THE DUHEM-POPPER-QUINE THESIS

by

THOMAS AVERY

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Abstract

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Thomas Avery

Advisor: Professor Charles Landesman

In this dissertation I examine critically the scientific holism of Pierre Duhem, Karl Popper and W.V. Quine. I contend that there is a central thesis, which I have dubbed the "Duhem-Popper-Quine thesis," that is common to the work of these three authors but that in each author's work it is reflected differently.

Duhem's holism was rather sweeping--he contended that no isolated hypothesis can be refuted by the results of experiment--but also rather restricted, being limited to physical science. I argue that it was too restricted, because it was too closely tied to certain features of modern physical theory.

Popper's holism was less restricted--it applied to all of empirical science--but more guarded than Duhem's. Popper, however, confined his attention to empirical science as traditionally conceived. He rejected the possibility that (purported) laws of logic may be revised in the face of recalcitrant experience.

Quine was the most radical of the three holists. (Unlike Duhem, he did not insist on a sweeping holism to which there are no counterexamples.) The most controversial aspect of Quine's holism was his willingness to countenance the revisability of not only hypotheses in empirical science but also what we take to be the laws of logic.

It is this last aspect of Quine's holism that I find implausible and I argue that the law of noncontradiction is an important counterexample to his claim that "no statement is immune to revision." This, I contend, is the key to the superiority of Popper's holism over Quine's, despite a few weaknesses in Popper's position.

DEDICATION

I dedicate this dissertation to the memory of my father, Anthony John Avery (1923-1960), and my brother, Anthony Jude Avery (1958-1980).

Requiescant in pace.

ACKNOWLEDGMENT

There are several people whose support and encouragement made it possible for me to complete this dissertation. Professors Arnold Koslow and Martin Tamny read earlier drafts and gave me valuable advice. I could not have finished this work without the invaluable help of my dissertation supervisor, Professor Charles Landesman, whose patience I greatly appreciate. I also owe a great debt to my wife, Carole McGee, whose love I deeply cherish.

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CHAPTER ONE:

Three Scientific Holists

Introduction

What has come to be known as "the Duhem-Quine thesis" has received a good deal of attention in the recent literature in the philosophy of science. There have been many papers devoted to the topic and more than a few authors have discussed it, in passing, in books and papers primarily devoted to other topics.

First of all, the views of Pierre Duhem have been examined. There is some interest in Duhem's work for its own sake, of course, but there is also some interest in his work because of the influence it has had, and the light it may shed, on the work of others, especially that of W.V. Quine. In particular, Duhem's contention that it is impossible to falsify what he called "an isolated hypothesis" in physical theory and his contention that there cannot be what he dubbed "'a crucial experiment'" in physical theory have commanded much recent attention.[1]

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[1]The Aim and Structure of Physical Theory (Princeton: Princeton University Press, 1954), the English translation of Duhem's La Théorie Physique: son objet et sa structure (Paris, Chevalier and Riviere, 1906), has been reprinted with a new introduction by Jules Vuillemin. The Philosophy of W.V. Quine (La Salle, Illinois: Open Court, 1974) Library of Living

Second, the work of Karl Popper has long commanded attention and continues to provoke controversy. Because of the central role that falsification plays in his philosophy of science, Popper's views have sometimes been contrasted with those of Duhem and Quine, as if they were incompatible. Indeed, it has often been Popper himself, and some of his outspoken allies, who did the contrasting.[2]

And third, of course, much attention has been focused on the work of W.V. Quine. Many philosophers have discussed his views in books and journal articles; and Quine himself has continued to refine and clarify those views, often seeking to engage his contemporaries in philosophical discussion.[3]

Philosophers series, contains a discussion by Vuillemin of the similarities and differences between Duhem's and Quine's views.

[2]Despite Popper's early affinity for Duhem, evinced in his early book, The Logic of Scientific Discovery (London: Hutchinson, 1959) later (and more polemical) works like Conjectures and Refutations (London: Routledge and Kegan Paul, 5th ed., 1989) contain criticism of "the Duhem-Quine thesis." Imre Lakatos, one of Popper's students, in The methodology of scientific research programs (Cambridge: Cambridge University Press, 1978) included a critical discussion of "the strong interpretation" of "the Duhem-Quine thesis."

[3]Quine's latest book, Pursuit of Truth (Cambridge, Massachusetts: Harvard University Press, 1992), contains not only clarifications and modifications of his "moderate holism" but also some new analyses of the testing of scientific hypotheses.

In this dissertation I will examine critically the relevant portions of the work of Duhem, Popper, and Quine, and some of their critics. I contend that there is a central thesis, reflected in three different ways in the work of these three different authors, that can appropriately be called "the Duhem-Popper-Quine thesis." In this introductory chapter I will articulate, and briefly discuss and defend, this central thesis. In each of the three subsequent chapters I will examine the pertinent views of one of the main authors. I will argue that in each case those views are open to criticism. Among my conclusions will be that Duhem's holistic thesis (concerning what he called "isolated hypotheses" in physics) is too restricted and that Quine's holistic thesis (what he calls his "moderate holism") is not restricted enough. Popper's position too, I conclude, is open to criticism. Nevertheless, I will argue, in the fifth and final chapter, that Popper's version of the central thesis, with a small but important modification, is more defensible than the versions of either Duhem or Quine.

In Chapter Two, I will focus on the relevant part of Duhem's work. I will show that he presented two related but distinct and logically independent theses that are not always clearly distinguished. The second of these two

theses, on the impossibility of "a crucial experiment" in physics (an expression he almost always used in quotation marks), though not examined by Quine, is briefly discussed (but not really contested) by Popper. Properly understood, it can be seen to be defensible, and consistent with Popper's and Quine's views. The first thesis, however, is the source of most of the recent discussion among many different philosophers, including not only Popper and Quine, but several others as well.[4] I will argue that this holistic first thesis is modest and rather restricted--Duhem was writing about physical science not about empirical science in general--but that the French physicist's position is not entirely safe from criticism. His thesis, I believe, is too closely tied to certain features of modern physical theory.

In Chapter Three, I will take a closer look at the pertinent aspects of Popper's position. A critical examination, I believe, will show both its strengths and its weaknesses. I will argue, for example, that there are tensions within Popper's falsificationism that are related

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[4]A new anthology, Can Theories Be Refuted? (Dordrecht, Holland and Boston: D. Reidel, 1976) ed., Sandra Harding, is subtitled Essays on the Duhem-Quine Thesis and contains the work of a dozen authors. The title, however, seems a bit misleading. The question is not whether theories can be refuted.

to his holism but that these tensions can be resolved by making an important adjustment in the former. I will also defend Popper's position against criticism that seems to me to be based on misunderstandings.

In Chapter Four, I will focus on the relevant part of Quine's work. It seems to me obvious and uncontroversial that his brand of holism is more radical than that of either Duhem or Popper. It also seems to me that Quine has retreated from an earlier, more extreme, holism to a later, more moderate position. Be that as it may, I will argue that even his more mature holism is beset with problems of its own.

In addition, at several points in the next three chapters, I hope to show that some of the "interpretations" made of what has been called "the Duhem-Quine thesis" are really distinct claims that go beyond any views that can reasonably be attributed to Duhem, Popper, or Quine.

Finally, in Chapter Five, I will summarize my conclusions about each version of the central thesis, make some further observations and briefly make the case that Popper's holism, slightly modified, is the most defensible of the three. The most important part of this case will consist of a comparison between Popper's holism and Quine's holism and a critical discussion of the two.

The Duhem-Popper-Quine Thesis

What exactly is "the Duhem-Popper-Quine thesis"? Let me lay the groundwork for discussing this important central claim.

Consider any argument in which we validly deduce a conclusion that can be directly confronted with the results of observation or experiment from a set of premises containing at least two members that cannot be directly confronted with such observational or experimental results. Let's call each such premise a "nontest statement of scientific theory" (or, more briefly, a "nontest statement") and the conclusion derived from them a "test statement of scientific theory" (or, more briefly, a "test statement"). And let's call a statement of the observational or experimental results with which a test statement is confronted an "evidence statement."

For my purpose in briefly stating and defending the central thesis, it doesn't matter exactly what logical form any of these statements takes. (As we will see, there are some similarities and some differences among our three authors on this score.) Nor does it matter exactly why the nontest statements of scientific theory cannot be directly confronted with the evidence statements. It does not matter, for example, whether no rigid distinction can be drawn between theoretical terms or sentences, on one hand,

and observational terms or sentences, on the other. It also does not matter whether it is always the test statement that is rejected when this confrontation with evidence statements goes badly. (Indeed, what plays the role of test statement in one inquiry may play the role of evidence statement in another.)

What matters is that individual nontest statements cannot be made to face the music and that the test statements have to do so in their stead. It matters also that there be at least two nontest statements from which each test statement is validly deduced. If there were only one "nontest" statement in a given deduction, then having it face the music would be a straightforward matter. We would deduce a test statement from it and then confront that statement with an evidence statement. If the test statement turned out false then we could validly infer the falsehood of the "nontest" statement from which it was deduced.

It is easy to reconstruct such a falsifying argument. Let 'p' represent the "nontest" statement (i.e., the premise of our original argument) and let 'q' represent the test statement (i.e., the conclusion deduced from that premise). And let ' $\rightarrow$ ' be taken as the symbol for the material conditional. Then 'p $\rightarrow$ q' represents the conditional corresponding to our original deduction. The following

argument schema, the modus tollens of classical truth-functional logic, captures as much as necessary of the logical form of any such falsifying argument:

(1-A) $p \rightarrow q$

$-q$

$-p$

If this were all there is to the logic of falsification in science, then there would be no room for a Duhem-Popper-Quine thesis. The "nontest" statement in any such case would be unambiguously falsified, since the test statement is false.

Obviously, however, this is not all there is. Even a fairly simple realistic derivation of a test statement is usually a good deal more complex. In the first place, the test statement is supposed to be <u>deduced</u> from nontest statements and this would require that '$p \rightarrow q$' (representing the conditional corresponding to the original deduction) represents either a logical truth or a mathematical truth. In the second place, this is not a case of immediate inference, in which we infer a conclusion from a single premise. Scientists do not, for example, predict that a given piece of wood will float on water on the basis of the conjecture that it will not sink. In any interesting

case, our test statement is deduced from more than one nontest statement and, of course, it is not simply equivalent to one of them.

A second attempt at the reconstruction of a falsifying argument is still not very complicated. Let 'p' and 'q' represent the nontest statements and let 'r' represent the test statement derived from them. The following schema captures more (though not all) of the logical form of any such falsifying argument:

(1-B) $(p \ \& \ q) \rightarrow r$

$-r$

$-(p \ \& \ q)$

But since the conclusion here is logically equivalent to:

$-p \ v \ -q$

we are left to infer that one or the other (or both) of the two nontest statements, which 'p' and 'q' represent, is (are) false. This ambiguous outcome will haunt us whenever the test statement represented by 'r' is derived from more than one nontest statement. The more nontest statements are involved in the derivation, the more haunting the outcome becomes.

What I am calling the central thesis, the "Duhem-Popper-Quine" thesis, is a compound thesis: first, if we

determine the falsehood of the test statement in such a case we do not thereby determine the falsehood of any particular one of the nontest statements from which it was, with the aid of the others, deduced; second, this ambiguous falsification procedure is the typical case in science.

This, I believe, is the central thesis that is reflected in the work of each of these three authors. None of them states it in exactly these terms. Nevertheless, I believe, it is what unites their work, despite the differences among them. As we will see, Duhem restricted his holism to physical science and Popper extended it, though somewhat guardedly, to all of empirical science. Quine was the most radical of the three, extending his holism, at least potentially, to all of science, not excluding logic and mathematics. Quine was also (initially at least) the most extreme of the three, taking all of science as "the unit of empirical significance." He was later, I will argue, to retreat to a more moderate position, taking smaller chunks of [nontest statements of] theory as competent to answer for the test statements derived from them.

There hardly seems to be a plausible case to be made that the first part of "the Duhem-Popper-Quine thesis" is not true. There does seem, however, to be a plausible case

to be made that, although true, it is trivial. Isn't it a commonplace truth about logic that from the falsehood of the conclusion of a valid argument one can correctly infer only that at least one of the premises must be false? And isn't it a commonplace truth about classical truth-functional logic that the falsehood of the consequent of any true conditional allows us to infer only the falsehood of the whole antecedent? But when that antecedent is a conjunction, its negation is equivalent to a disjunction of the negations of each conjunct. In other words, it seems that if you add one of De Morgan's laws to modus tollens (granted that the antecedent of the conditional is a conjunction), you get the first component of the two-part "Duhem-Popper-Quine thesis." Is that all there is to this vaunted central thesis?

Well, not quite. I have said that in any given case there must be at least <u>two</u> nontest statements. Plainly, there must be at least one. If there were <u>no</u> nontest statements then there would be no derivation of a test statement. Scientists could (and would) test the original statements without any such derivation. If there were only <u>one</u> "nontest" statement then having it face the music would be a straightforward matter, as outlined above. It is an important part of the central thesis that the testing of

hypotheses in science typically involves the derivation of each test statement from more than one nontest statement of scientific theory.

It might still be objected that at least the first part of the central thesis is trivial. Only the applicability of an ordinary truism about logic to the testing of hypotheses in science is an important question. The truism itself is hardly worth stating. I would respond by saying that even an apparently trivial thesis may be informative and even important. Nor is this response foreign to the spirit in which our authors wrote. Popper, for example, wrote in his most famous work on scientific method that: "Profound truths are not to be expected of methodology."[5] In the revised English edition of this book, in a footnote to this sentence, he added: "I am still inclined to uphold something like this," although some of his methodological theorems, are, in his words, "perhaps unexpected and not quite on the surface."[6]

And Quine saw his own "moderate holism" as no less important for being obviously true.

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[5]Karl Popper, The Logic of Scientific Discovery (London: Hutchinson, 1959), 54.

[6]Ibid. (1968), 54.

> Holism...is an obvious but vital correction of the naive conception of scientific sentences as endowed each with its own separable empirical content.[7]

Some people, Quine believes, hold naive conceptions of scientific method. And so long as they do, even supposedly obvious points that bear on scientific methodology are worth examining and discussing.

Let me now turn to a critical examination of the pertinent views of the first of our three main authors: the French physicist, and historian and philosopher of physical science--Pierre Maurice Marie Duhem.

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[7]W.V. Quine, Pursuit of Truth (Cambridge, Massachusetts: Harvard University Press, 1992), 16.

CHAPTER TWO:

Duhem's Restricted Holism

Introduction

Pierre Duhem has become famous not only as a physicist and a teacher of physics but also as a historian and philosopher of physical science. His published work ranges from articles on medieval thinkers such as Nicholas Oresme to a book on thermodynamics, and includes a paper on physics and metaphysics entitled "The Physics of a Believer." Although he is often associated today with W.V. Quine and the holism that Quine has espoused, he has contributed much more to science and the philosophy of science than his version of "the Duhem-Popper-Quine thesis."

The locus classicus for Duhem's philosophy of science is his book La Théorie Physique: son objet et sa structure,[1] which was translated into English by Philip P. Wiener as The Aim and Structure of Physical Theory.[2] Here he presented his mature views on the subject; and here, in Part II,

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[1]Pierre Duhem, La Théorie Physique: son object et sa structure (Paris: Chevalier and Riviere, 1906).

[2]Pierre Duhem, The Aim and Structure of Physical Theory. Translated by Philip P. Wiener. With a new Introduction by Jules Vuillemin. (Princeton: Princeton University Press, 1954).

Chapter VI of this book, he presented the views that have received so much attention in recent years.

In this chapter I will examine Duhem's version of "the Duhem-Popper-Quine thesis." I will argue that there are actually two related but distinct and logically independent Duhem theses that are often confused, and that both of these are weaker and more restricted than may they appear to be.

In the next section of this chapter I will examine Duhem's first thesis, which concerns the impossibility of falsifying what he called "an isolated hypothesis" in physics by physical experiment. (This is his version of the central thesis.) In the following section, I will examine Duhem's second thesis, which concerns the impossibility of what he termed "'a crucial experiment'" in physics. (Duhem almost always used the latter expression--but almost never, the former--in quotes.)

The fourth section treats of Duhem's views on the aim of physical theory and the bearing those views have on his holistic thesis. The fifth section examines more closely his analysis of reasoning in science. The sixth section considers the mathematical structure of modern physical theory and its relationship to his first thesis. In the final section of the chapter I add some final criticism and draw some conclusions about Duhem's holism.

Let me begin with the first and more controversial thesis. It is the one to receive so much critical attention in recent years and is his version of the central claim that I have called "the Duhem-Popper-Quine thesis." The thesis is that "an isolated hypothesis" in physics cannot be falsified by physical experiment. (The analytical subtitle of section two of Part II, Chapter VI of the book, which I will refer to hereafter as Aim and Structure, is "An Experiment in Physics Can Never Condemn an Isolated Hypothesis but Only a Whole Theoretical Group.")

Isolated Hypotheses

What did Duhem mean by "an isolated hypothesis" in physical theory, as opposed to "a whole theoretical group"? And how does his thesis that an isolated hypothesis in physics can never be refuted by physical experiment reflect the central thesis stated and briefly defended in Chapter One?

First of all we should notice that Duhem restricted his analysis to physical science, declining to extend it beyond this horizon.

> We shall in this book offer a simple logical analysis of the method by which physical science makes progress. Perhaps certain readers will wish to extend the reflections put forth here to sciences other than physics....We have imposed narrow limits on our researches in order

> to explore more thoroughly the restricted domain we have assigned to our inquiry.[3]

Although some authors have wanted to extend Duhem's analysis beyond the limits he set, we should remember that he himself did not do so. When he used the word "hypothesis," he was talking about statements in physical science and he sometimes used this word more narrowly than we would use it today, even when talking about physical science. This is part of what he said about the first two steps in constructing a physical theory.

> 1. Among the physical properties which we set ourselves to represent we select those we regard as simple properties....We make them correspond to a certain group of mathematical symbols, numbers, and magnitudes....
> 2. We connect the different sorts of magnitudes...by means of a small number of propositions which will serve as principles in our deductions. These principles may be called "hypotheses" in the etymological sense of the word for they are truly the grounds on which the theory will be built....[4]

The English word "hypothesis," like the French word "hypothèse" of which it is here the translation, is derived from a Greek word that is usually translated "base" or "foundation." Duhem sometimes used the word "hypothesis," [i.e., "hypothèse"] to denote a single proposition or

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[3]Duhem, Aim and Structure, 3.

[4]Ibid., 19-20.

principle that forms part of the foundation of a theory. (Sometimes he used the phrase "fundamental hypothesis" ["hypothèse fondamentale"] for "hypothesis" in this narrow sense.)[5]

Many writers today use the word "hypothesis" rather broadly. (In fact, Duhem himself occasionally used it somewhat broadly.) Karl Popper, for example, used the words "conjecture" and "hypothesis" interchangeably, not giving any special narrow sense to the latter word, and not restricting its use to denote statements in physical theory. Thus, in a discussion of a criticism of his views by Gilbert Ryle, Popper wrote

> Ryle argues...that it is wrong to say "that all the general propositions of science... are mere hypotheses"; and he uses the term "hypothesis" in exactly the same sense in which I have always used it and in which I am using it now: as a "proposition... which is only conjectured to be true."[6]

Quine, too, has used "hypothesis" more broadly than Duhem did here, covering statements both in science and elsewhere with the term. Thus, in a nontechnical discussion, Quine and Ullian wrote:

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[5]Duhem, La Théorie Physique, 303-328.

[6]Karl Popper, "Conjectural Knowledge: My Solution of the Problem of Induction," in Objective Knowledge 2nd ed. (Oxford: Oxford University Press, 1979), 9.

> It is now recognized that deduction from self-evident truths and observation is not the sole avenue to truth nor even to reasonable belief. A dominant further factor, in solid science as in daily life, is hypothesis. In a word, hypothesis is guesswork; but it can be enlightened guesswork.[7] (italics in the original)

Both Popper and Quine, for example, have occasionally labeled as "hypotheses" such ordinary universal generalizations as "All swans are white" and "All ravens are black." This may suggest some ordinary counterexamples to Duhem's thesis. It seems, for example, that the universal generalization "All swans are white" is an isolated conjecture that can be (indeed, has been) falsified. Duhem might have agreed. Such empirical generalizations are unlike the bases or foundations on which a physical theory is built. They are also unlike the statements of the laws of modern physics. As we will see later, they are much closer to the formulation of the laws of common sense, or to the laws of physical science when, in Duhem's words, "physics was still dependent upon common sense." One can condemn an isolated statement such as "All swans are white," perhaps, but such a statement is not a "hypothesis" in physical science in the sense in which Duhem usually used the term. Its refutation, then, is not a counterexample to

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[7]W.V. Quine and J.S. Ulian, The Web of Belief 2nd ed. (New York: Random House, 1978), 65.

his holistic thesis.

Duhem's view was that the hypotheses, or fundamental hypotheses, of physical theory are what I have called "nontest statements of scientific theory." They do not face the verdict of physical experiment directly but only indirectly. It is only the statements derived from them, what I have called the "test statements of scientific theory," that are summoned to court.

The testing of hypotheses in physical science is, according to Duhem, accomplished by the derivation of predictions from them and the comparison of those predictions with the results of physical experiment. It is a vital part of Duhem's argument that the deduction of a prediction from the fundamental hypotheses of a theory in physical science involves more than just a single premise and conclusion. Usually, in fact, it involves more than two premises and a conclusion. Here is a fairly simple sketch of one of his own examples.

> We know that Newton conceived the emission theory for optical phenomena. The emission theory supposes light to be formed of extremely thin projectiles, thrown out with very great speed by the sun and other sources of light; these projectiles penetrate all transparent bodies; on account of the various parts of the media through which they move they undergo

> attractions and repulsions;....[8]

We have here not a single emission hypothesis but a whole set of hypotheses that together form the basis of a theory. Duhem summarized the theoretical situation.

> These essential hypotheses joined to several others...lead to the formulation of a complete theory of reflection and refraction of light...[9]

And it can be deduced from these hypotheses that light travels faster in water than in air. When Foucault modified the experiment suggested by Arago and found that light was propagated less rapidly (instead of more rapidly) in water than in air, he showed not that the hypothesis of emission is faulty but that (in Duhem's words) "the system of emission is incompatible with the facts" shown by experiment.

> I say the system of emission and not the hypothesis of emission;....But in condemning this system as a whole by declaring it stained with error, the experiment does not tell us where the error lies. Is it in the fundamental hypothesis that light consists in projectiles thrown out with great speed by luminous bodies? Is it in some other assumption concerning the actions experienced by light corpuscles due to the media through which they move?[10]

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[8]Duhem, Aim and Structure, 186.

[9]Ibid.

[10]Ibid., 187.

The point of all this is that the "fundamental hypothesis" cannot be directly confronted with experience. More than one statement, including the hypothesis in question, is required to deduce a prediction that can be compared with the report of an experiment. When that report conflicts with the prediction, and we take the report to be true, we cannot tell exactly which of the statements used in the deduction is to be blamed for the false prediction that we deduced. Duhem summarized this thesis after discussing his examples.

> In sum, the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed.[11]

This, then, in brief outline, is Duhem's version of "the Duhem-Popper-Quine thesis." It amounts, to put it in the terms introduced in Chapter One, to the claim that every isolated hypothesis ["hypothèse isolée"] in physical science is a nontest statement of scientific theory.

Let me, before going any farther, distinguish between a particular and a universal thesis. The particular thesis is

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[11]Ibid.

that some isolated hypotheses cannot be falsified and the universal one is that none of them can. Duhem, here, was plainly defending the stronger universal thesis. Since he made the sweeping claim that the physicist "can never subject an isolated hypothesis to experimental test" it is important to consider some purported counterexamples presented by critics. After all, the examples used to illustrate his sweeping thesis may be presented as confirming instances of it but can never suffice to prove it.

The first counterexample or, more exactly, kind of counterexample, concerns the testing of a system of physical geometry and was given by Adolf Grunbaum as the second half of a two-part criticism of (what he took to be) Duhem's position.

> This paper offers a refutation of P. Duhem's thesis that the falsifiability of an isolated empirical hypothesis H as an explanans is unavoidably inconclusive. Its central contentions are the following:
>
> (1) No general features of the logic of falsifiability can assure...that H can always be preserved as an explanans of any empirical findings O whatever by some modification of the auxiliary assumptions A in conjunction with which H functions as an explanans....
>
> (2) The categorical form of the Duhemian thesis is not only a non-sequitur but actually false. This is shown by adducing the testing of physical geometry as a counterexample to Duhem in the form of a rebuttal to A. Einstein's geometrical

> articulation of Duhem's thesis.[12]
> (italics in the original)

The claim that "the Duhemian thesis" is a non-sequitur is based on a misunderstanding of Duhem. Grunbaum here attributed to the French physicist a view that the latter did not hold. Duhem did not speak of attempting to preserve an isolated hypothesis as an explanans of empirical findings. He spoke rather of attempting to falsify (or, in his words, "condemn") an isolated hypothesis. That is not at all the same thing.

Grunbaum characterized Duhem's thesis as the claim that the logic of unavoidably inconclusive falsification is best represented as the claim that from a premise of the form

$$(2\text{-}1) \qquad [(H \,\&\, A) \rightarrow O\,] \,\&\, -O$$

a conclusion of the form

$$(2\text{-}2) \qquad (\exists A')[(H \,\&\, A') \rightarrow O'$$

is somehow supposed to follow.[13] Here "H" represents an isolated physical hypothesis, "A" represents a set of auxiliary assumptions, "O" represents the observational prediction that fails to come true, "O'" represents the

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[12]Adolph Grunbaum, "The Duhemian Argument," in Can Theories Be Refuted? Essays on the Duhem-Quine Thesis ed. Sandra Harding (Dordrecht, Holland and Boston: D. Reidel, 1976), 116.

[13]Ibid., 117-118.

actual observational result that is incompatible with "O", and "A'" represents a revised set of nontrivial auxiliary assumptions.

Grunbaum was surely right that jumping from the first to the second schema is a non-sequitur. The logic of falsifiability, he correctly pointed out, does not guarantee that no matter what the observational findings may be, any isolated physical hypothesis is such that we can add nontrivial auxiliary assumptions to it and deduce a statement describing those findings. But he was surely wrong to attribute this view to Duhem. Failure to falsify, or condemn, an isolated hypothesis on the basis of unexpected observational or experimental results is not the same thing as success in deriving those unexpected results from that hypothesis (with new auxiliary hypotheses).

The second part of Grunbaum's criticism involves providing a counterexample. Although he addressed himself to Einstein's articulation of the holistic thesis, Grunbaum plainly intended to provide a counterexample to Duhem's thesis, and in this, I believe, he failed.

He pointed out that we obtain a physical geometry by giving a physical interpretation of the postulates or axioms of a formal geometric system (say, Euclid's system). This is done by what are sometimes called "rules of corres-

pondence" that connect the formal system with, for example, solid bodies such as rigid rods. It is necessary, however, to correct for such deforming or perturbational influences as heat, which causes bodies to expand but affects them differently depending on their chemical composition.

Grunbaum then considered two cases, one in which deforming influences are supposed to be absent and one in which they are present and corrections have to be made for them. I will examine the first in more detail since it is surely more favorable to his position and merely avoids certain technical complications. I will comment only briefly on the latter. If he is wrong about the first case then he is wrong about the second one too.

> (i) If we are confronted with the problem of the falsifiability of the geometry ascribed to a region which is effectively free from deforming influences, then the correctional physical laws play no role as auxiliary assumptions, and the latter reduce to the claim that the region in question is, in fact, free from deforming influences. And if such freedom can be affirmed without presupposing collateral theory, then the geometry alone rather than only a wider theory in which it is ingredient will be falsifiable.[14] (italics in the original)

What Grunbaum was seeking to avoid here was a certain trap. If the system of geometry is regarded as testable inde-

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[14]Ibid., 122.

pendently of a physical theory, but the physical theory is intertwined with that very system of geometry in stating, for example, the laws concerning the deforming influence of temperature or pressure on the length of solid rods, then the prospects for isolating that system of geometry seem rather dim, to say the least.

Grunbaum's solution to the problem was to specify a way of ascertaining the absence of deforming influences that did not rely on physical theory.

> ...two solid rods of very different chemical constitution which coincide at one place in the region will also coincide everywhere else in it independently of their paths of transport.[15]

Laurens Laudan has argued that Grunbaum did not provide here a genuine counterexample to Duhem's holistic thesis, for more than one reason.

> We need not probe into the physics of the problem to understand the substance of Grunbaum's counterexample....But there are two factors here which make this argument powerless against Duhem's position. In the first place, a system of geometry...is not the sort of thing which counts for Duhem as an 'isolated hypothesis'....The second, and more serious flaw in Grunbaum's counterexample, if I understand it correctly, is that A, though probable

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[15]Ibid.

> is not known to be true...[16]

It seems to me that the first part of Laudan's response by itself is enough to defend Duhem's holism against Grunbaum's objection, regardless of the latter's success in separating physics from geometry in this case. For if a system of geometry is not an isolated hypothesis in Duhem's sense then Grunbaum simply has not provided a counterexample to Duhem's thesis about isolated hypotheses.

To be fair, I should point out that Grunbaum, in discussing the isolation of a hypothesis "H," did consider decomposing a system of geometry into subhypotheses.

> For suppose that H is the hypothesis of Euclidean geometry and that we consider absolute geometry as one of its sub-hypotheses and the Euclidean parallel postulate as the other....If...the prevailing geometry were to turn out to be spherical, then the mere replacement of the Euclidean parallel postulate by the spherical one could not possibly save absolute geometry from refutation.[17]

The problem is that in decomposing the system of geometry into subhypotheses Grunbaum did not go far enough. Since absolute geometry (i.e., Euclidean geometry minus the parallel postulate) is still not an isolated hypothesis, its

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[16]Laurens Laudan, "Grunbaum on 'The Duhemian Argument'," in Harding (1976), 160.

[17]Grunbaum, "The Duhemian Argument," 123.

refutation would still be the refutation of a whole group of hypotheses. Grunbaum did not succeed here in refuting Duhem.

Laudan had a second response to Grunbaum's criticism of Duhem. It was that the auxiliary hypothesis about the absence of deforming influences, though probable, was not known to be true. For Grunbaum had argued that the schema that correctly represents the logical situation is the following:[18]

$$(2\text{-}3) \qquad [\{(H \ \& \ A) \rightarrow O\} \ \& \ (-O \ \& \ A)] \rightarrow -H.$$

But if "A" represents the hypothesis of the absence of deforming influences, and this hypothesis is not already assumed to be true, then the falsehood of "O" does not entail the falsehood of "H", whether "H" represents an isolated hypothesis or not. If we need not assume "A" to be true then the logical situation is correctly represented by the following:

$$(2\text{-}4) \qquad [\{(H \ \& \ A) \rightarrow O\} \ \& \ -O] \rightarrow - \ (H \ \& \ A)$$

and, thus "H" is not unambiguously falsified, whether it represents a single isolated hypothesis or not.

Similar remarks could be made about the case in which there are perturbational influences. For however compli-

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[18]Ibid., 122.

cated the auxiliary hypotheses represented by "A" in such a case are, if "H" is not isolated then Duhem's thesis still stands. And if "H" itself represents not a single hypothesis but a whole group of hypotheses then, again, Duhem's thesis still stands.

In addition to Grunbaum's objection to the first Duhem thesis, there is an objection that has been pressed by Popper. Unlike Grunbaum's objection it is not special to physical science but is intended to apply to any empirical science in which a theory or part of a theory can be axiomatized. Popper had in mind those independence proofs that show that one axiom is logically independent of the others.

> The more simple of these proofs consists in the construction, or rather in the discovery, of a model--a set of things, relations, operations, or functions--which satisfies all the axioms except the one whose independence is to be shown: for this one axiom--and therefore for the theory as a whole--the model constitutes a counter example.[19] (italics in the original)

In such a case, it is contended, one can refute an isolated hypothesis. The axiom that is independent of the others is falsified and thus, Popper argued, Duhem cannot be right.

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[19]Karl Popper, "Truth, Rationality, and the Growth of Scientific Knowledge," in Conjectures and Refutations 5th ed. (London and New York: Routledge, 1989.), 239.

Popper was aware, of course, that not all physical theories are axiomatized in such a way as to make this possible. He was also aware that it may not be possible in every case to find a model that satisfies all of the axioms except one. His point was simply that this can sometimes be done and that this is enough to refute the sweeping claim that it can never be done.

Mary Hesse has argued, however, that this objection, too, is powerless against Duhem.

> We may be able to axiomatise the whole theoretical system in such a way as to isolate the effect of a single axiom, which may then be refuted in isolation. But...its ideal possibility still does not refute [Duhem], because no axiomatisation can fully account for the empirical applicability of the system, and the correctness of the conditions of application (the so-called "correspondence rules") might always be called into question to avoid abandonment of any of the axioms.[20]

I will argue, in Chapters Three and Four, that there is more to Popper's objection than Hesse seems to realize. Part of what Hesse has overlooked, though, is more appropriate as a criticism of some of Quine's early views than of any of Duhem's views.

It seems to me that Hesse's response to Popper is an effective defense of Duhem if we construe Duhem's phrase

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[20]Mary Hesse, "Duhem, Quine and a New Empiricism," in Harding (1976), 189.

"whole theoretical group" broadly enough to include the "so-called 'correspondence rules'." Of course, if we construe the term "isolated hypothesis" so narrowly in this case that it covers only the geometrical axioms taken individually, and construe the phrase "whole theoretical group" so narrowly that it covers only the axioms taken collectively, and take for granted the "so-called 'correspondence rules'" then Hesse has not adequately responded to Popper's objection. It would seem that in such a case we <u>can</u> falsify one of the axioms (one of the "fundamental hypotheses") in isolation from the others. All of this, however, presupposes that appropriate restrictions are placed on the sort of model that we consider. I will come back to this point shortly.

Let me give Duhem the benefit of the doubt here. Perhaps he would have wanted to consider the correspondence rules of an axiomatic system as part of the "theoretical group" that was condemned by physical experiment. In that case this objection, if it counts at all, counts only against a view that we should not attribute to him.

There is some reason, however, to doubt that Popper's objection, at least as it stands, counts against <u>any</u> view about the falsification of isolated hypotheses, whether or not we attribute that view to Duhem. For Popper did not

place any restrictions on the model that is to serve as a counterexample to some empirical theory and surely this will not do. At a minimum, I submit, we can demand that the model in question be a model of reality, or at least of that part of reality of which the allegedly refuted theory is supposed to be true. Otherwise, Popper is open to the charge that he has simply confused a proof of the independence of an isolated hypothesis with a refutation of that hypothesis. Surely a realist like Popper, who accepts a correspondence theory of truth, should not forget that a model that satisfies all of the axioms of a theory but one may not correspond in any way to the real world.

An example may help to bring home this point. A proof that the parallel postulate is independent of the other postulates or axioms of Euclidean geometry does not refute that postulate. Consider Poincaré's discussion of a non-Euclidean geometry developed by Riemann.

> Let us imagine to ourselves a world only peopled with beings of no thickness, and suppose these "infinitely flat" animals are all in one and the same plane, from which they cannot emerge....But now suppose that these imaginary animals, while remaining without thickness, have the form of a spherical, and not of a plane figure, and are all on the same sphere, from which they cannot escape. What

kind of a geometry will they construct?[21]

These imaginary beings (supposedly endowed with reasoning power) will, according to Poincaré, believe that space has only two dimensions. And they will construct a non-Euclidean geometry, one of whose theses is that there are no lines parallel to a given line through a point not on that line. Such a geometry will be every bit as consistent as the Euclidean variety and this imaginary world would seem to constitute, in Popper's terms, a model of which the parallel postulate is not true.

Nevertheless, the constructing of a consistent non-Euclidean geometry and the discovery of a model of which this geometry holds is not, by itself, a refutation of the (isolated) parallel postulate. Nor does it show that the results of an actual observation or experiment can condemn this isolated hypothesis. For this model may be every bit as fanciful as Poincaré's world of imaginary beings without thickness who are trapped on the surface of a sphere. And isolated statements that are easily "refuted" in such a world may yet be nontest statements because they cannot confront directly the evidence in the actual world.

Popper, then, did not succeed in giving a counter-

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[21]Henri Poincaré, Science and Hypothesis. Translated by W.J.G. (New York: Dover, 1952), 37.

example, or kind of counterexample, to Duhem's holistic thesis. First, as Hesse noted, he did not consider the possibility of changing the rules of correspondence to protect one of the axioms. Second, he did not rule out the possibility that finding a model that satisfies all but one of the axioms requires making the kind of fanciful assumptions that Poincare made in his didactic tale.

Crucial Experiments

The second Duhem thesis is that there cannot be any "'crucial experiment'" in physics. (The analytical subtitle of section three in Part II, Chapter VI of Aim and Structure is "A 'Crucial Experiment' ["'experimentum crucis'"] Is Impossible in Physics." Duhem put the key phrase in scare quotes. Indeed, in the French original, he used the Latin phrase borrowed from Francis Bacon.)

Although it has received less critical attention than Duhem's first thesis, this second thesis has, from time to time, been counted as one of Duhem's central contributions. Stanley L. Jaki, for example, has recently attached great importance to it.[22] It has also been misunderstood, being

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[22]Stanley L. Jaki, Uneasy Genius: The Life and Work of Pierre Duhem (The Hague, Boston: Martinus Nijhoff, 1984) 335, 351, 370.

occasionally confused with a different claim that is not logically independent of the holistic first thesis.

It is true that Duhem believed his first thesis to apply to supposedly crucial experiments, arguing that such experiments, in effect, decided between two theoretical groups and not between two isolated hypotheses. This point, however, is not logically independent of his first thesis and Duhem sought to push his analysis further. Referring to the experiment that supposedly decided between the system of emission and that of wave propagation in optical theory, he wanted to make a point beyond his holistic first thesis.

> But let us admit for a moment that in each of these systems everything is compelled to be necessary by strict logic, except a single hypothesis; consequently, let us admit that the facts, in condemning one of the two systems, condemns once and for all the single doubtful assumption it contains. Does it follow that we can find in the "crucial experiment" an irrefutable procedure for transforming one of the two hypotheses before us into a demonstrated truth?[23]

Duhem's answer to his last question is negative. He was not thinking of attempts to falsify one theory while at the same time confirming (or, in Popper's terms, "corroborating") a competing one. He was thinking of attempts to falsify every theory but one, thereby verifying (or, in Duhem's terms,

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[23]Duhem, Aim and Structure, 189.

"demonstrating") that remaining one.

> 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....[24]

This kind of argument is often used in mathematics but, Duhem argued, has no place in physics.

> Those who assimilate experimental contradiction to reduction to absurdity imagine that in physics we may use a line of argument similar to the one Euclid employed so frequently in geometry. Do you wish to obtain from a group of phenomena a theoretically certain and indisputable explanation? Enumerate all the hypotheses that can be made to account for this group of phenomena then, by experimental contradiction eliminate all except one; the latter will no longer be a hypothesis, but will become a certainty.[25]

This second Duhem thesis is logically independent of the first. One can argue--and Duhem, as noted above, does argue--that there could be no "crucial experiment" in physics even if it were possible to refute an isolated hypothesis: conclusively falsifying a given hypothesis does not conclusively verify any competing one because the two hypotheses are not jointly exhaustive of the possibilities.

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[24]Ibid., 188.

[25]Ibid.

Here, reconstruction of the argument is more complicated than it was in the case of the holistic claim that I have called "the Duhem-Popper-Quine thesis." The following argument schema captures what is essential in the logical structure of an argument based on a "crucial experiment":

(2-A)

$$p \rightarrow r$$

$$q \rightarrow -r$$

$$-r \text{ v } r$$

$$-p \text{ v } -q$$

Of course, such a simple argument structure would allow the falsification of either isolated hypothesis (represented by 'p' and 'q'). More to the point, though, because it is logically possible that both of the hypotheses are false, (since '-p v -q' is true) the refutation of one does not entail the demonstration of the other. If, for example, we discover that '-r' is true; it follows logically that 'p' is false. It does not follow logically, though, that 'q' is true. To argue from the truth of '-r' to the truth of 'q' in this case is to commit the fallacy of affirming the consequent.

Although it is more complicated to reconstruct a case in which there are more than two possible theories from which to deduce statements describing a given set of

phenomena, the essential point is not changed. Physical science is unlike geometry in this respect.

Euclid once presented a proof that the area of a given circle is equal to the area of a right triangle whose base is equal to the radius of the circle, and whose altitude is equal to the circumference of the circle. Here there are only three possibilities to be enumerated: the area of the circle is less than, equal to, or greater than the area of the triangle. Euclid's strategy was easy to understand: eliminate the first and third possibilities and only the second one remains.

In physical science a basic step is missing: you cannot enumerate all of the possibilities.

It is a commonplace truth about logic that the same conclusion can be validly deduced from any number of different premises. Of course, a true conclusion can be validly deduced from premises that turn out to be false. If, however, these premises are not themselves subject to experimental test, except by testing the consequences deduced from them (if, in other words, they are nontest statements of scientific theory), then we have potentially an unlimited number of sets of hypotheses from which a true conclusion can be derived. Eliminating one of these sets by further testing does not automatically prove or demonstrate

any one of the others.

Duhem used a historical example to illustrate his point, once again recalling the famous dispute in optics. The experiment showing that light traveled more quickly in air than in water was hailed as a "crucial experiment." Duhem disagreed.

> Light may be a swarm of projectiles, or it may be a vibratory motion whose waves are propagated in a medium; is it forbidden to be anything else at all? Arago undoubtedly thought so when he formulated this incisive alternative: Does light move more quickly in water than in air? "Light is a body. If the contrary is the case then light is a wave." But it would be difficult for us to take such a decisive stand; Maxwell, in fact, showed that we might just as well attribute light to a periodical electrical disturbance that is propagated within a dielectric medium.[26]

(We should note in passing that Duhem's point is buttressed here by the logical possibility that the velocity of light in air is <u>equal</u> to its velocity in water. In such a case, would Arago have said that light is neither a body nor a wave?)

Duhem's targets here were any nineteenth-century Baconians, such as Arago, who were disposed to see an "experimentum crucis" in an experiment that falsified all but one of two or more competing sets of hypotheses. To

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[26]Ibid., 190.

refute all but one of the theories actually proposed to account for a given phenomenon is not to refute all of the theories that can possibly be proposed.

One might, of course, press another point about crucial experiments but it is important not to confuse this with Duhem's second thesis. Consider an argument schema that combines elements from each of two previous schemata--the one used in Chapter One to reconstruct the holistic "Duhem-Popper-Quine thesis" and the one used earlier in this section to reconstruct Duhem's thesis about "'crucial experiments'":

(2-B)

$$(p \ \& \ q) \rightarrow r$$

$$(s \ \& \ t) \rightarrow -r$$

$$-r \ v \ r$$

$$-(p \ \& \ q) \ v \ -(s \ \& \ t)$$

Unless the same hypothesis or set of hypotheses represented by 'q' can be added to the hypothesis represented by 'p' to derive the result represented by 'r', and can also be added to the very different hypothesis represented by 's' to derive the very different result represented by '-r', then the "crucial experiment" not only does not demonstrate the unrefuted hypothesis but also does not isolate either the refuted or the unrefuted one. At

best, it decides between two sets of premises, 'p & q' and 's & t,' refuting one set and confirming the other. It does not decide between two isolated hypotheses.

This point, however, is not logically independent of Duhem's first thesis, it is merely a special application of it. Duhem made it a point to go beyond a mere application of his holistic thesis to "crucial experiments"! His second thesis is separate and independent. It stands or falls on its own merits.

In practice the situation is often more complicated than I have suggested here. There may be several premises taken together that logically imply the prediction that is falsified and some of these may be derived from still other statements. There is, however, a limit to how much we can consider involved in a failed prediction. Popper has occasionally written as if one can always compare two different theories seen in relief against a common background. Thus, in one discussion of crucial experiments, he wrote

> A theory is tested not merely by applying it, or by trying it out, but by applying it to very special cases--cases for which it yields results different from those we should have expected without that theory, or in the light

of other theories.[27]

And then, considering an objection to the view he had just sketched, he added a response.

> Against the view here developed one might be tempted to object (following Duhem) that in every test it is not only the theory under investigation which is involved, but also the whole system of our theories and assumptions--in fact, more or less the whole of our knowledge--so that we can never be certain which of all these assumptions is refuted.[28]

His response to this objection followed immediately.

> But this criticism overlooks the fact that if we take each of the two theories (between which the crucial experiment is to decide) <u>together</u> with all this background knowledge, as indeed we must, then we decide between two systems which differ <u>only</u> over the two theories which are at stake.[29] (italics in the original)

This is misleading in two respects. First, if we consider Popper's own model of scientific explanation, which is similar to Hempel's deductive-nomological model, we can see that both lawlike statements and statements of initial conditions figure in the premises from which a prediction is deduced. (I am, for the sake of argument, supposing a

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[27]Karl Popper, "Three Views Concerning Human Knowledge," in <u>Conjectures & Refutations</u>, 112.

[28]Ibid.

[29]Ibid.

symmetry between explanation and prediction, a symmetry that Popper, unlike such authors as Israel Scheffler, did not question.)

> To give a causal explanation of an event means to deduce a statement which describes it, using as premises of the deduction one or more universal laws, together with certain singular statements, the initial conditions. (italics in the original)[30]

Thus, according to Popper, it is from the universal laws and initial conditions, taken together, that a prediction is deduced and it is the universal laws and initial conditions, taken together, that are falsified if the prediction fails to come true. The initial conditions, I have argued, will not usually be the same for the two sets of universal lawlike statements that are being tested in a crucial experiment. If these conditions are not considered part of the theory itself, then we decide between two systems which differ not only over the two theories that are at stake but also over the two sets of initial conditions that are added to the theoretical premises to get the false conclusion (prediction).

Of course, we can stipulate that it is the universal law that is the test statement in such a case, and that the conjunction of the statement of initial conditions with the

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[30]Popper, Logic of Scientific Discovery, 59-60.

report of the failure of the prediction is its potential falsifier. In such a case, however, the two systems, having different initial conditions, will have different falsifiers.

Furthermore, even if we ignore this complication, Duhem was not committed to the view that we cannot decide between two theoretical groups by ruling out one of them. At most he was committed to the view that we cannot decide between two isolated hypotheses.

Popper's comments are misleading in another respect. If the universal laws in question are, in turn, deduced from still other laws then the falsification of the conclusion (i.e., the prediction) will have ramifications beyond the premises explicitly involved in the deduction of this prediction. Even then, however, this will hardly involve "more or less the whole of our knowledge."

Duhem did not suppose that our whole system of knowledge is involved when a physical theory is being tested. In the first place, he restricted his analysis to physical theory. Although he believed that far-flung parts of physical science may be involved in physical experiment, he did not believe that all of science, much less all of our knowledge, was involved. In the second place, he did not even contend that all of physical theory is involved in

every physical experiment. I will argue in Chapter Four that even Quine has backed away from the extreme sort of scientific holism that Popper has attacked. One would have to do more than just follow Duhem, one would have to go far beyond him, to press the objection that Popper here considered and rejected.

It may be the case that Duhem's use of examples makes it easy to confuse his thesis about the falsification of an isolated hypothesis in physics with his thesis about the impossibility of "a crucial experiment" in physics. After all, he was making Foucault's famous experiment do double duty: first, he argued that it overthrew "the system of emission" and not simply the "hypothesis of emission" (phrases coined by Duhem); second, he argued that refuting the "system of emission" did not demonstrate the "system of wave propagation" (if I may coin a phrase). Duhem was using one historical example to illustrate two distinct points.

In fact, there is a perfectly ordinary sense of the phrase "crucial experiment" in which Duhem did not contest the claim that there are crucial experiments in physics. Popper, I believe, was partly correct when he summarized Duhem's view of crucial experiments in a footnote to part of a passage quoted earlier.

Duhem, in his famous criticism of crucial ex-

> periments (in his Aim and Structure of Physical Theory), succeeds in showing that crucial experiments can never establish a theory. He fails to show that they cannot refute it.[31] (italics in the original)

I would merely add here that although Duhem argued that an isolated hypothesis cannot be refuted, he did not contend that a theoretical group cannot be refuted. And, of course, he argued that refuting a theory cannot demonstrate any of its competitors but he did not contend that refuting a theory cannot leave some of its competitors unrefuted, and thus either confirmed or corroborated.

Duhem, I believe, succeeded in showing what he attempted to show about what he termed "crucial experiments"; he "failed" to show only what he never set out to show in the first place.

The Aim of Physical Theory

Examining more closely some of Duhem's general views about physical science will, I believe, give a better understanding of how what I have called "the Duhem-Popper-Quine thesis" is reflected in his account of physical theory. What he had to say about the aim of physical theory, for example, will help to shed some light on his

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[31]Popper, "Three Views," 112.

somewhat restricted holism.

It is fairly well known that Duhem rejected the belief that physical theories can explain the phenomena in their domain.

> A physical theory is not an explanation. It is a system of mathematical propositions deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible a set of experimental laws.[32]

It seems to me that Duhem's point is easily misunderstood. He was not simply denying, for example, that a physical theory is the same thing as an answer to an explanation-seeking why question. He was making what he took to be an important point about what some philosophers of science (Popper, for instance) would call "ultimate explanation." Some elaboration may help to make his position clear.

It is easy for us today to take for granted the view that science, including physical science, aims at explanation. Most contemporary philosophers of science in the English-speaking world believe that explanation is at least one of the legitimate aims of physical theory, and of scientific theory in general. Wesley Salmon, for example, has entitled a recent essay Four Decades of Scientific

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[32]Duhem, Aim and Structure, 19.

Explanation and has reckoned the start of this continuing discussion of the topic with the publication of Hempel and Oppenheim's famous paper, "Studies in the Logic of Explanation" in 1948.[33]

Even a cursory examination of the literature, however, is enough to show that the debate has been mostly concerned not with whether scientific theories can be used to explain scientific phenomena but with a detailed explication of the concept of explanation itself. Is all explanation deductive? Is there a symmetry between explanation and prediction? Must explanations make use of general laws? These are some of the questions that writers like Hempel, Salmon, Scheffler and others have discussed. It is usually taken for granted that theories, including physical theories, can be used to explain the phenomena of our world.

At least part of the dispute between Duhem and more recent authors, though, is verbal. Some of the types of explanation that are discussed in the recent literature might not have been called "explanations" by Duhem. Hempel distinguished between the explanation of laws and the explanation of individual events. In the 1948 essay mentioned above, what Hempel called the "basic pattern of

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[33]Wesley Salmon, Four Decades of Scientific Explanation (Minneapolis: University of Minnesota Press, 1989)

scientific explanation" is plainly a pattern for the (deductive-nomological) explanation of individual events.[34] Duhem was criticizing a view that had to do with the possibility of explaining laws.

Even Hempel's notion of the explanation of laws, however, seems to differ somewhat from Duhem's notion of what constitutes an explanation. Here is Duhem's definition of the term.

> To explain (explicate, explicare) is to strip reality of the appearances covering it like a veil, in order to see the bare reality itself.[35] (italics in the original)

Some of what Hempel had to say about the explanation of laws is not inconsistent with what Duhem had to say about the reality underlying appearances because the two men were talking about different things. In the case of what Hempel calls "deductive-statistical explanation," for example, there isn't any assumption that the explanans tells us anything about that underlying reality. Such an explanation (in Hempel's sense of the term) may involve simply the deduction of the statement of a statistical law from a

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[34]Carl Hempel, "Studies in the Logic of Explanation," in Aspects of Scientific Explanation and Other Essays in the Philosophy of Science (New York and London: The Free Press, 1965) 247-251.

[35]Duhem, Aim and Structure, 7.

theoretical principle of statistical form by means of the probability calculus.[36] However much Hempel, and those inspired by him, insist on calling such things "explanations," they are not the sort of things that Duhem was talking about.

On the face of it, Duhem's view of physical theory, however, is plainly anathema to such a staunch realist as Popper. The French physicist made a distinction between physical theories that purport to provide us with what he termed "certain" explanations and those that purport merely to provide us with what he termed "hypothetical" explanations.

> The explanation which acoustic theories give of experimental laws governing sound claims to give us certainty....
> Most often we find that physical theory cannot attain that degree of perfection; it cannot offer itself as a <u>certain</u> explanation of sensible appearances, for it cannot render accessible to the senses the reality it proclaims as residing underneath those appearances. It is then content with proving that all our perceptions are produced <u>as if</u> the reality were what it asserts; such a theory is a hypothetical explanation.[37]
> (italics in the original)

Duhem saw those who constructed physical theories (as

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[36]Hempel, "Aspects of Scientific Explanation" in <u>Aspects</u>, 381.

[37]Duhem, <u>Aim and Structure</u>, 8.

distinct from metaphysical theories) as primarily concerned with what is sometimes called "saving the phenomena" or "saving the appearances."

> Despite Kepler and Galileo, we believe, today, with Osiander and Bellarmine, that the hypotheses of physics are mere mathematical contrivances devised for the purpose of saving the phenomena.[38]

His view of physical theory, although not exactly instrumentalist, was plainly not realist. As far as he was concerned, the ability to look behind the veil of appearances to get a glimpse of ultimate reality is a gift not given to physicists. This is a job for metaphysicians, aided by faith and revelation. Physicists cannot show us how sensible appearances are the effects of ultimate causes. What they can do, according to Duhem, is construct physical theories that will represent experimental laws in such a way as to make possible an economy of thought and approach a natural classification of those laws.

For Popper, however, hypothetical explanations, in the sense that Duhem used the expression--and here his use of the word "hypothetical" was fairly close to Popper's use of the word "conjectural"--are just what science is all about.

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[38]Pierre Duhem, To Save the Phenomena: an essay on the idea of physical theory from Plato to Galileo. Translated by Edmund Doland and Chaminah Maschler, (Chicago: Chicago University Press, 1969), 117.

Making conjectures about the underlying realities, and testing them is, according to Popper, a practice central to the scientific enterprise.[39]

Even Popper, however, rejected anything that he thought smacks of what he called "ultimate explanation" and especially "ultimate explanation by essences." Physical theory does not, in his view, give us certainty (and neither, he thought, does metaphysical theory). In this respect, his views are similar to Duhem's.

> Now what the instrumentalist philosophers of science, from Berkeley to Mach, Duhem, and Poincaré, have in common is this. They all assert that explanation is not an aim of physical science, since physical science cannot discover 'the hidden essences of things.' The argument shows that what they have in mind is what I call ultimate explanation.[40]
> (italics in the original)

If what Duhem called "explanation" and what Popper called "ultimate explanation" are the same thing--and it seems that they are--then their views on this topic are really not very far apart after all.

Pursuing the French author's views about explanation a bit farther will lead us to an important consequence of

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[39]See, for example, his discussion in "Three Views" in Conjectures and Refutations, 115ff.

[40]Popper, "Three Views," 104.

those views.

Duhem wanted to maintain a sharp division between physics and metaphysics, and the view of physical theories as explanations seemed to him to make physics depend upon metaphysics.

> While we regard a physical theory as a hypothetical explanation of material reality, we make it dependent on metaphysics. In that way, far from giving it a form to which the greatest number of minds can give their assent, we limit its acceptance to those who acknowledge the philosophy it insists on. But even they cannot be entirely satisfied with this theory since it does not draw all its principles from the metaphysical doctrine from which it is claimed to be derived.[41]

Duhem was not here rejecting metaphysics altogether, neither as cognitively insignificant nor as philosophically unimportant. He was not even arguing that physicists ought to avoid holding any metaphysical (in the broadest sense of the term) opinions. He was, after all, proud to consider himself a common sense realist and to don the appellation "apostle of common sense."[42]

What he rejected was the logical connection of a physical theory with a metaphysical system. He had more than one reason for doing this. The desire to avoid

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[41]Duhem, Aim and Structure, 19.

[42]Jaki, Stanley L., Uneasy Genius, 259.

entangling physics in endless disputes over metaphysical systems was an important one. Duhem was hoping to analyze how physical science progresses. Getting mired in endless disputes does not bring progress.

Also important was Duhem's concern for logical rigor in physical theory, a logical rigor that seemed to him to be compromised by any futile attempt to derive specific experimental laws of physics from general metaphysical principles.

Although it was not explicitly discussed by Duhem, there is an important consequence of severing the tie between physics and metaphysics--one that has a bearing on Popper's criticism concerning the use of a model to refute an isolated axiom. It is that the fundamental hypotheses of a physical theory from which experimental laws are ultimately to be deduced are not constrained by metaphysical doctrines about the ultimate nature of things. These hypotheses are to be constrained only by a combination of logic and good sense, as applied to the results of physical experiment. As a result, no hypothesis or set of hypotheses was ruled out in advance because it did not begin by postulating the right sort of entities as the ultimate things in the universe on which all physical explanations must rest or with which they must begin.

An important and pertinent consequence of Duhem's rejection of explanation in physical theory, then, is that one cannot condemn an isolated hypothesis in a physical theory by showing that the reality underlying appearances provides us with a model that holds for all of the hypotheses but the one in question. This reality underlying appearances is something that, according to Duhem, is outside of the domain of physical theory.

Reasoning on the Facts

It will be useful, I think, to take a closer look at Duhem's conception of reasoning in physical science and to consider some of the things he said about the structure of physical theory. Part of his structural analysis can give us some insight into his holism, and his reasons for restricting it to physical theory.

As noted above, Duhem restricted his analysis to physical science, leaving it to others to apply this analysis more generally if they wished.

> We shall in this book offer a simple logical analysis of the method by which physical science makes progress. Perhaps certain readers will wish to extend the reflections put forth here to sciences other than physics....We have imposed narrow limits on our researches in order to explore more thoroughly the restricted

domain we have assigned to our inquiry.[43]

How broad an extension of his reflections Duhem would have approved, though, is a bit difficult to say. The evidence suggests that he would have been rather cautious. It wasn't only a matter of his restricting the scope of his inquiry so that he could treat his subject more thoroughly. Nor was it just a matter of his modestly confining his attention to the science he knew best. Duhem believed that there are important and relevant differences between some sciences and others. He drew a sharp contrast, for example, between physics and other sciences "still close to their origins."

> When many philosophers talk about experimental sciences, they think only of sciences still close to their origins, e.g., physiology or certain branches of chemistry where the experimenter reasons directly on the facts....In such cases the comparisons between the deductions of a theory and the facts of experiment is subject to very simple rules.[44]

These rules, though perhaps difficult to obey, were not especially difficult to understand. They presented a moral challenge more than an intellectual one. Scientists, for example, should put aside vanity and envy and any other vice

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[43]Duhem, Aim and Structure, 3.

[44]Ibid., 180.

that might incline them to find confirmation too easily for their own pet hypotheses or to find disconfirmation too easily for those of their rivals. In many cases, scientists are supposed to leave their theories (and, in all cases, their biases) outside of the laboratory before performing an experiment and observing the results. For reasons that deserve closer consideration, though, Duhem held that the former is simply not possible in the case of physical science. Physical theory cannot be left outside of the laboratory. Not only is it necessary to provide the auxiliary hypotheses that enable the experimenter to deduce his or her predictions from fundamental hypotheses, but, as we shall see, it is also necessary to enable him or her to interpret the results obtained with the aid of the laboratory instruments used.[45]

It may be argued that Duhem saw this closeness to origins as a matter of degree but I am not convinced that this is true. In the first place, he wrote of sciences as being, or not being, close to their origins, not in comparative terms of one science's being closer to its origins than another science. Furthermore, he gave no intermediate examples between physics and physiology that

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[45]Ibid., 180-182.

would suggest a series of empirical sciences ranked in order of their closeness to their origins.

Duhem did not, in so many words, make a distinction between theoretical terms and observational terms or between theoretical sentences and observational sentences in science. And he did appreciate the theory-ladenness of observation in physical science. Such an appreciation is woven throughout his analysis of physical theory.

It seems to me, however, that what Duhem did do was, in effect, to make a rigid distinction between theoretical sciences and observational sciences and to classify physics as a theoretical science. It is only in an observational science that one can "reason directly on the facts."

Does Duhem's version of the central thesis apply at all when a scientist "reasons directly on the facts"? I will argue that Duhem would probably have answered this question in the negative and that, furthermore, he applied his holism to physical theory precisely because in physics scientists do not "reason directly on the facts." Much of Duhem's analysis, I contend, can be summarized as the attempt to spell out in some detail what exactly constitutes the physicist's "reasoning indirectly on the facts." (This is my phrase, not Duhem's.)

What exactly is the difference between "reasoning

directly on the facts" and "reasoning indirectly on the facts"? It seems to me a good idea to begin with some of Duhem's own examples of reasoning. The first contrast that he drew with respect to direct and indirect reasoning is the contrast between physics and common sense.

> Just as the laws of common sense are based on the observation of facts by means natural to man, so the laws of physics are based on the results of physical experiments....
>
> Let us consider one of the simplest and most certain of common-sense laws: All men are mortal. This law surely relates two abstract concepts, the abstract idea of man in general... and the abstract idea of death...indeed, it is only on this condition, viz., that the concepts related are abstract, that the law can be general. But these abstractions are in no way theoretical symbols, for they merely extract what is universal in each of the particular cases to which the law applies.[46]

Duhem's point appears to be not just that direct observation without the aid of instruments gives us evidence supporting the law. He seems also to have believed in a kind of spontaneous inductive reasoning that doesn't depend upon theory. This seems clearer with another example.

> We see the flash of lightning before we hear thunder. The ideas of lightning and thunder which this statement ties together are abstract and general ideas, but these abstractions are drawn so instinctively and naturally from particular data that with each bolt of lightning we perceive a glare and

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[46]Ibid., 165.

> a rumbling in which we recognize immediately the concrete form of our ideas of lightning and thunder.[47]

This law, according to Duhem, still had the form that physics had when it was dependent upon common sense. The abstractions involved in both cases are the kind that are drawn, according to him, "instinctively and naturally" from concrete reality. Both the peasant and the metaphysician, Duhem contended, understand the generalization "All men are mortal" and know it to be true. Both the modern European and the ancient Roman, he believed, can find the same certainty and clarity in the generalization "We see the flash of lightning before we hear thunder." In Duhem's view, both laws are based on the observation of facts by means "natural to man". We see each flash of lightning and hear each rumble of thunder. We watch individual human beings live and die.[48]

This common sense establishing of truths pertains, in Duhem's view, not only to ordinary observations but also to some fairly ordinary scientific experiments. Here is a contrast between physics, on one hand, and an experimental science "still close to its origins" on the other.

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[47]Ibid.

[48]Ibid., 167.

> Here, for example, is a physiologist who admits that the anterior roots of the spinal nerve contain the motor nerve-fibers and the posterior roots the sensory fibers. The theory he accepts leads him to imagine an experiment: if he cuts a certain anterior root, he ought to be suppressing the mobility of a certain part of the body without destroying its sensibility; after making the section of this root, when he observes the consequences of his operation and when he makes a report of it, he must put aside all his ideas concerning the physiology of the spinal nerve; his report must be a raw description of the facts...[49]

It is the possibility of this "raw description of the facts" that appears to make possible "reasoning directly on the facts."

Unlike many modern epistemologists since the time of Descartes, his famous compatriot, Duhem did not entertain a skeptical attitude toward the testimony of the senses. In fact, he considered the evidence of the unaided senses as comparatively unproblematic.

> The uninitiated believe that the result of a scientific experiment is distinguished from ordinary observation by a higher degree of certainty. They are mistaken, for the account of an experiment in physics does not have the immediate certainty, relatively easy to check, that ordinary, non-scientific testimony has.[50]

This immediate certainty, he contended, though purchased at

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[49]Ibid., 181.

[50]Ibid., 163.

the price of precision and minuteness of detail, is not unduly threatened by the possibility of error. Duhem didn't tell us that the testimony of the senses is indubitable but he did dismiss, with perhaps too cursory an analysis, the scruples of more fastidious thinkers.

> When a sincere witness, sound enough in mind not to confuse the play of his imagination with perception, and knowing the language he uses well enough to express his thought clearly, says he has observed a fact, the fact is certain: if I declare to you that on such and such a day at such and such an hour, I saw a white horse in a certain street, unless you have reasons to consider me a liar or subject to hallucinations, you ought to believe that on that day, at that hour, and in that street there was a white horse.[51]

There is no hint here that perhaps on occasion we ought to dismiss an ordinary observation or two on the grounds that we might otherwise have to abandon some well-confirmed theory. Duhem did not suggest, for example, that conflicting with such a theory would constitute good reasons for pleading hallucination or even optical illusion.

The view of reasoning changes drastically when we consider physical theory and experiment, when we begin to "reason indirectly on the facts." We need to take a closer look at physical theory to understand why. How do we get

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[51]Ibid., 158-159.

away from what he called a "raw description of the facts"? According to Duhem, an important factor is the introduction of mathematics. Theoretical physics, he held, is mathematical physics.

The Structure of Physical Theory

In order to understand better Duhem's reasons for restricting his analysis to physical science, we need to look more closely at his analysis of the structure of physical theory itself.

The laws of common sense, which, according to Duhem, are based on observation and induction, are stated in qualitative terms like those that figure in the propositions and syllogisms of traditional Aristotelian logic. "All men are mortal," (where "men" is understood as a generic term for all human beings) was, for him, a paradigm case of such a common sense law. This law connects "the abstract idea of man in general" with "the abstract idea of death" in an ordinary universal affirmative sentence or proposition. Duhem believed that we are led rather "instinctively and naturally" from the observation of concrete cases to the formulation of such laws connecting two abstract ideas.

The fundamental hypotheses of physical theory, the French physicist believed, are very different from such

common sense truths. They are a different sort of product of human thought and we reach them by an entirely different process.

Duhem gave a brief summary of how one goes about building up a physical theory, partitioning the process into four separate steps. Let me consider this process one step at a time.

> 1. Among the physical properties which we set ourselves to represent we select those we regard as simple properties, so that the others will supposedly be groupings or combinations of them. We make them correspond to a certain group of mathematical symbols, numbers, and magnitudes through appropriate methods of measurement....[52]

We do not, according to Duhem, begin the construction of a physical theory by just observing particular concrete objects and their properties and then abstracting and generalizing until we get to something like the laws of common sense. We begin more deliberately by choosing certain properties and making them correspond to certain mathematical symbols.

Duhem's first example of making a physical property correspond to numbers "through appropriate methods of measurement" concerns the property of length. By comparing

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[52]Ibid., 19-20.

the lengths of different physical objects we come across the notions of equal and unequal lengths and we notice that the "equal to" and "greater than" relationships are transitive.

> Two lengths equal to the same length are equal to each other.
> If the first length is greater than the second and the second greater than a third, the first is greater than the third.[53]

We can express this in mathematical symbols. Let A, B, and C be the lengths of three objects, then

$$\text{If } A = B \text{ and } B = C \text{ then } A = C.$$

and

$$\text{If } A > B \text{ and } B > C \text{ then } A > C.$$

(Let us ignore, for the sake of argument, the case in which the difference in length between A and B and that between B and C are too small to be detected by themselves but when combined are large enough to create a detectable difference between A and C.)

We should also notice that the properties of associativity and commutativity apply. Thus, in symbols:

$$A + B = B + A$$

and

$$A + (B + C) = (A + B) + C$$

and, of course, also

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[53]Ibid., 108.

$$A + B > A \text{ and } A + B > B.$$

Thus, if we lay two objects end to end in the proper way and measure their combined length, it won't matter in which order we laid them. If we lay three objects end to end it won't matter in which order we add their lengths. And if we lay two objects end to end, their combined length will be greater than the length of either object by itself.

There is closure for lengths (as for numbers) under such operations as addition. A length added to a length is always a length. Such characteristics justify us in extending the rules of calculation in arithmetic to the lengths of physical objects. We can choose a standard of length as a unit and express the length of any object as multiples of this unit, with the aid of numbers. Duhem summarized this point as follows

> Thus, through the choice of a standard length and through measurement, we give to the signs of arithmetic and algebra, set up to represent operations done with numbers, the power to represent operations performed with lengths.[54]

We should add that the choice of a standard unit of measurement is a matter of convenience. We can measure length in inches and feet or in centimeters and meters. We

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[54]Ibid., 110.

can measure temperature on the Fahrenheit or the Celsius scale, or on some other scale. (It turns out to be convenient, for example, to state the ideal gas law using the Kelvin scale so that all temperatures are given in positive numbers.)

One of the consequences of using mathematics to aid physics in this way is that one potentially extends the possibility of calculating (with lengths, for example) beyond what it may be feasible to measure with any instrument and, of course, well beyond what can be perceived "by means natural to man." Whether reckoning the distance to far-away galaxies in light years or figuring microscopic distances in microns, we can now calculate the dimensions of supposed objects that we can never even hope to see with the naked eye.

Another consequence is that one substitutes the exactness of mathematics for the inexactness inherent in measurement. If a ruler cannot distinguish between a length of 999.9 millimeters and one of 1000.1 millimeters then one can report the length of the object we are using that ruler to measure with any of the infinitely many real numbers between 999.9 and 1000.1. If the length of the object is recorded as one meter, we should remember that this is, in reality, only approximate and not exact. This question of

precision, of course, does not arise with such common sense laws as "All men are mortal."

Duhem took the analysis farther by insisting that not only quantities but also qualities can be represented with the aid of numbers, provided that the qualities can differ in intensity. His example was the quality of heat.

Despite some similarities, there are important differences between heat and length, and physicists, in fact, treat properties such as heat differently from properties such as length.

> In the domain of quality, there is no room for addition; the latter does apply, however, when we study the quantitative phenomenon which provides a suitable scale on which to calibrate the different intensities of a quality.[55]

The use of the familiar mercury thermometer makes it possible to establish an interval scale to measure the intensity of heat (temperature) of a body by measuring the length of a column of mercury caused to expand by an increase in the intensity of heat. This enables us to apply arithmetic in the case of heat, in fact, by substituting for the intensity of heat of a body, the length of that column of mercury.

Notice that considerations of exactness and inexactness

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[55]Ibid., 118.

are extended to the use of instruments like the mercury thermometer. Like recorded lengths, recorded temperatures are also only approximate and not exact. Furthermore, what is true of the application of arithmetic is also true of the application of geometry. A solid may be not exactly a cube, its six faces not exactly square, its twelve edges not exactly straight line segments. The physicist, according to Duhem, will do his or her reasoning on the ideal solid, not on the actual solid that it represents.[56] (Perhaps it would have been better if he had said that the way the physicist reasons about actual solids is to use ideal solids to represent them.)

Another important consequence of using mathematics to aid physics in this way, hinted at before, is that physical theory cannot be left out of the laboratory. Whenever the intensity of a quality is measured by the quantity of change registered in some property other than that quality, the portion of physical theory that connects the two properties is brought into the picture. It is not merely the exactness of a thermometer reading, for example, that is at issue in an experimental result that turns on the measurement of temperature. There is also the accuracy of the lawlike

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[56]Ibid., 133-134.

statement connecting a given increase in temperature with a given expansion of a column of mercury. This is one very important reason that Duhem considered it impossible for a physicist to leave physical theory out of the laboratory when observing the results of an experiment.

Duhem continued his analysis of theory formation. Representing what we regard as simple properties is followed by a second step.

> 2. We connect the different sorts of magnitudes, thus introduced, by means of a small number of propositions which will serve as principles in our deductions. These principles may be called "hypotheses" in the etymological sense of the word for they are truly the grounds on which the theory will be built....[57]

It is a bit more difficult to analyze the formation of hypotheses by the physical scientist because, according to Duhem, there is not only an element of intuition involved in choosing a set of hypotheses but also a process of development. "Hypotheses Are Not the Product of Sudden Creation, but the Result of Progressive Evolution," he told us at the head of section two of Part II, Chapter VII of his book.[58] Some hypotheses may be several centuries in the making.

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[57]Ibid., 19-20.

[58]Ibid., 220.

This should not be surprising. It is fairly easy to imagine a casual observer seeing bolts of lightning in the distance and then hearing crashes of thunder and, quite naturally, coming up with the generalization "One sees the lightning before one hears the thunder." It is rather difficult to imagine such an observer so naturally coming up with quantitative hypotheses about light waves and sound waves.

Consider, for a moment, a hypothesis already formulated: the hypothesis that light is not a stream of particles emitted by a body but a set of transverse waves, of different wavelengths, propagated in a medium such as air or water. We may add that the velocity of these waves is finite and varies somewhat with the medium in which they are propagated. We needn't spell out these propositions in detail in order to notice that such terms as "wavelength," "frequency" and "amplitude" do not, in the case of light waves, denote anything that we can observe by means "natural to man." Yet they can figure in the calculations of physicists trying to test some portion of optical theory or, with the aid of optical theory, trying to test some other portion of physical theory.

Duhem continued. It is necessary to derive some testable consequences from the theory erected upon the

foundation provided by the fundamental hypotheses. This is done with the aid of logic and mathematics.

> 3. The diverse principles or hypotheses of a theory are combined together according to the rules of mathematical analysis...[59]

Thus, we need to derive consequences of these hypotheses that will pertain to the familiar phenomena of reflection, refraction and diffraction of light. Duhem did not, however, regard it as necessary that every operation performed upon numbers reflect an operation that can be performed on physical objects to whose properties those numbers were made to correspond. Some distances, for example, are far too great or far too small to be measured with any ruler. Some temperatures are far too high or far too low to be recorded on any mercury thermometer. It <u>is</u> necessary, of course, that the numerical results obtained from mathematical deduction be compared with the numerical results obtained from experiment but it is <u>not</u> necessary that every number that figures in the deduction be so comparable.

This brings us to the final step. We must compare the results of mathematical deduction with the results of experimental inquiry.

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[59]Ibid., 19-20.

> 4. The various consequences thus drawn from the hypotheses may be translated into as many judgments bearing on the physical properties of the bodies.... These judgments are compared with the experimental laws which the theory is intended to represent...[60]

This sketch of the process of constructing a physical theory touches upon some of the features of this process that, Duhem believed, are central to the structure of physical theory. They are also relevant to his version of the central thesis.

Duhem insisted that in physical experiments it is not the mere observation of a phenomenon that matters; the theoretical interpretation of that phenomenon is what counts. Suppose that one of the consequences drawn from our hypotheses is translated into a statement specifying the heat of a given body at a given time. We do not simply observe that the body has such and such an intensity of heat. What we do is use an instrument--a mercury thermometer, for example--which measures the length of a column of mercury inside a glass tube. Physical theory informs us that the length of this column of mercury expands in a regular way as an effect of the heat applied to it.

Here there is not only the question of idealization but

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[60]Ibid., 20.

also the question of the introduction of a new part of physical theory with the introduction of the instrument. We need to remember that it is only because physical theories that we accept tell us that mercury expands in a lawlike way with an increase in the intensity of heat that we can, in good conscience, use the thermometer to measure an increase or decrease of temperature.

We also should not forget the pervasive role of optical theory in physical science. It is only because we accept a system of hypotheses that sanctions the use of such devices as telescopes and microscopes that we can use them with a good conscience. In the absence of optical theory, we might well be reasonable to repeat the refusal of scholastic philosophers to look through Galileo's telescope on the grounds that the lenses are distorting what we see, just as the curved mirrors in an amusement park's funhouse distort what we see.

Physical theory, then, is thoroughly intertwined with physical experiment, since even the interpretation of the results of an experiment relies, to some extent, on some aspect of physical theory. It would seem that we are, at this point, even farther than we might have expected from having physical experiment condemn isolated hypotheses of physical theory. Not only do we need the cooperation of a

number of hypotheses in order to derive a prediction that can be confronted with the results of a physical experiment, but we also need the cooperation of other hypotheses even to interpret the very results of that experiment. Is it any wonder that Duhem believed that a physical experiment "can never condemn an isolated hypothesis but only a whole theoretical group"?

Conclusion

As I have argued above, Duhem's theses, when viewed correctly, are much more modest than they first appear to be.

Duhem did <u>not</u> deny, for instance, that there are ordinary cases in which a statement that one might call "an isolated hypothesis" in science can be falsified. A universal generalization such as "All swans are white," framed in qualitative terms and (perhaps) the result of abstraction and inductive generalization, is (quite plausibly) such an ordinary case. Such low-level empirical generalizations--they are similar to what Duhem called "laws of common sense"--are not the stuff of which modern theoretical physics is made.

What he <u>did</u> deny was that what he called "an isolated hypothesis" of physical theory can be falsified. Whether a fundamental hypothesis of physical theory, or some other

hypothesis joined to it as part of a theoretical group, it did not have testable consequences of its own and so did not directly confront the evidence of physical experiment itself. The emission hypothesis of light is an example of such an isolated hypothesis.

Furthermore, Duhem did not deny that there can be what might be called "a crucial experiment" in physics: an experiment that confirms or corroborates one whole theoretical group while falsifying another. The famous experiment in optics falsified the system of emission (though not the hypothesis of emission by itself). In Duhem's terms it showed the system of emission to be "stained with error" without showing which individual hypothesis was the source of the stain. Those physicists who regarded the system of wave propagation as confirmed by this experiment received no objection from their French colleague.

What Duhem did deny was that there can be what he called "a 'crucial experiment'" that demonstrates either an unrefuted isolated hypothesis or a whole unrefuted theoretical group, in the manner of an indirect proof in geometry. Physics is not like mathematics in this respect: there is no room for reductio ad absurdum argument in the assessment of experimental contradiction, since the two

hypotheses or theoretical groups are related as contraries, not as contradictories.

It is also important to add that Duhem never denied that sometimes maintaining a given hypothesis in the face of contradictory evidence against the system of which it is a part is simply bad judgment. In the same chapter in which he presented his two theses he discussed this issue in the last section. "Good Sense," begins the title of section 10 of Chapter VI, "Is the Judge of Hypotheses Which Ought to Be Abandoned."

> Pure logic is not the only rule for our judgments; 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.[61]

Good sense, however, is somewhat vague and does not have the force of logic. Thus, lengthy disputes among the partisans of different theories are possible, as the history of physics shows. (The situation is not so bad in physics as it is in metaphysics. The disputes in physics are long but not interminable.) Logic alone cannot tip the scales to one side or the other. Duhem capped off his discussion of

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[61]Ibid., 217.

this point with a final comment about one of his favorite examples:

> In any event this state of indecision does not last forever. The day arrives when good sense comes out so clearly in favor of one of the two sides that the other side gives up the struggle even though pure logic would not forbid its continuation. After Foucault's experiment had shown that light traveled faster in air than in water, Biot gave up supporting the emission hypothesis; strictly, pure logic would not have compelled him to give it up, for Foucault's experiment was not the crucial experiment that Arago thought he saw in it, but by resisting wave optics for a longer time Biot would have been lacking in good sense.[62]

As I've noted above, Duhem was not opposed to extending his analysis beyond the boundaries of physical science, he merely refrained from extending this analysis himself. How approvingly he would have regarded such an extension would, no doubt, depend on how similar the science in question seemed to physical science.

An important element was the role of mathematics in that science. Those social scientists, for example, who see a parallel between the use of mathematics in physics and its use in economics might argue that the French physicist would have given his blessing when they extended his thesis about isolated hypotheses to their domain. This is especially so if they see the logical structure of economics as basically

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[62]Ibid., 218.

similar to that of physics.

On the other hand, some social scientists have believed that there are differences here between physics and economics that have a bearing on this issue. Friedrich Hayek, for example, argued that while the method of natural sciences like physics is resolutive and compositive, the method of the social sciences is strictly just compositive.

> While in the [social sciences] it is the attitudes of individuals which are the familiar elements and by the combination of which we try to reproduce the complex phenomena...the physical sciences necessarily begin with the complex phenomena of nature and work backward to infer the elements from which they are composed.[63]

According to Hayek's analysis, there are isolated hypotheses forming the foundations of social (e.g. economic) theory that can be established "by means natural to man," to borrow a phrase from Duhem. These hypotheses would simply tie together the acting individuals of economic theory in principles some of which could themselves be confronted with empirical evidence. Such hypotheses would themselves be, in the language of Chapter One, test statements of scientific theory.

Since Duhem made no pronouncement on the role of mathematics in social theory and presented no analysis of

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[63]Friedrich Hayek, The Counter-Revolution of Science (Glencoe, Illinois: The Free Press, 1952) 65-66.

the formation of hypotheses in social theory, it seems impossible to tell whether he would have sided with Hayek. He would surely have balked, however, at extending his holistic thesis to cover sciences that are, in his words, "still close to their origins." It seems to me that he had good reason to balk. A look at some fairly simple arguments may help us to see why.

Consider an ordinary categorical syllogism:

(2-5) All mammals have lungs.

(2-6) All dolphins are mammals.

(2-7) All dolphins have lungs.

Suppose, for the sake of argument, that we have examined a given dolphin and determined that it does not have lungs. We may say, then, the conclusion of the syllogism, "All dolphins have lungs," is falsified. It is tempting to add that Duhem's holistic thesis applies here: we cannot tell which of the two premises from which this statement is derived is false. (Of course, they cannot both be true.) We cannot, that is, falsify either of the two premises (2-5 and 2-6 above) in isolation.

This, however, is a temptation that should be resisted. Here we have a case in which the "laws" of a science, if we want to call them that, are still (in Duhem's phrase)

"dependent upon common sense." Assuming that we can tell, "by means natural to man," whether a given dolphin has lungs, we can also tell by such means whether that dolphin is a mammal. The second premise, then, can be tested by observation as easily as the conclusion and this is not a case to which the central thesis applies. In other words, if (2-7) is a test statement, then so is (2-6) and we do not have here the derivation of a test statement from two or more nontest statements.

To the extent that a given science is built up of such lawlike statements--and early modern taxonomy in biology may be a good illustration--it is still close to its origins. It seems unlikely that Duhem would have approved of our extending his analysis to it. It seems to me that he would be right.

Not every case outside of physics, however, is as easy as this. In my view, it would be a mistake to suppose that the plausibility of "the Duhem-Popper-Quine thesis" depends on the introduction of mathematics into science. Perhaps the mathematization of theory in a given domain is a sufficient condition for the extension of Duhem's holistic thesis to it but it surely is not a necessary condition. What <u>is</u> a necessary condition is the impossibility of confronting a hypothesis directly with the results of

observation or experiment. The use of qualitative or comparative terms or concepts in a science is consistent with this impossibility.

We needn't suppose that all of the terms of a given science are neatly divided into theoretical and observational terms to suppose that there are at least some qualitative (or classificatory) terms or concepts in a science that are such as to render some of the statements in which they occur nontest statements in the sense of Chapter One. A categorical syllogism whose middle term is such a term may provide us with a case to which Duhem's holistic thesis applies, even without the introduction of mathematics. And let one of the premises of such a syllogism be deduced by a long chain of argument from other nontest statements of scientific theory and we can easily have a case in which the results of observation or experiment cannot condemn an isolated hypothesis but only a whole theoretical group.

Any branch of science in which such arguments figure is a branch of science to which Duhem's holistic thesis can plausibly be extended. The use of intervening variables in some branches of psychology may provide a good example of a science in which there are terms that render the statements

in which they appear nontest statements.[64] Indeed, Duhem's holism would seem to apply to any science that is theoretical to the extent that it involves the deliberate construction of a deductive system, as opposed to the mere registering of ordinary empirical generalizations. This deliberate construction need not involve the introduction of mathematics.

Furthermore, Duhem's first thesis can be extended to any science that has certain kinds of hypothesis containing mixed multiple quantifiers, regardless of the absence of mathematics and regardless of the nature of its qualitative terms. Indeed, it can be extended to his own example of a law of common sense:

(2-8) All men are mortal.

Although this seems to be an ordinary categorical sentence or proposition, it is surely better analyzed as a statement of mixed multiple quantifiers.

(2-9) For every man there is a time at which that man will die.

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[64]Some authors have made a distinction between what they call "intervening variables" in a narrow sense and what they dub "hypothetical constructs." See MacCorquodale, Kenneth and Paul E. Meehl, "Hypothetical Constructs and Intervening Variables" in Herbert Feigl and May Brodbeck, ed. Readings in the Philosophy of Science (New York: Appleton-Century-Crofts, 1953)

or, in the customary symbolism,

$$(2\text{-}9^*) \qquad (x)(\exists t)(Mx \rightarrow Dxt).$$

As Quine has pointed out in a critical discussion of Popper's views, this statement cannot (to borrow Duhem's phrase) be "condemned" in isolation. For no matter how long a (finite) period of time we observe someone, without seeing his or her demise, there may still be some future time at which that person dies.[65] Here there is no mathematics, no physics to speak of, and no term that would normally be classified as theoretical. And yet there is no way to confront this statement directly with the results of observation or experiment.

Duhem did not discuss hypothesis, theory, observation and experiment in terms that would become familiar a generation or so later in philosophy of science. He did not, for example, explicitly make a rigid distinction between observational and theoretical terms in physical science. What he <u>did</u> do, however, was (in effect) to create a rigid distinction between observational and theoretical sciences and to focus his attention on that theoretical science in which mathematics is thoroughly interwoven with

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[65]Cf. W.V. Quine, "On Popper's Negative Methodology," in <u>The Philosophy of Karl Popper</u>. Edited by Paul Arthur Schilpp. The Library of Living Philosophers (La Salle, Illinois: Open Court, 1974)

theory. He failed to see that the difference between theoretical physics and other sciences still close to their origins in common sense is a matter of degree.

He also seems to have underestimated the theory-ladenness of observation outside of physics. Recall his brief discussion of a simple experiment in physiology. When the physiologist tests his hypothesis that the anterior roots of the spinal nerve contain the motor nerve-fibers and the posterior roots the sensory fibers, he or she cuts a certain anterior root and observes the results. Duhem told us that "his report must be a raw description of the facts."

Suppose, for the sake of argument, that we can appropriately describe such a report as a "raw description of the facts." A physiologist describing the results of an experiment on a living animal would fairly quickly move a good distance from any such raw description. Even such an ordinary operation as taking the pulse of another human being while that person is engaging in physical exercise involves some processing of the facts. If you take a pulse at the carotid artery, for example, you may unintentionally slow the beating of the heart because the pressure may stimulate the vagus nerve, which is located near the artery. And if you try to take the pulse with your own thumb, the sensation you get may be unrelated to the other person's

heart rate. There is an artery in your thumb and you may simply be feeling your own pulse. A "raw description of the facts" uninformed by physiological theory may lead you, in either case, to underestimate the effects of the exercise on the heartrate of the person in question.

As we will see in Chapter Four, Duhem differed from Quine in one important respect. Although he did not object to extending his analysis beyond physics, he did not envision extending it as far as logic and mathematics. He sought to give "a simple logical analysis of the method by which physical science makes progress." Replacing old physical theories with new ones that stand up to more exacting experiments conducted with more accurate instruments, he believed, plays a part in that progress. Discarding the principles of traditional logic and mathematics does not.

Duhem did not discuss at any length a view of the sentences or propositions of logic and mathematics either as analytic truths or as synthetic truths that can be known a priori. There is evidence, however, that he took some such view for granted.

> The materials with which [physical] theory is constructed are, on the one hand, the mathematical symbols serving to represent the various quantities and qualities of the physical world, and, on the other hand, the general postulates

> serving as principles. With these materials theory builds a logical structure; in drawing the plan of this structure it is hence bound to respect scrupulously the laws that logic imposes on all deductive reasoning and the rules that algebra prescribes for any mathematical operation.[66] (italics added)

As we will see in Chapter Four, Quine takes a somewhat different view of the status of logic and mathematics.

We must be careful not to read into Pierre Duhem views that are anachronistic. Although we can learn much from him we should not assume that he was merely anticipating debates that would take place two generations after his death. We should also not assume that, in such debates, he would automatically take sides with an author who invoked his name and quarrel with one who took his name in vain. Although there are some important differences, there are also many similarities between Duhem's views and those of Karl Raimund Popper, the Austrian-born philosopher of science to whom we now turn our attention.

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[66]Duhem, Aim and Structure, 205.

CHAPTER THREE:

Popper's Guarded Holism

Introduction

Some 60 years have passed since the publication of Karl Popper's first major work, Logik der Forschung.[1] During those six decades, the Austrian philosopher of science became famous--some would say that he became infamous--for his vigorous and persistent advocacy of what we have learned to call "falsificationism."

In this chapter I will examine critically both Popper's holism and his falsificationism. I will argue that his guarded holism, restricted to empirical science as traditionally understood, is, with a slight modification, defensible. It will be one of my conclusions, for example, that it is consistent for Popper to hold, on the one hand, that if his methodological proposals are adopted then any one of the hypotheses of empirical science is falsifiable in principle, and to hold, on the other hand, that many of the hypotheses of empirical science cannot be falsified in isolation, even if those proposals are adopted. I will also argue that what Popper said about crucial experiments is

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[1]Karl Popper, Logik der Forschung (Vienna: J. Springer, 1935)

consistent with Duhem's second thesis.

In the second of the seven sections of this chapter I will present an outline of Popper's views on the deductive testing of theories in empirical science. This will provide an important background for his guardedly holistic view of testing. In the third section, I will take a closer look at that guarded holism. Despite some differences between Duhem and Popper, there are strong similarities between their views, although, for example, the latter did not confine his attention to physical science. In the fourth section, I will take a closer look at what Popper had to say about crucial experiments and will argue that, here too, he and Duhem were not very far apart, despite initial appearances.

In the fifth and sixth sections I will examine two different sides of Popper's falsificationism: first, the logical side; and second, the methodological side. Popper paid a good deal of attention to the confrontation between what I have called "test statements of scientific theory" (derived from what I have called "nontest statements of scientific theory") and what I have dubbed "evidence statements." A careful examination of what he said on this topic will help to show both the strengths and the weaknesses of his falsificationism, and where it is necessary to modify it.

Finally, in the seventh section of the chapter, I will review the conclusions drawn from my critical examination of Popper's holism and his falsificationism and make some further observations about them in light of his philosophy of science as a whole. I will argue that his holism is defensible, although it does create problems for some other aspects of his philosophy of science. (My discussion of a slight modification in Popper's holism will be deferred until Chapter Five.)

The Deductive Testing of Theories

Written during the heyday of the logical positivism of the Vienna Circle, and actually published (in the fall of 1934) as one monograph in a series edited by several members of the Circle, Logik der Forschung was a controversial book. It was also (and still is, I believe) a somewhat misunderstood book. Finally translated from German into English by the author, with some assistance from Julius and Lan Freed, it was published with the title The Logic of Scientific Discovery nearly 25 years after its initial publication in Austria.

This early work of Popper's, perhaps the best single source for his philosophy of science, presents a view that contrasts sharply with the inductivism and verificationism

of the logical positivists. This should not be surprising. Popper fully intended to draw such a sharp contrast.

> The theory to be developed in the following pages stands directly opposed to all attempts to operate with the ideas of inductive logic. It might be described as the theory of the deductive method of testing, or as the view that a hypothesis can only be empirically tested--and only after it has been advanced.[2] (italics in the original)

Popper found unacceptable the view that science, at any point, follows any sort of inductive procedure. Not only did he reject outright the claim that scientists discover empirical laws by a method of inductive generalization from observed instances, but he also rejected the claim that scientists can conclusively verify, or even confirm, such empirical laws simply on the basis of observed positive instances.

There are actually two distinct arguments that Popper gave for rejecting inductive generalization as the method of scientific inquiry: a logical argument and an epistemological one. The logical argument hinges on the premise that no number of individual positive instances, even if accepted, can prove (or justify, or verify) an unrestricted universal generalization. Popper stressed this point repeatedly, both in his early work and in his more recent

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[2]Popper, Logic of Scientific Discovery, 30.

books. His early statements, though perhaps couched in less polemical terms than some of his later pronouncements, were rather straightforward.

> Now it is far from obvious, from a logical point of view, that we are justified in inferring universal statements from singular ones, no matter how numerous; for any conclusion drawn in this way may always turn out to be false: no matter how many instances of white swans we may have observed, this does not justify the conclusion that all swans are white.[3] (italics in the original)

As Popper explained, the statement "All swans are white" is to be understood here as what he called a "strictly universal statement," covering every swan throughout all of space and time, and not merely a "numerically universal statement," covering only those swans that are now living on earth or only those that have been observed.[4] Both the conclusion "All swans now living on earth are white" and the conclusion "All swans that have so far been observed are white," for example, can be justified by observing finitely many white swans (and no nonwhite ones).

Popper's later statements on this score were quite straightforward, although couched in slightly different

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[3]Ibid., 27.

[4]Ibid., 62-64.

language. He never abandoned his opposition to inductivism. (In both early and later work, Popper acknowledged his indebtedness to Hume.)

> Can the claim that an explanatory universal theory is true be justified by 'empirical reasons'; that is by assuming the truth of certain test statements or observation statements...
>
> My answer to the problem is the same as Hume's: No, it cannot...[5]

Here Popper, as he often did, used the word "theory" so broadly that it would include ordinary universal generalizations of the kind that we have been considering. (As we will see, his looseness in using this word can easily give rise to problems and misunderstandings.)

Popper's epistemological argument against inductive generalization is perhaps less indebted to Hume than to Kant. It depends on the impossibility of observing repeated instances that might verify, or confirm, a universal generalization in the absence of some theory or conceptual framework according to which observed instances can be classified as instances of the same generalization.

> But in fact the belief that we can start with pure observations alone, without anything in the nature of a theory, is absurd....
>
> Observation is always selective. It needs a chosen object, a definite task, an

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[5]Popper, "Conjectural Knowledge" in Objective Knowledge, 7.

> interest, a point of view, a problem. And its description presupposes a descriptive language, with property words; it presupposes similarity and classification, which in its turn presupposes interests, points of view, and problems.[6]

It seems to me that this second argument, however cogent and salutary, is really directed against a very unsophisticated sort of inductivism. Though it may be a useful corrective to some of the statements of such a philosopher of science as Francis Bacon, it is really not an appropriate criticism of more sophisticated inductivists like Rudolf Carnap and Hans Reichenbach.

Whatever the merits of Popper's epistemological argument as a criticism of other thinkers, though, it is useful as a clue to his own position. For Popper emphatically rejected the tabula rasa view that we often associate with John Locke. We do not, according to him, begin learning about the world around us by observing it and building up a stock of generalizations about it. Instead, we come into this world with a stock of expectations, many of which are disappointed. It is this process of expectation followed by disappointment, made self-conscious, that (very roughly speaking) is his version of the scientific

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[6]Popper, "Science: Conjectures and Refutations" in Conjectures and Refutations, 46.

method.

> I assert that every animal is born with expectations or anticipations, which could be framed as hypotheses; a kind of hypothetical knowledge. And I assert that we have, in this sense, some degree of inborn knowledge from which we may begin, even though it may be quite unreliable. This inborn knowledge, these inborn expectations, will, if disappointed, create our first problems; and the ensuing growth of our knowledge may therefore be described as consisting throughout of corrections and modifications of previous knowledge.[7] (italics in the original)

Thus, Popper believed, scientists do test their theories, but all such testing is strictly deductive. He distinguished four lines along which the critical examination of theories can take place.[8]

First, the conclusions deduced from the theory can be compared with one another to test the theory for consistency. Second, the logical form of the theory itself can be examined to see whether it is, for example, a set of logical truths as opposed to a set of empirical statements. Third, the theory can be compared with other theories in its domain to see whether it would represent an advance if it were to survive empirical tests. Finally, the theory itself can be

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[7]Popper, "Evolution and the Tree of Knowledge" in Objective Knowledge, 258-259.

[8]Popper, Logic of Scientific Discovery, 32-33.

submitted to empirical tests.

Before considering the last of these four lines of inquiry in more detail, I want to take a brief closer look at the third line of inquiry because it may need some clarification. Even more than Duhem, Popper was motivated by a desire to understand the way in which science can make progress.

> The central problem of epistemology has always been and still is the problem of the growth of knowledge. And the growth of knowledge can be studied best by studying the growth of scientific knowledge.[9] (italics in the original)

It goes without saying that since he believed that scientific knowledge grows, he believed that there is such a thing as progress in science. Although this is, in itself, a difficult and complex topic, a few brief comments are in order to give an idea of what sort of progress Popper had in mind here.

First, Popper rejected the belief that science grows by accumulating observation reports (along with the belief that these observation reports enable us to verify universal generalizations). Second, he rejected the view that scientists ought to hedge their bets by qualifying their hypotheses to guard against falsification by counterexample.

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[9]Ibid., 15.

This is especially pertinent since, in his view, not every trade-in of a falsified hypothesis for an unfalsified one represents an advance. Only if the new hypothesis is bolder, more falsifiable, than the previous one (that is, if it rules out more) does it, in Popper's view, represent an advance over its predecessor.

For example, trading in the hypothesis "All swans are white" for the more cautious one "All European swans are white," after discovering nonwhite swans in Australia, does not represent such an advance because we get a newer hypothesis that says (rules out) less in exchange for an older hypothesis that says (rules out) more. Instead of timidly hedging their bets, scientists, according to Popper, should be daring and resourceful. There may frequently be no clear path to a bolder, more falsifiable hypothesis that has not already been refuted but that is no reason to shrink from the challenge of trying to find one.

Popper elaborated a good deal on the last of his four lines of inquiry and this elaboration began almost immediately after distinguishing among them.

> With the help of other statements, previously accepted, certain singular statements--which we may call 'predictions'--are deduced from the theory; especially predictions that are easily testable or applicable. From among these statements, those are selected which are not derivable from

> the current theory, and more especially those which the current theory contradicts. Next we seek a decision as regards these and other derived statements.[10]

It was an important part of the procedure, according to Popper, that there was no appeal, at any stage, to anything that could plausibly be considered inductive reasoning. Even a positive result of the empirical testing outlined above did not verify (or confirm) the hypothesis that was being tested.

The important results were negative ones, for they falsified the hypothesis. Those hypotheses that survived tests were, for the time being, retained. The failure to falsify them meant that they were, to use Popper's term, "corroborated," but they were not immune to falsification in the future. Their acceptance was only temporary and provisional. Furthermore, mere positive instances, though they might be regarded by some inductivists as confirming a hypothesis, were not regarded by Popper as corroborating it. This is most clearly brought out in a later work.

> Every genuine test of a theory is an attempt to falsify it, or to refute it....
> Confirming evidence should not count <u>except when it is the result of a genuine test of the theory</u>; and this means that it can be presented as a serious but unsuccessful

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[10]Popper, <u>Logic of Scientific Discovery</u>, 33.

> attempt to falsify the theory.[11]
> (italics in the original)

I have already suggested that Popper sometimes used the word "theory," or such phrases as "explanatory theory," very broadly. In fact, there is no implication here that "theories," in his broad sense of the word, contain theoretical terms different from those in observational statements or that they are about unobservables. There is not even a requirement that a "theory" consist of more than a single statement. "Scientific theories are universal statements." he told us, contrasting universal statements with singular statements.[12] (It would have been more faithful to his actual usage, I think, if Popper had said that scientific theories are sets of statements that have universal statements among their members, leaving open the possibility that a given set [i.e., theory] has a universal statement as its only member.)

One of Popper's examples of a causal explanation was similar to some of Hempel's examples of explanations and his analysis of this example was similar as well.

> To give a causal explanation of an event means to deduce a statement which describes

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[11]Popper, "Science: Conjectures and Refutations" in Conjectures and Refutations, 36.

[12]Popper, Logic of Scientific Discovery, 59.

> it, using as premises of the deduction one or more universal laws, together with certain singular statements, the initial conditions. For example, we can say that we have given a causal explanation of the breaking of a certain piece of thread if we have found that the thread has a tensile strength of 1 lb. and that a weight of 2 lbs. was put on it.[13]
> (italics in the original)

A few passing remarks seem to be in order. First, Popper (unlike Hempel) did not accept what the latter called "inductive-statistical" explanations using one or more nonuniversal, or statistical laws. Second, Popper (again unlike Hempel) did not require that what he called "universal laws" be true. (Some of the statements that Popper called "universal laws" would be called "lawlike sentences" by Hempel, and some of what Popper called simply "explanations" would be called "potential explanations" by Hempel.)[14]

I should add, however, that Popper (like the early Hempel) did believe in a symmetry between explanation and prediction. This is clear from his treatment of the thread-breaking example as illustrating either an explanation (as above) or a prediction (as below).

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[13]Popper, Logic of Scientific Discovery, 59-60.

[14]Hempel, "Studies in the Logic of Explanation" in Aspects, 251, 264-270, 277-278.

> It is from universal statements in conjunction with initial conditions that we deduce the singular statement, 'This thread will break.' We call this statement a specific or singular prediction.[15]

Predictions that are derived from universal statements and statements of initial conditions--and we should also include postdictions, or "retrodictions" as Popper dubbed them--are not always true. Therein lies the rub. When a prediction fails to come true, the set of statements from which it was derived has failed its test. The tricky question, of course, is which statement in the set is responsible for the failure.

Popper did not believe that this question always had a clear and unambiguous answer but neither did he believe that this question was so difficult that it could never be answered. His holism was less restricted than Duhem's--he did not confine himself to physical science--but it was also (and partly because it was less restricted) more guarded than Duhem's holism. Popper did not take the position that the results of observation or experiment could never condemn an isolated hypothesis in empirical science.

Isolated Hypotheses and Theoretical Groups

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[15]Popper, Logic of Scientific Discovery, 60.

There are both similarities and differences between the views of our first two authors. One difference is that Popper's account of the testing of theories involves no rigid distinction among the empirical sciences, or, for that matter, between scientific reasoning and common sense reasoning. Unlike Duhem, Popper drew no distinction between sciences still close to their origins and those that have moved farther away (in effect, a distinction between theoretical and observational sciences). And he recognized no field in which one arrives at general laws by a process of abstraction and induction from observed instances.

There is, however, an important similarity. Although he was concerned with empirical science generally, and not just with physical science, what Popper said about falsifying an individual statement, as opposed to a whole system of statements, is strikingly similar to what Duhem said about falsifying an isolated hypothesis, as opposed to a whole theoretical group.

Recall Duhem's summary of his own position, quoted above (Chapter Two).

> In sum, the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought

> to be modified; but the experiment does not designate which one should be changed.[16]

Popper, though he used slightly different terminology, initially articulated a view of the possibility of testing isolated hypotheses that is very similar to Duhem's view.

> The falsifying mode of inference here referred to--the way in which the falsification of a conclusion entails the falsification of the system from which it is derived--is the modus tollens of classical logic....
>
> By means of this mode of inference we falsify the whole system (the theory as well as the initial conditions) which was required for the deduction of the...falsified statement.[17] (italics in the original)

He drew his conclusion from this analysis a bit later in the same passage.

> Thus it cannot be asserted of any one statement of the system that it is, or is not, specifically upset by the falsification.[18]

This analysis applies to the falsification of singular predictions derived from universal statements together with singular statements of initial conditions. It can also be extended to include the falsification of a universal statement derived from other universal statements. Although the conclusion is upset by the falsification, we cannot say of

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[16]Duhem, Aim and Structure, 187.

[17]Popper, Logic of Scientific Discovery, 76.

[18]Ibid.

any given one of the premises that it is definitely upset.

Although Popper did not restrict his holism to physical science, he might have had some sympathy with a point made at the end of Chapter Two. Whenever a hypothesis from which (in conjunction with other statements) a false statement is derived can be directly confronted with the evidence then we can, under some circumstances, say that it is specifically upset. In such a case, the hypothesis is itself a test statement rather than a nontest statement. The example that I gave in Chapter Two was the following derivation of a test statement:

(2-5) All mammals have lungs.

(2-6) All dolphins are mammals.

(2-7) All dolphins have lungs.

I argued that the second of these two premises can be confronted with the evidence as easily as the conclusion and therefore does not qualify as a nontest statement. It isn't the case, however, that examples like this are all that you find when you venture beyond physical theory.

Popper did not make rigid distinctions between or among the empirical sciences and he did not present an analysis of the structure of physical science that suggests that its hypotheses are more removed from experience than are those

of, say, economics or biology. Nor did he limit his analysis to those empirical sciences in which mathematics, or the use of devices like telescopes, microscopes and mercury thermometers, plays an important part. He did, however, make a hard and fast distinction between empirical sciences such as physics and biology, on one hand, and the "demonstrative sciences" (his phrase) of logic and mathematics, on the other.

Before turning to Popper's view of the demonstrative science of logic though, I want to consider some of the things he said about the empirical sciences. Some of his later comments suggest that he modified his holism and some contemporary philosophers of science have treated Popper and Duhem as if their views are inconsistent, perhaps as a result of reading these later comments. It seems to me that at least part of the dispute stems from a misunderstanding. Some of Popper's critics have misunderstood his views (and those of his allies) and he and his allies, in turn, have misunderstood the views of some of those critics.

Mary Hesse, for example, undertook to defend Duhem against Popper. Even Popper himself sometimes wrote as if he had to criticize Duhem's and Quine's views in order to defend his own. One passage, quoted, in part, by Hesse is a

case in point.[19] The original passage is from one of Popper's later works.

> It seems to me quite clear that it is only through these temporary successes of our theories that we can be reasonably successful in attributing our refutations to definite portions of the theoretical maze. (For we are reasonably successful in this--a fact which must remain inexplicable for one who adopts Duhem's and Quine's views on the matter.)[20]

Although this latter sentence is literally a parenthetical remark made in the context of the discussion of a different issue, it is a clear indication that Popper was critical of what he took to be Duhem's and Quine's views on the matter.

Popper's claim that we are reasonably successful in attributing various refutations to definite portions of the respective theoretical mazes in question should be seen in the light of certain earlier passages in the same essay in which he presented what he considered his answer to Duhem and Quine.

> We can never be certain that we shall challenge the right bit [of background knowledge]; but since our quest is not for certainty, this does not matter. It will be

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[19]Hesse, "New Empiricism," 189.

[20]Popper, "Growth of Knowledge," 243.

> noticed that this remark contains my answer to Quine's holistic view of empirical tests; a view which Quine formulates (with reference to Duhem), by asserting that our statements about the external world face the tribunal of sense experience not individually but only as a corporate body.[21]

Popper's response to this assertion conceded more than one might expect.

> Now it has to be admitted that we can often test only a large chunk of a theoretical system, and sometimes perhaps only the whole system, and that, in these cases, it is sheer guesswork which of its ingredients should be held responsible for any falsification; a point which I have tried to emphasize--also with reference to Duhem--for a long time past.[22]

Hesse went on to quote Popper's contention that "the holistic argument goes too far" and examined what she considered to be the three ways in which Popper claimed that an isolated hypothesis can be identified.

> (i) We may provisionally take for granted the background knowledge common to two theories for which we design a crucial experiment, and regard the experiment as refuting one or other of the theories rather than the background knowledge....
> (ii) We may be able to axiomatise the whole theoretical system in such a way as to isolate the effect of a single axiom, which may then be refuted in isolation....
> (iii) When successful predictions have occurred, Popper seems to suggest, we

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[21]Ibid., 238-239.

[22]Ibid., 239.

> are more reluctant to abandon those parts of the theory responsible for them...[23]

She has, it seems to me, overlooked an important point buried in Popper's criticism, although his careless exposition may be partly responsible for this.

> It is possible in quite a few cases to find which hypothesis is responsible for the refutation; or in other words, which part, or group of hypotheses, was necessary for the derivation of the refuted prediction.[24]

Popper's use of the phrase "in other words" here, suggesting that his second remark is merely paraphrasing his first one is a bit misleading. It may be possible to isolate from a set of nontest statements of theory a proper subset that is necessary for deriving a test statement, the other nontest statements being irrelevant to the derivation. It doesn't follow, however, that exactly <u>one</u> of those nontest statements can be isolated.

A further caveat is in order. Even if a false test statement can be derived from two nontest statements without the assistance of a third, it doesn't follow that the third statement is off the hook. Perhaps the same false test

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[23]Hesse, "New Empiricism," 189.

[24]Popper, "Growth of Knowledge," 239.

statement can be derived from that third statement together with a fourth. The fact that a given nontest statement is excluded from one derivation doesn't mean that it is excluded from all of them. If there is more than one way to derive the false test statement from the nontest statements of a theory then there is more than one way to single out a subset of nontest statements that is put at risk by the falsification. Whatever nontest statements, taken together, logically imply the false test statement are, each one individually, at risk.

Popper put a good deal of stress on the axiomatization of a theory.

> Now let us say that we have an axiomatized theoretical system, for example of physics, which allows us to predict that certain things do not happen, and that we discover a counter example. There is no reason whatever why this counter example may not be found to satisfy most of our axioms or even all our axioms except one whose independence would thus be established.[25]

Hesse, I believe, was only partly correct when she criticized Popper's view.

> But even if we disregard the extreme impracticability of such axiomatisation in the case of most interesting scientific theories, its ideal possibility still does not refute Q[uine] because no axiomatisation can fully account for the empirical applicability of

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[25]Ibid.

> the system, and the correctness of the conditions of application (the so-called 'correspondence rules') might always be called into question to avoid abandonment of any of the axioms.[26]

Popper, however, made an important point. In the kind of case that Hesse imagines, if we can exonerate, say, two of the axioms and indict the remaining axiom(s) and the correspondence rules, something is gained. We can put the point in Duhemian terms: when a theoretical group is condemned by observation or experiment we can sometimes break up that group into smaller parts and show that only one of those parts is condemned. Vuillemin has misunderstood Popper on this point.

> In fact the whole theory of Die Logik der Forschung was to show how an isolated hypothesis of a scientific theory can be falsified, because even if the theory has not been completely axiomatized and its several hypotheses have not been shown to be independent, the different parts of physics are clearly enough organized to allow us to decide which subsystems are questioned by a falsifying observation (para 16). This enterprise, completely adverse to a holistic philosophy, is the real ground for the asymmetry between confirmation and falsification.[27]
> (italics in the original)

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[26]Hesse, "New Empiricism," 189.

[27]Jules Vuillemin, "On Duhem's and Quine's Theses," in The Philosophy of W.V. Quine. Edited by Paul Arthur Schilpp and Lewis Edwin Hahn. The Library of Living Philosophers. (La Salle, Illinois: Open Court, 1986) 617.

Vuillemin's reference is to section 16 of Popper's book, which ends as follows:

> ...the falsification of a logically deduced statement may sometimes not affect the whole system but only some part of it, which may then be regarded as falsified. This is possible because, although the theories of physics are in general not completely axiomatized, the connections between its various parts may yet be sufficiently clear to enable us to decide which of its sub-systems are affected by some particular falsifying observation.[28]

Plainly, showing how some part of a theory can be falsified is not the same thing as showing how an isolated hypothesis can be falsified. As we will see a bit later, Vuillemin has misunderstood Popper's remarks about the asymmetry between verification and (not confirmation but) falsification as well.

If Hesse and Vuillemin have missed one of Popper's points, however, Popper has apparently missed a few points himself. It seems that the Austrian philosopher sometimes read other authors as taking a stand that was, in some respects, stronger than the one they actually took.

First, although Duhem denied that an isolated fundamental hypothesis in physics can be refuted, this leaves open the question of how large or small a chunk of

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[28]Popper, Logic of Scientific Discovery, 72.

theory can be refuted. That any chunk larger than a single hypothesis can be isolated would seem to be consistent with his position.

Second, neither Duhem nor (as we will see) Quine denied that one can attribute the falsification of a prediction to an isolated hypothesis. They merely insisted that logic does not point the finger of blame at any one hypothesis of a failed theory. Any finger pointing in such a case depends on what Duhem called "good sense," and not on the dictates of logic alone. Popper, for his part, insisted that we need not throw up our hands in despair--we don't have to rely solely on logic because we can make our own accusations. Here, though, Duhem's views are consistent with his own. We need only to add that it is a matter of conjecture (in Popper's sense) that a given nontest statement of theory is responsible for the refutation of a test statement.

Popper's sanguine attitude about our ability to find the guilty nontest statement of a theoretical system needs to be tempered with some reflections about reasonable doubt. An obvious way to respond to the falsification of a test statement is to replace the nontest statement that you conjecture to be responsible for it and then try, with the aid of the replacement, to derive other (hopefully, only true) test statements. It is, however, quite possible to

convict an innocent hypothesis without realizing it because deducing true conclusions instead of false ones is no guarantee that our premises are all true.

It is possible that after deducing a false test statement from two nontest statements--one of them true and one of them false--we will mistakenly replace the true nontest statement instead of the false one. We may, then, end up with two false premises (nontest statements) that will enable us to deduce a true conclusion (test statement). It needs to be emphasized that Popper's conjecture that we are reasonably successful in this sort of guesswork is, itself, a piece of guesswork at the metalevel. There is no guarantee that we have guessed right simply because we can now deduce a true test statement, but not the old false one, from the statements we are conjecturing to be true. (And, of course, we have not <u>proved</u> that a test statement is true merely because we have not yet falsified it.)

We should also remember that, according to Popper, the growth of scientific knowledge is best promoted not by cautiously modifying theories in a piecemeal way to avoid falsification but by making bold conjectures and submitting them to severe tests. It is ironic that Popper argued so forcefully for the possibility of refuting, on occasion, either an isolated hypothesis or a small portion of theory.

His polemics would be more understandable if, like Quine, he believed in a maxim of minimum mutilation. On the contrary, however, Popper believed nothing of the kind. The guardedness of his holism contrasts here with the boldness of his falsificationism.

Of course, it does seem reasonable to guess that it is the new untested hypothesis added to a set of older corroborated ones that is responsible for the new refutation. Nor is this inconsistent with Popper's method of conjectures and refutations. Maybe this new hypothesis can be replaced with an even newer and bolder one that the scientist has up his or her sleeve.

Sometimes, however, the situation is quite different. Faced with the challenge of coming up with something better, the good Popperian will not always narrow the focus to the smallest portion of theory implicated in a refutation but, at least on occasion, will instead broaden the scope to include a much larger chunk so that the modifications that are made result in a bolder, more falsifiable theory.

I have said that Popper and his critics have sometimes misunderstood one another. One source of misunderstanding is that Popper discussed falsification and falsifiability in two different contexts. When, as above, he was describing the deductive testing of theories, mindful of the cases in

which those theories failed their tests, he sounded like a holist in the manner of Duhem. When a test statement is shown to be false, we are not shown which of the nontest statements of scientific theory (from which that test statement was deduced) is at fault.

When, however, he was discussing the asymmetry between verification and falsification--an important topic to him since he was eager to distinguish his views from those of the members of the Vienna Circle--he concentrated on the falsification (vs. the verification) of individual statements. It is not hard to see why he did this. The asymmetry between verification and falsification is so easily seen when you are looking at universal statements taken one at a time. This is how Popper summarized his own proposal to take falsifiability as the criterion of demarcation between science and nonscience.

> My proposal is based upon an asymmetry between verifiability and falsifiability; an asymmetry which results from the logical form of universal statements. For these are never derivable from singular statements, but can be contradicted by singular statements. Consequently it is possible...to argue from the truth of singular statements to the falsity of universal statements.[29] (italics in the original)

A strictly universal statement, whether affirmative or

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[29]Ibid., 41.

negative, cannot be conclusively verified by any number of positive instances. No finite number of white swans that we have observed is sufficient to prove that all swans are white. Only one nonwhite swan that we observe, though, is sufficient to disprove that all swans are white.

In light of all this, Quine's critical response to Popper's asymmetry thesis, stimulated by Vuillemin's comments quoted above seems to me a bit odd.

> But I am puzzled that Popper should find here, as Vuillemin puts it, "the real ground for the asymmetry between confirmation and falsification." To falsify a single hypothesis, after all, is to confirm its negation; symmetry obtains.[30]

This passage is doubly puzzling. Surely to falsify a given hypothesis is not merely to confirm, but actually to verify its negation. Whatever statements logically imply that a given statement is false also logically imply that the negation of that statement is true--we are still within the parameters of bivalent logic here--and logical implication is something stronger than mere confirmation.

And just as surely, the negation of a universal statement is not itself a universal statement and does not figure in the deduction of predictions in the way that a

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[30]Quine, "Reply to Jules Vuillemin," in The Philosophy of W.V. Quine, 621.

universal statement does. From the statement "All swans are white" we can, with the aid of the statement that a given animal is a swan, deduce that it is white. From the statement "Not all swans are white," together with the statement that a given animal is a swan, we can deduce neither that it is white nor that it isn't. Pace Quine, asymmetry obtains.

Quine and Vuillemin were both right, however, to stress the asymmetry between verification and falsification when considering not universal statements taken one at a time but theories as conjunctions of statements. If you falsify one conjunct then you falsify the whole conjunction but if you verify one conjunct you do not thereby verify the whole conjunction.

> But a scientific theory consists of laws in conjunction, not alternation; and its evidence lies in the singular consequences. Failure of such a consequence refutes the theory, while verification of such a consequence is as may be.[31]

Although he did not make any rigid distinctions among the empirical sciences, Popper did make one between what he called the "demonstrative sciences," logic and mathematics (and especially logic), on the one hand, and empirical

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[31]Quine, "On Popper's Negative Methodology" in The Philosophy of Karl Popper, 220.

sciences such as physics on the other. Although he did not explore the possibility of revising mathematics in response to the refutation of a theory he did examine and reject the possibility of revising logic. Popper's philosophy of science is wedded to classical logic and he was loath to divorce the two.

> If we want to use logic in a critical context, then we should use a very strong logic, the strongest logic, so to speak, which is at our disposal; for we want our criticism to be severe....Thus we should (in the empirical sciences) use the full or classical or two-valued logic.[32] (italics in the original)

Thus, his willingness to extend his holism beyond physical science to include all of empirical science stopped short of including the demonstrative science of logic. However large the chunk of theory put at risk by the refutation of a test statement derived from that theory, it is not large enough to include any logical laws or rules used in the derivation of that test statement. Popper emphatically rejected the proposal that we use, for example, a multi-valued logic, whether to solve certain problems in quantum theory or for any other reason.

Susan Haack has found Popper's case for the exclusive

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[32]Popper, "A Realist View of Logic, Physics, and History," in Objective Knowledge, 305.

use of classical logic wanting. First, in response to Popper's advocacy of using the strongest logic we can, she pointed out that the classical propositional or sentential calculus is less strong than any of several different modal propositional or sentential calculi. Yet, she argued, Popper would hardly have agreed that we should use the strongest modal calculus available when engaging in a critical discussion of theories in empirical science.[33]

This point seems to me well-taken. I believe, however, that Popper's case can be improved with a slight alteration. If we want our criticism to be severe then we should not weaken our logic. Since we began with classical logic, we should not weaken it in response to criticism of our conclusions. Although one might question our beginning with classical logic, if we grant that it is reasonable to start with the more firmly entrenched system, then Popper's logical conservatism seems more plausible.

Haack, though, deployed a second argument against Popper. She contended that his argument for classical logic is ultimately viciously circular.

> According to Popper's criterion of demarcation, a statement is scientific to the extent that it is falsifiable by basic statements....But

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[33]Susan Haack, Deviant Logic (Cambridge: Cambridge University Press, 1974) 37-38.

> this means that Popper's argument against the revisability of logic has the following form. Logic must not be revised, because to do so would be to impede the progress of science. It would impede the progress of science because it involves revising something (logic) which doesn't count as part of science, rather than something (e.g. physics) which does. But logic is excluded from science precisely on the grounds that it is not falsifiable (i.e. revisable) in the light of experience. The argument has come full circle. So I reject it.[34]

I believe that Haack misconstrued her opponent's argument. Popper's advocacy of the use of classical logic extended beyond empirical science to any critical context. Even metaphysicians criticizing their opponents' conclusions, or defending their own conclusions against criticism, are expected to adhere to classical two-valued logic. It doesn't matter that a given metaphysician's statements are not falsifiable by basic statements (and thus are not scientific according to Popper's criterion). What matters is that one can present arguments against those statements, arguments whose conclusions are incompatible with some of them. Popper's demarcation of the empirical from the nonempirical is irrelevant in this context.

Furthermore, Popper was not opposed to every use of a nonclassical logic. As he saw it, logic has two important uses, demonstration and refutation.

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[34]Ibid., 38.

> I look upon logic as the theory of deduction or of derivability, or whatever one chooses to call it. Derivability or deduction involves, essentially, the transmission of truth and the retransmission of falsity: in a valid inference truth is transmitted from the premisses to the conclusion. This can be used especially in so-called 'proofs'. But falsity is also retransmitted from the conclusion to (at least) one of the premisses, and this is used in disproofs or refutations and especially in critical discussions.[35] (italics in the original)

Sometimes using a logic that is weaker than standard logic is, according to Popper, acceptable and even valuable. These are times, though, that involve the transmission of truth from premises to conclusion rather than the retransmission of falsity from conclusion to premise(s).

> So if one can prove mathematical theorems with methods weaker than the full battery of classical logic, then this is extremely interesting from a mathematical point of view. Thus in proof theory we are interested in weakening if possible our classical logic, and we can, for example, introduce intuitionist logic or some other weaker logic....[36]

It is true, however, as Haack contends, that Popper was opposed to using a weaker logic in the empirical (as opposed to the formal) sciences. This, however, has to do not with the demarcation between science and nonscience but with the

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[35]Popper, "A Realist View," 304.

[36]Ibid., 306.

difference between proof and disproof. Empirical science is one of (but not the only one of) the fields in which critical discussion was of paramount importance for Popper. And critical discussion, he believed, has to do with the retransmission of falsity rather than simply the transmission of truth. Thus, it was important to him that the logic used in critical discussion not be too weak.

Popper insisted that his view of logic is a realistic one and that it fits well with his realistic view of empirical science. Central to this realistic view is his contention that Alfred Tarski rehabilitated the correspondence theory of truth. No less central is his contention that Tarski showed that logical consequence is truth transmission (and we might add, falsity retransmission).

> I would assert that not the least important of the achievements of Alfred Tarski is that by introducing two ideas into logic, he has actually made logic very much a realistic affair. The first is Tarski's idea (partly anticipated by Bolzano) that logical consequence is truth transmission. The second, I would say, is the rehabilitation of the correspondence theory of truth...[37]

Although I believe that she has misconstrued Popper's argument, however, I would not summarily dismiss Haack's

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[37]Ibid., 308.

objections to Popper's devotion to classical logic. Popper's opposition to revising logic in the face of criticism of one's theory seems, at first glance, to be based on the effect such revision may have on critical discussion (and Popper equated critical discussion, in empirical science and elsewhere, with rational discussion).

> What I should wish to assert is (1) that criticism is a most important methodological device; and (2) that if you answer criticism by saying, 'I do not like your logic: your logic may be all right for you, but I prefer a different logic, and according to my logic this criticism is not valid', then you may undermine the method of critical discussion.[38]

Haack could respond to Popper by saying that those who regard logic as revisable are not undermining the method of critical discussion but expanding it to include the organon of criticism itself. What plausible objection could there be to such an expansion? And Haack might respond further that although abandoning classical logic whenever one's pet hypotheses get into trouble may be objectionable, abandoning well corroborated empirical theories whenever one's pet hypotheses get into trouble is no less objectionable. That doesn't grant any special status to logic.

It is not necessary to be capricious about revising

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[38]Ibid., 305.

classical logic. One can go about it deliberately and systematically, replacing classical two-valued by a carefully constructed three-valued system, for example. Revising logic need not be as impetuous as Popper seemed to think it is.

Haack originally divided philosophers of logic into two camps.

> Some of those who propose systems which they take to be rivals to classical logic think that logic may be in some absolute sense verified or falsified; I shall call these realists. Others think that the choice of logic is to be made on grounds of convenience, simplicity, economy; I shall call these pragmatists.[39] (italics in the original)

And sometimes, she used the term "absolutist" in a way that suggests that it is coextensive with "realist" in this context.[40] In a later book, in a separate classification, she divided logicians into instrumentalists, on the one hand, and monists and pluralists, on the other.[41] It is not obvious that "pragmatist" and "instrumentalist" are coextensive. It is also not obvious how to classify Popper in Haack's terms, despite his claim to be a realist.

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[39]Haack, Deviant Logic, 2-3.

[40]Ibid., 26-30.

[41]Susan Haack, Philosophy of Logics (Cambridge: Cambridge University Press, 1978), 221-232.

One might argue, for example, that Popper was actually a kind of closet pragmatist who saw the convenience, simplicity and economy of classical logic as important for the method of critical discussion. I disagree, however, that he should be understood this way and I doubt that this line of argument is even open to him. For Popper understood the simplicity of a theory in terms of its falsifiability. Granted his overall outlook it is not surprising that he considered the criteria for the simplicity of a theory adequately clear and that he found simplicity in a theory very desirable.

> The epistemological questions which arise in connection with the concept of simplicity can all be answered if we equate this concept with degree of falsifiability.[42] (italics in the original)

This, by itself, however, does not help to explain or defend Popper's continued devotion to classical logic. After all, one could simply point out that Popper's position is that classical logic is not falsifiable. He could not, then, consistently maintain that it is simpler than any nonclassical system if this simplicity is degree of falsifiability.

Popper's position appears somewhat difficult to

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[42]Popper, Logic of Scientific Discovery, 140.

classify in Haack's terms. Although he labeled himself a "realist," it may seem that his argument against revising classical logic, or adopting a rival nonclassical system, would better be called "consequentialist." If the danger of undermining critical discussion is the central issue then Popper's adherence to classical logic, like some deviant logicians' adherence to a nonclassical system, was apparently not defended on the grounds that the favored system is, in some absolute sense, verified. But neither was it defended on the grounds of convenience, simplicity, economy. Rather it was defended with the claim that the consequences of abandoning it would be (or might be) harmful.

One might, perhaps, defend Popper's allegiance to classical logic in a way that is consistent with his position and that would count as realistic in Haack's sense of the term. Thus, one might contend that the very idea of revising logic in response to the failure of a prediction, or the falsification of a test statement is misguided. If logic is the theory of valid inference then perhaps, in some sense, a logical system <u>can</u> be absolutely falsified. We test a logical system by seeing whether informal arguments that we judge to be valid, when translated into its formalism, are formally valid and informal arguments that we

judge to be invalid, when translated into its formalism, are invalid.[43]

Of course, there is some give-and-take involved here just as there is some give-and-take in the testing of hypotheses in empirical science. We may, on reflection, revise our judgment that a given informal argument is valid (or invalid). The point, however, is that the method of testing a system of logic is different from the method of testing a system of empirical statements. We may regard a formal system of logic as refuted because there are invalid informal arguments that are formally valid when translated into its terms. (We may also regard a formal system as inadequate because there are valid informal arguments that are formally invalid when so translated.[44] It is inappropriate, however, to revise our logic--to adopt a revised logical system, that is--in response to the failure of a prediction or the falsification of a test statement.

This is not exactly a novel approach to formal logic. One of the reasons why classical quantificational logic is considered superior to traditional syllogistic logic is that there are valid informal arguments, for example those that

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[43]Cf. Haack, Philosophy of Logics, 14-16.

[44]On the difference between inadequacy and error in this context, see Haack, Deviant Logic, 2.

hinge on relational terms, that cannot be translated into valid categorical syllogisms. And one of the reasons why some nonclassical logics are touted as superior to classical logic is that they allegedly give verdicts on the validity of arguments that accord better with judgments of validity that are accepted upon reflection.

If this approach to formal logic is correct, then there is no place for revising logic in the face of recalcitrant experience. If it is impossible for the premises of an argument to be true and its conclusion false then the argument is valid. If that conclusion is a test statement, and the premises are nontest statements of scientific theory, then there is no reason to revise classical logic, or adopt a nonclassical logical system, just because the test statement turns out false. It is somewhat surprising that Popper did not deploy an argument of this sort. Nevertheless, such an argument is, I believe, open to him. (In Chapter Five, I will consider further Popper's opposition to revising classical logic.)

Crucial Experiments

Recall that in elaborating on the empirical testing of theories, Popper discussed the derivation of testable predictions from a theory. From among these predictions

"those are selected which are not derivable from the current theory, and more especially those which the current theory contradicts."[45] It seems obvious that Popper was thinking of crucial experiments, at least in some sense of the term.

There is, however, an important difference between what Duhem meant by "'crucial experiment'" and what Popper meant by "crucial experiment." Popper took his cue from Francis Bacon, but departed from Bacon's script. He argued that we can test a theory by focusing on those cases where it leads us to expect results different from those we would otherwise have expected, as a result either of the past acceptance of an older theory or of the more recent acceptance of a current competing theory.

> Such cases are 'crucial' in Bacon's sense; they indicate the cross-roads between <u>two</u> (or more) theories. For to say that without the theory in question we should have expected a different result implies that our expectation was the result of some other (perhaps an older) theory, however dimly we may have been aware of this fact. But while Bacon believed that a crucial experiment may establish or verify a theory we shall have to say that it can at most refute or falsify a theory.[46] (italics in the original)

Popper addressed himself directly to Duhem's second

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[45]Popper, <u>Logic of Scientific Discovery</u>, 33.

[46]Popper, "Three Views," 112.

thesis in a footnote to the above passage.

> Duhem, in his famous criticism of crucial experiments (in his Aim and Structure of Physical Theory), succeeds in showing that crucial experiments can never establish a theory. He fails to show that they cannot refute it.[47] (italics in the original)

Popper, though, did not mean by "crucial experiment" an experiment that refuted one or more theories and thereby demonstrated the one remaining unrefuted theory in a domain. He never supposed that scientists (in physics or elsewhere) can falsify all of the possible theories in a domain but one, leaving that one verified. Instead he believed that they can refute some, and perhaps all but one, of the actual theories in a domain, thereby narrowing the field, at least temporarily. But he insisted that there is always the possibility that some newcomer will arise to challenge the previously unfalsified theory or theories and that this challenger will suggest some new crucial experiment that results in the overthrow of the previously unrefuted contender or contenders.

Plainly this position is consistent with Duhem's second thesis, even if Popper didn't realize that it is. As I commented in Chapter Two, Duhem "failed to show" only what he never set out to show in the first place. Whatever the

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[47] Ibid.

(other than verbal) disputes between Popper and Duhem, a disagreement about "crucial experiments" (in Popper's sense of the term) is not one of them.

Logical Aspects of Falsificationism

Falsification looms large in Popper's theory of scientific method. It is, in fact, what he held to be the mark that distinguishes (empirically) scientific statements from nonscientific ones. Whatever supposedly empirical statements cannot be falsified are, according to him, nonscientific.

Having claimed that inductive reasoning has no role in science, Popper gave center stage to the kind of deductive reasoning that proceeds, as he put it, "in the inductive direction," that is, from singular statements to universal ones.[48] He held that the advancing of hypotheses in science is not to be controlled inductively by requiring that they have positive evidence beforehand. Instead the controls on scientific hypotheses are deductive, and are to come after they are advanced, by requiring that they face possible negative evidence.

Popper devoted a good deal of time to discussing the

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[48]Popper, Logic of Scientific Discovery, 41.

relationship between what I have called the "test statements of scientific theory" and the "evidence statements" that they confront. He discussed this confrontation along logical lines as the possible clash between universal statements and certain singular statements, or conjunctions of singular statements,--Popper's term is "basic statements"--that may be inconsistent with them.

> ...I propose the following definition. A theory is to be called 'empirical' or 'falsifiable' if it divides the class of all possible basic statements unambiguously into the following two nonempty subclasses. First, the class of all those basic statements with which it is inconsistent (or which it rules out or prohibits): we call this the class of the potential falsifiers of the theory; and secondly, the class of those basic statements which it does not contradict (or which it 'permits').[49] (italics in the original)

Since this is a definition it seems that Popper, although he wrote "if" and not "if and only if," was giving both necessary and sufficient conditions for calling a theory "empirical" or "falsifiable." It also seems that the word "theory" here has to be restricted in its application either to a set of several nontest statements of scientific theory, considered jointly, or to those universal statements that can be derived from such a set and that function as test statements, considered individually. Nontest statements

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[49]Ibid., 86.

that are part of an empirical theory do not severally divide the class of basic statements unambiguously into those each prohibits and those each permits. It is precisely because they do not that they are classified as nontest statements in the first place.

In Chapter Two I used the notation of the truth-functional calculus to elucidate several points concerning Duhem's views about falsification, isolated hypotheses and crucial experiments. When dealing with Popper's logical analysis of explanation, prediction and hypothesis testing, it is more helpful to use the predicate calculus of first order. Provided that we exercise some caution this will not lead us astray.

The universal statements that figure in Popper's account of the explanation and prediction of individual events, and that play the role of test statements, correspond to such general propositions or sentences of the predicate calculus as

$$(3\text{-}1) \quad (x)(Sx \rightarrow Px).$$

The initial conditions and singular predictions respectively correspond to such sentences as

$$(3\text{-}2) \quad Sa$$

and

$$(3\text{-}3) \quad Pa.$$

(Here 'a' is an individual constant, and we take for granted that every individual constant denotes some individual or other.) We can deduce (3-3) from (3-1) and (3-2) and this corresponds to (a very simple case of) the deduction of a prediction from a universal statement and a set of initial conditions.

We can also transform (3-1) in a familiar way into

(3-1*) $-(\exists x)(Sx \ \& \ -Px)$

and this corresponds to the transformation of a universal statement like "All swans are white" into a nonexistence statement like "There are no nonwhite swans." Furthermore, We can conjoin (3-2) with the negation of (3-3) to form

(3-4) $Sa \ \& \ -Pa$.

Here we have what corresponds to the confrontation between a test statement and an evidence statement. The universal statement is transformed into a nonexistence statement that is incompatible with the conjunction of two singular statements. At this point, though, we should exercise a bit of caution.

The singular statements that figure in Popper's account of hypothesis testing are what he calls "singular existence statements."[50] If we represent these statements with first

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[50]Ibid., 100-102.

order sentences that join individual constants to predicates then we have to understand that the "individuals" for which these constants stand are individual regions of space and time. A sentence like

(3-4) Sa & -Pa

corresponds, in Popper's analysis, to a statement that at a given time and place there is a nonwhite swan or a nonblack raven.

Although a test statement can conflict with a basic statement, we need not accept a particular basic statement as true. There is, first of all, a simple truism about logic to recall: if two statements are inconsistent it follows only that they cannot both be true. It doesn't follow that one of them in particular is false. Second, there are cases in which scientists may regard the evidence as tainted, rather than the test statement that was to be confronted with that evidence. Popper proposed a way to deal with this issue, although, as we shall see, he was not so clear about this issue as he could have been.

> We say that a theory is falsified only if we have accepted basic statements which contradict it....This condition is necessary but not sufficient; for we have seen that non-reproducible single occurrences are of no significance to science. Thus a few stray basic statements contradicting a theory will hardly induce us to reject it as falsified. We shall take it as falsi-

> fied only if we discover a reproducible effect which refutes the theory.[51] (italics in the original)

A reproducible effect, however, is something we would normally describe with a universal rather than a singular statement.

> In other words we only accept the falsification if a low-level empirical hypothesis which describes such an effect is proposed and corroborated. This kind of hypothesis may be called a falsifying hypothesis.[52] (italics in the original)

Popper's account of falsification here is a bit confusing on more than one point. First, he has entangled a logical issue and a methodological one. Accepting even one basic statement that contradicts a theory requires that we say that theory is falsified. The condition that he called "necessary but not sufficient" is actually both necessary and sufficient. A separate question, though, is when to accept a basic statement. Popper's point might have been a bit clearer if he had put it this way:

> We say that a theory is falsified if and only if we have accepted at least one basic statement that contradicts it. Nevertheless, since non-reproducible single occurrences are of no significance to science, we will not accept stray basic statements. We will accept basic statements only if we discover

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[51] Ibid., 86.

[52] Ibid., 86-87.

> a reproducible effect that refutes the theory. In other words we will accept the falsification only if a low-level empirical hypothesis (a "falsifying hypothesis") which describes such an effect is tested and corroborated.

As suggested above, a bit of caution is in order. I have already said that corresponding to

$$(3\text{-}1) \quad (x)(Sx \rightarrow Px)$$

above are such universal statements as "All swans are white" and "All ravens are black," which are unrestricted as to time and place. Corresponding to

$$(3\text{-}1^*) \quad -(\exists x)(Sx \ \& \ -Px)$$

are such nonexistence statements as "There are no nonwhite swans" and "There are no nonblack ravens," also understood as covering all of space and time. Corresponding to

$$(3\text{-}4) \quad Sa \ \& \ -Pa$$

are such singular statements as that at such and such a time and place there is a nonwhite swan or that at such and such a time and place there is a nonblack raven. (To make the exposition a bit easier I will hereafter refer to either the sentences in English or to their first order counterparts as "statements.")

Unfortunately, it is not entirely clear from Popper's account exactly what would, and what would not, count as a falsifying hypothesis. Popper elaborated in a footnote to

the passage quoted above.

> The falsifying hypothesis can be of a very low level of universality (obtained, as it were by generalising the individual co-ordinates of a result of observation....) Even though it is to be inter-subjectively testable, it need not in fact be a strictly universal statement. Thus to falsify the statement 'All ravens are black' the inter-subjectively testable statement that there is a family of white ravens in the zoo at New York would suffice.[53]

It would seem, in fact, that one need only generalize with respect to time in some cases to get what Popper calls "a reproducible effect." From the statement that there is a nonblack raven at a given time and place one might generalize by loosening the restriction with respect to time and state that there is a nonblack raven at a given place. If, for example, the nonblack raven is in a cage at the Bronx Zoo then anybody who goes to the right place during the raven's lifetime in captivity will, it seems reasonable to suppose, observe it.

Furthermore, it appears that what are sometimes thought of as merely singular statements attributing, say, blackness and ravenhood to an individual being are thought of by Popper as implying universal statements that may be somewhat restricted as to time and place. (They would sometimes be,

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[53]Ibid., 87.

in his terminology, "numerically universal" and not "strictly universal" statements.)

> Universal laws transcend experience, if only because they are universal and thus transcend any finite number of their observable instances; and singular statements transcend experience because the universal terms which normally occur in them entail dispositions to behave in a law-like manner, so that they entail universal laws (of some lower order of universality, as a rule).[54]

Thus, to say that something is a raven is to say that it will always (during its lifetime, that is) exhibit certain corvine behavior. We can observe this supposed raven repeatedly, if we choose, to determine whether it is, indeed, what we thought it was. We can, that is, regard the predicate "is a raven" as dispositional. To say that something is a raven and is black is to say not only that it has a certain shape and color but also that, among other things, it will present itself in certain specifiable ways. It will, for example, always appear to be black when seen in natural sunlight; it will grow no larger than a certain size, and so on.

Numerically universal statements, then, give us one type of falsifying hypothesis.

It seems, though, that a strictly universal statement

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[54]Ibid., 425.

would also count as a falsifying hypothesis if it is inconsistent with the original hypothesis and the set of all things of which the subject term is true is a proper subset of the set of all things of which the subject term of that original hypothesis is true. Perhaps the following example will help illustrate the point. (It is not one of Popper's examples.) Suppose that our original strictly universal statement is "All mammals are viviparous." If we find a single mammal that lays eggs, then we have the raw material we need to manufacture a falsifying hypothesis. At a certain cage in the Bronx Zoo, for example, there is a mammal that lays eggs. (Perhaps we should specify that it lays eggs at predictable intervals so that there is not just one time at which its egg-laying can be observed.)

We would also have a falsifying hypothesis, albeit one of a somewhat higher level of universality, if we come up with the statement "All platypuses are oviparous mammals." Here we have not simply generalized a singular existence statement by loosening up our time constraints but have come up with a strictly universal statement that, if corroborated, will also do the trick. If it is true then our original hypothesis is false.

The Duhem-Popper-Quine thesis comes into view when we

consider the role of the universal statement

(3-1) $(x)(Sx \rightarrow Px)$

above as a conclusion, a test statement, deduced from two or more premises, or nontest statements of scientific theory.

The incompatibility of the basic statement

(3-4) $Sa \ \& \ -Pa$

with the test statement (3-1), and our acceptance of (3-4) requires us to reject (3-1) as false. The falsification of the test statement brings us one step farther since it is derived from nontest statements of theory. We can validly conclude from its falsehood that at least one of those nontest statements is false. Perhaps the test statement (3-1) was deduced from

(3-5) $(x)(Mx \rightarrow Px)$

and

(3-6) $(x)(Sx \rightarrow Mx)$.

In such a case, granting the falsehood of (3-1) we cannot logically grant the truth of both (3-5) and (3-6). Logic, however, leaves the other three options open: the first is true and the second, false; the first is false and the second, true; both are false.

Many examples of deductive explanations and derivations of predictions (including Popper's own example, given above, concerning the piece of thread) are, however, more

complicated than this simple illustration allows and some of those complications present a problem for Popper's analysis.

One sort of complication arises when we consider the possibility of using more than one universal statement or more than one statement of initial conditions (or more than one of each) to derive a prediction. Another sort arises when we consider the possibility of deriving a quantitative prediction from lawlike statements and statements of initial conditions, with the aid of mathematics. (This is the sort of case that Duhem usually had in mind.)

A third, and sometimes very troublesome, sort of complication arises if we consider certain statements governed by multiple quantifiers.

There are some multiply quantified statements that present no special difficulties for Popper. Consider such statements as "All metals expand whenever heated." This might be symbolized as

$$(3\text{-}7) \quad (x)(t)[(Mx \ \& \ Hxt) \rightarrow Ext]$$

and read as "For everything x and for every time t, if x is a metal and x is heated at time t then x expands at time t." Such multiply quantified statements are not very far removed from the ordinary universal statements we considered above. They can be contradicted by singular statements, or conjunctions of singular statements, and we can, then, classify

them as test statements. Any statement reporting that a given sample is a metal and was heated at a certain time and place and did not expand at that time and place would serve as a potential falsifier.

Also, the statement that a particular sample of a metal, say the penny in my pocket right now, never expands when heated would seem to serve as a falsifying hypothesis. It seems, in fact, that even the statement that all samples of a particular metal, say, copper, never expand when heated would serve as a falsifying hypothesis.

Some statements governed by mixed multiple quantifiers, however, do present a special problem for Popper's logical analysis of falsification.

> The fact of the matter is that a law with the complexity of 'All men are mortal' admits of no direct evidence for or against: no direct refutation and no direct demonstration. When we reach even this moderate level of complexity, any implication of verifiable or falsifiable consequences must depend upon collaboration. Sentences governed by multiple mixed quantifiers may, when taken in conjunction imply some singular sentences, even though they imply no such sentences when taken separately.[55]

To understand Quine's point it may be necessary to consider his example more closely. To say of a particular

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[55]Quine, "On Popper's Negative Methodology," 219-220.

human being that he or she is mortal is to say that there exists a time at which this person will die. To say of every person that he or she is mortal is to say that for each one of us there is a time at which we will die. This can be symbolized as

$$(3\text{-}8) \quad (x)(\exists t)(Px \rightarrow Dxt)$$

and rendered in somewhat inelegant English as "For everything x there is a time t such that, if x is a person then x dies at time t." We considered this example in Chapter Two because "All men are mortal" was Duhem's classic example of a law of common sense. Yet there is no way to confront this law directly with the evidence of observations or experiments. (The contrast that Duhem thought he saw between such laws and the laws of physics seemed to disappear upon closer examination.)

This sort of statement presents a pressing problem for Popper because he gave a definition of falsifiability in terms of the logical relationship between a "theory" -- remember his broad use of the term -- and the set of basic statements. Quine's well-taken point is that this relationship doesn't hold for even such a moderately complex statement as "All men are mortal." Popper insisted that his criterion of demarcation rested on an asymmetry between

falsifiability and verifiability but this asymmetry, as Quine correctly noted, disappears in the case of statements like (3-8). No singular statement or conjunction of singular statements like (3-4) is ruled out by (3-8). No singular statement like (3-2) can be combined with it to deduce another singular statement like (3-3).

It is true, of course, that one can classify such hypotheses as nontest statements rather than test statements. That would make them more like (3-5) and (3-6) above then like (3-1). In such a case, though, the logic of statements like (3-8) creates some odd results in light of Popper's falsificationism. For example, from the statement "For every person there is a time at which that person dies" and the statement "Tom is a person" one can deduce the conclusion "There is a time at which Tom dies." This last statement, however, is itself verifiable but not falsifiable and because of its form qualifies, in Popper's terms, as metaphysical rather than scientific.

It turns out that not all statements governed by mixed multiple quantifiers are problematic for Popper. In his response to Quine, Popper gave some examples of what he called "all-some" statements that <u>are</u> falsifiable. "All

brothers have sisters" is one such.[56] This, though, is a special example that turns on the use of such relational terms as "brother" and "sister" and on the spatial and temporal restrictions that they introduce. We needn't search through all of space and time to find out whether, for a given set of brothers, there exists someone who is their sister.

Quine's example is far from unique. "For every substance there is a solvent" is a fairly common example that fits his analysis. We could symbolize it as

$$(3\text{-}9) \qquad (x)(\exists y)(Sx \rightarrow Dyx)$$

and render it as "For every x, if x is a substance then there is a y such that y dissolves x." To say that there is no solvent for a given substance is to make a strictly universal rather than a singular statement or conjunction of singular statements. Such a statement is itself a test statement and can be directly confronted with the evidence. Dissolve the substance in some solvent at a given time and place. A singular statement reporting this dissolution would indeed be a basic statement but the original multiply quantified statement (3-9) could not be directly verified or falsified by it.

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[56]Popper, "Replies to My Critics," in The Philosophy of Karl Popper, 993.

Here, then, we have another case of a statement that cannot be directly confronted with the evidence of observations or experiments. We can simply add the claim that such and such a thing is a substance and deduce that for this thing there is a solvent but the statement that there is a solvent for a given substance is not a test statement: it is an unrestricted existential statement and, as such, is not falsifiable. Hence, it is, according to Popper, metaphysical and not scientific, just as is "There is a time at which Tom dies."

This problem, I believe, is soluble. It stems, in my view, from Popper's insistence on characterizing falsifiability in logical terms, especially since many even moderately complex cases do not fit his paradigm. The problem is exacerbated by his careless use of the word "theory" to denote everything from a universal statement like "All mammals are viviparous" to a whole system of statements like an axiomatization of Euclid's plane geometry, together with correspondence rules.

The solution, I believe, begins with a distinction between falsification and falsifiability. Falsification is a logical matter having to do with the relationship between test statements and the evidence statements that can conflict with them. If an evidence statement is true then

the test statement that conflicts with it is false.

Falsifiability is partly logical and partly methodological. The logical part has to do with the derivability of the test statement from nontest statements, making possible the reverberation of its falsification back through the system. The methodological part has to do with an agreement or resolution not to protect some part of the system from that reverberation.

If we accept this then, I think, a satisfactory response to Quine's objection is open to Popper. The first part of that response is that such statements as "There is no solvent for this substance," itself a falsifiable statement, is incompatible with "For every substance there is a solvent." This relationship is not different in any important way from the relationship between "All mammals are viviparous" and "All platypuses are oviparous mammals." The "all-some" statement is incompatible with a low-level falsifying hypothesis that can itself be tested and may be well-corroborated.

Thus, with some modification in Popper's account of testing we can handle this little wrinkle. In the case of certain all-some statements, which turn out to be nontest statements, we do not deduce a test statement from them directly. Instead we confront them with well-corroborated

statements that are incompatible with them. (These well-corroborated statements would function as evidence statements here, though they might function as test statements in other contexts.)

The second part of the response to Quine's objection is to concede that such multiply quantified statements are directly neither verifiable nor falsifiable by basic statements but to add that this is not a fatal flaw so long as we agree that they are not thereby immunized against falsification. This response will be discussed more fully in the next section.

An important point to understand about Popper's falsificationism is that it is not simply a question of either syntax or semantics. It matters not just how statements are related to one another nor how we relate such predicates as "is mortal" to flesh and blood human beings. What matters, too, is how we resolve to treat both the statements that we submit to test and the nontest statements of scientific theory from which we derive them.

Methodological Aspects of Falsificationism

In developing his (meta)theory of the deductive testing of theories Popper was concerned about drawing a line of demarcation between what was (empirically) scientific and

what was not. He did not seek to "kill metaphysics by calling it names," as he accused the members of the Vienna Circle of trying to do. He was merely trying to separate metaphysics from empirical science. He never accepted either a verifiability or a falsifiability theory of meaning. As he saw it, the logical positivists not only were presenting a flawed solution to their problem; they were misinterpreting the central problem in the first place.

> Positivists usually interpret the problem of demarcation in a naturalistic way; they interpret it as if it were a problem of natural science. Instead of taking it as their task to propose a suitable convention, they believe they have to discover a difference, existing in the nature of things, as it were, between empirical science on the one hand and metaphysics on the other.[57] (italics in the original)

Instead of approaching the problem naturalistically, by discovering some difference between empirical science and metaphysics, Popper took it as his task to propose a suitable convention.

> My criterion of demarcation will accordingly have to be regarded as a proposal for an agreement or convention. As to the suitability of any such convention opinions may differ; and a reasonable discussion of these questions is only possible between parties having some purpose in common.[58]

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[57]Popper, Logic of Scientific Discovery, 35.

[58]Ibid., 37.

(italics in the original)

Obviously, because of his anti-inductivism, Popper could not consistently maintain that empirical science differed from metaphysics in that it was based on inductive reasoning. He maintained, instead, that the statements of empirical science differ from those of metaphysics in that they are falsifiable.

In light of what he said about the logic of falsification, it seems natural to apply Popper's criterion of demarcation between what is empirically scientific and what is not to individual statements, and especially to test statements. It can also be applied to nontest statements and even to groups of statements woven together into systems. In either case, his proposal for a convention is simply that no empirically scientific "theory," in his very broad sense of the term, should be immunized against falsification. Grant that a test statement derived from two or more nontest statements has been refuted and you do not thereby grant that either one of the nontest statements has thereby been refuted. Nonetheless it is possible, as Popper often pointed out, to immunize one or more of these nontest statements against falsification simply by deciding to hold the other nontest statements responsible for the refutation (and by making whatever other adjustments this decision

necessitates). Logic alone does not lead us to the unambiguous falsification of a nontest statement simply because the falsity of the test statement is accepted. It also, however, does not, by itself, prevent us from protecting one of those nontest statements by adopting a policy of always blaming one of its cohorts for the refutation of any test statements derived with its help.[59]

It was not a contention of Popper's that the failure of a prediction can lead us to the unambiguous falsification of one of the hypotheses from which the prediction was deduced. Nor was it a contention of his that the falsehood of a universal statement can lead us to the unambiguous falsification of the statements from which it was, in turn, deduced. Rather it was his proposal that we regard it as the scientific method to treat each of these hypotheses as falsifiable <u>in principle</u>. To say of each one of the hypotheses which, in concert, logically imply a falsehood that it is in principle falsifiable is <u>not</u> to say that in any given case we can show definitively which one is falsified. It is to say that we do not (or rather, to propose that we not) treat any one of them as in principle immune to falsification.

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[59]Ibid., 40-42.

In Popper's view, then, methodological decisions are indispensable precisely because, in some situations, logic leaves us free to decide what to do. Indeed, it was methodological, rather than logical, considerations that led Popper to his criterion of demarcation in the first place. Recalling his days as a student at the University of Vienna shortly after The First World War, he noticed a contrast between the attitude of a scientist like Einstein and (what he came to regard as) pseudoscientists like many of the Marxists, Freudians and Adlerians he met.

> My main idea in 1919 was this. If somebody proposed a scientific theory he should answer, as Einstein did, the question: "Under what conditions would I admit that my theory is untenable?"....
>
> I had been shocked by the fact that the Marxists (whose central claim was that they were social scientists) and the psychoanalysts of all schools were able to interpret any conceivable event as a verification of their theories.[60]

It did not take long, however, for Popper to realize that any theory can be immunized against refutation. (He credited Hans Albert for suggesting the term "immunized" in this context.)

> It might be said that, even if the asymmetry is admitted, it is still impossible, for various reasons, that any theoretical

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[60]Popper, "Intellectual Autobiography" in The Philosophy of Karl Popper, 32.

> system should ever be conclusively falsified. For it is always possible to find some way of evading falsification, for example by introducing ad hoc an auxiliary hypothesis or by changing ad hoc a definition.[61]
> (italics in the original)

Popper's response to this objection was to concede a point of logic while insisting on a point of methodology.

> I must admit the justice of this criticism; but I need not therefore withdraw my proposal to adopt falsifiability as a criterion of demarcation. For I am going to propose...that the empirical method shall be characterized as a method that excludes precisely those ways of evading falsification which, as my imaginary critic rightly insists, are logically possible.[62]
> (italics in the original)

Popper, thus, had two different but related proposals that form the core of his falsificationism. The first is a proposal that we characterize statements as empirically scientific on the basis of their logical relationship to the set of basic statements, i.e., those singular existence statements (and conjunctions of singular existence statements) that form the "basis" of science. The second is a proposal that we characterize as empirically scientific not the statements themselves but our method of dealing with them. Once we disentangle these two separate strands of

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[61]Popper, Logic of Scientific Discovery, 41-42.

[62]Ibid., 42.

falsificationism, Quine's problem about statements with mixed multiple quantifiers is solved and Popper's falsificationism and his guarded holism can peacefully coexist.

It is only because he insisted on a strict logical characterization of empirically scientific statements that Popper had a problem with certain statements governed by multiple mixed quantifiers in the first place. Abandon the logical characterization as a necessary condition for the falsifiability of a nontest statement and the problem disappears.

It does not follow, of course, that all of Popper's problems will so easily disappear. But if we see his falsificationism as primarily a methodological falsificationism--and I think it is in the spirit of his philosophy of science that we do so--then the logic of falsification can be loosened up a bit without any important loss. We can admit complications like the one Quine considered without losing sleep because we don't have to suppose that the logic of hypothesis testing in empirical science is really as simple as Popper initially seems to have believed. Let it be as complicated as one can imagine. The simplicity is not in the logical analysis but in the methodological resolve: no statement of empirical science

shall be granted immunity in the court of experience.

Conclusion

I have argued that Popper's and Duhem's positions are somewhat closer to each other than one might at first imagine. Despite differences of perspective, and some differences in terminology, Popper held views about the falsification of isolated hypotheses, and about the limits of crucial experiments, that are similar to Duhem's views, even if Popper himself did not realize this.

Indeed, the differences between the two authors on the possibility of crucial experiments, for example, seem to be entirely verbal. On the one hand, both rejected the view that an experiment that resulted in the refutation of one theory would result in the demonstration of any one of its competitors. On the other hand, neither rejected the view that such a refuting or falsifying experiment might be said, in some sense, to confirm or corroborate one of the theories that it left unrefuted.

The two did differ, however, on the question of isolated hypotheses. I have called Duhem's position a "sweeping holism" and Popper's position a "guarded holism." Unlike Duhem, Popper did not believe that no isolated hypothesis could be falsified by the results of observation

or experiment but this guardedness seems appropriate in light of its comparative unrestrictedness. Whereas Duhem had limited his analysis to physical science, Popper extended his analysis to all of empirical science. Having ventured beyond the limits that Duhem had set for himself, the Austrian philosopher had to contend with possible counterexamples that would not have troubled the French physicist.

The guardedness of Popper's holism, however, has sometimes been overshadowed by the unguardedness of some of his remarks. His criticism of "the Duhem-Quine thesis" as going too far shows that he took Duhem (and, as we will see in the next chapter, also the later Quine) as taking positions somewhat stronger than they actually did take. Nevertheless, he made an important point that is often overlooked. Sometimes the whole theoretical group that is refuted by an observation or an experiment can be broken down into smaller parts, some of which are not refuted.

Unfortunately, though, Popper's broadly holistic remarks sometimes ignore the actual logic of testing as he himself laid it out. Indeed, what he said about taking the whole of our background knowledge into account seems flatly inconsistent with what he said about axiomatizing theories. If we can find which nontest statements of scientific theory

are necessary for the derivation of a test statement then we needn't take the rest of our knowledge into account when that test statement turns out false. Whatever statements are involved in the derivation may be tainted but in virtually no case would we be comparing two different theories seen against the background of the whole of our knowledge.

It is suprising and interesting that Popper did not, in defending the guardedness of his holism, exploit the tension between a holistic account of empirical science and a methodologically individualistic view of social science. At the end of Chapter Two, when I discussed the possibility of extending Duhem's holism beyond physical science, I considered briefly the view held by Friedrich Hayek (and others) that the structure of such a social science as economics differs radically from that of physical science. There I argued that this has a bearing on whether Duhem's thesis can be extended to economics. This did not present a special problem for Duhem since he was not committed to Hayek's view of economics and, in any case, was not committed to extending his holism to economics. A case can be made, however, that it <u>does</u> present a special opportunity for Popper, who <u>was</u> committed to Hayek's view (or to something close to it). For anyone who holds a

methodologically individualistic view of social science has a prima facie reason to reject a sweeping holism with respect to all of empirical science.

Popper did not give a detailed treatment of his own brand of methodological individualism and he may not have realized its relevance to his holism. Consider, however, his emphatic rejection of a view about social science that he characterized as "naive collectivism."

> This view must be rejected as naive. It completely overlooks the fact that these so-called social wholes are very largely postulates of popular social theories rather than empirical objects; and that while there are, admittedly, such empirical objects as the crowd of people here assembled, it is quite untrue that names like 'the middle-class' stand for any such empirical groups. What they stand for is a kind of ideal object whose existence depends upon theoretical assumptions.[63]

The opportunity that this presents for a guarded holism is not simply that it provides a counterexample to a more sweeping holism but that it provides an entire branch of empirical science with a whole host of counterexamples. This is because the structure of that branch of empirical science is such that some of its fundamental hypotheses are, unlike those of physics, test statements rather than nontest statements of scientific theory.

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[63]Popper, "Prediction and Prophecy in the Social Sciences," in Conjectures and Refutations, 341.

(Some popularized versions of such a methodologically individualistic approach to economics, for example, begin with some kind of "Crusoe economics" and attempt to reconstruct a complex economic system by starting with the behavior of a lone individual.)[64]

There is, however, a tension between Popper's guarded holism and what I would call his "minimalism." On the one hand, Popper sometimes insisted on a holistic view of the deductive testing of theories. When a test statement is refuted we cannot tell which of the nontest statements from which it was derived is responsible for this refutation. On the other hand, he sometimes insisted on an minimalistic response to the outcome of this deductive testing. When a test statement is refuted we can often place the blame on a small group of nontest statements and, sometimes, even on an individual statement.

A charitable interpretation of these apparently conflicting tendencies is that Popper was seeking a happy medium between the implausible view that we can <u>always</u> tell which isolated hypothesis is at fault when a theory runs afoul of the facts and the equally implausible view that we can <u>never</u> tell, in such a case, which hypothesis, or small

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[64]Murray N. Rothbard, <u>Man, Economy, and State</u> (Los Angeles: Nash, 1962) 37-66.

set of hypotheses is at fault. Unfortunately, except for connecting the issue with the axiomatization of a theory, Popper gave no attempt at a unified account of how and where to find this happy medium. The result is a confusing series of remarks about holism which appear to be mutually contradictory.

Incidentally, Popper's holism creates complications for his views both about the falsification and about the corroboration of the nontest statements of scientific theory.

In the case of test statements it seems fairly straightforward how falsification and corroboration work in his philosophy of science. A test statement is confronted with the evidence of observation or experiment in an attempt to refute it. A successful attempt gives us a falsified hypothesis (such as "All swans are white") and an unsuccessful attempt gives us a corroborated one (such as "All ravens are black"). Short of reassessing the evidence itself, however, classifying a test statement as falsified is irrevocable. Accept the report of a nonwhite swan and the hypothesis that there is no such animal is thereby falsified. Period. Short of reassessing the evidence, or of finding new evidence, classifying a test statement as

corroborated is also irrevocable. So long as we accept reports of only black ravens (despite our best attempts to find nonblack ones), the hypothesis that there are no nonblack ravens is thereby corroborated. Period.

In the case of nontest statements, however, it is entirely different. Whenever there are two or more nontest statements of a scientific theory that can each individually be held responsible for a refutation, the classification of one of them as falsified can be revoked simply by changing our minds and holding another one responsible for the refutation. In fact, there can be a wide variety of simultaneous classifications of the relevant nontest statements. For two nontest statements, there are three possible combinations of truth values consistent with the falsehood of the test statement deduced from them: true-false, false-true and false-false. In general, for n different nontest statements, there are $2^n - 1$ possible combinations of truth values for those statements, granted the falsehood of the test statement deduced from them.

Each different combination of truth values represents a different decision about what nontest statements to regard as falsified (and what to regard as not falsified) by the falsification of a test statement, and that creates $2^n - 1$ different avenues for further research. Popper, of course,

would have us take the boldest path, which is to propose for testing the most falsifiable new theory that we can construct from the remnants of the old one. It is not at all obvious, however, that the different ways of replacing nontest statements within the refuted theory will result in new theories that are comparable with respect to falsifiability.

Furthermore, it would seem that (at least some of the time and perhaps much of the time) attempting to narrow the focus by concentrating on the smallest chunk of theory that is to be regarded as refuted by an observation or experiment will result in the conjecturing of a new theory that is actually less falsifiable than a theory that would have resulted if a larger chunk of theory had been regarded as refuted. This is the case even when the new theory that results from the refutation is more falsifiable than the older one it replaces.

Although Popper's holism is less restricted than Duhem's, it is (as we will see) more restricted than Quine's. Popper did not stay within the boundaries of physical science but he never ventured beyond the traditional boundaries of empirical science to embrace the revisability of logic and mathematics. For him a distinction between formal sciences

like logic and mathematics (and especially logic) and empirical sciences like biology and physics is both plausible and important.

Although, in his earlier work, Popper took for granted a simple dichotomy between analytic and synthetic sentences, he later had some misgivings about this distinction.[65] The later Popper did not insist that mathematics, for example, is analytic but he did insist that, unlike classical physics, classical bivalent logic was not open to revision. He was willing to treat the latter as immune to falsification, even as he regarded it as the hallmark of the former, and indeed of all empirical science, that none of its statements has this kind of immunity. In this respect, his views differ from those of the American-born logician to whom we now turn. It would be difficult for anyone to conjure up a more thoroughgoing holist than Willard Van Orman Quine.

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[65]Popper, "The Demarcation Between Science and Metaphysics" in Conjectures and Refutations, 262.

CHAPTER FOUR:

Quine's Radical Holism

Introduction

Perhaps the most influential American-born philosopher of the twentieth century, W.V. Quine has left his mark in many different areas. Such a well-known essay as "Two Dogmas of Empiricism" and such books as Word and Object and The Roots of Reference,[1] have assured him a central place in contemporary Anglo-American philosophy.

Quine has become famous, not only for his work in logic and the philosophy of logic and language, but also for his work in the theory of knowledge. His well-known holism is not only linguistic, but also logical and epistemological in character. He was concerned with questions of the evidence for or against statements like "All swans are white" and "All ravens are black" as well as with questions of meaning and reference that arise in connection with such statements.

In this chapter I will examine Quine's holism critically, at some points comparing and contrasting his

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[1]See W.V. Quine, "Two Dogmas of Empiricism," From a Logical Point of View (Cambridge, Massachusetts: Harvard University Press, 1953); Word and Object (Cambridge, Massachusetts: M.I.T. Press, 1960); and The Roots of Reference (La Salle, Illinois: Open Court, 1974)

views with those of Duhem and Popper. My attention throughout this critical examination will be focused primarily on the logical and epistemological aspects of that holism. I will consider several objections that have been raised, or can be raised, against Quine's views, as well as his actual and possible responses to them. Although he has plausible responses to some of these objections, this is not the case for all of them, or so I will argue.

In the second of the seven sections of the chapter I will lay the groundwork for my examination of Quine's "moderate holism" (his phrase) with a brief survey and discussion of some of his earlier and some of his later remarks. In the third section I will concentrate on the logical core of Quine's moderate holism. Here is his version of the central thesis, the "Duhem-Popper-Quine thesis." Here he extended Duhem's holistic views about physics to all of science. (As we will see there are also both similarities and differences between Quine and Popper.)

In the fourth section I will turn my attention to the more broadly epistemological part of Quine's moderate holism. In particular, I will consider the extralogical principles that guide the making of adjustments in theory whenever logical considerations determine that adjustments must be made.

In the fifth and sixth sections of the chapter I will take a closer look at two different corollaries of Quine's holism: the claim that any statement can be held true come what may (with an important proviso added); and the claim that no statement is immune to revision--or, to put it another way, that no statement must be held true come what may. (In the second case, I believe, the same proviso should be added.) This is perhaps the most radical and the most controversial part of Quine's holism and I will argue that it is seriously flawed.

Finally, in the seventh section of the chapter, I will review the conclusions drawn from my examination of Quine's position and will finish laying the groundwork for the fifth and final chapter of the dissertation, in which I argue that, despite its imperfections, Popper's version of the central thesis is more defensible than either Duhem's version or Quine's version.

Holism, Extreme and Moderate

The phrase "The Duhem-Quine thesis" was coined because it was Quine who gave Duhem's holistic thesis so much prominence, even as he modified it and extended it beyond the boundaries that Duhem had set for it. In The Aim and Structure of Physical Theory, Duhem had confined his

analysis to physics, deliberately leaving it to others to extend it beyond this domain if they were so inclined. W.V. Quine is the most famous of those philosophers who were so inclined.

Our story begins with a seminal essay written more than forty years ago. In "Two Dogmas of Empiricism" Quine gave the classic early statement of his holistic views.[2] In the first four sections of "Two Dogmas" he examined the various ways that have been, or might be, used to draw a distinction between statements that are analytic and those that are synthetic. The central problem, as he saw it, is not with supposedly analytic statements, like

(4-1) No unmarried man is married

that can be classified as logical truths. It is with those supposedly analytic statements, like

(4-2) No bachelor is married

that are not themselves (but can supposedly be turned into) logical truths. The trick is to substitute for one term (in this case "bachelor") another, cognitively synonymous, term (in this case, "unmarried man"). The effect, of course, would be to turn statement (4-2) into statement (4-1).

The problem that Quine saw here is the lack of a clear

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[2]Quine, "Two Dogmas of Empiricism"

explication of the notion of cognitive synonymy in question. At the end of the fourth section of the essay, he announced his conclusion.

> But, for all its a priori reasonableness, a boundary between analytic and synthetic statements simply has not been drawn. That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith.[3]

It is in the fifth section of "Two Dogmas" that Quine's holism entered the picture. Holism came in because it seemed plausible to some to argue that the analytic-synthetic distinction can be drawn with the aid of the verification theory of meaning. This hallowed doctrine, which Quine traced back to C.S. Peirce, is simply this:

> ...that the meaning of a statement is the method of empirically confirming or infirming it. An analytic statement is that limiting case which is confirmed no matter what.[4]

From "the method of empirically confirming or infirming" individual statements, Quine led his readers to "the relation between a statement and the experiences" that confirm or infirm it. That led to Rudolf Carnap's program to reduce the statements of scientific discourse to

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[3]Quine, "Two Dogmas of Empiricism," 37.

[4]Ibid.

statements in a language of immediate experience. This program, Quine argued, fell short not just in practice, but also in principle.[5] Although this radical reductionism no longer figured in Carnap's philosophy, it still figured, according to Quine, in the views of many empiricists.

> The dogma of reductionism survives in the supposition that each statement, taken in isolation from its fellows, can admit of confirmation or infirmation at all. My countersuggestion...is that our statements about the external world face the tribunal of sense experience not individually but only as a corporate body.[6]

The footnote to this paragraph--"This doctrine was well argued by Duhem, pp. 303-328. Or see Lowinger, pp. 132-140."--gives the only reference to Duhem in the entire essay. (The footnote refers to Chapter VI of the original [i.e. French] edition of Duhem's book and to what was then the standard English-language commentary on Duhem's philosophy of science.) This was a generous acknowledgement by the American author that a Frenchman had foreshadowed his doctrine some four decades earlier, in a book that had not yet been translated into English. Nevertheless, Quine did not discuss the similarities and dissimilarities between

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[5]Ibid., 40.

[6]Ibid., 41.

Duhem's views and his own. There is no indication in this footnote, for example, that Duhem had confined his analysis to physical theory.

In the last two sections of "Two Dogmas," it seems to me, Quine took a more extreme holistic position than he was later to find warranted.

> The statement...came with Bentham to be recognized as the unit accountable to an empiricist critique. But what I am now urging is that even in taking the statement as unit we have drawn our grid too finely. The unit of empirical significance is the whole of science.[7]

This last statement smacks of hyperbole. It goes beyond any conclusion that can reasonably be attributed to Duhem. The "unit of empirical significance" for the French physicist was never even as comprehensive as the whole of physical science, much less the whole of science.

More to the point, though, it goes beyond any conclusion for which Quine seriously argued in this essay. Perhaps his intent was to exaggerate for effect, but his remark seems to me a nonsequitur. From the claim that statements do not face the tribunal of experience individually, it follows that if they <u>do</u> have their day in court, they do so only in groups--not that they do so only

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[7]Ibid., 42.

in one all-encompassing group.

A decade later, Quine responded in a brief footnote to objections to what he conceded was "an excessive holism espoused in occasional brief passages of mine" and found those objections "largely warranted."[8] He did not elaborate on this concession but did claim that some objectors had missed an important point.

> What comes of the association of sentences with sentences is a vast verbal structure which, primarily as a whole, is multifariously linked to non-verbal stimulation....In an obvious way this structure of interconnected sentences is a single connected fabric including all sciences, and indeed everything we ever say about the world; for the logical truths at least, and no doubt many more commonplace sentences too, are germane to all topics and thus provide connections.[9]

Before deciding whether Quine has retreated from an extreme early position, we should remember that it is not just "the unit of empirical significance" that is at issue here. Quine has denied, as vociferously as any other contemporary American philosopher, that science is divided into analytic and synthetic statements, as some earlier empiricists believed. In the last section of "Two Dogmas," he sketched a picture different from the earlier empiricist

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[8]Quine, Word and Object, 13.

[9]Ibid., 12-13.

view.

> The totality of our so-called knowledge...is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions re-adjustments in the interior of the field. Truth values have to be redistributed over some of our statements. Reevaluation of some statements entails reevaluation of others, because of their logical interconnections--the logical laws being in turn simply certain further statements of the system, certain further elements of the field.[10]

He drew some pertinent conclusions in the very next paragraph.

> If this view is right, it is misleading to speak of the empirical content of an individual statement--especially if it is a statement at all remote from the experiential periphery of the field. Furthermore it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements, which hold come what may. Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system.... Conversely, by the same token, no statement is immune to revision.[11]

In the fifth and sixth sections of this chapter I will examine more closely the claim that there is a difference between those statements that are immune to revision and those that are not. We should notice in passing, however,

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[10]Quine, "Two Dogmas of Empiricism," 42.

[11]Ibid., 43.

that Quine did give an example of an isolated statement close to what he called "the experiential periphery of the field" that might be infirmed independently of the whole system of science.

> For example, we can imagine recalcitrant experiences to which we would surely be inclined to accommodate our system by reevaluating just the statement that there are brick houses on Elm Street, together with related statements on the same topic.[12]

It may be worth remarking, however, that our inclination to accommodate a system of beliefs to recalcitrant experiences in one way rather than another is just that: an inclination. We still have the option of going against the grain, and Quine believes that, on occasion, we will do (and perhaps ought to do) exactly that. Even for statements that are remote from experience, however, the claim that "the unit of empirical significance is the whole of science" seems to me an exaggeration.

In a later essay, Quine appears to have retreated from the former position to a less extreme one. (He also altered his language slightly. He gave up the word "statements," in favor of the word "sentences," because of the use of "statement," by some Oxford philosophers, for the products

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[12]Ibid.

of certain speech acts that are performed by uttering declarative sentences.[13] Quine's later position seems to me more moderate, and more plausible, than his earlier one.

> When we look thus to a whole theory or system of sentences as the vehicle of empirical meaning, how inclusive should we take this system to be? Should it be the whole of science? or the whole of a science, a branch of science? This should be seen as a matter of degree, and of diminishing returns. All sciences interlock to some extent....It is an uninteresting legalism, however, to think of our scientific system of the world as involved en bloc in every prediction. More modest chunks suffice.[14]

Whether we regard this as a retreat or a clarification, however, Quine did not tell us here either how modest a chunk of theory is involved in a given prediction (or test), or how to decide this question. Furthermore, it hardly seems appropriate to call his earlier, unqualified holism an "uninteresting legalism." On one interpretation the claim is difficult to take as literally true. Is the theory of evolution by natural selection, for example, really involved en bloc (along with the rest of science) in a prediction, derived from a theory of the business cycle, about the rate

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[13]W.V. Quine, Philosophy of Logic (Cambridge, Massachusetts: Harvard University Press, 1986), 2.

[14]W.V. Quine, "Five Milestones of Empiricism," Theories and Things (Cambridge, Massachusetts: Harvard University Press, 1981), 71.

of unemployment next year? Is it just an "uninteresting legalism" to think that it is?

Even if the whole of science were a single axiomatized system would such an extreme holism follow? After all, even in an axiomatized system of statements, it is often the case that some of the theorems can be deduced within the system independently of some of the axioms. Our total web of belief, however, is not even close to being a fully axiomatized system, despite the many connections among the different branches of science. I believe that Quine's "uninteresting legalism," was an exaggeration for effect rather than a defensible position.

It seems to me that if Quine was not retreating from an extreme position then he was, initially at least, conflating two separate issues. In Theories and Things (as quoted above) he says that all sciences "interlock to some extent." In Word and Object (also as quoted above) he speaks of "a single fabric including all sciences" and adds that the interconnections among the sciences are provided by the logical truths, and perhaps by other sentences too, which are "germane to all topics."

This relation among the different branches of science is a good deal looser than the relations of logical implication or logical consistency. Surely, Quine was not

suggesting that all of science is at risk whenever a test statement turns out false in the same way that whatever nontest statements of scientific theory logically imply that test statement are at risk. Apparently he was suggesting merely that all of science is potentially at risk when it comes time to make the adjustments that are stimulated by the results of such a test. If we respond to some unexpected counterobservation by changing some part of logic or mathematics, for example, then our change will reverberate throughout the sciences. Such a response to the failure of a prediction derived from a theory of the business cycle then surely would have ramifications for evolutionary theory--because changing logic and mathematics would have ramifications for evolutionary theory.

I would argue, however, that these two issues are best kept separate. Regarding as suspect all of the statements that, together, logically imply a falsehood is not the same thing as looking suspiciously at all of the statements that we may consider changing while trying both to restore consistency to our web of belief and to keep it as simple as possible.

In the first place, one always has to reject at least one of the statements that logically implies a falsehood, on pain of inconsistency. One does not always have to reject

one of the statements that connect the tainted area of theory with other branches of science.

In the second place, there is at least an initially plausible case to be made that one never has to reject one of the logical or mathematical statements that interconnect the sciences, regardless of the falsehood of one's test statements. Thus, one might argue, distant branches of science are not involved when a test statement fails in a connected branch--even if those distant branches contain many statements that are not themselves immune to revision. The statements that are "germane to all topics" and that "provide connections" do not spread the contagion of falsehood from one branch of science to another because those germane statements are themselves immune to revision. There is no parallel to this in the case of logical implication. Any statement that joins in logically implying a false statement is at risk unless it is itself immune to revision.

I conclude, then, that we can and ought to distinguish between the direct and the indirect involvement of nontest statements of scientific theory in the failure of a test statement. Statements that join in logically implying a false test statement are directly involved in that test's result. Statements that join in logically implying any

false statement are, in fact, directly involved. Other revisable statements are, at most, only indirectly involved, especially if they are just tenuously connected to the statements that are directly involved.

Be that as it may, Quine has lately reaffirmed and clarified a more moderate brand of holism. First, he introduced a new notion, that of an observation categorical. (This is not to be confused with the notion of an observation conditional introduced in an earlier work. The new expression was introduced in his latest book.

> A generality that is compounded of observables in this way--'Whenever this, that'--is what I call an observation categorical. It is compounded of observation sentences. It is a generality to the effect that the circumstances described in the one observation sentence are invariably accompanied by those described in the other.[15]
> (italics in the original)

One of the examples he gave is

> (4-3) When[ever] a willow grows at the water's edge, it leans over the water.

Another example is

> (4-4) Whenever there is a raven, it is black

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[15]W.V. Quine, Pursuit of Truth (Cambridge, Massachusetts: Harvard University Press, 1992), 10.

or, more succinctly,

(4-4') All ravens are black.

Quine gave this notion a role to play in his latest version of holism.

> In order to deduce an observation categorical from a given hypothesis, we may have to enlist the aid of other theoretical sentences and of many common-sense platitudes that go without saying, and perhaps the aid even of arithmetic and other parts of mathematics.
>
> In that situation, the falsity of the observation categorical does not conclusively refute the hypothesis. What it refutes is the conjunction of sentences that was needed to imply the observation categorical....This is the important insight called holism.[16]
> (italics in the original)

(Here we have, by the way, a use of the word "hypothesis" that seems fairly close in spirit to Duhem's narrow use of the term.) This gives us a summary of Quine's more mature view and, moreover, a criterion by which to decide how modest a chunk of theory is directly involved in the falsehood of a test statement derived from that theory. When an observation categorical is refuted, what is refuted along with it is just the conjunction of nontest statements that was needed to imply it. In turn, the statements needed to imply those nontest statements are also, of course, directly involved.

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[16]Ibid., 13-14.

(It seems that Quine meant "needed to imply" the observation categorical here as "needed in a given derivation" of it. It is, I believe, beside the point to argue that the same observation categorical is logically implied by, or can be deduced from, two or more nonintersecting sets of premises and that, therefore, no one set of theoretical statements is literally "needed" to imply it. Although any conjunction of statements that logically implies a falsehood is tainted, Quine focused his attention here on that conjunction of allegedly true statements from which a scientist actually deduces a false observation categorical.)

The modest chunk of theory that suffices, then, is, in the first instance, the chunk of statements that together logically imply the falsehood. It may, of course, be necessary to consider larger chunks of theory when deciding what further adjustments are to be made once we have decided which of the implying nontest statements of scientific theory to discard.

Notice, by the way, that observation categoricals are not the sort of statements usually used to make predictions. Predictions are typically made with singular statements telling us, for example, the color of the next raven we will

see rather than with universal statements telling us the color of all of these birds. The logical point, however, is the same in either case. Whatever statements, taken together, logically imply a falsehood cannot all be true.

As mentioned above Quine applies the term "moderate" to the version of holism he now espouses.

> Holism in this moderate sense is an obvious but vital correction of the naive conception of scientific sentences as endowed each with its own separable empirical content.[17]

Why Quine sees his moderate holism as "obvious but vital" and necessary to correct a "naive" conception may be easier to understand when we look at the logical core of that holism. This is his version of the central thesis that I have labeled "the Duhem-Popper-Quine thesis." It is the least controversial part of Quine's package, and it is the part that comes with Duhem's and Popper's packages as well.

The Logical Aspects of Moderate Holism

The central logical issue of Quine's holism is the restoration of consistency to a system of statements after it has been upset by the introduction of some new statement. If you add a statement telling of a nonwhite swan to a system

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[17]Ibid., 16.

of statements that contains an observation categorical informing us that there are no such things then you have trouble.

Perhaps the easiest and most obvious way to maintain the consistency of such a system is just to reject the new statement. Occasionally, Quine believes, this is the road to take--but it is, for good reason, the road less often taken. Rejecting new counterobservations as a matter of course is a bad habit to develop. It makes a mockery of the whole notion of testing hypotheses, and it makes a near-impossibility out of the whole notion of progress in science.

The common scientific road is to reject one or more of the statements previously, but tentatively, accepted and this is a path no less recommended for being well-traveled. The process, according to Quine, usually begins with the testing of observation categoricals.

Observation categoricals link theory to observation because they can be logically implied by the nontest statements of scientific theory and can logically contradict evidence statements that are derived from the reports of observations or experiments. This marks for them the role of test statements as outlined in Chapter One. Thus a statement like

(4-4′) All ravens are black

can be confronted with a statement like

(4-5) Some ravens are not black

which, in turn, is implied by

(4-6) This raven is not black.

(Here, I am assuming, of course, that there is an actual raven designated by the phrase "this raven.") Although observation categoricals cannot be conclusively verified by observations, they can be falsified by them.

> The observational test of scientific hypotheses, in turn, and indeed of sentences generally, consists in testing observation categoricals that they imply. Here again, as in the case of the observation categorical itself, there is no conclusive verification, but only refutation. Refute an observation categorical, by an affirmative and a negative observation, and you have refuted whatever implied it.[18]

In the case of (4-4′), you refute the observation categorical by a conjunction of the affirmative observation (that something is a raven) and the negative observation (that it is not black). If this observation categorical is logically implied by a conjunction of theoretical statements, perhaps together with some nontheoretical ones, then this conjunction of statements is itself refuted.

Quine's analysis of the refutation of observation cate-

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[18]Ibid., 12.

goricals is reminiscent of Popper's analysis of the refutation of strictly universal statements. Despite many differences between their views, there are a few important similarities. There is, however, an interesting difference here between Popper and Quine, on the one hand, and Duhem, on the other. For Duhem paid little attention to the deduction of categorical statements from groups of hypotheses. He was more concerned with the use of mathematics to derive quantitative conclusions from quantitative premises.

It is an interesting question whether Quine supposes the observation categoricals to have existential import. Popper, for his part, does not make this supposition about the strictly universal statements that function as test statements in his account. In the end, however, it may not matter. In both cases the test statement is not a candidate for verification or demonstration but for falsification or refutation. Hence, it can be read as the equivalent non-existence statement. What have existential import are the statements that contradict them, the reports of observations or experiments that, if true, prove the test statements false.

There are both logical and extralogical issues intertwined

in the scientific response to the failure of an observation categorical. This is how Quine sees it.

> Over-logicizing, we may picture the accommodation of a failed observation categorical as follows. We have before us some set S of purported truths that was found jointly to imply the false categorical....Now some one or more of the sentences in S are going to have to be rescinded. We exempt some members of S from this threat on determining that the fateful implication still holds without their help. Any purely logical truth is thus exempted, since it adds nothing to what S would logically imply anyway; and sundry irrelevant sentences in S will be exempted as well.[19]

But not every statement, of course, is exempted.

> Of the remaining members of S, we rescind one that seems most suspect, or least crucial to our overall theory. We heed a maxim of minimum mutilation. If the remaining members of S still conspire to imply the false categorical, we try rescinding another and restoring the first....We continue thus until the implication is defused.
>
> But this is only the beginning. We must also track down sets of sentences elsewhere, in our overall theory, that imply these newly rescinded beliefs; for those must be defused too. We continue thus until consistency seems to be restored.[20]

Let me make a couple of remarks in passing. The first is that one of Quine's statements above is a bit puzzling. In light of his earlier insistence that the laws of logic are just "certain further elements of the field" and that

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[19]Ibid., 14.

[20]Ibid.

they are therefore revisable, it seems odd that he exempts them from consideration for being rescinded, at least in the first pass, on the grounds that the fateful logical implications hold without them. Doesn't this give them a privileged status that they are not supposed to have? (I will examine this issue at greater length in the sixth section of this chapter.)

The second passing remark is that it seems ironic that Quine should suggest that scientists heed a maxim of minimum mutilation after having championed such a thoroughgoing holism. After all, the minimum mutilation of theory possible is surely the excision of a single hypothesis separable from the rest of the theory. But the feasibility of such a minimum mutilation of theory is exactly what he was at great pains to deny. Perhaps Quine intended his practical maxim to qualify his holism, well understanding that the ultimate minimum was very rarely attainable. His moderate holism, in fact, might fairly be characterized as radical in theory but conservative in practice. (I will come back to this point, too, in a later section of this chapter.)

Strictly speaking, a maxim of minimum mutilation is not a logical maxim. Nor is it a hard-and-fast rule. There is usually more than one way to restore consistency to a system

of statements that has been upset by the addition of a new statement. Quine, in fact, more than once insists that scientists will do a good deal of mutilating of current theory if they can thereby see their way clear to a simple, and consistent new theory.[21]

Quine has spelled out in some detail the logical aspects of the process hinted at by Duhem and spelled out, as part of a different philosophy of science, by Popper. Duhem, though he did not present a step-by-step protocol for the accommodation of a failed prediction, and though he was not concerned with observation categoricals, might well have given a qualified approval of Quine's over-logicized account. Popper, of course, would heartily disapprove of the maxim of minimum mutilation. But for that difference, however, he too might have given Quine a qualified approval. Here is an area of partial agreement among the three authors.

Recall the argument schema given above in Chapter One:

(1-B) $(p \ \& \ q) \rightarrow r$

$-r$

$-(p \ \& \ q)$

[21]Ibid., 15.

Recall, too, that the first premise is supposed to be a logically true (or valid) conditional so that the consequent is logically implied by the antecedent. The falsehood of the consequent requires the falsehood of the conjunction of statements that logically implies it. Quine's observation categorical plays the role of consequent 'r' in this case. The statements from which it is deduced play the role of the antecedent 'p & q'. (The conjunction, of course, may be composed of more than two separable statements. Furthermore, each of the conjuncts may themselves be logically implied by still further statements.)

Refuting a conjunction, however, is not the same thing as refuting any of the conjuncts individually. If none of these conjuncts is itself an observation categorical (and, thus, none can be directly confronted with observations) we have the same situation we saw in the discussion of Duhem's and Popper's holism: the falsehood of the statement tells us that something is wrong with the theory but does not tell us exactly what that something is.

I should add that an observation categorical need not be deduced, as in traditional syllogistic logic, from other categorical statements. Statements like

(4-4') All ravens are black

can be derived from sets of statements that contain

multiply-quantified statements or those with many-place predicates, as well as from those that contain only one quantifier each and only one-place predicates.

I should also add that things get a bit more complicated when we take a step beyond Quine and consider statistical generalizations, whether, like

(4-7) Most swans are white,

they do not contain numerical percentages, or, like

(4-8) 75% of all swans are white,

they do. There are two reasons for this. The first is that the derivation of statistical generalizations often involves more than deducibility by the logic of truth functions, quantifiers, and identity (which is what Quine counts as logical implication).[22] Often, the probability calculus also figures in their derivation.

The second reason is that statistical generalizations cannot be falsified in the way that universal generalizations can be. A single nonwhite swan serves to refute the statement that all swans are white but even a whole flock of such nonwhite birds will not refute the statement that most swans are white.

(I will defer further discussion of statistical

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[22]Ibid., 14.

generalizations and other statements to the sixth section. For the rest of this section I will concentrate on observation categoricals.)

Of course, once we have decided that a given statement is to blame for the falsehood of the derived statement, we have thereby decided that any statements that logically imply it are suspect. And so on, along that particular thread of implication. This does not quite exhaust Quine's holism. Before continuing, though, I should examine a criticism that is based on a misunderstanding of the logical core of that holism.

As noted in Chapter Two, Adolf Grunbaum has criticized "the Duhem-Quine thesis" on two grounds. The latter of his two lines of argument was more direct: he presented what he purported to be a counterexample. As discussed above, what Grunbaum's counterexample has illustrated, according to Larry Laudan, is not the refutation of what counted for Duhem as an isolated hypothesis but rather the refutation of a set of several different hypotheses (or axioms) taken together.

Regardless of the status of Grunbaum's proposed counterexample, his other line of argument is more pertinent here, as a criticism aimed at Quine's views.

No general features of the logic of falsi-

> fiability can assure, for every isolated empirical hypothesis H and independently of the domain to which it pertains, that H can always be preserved as an explanans of any empirical findings O whatever by some modification of the auxiliary assumptions A in conjunction with which H functions as an explanans.[23]

It is important to notice that Grunbaum here was talking not merely of failing to isolate a hypothesis, nor of holding a hypothesis true come what may but of preserving it as an explanans of some given explanandum, which is far from the same thing. For a hypothesis to count as an explanans, or as part of an explanans of some phenomenon, it is necessary (but not sufficient) that the hypothesis be true. Preserving some hypothesis as part of an explanans requires in addition that, since we are dealing here with deductive explanation, it be at least a necessary ingredient in the deduction of the explanandum.

In responding to Grunbaum's criticism in defense of Duhem, Laudan has taken pains to distinguish a weaker and a stronger interpretation of the Duhem-Quine thesis and to absolve Duhem of all responsibility for the stronger interpretation.

> It appears, then, that there are two versions of the [Duhem-Quine] thesis: a stronger one (which Grunbaum attacks) and a weaker (which

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[23]Grunbaum, "The Duhemian Argument," 116.

> I believe is Duhem's actual position). They can be formulated as follows:
>
> Stronger D-thesis: For every hypothesis and every observation statement, there exists a set of non-trivial auxiliary assumptions A', such that H and A' entail O.
>
> Weaker D-thesis: In the absence of a proof that no appropriate hypothesis-saver exists (i.e., unless we prove that $-(\exists A')(H + A' \rightarrow -O)$), then -O is not a conclusive refutation of H, even if $H + A' \rightarrow -O$. [sic][24]
>
> (italics in the original)

(There seems to be a typographical error in stating the weaker thesis. The last clause should read "even if $H + A \rightarrow O$.")

Laudan has argued (convincingly, in my view) that Grunbaum's criticism does not apply to Duhem. I would argue that it does not apply to Quine either. Quine himself, in fact, has recently disavowed exactly the position that Grunbaum has attacked.

> It is difficult to see how anyone can question holism, in the sense now before us. Grunbaum has indeed argued against holism, but in a stronger sense than is here entertained. He construes holism as claiming that when a prediction fails, we can always save the threatened hypothesis by so revising the backlog of accepted theory that it, plus the threatened hypothesis, will imply the failure of the prediction. I am making no such presumption. Inactivating the false implication is all that is at stake. Explaining the unexpected counter-observation is quite another step of scientific progress which may

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[24]Laudan, "Grunbaum on 'The Duhemian Argument'," 159.

> or may not be made in the fullness of time.[25] (italics in the original)

It may seem that Quine was being excessively cautious in this regard. After all, one can come up with examples in which the threatened hypothesis can be preserved as an explanans, or part of an explanans, after the conclusion derived from it (with the aid of auxiliary hypotheses) has failed.

Is the velocity of light greater in air than in water? We can salvage the emission hypothesis (though not the entire system of emission) by making some adjustment in the auxiliary assumptions. Duhem briefly considered this possibility. After the passage in which he contended that it was the "system of emission" that was overthrown by Foucault's experiment he added this.

> It would be rash to believe, as Arago seems to have thought, that Foucault's experiment condemns once and for all the very hypothesis of emission.... If physicists had attached some value to this task, they would undoubtedly have succeeded in founding on this assumption a system of optics that would agree with Foucault's experiment.[26]

It may be that this sort of ploy will work in some cases in which mathematics figures in the explanans in some

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[25]Quine, Pursuit of Truth, 16.

[26]Duhem, Aim and Structure, 187.

pertinent way. If, for example, the prediction is that some observable quantity will fall within a certain range and it fails to do so, then changing some number in the explanans may get the number in the explanandum to come out right. Thus, perhaps, the emission hypothesis, can be, as Grunbaum put it, "preserved as an explanans."

It seems to me, however, that there is an important, and fairly obvious, class of cases in which this will not work at all. These are the cases in which the falsified statement is an observation categorical such as

(4-4') All ravens are black.

Consider, in fact, <u>any</u> case in which a universal affirmative generalization is deduced from two other universal affirmative generalizations.

Any observation categorical of the form

(4-9) $(x)(Sx \rightarrow Px)$

may turn out to be false but it follows only that, if it was deduced from two other categoricals, then one or the other (or both) of those categoricals is false. The contradictory of this observation categorical is not itself an observation categorical but is rather a particular negative of the form

(4-10) $(\exists x)(Sx \ \& \ -Px)$

and is not, without an existence assumption, logically implied by any observation categoricals. Furthermore, even

if we replace one or the other of the statements from which we deduced (4-9) there is no guarantee that we will be able to deduce a true observation categorical that, with an existence assumption, would logically imply (4-10).

It seems, then, that Quine was not at all too cautious in his remarks on this score, especially since he was dealing primarily with observation categoricals. These are exactly the kinds of case in which we cannot always make an easy adjustment in those premises that enabled us to deduce the original conclusion in order to enable us to deduce the contradictory of that conclusion.

The Epistemological Aspects of Moderate Holism

There are more than strictly logical issues involved in the accommodation of a failed observation categorical. Since there is more than one way to restore consistency to the system of statements that was upset by the introduction of the new statement, the question arises of how to choose among them. A convenient way of approaching these extralogical issues, I believe, is by way of an objection to Quine's holism.

Grunbaum's criticism of "the Duhem-Quine thesis" is not the only criticism that can be linked to different interpretations of the thesis and Laudan's way of

distinguishing between what he termed a "weaker" and a "stronger" thesis is not the only way to draw this distinction.

Championing a position that he called "sophisticated methodological falsificationism," Imre Lakatos accepted what he considered the "weaker interpretation" of holism but emphatically rejected what he considered the "stronger interpretation."

> Finally, I should like to discuss the "Duhem-Quine thesis" and its relation to falsificationism...
>
> This thesis has two very different interpretations. In its weak interpretation it only asserts the impossibility of a direct experimental hit on a narrowly specified theoretical target and the logical possibility of shaping science in indefinitely many ways...
>
> In its strong interpretation the Duhem-Quine thesis excludes any rational selection rule among the alternatives; this version is inconsistent with all forms of methodological falsificationism.[27]
> (italics in the original)

This way of drawing the distinction is, as mentioned, different from Laudan's way of drawing it. Lakatos' "weak interpretation" corresponds to the logical core of Quine's holism as we have examined it, and to the holism of Duhem

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[27]Imre Lakatos, Philosophical Papers, ed. by John Worrall and Gregory Currie, vol. 1, The methodology of scientific research programmes (Cambridge, Cambridge University Press, 1978), 96-97.

and Popper as well. This is, in a nutshell, the central thesis that unites their work. Lakatos accepted the thesis in this interpretation. Perhaps his stronger version of the Duhem-Quine thesis can be attributed to Duhem, although this is a matter of dispute. It is even more disputable that it can be attributed to Quine.

It is not easy to determine what Lakatos would have considered a "rational selection rule." Plainly it could not be simply a logical selection rule, in the sense of a rule of formal logic that would govern selection among the possible ways of "shaping science" in response to the falsification of a prediction or of an observation categorical. The denial that there is such a rule is exactly what Lakatos' weaker interpretation implies, and this is the version that he accepts. After all, if there is more than one way of restoring consistency to the system of statements that constitutes science, then logic alone will not tell us exactly how to shape that system.

When discussing this issue, Duhem did fall back on a somewhat vague notion of "good sense," a regulating influence that lacks the force of logic.

> These motives which do not proceed
> from logic and yet direct our choices,
> these "reasons which reason does not
> know"...constitute what is appropriately

called good sense.[28]

(The phrase in quotes is a reference to Blaise Pascal's famous dictum that "the heart has its reasons that reason does not know," which Duhem quoted approvingly in more than one context.) It surely seems that any selection rule that is sanctioned only by "reasons that reason does not know" is an unlikely candidate for the title of "rational selection rule." Quine, though, had a somewhat different view. The very last sentence of "Two Dogmas" is pertinent here.

> Each man is given a scientific heritage plus a continuing barrage of sensory stimulation; and the considerations which guide him in warping his scientific heritage to fit his continuing promptings are, where rational, pragmatic.[29]

Lakatos' "strong interpretation" of "the Duhem-Quine thesis" brings us beyond the logical core of Quine's holistic doctrine. For logic tells us only that when a statement is false, not all of the statements that logically imply it can be true. There is, however, more than one way of "inactivating the false implication" and restoring consistency to the system, as Quine puts it. There are indefinitely many ways of "shaping science" as Lakatos says. Where do we go from here? What do we (or should we) do when

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[28]Duhem, Aim and Structure, 217.

[29]Quine, "Two Dogmas of Empiricism," 46.

we discover that we have to make adjustments in the fabric of science but have more than one way of making those adjustments?

It seems to me that the best way to decide what method of responding to the refutation of a test statement is rational is to consider a goal and judge proposed methods as either conducive or not (or more or less conducive) to achieving it. Popper contended that reasonable debates about proposals for conventions (such as his proposal about demarcating science and nonscience) can only take place between or among people with some goal or purpose in common.[30]

Quine, most recently, was fairly direct about the goal that he adopts for science. He presented his maxims in combination with a discussion of this goal.

> Simplicity of the resulting theory is another guiding consideration, however, and if the scientist sees his way to a big gain in simplicity he is even prepared to rock the boat very considerably for the sake of it. But the ultimate objective is so to choose the revision as to maximize future success in prediction: future coverage of true observation categoricals. There is no recipe for this, but maximization of simplicity and minimization of mutilation are maxims by which science strives for vindication

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[30]Popper, Logic of Scientific Discovery, 37.

in future predictions.[31]

What seems to emerge here is that there is not a single selection rule, rational or otherwise, that will guide choice in the event of recalcitrant experience. There are, instead, two competing maxims -- perhaps Lakatos would have called them "selection rules" -- maximize simplicity of resulting theory and minimize the mutilation of current theory. Perhaps the ultimate objective serves as an overarching rule to which the two maxims are subsidiary. The cardinal rule, then, would apparently be this: maximize future success in prediction. Though there may be no recipe for doing this, there is at least a unifying principle.

Unfortunately, some of Quine's other comments further complicate the picture.

> Not that prediction is the main purpose of science. One major purpose is understanding. Another is control and modification of the environment. Prediction can be a purpose too, but my present point is that it is the test of a theory, whatever the purpose.[32] (italics in the original)

Quine seems to be making a distinction between the ultimate objective one has when revising a scientific theory, or system of sentences, and the main purpose one has

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[31]Quine, Pursuit of Truth, 15.

[32]Ibid., 2.

when doing scientific investigation in the first place.

This may be a plausible view if one adds that maximizing future success in prediction or maximizing future coverage of observation categoricals is the theoretical prerequisite of such practical concerns as either modifying or controlling the environment, on the one hand, or adjusting to it on the other. Predictions about rainfall in the years to come may stimulate farmers to build irrigation canals to help water their crops. Predictions about rainfall in the days to come will let the rest of us know whether to bring an umbrella to work.

It seems to me, however, that his view is somewhat problematic if we believe that the main purpose for doing science is to achieve an understanding of the phenomena of our world but that the ultimate objective of revising an upset theory is to maximize success in future predictions.

In the first place, the goal of maximizing future success in predictions is ambiguous. We may want to revise our theory so that we make fewer false predictions in the future than we have made in the past. Or again, we may want to revise our theory so that we make more true predictions in the future than we have made in the past, which is not the same thing. Perhaps, in fact, we may follow a third path: striving to improve the ratio of true predictions to

false ones, even if that means making more false predictions than we've previously made, or making fewer true ones. And, of course, we may assign some predictions more weight than others. Getting these weightier predictions right or wrong is more important than getting the lighter ones right or wrong.

In the second place, the goal of maximizing future success in predictions, in any of these senses canvassed just now, may not be the path to take if I want to understand as much as I can--especially if I am impatient. There is a method to Popper's (apparent) madness when he suggests that the whole point is to make the mistakes as quickly as possible.[33] A high number of false predictions, a low number of true predictions, and a poor ratio of true to false predictions--at least over the short term--are all quite consistent with a policy of trying to get closer to the truth. Bold theoretical conjectures may lead to false observation categoricals more often than more timid conjectures do but they may lead us more quickly to explanations of phenomena that puzzle us. And once we have those explanations, we may not trouble ourselves to derive

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[33]See, for example, the Preface to Conjectures and Refutations, together with the motto, quoting John Archibald Wheeler.

too many more predictions from them.

And, of course, maximizing future success in any of the senses touched on above is not a plausible objective if it is a purely quantitative affair. We can have as many true predictions as our hearts desire if we find enough true theoretical statements, no matter how trivial, from which to derive them. On any plausible view, some predictions are more important than others--and these are the ones it is more important to get right. This brings us back to the previous point. How can we assign a weight to any given prediction unless we have a main purpose that will tell us which truths we really need or want to know and which truths are best left to more trivial pursuits than science?

The upshot of all of this is not merely that Quine's views and Lakatos' views are incompatible with one another. It is that without a clear decision on what the aim of science is to be, together with a plausible case that some way or ways will achieve that aim better than others, Quine does not have a way of ruling out the falsificationist methodology of either Popper or Lakatos. Faced with the indefinitely many ways of shaping science, Popper, for example, could argue (and, of course, he did argue) that some ways are better than others. In fact, Popper's favored ways require forsaking the maxim of minimum mutilation. We

should, the Popperian will insist, respond to the failure of an observation categorical by so revising the statements of science as to maximize the falsifiability of resulting theory (so long as the resulting theory is not yet falsified). If there is--though there may not always be--a unique way to do this then we have a serious challenge to at least one aspect of Quine's position. True, the laws of logic do not determine which sentences to revise in the event of a failure of an observation categorical. This is Lakatos' "weak interpretation" of Duhem's and Quine's holism. The rules of methodology, however, may make that determination instead. If they do then unless we accept Quine's views about the purpose of science, we can plausibly argue that the logical core of his moderate holism needs to be supplemented by something other than his favored maxims to guide scientists in making revisions when theory collides with facts.

Revisability I: Holding Statements True Come What May

As I stated in the first section of this chapter, Quine's brand of holism is characterized by more than its rejection of the claim that each individual statement of empirical science can be confronted with the results of observation or experiment. He also rejects the claim that the several

statements of science that face the tribunal of experience jointly are divided into those that can be revised in the face of recalcitrant experience and those that cannot. This is an important addition.

In this section I will examine one side of this position: that, in Quine's words, "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system." In the next section I will examine the other side of the position: that, also in Quine's words, "no statement is immune to revision." (Here, too, I believe, the same proviso about drastic adjustments is appropriate.)

It is tempting to regard these apparently separate claims as nothing more than opposite sides of the same holistic coin. I believe, however, that there is a good reason for treating them separately: when he made these claims Quine seems to have had two different kinds of statement in mind. Thus, he might have made his point by saying that any supposedly synthetic sentence can be held true come what may and that no supposedly analytic sentence is immune to revision.

At first glance it may seem that the claim that any statement can be held true come what may (with the added proviso) is a simple truism about logic. For the rejection

of an observation categorical, or, indeed, of any other statement, logically requires the rejection of at least one of the statements that logically implies it. In the typical case, any one of the statements that do the implying can be held true provided that at least one of the others is held to be false. Quine, though, was making more than this fairly straightforward point.

There is a tradition in modern empiricism of regarding two different kinds of statement as immune to revision. First, there are those sentences that were often classified as analytic, usually including the truths of logic and mathematics. Second, there are those sentences that were often classified as synthetic but that were believed to form the empirical basis of science. Unlike other synthetic sentences they were not, according to some thinkers, mere hypotheses. Moritz Schlick, for example, took a position somewhat different from Quine's.

> While in the case of all other synthetic statements determining the meaning is separate from, distinguishable from, determining the truth, in the case of observation statements they coincide, just as in the case of analytic statements. However different therefore "confirmations" are from analytic statements, they have in common that the occasion of understanding them is at the same time that of

verifying them....Both are absolutely valid.[34]

We needn't consider all of the types of analytic sentence that figure in the ongoing debate between, for example, Quine and Jerrold Katz on the issue of analyticity (and intensionalism generally). Whatever the merits of Katz's case for analyticity, and whatever the merits of Quine's case against it, I will, in the next section, confine my attention to sentences of logic and mathematics. We also needn't canvas all of the positions taken on what were called "basic sentences" or "protocol sentences" during the positivist debates of the 1930s. Whatever the strengths or weaknesses of any one of those positions, I will, in this section, concentrate on the more general question of what consequences follow from holding true the contradictory of any such statement, come what may.

For his part, Quine has rejected the belief that any statement of science is incorrigible. This, I contend, is why he made a separate point of insisting that any statement can be held true come what may. For if any supposedly synthetic sentence, including an observation categorical, can be held true no matter what, then its contradictory need

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[34]Moritz Schlick, "The Foundation of Knowledge," in Logical Positivism, ed. A.J. Ayer (New York: The Free Press, 1959), 225.

not be regarded as unrevisable. Though the contradictory of one of Quine's observation categoricals would not be an observation statement in Schlick's sense, it might be derivable from such a statement.

Like Neurath before him, Quine rejects the claim that there are incorrigible observation statements that form the foundation on which science is built. To hold that any such foundational statements are incorrigible, however, is to hold not merely that any statements that contradict them are corrigible. It is to hold the even stronger position that, in any confrontation between the former and the latter, it is the latter that has to lose. Find an incorrigible statement that speaks of a single nonwhite swan and you have telling evidence against the observation categorical

(4-11) All swans are white

and, under these circumstances, the latter statement cannot be held true.

When Quine says that any statement can be held true come what may, he emphatically includes observation categoricals, and indeed observation sentences generally on the list.

> Even a statement very close to the periphery can be held true...by

pleading hallucination...[35]

This is a position somewhat more radical than Duhem's position and it involves more than a healthy skepticism about the incorrigibility of observation statements. For, in an extreme case, one can hold a statement like (4-11) to be true in the face of any alleged negative observation statement that tells of a nonwhite swan. And the difference between an extreme case and a moderate one is a difference of degree and not a difference in kind.

The more often we have to plead hallucination, the more drastic our revisions may be. A lone report of a white raven or a black swan may be called into question without ruffling too many feathers, especially if it is made by an inexperienced observer. A whole flock of such reports, made by many experienced birdwatchers, can hardly be so easily dismissed. (Recall that Popper proposed that scientists not accept stray basic statements but accept such statements only when a falsifying hypothesis was conjectured and corroborated. Interestingly enough, Quine has no corresponding proposal.)

He did not, of course, suppose that rejecting inconvenient observation statements was the usual course in

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[35]Quine, "Two Dogmas of Empiricism," 43.

empirical science.

> Normally, observation is the tug that tows the ship of theory; but in an extreme case the theory pulls so hard that observation yields.[36]

Nor, apparently, did he think that he was merely describing the reluctance of scientists to abandon such an inconvenient statement. There is a normative issue here.

> In the cases where we waive an observation--and they had better be pretty special--...[37] (italics added)

The reason why they had better be pretty special, in Quine's view, is that the making of successful predictions is such a central part of the scientific enterprise and the testing of observation categoricals is the testing of science. A policy of refusing to admit the failure of one's predictions or the falsehood of one's observation categoricals makes them successful only in a Pickwickian sense, and such a policy involves the testing of theories only in a Pickwickian sense, too.

Yet if we plead either hallucination or optical illusion in the case of the report of the nonwhite swan then some fairly drastic adjustments may be necessary. If, for example, we decide to hold true the claim that all swans are

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[36]Quine and Ulian, The Web of Belief, 29.

[37]Ibid.

white, we may have to reject the claim that a group of truthful and experienced ornithologists were right when they said that they saw a nonwhite swan right in front of them in broad daylight under conditions that do not normally create optical illusions or hallucinations.

It is not a question of preserving the observation categorical, or any of the sentences that jointly imply it, as part of the explanans of any explanandum. As Quine insists, such an advance may or may not be made in the fullness of time.

There may be a question, of course, of explaining (not the failure of the observation categorical but) the failure of the observers. For the puzzling phenomenon here is why the competent scientists went wrong.

There may also be a question of protecting one theory while wreaking havoc with another. Optical theory is involved in judging the results of observation and experiment in physical theory most obviously whenever such devices as microscopes and telescopes are used. (The use of instruments sanctioned by some part of physical theory was, of course, an important consideration for Duhem.) Optical theory is also involved where vision is used at all. Likewise, acoustical theory is involved whenever hearing comes into play. Similar remarks could be made about other

parts of physical theory and the senses of smell, taste, and touch.

Furthermore, theories of perception are also involved in all of these cases. If this does not seem obvious it may be because we are accustomed to operating at a common sense level, without the explicit introduction of theory. This is so not only when we suppose that conditions for perception are normal (the usual case) but also when they are not. Feyerabend briefly touched on this point.

> Thus our habit of saying 'the table is brown' when we view it under normal circumstances, with our senses in good order, but 'the table seems to be brown' when either the lighting conditions are poor or when we feel unsure of our capacity of observation expresses the belief that there are familiar circumstances when our senses are capable of seeing the world, 'as it really is' and other, equally familiar circumstances, when they are deceived.[38]

Thinking of the fabric of scientific statements as "impinging on experience only along the edges" does not do justice to the situation.[39] For optical theory and theories of perception are sometimes closely interwoven with other theories in empirical science. If we refuse to revise an observation categorical in the face of repeated counter-

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[38]Paul Feyerabend, Against Method (New York and London: Verso, 1978), 31.

[39]Quine, "Two Dogmas of Empiricism," 42.

observations then we may have to face the possibility (indeed, even the necessity) of revising some theory of perception. This is especially so if we persist in rejecting a series of counterobservations made under circumstances that, the theory tells us, are such as to render the observations veridical.

I would like to suggest, in fact, that Quine's metaphor of the fabric of science, with its center and its periphery, is somewhat misleading in cases like this. It suggests that we have a whole series of observation categoricals like "All swans are white" or "All ravens are black" woven along the edge of a fabric, and connected by strands of logical implication to more secure statements in the center of the cloth. The edge of this fabric, then, is tugged at various points by individual statements that are entirely independent of one another. If one of those statements tells of a nonwhite swan or a nonblack raven then either the observation categorical is unwoven from the fabric (together with at least one of the statements in the group that implied it) or the statement that describes the counter-example is not woven into the fabric. (I will discuss in the next section the possibility of rejecting the logical law that forbids including both sentences in the fabric of science.)

I suggest that a better metaphor is needed to show how perceptual theory is woven together with the statements that confront observation categoricals. When we have to choose between rejecting an observation categorical and rejecting a statement that conflicts with it, we may be rending the fabric of science whichever choice we make.

Furthermore, there is an especially pressing problem in cases of a sort that did not seem to trouble Quine: cases of crucial experiments. The negation of an observation categorical is not itself an observation categorical, of course, but the testing of observation categoricals does lend itself to the sort of analysis of crucial experiments that I gave in Chapters Two and Three. It is conceivable, for example, that each of two observation categoricals, related as contraries rather than as contradictories, might be deducible from nontest statements of its own scientific theory. Suppose, to elaborate on one of Quine's own examples, that

(4-12) All riverine willows lean toward the water

is deducible within one theory, while

(4-13) No riverine willows lean toward the water

is deducible within another, competing one.

Any willow tree that we find by the side of the river will either lean toward the water or it won't. Assume, for

the sake of argument, that there is no question about whether we can tell which it does. It is not obvious how to describe the situation in terms of rending the single connected fabric of science. Are the two competing theories part of the same single fabric? Are we rending that fabric whichever way the evidence statement that we accept pulls us? And are we rending that fabric whether we accept this evidence statement as true or not?

Revisability II: No Statement is Immune to Revision

Just as Quine holds that <u>any</u> statement, including any supposedly synthetic statement, <u>can</u> be held true come what may (if we make drastic enough adjustments elsewhere in the system of which it is a part) so also does he hold that <u>no</u> statement, including no supposedly analytic statement <u>must</u> be held true come what may. The latter is just a re-statement of his claim that no statement is immune to revision. I believe that the proviso is needed in the second case too, for the revisions necessary in the latter may be no less drastic than those necessary in the former. (I doubt that Quine would object to this point. It seems to me that I am merely making explicit in this claim what is clearly implicit in his discussion of it.)

There are both similarities and dissimilarities here

between Duhem's and Quine's views. Although he did not speak of analytic and synthetic statements in physical theory, Duhem did take issue with the conventionalism of Poincaré and others on the question of definitions. It was contended that some individual statements of physical science are immune to refutation, being definitions or principles rather than empirical laws. Duhem saw the situation differently. Here is his summary of the view that he opposes.

> Certain fundamental hypotheses of physical theory cannot be contradicted by any experiment, because they constitute in reality definitions, and because certain expressions in the physicist's usage take their meaning only through them.[40]

The example that he considered concerns the free fall of a body near the surface of the earth. If the acceleration of such a body is found not to be constant (as it is supposed to be) then, far from abandoning the purported law in question, physicists would conclude that the body that has been observed was not falling freely, that something obstructed its fall. Thus, it is part of the definition of "freely falling body" that its acceleration be constant. This is not a lawlike statement that can be

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[40]Duhem, Aim and Structure, 209.

falsified but is a principle that is, by convention, immunized against falsification, or so the story goes.

Duhem's analysis differed. He agreed that, in such a case, physicists would not abandon a hallowed law of physics. He disagreed, however, that such a law was granted the status of definition, or that it was forever irrefutable. This was just an occasion for exercising good sense in deciding not to abandon a widely accepted portion of physical theory in order to accommodate an experimental result that can be handled more easily in a different way. Duhem saw no case of definition here. Of course, physicists will hold true the law that bodies in free fall near the surface of the earth will fall at increasing velocity but constant acceleration. If a given body falls to the earth at a velocity that does not increase constantly over time then they will simply conclude that the body is not falling freely. But Duhem did not believe that physicists had to draw this conclusion at all.

> But what impels the physicist to act thus is <u>not</u> logical necessity. It would be awkward and ill inspired for him to do otherwise, but it would not be doing something logically absurd...[41]
> (italics in the original)

In fact, even those portions of theory that have been

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[41]Ibid., 211.

universally accepted for generations are not immune to refutation, in his view.

> Indeed, we must really guard ourselves against believing forever warranted those hypotheses which have become universally adopted conventions, and whose certainty seems to break through experimental contradiction by throwing the latter back on more doubtful assumptions. The history of physics shows us that very often the human mind has been led to overthrow such principles completely, though they have been regarded by common consent for centuries as inviolable axioms, and to rebuild its physical theories on new hypotheses.[42]

Jules Vuillemin has commented on the parallel between Duhem's and Quine's views on this topic.

> In point of fact, Poincaré's conventions were for Duhem what Carnap's analytic truths are for Quine....By taking exception to the autonomy of Poincare's conventions he [Duhem] anticipated, within his restricted horizon, Quine's criticism of the "first dogma."[43]

The dissimilarities between Quine and Duhem come more clearly into focus when we take a closer look at the restricted horizon within which Duhem worked. In the first place, of course, he was analyzing physical science and not empirical science more generally. In the second place he did not suggest that logic and mathematics were on a par

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[42]Ibid., 212.

[43]Vuillemin, "On Duhem's and Quine's Theses," 596-597.

with physics, in the sense that, say, a logical law could be revised in the face of experience.

Quine, for his part, did suggest that purported logical laws were open to revision and used as a possible example, the law of excluded middle. Duhem, of course, was writing before modern quantum physics but there is no indication that he would have agreed with the American logician on this point.

As suggested earlier in this chapter, the rejection of the claim that the class of individual statements of empirical science divide into those that face the possibility of revision and those that do not can be read as two separate claims. The first is that, in Quine's words, quoted above, "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system." The second is that, "Conversely, by the same token, no statement is immune to revision."[44]

I have suggested above that although he did not repeat the proviso "if we make drastic enough adjustments..." in the second case, it surely applies there as well. Recall that the statements of logic, for example, are regarded by Quine as "germane to all topics." It is precisely this

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[44]Quine, "Two Dogmas of Empiricism," 43.

germaneness that would necessitate drastic adjustments elsewhere if one or more of these sentences were stripped of the immunity that they have traditionally enjoyed.

Even with the proviso, however, Quine's position is a bold one to take. A single counterexample--one statement that is immune to revision--refutes the claim. As if to underscore his point, Quine contended that "the logical laws [are] in turn simply certain further statements of the system, certain further elements of the field."[45]

Duhem restricted his analysis to physical science and (to him) a central feature of theoretical physics was its reliance on mathematics. Theoretical physics is mathematical physics and the mathematical development of a theory is an important step on the journey from the fundamental hypotheses of physical theory to the reports of observation and experiment. Are the statements of mathematics itself, including arithmetic, to be revisable?

There is a stark contrast between Duhem and Quine when it comes to mathematics because what figures so centrally in the generation of test statements from the nontest statements of physical theory, according to Duhem, figures hardly at all in Quine's analysis of the testing of

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[45]Ibid., 42.

hypotheses in empirical science. Physical laws relating the volume, temperature and pressure of a gas, or relating the gravitational attraction between two bodies to their masses and distance from one another, are not observation categoricals.

Of course, statistical generalizations cannot be refuted in the same way that observation categoricals can be refuted. What if, in addition, the supposed truths of mathematics are up for grabs? How can we even derive statistical generalizations to serve as test statements, if the aid of the probability calculus is needed? Furthermore, if a statistical generalization at the level of test statements is disconfirmed then how can this require any reevaluation of the nontest statements from which it was derived?

Quine is not without a response to these questions. He assimilated our notion of mathematical necessity to the maxim of minimum mutilation.

> In particular the maxim constrains us, in our choice of what sentences of S to rescind, to safeguard any purely mathematical truth; for mathematics infiltrates all branches of our system of the world, and its disruption would reverberate intolerably.[46]

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[46]Quine, Pursuit of Truth, 15.

Here we have the basis for Quine's response to the objection that mathematics is revisable. Despite his claim that no statement is immune to revision, he is quick to point out that there are some statements that we are (and, no doubt, ought to be) very reluctant to revise. The drastic nature of the further revisions that would be necessary is the reason for our reluctance. Thus, he is led to a position on the revisability of statements that recalls his more general holistic position and that it would not be unfair, I think, to characterize as radical in principle but conservative in practice. Even if we accept this position with respect to mathematics, however, it doesn't follow that we must accept it with respect to logic.

Despite a frank acknowledgement of the high price that one has to pay for doing so, Quine adheres faithfully to classical two-valued logic.[47] It seems to me, however, that important objections can be raised to the claim that no statement is immune to revision. And it is not just that revising the laws of logic would require such drastic revisions elsewhere in our system of science that we would never (or, rather, hardly ever) contemplate such a measure.

I will defer to the final chapter my discussion of a

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[47]See especially Quine, "What Price Bivalence?" in Theories and Things, 31-37; and Pursuit of Truth, 92.

central objection to Quine's position on the revisability of logic. There I will compare and contrast Quine's and Popper's views on this topic and will make the case that Popper's position, and indeed his holism generally, is more plausible than Quine's. Before making this case I will, in the next and final section of this chapter, consider a preliminary objection to Quine's position on the revisability of logic stemming from his views about translation.

Conclusion and an Objection

It seems fair to say that Quine is the most radically holistic of our three authors. I have argued that he has retreated from an earlier, more extreme, to a later, more moderate, position but that this later position is still open to criticism. I have examined both the logical and the epistemological sides of his moderate holism as well as the twin corollaries of this holism concerning the revisability of statements in the face of recalcitrant experience.

In this final section of the chapter I would like to make some further observations and present an objection to Quine's controversial claim that no statement is immune to revision.

In defending his thesis of the indeterminacy of translation, Quine considered the situation that he characterized as "radical translation," the rendering of utterances from a previously unencountered language into our known.

> The utterances first and most surely translated in such a case are ones keyed to present events that are conspicuous to the linguist and his informant....The linguist will at first refrain from putting words into his informant's mouth, if only for lack of words to put. When he can, though, the linguist has to supply native sentences for his informant's approval...[48]

Gradually, the linguist will learn to interpret certain utterances as assent or dissent on the part of his or her informant. Then, of course, the business of submitting native sentences to the informant for approval begins in earnest.

By and by we get to the translation of the truth-functional connectives. By reference to assent and dissent, Quine stated semantic criteria for determining whether a given native utterance is to be taken as expressing a given truth function.

> The semantic criterion of negation is that it turns any short sentence to which one will assent into a sentence from which one will dissent, and vice versa. That of conjunction is that it produces compounds to which...one is prepared to assent always

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[48]Quine, Word and Object, 29.

> and only when one is prepared to assent to each component. That of alternation is similar with assent changed twice to dissent.[49]

Granted these semantic criteria, the translation of a native assertion as a statement of the form 'p & -p' or '- (p v -p)' would simply be a bad translation. For assent to any native sentence translated into English as a sentence of the form 'p & -p' or dissent from any native sentence translated into English as a sentence of the form 'p v -p' would be evidence of a mistake on the part of the translator. It seems clear, for example, that two utterances that meet the semantic criteria for negation and conjunction could not combine with an assertion in such a way as to give us a compound assertion to which the native simultaneously both assents and dissents.

Quine considered just this point.

> That fair translation preserves logical laws is implicit in practice even where, to speak paradoxically, no foreign language is involved. Thus when to our querying of an English sentence an English speaker answers 'Yes and no', we assume that the queried sentence is meant differently in the affirmation and negation....Again, when someone espouses a logic whose laws are ostensibly contrary to our own we are ready to speculate that he is just giving some familiar old vocables ('and', 'or', 'not', 'all', etc.) new meanings.[50]

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[49]Ibid., 57.

[50]Ibid., 59.

I believe that there is an important problem here for Quine. If we accept the semantic criteria he gave for the truth-functional connectives--and from these we can construct any of the other truth functions--then it seems that classical bivalent logic is immune to revision because any supposed revision can simply be regarded as a change of meaning of the connectives. Indeed, Quine himself has applied this argument to the case of a deviant logician who would deny the law of noncontradiction.

> To turn to a popular extravaganza, what if someone were to reject the law of non-contradiction and so accept an occasional sentence and its negation as both true? An answer that one hears is that this would vitiate all science...
>
> In answer to this answer, one hears that such a full-width trivialization could perhaps be staved off by making compensatory adjustments to block this indiscriminate deducibility of all sentences from an inconsistency...
>
> My view of this dialogue is that neither party knows what he is talking about. They think they are talking about negation, '-', 'not'; but surely the notation ceased to be recognizable as negation when they took to regarding some conjunctions of the form 'p & -p' as true, and stopped regarding such sentences as implying all others.[51]

It seems to me that a similar line of argument would apply to <u>any</u> deviation from classical bivalent logic and at least one author, Susan Haack, has taken this as an attack by Quine on the very possibility of a deviant logic.

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[51]Quine, <u>Philosophy of Logic</u>, 81.

> We impute our orthodox logic to [the deviant logician], or impose it upon him, by translating his deviant dialect.[52]

She has concluded that Quine has flatly contradicted himself, or rather, that he later adopted a position flatly inconsistent with the one that he took earlier.

> It is worth observing at the outset that this argument of Quine's, which, if it were sound, would show that there can be no genuine rivals to classical logic, is incompatible with another thesis, propounded in e.g. the last section of 'Two Dogmas of Empiricism' (Quine [1951]), to the effect that none of our beliefs, beliefs about the laws of logic included, is immune from revision in the light of experience.[53]

Haack's point is well taken. Quine's claim that we impute our orthodox logic to the deviant logician, or impose it upon him or her, does seem, on the face of it, to be incompatible with the thesis that even the laws of logic are revisable.

Incidentally, we do not have here the sort of case in which the indeterminacy of translation threatens. According to Quine, there may be two different manuals of translation from another language into our own that are mutually inconsistent but are each consistent with the verbal

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[52]Quine, Philosophy of Logic, 81; quoted in Haack, Deviant Logic, 15.

[53]Haack, Deviant Logic, 15.

behavior of the natives whose language we are translating. But it is precisely Quine's semantic criteria for the truth functions that prevent this indeterminacy from threatening here. For if assent and dissent are the basic behavioral coordinates then we take a locution to be negation if and only if it turns assent into dissent, and vice versa, when prefixed to a sufficiently short sentence. And we take a locution to be disjunction if and only if it creates a compound from which one dissents in exactly those cases when one would dissent from each of the components. If a deviant logician dissents from both a statement and (what we take to be) its negation, then we would understand him or her to mean something different from what we mean by "or" or "not" (or both).

Believing in the possibility of genuine rivals to classical logic, Haack challenged Quine's decision to take assent and dissent as the two basic behavioral coordinates that give the linguist the grounds for translating native locutions as truth functions. She argued, instead, that one could take assent, dissent and puzzlement as basic, and state alternative semantic criteria.

> The <u>disjunction</u> of two sentences is that sentence to which one would assent if one assents to either component, from which one would dissent if one dissents from both components, and to to which one would react with puzzlement if one

> reacts with puzzlement to both components, or reacts with puzzlement to one component and dissents from the other.
>
> The negation of a sentence is that sentence to which one would assent if one dissents from the sentence, from which one would dissent if one assents to the sentence, and to which one would would react with puzzlement if one reacts with puzzlement to the sentence.[54]
> (italics in the original)

It seems to me that without an effective challenge to Quine's semantic criteria, we are forced to rule out the possibility of any deviation from classical bivalent logic, with (among other things) its laws of noncontradiction and excluded middle. The question is why Haack's challenge is effective. Why regard assent, dissent and puzzlement as the basic behavioral coordinates and adopt Haack's semantic criteria for translating the connectives?

A major problem with Haack's suggestion, in my view, is that puzzlement is not on a par with assent and dissent. A native's response to the linguist's assertion needn't, of course, be either assent or dissent. Puzzlement, amusement or embarrassment are other possible responses. Assent and dissent, however, are basic in that they can be coordinated with truth values. Absent an intent to deceive or conceal the truth, one assents to what he or she believes to be true and dissents from what he or she believes to be false.

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[54]Ibid., 19.

Puzzlement, amusement or embarrassment can be explained away as the result of any one of several different factors that have nothing to do with the truth value of any assertion that is made. (Quine had a reason for insisting that the sentences submitted by the linguist to the native informant for assent or dissent be short enough to be comprehended by the latter. Failure to comprehend an assertion might produce a puzzled response.)

Even when puzzlement is related to truth value, it doesn't follow that it is a basic behavioral coordinate. It may be, for example, a native's reaction to an assertion presented by the linguist for assent or dissent when the native has no idea of the truth value of the assertion (and perhaps even supposes the linguist to know this). Such a reaction wouldn't, by itself, present any reason for questioning Quine's semantic criteria. We can easily, for example, have the following sort of case. Presented first with an assertion and then with its negation, the native responds twice with puzzlement. Then, however, the informant assents to the compound that joins the two with an utterance that we have previously translated as "or."

Quine, of course, could not in such a case translate the utterance in question as disjunction on this evidence but could do so on the basis of other evidence. If the

locution in question met his criteria for disjunction then the puzzlement could simply be counted an irrelevant phenomenon chalked up to, say, the undecidability of each component assertion. (The native informant, for example, may have no idea whether there are, or are not, an even number of gavagais in his or her village. Nevertheless our bivalent hero or heroine will agree that it must be one or the other.) Granted Quine's willingness to tolerate undecidable statements as a price of accepting bivalence, he has no special problem here.

It seems, then, Quine's dictum that "fair translation preserves logical laws" is incompatible with his dictum that "no statement is immune to revision" because any supposed revision of a purported logical law can be translated away when (to use Quine's words) we "impute our orthodox logic" to the deviant logician or "impose it upon him." Haack has given no convincing reason why this view of translation is mistaken.

Of course, one may accept the claim that Quine's position on translation of the truth-functional connectives is incompatible with his position on the revisability of classical logic and accept the latter while rejecting the former.

This issue of revisability, I believe, deserves further

examination. In the next and final chapter of this dissertation I will summarize my conclusions about the three different versions of the Duhem-Popper-Quine thesis and will make the case that Popper's version is the most defensible of the three, taking another look at this question of the revisability of logic.

CHAPTER FIVE:

The Duhem-Popper-Quine Thesis Revisited

Introduction

In this dissertation I have argued that there is a central thesis that is reflected in the work of Pierre Duhem, Karl Popper and W.V. Quine, and I have chosen to call it "the Duhem-Popper-Quine thesis." In this final chapter I intend to review and summarize what I have said about the views of our three different authors, to make some further observations, and to present a case that Popper's version of this important central thesis, with a small but important modification, is more defensible than that of either Duhem or Quine.

Summary, With Some Further Observations

In Chapter One I briefly characterized the Duhem-Popper-Quine thesis as a two-part claim: first, when a test statement of scientific theory is confronted with the evidence of observation or experiment, and is refuted, we cannot tell which of the nontest statements of scientific theory from which it was derived is responsible for the refutation; second, this ambiguous falsification procedure is the typical case in science.

I conceded that although there seems to be no plausible case to be made that the Duhem-Popper-Quine thesis is not true, there does seem to be a case to be made that it is only trivially true. I responded to this anticipated objection that even apparently trivial truths can be important. Let me add here that the less apparent the trivial truth, the greater its importance may be. There is, however, more that can be said for the holistic central thesis.

What can be said is that what makes the thesis important is the sheer frequency of the ambiguity of falsification. Granted my characterization of what I have, with some abbreviation, called "test statements" and "nontest statements," it <u>is</u> trivially true that the former, but not the latter, can be directly confronted with the evidence of observation or experiment. What is <u>not</u> trivial is the necessity of inventing a distinction between the test statements and nontest statements of scientific theory in the first place. A closer look at the work of Duhem, Popper and Quine helped to show just how important it is to make this distinction. For statements that cannot be directly confronted with empirical evidence are abundant in science.

And their abundance does not depend on a narrow characterization of the statements--it seemed appropriate to

call them "evidence statements"--with which only test statements can be directly confronted. Even a rather broad characterization of these evidence statements will do. We can allow them, for example, to be about easily identifiable characteristics of ordinary physical objects so that we do not rule out too many candidates as unqualified to be test statements. In that case, even the old standby, "All men are mortal," would not pass muster.

The Duhem-Popper-Quine thesis, then, is both true and important. I considered how this important central thesis is reflected in the work of our three main authors.

In Chapter Two I distinguished two Duhem theses, one concerning the refutation of what the French author called "isolated hypotheses" in physical science and the other concerning the possibility of what he dubbed "'crucial experiments'" in physical science. The former was his version of the Duhem-Popper-Quine thesis.

I argued that Duhem's version of this thesis was flawed in that it was too closely tied to certain features of modern physical theory. Having made too rigid a distinction between those sciences that are, in his words, "still close to their origins" and those that are not--in effect a rigid distinction between observational and theoretical sciences-

-he failed to see a gradual shading of sciences from those that are closer to their origins in common sense to those that have moved farther away. Indeed, Duhem failed to see that the thesis applied even to his own example of a common sense truism. As noted above, "All men are mortal" cannot be directly confronted with the evidence.

In physical science, the use of mathematics and the use of physical instruments are closely interwoven with the confrontation between test statements and the evidence statements garnered from physical experiments. But, I argued, neither the use of mathematics nor the use of instruments is a necessary condition for the ambiguity of falsification of nontest statements of scientific theory as the result of the refutation of the test statements derived from them. (It may be the case, however, that they are jointly sufficient conditions.) Furthermore, Duhem's sweeping thesis about isolated hypotheses--he argued that no isolated hypothesis could be refuted by experiment--was the result of restricting his analysis to physical science. Venture beyond the narrow boundaries that he set and you have to face counterexamples to the sweeping thesis that need never have troubled the French physicist.

Despite some flaws in Duhem's analysis, however, I believe that there is much to be said in its favor. He gave

a fairly detailed and interesting account of the construction of physical theory and of the derivation of test statements from nontest statements of physical theory, with the aid of mathematics. He also gave a plausible account of the role that instruments play in physical theory and the design of physical experiments. By confining his attention to the science he knew best, Duhem was able to give a more thorough analysis of his subject than would otherwise have been possible. Even his controversial views about explanation and the aim of physical theory turned out, on closer examination (and granting his special use of the term "explanation"), to be more defensible than they might at first have appeared to be. Of our three main authors, Duhem presented a version of the central thesis that was the most thoroughly informed by the actual practice of research in a particular science.

In Chapter Three I discerned a guarded holism in Popper's analysis of empirical science. Like Duhem he stressed the ambiguity of falsification that results from the refutation of a test statement. Unlike Duhem, he did not confine his attention to physical science and he did not make the sweeping claim that no isolated hypothesis could be refuted by the results of observation or experiment. Indeed, Popper

not only admitted but vigorously insisted that there are counterexamples to the sweeping holistic central thesis. The failure to score a direct hit against any specified nontest statement when a test statement is refuted may be the typical case in empirical science but it is not the only kind of case that can be found.

In examining Popper's guarded holism I discussed a problem with his uncompromising, and somewhat unguarded, falsificationism. For statements like "All men are mortal" pose a more serious problem for him than they do for Duhem. I argued, though, that this problem can be solved if we refrain from characterizing the statements of empirical science by their logical relationship to the set of evidence statements that he termed "basic statements." Instead we can characterize the statements of empirical science solely by our methodological resolve not to immunize any of them against refutation by the results of observation or experiment.

Although Popper extended the central thesis beyond the boundaries of physical science, he did not extend it beyond the boundaries of empirical science, as traditionally understood. Most importantly, his philosophy of science is wedded to classical logic and he did not envision any divorce or separation between the two. I briefly defended

this as a plausible position to hold and in the final section of this chapter I will make some further remarks in its defense, while acknowledging that Popper's case for the unrevisability of classical logic is not entirely satisfactory. I suggest a slight modification in his position.

Popper was sensitive to two important issues: the possibility of finding counterexamples to the sweeping holistic thesis and the possibility of reducing the number of nontest statements tainted by the refutation of a test statement, even when one could not single out any specified nontest statement as the only one so tainted.

It is interesting and ironic that the author least sympathetic to a maxim of minimum mutilation was the one to argue most vigorously and, I believe, quite convincingly, that when a theory runs afoul of the facts, the damage can sometimes be fairly narrowly contained. It is also interesting and ironic that Popper seems to have become better known for his opposition to a sweeping holism than for his adherence to a guarded holism.

In Chapter Four I examined Quine's more thoroughgoing holism. I argued that he had retreated from an earlier, more extreme, to a later, more moderate, position. I also

argued that even his more moderate holism is open to criticism.

Quine was the most radically holistic of the three authors. He has, I argued, retreated from the extreme position that, in his words, "the unit of empirical significance is the whole of science" to the more plausible stand that smaller chunks of theory can face the tribunal of experience. Although he has retreated in one direction, however, Quine has not retreated in another. He still defends his radical view that even the laws of logic and mathematics are not, in principle, immune to revision.

Duhem had restricted his holism to physical science, without any clear indication of how far beyond this horizon he was willing to countenance its extension. Popper extended his holism to empirical science with a clear indication that he would not countenance its extension to include what he called the "demonstrative science" of logic. Quine extended his holism about as far as it could go. In principle, at least, even the laws of logic and mathematics are up for grabs.

In practice, however, Quine is far more conservative than his thoroughgoing theoretical holism might lead one to suspect. His philosophy of science is not wedded to classical logic as Popper's is but he remains faithful to

his first love, even as he flirts with the idea of embracing some nonclassical logical system. Quine is quite sensitive to the conservative demand for caution. Since logic is germane to all (and mathematics, perhaps, to nearly all) topics, tinkering with them would have far-reaching consequences. We rebuild the ship of science while afloat in it but we have to be careful about rocking the boat. It is consistent with Quine's view that classical logic is the last thing we should consider revising in the face of recalcitrant experience: a step to be taken only when all else fails. He differs from Popper only in that the latter insisted that this is a step that should never (and not just hardly ever) be taken.

Quine is also sensitive to the presence of counterexamples to a sweeping holism, even if he was less aggressive than Popper in pressing this point.

A Guarded and Moderately Restricted Holism

It is my contention that Popper's holism is more defensible than that of either Duhem or Quine. In this final section I will try to make the case for that conclusion, although I will acknowledge that Popper's position needs a slight modification.

The two most important issues, I believe, concern the

guardedness and the restrictedness of Popper's holism. Of these two issues, the latter is more central. It is precisely because he did not restrict his holism to physical science, but instead extended it to all of empirical science, that Popper was more guarded than Duhem was. By venturing beyond the boundaries within which Duhem had confined his analysis, the Austrian philosopher was forced to face the possibility of counterexamples that need never have troubled the French physicist. (One reason that he could so easily venture where Duhem did not was that he did not tie his holism to such features of modern physics as the mathematization of theory and the use of physical instruments.)

From the claim that physical experiment can never condemn an isolated hypothesis in physical science (but only a whole group of hypotheses) it doesn't follow that observation or experiment in branches of science outside of physics cannot sometimes condemn an isolated hypothesis (and not just a whole group of hypotheses). And these condemnable hypotheses need not all be such ordinary empirical generalizations as "All swans are white" or "All ravens are black." In Chapter Three I argued that some of the fundamental hypotheses of such a social science as economics may provide counterexamples to a sweeping holism.

The more controversial issue, however, does not concern the comparative unrestrictedness that distinguishes Popper's holism from Duhem's but the comparative restrictedness that distinguishes it from Quine's. For Popper did not extend his holism beyond the boundaries of empirical science as traditionally conceived. Whatever chunk of nontest statements of scientific theory is tainted by the refutation of a test statement when that statement collides with the facts, that chunk does not include whatever principles of logic were used to derive the test statement in question.

Although Popper labeled his view of logic "realistic," he seemed to be defending his opposition to considering classical logic open to revision in the face of recalcitrant experience on what I called "consequentialist" grounds. Such a defense would make his position very similar to Quine's. For although Quine considered any statements in our web of belief to be revisable, including what we take to be logical truths, he acknowledged that we would revise such statements only in rather extreme cases, because of the far-reaching consequences of doing so. Both authors, it seemed, took positions on the revisability of classical logic with an eye toward the consequences that would come in the wake of revision.

On the face of it, though, they differed in two ways.

First, Popper held that we should never, whereas Quine held merely that we would (or perhaps, should) hardly ever revise the principles of classical logic. Second, they focused on different consequences. Quine was concerned with the excessive mutilation of theory that might result from too impulsive a tampering with logic and mathematics. Popper was more worried about the undermining of critical discussion from too casual an attitude toward classical logic, which he considered the organon of criticism.

To be more precise, Popper's opposition to the revisability of classical logic in the face of recalcitrant experience was an opposition to any revision that would weaken classical logic. He was not, in principle at least, opposed to every revision that might strengthen it. And he was not opposed to every revision that would weaken classical logic as a tool to be used (for example, in mathematics) in proof; he was opposed only to any revision that would weaken classical logic as a tool to be used in disproof (that is, in critical discussion, in empirical science and elsewhere).

In practice, however, it is weakening classical logic that is at issue. This is sometimes put in terms of the revisability of one or more of the laws (or purported laws) of classical logic. Quine holds that such purported logical

laws are, in principle, not immune to revision, however reluctant we may be to revise them (and however prudent our reluctance). Popper, in effect, immunized the classical laws against revision. I will offer a defense of his position, with one important modification, and will argue that Quine's position (that no statement is immune to revision) is open to refutation by a fairly obvious counterexample.

A Refutation and a Defense

In the last section of Chapter Four I argued that Quine's claim that no statement is immune to revision is incompatible with his claim that fair translation preserves logical laws. For if we impute our orthodox logic to the deviant logician, or impose it upon him or her, then any supposed revision of classical logic can simply be explained away as faulty translation. As Quine put it in connection with the law of noncontradiction, the deviant logician's predicament is that "when he tries to deny the doctrine he only changes the subject."[1]

Nevertheless, this cannot be the last word on the subject. After all, one can accept what Quine had to say

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[1]Quine, Philosophy of Logic, 81.

about the revisability of logical principles while rejecting what he said about the translation of logical particles. Maybe he was right about the former and wrong about the latter.

In this final section I will argue that Quine's claim that no statement is immune to revision is a mistake and that Popper's explicit opposition to giving up the law of noncontradiction is an important point in his favor. I will, however, close with the argument that Popper's opposition to any revision of classical logic is not so easily defensible and will suggest a small but important change in it.

Let me start with the law of noncontradiction. Popper has contended that our acceptance of this law is what ultimately drives critical discussion, including the critical discussion of what I have labeled the "test statements" of scientific theory in the light of empirical evidence.

> For criticism invariably consists in pointing out some contradiction; either a contradiction within the theory criticized, or a contradiction between the theory and another theory which we have some reason to accept, or a contradiction between the theory...and certain statements of fact.[2]

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[2]Popper, "What Is Dialectic," in Conjectures and Refutations, 316.

And criticism, for Popper, is the lifeblood of science and the fountainhead of scientific progress.

> But criticism is, in a very important sense, the main motive force of any intellectual development.[3]

It is, I believe, plausible to hold that the law of noncontradiction is not revisable in the light of experience and, thus, implausible to hold that no statement is immune to revision. It will help to take a closer look at the law of noncontradiction in classical logic.

Classical logic, of course, is bivalent. Taking every well-formed formula to be a statement, we can lay down the twin pillars of bivalence as follows: first, every statement is either true or false; and second, no statement is both true and false.

Let 'p' be a statement of classical truth-functional logic. The law of noncontradiction, then, is rendered as

(5-1) - (p & -p)

and is logically true on truth-functional grounds or, as Quine and others sometimes put it, truth-functionally true. This hallowed law is a theorem of classical logic in any of its axiomatizations.

Now, take a test statement such as

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[3]Ibid.

(5-2) All swans are white

which we can take to be either one of Quine's observation categoricals or one of Popper's strictly universal statements and which we suppose to have been derived from the nontest statements of some theory. (We needn't suppose this test statement to have existential import.) Let's also assume that, on the basis of the evidence statements we have gathered, we have derived the conclusion "Some swans are not white," which is equivalent to

(5-3) Not all swans are white

[i.e., "It is not the case that all swans are white."]

If we conjoin (5-2) and (5-3) and negate the conjunction to get a statement of the form (5-1) then we have

(5-4) It is not the case that both all swans are white and not all swans are white.

As Popper would have it, our unwillingness to put up with contradictions like the conjunction of (5-2) and (5-3) is what drives us to reject either the test statement or the evidence statement in such a case since this unwillingness prevents us from accepting both statements of a pair of contradictories. (We can, of course, suspend judgment for the time being and decide not to reject either one without further investigation but that is simply to postpone the

inevitable.)

The point here is not that if we somehow let go of the law of noncontradiction in response to the clash between a test statement and an evidence statement then we would have to make very drastic adjustments in empirical science and so would choose never (or hardly ever) to do so. If that were all there was to be said then the law of noncontradiction might be revisable after all (although only, perhaps, in an extreme case). And the point is not that if we let go of this law then somehow we will be setting a dangerous precedent that may lead to the undermining of critical discussion in empirical science.

The point is that without the law of noncontradiction we have lost our reason to reject either a test statement or the evidence statement that conflicts with it. Ordinarily we might say that we have to reject either the test statement or the evidence statement "on pain of contradiction." But there is no pain of contradiction if there is no law of noncontradiction.

Indeed without the law of noncontradiction we can no longer make any sense out of the project of confronting test statements with evidence statements in the first place. Recall what Quine had to say about logical connections among statements in the fabric of science.

> A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements. Reevaluation of some statements entails reevaluation of others, because of their logical interconnections...[4]

In the absence of the law of noncontradiction, what could such a conflict with experience be?

The law of noncontradiction is, on the face of it, a counterexample to Quine's claim that no statement is immune to revision. And Popper's refusal to consider this law revisable is a crucial point in his favor.

Of course, Quine did not suggest that we would abandon either the law of noncontradiction or the law of excluded middle in the same way that we might, for example, simply abandon a universal generalization to which we have found a counterexample. What may be termed the "revision" of a law of classical logic, in the eyes of those who advocate revising classical logic, is accomplished by devising a rival system that lacks the law in question. To be fair to such revisionists we should not dismiss their position too quickly.

Perhaps some intrepid soul might advocate that we give up the insistence that no statement of empirical science is

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[4]Quine, "Two Dogmas of Empiricism," 42.

both true and false but this seems the least promising road for a revisionist to take. To make sense out of the testing of statements we have to suppose that if a test statement is false then that precludes its also being true. And to make sense out of the abandonment of any nontest statements as the result of the refutation of test statements derived from them, we have to suppose that if a nontest statement is false then that precludes its also being true. Indeed it is difficult to make sense out of the claim that there are statements of scientific theory that are both true and false. It seems, on the face of it, that any system that allows statements to be both true and false is simply incoherent.

Hilary Putnam has discussed the claim that there is at least one a priori truth: that not every statement is both true and false.[5] I would take the position that no statement of empirical science, including both test statements and nontest statements, is both true and false and that this (meta)statement is itself unrevisable.

Perhaps a champion of nonclassical logic would respond at this point that D.A. Bochvar developed a three-valued

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[5]Hilary Putnam, "There Is At Least One A Priori Truth" in Philosophical Papers, vol. 3 Realism and Reason (Cambridge: Cambridge University Press, 1983), 98-114.

logical system in which statements could take one of the following values: true, false, and paradoxical. Although this is not the same as a system in which some statements are both true and false, it may seem to have an affinity with such a system. Such a nonclassical logic, however, is hardly of any help in this case. Bochvar's proposal was to adopt a three-valued system as a way of dealing with the liar paradox, and his system has been criticized as inadequate to this task.[6]

Even if Bochvar's system were somehow adequate to solve the problem for which it was devised, however, it is surely not appropriate to replace classical logic as the organon of critical discussion in science. For the test statements of empirical science are not paradoxical sentences like "This statement is false." And bringing such statements before the court of empirical evidence does not involve the sort of logical analysis that is provoked by such paradoxical sentences.

It seems, then, that Quine's claim that no statement is immune to revision is a mistake. It does not follow, however, that Popper's position is without difficulty. For he has argued against <u>any</u> revision of classical logic and

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[6]Haack, <u>Philosophy of Logics</u>, 140.

that would rule out adopting a nonclassical system that lacks any other classical law, including (for example) the law of excluded middle. This last, however, is one sort of revision that is commonly advocated. The possibility that Quine raised in "Two Dogmas" involved the abandonment not of the law of noncontradiction but of the law of excluded middle. And various multivalent logical systems that lack the latter as a theorem have been developed in response to perceived difficulties with classical logic.

Both the law of noncontradiction and the law of excluded middle are theorems of classical logic, of course, and since both are tautologies they are logically equivalent in classical logic. It has seemed to some, however, that the law of excluded middle might be expendable even if the law of non-contradicton is not, and that a suitable revision of classical logic could be accomplished if one were willing to abandon the insistence that every statement is either true or false (while still insisting, however, than no statement is both).

The question, of course, is not whether there might be a use for such a system--Popper has explicitly conceded that there is a use for intuitionistic and other logical systems that weaken one's proof capabilities by abandoning, say, reductio arguments in proving theorems indirectly. The

problem is whether such a system has any value as an organon of critical discussion or whether, as Popper suggests, it simply undermines the critical discussion of, for example, what I have labeled the "test statements" of scientific theory. Popper may be right about this and there is something to be said for resisting the temptation to abandon bivalence.

Consider, for example, that if we were to adopt any system in which we allow statements to be neither true nor false but to take instead some other value then we might have to abandon even the modest role for crucial experiments that Popper saw for them. It isn't that we would be abandoning indirect proof and hence abandoning the position that refuting one theory could demonstrate a competing one. Neither Popper nor Duhem held such a position. The point is that if we use any system in which the law of excluded middle is not a theorem then we could no longer say that a statement 'p' derived from one theory and another statement '-p' derived from a competing theory would exhaust the possibilities. A "crucial experiment," then might fail to rule out either of the two theories between which it was supposed to decide (however provisional the decision in favor of the unrefuted theory).

Consider also that if we allow test statements to be

neither true nor false then we may at the very least be greatly complicating the process of confronting them with empirical evidence since there could be two or more different unfavorable outcomes instead of just one. A test statement, for example, might take a third truth value, or no truth value at all. And what if we allow the nontest statements of scientific theory to be neither true nor false? Where does all of this take us?

Unfortunately, pressing such questions will not give Popper as much help as he needs. The most that one could say is that the critical discussion of scientific theories might become much more complicated with a multivalent system. And complicating critical discussion may be even worse than complicating current empirical theory since critical discussion will affect science well into the future, even after current theory has long since been revised (and perhaps simplified). Although there may be good reasons for avoiding such complexity, it is difficult to see why they are in principle any stronger than Quine's reasons for being conservative when it comes to tampering with bivalence. It may be something that we should hardly ever do but that is not to say that it is something we should <u>never</u> do.

In the end, it seems to me, Popper need not have taken

such a strong position. He need not have disallowed in principle the possibility of adopting any nonclassical logical system in response to some collision between theory and fact. Granted his unwillingness to develop a line of argument like that suggested at the end of Chapter Three, he was without a general case against revising classical logic that is as strong as the case against abandoning the law of noncontradiction. Like Quine, of course, he could have been rather cautious about any tampering with logical theory because of the far-reaching effects of doing so. He would have differed from Quine in what effects he considered and that is not an entirely trivial difference. He would also have differed from Quine in the very important way touched upon before. For he would not have held that no statement is immune to revision in the face of recalcitrant experience and in this his holism would have avoided an important mistake.

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