibid.*, p. 117).

According to Grünbaum, a counterexample for that assertion comes from the development of physical geometry, where a simultaneous falsification of Euclidean geometry seemed to confirm spherical geometry as well. This falsification was possible in terms of an assumption of merely one (considered) auxiliary hypothesis (A) – expressed by, "a proposition about the perturbation-free characteristic of solid rods" (Laudan, 1965/1976, p. 160), as being true.

The key point of Grünbaum's argument rests on an assertion, with physical geometry being the employed example, where an auxiliary assumption (A) is to be supposed with a high probability as being true. Additionally, there is no other auxiliary hypothesis or assumption that is tested (within a theoretical setup) in the experiment. With this, assumption (A) is asserted as true in virtue of independent empirical evidence.

Thus, in the case of the experiment in question, when a noted empirical evidence ($\sim O$) is in contradiction with a prediction (O) of a target hypothesis (H), and only one considered auxiliary assumption (A) is regarded

as true, it is possible to conclusively falsify the target hypothesis, i.e. as in the case of Euclidean geometry. Schematically,

$$[(H \,\&\, A \rightarrow O) \,\&\, (\sim O \,\&\, A)] \rightarrow \sim H$$

Furthermore, there is no need to introduce another auxiliary hypothesis saver (A') in the aforementioned experiment by reason of a true assertion of the present auxiliary hypothesis (A). Any saver hypothesis (A') would be false because it is incompatible with (A), which is regarded with a high probability as being true. The introduction of any saver hypothesis (A') would also create difficulties in the formulation of the target hypothesis itself, i.e. in the Euclidean geometry of Grünbaum's counterexample.

Therefore, according to Grünbaum, the development of physical geometry provides us with a decisive example of a successful crucial experiment. Thus, there is at least one factual case from empirical science that undermines Duhem's arguments.

4. A Defence of the Duhemian Thesis

In the proceeding section, I propose a defence of the Duhemian thesis by emphasising that in fact Grünbaum's criticism passes a point of Duhem's arguments and rests on a misinterpretation of the proper range of Duhem's claim. In order to obtain this goal, it is helpful to follow the arguments offered by Laurens Laudan, Gary Wedeking, and Roger Ariew in their objections to Grünbaum's criticism.

According to Laudan (1965/1976, p. 165), Wedeking (1969/1976, p. 178), and Ariew (1984, p. 316), Grünbaum seems to mislead the position of interests of Duhem and in a description of scientific practice, he shifts the *onus probandi* to a scientist who refuses to accept a disconfirmed hypothesis (H) as false (and tries to save it by changing an auxiliary hypothesis (A')). But, in my opinion, Duhem did not sustain such a strong claim, but a weaker one, namely (echoing the terminology of Grünbaum) that a scientist who denies the target hypothesis (H) must be able to demonstrate that there is no auxiliary savable hypothesis (i.e. such a hypothesis that would make a target hypothesis compatible with its empirical prediction and contradictory empirical data). But, according to Duhem, this is in fact not possible in virtue of the ambiguity of the falsification procedure itself.

To support this opinion, it will be helpful to recall Duhem's arguments from the first section of this essay. According to Duhem, a rejection of cru-

cial experiments relies on the ambiguity of the falsification procedure. Every experiment deals with a whole theoretical setup, which contains a target hypothesis and a wide range of assorted auxiliary hypotheses and assumptions according to the circumstances of the experiment, proper functioning experimental instruments, and background (often tacit) physical theories. Therefore, when an empirical prediction of a target hypothesis is in contradiction with observed empirical data, one is only warranted to assert that at least one of the set of hypotheses and assumptions is disconfirmed or false, but it is impossible to determine exactly which one is false. "[W]here the error lies is just what an experiment does not tell us" (Duhem, 1906/1954, p. 185).

However, "to continue to maintain H [a target hypothesis] in the face of $\sim O$ [unforeseen empirical data in contradiction with the prediction of H] is not necessarily to assert that a suitable A' [a possible saver hypothesis] *exists*, but simply to allow for the *possibility* that H may still be compatible with $\sim O$, given some suitable A'" (Laudan, 1965/1976, p. 159; see Duhem, 1906/1954, pp. 216–217). In other words, Duhem does not claim that every single hypothesis in a theoretical setup could be saved, but merely that unless we had proved that it cannot be saved, it is not falsified. Therefore, a physicist is, "logically justified in seeking some sort of rapprochement between his hypothesis and the uncooperative data" (Laudan, 1965/1976, p. 159). Otherwise, schematically, in order to assert that the target hypothesis (H) is falsified, the physicist should prove that there is no possible auxiliary saver hypothesis (A') such as:

$$\sim(\exists A')[(H \,\&\, A') \rightarrow \sim O]$$

In other words, Duhem did not sustain that it has to be an established proof that there is any possible auxiliary hypothesis or assumption, as Grünbaum has implied in his formula $(\exists A')[(H \& A') \rightarrow O']$. Duhem merely noticed that it was not warranted (logically) to deny that there is no such hypothesis (see Ariew, 1984, pp. 316–317). Grünbaum seems to miss this point: it is equally inconclusive logically to provide the proof that there is *any* (Grünbaum's reading of Duhem's claim) and that there is *no* (Duhem's claim) such saver hypothesis.

As rightly noticed by Laudan, "Duhem wanted to show, not that falsification never occurred, but that such falsification was necessarily ambiguous" (1965/1976, p. 158). Additionally, in the face of a lack of proof that there is no appropriate auxiliary saver hypothesis, a conclusive refutation of an individual target hypothesis is not possible.

In his second objection, Grünbaum charges Duhem's thesis with a falsity by demonstrating an example of a conclusively falsified theory from

physical geometry. However, this counterexample seems to be inadequate in regards to the Duhemian thesis. Following Laudan (1965/1976, p. 160) and Wedeking (1969/1976, pp. 176–179), I stress the two following points of inadequacy. Firstly, as a counterexample to Duhem's thesis, Grünbaum considers Euclidean geometry, which is in fact a whole set of hypotheses. Duhem's claim concerning the impossibility of crucial experiments (in virtue of the ambiguity of the falsification procedure in principle) challenged not a set of hypotheses, but rather a separated hypothesis. Duhem, "insisted only that isolated hypothesis – not systems of hypotheses such as geometry or classical mechanics – were non-falsifiable" (Laudan, 1965/1976, p. 160). Therefore, even if we would accept Grünbaum's example, it is not enough to declare that the Duhemian thesis is false. In fact, the counterexample passes Duhem's argument.

Secondly, according to Grünbaum, a high probability of the truth of an auxiliary assumption leads to the refutation of a target hypothesis when a set of tested theories contains merely these two elements: an auxiliary assumption and a target hypothesis. That is, if we know that an auxiliary assumption (A) is true, then we can infer that the target hypothesis (H) is false in the face of disconfirmation of a set of these two ($H \,\&\, A$) by empirical evidence ($\sim O$) – that is contrary to an empirical prediction (O) of the target hypothesis. Schematically, when $[(H \,\&\, A) \rightarrow O \,\&\, \sim O] \rightarrow \sim(H \,\&\, A)$, and A is true, then $\sim H$ (in virtue of the *modus tollens*). However, "highly probable" is not the same as "known to be true." There is no logical ground for an interference from the premise of a high probability to a conclusion of a true value of an assumption. Moreover, as noted by Wedeking (1969/1976, p. 177), Grünbaum's objection rests on "probabilities" and not "certainties." With this, Grünbaum is not justified in assuming that auxiliary assumptions can ever be "inductively certain." The high probability of auxiliary assumptions allows us to infer a low probability of a target hypothesis. But, even in this case, the probability of an auxiliary assumption is merely an approximation of truth (i.e. it never achieves the value of truth in virtue of its bivalence). Therefore, the simultaneous probability of a target hypothesis is never equalized to being false (i.e. has a value of being false in virtue of its bivalence).

Grünbaum's interpretation of the Duhemian thesis is strikingly similar in some respects to Quine's holistic thesis. What is remarkable is that there are numerous references in contemporary philosophy and sociology of science to the so-called Quine-Duhem thesis (not separately to Duhem's or Quine's theses). It should be noted that Quine's holistic thesis of indetermination (or the ambiguity of falsification) is stronger than Duhem's thesis (see

Giannoni, 1967/1976; Wedeking, 1969/1976; Hesse, 1970/1976; Vuillemin, 1979; Ariew, 1984; Gillies, 1993; Weinert, 1995; and Weber, 2009). Following Gillies (1993), we can notice that there are three major differences between the respective thesis of Duhem and Quine, namely: (1) Duhem limited the scope of his thesis to just physics, whereas Quine's thesis is supposed to apply to any branch of human knowledge; (2) Duhem maintained that hypotheses in physics cannot be tested in isolation, but only as a part of a whole set of hypotheses, but this set is limited and in this sense operational, while for Quine, the set, "extends and ramifies until [it] includes the whole of human knowledge" (*ibid.*, p. 111); and (3) Duhem offered a theory of "good sense" as a criterion of the refutation of a theory, whereas Quine sustains that, "[a]ny statement can be held true come what may, if we make drastic adjustments elsewhere in the system" (Quine, 1951, p. 43). I think that Grünbaum misses these differences, in particular the third one, and ascribes to Duhem a claim that in fact is sustained by Quine.

5. The Pragmatic Dimension of Theory Choice

As we have seen in the previous section, the most serious puzzle for the Duhem-Quine thesis is the selection of competing theories in the face of a lack of crucial experiments. If experience is not enough for choosing one theory and the competing theories have the same empirical consequences, a new tool is needed to make a theory choice. For Duhem, this tool is so-called "good sense." But what exactly is "good sense"?

The most extended account of "good sense" we can find in a slightly neglected Duhemian lecture series from 1914, entitled *La science allemande*, and published in 1915 (hereafter quoted in the English translation as the *German Science* (1915/1991)). Duhem, comparing the French and German scientific practices, focused heavily on an example of Louis Pasteur (1822–1895). What is important for him, comparing a more deductive German tradition with a more intuitive French tradition, is an actual scientific practice. For Duhem, "good sense" was a combination of deductive reasoning and experimental methods with a "spirit of criticism and ingenuity." Paster's scientific practice was an ideal example of this spirit for Duhem:

> In this work [of Paster] of successive improvements upon an initial necessarily rash and often false idea which finally leads to a fruitful hypothesis, the deductive method and intuition each play their role. But how much more complex and difficult it is to define their roles than it is in a science of reasoning [...]. In another way, again, good sense will intervene at the moment at which one

> realizes that the consequences of a preconceived idea are either contradicted or confirmed by the experiment. This realization is in fact far from being entirely simple; the confirmation or contradiction is not always explicit and straightforward, like a simple 'yes' or 'no' (Duhem, 1915/1991, pp. 22–23).

Hypotheses, experiments, and deductive reasoning must, "be assisted by a certain sense of soundness that is one of the forms of good sense" (*ibid.*, p. 23). A heuristic element of the elimination of one hypothesis and its subsequent replacement by another is not a matter of simple deductive or quasi-deductive reasoning. This elimination is a more complicated process of abandoning preconceived ideas and to being open to the appearance of new ones, with these new ideas sometimes being completely original and unconsidered:

> Here it is necessary (Pasteur excelled in doing this) to pay attention to what each of the observations which have condemned the initial idea suggests, to interpret each of the failures which destroyed that idea, and to synthesize all these lessons for the purpose of fabricating a new thought which will pass once again under the scrutiny of the actual results. What a delicate task, concerning which no precise rule can guide the mind! It is essentially a matter of insight and ingenuity (*ibid.*, p. 24).

Duhem then suggests that "good sense" is probably echoing the most Pascallian "intuitive mind [*esprit de finesse*]" (*ibid.*, p. 25) in the heuristic process of scientific discovery.

According to the most recent interpretation of Duhem as a virtue epistemologist (see Stump, 2007; Carrier, 2011; Fairweather, 2012; and Ivanova and Paternotte, 2013), "good sense" could be understood more precisely as a set of virtues or skills that scientists use to make their decision while choosing one theory over competing theories. This suits well with the above interpretation of Duhem's "good sense" in Pascaline terms as the set of appropriate virtues for making a choice that contains both intellectual and moral skills. In other words, as in Pascal's "intuitive mind" echoed by Duhem in his "good sense" understanding, to make a theory choice a scientist needs to have intellectual abilities as well as an appropriate attitude in his will. As observed by Stump, "[s]cientists must have intellectual and, indeed, moral virtues in order to reach scientific knowledge, especially when choosing between empirically adequate theories" (2007, p. 150).

In my opinion, this virtue epistemology description of "good sense" should be supplemented by a pragmatic genealogy of scientific virtues. Duhem pointed out on various occasions that "good sense" as a set of virtues or skills could be originated and cultivated by every scientist only through

scientific practice. Duhem himself was a scientific practitioner and moreover he analysed scientific practices over the ages in his monumental works concerning the history of science.

Following Ivanova (2010, p. 60), we can notice that Duhem identified the numerous criteria used in practice by scientists to select a proper theory from competitors on various occasions. These criteria might be "the scope of the theory, the number of hypotheses (Duhem, 1996, p. 22), novel predictions (Duhem, 1954, pp. 28, 195), comparability with other theories (*ibid.*, pp. 221, 255), simplicity, unification into a single system of hypotheses (*ibid.*, p. 294), and elegance and convenience" (Ivanova, 2010, p. 60). However, there is no one unified configuration of these criteria used for theory choice. Criteria are used in a flexible way appropriate to circumstance and are guided by "good sense." Thus, "good sense" is a set of skills obtained through the scientific practice that gives a scientist both a specific sensitivity and perception of the analyzed problem and is a specific character used in order to make a theory choice even if external empirical circumstances are not helping a scientist as crucial experiments do. This resonates with Aristotelian virtue ethics very much.

According to Aristotle, human beings obtain a specific moral perception only through moral practice, that is in obtaining proper moral virtues (skills) through the means of the practice thereof (see Szalek, 2010, p. 359). As there is no direct reference to moral values in the world, moral perception is guided and modelled through a process of moral character building, and this process consists of practicing and accommodating moral virtues. This pragmatic element, described by Aristotle in terms of moral education, is crucial for virtue and character formation. I argue that if we take into account this pragmatic enhancement of virtue theory, we can understand the proper meaning of the Duhemian "good sense." This pragmatic element of Duhemian thought is something which I think was neglected in recent literature concerning Duhem's theory choice. This has separated the understanding of "good sense" as virtue epistemology of theory choice from the pragmatic dimension of its formation. However, if we combine them together, it becomes obvious why Duhem's holism coincided so well with the Quine's alleged methodological pragmatism.

For Duhem, "good sense," as a tool for choosing one theory over competing ones, might be understood as a set of virtues that scientists should possess in order to be able to make a sound selection between theories. Choices can be made by means of various criteria of selection, which are chosen each time independently by a scientist who considers circumstances and is not limited by any choice algorithm (see Duhem, 1906/1954, p. 288).

To obtain such skills of selection, which means to form a scientific character that enables a scientist to choose, a scientist needs to be engaged in the practice of the scientific community. Only through this practice can a scientist obtain the set of skills that could in turn model a sort of flexibility of one's scientific character in regards to the appropriate criteria chosen as a guide for the selection of a proper theory in light of changeable circumstances. A good example of such scientific practice was given by Duhem in the said case of Pasteur.

In properly understood "good sense," we have then three elements: (i) appropriate scientific intellectual and moral *virtues*, (ii) an appropriate scientist's *character* modelled by these virtues, and (iii) the scientific community *practice* that forms virtue and character. In other words, Duhem by demonstrating that there is no one firm tool of theory choice in the form of a crucial experiment, simultaneously proposed a scientific agent centred tool for such theory choice that followed a specific combination of an agent's epistemic virtues and the process of acquiring epistemic virtue through scientific practice. The latter component seems to be a supplementary, neglected second part of the Duhem-Quine thesis.

This understanding of the thesis enables us to preserve both the tenets of the virtue epistemology interpretation of "good sense" of Stump (2007) and the ideal scientist model reading of "good sense" of Ivanova (2010). As rightly observed by Stump, for Duhem, a scientist makes his decision about a theory while relying on "the intellectual and moral virtues of the scientist as a cognitive agent" (2007, p. 151). In this interpretation, scientific knowledge requires the intellectual virtues of the scientist that enable him to decide about a choice of the particular theory in question. It seems that epistemic virtues like moral virtues could be treated here as essential skills that are needed for making a decision in the face of a lack of the possibility of empirical verification of a theory by crucial experiments. However, the process of practicing science by repeating specific operations teaches scientists not only needed skills but also, through acquiring those skills, forms the character of a scientist. This character-forming process resonates with the ideal scientist as a model for scientific practitioners in the sense proposed by Ivanova (see 2010, pp. 63–64). This is, however, an end of the pragmatic formation process, but not as an initial ready-made goal that scientists want to achieve by following some codified formal model of the perfect scientist. As the Duhemian example of Pasteur demonstrated, there are different skills that are required in different circumstances of experimental scientific work, and it is up to the pragmatically formed character of a scientist to respond with flexibility to such circumstances. In other words, virtue epistemology

is supplemented here by a pragmatic explanation of appropriate scientific epistemic and moral virtues.

6. Conclusions

Duhem's criticism of crucial experiments is, in my opinion, well qualified and moderate. Duhem seems to express his objections in a plausible and warrant manner. Here I substantiate a weaker interpretation of Duhem's thesis by emphasising the following elements of his considerations concerning crucial experiments. Firstly, Duhem limited his arguments to only the scientific procedures of physics, taking this discipline as an example of an empirical science at an advanced stage of theoretical development (Curd and Cover, 1998, p. 361; Duhem, 1906/1954, p. 3). Secondly, Duhem was interested in the actual practice of research in this discipline. Presumably, this is the reason why the target of his criticism was directed at the idea of the crucial experiment (with this being one important instance of this practice). In Duhem's account, the crucial experiment was an extreme position, which holds that experiments could be established as a probe to falsify theories conclusively, merely by virtue of the deductive method (by the means of the reasoning of *modus tollens* and *reductio ad absurdum*), while simultaneously confirming a rival theory as true and certain. Duhem rightly noted the logical constraints and inadequacy of such an empirical science as physics, and boldly described the pragmatic attitude of scientists in the face of unexpected empirical evidence. Duhem considered possible ways for the protection of some target theory from refutation through the modification of one or more auxiliary assumptions within a tested (set of) theory.

However, Duhem did not assert anywhere in *Aim and Structure* that some theory could not be assessed as more confirmed than another. This conclusion would lead him directly to a sceptical-like conclusion about scientific practice in physics and would deny the progress of science. In actuality, Duhem aimed to offer a description of the, "method by which physical science makes progress" (Duhem, 1906/1954, p. 3). Moreover, in his ordinary practice as a physicist, Duhem had to opt for one theory rather than another – e.g., he supported the wave theory of light as better confirmed (Curd and Cover, 1998, p. 361; Gillies, 1993, pp. 104–107). In fact, Duhem left the possibility that some experiments could lead to a rejection of some theories as disconfirmed. Theories could be refuted partly as a result of an experiment, but not in the same (deductive) manner as happens in mathematics.

Logic alone is not suitable in physics for the abandonment of one theory and the acceptance of another. Instead of logical tools, Duhem proposed the concept of "good sense" as, "the judge of hypotheses ought to be abandoned" (Duhem, 1906/1954, p. 216). It is significant that Duhem's considerations about "good sense" were presented as the concluding remarks of chapter six of *Aim and Structure*, where he discusses the impossibility of crucial experiments. I think that neglecting this part of Duhem's considerations could lead to a misinterpretation of the range of his criticism. It is true that Duhem argued against crucial experiments in virtue of the ambiguity of experimental procedure. But he did not refuse to abandon some theories in favour of others in virtue of "good sense" (Jaki, 1984, p. 351).

> Pure logic is not the only rule for our judgements; certain opinions which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable. These motives which do not proceed from logic and yet direct our choices, these 'reasons which reason does not know' and which speak to the ample 'mind of finesse' but not to the 'geometric mind' constitute what is appropriately called good sense (Duhem, 1906/1954, p. 217).

Logic by itself is not able to help a scientist establish one theory rather than another: "the choice between the various hypotheses is entirely open" (Gillies, 1993, p. 106). The scientist, in his decision, must be conducted by "good sense" in terms of the judgement of whether a hypothesis is *ad hoc*. By "good sense," Duhem understood not a logical ability, but still a rational one, "something like intuition, as opposed to deduction – *raison* as opposed to *raisonment* [...] where good sense is characterised as that which allows one to intuit the truth of axioms in the deductive science [...] and that which allows one to intuit fundamental principles or assumption in experimental science" (Ariew, 1984, p. 317). Although this concept could seem to be a little bit ambiguous, one thing is clear. The remarks of Duhem's proposal concerning the assessment of theories guide us to conclude that he did not hold the strong version of the anti-falsificationist thesis that was ascribed to him by A. Grünbaum. With this, Duhem's proposal escapes refutation and criticism of the Grünbaum-like kind. According to Duhem's proper position, the impossibility of crucial experiments is more plausible and modest than as was the target of attack.

Moreover, although in a draft form, Duhem's concept of "good sense" seems to be surprisingly modern and instrumental in regard to current scientific practice. This "good sense" may be understood as a set of intellectual and moral virtues that scientists should possess in a pragmatic way when choosing between competing theories. The pragmatic dimension of this way

of theory choice guarantees flexibility as regards the appropriate criteria that must be chosen as a guide for the selection of a theory. The interpretation proposed in this paper emphasised three elements of such virtue epistemology and pragmatic interpretation of "good sense" as theory choice: (i) appropriate scientific, intellectual, and moral *virtues*, (ii) an appropriate scientist's *character* modelled by these virtues, and (iii) the scientific community *practice* that forms virtues and character. For Duhem, theory choice consisted of a specific combination of an agent's epistemic and moral virtues and the process of the acquiring thereof through scientific practice. As such, theory choice should be regarded as a pragmatic supplement of the Duhem-Quine thesis.

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N O T E S

[1] Duhem's essential works concerning the history and philosophy of science include: *La théorie physique, son object et la structure* (Paris, 1906), *ΣΩΖΕΙΝ ΤΑ ΦΑΙΝΟΜΕΝΑ: Essai sur la notion de théorie physique, de Platon á Galilée* (Paris, 1908), *Études sur Léonard de Vinci*, 3 vols. (Paris, 1906–13), and *Le système du monde. Histoire des doctrines cosmologiques de Platon à Copernic* (Paris, 1913–59).

[2] The term "crucial experiment" was coined by Francis Bacon in the Book II, section 36 of his *Novum Organum* (1620/1994). In analogy to the cross made when two roads meet and the subsequent need to point out directions ("instantiae crucis"), he held that there are some experiments that promote the holding of some theory as true from a group of rival theories (see Achnistein, 1998, p. 1). Although having seventeenth century origins, the term found wide use in the nineteenth century. "In nineteenth-century chemistry, geology, optics, and thermodynamics, much use was made of what were thought to be crucial experiments. The notion of a crucial experiment, and the rigorous empiricism which accompanied it, were particularly common among those scientists who saw themselves in the experimental tradition of Newton, Bacon, and Lavoisier. But the phrase 'crucial experiment' enjoyed even wider currency in nineteenth-century philosophies of science than in science itself; perhaps it was so compatible with that century's view of scientific progress as the evolution from 'false' theories to 'true' ones, crucial experiments being the instrument whereby the wrong was separated from the right" (Laudan, 1965/1976, p. 156).

[3] In this case and hereafter, I follow a way of schematic explication of the Duhemian arguments that could be found in the literature on Duhem (see for example, Curd and Cover, 1998; Gillies, 1993; Laudan, 1965/1976; and Grünbaum, 1960/1976). However, I adapt this to the aim of this paper, appropriately changing the notation of their formalizations.

[4] The two strongest attacks on the Duhemian thesis were done by Adolf Grünbaum and Karl Popper. However, merely Grünbaum offered explicit criticism. In the case of Popper, his criticism was rather a marginal part of his systematical conceptions of the philosophy of science and was presented mainly implicitly (see Harding, 1976, p. XIII). Then, due to the subject of this paper, I pay attention solely to Grünbaum's case. If I did otherwise, it would be necessary to compare the conceptions of Popper and Duhem, which is definitively out of the scope of this paper.

[5] In the above quotation, in order to use a uniform form of notation in this paper, I have slightly modified Grünbaum's original formula from the following: $[(H \cdot A) \rightarrow O] \cdot \sim O$, to my version: $[(H \,\&\, A) \rightarrow O] \,\&\, \sim O$.

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