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The epistemological limitations of scientific inquiry: Crucial experiments revisited

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Abstract
Scientific knowledge is the outcome of research and experimentation. How this goal is to be achieved is and has been a topic of much debate over the centuries. In the 17th century, the English philosopher and scientist Francis Bacon (1561-1626) regarded science as the systematic collecting and gathering of facts. Upon this view, nature tells us what the facts are on the assumption that we learn how to listen properly to nature. We design experiments and run them, and the verdict is to nature. Thus, "crucial" experimentation is the hallmark of science. A crucial experiment is an experiment that unambiguously favours one or the other of two competing hypotheses under test, no matter the outcome. At some stage in the development of science, it was believed that such crucial experiments can be designed. However, some philosophers deny that they are possible in some domains of scientific inquiry, while others deny that they are possible in any domain of scientific inquiry. In this context, the French physicist, historian and philosopher of science Pierre Duhem (1861-1916) argued that in physics, crucial experiments are not possible. While the American philosopher W. V. Quine (1908-2000) adopted the radical view that crucial experiments are forthcoming in no domain of scientific inquiry. In the philosophy of science, these two views are joined in what came to be known the **Quine-Duhem Thesis**. This study examines the controversial **Quine-Duhem Thesis** and discusses whether, or not, it is considered a threat to the rationality of science. The fundamental features of rationality in science are to be addressed. It is concluded that the Quine-Duhem thesis does not make an epistemological point and Quinean holism does not destroy the objectivity of science.

Keywords: Holism, under-determination, scientific inquiry, crucial experiment, rationality

I. Introduction

I. 1 Philosophy and Experimental Sciences

About the importance of experimentation in science, Duhem and Quine both made similar claims. We now refer to this as "The Duhem-Quine Thesis" as a result of this. However, their actual emphasis was very different.

When many philosophers discuss experimental sciences, they only consider those that are still in their infancy, such as physiology or certain branches of chemistry, where experimenters base their conclusions solely on the facts by applying a method that is nothing more than common sense given more consideration and where mathematical theory has not yet developed its symbolic representations. Simple experiments and observations that don't rely on other hypotheses can be helpful for developing sciences. However, these experiments have already been done in more developed fields. We already know that if we drop an apple, it will fall, but we may not be aware of what occurs if we sever an animal's nerve root. The earlier illustration suggests that mechanics is at least partially more developed than Duhem claims. We are aware of phenomena like gravity (at least in their commonplace terrestrial manifestations) because of our informal and inevitable observation of the world, or just because our eyes are open.

However, the types of experiments physicists really do when researching mechanics are significantly more complex than just dropping an apple. They need prior understanding of physical constants as well as the connections between physical forces (such as gravity) and attributes (such as mass and velocity). To construct an experiment with any real level of sophistication, these ideas must be conveyed using the "symbolic representations" of mathematics.

Thus, physics, or at least its developed subfields, is subject to the Duhem thesis. Duhem asserts that mathematization is a process that disciplines often go through at some point, and it is true that other sciences, such as biology, today rely on mathematical formulations of generalized ideas considerably more than they did in Duhem's day. Thus, compared to one hundred years ago, new biological hypotheses are considerably more likely to rely on supporting theories, and as a result, fall outside the scope of the Duhem thesis.

I.2 Auxiliary Theories and Crucial Experiments

Physics experiments often involve the use of physical instruments, which can only be read and comprehended in the context of supplementary theories in addition to the cognitive equipment offered by those theories. The thermometer, for instance, relies on both a theory of temperature and the assertion—which is probably too basic to be referred to as a theory—that a thermometer's appearance and the temperature of its environment are related. Even while Duhem admits that experiments in other disciplines rely on instruments, he contends that "the [auxiliary] theories used, as well as the instruments employed, belong to the domain of physics." When using their instruments, other scientists like chemists and biologists rely on the theories of physics, a different science, whereas physicists rely on the theories of their own subject.

As Gillies claims, this distinction appears to be philosophically meaningless. He writes, there are undoubtedly theories in physiology and chemistry that are vulnerable to the Duhem thesis." (Gillies, 1993). Duhem, though, acknowledges as much. "From the standpoint of logic," he says, "the difference is of little importance; for the physiologist, chemist, as well as for the physicist, the statement of the result of an experiment implies, in general, an act of faith in a whole group of theories."" Duhem expressly contrasts physics with physiology rather than applying his theory to both physiology and chemistry. In a physiological experiment, Duhem anticipates that the hypothesis being tested will be physiological, but all supporting theories—if any—will be physical. If the experiment is unsuccessful, the physiologist can point the finger at either a seemingly cutting-edge physiological theory or a tried-and-true physical theory. In Duhem's thinking, the development of physics is taken as axiomatically supporting evidence for the plausibility of physical hypotheses. Since the maturity of a science does not imply the maturity of each constituent theory, this maturity does not necessarily support a particular physical theory in the same way, but it is likely that the instruments used by physiologists in the early 20th century did not depend on recent or speculative physical theories.

In any event, Duhem considered that his theory applied to certain experiments that weren't physics-related, but he also believed that other experiments in emerging sciences, like severing a nerve root, were so fundamental that they just required "common sense brought to greater attention."

The Duhem thesis's assertion that its implications transcend beyond the confines of the natural sciences—that a physiological investigation may be dependent on supplementary physical theories, for example—can be significantly strengthened. As an illustration, consider W.V.O. Quine's dictum that "the unit of empirical significance is the whole of science." The Quine thesis is not implied by the Duhem thesis, though. Duhem asserts that the knowledge of the equipment used in the experiment depends on auxiliary theories, while Quine asserts that all auxiliary theories are necessary. The former is not only more tenable, but also more useful: if the Duhem thesis is true, a scientist who is aware of the theories on which his or her apparatus depends can list those theories, and if an experiment fails, he or she and their colleagues can try additional experiments and observations to figure out which theory, primary or auxiliary, is incorrect. The failure of an experiment, in contrast, would just demonstrate that something in science is flawed, according to the Quine thesis, and would not reveal any knowledge on how to correct it.

Early twentieth-century scientific instruments are not the same as those used in the early twenty-first century. They consist of "instruments" such as genetically modified lab animals, whose use is essentially based on biological and biochemical rather than physical ideas. As a result, it is now conceivable for the Duhem thesis to work in biology just as extensively as Duhem claimed it did in physics, and for a test of a biological hypothesis to depend on biological auxiliary theories.

The Duhem thesis's scope has therefore been widened as a result of advancements in science during the past century. But Duhem's arguments continue to be strong. The Duhem thesis itself continues to be a source of insight into what scientists can and cannot learn through experiment and observation, despite the extreme adversaries it faces, such as the Quine thesis, which questions our capacity to even establish the validity or untruth of ideas.

Taken literally, The Duhem thesis is the idea that "an experiment in physics can never condemn an isolated hypothesis but only a whole theoretical group." It was first put out by Pierre Duhem. The Duhem thesis has been interpreted in many different ways since Duhem released his study. I'll pay particular attention to his assertions on theoretical holism.

Duhem contends that physicists must rely on theories other than the one they plan to test in a particular experiment. The employment of physical and conceptual scientific instruments necessitates the application of auxiliary theories. It is difficult to govern a single instrument or to interpret a single reading without theory, according to Duhem. Any experiment with such apparatus is a test of the theories on which it is based. The usage of a scientific tool like a thermometer or a principle like inertia itself depends on theories. Therefore, a physicist should not automatically conclude that a hypothesis is false if they use it to predict the outcome of an experiment and the prediction turns out to be false; the prediction could also be false if one of the auxiliary theories is incorrect. What he discovers, according to Duhem, is that at least one of the assumptions that make up this group is invalid and has to be updated; however, the experiment leaves it unclear which one needs to be adjusted. (Duhem, 1954, 187)

The Duhem thesis is controversial for several reasons, including whether it should actually be limited to physics as Duhem claimed. Should Duhem's emphasis on physics specifically be disregarded, or should his claim that no experiment in any domain can "condemn an isolated hypothesis" be taken at face value?

According to Donald Gillies, Duhem is correct to limit the scope of his argument, but incorrect to associate it with that of a certain field of research, namely physics." (Gillies, 1993) Gillies is correct in saying that the Duhem thesis is not now appropriate for physics. However, given the state of knowledge at the beginning of the 20th century, Duhem had excellent justifications for limiting the subject matter of his thesis to physics. The field to which the Duhem thesis is applicable has altered as a result of the sciences' changes over the past century.

Like modern physics, the physics of Duhem's era had features that set it apart from other disciplines. Two of these differences are highlighted by Duhem himself: the development of physics into a mature mathematical science and the use of physical theories as the foundation for the design of scientific equipment. These were two features that physics retained in Duhem's day far more firmly than other disciplines; they were also significant differences. Since then, physics has lost some of its originality in each of these areas, while it still exhibits these characteristics more overtly than, say, biology. These characteristics have emerged in various fields, expanding the scope of the Duhem thesis.

The main factor Duhem used to distinguish physics from other sciences was that it was an established field of study. It had its most profound changes with Isaac Newton's "Philosophiae Naturalis Principia Mathematica" almost two hundred years before Duhem, therefore it had more time to advance than other sciences. Less than 50 years have passed since Charles Darwin published "On the Origin of Species by Means of Natural Selection," and biology has experienced significant theoretical changes since Duhem, including a number of DNA-related discoveries. Science in biology has advanced in sophistication.

The twentieth century saw significant breakthroughs in physics as well, including the discovery of relativity and quantum mechanics. These discoveries make one wonder how developed physics actually is. Despite hundreds of years of seeming advancement and theory creation, many fundamental concerns remain unresolved. Although physics may appear less mature now than it was in the early 20th century, it has still developed more than other natural disciplines and is, in some ways, more mature.

The type of this maturity need not be unclear because Duhem himself explains how maturity relates to his thesis at the start of his Work.

2. The Quine-Duhem Thesis

A Quine-Duhem The topic of thesis has generated a lot of discussion and controversy. It asserts that any observable data that initially appears to be contrary to a theory can always be accommodated.

(Klee, 65) The rationality of science is therefore threatened since it is denied that a practitioner may be driven to reject a hypothesis by the data itself. This thesis states that people, not nature, are in control since we make the inference that a hypothesis is false based on the evidence, not nature. If we adopted a comprehensive view of scientific ideas, evidence might be made to support the hypothesis being tested. According to a holistic view of theories, a theory's concepts and laws create one interconnected network or system. Therefore, a substantial body of theory rather than a single prediction is what is being tested experimentally. By changing other assertions in the substantial body of theory to which it is related, it may be possible to salvage a single claim that is being tested.

Therefore, there are alternatives to rejecting the claim that seems to be immediately under test in order to account for apparently contradictory data. The Quine-Duhem Thesis's key realization is this. Without switching to a more comprehensive model of scientific ideas, it is impossible to get this vision.

What has been observed thus far provides evidence for a hypothesis, which is then tested by what is predicted to be observed next.The emphasis is on the relationships between a theory and the data, with less focus on relationships between theories themselves. This may provide a fundamental understanding of how we learn about nature and the justifications for believing what we do, but it offers nothing about how we learn to comprehend nature. Understanding and knowledge are two distinct but equally significant scientific achievements.

When correctly interpreted, Duhem's and Quine's holistic theses provide methodologically responsible methods of resolving a contradiction between a theoretical system and experience; they only rule out the possibility of doing so in a way that is epistemically compelling.

The "Duhem-Quine thesis" contends that because scientific testing are comprehensive, rejecting a specific hypothesis as a result of a negative experience is fundamentally meaningless. Making the necessary alterations "elsewhere in the system" will always allow one to maintain the current hypothesis. But given the numerous discrepancies between Duhem's and Quine's formulations of the holism problem that have been made clear by subsequent debates (Harding 1976, Vuillemin 1987, Krips 1982, Ariew 1984), the term "Duhem-Quine thesis" seems misleading.

There are certain objections to Duhem and Quine's holist theses that may be viewed as antiholistic justifications. Neither Duhem nor Quine contended that a false hypothesis could be maintained permanently or made unchallengeable at will. According to Duhem, such a theory is meant to ultimately fail alongside the system in which it is ingrained, "under the weight of the contradictions inflicted by reality on the consequences of this system taken as a whole." (Duhem [1906] 1954, 216)

The diversity of scientific tests or beliefs can theoretically be upheld "come what may," but in some cases, where a theory can only be upheld "at the cost of systematic waiving," the entire body of beliefs comprising it can be logically rejected (Quine and Ullian 1978, 32) of supporting theories used to understand contradictory facts. A technique like this would eventually lead to the entire system being "an unreliable instrument of prediction and not a good example of scientific method." (ibid., 32). This would make it impossible to identify "the weak link that compromises the entire system." (Duhem [1906] 1954, 216). However, this does not imply that the holistic viewpoints may prevent scientific advancement.

In the face of conflicting evidence, scientists are successful in determining which hypothesis to reject. We may consider it childish and unreasonable for the physicist to insist obstinately at all costs, at the cost of ongoing repairs and numerous complicated stays, that the worm-eaten columns of a building tottering in all directions are stable when by demolishing these columns it would be possible to persuade the public that the building is stable despite the fact that it is in fact unstable fact that such decisions, according to Duhem, are based on the "good sense" of physicists. (Ibid., 217) The same goes for Quine. What are the "considerations of equilibrium" mediating, though tangentially, the relationships between specific experiences and certain theoretical system statements? Both "conservatism" and "the quest for simplicity" are mentioned in them (Quine 1953, 46). Elsewhere Quine also included humility, generality, refutability, and accuracy as criteria for evaluating theories in addition to conservatism and simplicity (Quine & Ullian, 1978, chaps. 6, 8).

The objectivity of science is not threatened by these factors. As a result, Duhem and Quine value the liberty of scientists to choose how to proceed in each specific situation. Such choices are epistemically ambiguous due to their overtly pragmatic nature. The antiholistic "counterpoints" that are periodically audible through the central holistic themes of Duhem and Quine, however, can be minimized, and they can be expanded into the leitmotif of scientific reason. Popper has made some suggestions on how this may be accomplished. (1959, 1963), Lakatos (1978), and Greenwood (1990). Additionally, a model of scientific testing may be used to unite and strengthen diverse antiholistic arguments. The "String Model of Scientific Tests," as it is known, proposes that a typical theoretical system consists of a "core," a region of "intermediary elements," and a "periphery" that is open to input from the outside world. (Balashov, 609)

3. Holism vs. Antiholism

It is possible to view approaches to dealing with holism as attempts to develop methodologically sound procedures that would make it possible to discriminate between "strain conductors" and "strain recipients," as they are known. In light of the latter's rebellious history, the former will then be deserving of retention, whilst the latter will be a candidate for replacement.

A certain type of antiholistic arguments, associated with Popper's falsificationism (Popper, 1959, chapter 1), is centered on identifying the components of a theoretical system that are seldom identifiable as "strain recipients" in a negative manner. Their power in the string model originates from independent corroboration, which they enjoy through taking part in a number of strings with positive examples of the system under test as well as, generally, in the positive instances of other theoretical systems.

The search for "weak spots" is narrowed by identifying the specific "basic statements" and pertinent pieces of background information that can be made unproblematic in the presence of a specific piece of refractory evidence. However, by itself, this might not be enough to make the tests conclusive. Because just a little portion of the task—the negative separation of those items that are scarcely "strain recipients"—is necessary. In general, it should be finished by positively identifying the responsibilities that the remaining components perform. Such identification was not specifically included in Popper's original scheme.

The "Duhem-Quine thesis" was viewed in this program as a notion accountable for a certain sort of scientific activity that is incompatible with the falsificationist code of rationality and should thus be avoided, rather than as a different issue in and of itself. According to this viewpoint, a scientist should typically be more concerned with the feasibility of such a distribution of these roles that might guide modus tollens to a chosen element than with the identification of the roles performed by various parts of a theoretical system. The goal of this approach was to achieve demarcation. Its validity stems from the fundamental necessity that the roles be assigned before the test is administered rather than after the findings are known, therefore rejecting "exactly those ways of evading falsification which... are logically admissible." (Popper, 1959, 16)

This criteria is partially satisfied by the independent support received by some system under test components. Despite being insufficient on its own, it cannot disprove the whole explanation of testing. Thus, any effective anti-holistic method appears to need allowance for such a support. What counts is how much independent support the related parts of a theory have. Examining the set of positive-instance strings in which the relevant components are contained is one approach to evaluating this. The antiholistic approach will include such an examination heavily.

Greenwood (1990) created a new antiholistic defense. He stated that because the theories being tested ("explanatory theories") were first founded on specific "exploratory theories," the specific roles that certain factors played in the past support of a theory sometimes exclude them from being strain receivers in later instances of testing. If the latter are permitted to be altered to account for obstinate observations, the theories being tested will consequently lose the earlier support that they had.

Greenwood cites, as one example, the abnormal precession of Mercury's perihelion, which LeVerrier first measured in 1849 and which could not be explained by doubting the validity of modern telescopic observations. (ibid., 565-566) Because this would seriously undercut the Newtonian mechanics' preceding justification, which was partly based on meticulous studies of planetary motions.

In addition to the "independent corroboration" that each component of a theoretical system receives, Greenwood's antiholistic claims also differ from and, in some ways, are stronger than Popper's in that they have a remarkable connection to the confirmation base of a "core" theory.

Given this connection, " the widely held belief that exploratory hypotheses may always be changed in response to anomalies in order to maintain the explanatory theory's evidentiary parity with its competitors " is justified. (ibid., 567) completely incorrect. Not usually can such a change be achieved. However, this does not imply that it will never be created. Greenwood is right to point out that it depends on how one may change or replace certain "auxiliary assumptions" in order to account for stubborn facts and maintain previous evidence in favor of an explanatory theory. (ibid., 569)
A specific negative-instance string may contain a number of intermediary components, not all of which are equally involved in a core theory's confirmation foundation, corresponding to a stubborn evidence that needs to be accommodated. Thus, a creative holist may come up with a clever and inexpensive strategy to deflect the blow of contradictory data away from an explanatory hypothesis.

Lakatos supports Duhem and Quine's assertion that the hold of holism with regard to confirmation/falsification procedures in science is strong enough to be regarded seriously when we look at his antiholistic method, which varies dramatically from Popper's and Greenwood's approaches (Lakatos 1978, 98). Only by accepting certain progressive patterns of theoretical evolution as evident may the devastating consequences of holism on scientific reason be lessened. When choosing which element of the entire theoretical system to replace in the face of obstinate data, the modified system's capacity to forecast novel facts and to gain credit for such predictions through their subsequent empirical confirmation should be assessed in accordance with the progress criterion.

In other words, without trying a few options to see which ones result in content-increasing "problem shifts" and which of the latter advances us the most, it is impossible to construct any reasonable notion about which element in a string is a strain recipient that should be abandoned or replaced in light of some counter instance. The alternative that does this can thus be deemed progressive, benefiting from hindsight's many advantages. Now, one might interpret the Lakatosian criteria of progress as either a role identifier or a justifier of such identification. This criteria divides a system's components into "weak" and "strong" ones in the first instance. A component is deemed "weak" if maintaining it at the expense of other components causes the entire system to externally regress, as opposed to rejecting or replacing it, which produces empirical progress.

In contrast, a concept is deemed "strong" if rejecting it consistently results in empirical degeneration. Clearly, the goal is to determine the relative strength/weakness of the associated components in terms of the system's empirical suitability as changed by the suggested role identification. The issue with this strategy, at least in the way Lakatos uses it, is that it purports to address all methodological conundrums associated with the test circumstance.

Other criteria, such as Popper's "independent corroboration," cannot be used for role identification, according to Lakatos, because they may be deceptive, particularly when used to support a strict policy (i.e., the complete rejection of the falsified theories and hypotheses demanded by the "methodological falsificationism"; ibid., 23–31). The purpose of creating the progress criterion was to replace existing criteria, not to add to them.

According to Lakatos, the only logical method of determining the roles performed by various components of a theoretical system (i.e., a research program) is to experiment with various changes and evaluate the results using the modified system's empirical behavior. (ibid., 40-41, 45, 99). But it's obvious that this is impossible. Furthermore, even if it were feasible to attempt everything and if scientists really did, the outcomes of the trials would only ever represent the research program's short-term dynamics at each step of the role identification process. Such dynamics are not always indicative of any long-term trend needed for epistemic assessment. In actuality, scientists seldom rely on pure speculation when making their initial proposals for role identification. They are generally influenced by factors like Popper's "independent corroboration" and Greenwood's "no-go" defenses against altering certain exploratory ideas.

With these additional factors, it becomes possible to use the progress criteria not as the primary and only role-identifier but rather as a justifier for the role identification that has already been formed, even if only provisionally, on the basis of another criterion. Such a criteria is unlikely to be independent of issues with empirical sufficiency. But depending on the specifics of a test setting, it can be vastly different from and not reducible to, these factors. Let's say one is discovered. It may thus be claimed that the application of both criteria simultaneously will have more epistemic significance, if only because the well-known "no-miracle" argument is instantly raised. It must be explained why a role identification proposed by the first criterion also occurs to meet the second one (the progress criterion), which is distinct from the first. One plausible explanation is that the current system has certain epistemic advantages.
A effective antiholistic technique must be based on the joint application of some other criteria relevant to the test scenario and the criterion of empirical adequacy (including the dynamics of successful and failed predictions).

The explanation of empirical testing is overly abrupt and objective, which ignores the subtleties of theoretical effect on the findings. Science is given the opportunity to appear a little better than it actually is in this way. However, science is permitted to appear far worse than it actually is by disregarding the complexity of theoretical effect on other theories, the kinds of inter-theoretic links that are the basis of knowing. The approach appears to support a science that has a wealth of knowledge about nature but lacks comprehension of any of it. Therefore, a bigger, more comprehensive model is needed to accurately represent the scientific method. Clarifying the place of aesthetic judgment and beauty in research is also made easier by the more comprehensive explanation of the scientific process that includes a statement of inter-theoretic structure.

According to Duhem and Quine's Quine-Duhem thesis, it is always feasible to account for a prediction that doesn't work by changing or replacing the auxiliary hypotheses, causal posits, etc. that were used to derive the prediction. Although this is true in theory, doing so would frequently undermine other important features of the theory, and the penalty would be severe. (See Greenwood, 1994: 62–5 for a detailed critique with examples)

Quine embraces the notion of holistic verificationism, combining it with the claim that the theory, not the phrase, serves as the unit of confirmation. He claims that discussing the confirmation or disproof of a single statement is "not significant," but we may discuss the confirmation or disproof of a theory. Quine's attention is on the confirmation unit. He makes no additional attempts to analyze or critique the idea of confirmation. The argument appears to apply to theories an intuitive sense of confirmation. The idea of "confirmation holism" holds that the theory, rather than the phrase, serves as the fundamental unit of confirmation. Duhem advocated it, and the positivists approved of it (e.g. Carnap 1937, p. 318; Ayer 1936, p. 125f.) But Quine was the first "holistic verificationist" to contend that a verification principle that applies to specific sentences is impossible given confirmation holism. (p. 991)

In his response to Chomsky in Words and Objections, Quine delivered one of his first proclamations of the theory of underdetermination: "The totality of possible observations of nature, made and unmade, is compatible with physical theories that are incompatible with one another." The first point to note is that Quine exclusively discusses underdetermination in relation to theories of the entire universe, never in relation to any weaker theories. Furthermore, it does not follow that any component theory is underdetermined if one's global world theory is underdetermined. The second thing to keep in mind is that underdetermination and holism are not synonymous (i.e., the Duhem-Quine thesis).

3.1 Holism vs. Underdetermination

Holism should not be confused with this notion of underdetermination. Holism has been referred to as the Duhem thesis and, more charitably, the Duhem-Quine thesis. According to this, scientific claims are not individually susceptible to contradictory evidence since they can only be inferred collectively as a theory. In the face of contradictory observations, any one of the claims can be maintained by amending the others. The underdetermination thesis is supported by the holism thesis. If we are constantly able to pick between different effective modifications of our theory in response to contradictory data, then obviously all potential observations are inadequate to decide theory uniquely. (Quine, 1975,313. See also, Gibson, 27-39.)

However, as we shall show, the parallel becomes very shaky when we move from points on a plane to genuine scientific hypotheses. Quine is nevertheless self-assured enough to state the thesis, and part of that self-assurance comes from the evidence that holism (or, the "Duhem-Quine thesis") makes underdetermination possible. Holism, in contrast to underdetermination, is a fundamental tenet of Quine's philosophy. It has a systematic relationship with practically every facet of his philosophical outlook, especially his reservations about the analytic/synthetic divide, the theses of indeterminacy of translation and ontological relativity, and his views on epistemology, empiricism, and naturalism. He constantly refers to holism as being clearly and trivially true, which betrays how basic he thought it to be. Answering a criticism by Grünbaum (see his 1960 and 1962) for example, this is what he had to say: "I find your argument that the Duhem-Quine thesis, as you refer to it, is false if considered seriously. (...) I believe that the theory is probably simple as I have applied it. (Quine, 1976, 132)

Of fact, there are many, not all of which are trivial, ways to understand holism. According to Quine's version, one can choose from a number of different revision options that all result in the same overall conformity to reports of specific observations because scientific sentences are not each endowed with their own unique empirical content. Therefore, if a theory suggests a faulty prediction, a single statement in the theory can only be held accountable if several supporting hypotheses are assumed. To prevent the erroneous inference, multiple changes in various areas of the theory and background assumptions may individually be sufficient. Quine stated that the idea "must command assent," but "with reservations." (Quine, 1975, 313)

One objection is that, even when taken separately, observation sentences do have their own empirical substance. Since the concepts they include are also found in other theoretical statements, even these sentences are not autonomous from the theories in which they are contained. Observation sentences can be interpreted "holophrastically" or "analytically," in which case they work much like the more theoretical assertions of a theory (Quine, 1992, 7), In that situation, they are deemed ready for testing right away without the use of further presumptions or research. Observation sentences do not qualify as an exception to the unqualified claim of holism when viewed holophrastically; nevertheless, when viewed analytically, they do (Rogério Passos Severo, 2008, 148).

A second reservation to an unqualified assertion of holism has to do with its scope. In his *Two Dogmas of Empiricism*, Quine wrote that the "unit of empirical significance is the whole of science" (Quine, 1953, 42). In later writings, he regrets such a "needlessly strong statement of holism" (Quine, 1991, 268)

That bold claim could still be accurate, he said, but only in a "legalistic sort of way". (ibid.) Although statements that are far distant from observations, such as those of logic and pure mathematics, are not immune to revision, it is nonetheless true that even observation phrases may be updated in light of theoretical adjustments:

> "Revision even of the law of excluded middle has been proposed as a means of simplifying quantum mechanics." (Quine, 1953, p. 43)

However, he later emphasizes that the smallest unit of empirical importance need not represent the entirety of science "chunks of it" (Quine, 1991, p. 268); it is adequate if there is "a cluster sufficient to imply an observable effect of an observable experimental condition" (ibid.).16 Thus, it is possible to consider each branch, or clusters of nearby branches, to imply its own set of observation categoricals along with logic and mathematics, and as a result, to have some autonomy in relation to other branches. Both discontinuous and monolithic aspects of science exist. It has a variety of joints and variable degrees of looseness.

We are free to decide which claims to change and which to remain true in the face of a stubborn observation, and these options will disrupt different strands of scientific theory in different ways with differing degrees of severity. Even if this assertion may be defended in a formal manner, arguing that the unit is in essence the entirety of science offers no benefit. (Quine, 1975, 314-315)

Given that the thesis of holism has these two limitations, namely the separate empirical substance of observation sentences taken holophrastically and the relative autonomy of each field of inquiry, Quine nevertheless believes that the theory is clearly and even trivially true: A scientist can always choose one of several alternative possible updates to his general theory or to his specific area of study in the face of "adverse observations." No single statement, unless it is an observation sentence or the lengthy conjunctive phrase that expresses the entire theory, may be believed to be accountable for a theory's erroneous implications on its own. But even in those situations, different revisions to the theory as a whole may be developed that either stop the incorrect inference from occurring or alter the extensions of some of the concepts that are included in it. Thus, holism implies underdetermination.

If we are constantly able to pick from different effective modifications of our theory in the face of contradictory data, then obviously all potential observations are inadequate to decide theory uniquely. (Quine, 1975, 313)

The argument's conclusion is presented here as something conceivable but yet speculative: Holism "lends credence" without really proving underdetermination in any manner. It does not, and there are two causes for this. First, despite the fact that "we are always free to choose among various adequate modifications of our theory," we cannot assume that these changes will all result in theories that are equally valid in terms of empirical data.

Holism does not state if the theories that follow from these adjustments are empirically equal; it only states that various modifications may "inactivate" a faulty inference. In fact, it is conceivable that different changes will result in theories that are not empirically comparable in the majority of the pertinent circumstances. These alterations would each "inactivate" the incorrect implication in a unique way, and in the process, they may each "inactivate" (or "activate") some additional implications that aren't affected by alternate modifications.

The second argument is that even if all potential revisions to a particular theory only produced theories that are identical in terms of empirical results, those updated theories could not be sufficiently distinct from one another to qualify as competitors or even viable alternatives in any nontrivial sense. If the beliefs at question are global theories rather than just local theories, then this appears especially feasible. It is likely that, in the case of global theories, any changes that can be made to eliminate a false implication would result in theories that are not only logically similar but also theoretically comparable. They would postulate the same concepts and things and share a similar theoretical framework. The only distinctions between them would then be terminological; any statement in one theory could be translated into a sentence in the other. However, rather than being competitors or alternatives, ideas that are so intertranslatable are better viewed as variations of the same theory. We would not normally refer to a Chinese physics handbook and its Japanese translation as "alternative" or "rival" ideas.

There are even more arguments against holism supporting underdetermination. Holism may be explained as being obvious not just on the basis of an abstract reflection on the relationship between theories and observations in general, but also on the basis of sufficiently clear historical cases of theories that were modified in various ways in order to avoid recognized counter-examples. However, underdetermination cannot be supported by any such direct evidence.

Although certain situations of competing theories proposed as explanations for the same data are sufficiently obvious, no historical examples of competing theories that are empirically equal are readily available. Many competing Ptolemaic and Copernican astronomical theories, for instance, have been known examples of rival theories that have been shown to be either cases where there is no empirical equivalence or to be theoretically identical theories expressed in different languages (such as Heisenberg's and Schrödinger's formulations of quantum mechanics). In addition, there are no well-established historical examples of global theories. Because of this, there is no concrete proof that global theories, if we ever come up with any, will accept empirically similar rivals in a nontrivial sense.

A stronger argument than the one Quine defended is that there is evidence for the underdetermination of local ideas. It is obviously untrue when taken as an all-encompassing, unqualified assertion since not all local theories possess the necessary level of theoretical complexity for underdetermination to occur and because the majority of competing local theories may be incompatible with the current global theory as a whole. Of course, this does not preclude the possibility of local underdetermination for some theories. But it does demonstrate that we can only be certain that it has occurred by proving that global underdetermination is true at the same time. (Balashov, 150)

We cannot depend on the type of evidence from the history of science that is frequently cited in favor of holism to demonstrate the underdetermination of global conceptions. This is not a position that has direct proof, unlike holism. 19 The vast majority of historical instances of conflicting theories are not, in fact, instances of empirical equivalency. Additionally, there have never been any examples of global theories in the history of science. At this time, physics is the strongest possibility because it is the only field that strives for "full coverage." (Quine, 1981, 98)

4. Conclusion

In this study, we examined the controversial **Quine-Duhem Thesis** and showed that it does not threaten the rationality of science. For, this thesis suggests that it is always possible to accommodate any recalcitrant observational evidence to any theory provided that one attempts at adjusting the auxiliary hypotheses and *ceteris paribus* clauses involved in the test situation.

Thus, my understanding of the Quine-Duhem thesis is that it makes a logico-metaphysical point, that is, nature does not force upon us (à la Francis Bacon) a change in the truth-values of scientific assertions and hypotheses. In other words, it can be said that the falsification of a test hypothesis is not a question of letting nature unilaterally dictate changes in our scientific hypotheses which we then must obey; rather, it is a question of what our most rational, truth-preserving, methodological response ought to be to what we hear nature saying. Hence, the Quine-Duhem thesis does not make an epistemological point. Furthermore, Quinean holism does not destroy the objectivity of science. Even if some possible adjustments to recalcitrant observations that would throw out basic beliefs are made, it does not follow that we cannot claim to know any basic beliefs with reasonable objectivity. As has been rightly suggested by Lakatos, the devastating consequences of holism for scientific rationality can be avoided through counterbalancing them by recognizing certain progressive patterns of theoretical growth as evidential.

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References

Ariew, R. (1984). "The Duhem Thesis", *British Journal for the Philosophy of Science, 35*: 313-325.

Ayer, A. J. (1936). *Language, Truth and Logic*. Harmondsworth: Penguin, rpnt 1971.

Balashov, Yuri (1994). "Duhem, Quine, and the Multiplicity of Scientific Tests", *Philosophy of Science,* 61, 608-628.

Boylan, Thomas A. & O'Gorman, Paschal F. (2003). "Pragmatism in Economic Methodology: The Duhem-Quine Thesis Revisited", *Foundations of Science* 8, 3–21.

Carnap, R. (1937) .*The Logical Syntax of Language*. London: Routledge.

Churchland, P. (1979). *Scientific Realism and the Plasticity of Mind.* Cambridge: Cambridge University Press.

Duhem, P.: ([1906],1954). *Aim and Structure of Physical Theory*. Reprint. Translated by P. P. Wiener. Originally published as *La Théorie Physique: Son Objet, et sa Structure.* (Paris: Marcel Rivière & Cie). Princeton: Princeton University Press.

Fogelin, Robert J. (1997). "Quine's Limited Naturalism", *The Journal of Philosophy,* Vol. 94, No. 11, 543-563.

Gibson, Roger F. (1986). "Quine's Dilemma", *Synthese*, Vol. 69, No. 1, 27-39.

Gillies, Donald. (1993). *Philosophy of Science in the Twentieth Century: Four Central Themes*. Blackwell.

Greenwood, J. D. (1990). "Two dogmas of Neo-Empiricism: The 'Theory-Informity' of Observation and the Duhem-Quine Thesis", *Philosophy of Science 57*: 553-574.

Greenwood, J. D. (1994). *Realism, Identity and Emotion*. London: Sage.

Harding, S., (ed.) (1976). *Can Theories Be Refuted?Essays on the Duhem-Quine Thesis. Dordrecht: Reidel.*

Heikki J. Koskinen & Sami Pihlström (2006)."Quine and Pragmatism",

Klee, Robert (1997). *Introduction to the Philosophy of Science: Cutting Nature at its Seams.* Oxford: Oxford University Press, Inc.

Krips, H. (1982). "Epistemological Holism: Duhem or Quine?", *Studies in History and Philosophy of Science 13*: 251-264.

Lakatos, I. (1987). *Philosophical Papers. Vol. I, The Methodology of Scientific Research Programmes*. Cambridge, England: Cambridge University Press.

McDermott, Michael (2001). "Quine's Holism and Functionalist Holism" *Mind*, New Series, Vol. 110, No. 440, 977-1025.

Popper, K. R. (1959). *The Logic of Scientific Discovery*. New York: Basic Books.

Quine, W. V. (1953). Two Dogmas of empiricism. In W. V. Quine (1980), *From a logical point of view* (2nd rev. ed., 20-46). Cambridge, Mass.: Harvard University Press.

Quine, W. V. (1975). "On Empirically Equivalent Systems of the World", *Erkenntnis 9*, 313-318.

Quine, W. V. (1976). "A Comment on Grünbaum's Claim". In S. G. Harding (Ed.), *Can theories be refuted? Essays on the Duhem-Quine thesis* (Synthese Library, vol. 81, 132). Dordrecht: D. Reidel.

Quine, W. V. (1981). *Theories and Things.* Cambridge, Mass.: Harvard University Press.

Quine, W. V. (1991). 'Two Dogmas' in retrospect.*Canadian Journal of Philosophy, 21,* 265-274.

Quine, W. V. (1992). *Pursuit of truth* (rev. ed. [1st ed. 1990]). Cambridge, Mass.: Harvard University Press.

Quine, W. V. and J. S. Ullian (1978). *The Web of Belief*. New York: Random House.

Rogério Passos Severo (2008). "Plausible Insofar as It Is Intelligible: Quine on Underdetermination". *Synthese*, Vol. 161, No. 1, 141-165, Springer.

Transactions of the Charles S. Peirce Society, Vol. 42, No. 3, 309-345.

Vuillemin, J. (1987). "On Duhem's and Quine's Theses", in L. E. Hahn and P. A. Schilpp. (eds.), *The Philosophy of W. V. Quine,* La Salle, IL: Open Court, 595-622.