AN ANALYSIS OF DOPPELT'S DEFENSE OF KUHNIAN RELATIVISM AS APPLIED TO THE CHEMICAL REVOLUTION

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by

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Abstract.

Doppelt defends the key elements of Kuhn's thesis that scientific revolutions occur when one paradigm is replaced by another and that crucial aspects of competing paradigms are He concedes the merits in the views of incommensurable. those positivist critics of Kuhn who contend that for paradigms to be comparable their proponents must be able to communicate with one another, to agree on a common core of meaning for basic concepts and to deal with shared data and However, he maintains that problems. in identifying the problems which are held to be of fundamental importance and in adopting the standards by which explanatory adequacy is to be evaluated, rival paradigms do not overlap sufficiently for them to have genuine commensurability. This leads accept Kuhn's version of Doppelt to epistemological relativism which maintains that the rationality of the acceptance of new paradigms by the scientific community, at least the short-run, has irreducible in an normative dimension that is strongly conditioned by subjective factors.

Doppelt also accepts Kuhn's views with respect to the loss of data, and the question of cumulative progress. The absence of paradigm-neutral external standards allegedly allows each paradigm to assign priority to its own internal standards, thus providing persuasive grounds for the incommensurability of competing paradigms and for epistemological relativism. Nevertheless, he acknowledges that the validity of these arguments over the long term is a contingent issue which can only be resolved by a careful examination of the historical evidence.

A chemical revolution took place in the latter part of the eighteenth century when the oxygen theory replaced that on hypothetical phlogiston. based This transition is frequently cited as a typical example of a paradigm - one that illustrates Kuhn's claims for a shift in standards and a loss of data as central features of scientific revolutions. The phlogiston theory held that phlogiston was a normal constituent of air. It explained smelting as the transfer of phlogiston from the air (or from phlogiston-rich charcoal) to the earthy components of the ore, and held that the similar properties of the metallic products could be attributed to their phlogiston content. Combustion, including the calcination of metals and the respiration of living organisms, was viewed as a process involving the release of phlogiston to the atmosphere. The development of improved techniques for collecting gases and for measuring their volume and weight lead to emphasis on precise quantitative methods for evaluating chemical data as distinct from those based on simple quantitative descriptive observations.

These developments soon posed difficulties for the phlogiston theory (eg., the anomalous weight loss during combustion). Eventually, clarification of the composition of water and the use of the 'nitrous air' test for the ability of a gas to support combustion and respiration (its

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'goodness') led to the discovery of oxygen as a component of air and the demonstration that combustion involved combination with an exact quantity of this gas. Within a relatively short period of time, the oxygen theory gained general acceptance and the phlogiston theory was abandoned by most chemists.

A critical examination of the events which culminated in the chemical revolution fails to bear out the claim that it was accompanied by a significant loss of empirical data or that it did not represent genuine cumulative progress in scientific knowledge. Instead the history of this revolution indicates that paradigm-neutral external standards for evaluating explanatory adequacy (conservatism, modesty, simplicity, generality, internal and external coherence, refutability, precision, successful predictions) were available and played a crucial role in bringing about this Accumulating evidential warrant played the transition. decisive role in the triumph of the oxygen theory.

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I. Introduction

Gerald Doppelt maintains that Thomas Kuhn's The Structure Of Scientific Revolutions presents a powerful epistemological 'alternative' to the 'positivist' conception important of science by bringing out aspects of the historical development of scientific theory which is not given sufficient attention in positivist accounts. Τn addition, it is his opinion that the arguments of both Dudley Shapere and Israel Scheffler in their defense of various facets of a positivist account of scientific development and their criticisms of Kuhn's relativistic view fails to do justice to the dominant thread of epistemological argument in Kuhn's position which gives it far more plausibility, internal coherence, and systematic significance than is portrayed in the positivist view. However, Doppelt points out that even those who acknowledge Kuhn's contribution to the development of an historical perspective of science have argued that his outlook does not sustain his epistemological relativism and the main arguments for his thesis concerning the 'incommensurability' of rival scientific paradigms. Doppelt's primary objective is to argue that the positivist science is mistaken that conception of and the incommensurability of paradigms is correct. Doppelt states that

The radical thrust of Kuhn's relativism is the denial of the view, shared by positivists, practising

scientists, and layman, that later the or contemporary scientific theories constitute more rational, faithful, comprehensive, and deep accounts of the way the world is than their predecessors. Interrelated to this claim is Kuhn's rejection of the view that the explanatory superiority (on balance) of one paradigm over another relative to a common set of criteria constitutes the decisive reason actually at work in scientists' transition from an established theory to its revolutionary alternative. Kuhn's relativism hinges on his key arguments that competing and historically successive scientific theories are 'incommensurable' with one another: that they are in sufficiently some sense different, disparate, incongruous relative to one another to block the possibility of comparative evaluation on the same scale of criteria.

Furthermore, the incommensurability of rival scientific paradigms is based on the disparity, or incongruity between the following of their elements:

(1) because they do not speak the same scientific language,
(2) because they do not address, acknowledge, or perceive the same observational data,
(3) because they are not concerned to answer the same questions, or resolve the same problems, and
(4) because they do not construe what counts as an adequate, or even legitimate, explanation in the same way.²

However, Doppelt maintains that these elements that constitute the basis for incommensurability are not incompatible or unrelated but they do show some substantial ambiguous aspects and tensions within Kuhn's position for incommensurability.

¹Gerald Doppelt, "Kuhn's Epistemological Relativism: An Interpretation and Defense," In <u>Relativism: Cognitive and</u> <u>Moral</u>, ed. Jack W. Meiland and Michael Krausz, (London: University of Notre Dame Press, 1982), 114. Reprinted from <u>Inquiry</u>. 21 (1979). ²Ibid.

Thus Doppelt argues that we are led by Kuhn to the conclusion that when paradigms are compared, this is done in the

absence of shared scientific concepts, observational data, theoretical problems, and criteria of explanatory adequacy which stand independently of rival paradigms and in whose terms they can be commonly assessed. Without these common desiderata shared by the rival theories that punctuate scientific development, judgments of progress toward the truth, rationally compelling argument between rivals, and the existence of sufficient reasons for transferring theoretical allegiance from one to another also seem to go by the wayside.³

II. Doppelt's Defense of Kuhnian Relativism

For Doppelt there are essentially two questions to be answered with respect to Kuhn's view of the nature of incommensurability between paradigms. The first question pertains to which of the previously stated elements is primarily responsible for explaining and justifying other incommensurability and which is aspects of the most fundamental for relativism in general. The second question is concerned with the degree to which rival paradigms are disparate, as well as to determine just how much discontinuity between paradigms is necessary in order to justify the extent of Kuhn's intended relativism. 4

Doppelt contends that the essential feature of both Scheffler's and Shapere's interpretation of Kuhn is one that treats the incommensurability of rival scientific concepts or languages as the essential feature or ground of Kuhn's relativism. In addition, their interpretation of Kuhn's analysis is that he maintains that there is an absolute and extreme discontinuity between competing paradigms that effectively blocks any logical contact between them.⁵

Furthermore,

According to Scheffler and Shapere's line of interpretation . . . (call it the 'neo-positivist'

⁴Ibid.

interpretation), Kuhn's relativism depends on his key claim that every scientific paradigm is essentially imprisoned within (1) its own unique and untranslatable language, or conceptual framework; and it is for this reason that rival paradigms cannot share, thus do not, share commonly formulatable (2) observational data, (3) theoretical problems, and (4) criteria of explanatory adequacy.

On this neo-positivist interpretation,

Kuhn's relativism hinges on a thorough going conceptual relativism and related holistic doctrine of scientific meaning; according to the relativism so construed, everything a paradigm does - what it sees, the data it recognizes, the questions it poses, the explanations it offers all necessarily presuppose in every instance its own special and untranslatable theoretical concepts. As a result, rival paradigms cannot seek to explain the same observational data or answer the same questions concerning these data. This becomes Kuhn's most basic point of disagreement with a positivist conception of science.

On the other hand, Doppelt points out that a positivist account acknowledges that every new scientific theory may have its own special theoretical concepts and assumptions. However, in spite of this it is asserted by the positivist view that there exists an independent or neutral observational language that provides some essential overlap between paradigms so that there remains a common core of meaning for basic theoretical concepts even when there is a change from one paradigm to another.8 In other words, there is body of language that is paradigm neutral in respect to the specific scientific theories or paradigms being compared, linguistic core provides this adequate and means for

⁶Ibid. 7Ibid.,116. 8Ibid.

comparing the individual merits of competing theories. Thus contrary to Kuhn, "positivism maintains that it is precisely this continuity in scientific discourse which is presupposed in the very possibility of the validation of one theory as against another ([9], pp.47-66)."9

Doppelt acknowledges that given Shapere and Scheffler's interpretation of Kuhn's argument, and their emphasis on the most radical aspects of his position, it is easy to see how their criticism of Kuhn develops. For example, under their reading of his view, rival paradigms lack any access to a language, and thus they cannot be meaningfully common compared. According to Scheffler, Kuhn maintains that incommensurable theories must also be incomparable. And as a result "there can be nothing like genuine communication between rival paradigms, not to speak of rational argument or suasion ([9], pp.16-17)."10

If the disparity between paradigms is so great that they can share no common discourse, then there is no basis for rational debate and one must see the shift of allegiance from one paradigm to another as a process of 'conversion' or a 'leap of faith' where one is some how mystically converted to a new nomenclature, rather than being led to a rational acceptance of a more sound body of beliefs.¹¹ Thus, from a

⁹Ibid., where [9] refers to: Israel Scheffler, Science and Subjectivity, (Indianapolis; Bobbs-Merrill, 1967), 47-66 **i0** Ibid.

¹¹_Ibid.

positivist point of view, in order for Kuhn to explain the transition from one paradigm to another he must invoke "nonirrational factors scientific or such as the age, professional training or past career of the scientist in question."12 Thus, these critics (Scheffler and Shapere) maintain that, "by imprisoning every scientific paradigm in its own world of uncommunicable meanings, Kuhn effectively reduces the logic of scientific development to the psychology and sociology of 'conversion', mystical 'gestalt switches' from one way of 'seeing' the world to another ([9], pp. 18-19, 76-77;[10], pp. 366-8)."¹³

Doppelt recognizes that if we accept the Scheffler-Shapere assessment of Kuhn, then their criticisms against Kuhn's position are valid. For example, it can be argued that

if rival scientific paradigms are as insular, selfenclosed, and imprisoned within their own language as Kuhn maintains, in what sense can they be rivals or If they cannot communicate or argue, how compete? and what can they disagree? If on each is necessarily focussed on its own data and problems, in what sense do they offer incompatible accounts of the same subject-matter or domain? The clear implication is that Kuhn's incommensurability cannot account for facts of theoretical conflict in the evident scientific development ([9], p. 82; [11] p.391).

12 Ibid.

¹³Ibid., 117. where [9] refers to: Scheffler, 1967. <u>Science and Subjectivity</u>, and [10] is, Scheffler, "Vision and Revolution: A Postscript on Kuhn", <u>Philosophy of Science</u>, 39 (1972): 366-74.

39 (1972): 366-74. 14 Ibid. where [9] refers to: Scheffler, Science and Subjectivity, and where [11] is, Dudley Shapere, "The Structure of Scientific Revolutions", The Philosophical Review, 73 (1964): 383-94.

In addition he points out that Kuhn has some difficulty in giving a completely satisfactory or consistent explanation of the role anomalies are supposed to play in the development of science. If one is confronted with an anomaly for a particular paradigm which is an not anomaly for an alternative paradigm, then on Doppelt's view this means that there is a commonly definable observational point of contact between competing paradigms.¹⁵ Doppelt holds that "an 'anomaly' is an observed datum which the established paradigm cannot handle but which the new paradigm resolves in a way that lends it some initial credibility. If rival paradigms can thus speak to the same empirical situation, they must share some common concepts, data, and problems."¹⁶ However, this does not seem possible on Kuhn's account. Thus, Doppelt concludes that "Kuhn is inconsistent and must violate his own relativism in developing a half-way plausible account of scientific development. Indeed Scheffler suggests that Kuhn's anomalies are simply the positivist's falsifying or disconfirming evidence in disguise ([9], p.89)."¹⁷ Thus, the basis for Scheffler and Shapere's criticism of Kuhn focuses on "the holistic conception of scientific meaning, which, on their interpretation, is the indispensable pillar upon which Kuhn's entire incommensurability argument rests."¹⁸

- 15_{Ibid}.
- 17 Thid
- 18 Ibid.

Doppelt disputes "this position - despite the fact that it does capture some strains in Kuhn's complicated argument"¹⁹. He admits that there is much in Kuhn that supports the Shapere-Scheffler interpretation. However, Doppelt dispenses with Scheffler and Shapere's criticisms of Kuhn by offering a distinctly different interpretation and emphasis, rather than directly attacking the validity of their analysis. Doppelt proceeds to develop and defend what he believes to be the strongest aspect of Kuhn's epistemological relativism. He maintains that according to interpretation of Kuhn's relativism, "it is his the incommensurability of scientific problems between rival paradigms and not that of meanings which constitutes the most basic premise of the argument."²⁰ Thus, Doppelt argues "that the incommensurability of scientific problems provides the central basis for explicating and justifying the relativism argument as a whole."²¹ It will be my intention to deal with Doppelt's interpretation of Kuhn's position as it stands, rather than argue which interpretation is the correct one. However, I will attempt to evaluate Doppelt's interpretation of Kuhn's relativism and to argue against it as an accurate construal of the way in which scientific theories actually For this purpose, a more detailed examination of evolve. Doppelt's defense of Kuhn's position will be necessary.

^{19&}lt;sub>1bid., 115.</sub>

²⁰ Ibid., 118. 21 Ibid., 115.

On Doppelt's interpretation paradigms will still be incommensurable although they can at the same time have a large degree of overlap between language, problems, observational data and even some of the standards that guide scientific research as well as the standards by which we evaluate the merits of rival paradigms and theories. By providing these points of contact between rival paradigms Doppelt hopes to give a somewhat more plausible account of historical change from one paradigm to another and still leave some room for an explication of the rational debate that is responsible for the decision to change paradigms and the justification which supports such changes.

Thus, unlike Scheffler's and Shapere's interpretation of Kuhn, paradigms can be seen as not entirely imprisoned within their own conceptual schemes. Moreover, on Doppelt's interpretation of Kuhn there is no absolute epistemological break between paradigms, in as much as they can share important common features. Nevertheless, for Doppelt "there is insufficient overlap in the problems and standards of rival paradiqms rank them the to on same scale of criteria."22 Doppelt claims that the choice to embrace a new paradigm is not 'irrational' and that there also is some sense in which scientific progress takes place. On the other hand, he asserts "that the balance of reasons or the demands of scientific rationality never unequivocally favor one paradigm (either the old or the new) over its rival; and

²²Ibid., 118.

secondly, that in consequence, contemporary paradigms do not represent progress over what they replace in the sense of progress toward the truth concerning nature."²³

Doppelt quotes Kuhn to show the importance Kuhn assigns to particular problems which are considered to be the most crucial or basic and to differences in the standards used to evaluate explanatory adequacy as the major factors responsible for the incommensurability of rival paradigms.

But paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them. . . . As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science, or declared entirely 'unscientific.' Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant achievement. And as the problems change, so often, does the standard that distinguishes а real scientific solution from a mere metaphy speculation, word game, or mathematical play. metaphysical The normal-scientific tradition that emerges from а scientific revolution is not only incompatible but often actually incommensurable with that which had gone before. ([1], p.103)

By shifting emphasis from the cognitive to the normative function of paradigms, the preceding examples enlarge our understanding of the ways in which paradigms give form to the scientific life . . . when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solution.. .

That observation returns to the point from which this section began. . . To the extent . . . that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms. In the partially circular arguments that regularly result, each paradigm will be shown to satisfy more or less

the criteria it dictates for itself and to fall short of those dictated by its opponent . . . since no paradigm ever solves all the problems it defines and since no two paradigms leave all the same problems unsolved, paradigm debates always involve the question: which problems is it more significant to have solved? ([1], pp. 109-10)²⁴

Thus, an important aspect of Doppelt's interpretation, is his view that the "most revolutionary dimension of a new paradigm . . . is the fact that the new paradigm implies a shift of commitment to a new set of theoretical problems as the 'core' of the discipline - substantively different from the problematic which defined the hard core of science under the old paradigm."25 He claims that even "though rival paradigms share some of the same problems, they do not weigh their importance in the same way, assigning them different orders of significance and priority in the achievement of will count as the success of a paradigm, what or alternatively, a tolerable level of failure."26

Doppelt concludes that, "the primary claim advanced by incommensurability in Kuhn is that the standards of adequacy each paradigm implicitly sets for itself are sufficiently disparate from one to the next to block any uniform basis for a judgment that one is, on balance, more reasonable to accept than its rival."²⁷ Hence, the main point of emphasis on this interpretation is that incommensurability is the result of the fact that these "incompatible standards, are generated

24 Ibid.,119-120. where [1] is Thomas S. Kuhn, The Structure Of Scientific Revolutions 2nd ed., (Chicago: University of Chicago Press, 1970). 25 Ibid., 120. 26 Ibid. 27 Ibid. from each paradigm's tendency to disagree as to what counts as the fundamental problems any paradigm in the field ought to solve."²⁸ Thus stress is placed on the normative rather than the cognitive aspect of this issue.

In emphasizing this central point of his construal, Doppelt admits that during periods of paradigm debate there is often a 'communication breakdown', so that combatants talk at 'cross-purposes'. He contends that this breakdown is not caused by the lack of a common language, but takes place because scientists "lack a sufficiently common definition of the discipline and its criteria of explanatory adequacy to allow their discourse to terminate in rational consensus even concerning the relative merits and defects of their paradigms, apart from the key issue of which is superior."29 He makes the further claim that "conflict between scientific theories becomes much more like conflicts in ethical and political life than the absolute distinction between scientific and normative discourse advanced by classical positivism allows."30 Doppelt draws the conclusion that like ethics, irreducible science, has an normative dimension. Both "embody incompatible answers to the question of which aims, values, and problems ought to dominate and define a certain domain of activity."31

²⁸ Harvey Siegel, "Epistemological Relativism in its Latest Form," <u>Inquiry</u>. 23, (1980): 107. 29 Doppelt, "Kuhn's Epistemological Relativism," 120. 30 Ibid. 31 Ibid.

Crucial to Doppelt's reading of Kuhn's is the incommensurability thesis extent to which each paradigm incorporates its own distinctive standards of explanatory adequacy. For example, even if paradigms share only partially overlapping problems, it would still be possible for them to be commensurable if they share the same standards of explanatory adequacy. This is because if paradigms have standards in common, each would identify the same set of 'core' problems that the shared standard requires to be solved. However, Doppelt denies that paradigms share the same standards. The main point being stressed by Doppelt is that incommensurability between paradigms is not due to the fact that different paradigms merely identify different problems, even if they are the most basic set of core problems, or even if these problems are given different These differences could reflect only pragmatic priorities. considerations such as what is viewed as the best strategy for further research etc. What is important, is that

for Kuhn, these differences gain epistemological significance because they are built into the very standards of theoretical adequacy, the defining aims of the science, in terms of which each paradigm evaluates itself and its rivals. . . The kind of problems whose solutions define the standards of good theory for any given paradigm are generally resolved to a greater or less degree by that paradigm, but either unresolved, unrecognized or consigned to a minor theoretical importance by its rival(s). Each paradigm implicitly defines standards of scientific adequacy favoring its achievements and research program and unfavorable with respect to the work of its rivals.³²

³²Ibid., 121.

Another, substantial point to be considered is that observational data may also be incommensurable. This is because rival paradigms address different problems. consequently they seek to explain different observational data, and the "capacity of each paradigm to explain the range of data which its problems define as of key importance generates the major type of criterion of explanatory adequacy Kuhn has in mind."³³ Furthermore, rival "paradigms can share this much and nonetheless exhibit fundamental disagreements irresolvable by scientific argument concerning the set of problems and data that any adequate theory must treat (only some of which they share); and the order or priority among these problems in determining what is to count as scientific success, or a tolerable level of failure (the minimal achievement presupposed by the continuing plausibility of a theory)."³⁴ But Doppelt's reconstruction hinges on the "incommensurability of competing standards of adequacy of rival paradigms. . . . Such incommensurability, at least prima facie, depends on the absence of paradigm-neutral external standards of adequacy by which a paradigm's internal standards can be non-relativistically evaluated."35

Thus, on Doppelt's interpretation of Kuhn's views there are basic non-cumulative differences between successive paradigms which include both problems and observational data and these in turn contribute to differences in their

- 33_{Ibid}.
- 34 Ibid., 125.

³⁵Siegel, "Latest Form," 110.

standards of explanatory adequacy. In short, "rival paradigms are incommensurable because they imply different criteria of explanatory adequacy, the major criteria of each being how well it answers its own distinctive questions and explains its own privileged range of data."³⁶

Doppelt also claims that anomalies not are inconsistent with other aspects of this interpretation, as they are in the 'holistic' interpretation given by Scheffler and Shapere. He contends that, "Kuhn makes it clear that in scientific revolution, a new paradigm only prevails if (1) it resolves data and problems ('anomalies') which have come to important but irresolvable regarded as on the old be paradigm, and (2) it also effectively deals with some of the old paradigm's other problems (and data) as well as posing and resolving wholly new problems."37

Doppelt acknowledges that there must be enough common subject matter between rival paradigms for there to be actual conflict or disagreement and meaningful debate. On this construal it is possible for there to be a fair amount of overlap between observational data and problems, providing sufficient continuity between competing paradigms to allow for some rational debate. However, Doppelt argues that there is not enough overlap for commensurability and this view is thus consistent with relativism.

³⁶Doppelt, "Kuhn's Epistemological Relativism," 122.
³⁷Ibid. 125.

Doppelt maintains that an adequate understanding of Kuhn requires that we make a distinction between 'short-run' and 'long-run' relativism. He identifies two criteria by which to judge relativism when comparing different scientific theories, "(a) the 'loss-of-data' argument and (b) the 'shifts-in-standards' argument."38 Doppelt argues that Kuhn's strongest "challenge to the positivist view of progress in scientific knowledge turns on the claim that due to losses in observational explicanda in scientific development, it does not satisfy the positivist criterion of progress - increasing and cumulative empirical adequacy."³⁹ Relativism implies that inasmuch as each paradigm in the final analysis can only be evaluated by its own internal criteria of explanatory adequacy, there is no basis for judging one to be superior to another, so that no case can be made for progress.

Doppelt argues that Kuhn is correct in his view that a shift from one paradigmatic theory to another will often result in a loss of observational data as well as the abandonment of problems that were addressed in the replaced theory. However, Doppelt parts company with Kuhn on the question of whether or not in the long run, theories may 'recoup' the observational data they had previously (or 'temporarily') lost through a paradigm shift. He points out that there "is nothing in Kuhn's argumentation or examples

^{38&}lt;sub>Ibid., 127.</sub> 39_{Ibid.}

which would establish that future science in principle cannot explain all of the genuine observational explicanda of other historical theories; at best he offers an inductive argument against the likelihood of this prospect relative to the losses in data characteristic of scientific development up to the present."⁴⁰

Doppelt, points out that the most a loss-of-data thesis could plausibly establish is that at a certain point in its development a new theory "exhibits losses with respect to the genuine observational data and problems explained by . . . its predecessors."41 Thus, for Doppelt, "Kuhn's longrun relativism argument is reduced to an interesting shortrun relativism issue, that depends upon whether or not a new theory is "cumulative with respect to the observational predecessors."42 explicanda of its Doppelt concludes that there is no philosophical argument that can rule out the possibility "that 'in the long run' science will recoup all of its 'temporary' losses in observational explicanda and thus achieve cumulative progress."43 Thus, the 'loss-ofdata' question is a contingent proposition, which must be weighed against the historical record of scientific development.

In spite of his admission that it is at least theoretically possible for science to be cumulative,

⁴⁰ Ibid., 128.

⁴¹Ibid.

⁴² Ibid.

⁴³ Ibid.

Doppelt insists that Kuhn's 'loss-of-data' thesis still poses an important 'challenge' for the positivist. For both Doppelt and Kuhn, the positivist view of "scientific progress as an increasing and cumulative body of knowledge is not merely 'the' regulative standard of science which it 'can' fulfill but is in fact the standard actually fulfilled by contemporary physical theory."44 Doppelt has asserted, that the "very notion that scientific life allows progress seems to presuppose some significant dimension of continuity in its standards, however problems, concepts, and much thev otherwise change."45 Furthermore, he points out that any criterion of progress that "fails to incorporate this necessary dimension of continuity . . . is inadequate."46 Nevertheless, he expresses the view that "positivists insistence on total cumulativity . . . as a condition of progress is implausible."47

Doppelt concludes his treatment of the loss-of-data question, by pointing out that if it is true that there is a short-run loss-of-data - if a present theory does not explain all of the genuine observational explicanda of all of its predecessors then this will be sufficient to "unsettle the positivist assumption that scientific progress is unambiguously actualized in contemporary physical theory.

⁴⁴ Ibid.

⁴⁵ Gerald Doppelt, "Laudan's Pragmatic Alternative to Positivist and Historicist Theories of Science," <u>Inquiry</u>, 24, (1981): 269. 46 Ibid. 47 Tbid.

Defenders of a positivist account will want to reply to this Kuhnian challenge."⁴⁸

But Doppelt also asserts that even if a theory deals with all of the observational data that its predecessor was able to accommodate, this is not in itself an adequate basis that which to conclude the two theories from are commensurable. This is because, even if there is no loss of data from one theory to another "rival paradigms may still exhibit incompatible criteria of theoretical adequacy. e.q. concerning the non-observational problems to be solved or concerning what counts as a sufficiently 'simple' or 'accurate' explanation of (shared) observational data."49 Furthermore, he states that "rival paradigms sometimes maintain incommensurable standards because these standards incompatible trade-offs implicitly justify between 'simplicity', 'accuracy', breadth of observational explicanda', etc ([2], pp. 199; [4], p.262)."⁵⁰ In short, the relativist argument essentially "denies the existence or relevance of 'external' standards of scientific evaluation, and requires that theories be evaluated by their own internal standards."51

⁴⁸ Doppelt, "Kuhn's Epistemological Relativism," 129. 49 Ibid., 130.

⁵⁰ Ibid. where [2] is Kuhn, 'Postscript (1969) to Kuhn <u>Structures</u>, 174-210. and [4] is Kuhn, 'Reflections on my Critics', In <u>Criticism and the Growth of Knowledge</u>, ed. I. Lakatos and A. Musgrave (Cambridge: Cambridge University Press, 1970): 231-79. 51 Ibid.

Consequently, Doppelt supports Kuhn's view that a shift in the standards of evaluation from one paradigm to another challenges the positivist conception of science, where the criteria of scientific progress are seen as being better satisfied, and accompanied by cumulative empirical adequacy. On the relativist conception, scientific evaluation depends on standards that are *"internal* and specific to particular physical theories in the history of science", 52 and as Doppelt points out, Kuhn's position concerning the shift of standards supports a relativist criterion of scientific knowledge. Doppelt agrees with Kuhn that there exists some shift in standards from one paradigm to another. However, he somewhat more reluctant than Kuhn to seems accept the conclusion that knowledge is relative. Doppelt feels that 'positivist' and 'relativist' positions the regarding scientific knowledge are not the only ones worthy of consideration, but he does not present any adequate solution, other than to maintain that there are other alternatives to these views of the nature of scientific knowledge.

The shift of standards argument maintains "(1) that any physical theory can only be evaluated relative to its own standards of adequacy, and (2) that in fact successive physical theories in the development of science embody different, indeed incompatible standards of scientific valuation."53 According to this view, "every historical

52_{Ibid}. 53_{Ibid}.

system of scientific theory turns out to satisfy its own standards of knowledge more adequately than rival or alternate systems of theory."⁵⁴ Moreover, each theory is "thus 'best' in its own terms, and there are no other terms by which theories can be evaluated (hence, relativism)."⁵⁵ This view has been elaborated as follows:

Doppelt's reconstruction of Kuhn allows for a fair amount of 'logical contact' between rival paradigms. Nevertheless, such rivals are, on Doppelt's account, incommensurable in that they embody incompatible problems attitudes toward the fundamental any paradigm in the field ought to try and solve. Since paradigms disagree as to what the fundamental questions are, they disagree as to the proper standards of explanatory adequacy by which any paradigm in the field must be assessed, because each paradigm's standards will be a function of the set of problems each paradigm recognizes as fundamental to discipline. Epistemological relativism, the on Doppelt's account, results from the incommensurability of standards of explanatory adequacy of rival paradigms. Since paradigms are this respect, in incommensurable а paradigm's evaluation is relative to the standards of adequacy of the paradigm from which one is evaluating. Α paradigm will be assessed variously according to how well it meets the standards of adequacy of various paradigms -thus P₁ will be (typically) superior to P_2 , relative to the standards of adequacy of P_1 , while P_2 will be superior to P_1 , relative to the standards of adequacy of P_2 . Since P_1 and P_2 are incommensurable, they do not share common criteria of adequacy (though they may well share certain items of observational data, problems, and concepts); and since their criteria of adequacy are incompatible (if not, P_1 and P_2 would not be incommensurable), their assessment is relative to the paradigm-bound criteria adequacy of appealed to in making such an assessment.

However, as already mentioned earlier, Doppelt seems somewhat reluctant to accept a relativistic view of knowledge

54 55 Ibid. 56 Siegel, "Latest Form," 109-110.

which would seem to be inevitable if we accept a 'long-run' shift in scientific standards from paradigm to paradigm. If we are to have a conception of scientific knowledge in which we can have some sense of 'progress' from one tradition to another, it appears necessary that we allow for the possibility of 'cumulative' development.

One important aspect of Doppelt's argument is his contention that Kuhn has provided a persuasive relativist challenge to the positivist's conception of scientific rationality, at least in the 'short run'. The positivist contends that a scientific revolution is characterized by a gradual shift on the part of the bulk of the scientific community from one paradigm to another on the basis of shared criteria of evidential warrant that are sufficient to make a particular change of allegiance compelling and therefore rational. Positivists argue that, there are 'objective' paradigm-neutral standards which are used to justify the rational decisions a scientific community actually makes, so that scientific progress can be cumulative with respect to genuine scientific data.

Doppelt concedes that the evolution of theories in a particular scientific domain such as physics and chemistry can be analyzed or constructed so as to show that in the 'long-run' successive paradigms do fulfill these positivist assumptions. Nevertheless, he maintains that in order to substantiate the positivist's conception of scientific rationality, progress and development, it is still necessary

to demonstrate historically that these external standards actually were the one's responsible for a particular shift in paradigms (resulting in a scientific revolution). Doppelt admits that it might be possible to demonstrate that in the 'long-run' the ultimate evaluation of completed theories may recoup any temporary loss-of-data, or that successful new theories eventually can be shown to meet the positivist criterion of 'objective' and external paradigm-neutral standards. Nevertheless, in his view short-run relativism still represents a serious challenge to the positivist's conception of scientific rationality and development.

Doppelt acknowledges that a non-relativist conception of scientific knowledge can be defended, and that Kuhn's arguments for this position are not always consistent even with his own examples. Nevertheless, Doppelt maintains that the positivist needs to establish by the historical evidence that in the short-run rational argument was compelling or decisive in justifying the validity of the actual decisions made by most members of the relevant scientific community to switch to a new paradigm. And most importantly, Doppelt maintains that both those who retain their allegiance to the old paradigm and those who opt for a shift to a new paradigm are rational in their positions. This is because there is not enough overlap in shared standards to make one decision more compelling than another, even though partial overlap of problems and standards can permit some debate between competing paradigms. In other words, during a scientific

revolution rival paradigms are claimed to be evaluated primarily in terms of their own internal standards of evaluation.

Furthermore, Doppelt argues that the rationality of the 'conversion' that leads scientists to choose one paradigm over another is neither compelling from the evidence alone, the result of a more adequate explanation of the data. nor He concludes that sociological and psychological factors are responsible for the actual decisions mainlv made by scientists during a revolutionary period. Thus, it is asserted that these factors must be incorporated into any historically accurate understanding of the decision-making in order to do justice to an epistemologically process adequate conception of rationality as it actually operates during scientific revolutions.

Moreover, Doppelt asserts that even if one acknowledges that it is at least possible, either at present or at some future time, to show that successive theories do meet the criteria that positivist's а account of scientific justification requires (such that later theories are shown to provide a more adequate account of the data by virtue of their being simpler, having greater predictive success, more general in their applicability etc), this still is not an adequate response to the relativist's 'short-run' challenge to rationality. Doppelt argues, that these criteria either were not always present, or were at least not decisive when a particular scientific community's choice was made to abandon one paradigm and accept another, such as the choice to transfer allegiance from phlogiston to oxygen chemistry.

it is In summary, Doppelt's belief that during scientific revolutions paradigms are evaluated mainly in terms of their own contemporary internal standards, and that we cannot use the later standards of a subsequent theory to evaluate those which preceded it. He holds that during a scientific revolution external standards either do not exist or are not relevant to the short-term evaluation of rival paradigms. As a result, it is claimed that the positivist's attempt to analyze scientific rationality in terms of these external standards is a distortion or 'misrepresentation' of the actual history of science and the rational process as it actually works. This contrasts with the positivist's contention that the essential nature of scientific rationality requires paradigm-neutral standards for the evaluation of scientific theories. For the positivist, a decision can be considered to be rational when it 'more' adequately satisfies the criteria embedded in these external standards than its rivals do. It is this thesis which Doppelt denies as being a satisfactory account of the actual scientific decision-making process. And finally Doppelt maintains that if positivist standards are not satisfied in all scientific revolutions, then this presents an important challenge to the positivist's conception of science and scientific rationality.

Of course there is some question as to just how long a period of time should be involved in determining exactly what is to count as 'short-term' or 'long-term' in Doppelt's view, a problem which might complicate an adequate response. However, if a rather short time period is chosen in which to show that a new paradigm was justified in relation to such external standards, we can take it to suffice that this is adequate grounds to make a case for the positivist view. Doppelt denies neither the possibility of there being paradigm-neutral standards nor that, at present or in the 'long-run', scientific theories can be historically 'reconstructed' in attempt to an show that theories eventually recoup their 'short-term' loss of data and recover their explanatory adequacy. Thus it is possible, at least in theory, to demonstrate that the standards of former paradigms can be incorporated into present or eventually future paradigms. In other words, a particular paradigm may in the long-run meet the standards of past paradigms as well as those of its own. However, Doppelt, admits that an argument based on the short- run is a weakened form of the relativist Consequently he argues that the standards of doctrine. competing paradigms "are 'incompatible' in a weak sense, but not in a strong sense which would rule out 'cumulative' progress."⁵⁷ Although the adherents of rival paradigms may make different judgments as to which is the better theory, in Doppelt's view this does not need to imply that the standards

57 Ibid. 132.

of one theory violate the standards of another theory. Doppelt seems to interpret this in an additive sense where the old standards of the predecessor theory can be incorporated into or subsumed by those of the new theory. For Doppelt, this 'weakened' sense of the incompatibility of successive theories does not rule out the possibility of cumulative progress, in contrast to 'strong' incompatibility, where rival paradigms violate each other's standards, and as a result both sets of standards cannot be fulfilled.

However, in spite of Doppelt's concessions to the possible existence of paradigm-neutral standards, he remains sceptical as to the actual existence of such standards. Furthermore, even if the positivist could show that there are such standards, he remains doubtful as to whether they are actually used in scientific practice. Therefore, at least in the short-run, he holds that they cannot be used to provide an adequate account of scientific rationality, or to allow cumulative progress in scientific knowledge. In summary, it is Doppelt's position that even if it is granted that paradigm-neutral 'external' standards may exist, he doubts that they are actually used in any decisive epistemological way in regard to the major issues in the philosophy of science; scientific rationality and the process by which a change to a new paradigm is justified. Doppelt acknowledges that whether or not scientific theories in fact exhibit incompatible standards of adequacy in the long-run is a contingent issue.
Still Doppelt specifies, that his most fundamental criticism of Kuhn's long-run relativism concerning scientific knowledge is that "Kuhn does not develop any independent discussion of the philosophical nature of scientific knowledge."58 In spite of this Doppelt suggests that one alternative to relativism is that Kuhn's long-run relativism arguments "can be made compatible with the existence of progress in science, if we simply adopt as its criterion 'maximal problem solving ability' (which does not require 'cumulative' problems or data)."59 According to Doppelt, adopts 'maximal problem solving ability' as Kuhn а "criterion to formulate the sense in which he is 'a convinced believer in scientific progress ([2] p.206)."60

This possibility has been explored by Laudan who presents a view of science in which "the rationality and progressiveness of a theory are most closely linked - not with its confirmation or its falsification - but rather with problem solving effectiveness."⁶¹ He argues that there are "important non-empirical, even 'non-scientific' (in the usual sense), factors which have and which should have played a role in the rational development of science."⁶² Furthermore, he urges that we should "drop some of the traditional

58_{Ibid}.

⁵⁹Ibid., 133.

⁶⁰ Ibid.

 ⁶¹Larry Laudan, <u>Progress and Its Problems: Toward a</u>
 <u>Theory of Scientific Growth</u>(Berkeley: University of California Press, 1977): 5.
 ⁶²Ibid.

language and concepts (degree of confirmation, explanatory content, corroboration and the like), and if see а potentially more adequate model of scientific rationality begins to emerge. Let us see whether, by asking anew some of the elementary questions about science, we cannot get a slightly different perspective on scientific knowledge."63 Laudan then goes on to argue that science fundamentally aims at the solution of problems.

He also maintains that most philosophers of science have mistakenly identified the nature of scientific appraisal by focussing on the individual theory rather than on the research tradition. Moreover, he argues that we need to distinguish between "the rationality of acceptance and the rationality of pursuit if we are to make any progress at reconstructing the cognitive dimensions of scientific activity."64

Laudan maintains that the evaluation of problem-solving effectiveness is at least partly dependent upon a 'world view' that is held at a particular time. It is in this way that he hopes to do justice to the historical record. Unlike positivist conceptions that tend to force history to fit a pre-established model of rationality, he attempts to provide a means by which we can judge the rationality of various research traditions without imposing contemporary standards of rationality or problem selection and their solutions upon

^{63&}lt;sub>Ibid.,</sub> 4. 64_{Ibid.,} 5.

past world views. He points out that unencumbered "by modern notions of rationality, scientists of the past had to make decisions about the acceptability of contemporary theories by their criteria rather than by ours."⁶⁵ Laudan maintains that if is to explain why certain theories "the historian triumphed and perished, then he must (unless he takes the view that theory choice is always irrational) be able to show by the best available rational that some theories standards of the time - were superior to others."66 However, in spite of time and cultural 'parameters' of rationality there are some general features for assessing rationality within a particular epoch, such as "that for all times and all cultures, provided those cultures have a tradition of critical discussion (without which no culture lay claim to rationality), rationality consists can in accepting those research traditions which are the most effective problems solvers."67 However, at the same time recognizes that Laudan to "ignore the time-specific parameters of rational choice is to put the historian or philosopher in the outrageous position of indicting as irrational some of the major achievements in the history of ideas."68 He maintains that on his 'model'

what is specifically rational in the past is partly a function of time and place and the context. The things which count as empirical problems, the sorts of objections that are recognized as conceptual

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^{65&}lt;sub>1bid., 129.</sub> 66_{1bid., 130.} 67_{1bid.}

⁶⁸ Ibid. 131.

problems, the criteria of intelligibility, the standards for experimental control, the importance or weight assigned to problems, are all a function of the methodological-normative beliefs of a particular community of thinkers. . . . Aristotle was not being irrational when he claimed, in the fourth century B.C., that the science of physics should be subordinate to, and legitimated by, metaphysics even if that same doctrine, at other times and places, might well be characterized as irrational. Thomas Aquinas or Robert Grosseteste was not merely stupid or prejudiced when they espoused the belief that science must be compatible with religious beliefs.

Laudan rightly concludes that in the twentieth century we believe in the autonomy of scientific beliefs from extrasocietal beliefs, but he asserts that this view is of recent origin. However, he points out that the autonomy of science from other beliefs "does not necessarily entail that it was rational at other times and places."⁷⁰ Furthermore, he asserts that

in arguing that the cultural exigencies and pressures exerted on science must be taken into account, I am abandoning the possibility of rational nor am I insisting that nonscientific neither appraisal factors are present in every case of scientific I am simply suggesting that we need a choice. broadened notion of rationality which will show how the 'intrusion' of seemingly 'non-scientific' factors into scientific decision making is, or can be, an entirely rational process. Far from viewing the introduction of philosophical, religious and moral issues into science as the triumph of prejudice, superstition and irrationality, this model claims that the presence of such elements may be entirely rational: further, that the suppression of such 71 elements may itself be irrational and prejudicial.

Laudan goes on to write that "whether it is rational to use theological, moral, or philosophical arguments for (or

⁶⁹ Ibid., 130-131.
70 Ibid., 131-132.
71 Ibid., 132.

against) a new scientific theory or research tradition is a contingent matter which depends on how rational and progressive are the research traditions which provide such arguments."⁷² Thus, the

rationality or irrationality of any episode where 'nonscientific,' but intellectual, factors play a role must be assessed on a case-by-case basis, but the quiding principles here should be these: (1) in the case of competing research traditions, if one of traditions is compatible with those the most progressive 'worldview' available, and the other is not, then there are strong grounds for preferring the former; (2) if both traditions can be legitimated with reference to the same worldview, the rational decision between them may be made on entirely 'scientific 'grounds; (3) if neither tradition is compatible with a progressive worldview, their proponents should articulate a new, progressive worldview which does justify them, or develop a new research tradition which can be made compatible with the most progressive extant worldview.⁷³

Doppelt, is somewhat critical of Laudan's position. He holds that Laudan's intention of providing a nonrelativist account of scientific progress, as it now stands, does not succeed. Doppelt's main criticism of Laudan, is that even if we accept his philosophical arguments and scientific illustrations and agree that they "persuasively show 'that scientific debate is rational so long as it involves a discussion of the empirical and conceptual problems which theories and research traditions generate' ([1], p.124), Laudan fails to establish, or even make plausible, the central claim upon which avoidance of relativism depends."⁷⁴

- 72_{Ibid}.
- 73 Ibid.

⁷⁴Doppelt, "Laudan's Pragmatic Alternative", p.266., and where [1] refers to Laudan <u>Progress and Its Problems</u>.

Thus, Doppelt contends that Laudan's argument retains a strong dimension of relativism, even though he does think it might have the potential to be developed into an acceptable account of the progress of science. However, Doppelt points out that Laudan "has attempted to articulate . . . а paradigm-neutral standard of scientific rationality (problemsolving effectiveness') and demonstrates that it is operative in the historical development of science."75

Doppelt's view the possibility does exist "that In 'problems solving effectiveness', if not a plausible nonrelativist criterion of scientific rationality, may still provide a plausible criterion of scientific truth and progress."⁷⁶ But Doppelt insists that we require "some independent argument for the existence of external standards in science, or a theory of rational debate which entails their existence"77 which he finds lacking. Doppelt concludes that "in order for there to be such rationally compelling reasons, there would have to be paradigm-neutral external standards . . . nothing in the actuality or possibility of rational debate concerning rival paradigms and their rival standards implies or suggest the existence of . 'compelling' reasons, external standards, or the denial of a forceful Kuhnian relativism."78

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⁷⁵Doppelt, "Reply to Siegel, 121. **76**Doppelt, "Laudan's Pragmatic Al "Laudan's Pragmatic Alternative," 269.

⁷⁷ Doppelt, "Reply to Siegel, 121.

^{78&}lt;sub>Ibid</sub>.

Thus, Doppelt doubts Laudan's claim that standing above radical historical transformations the there is some neutral, external criterion of problems-solving effectiveness which is "both (1) implicit (if not explicit) in the main scientific debates and choices responsible for paradigm cases of 'progressive' shifts in research traditions, and/or (2) actually, objectively satisfied by these 'progressive shifts."79 However, Doppelt asserts that Laudan has not adequately illustrated 'his own criterion' or shown that it is at work in particular cases. He contends that, "for all we know they may well have involved Kuhnian-type irreducible normative shifts in the very criteria of 'problem-solving effectiveness' and not Laudan-type 'progress' from less to more effective tools of problem solving."80 And most importantly,

it is difficult even to know how his criterion is supposed to apply: How do (or did) scientists from admittedly opposing research traditions even imprecisely weight quantitative as against qualitative considerations (e.g. the number as against the importance of solved problems), empirical as against conceptual problems, internal as against external conceptual problems, external methodological problems as against world-view problems, etc. in supposedly arriving at some final. shared, rational over-all ranking research traditions?⁸¹ of different

For example, Doppelt maintains that, in fact, Laudan's conception of 'world-views' is relativistic. In the first place, Laudan's conception of world-views is seen as

80 Ibid.

⁷⁹ Doppelt, "Laudan's Pragmatic Alternative", 266.

^{81&}lt;sub>Ibid</sub>.

'ambiguous' because it is not clear "under what condition a world-view (or extra-scientific system of beliefs) can provide the basis for allowing or disallowing extrascientific arguments for and against scientific theories."⁸² For example, Doppelt maintains that on Laudan's view it "is a matter of (1)how well-entrenched the world-view is, and (2) of how 'progressive' the world-view is, now considered as itself a (nonscientific) tradition for problem-solving. The criterion of 'entrenchment is itself ambiguous."⁸³

Furthermore, Doppelt raises the crucial question of "how can historically disparate physical theories (e.g. Aristotle and Newton) be rationally compared if each is evaluated relative to the incompatible but equally well-entrenched world-views of their respective societies, own or intellectual communities?"84 Laudan maintains that "world views themselves are more or less 'progressive' and 'rational' depending upon their own respective 'problemsolving capacities."85 Thus according to Doppelt, Laudan's contention "poses the awesome if intriguing question of what is involved in representing (to cite Laudan's examples) 'the Greek myths' and 'Christian morality' as 'non-progressive' traditions, and the modern 'autonomy of science' tradition as one which has 'generated a considerable degree of progress'

82 83 1bid. 84 Thid

85-101a.

^{**b** J}Ibid.

([1], pp.131-132)."⁸⁶ This is an important point because, the "Greek myths and Christian morality solved a whole host of theoretical and practical problems which do not even exist for the scientific world-view of modern society."87 And even more importantly, a problem arises "because these disparate world-views differ so radically on what counts as an important or manageable problem and what counts as a proper solution, it is difficult to imagine what Laudan could possibly mean by asserting that one is more progressive and rational in terms of its 'problems-solving capacities' than another."88 Here again Doppelt is quick to point out that "the specter of relativism looms large."89 For, example, does "Laudan propose that by its own standards Christianity solves less of its important problems than is the case with the scientific world view?"90 Thus, given the fact that "problem-solving effectiveness of scientific theories depends in part on that of world-views, the clarity and nonrelativist character of Laudan's theory comes into jeopardy."91

In his discussion of problem-solving effectiveness as a potentially acceptable external standard which might be responsible for cumulative scientific progress, Doppelt points out that Kuhn "refuses to take such a criterion as ⁸⁶Ibid., 266-267 where [1] refers to Laudan, <u>Progress</u> and Its Problems. ⁸⁷Ibid., 267. ⁸⁸Ibid. ⁹⁰Ibid. ⁹⁰Ibid. ⁹¹Ibid.

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'the' criterion of scientific knowledge and truth."92 Tt appears that Doppelt's reading of Kuhn allows for scientific progress by virtue of 'maximal problem-solving ability', but at the same time Kuhn seems to be rejecting the notion that maximal problem-solving ability is 'cumulative'. This is because Kuhn argues that "every theory seeks to maximize its capacity to resolve its own problems - those it takes to define the discipline and to be especially revealing of the way the world is."93 Doppelt concludes that Kuhn finds no theory "willing to evaluate itself and its rival according to a criterion of problem-solving ability which abstract from the identification of the problems at issue."94 In addition, different theories do not necessarily agree on a "shared way of individuating and counting problems, of ranking their relative importance, or even of judging the relevant measure in solutions."95 of 'accuracy' And furthermore, because there is no universal concept of problem-solving we can not conclude that maximal problem-solving ability incorporates any cumulative principle from one paradigm to another.

In spite of this, on Doppelt's understanding of Kuhn's position, 'scientific knowledge' would require some kind of 'cumulative' progress as a consequence of a paradigm shift. For example, Doppelt points out that Kuhn has a "firm intuition that the progress of science as knowledge of the

⁹² Doppelt, "Kuhn's Epistemological Relativism," 133.

⁹³ Ibid.

^{94&}lt;sub>Ibid</sub>.

^{95&}lt;sub>Ibid</sub>.

world presupposes some essentially 'cumulative' dimension at the level of its content."⁹⁶ Doppelt concludes that Kuhn seems to assume "some common world which can be known in a 'cumulatively' adequate way."⁹⁷ However, Doppelt adds the caveat that "even if it can be shown that on some 'neutral' concept of 'more' one physical theory solves more of 'its' problems than its historical predecessors solved of 'theirs', this formulation already smacks of a relativism concerning scientific knowledge."⁹⁸

Finally, Doppelt concludes that a satisfactory account scientific knowledge of will require а 'cumulative assumption'. He states that it might "be possible to elaborate a theory of scientific knowledge that adheres to this assumption while rejecting its standard philosophical interpretation in positivism and Kuhn"99 but Doppelt offers little in showing how this is possible, other than to express the hope that in the long-run there would actually occur some kind of cumulative progress, be it with respect to problems, observational data or shared standards, or all of these.

Kuhn believes that scientific progress takes place, but denies the positivist conception of scientific progress as essentially a cumulative process, where successive scientific theories make closer and closer approximations to the truth (or the way the world is), when he states that

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⁹⁶ Ibid., 134. 97 Ibid. 98 Ibid. 99 Ibid.

scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles but also because it is some how а better representation of what nature is really like. One often hears that successive theories grow ever closer to, or approximate more and more closely to, Apparently generalizations like that the truth. refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory postulates nature and what is 'really there. 100

Furthermore, Kuhn goes on to say that,

Perhaps there is some other way of salvaging the notion of 'truth' for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its 'real counterpart in nature now seems illusive in principle. Besides, as a historian, I am impressed with the implausibility of the view. I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development.

Before moving on it will be profitable to summarize

Kuhn's relativism as rendered by Doppelt. He makes the

distinction between

Long-run Relativism Concerning Scientific Knowledge: - It is not the case that scientific development as a whole can constitute a progress in scientific knowledge and truth.

Short-run Relativism Concerning Scientific Knowledge: - It is not the case that every major stage of scientific development (in which one theoretical tradition is supplanted by a rival one) constitutes a progress in scientific knowledge and truth.

¹⁰⁰Thomas S. Kuhn, <u>The Structure Of Scientific</u> <u>Revolutions</u>, 2nd ed.(Chicago: The University Of Chicago Press, 1970): 206. **101**Ibid.

Short-run Relativism Concerning Scientific Rationality: - It is not the case within scientific development that new theories are always or even characteristically more rational to accept than their predecessors and come to be accepted by scientists because they are more rational (better supported by evidence, more simple, etc.).

At this stage in his discussion of Kuhn, Doppelt raises the question of what kind of a picture of scientific rationality we have, if we accept his argument for short-run relativism concerning scientific knowledge. In the first place he points out that positivist accounts of science typically assume that the "philosophical criteria which ground progress in scientific knowledge in the long-run to roughly correspond the actual criteria underlying scientific behavior and methodology at least so far as it is rational."103 Furthermore, the "positivist model of the standards by which scientific knowledge is evaluated is also taken to provide an account of actual scientific reasoning, debate, and theoretical choice throughout the development of science."104 In addition, on a positivist account of rationality "scientists in the past have transferred their allegiance to a new paradigm because it better satisfies the standard of increasing and cumulative empirical adequacy than its predecessor."105

However, Doppelt maintains that, "if this positivist standard is not satisfied in all scientific revolutions, then

¹⁰² Doppelt, "Kuhn's Epistemological Relativism," 135.
103 Ibid., 136.
104 Ibid.
105 Ibid.

¹⁰⁵ Ibid., 136-137.

it cannot capture the actual reasons at work in these transitions or the sense in which they are rational."¹⁰⁶ Thus, he draws the conclusion that short-run relativism concerning scientific knowledge "implies a closely related short-run relativism concerning scientific rationality; relative to the positivist criterion of increasing, cumulative empirical adequacy, it is not the case (a) that all new scientific theories are more reasonable to accept than the theories they replace and (b) that they are in fact accepted by scientists on these (positivist) grounds."¹⁰⁷ Doppelt characterizes positivism as a theory concerning the "reconstruction, comparison, and evaluation of fully developed theories; the process through which such theories are developed is either relegated to the psychology of discovery or assumed to be governed in its rational aspects by the same criteria which positivist accounts employ to analyze the finished products."¹⁰⁸ According to Doppelt "Kuhn's theory of science challenges this epistemological approach by treating scientific development in a way which implicitly drives a wedge between the epistemology of (completed) scientific knowledge and that of its rational development."¹⁰⁹ One of the features of Kuhn's argument is that it is "focused on the epistemological properties of scientific debates, choices, and conflicts in the development

¹⁰⁶Ibid., 137. **107**Ibid.

¹⁰⁸ Thid

¹⁰⁹ Ibid.

of these theories well before they can be compared as more or less fully developed alternatives."¹¹⁰

Basic to Kuhn's challenge to the positivist conception of rationality is that his "shift-of-standards thesis denies that rival paradigms share sufficiently overlapping criteria for evaluating evidence to permit either to establish or exhibit rational superiority over the other."¹¹¹ Now Doppelt correctly points out that a "positivist account can certainly grant that in the stages before the rivals are more or less fully developed, the available evidence is typically insufficient to indicate the rational superiority of one or the other."112 However, Kuhn offers a different conception of scientific rationality where he attempts to "undercut the positivist emphasis on 'insufficient evidence' as the central epistemological element in these periods; instead his account identifies this element as incompatible principles (i.e. criteria, standards) for weighing the importance of different evidence (observational sorts of explicanda and problems)."113 Thus it is maintained by this view of rationality "irreducible that there are normative disagreements concerning how the discipline ought to be defined in these periods; such disagreements underlie the in which divergent sense choices rational in are revolutionary periods and constitute an essential

¹¹⁰ Ibid.

¹¹¹_Ibid.

^{112&}lt;sup>-</sup>Ibid., 138.

^{113&}lt;sub>Ibid</sub>.

epistemological component in the rationality of the whole process through which these periods end."¹¹⁴

Those who support a positivist account of rationality would maintain that "if available evidence is insufficient to ground rational choice in the early stages of revolutionary debate, there must come a point where the evidence is sufficient, and this must be the point at which the bulk of the scientific community transfers its allegiance to the new paradigm."¹¹⁵ And it is further argued that "those who cling to the old paradigm at this point do so at a price to their scientific rationality."¹¹⁶ Kuhn counters this point of view by claiming that "most members of the scientific community transfer their allegiance to a new paradigm well in advance of the point where it can explain more than the old paradigm."¹¹⁷ It is unvaryingly maintained that "even at the point where the new paradigm explains far more than the old, some scientists maintain their allegiance to the old paradigm for reasons which are neither less scientific nor credible, according to Kuhn's model, than those in favor of the new paradigm."118 It is possible to maintain that these "choices are rational and develop in a rational way on the assumption that they essentially involve different criteria of science (or, principles of evidence) and an irreducible element of

- 114 Ibid.
- 115_{Ibid}.
- 116_{Thid}
- 117 Thid
- 118 Ibid.

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'conversion' from one to another."¹¹⁹ In Kuhn's view, "good reasons in favor of either paradigm can only become 'compelling' if its own criteria are already accepted."¹²⁰ Although Doppelt contends that 'criteria of evidence' are necessary for an explanation of scientific development, this In addition he points out that both the is not sufficient. 'loss-of-data' thesis and the relativism of scientific knowledge are independent of this conception of scientific rationality. Moreover, it is claimed that even if a "new paradigm at some late stage in its development succeeds in explaining all of the genuine observational explicanda of its predecessors and more; let us assume that we count this as progress in scientific knowledge, in retrospect - using the cumulative criterion. Nevertheless practically all the reasonable choices to resist or switch to this new paradigm (those responsible for its development) will have occurred before this point and cannot have rested on this cumulative criterion."121

One point which Doppelt stresses is that even if it becomes possible in retrospect to show that completed theories satisfied the 'cumulative criterion' we still could not claim that "past scientific communities actually responsible for the development of these theories did employ or would have accepted this criterion (even at the point

^{119&}lt;sub>Ibid</sub>. 120_{Ibid}., 139.

where it is satisfied)."¹²² He maintains that the relativism of rationality is "quite a powerful position even if we reject the relevance or decisiveness of this thesis with respect to scientific knowledge."¹²³

Finally Doppelt completes his argument by claiming that the price of devising "an objective and external criterion of rationality, as well as scientific knowledge, quite independently of the shifting standards internal to scientific development itself"¹²⁴ is too high. Such a price would be an inability to explain the "actual reasons and rational principles operative in scientific life"¹²⁵ at a particular time. In addition, he endorses a 'subjectivist conception of rationality' that takes into account a community's "own ends, norms, and experiences in ways which do not affect what is true or what they know."126 As he points out people do believe false things. Furthermore, he states that "within a certain structure of norms and values, people may reasonably do things which from an objective and external standpoint, are wrong."¹²⁷ However, Doppelt is careful not to support a relativistic conception of knowledge, as he says that "we require an 'objective' conception of knowledge (truth)."128

122 Ibid. 123 Ibid. 124 Ibid. 125 Ibid., 140. 126 Ibid. 127 Ibid. 128 Tbid.

In summary, according to Doppelt's reconstruction of Kuhn. "rival paradigms are . . . incommensurable in that they embody incompatible attitudes toward the fundamental problems any paradigm in the field ought to (try to) solve."129 And furthermore, in as much as "paradigms disagree as to what the fundamental questions are, they disagree as to the proper standards of explanatory adequacy by which any paradigm in the field must be assessed because each paradigm's standards will be a function of the set of problems each paradigm recognizes as fundamental to the discipline."130 Thus. different paradigms are incommensurable because they "set for themselves different and incompatible criteria of explanatory adequacy. . These incompatible standards generated are from each paradigm's tendency to disagree as to what counts as the fundamental problems any paradigm in the field ought to solve."¹³¹ Thus, for Doppelt, epistemological relativism, "results from incommensurability of the standards of explanatory adequacy of rival paradigms."¹³²

Harvey Siegel has taken issue with this version of Kuhnian relativism and Doppelt has attempted to defend his position against Siegel's criticisms. Siegel claims that "Doppelt's admission that coherent debate and comparison of rival paradigms is possible vitiates any strong

¹²⁹ Siegel, "Latest Form," 109.

^{130&}lt;sub>Ibid</sub>.

¹³¹Ibid., 107.

¹³²Ibid., 109.

incommensurability position."¹³³ Nevertheless, Doppelt maintains that meaningful debate and comparison between rival paradigms is "not sufficient . . . to allow their discourse to terminate in rational consensus (p.41)."¹³⁴

The principle point of contention is whether or not paradigm-neutral standards are available to resolve disputes between rival paradigms. Siegel argues that "different standards of adequacy are themselves, on Doppelt's account, open to meaningful comparison and rational evaluation . . . standards of adequacy are themselves open to debate, there is rational no reason to assume that consensus is impossible."135

Siegel contends that if rational debate about standards of theoretical adequacy can take place across paradigms, this would seem to imply that such "debate must, presumably, depend on the possibility of paradigm-neutral perspective: that is, such debate depends on the existence of paradigmneutral meta-standards by which paradigm-bound standards can be neutrally evaluated."¹³⁶ He argues that "while internal standards of adequacy may be operative within a paradigm, there must be external standards by which internal standards can themselves be neutrally judged."¹³⁷ Doppelt appears to

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¹³³ Ibid., 111. 134 Ibid. where (p.41) refers to Doppelt, "Kuhn's Epistemological Relativism. 135 Ibid., 112. 136 Ibid., 113. 137 Ibid.

accept the existence of external standards, but argues that "they are determined by internal ones."¹³⁸

On the other hand Doppelt maintains "that 'rational debate between rival paradigms only presupposes that each side recognizes the empirical success or the other's problemsolutions as successes, as a 'good reason' in the latter's favor."139 For Doppelt, "such 'rational' debate (involving the mutual exchange of good reasons) is perfectly compatible with the provocative Kuhnian relativism which denies that such good reasons in favor of a new paradigm can ever be rationally 'compelling' to those scientists who continue to adhere to the standards internal to the old paradigm."¹⁴⁰ Siegel disputes Doppelt's "distinction between good and compelling reasons, and his claim that reasons for paradigm change can never be compelling . . . the standards of science are themselves open to meaningful comparison and debate."141 coherent Doppelt contends that Siegel extrapolates from his argument concerning the theoretical possibility of paradigm-neutral standards to the conclusion that such standards must in fact exist. While not denying the conceptual and logical possibility of such standards, Doppelt objects to Siegel's inference and maintains that the issue is a contingent one. Thus, Doppelt asserts that

138_{Ibid}.

Ibid.

¹³⁹ Doppelt, "Reply to Siegel," 120.

¹⁴¹Siegel, "Latest Form," 113.

Siegel's repeated use of the language of possibilities (what scientists can or cannot do) misunderstands the epistemic status of the Kuhnian relativist argument. It is an argument from the facts evidence of actual scientific debate or and development, suitably interpreted. The question at issue is not whether it is possible in principle for exponents of rival paradigms to share 'external standards' by which their debates might be governed. It is rather the question of whether the historical evidence of even the most paradigmatically rational debates in the development of science makes it reasonable to believe that in fact there are such (explicit or implicit) paradigm-neutral standards. Kuhn's powerful interpretation of science denies this claim and I find nothing in Siegel's work which supports it.¹⁴²

Doppelt asserts that Siegel "mistakenly Moreover presumes that if one admits that scientists can bring reasons to bear on the internal standards themselves, there must be external meta-standards in the situation to prove the basis for these reasons."143 Doppelt concludes that in "sum nothing in the actuality or possibility of rational debate concerning rival paradigms and their rival standards implies or suggest the existence of Siegel's 'compelling' reasons, external standards, or the denial of a forceful Kuhnian relativism."144

In the final analysis Doppelt insists that if his interpretation is sound then the question as to whether or scientific development is (and not relativistic its epistemological implications) "cannot be resolved by purely philosophical argumentation but rather requires an

¹⁴² Doppelt, "Reply to Siegel", 120.

¹⁴³ Ibid., 121.

^{144&}lt;sub>Ibid</sub>.

examination of actual cases of scientific revolution to test Kuhn's own use of scientific examples."¹⁴⁵

¹⁴⁵ Doppelt, "Kuhn's Epistemological Relativism," 114.

III. <u>The Chemical Revolution as</u> <u>a Model of Paradigm Shift</u>.

In order to evaluate the extent to which Doppelt's description of Kuhn's thesis applies to the actual replacement of one paradigm by another it will be useful to examine a concrete instance of one scientific revolution in some detail, particularly with respect to those factors which Doppelt has acknowledged to be contingent features of such developments. During the latter part of the eighteenth century chemistry went through a drastic transition which Kuhn and some other historians of science have called a scientific revolution and this example is frequently cited as typical of paradigm shifts. Thus, Kuhn has asserted that "what Lavoisier announced was . . . the oxygen theory of That theory was the keystone for a reformulation combustion. of chemistry so vast that it is usually called the chemical revolution."146

In Kuhn's own words,

The much maligned phlogiston theory . . . gave order to a large number to physical and chemical phenomena. It explained why bodies burned . . . and why metals had so many more properties in common than did their The metals were all compounded from different ores. elementary earths combined with phlogiston, and the metals, produced common latter, common to all properties. In addition, the phlogiston theory accounted for a number of reactions in which acids were formed by the combustion of substances like carbon and sulphur.¹⁴⁷

Furthermore, traditional chemistry's attentions

¹⁴⁶Kuhn, <u>Structure</u>, 56. **147**Ibid., 99-100.

had directed toward qualitative. been the Explanations of qualitative properties were sought; for example, metallic shine was linked to the fiery qualities of phlogiston, and the earthy features of calxes were accounted for by their earthlike composition, and so forth. All this went by the board when Lavoisier ushered in the new chemical age of chemistry.¹⁴⁸

The phlogiston theory dealt mainly with an effort to explain combustion. Nevertheless,

combustion is very difficult to study. Most things made of commonly burn are many different we substances and give off many different gasses when burned. Moreover, combustion generally is rapid and Progress in such studies required finding violent. simple, well-controlled some subjects for 1770's, • • .In the experimentation. chemists developed a number of techniques for performing such experiments. The leaders were Joseph Priestley in England and Antoine Lavoisier in France. Priestlev supported the phlogiston theory; Lavoisier led the revolution that overthrew it.¹⁴⁹

In examining the events that took place during that time, we are particularly interested in seeing whether or not this revolution in chemistry, which led to the rejection of the phlogiston theory following the discovery of oxygen, has the characteristics that Kuhn and his disciples attribute to such revolutions (incommensurability, especially the loss of data, shift of standards and the problems that a particular paradigm considers important in view of these standards) and the extent to which these justify the degree of relativism which Doppelt is prepared to accept on Kuhn's behalf.

¹⁴⁸ George Gale, <u>Theory of Science: An Introduction To</u> <u>The History, Logic, And Philosophy Of Science</u> (Toronto: McGraw-Hill Book Company 1979): 135.

¹⁴⁹Ronald N. Giere, <u>Understanding Scientific Reasoning</u>, Second Edition(Toronto: Holt, Rinehart and Winston, 1984): 115.

For example, in showing how "changes in standards governing permissible problems, concepts, and explanations can transform a science"¹⁵⁰ Kuhn wrote that

revolution, one Before the chemical of the acknowledged tasks of chemistry was to account for the qualities of chemical substances and for the changes these qualities underwent during chemical reactions. With the aid of a small number of elementary 'principles' -of which phlogiston was onethe chemist was to explain why some substances are acidic, others metalline, combustible, and so forth. Some success in this direction had been achieved. We have already noted that phlogiston explained why the were so much alike, and we could metals have developed a similar argument for acids. Lavoisier's reform, however, ultimately did away with chemical 'principles,' and thus ended by depriving chemistry of some actual and much potential explanatory power. To compensate for the loss, a change in standards was During much of the nineteenth century required. failure to explain the qualities of compounds was no indictment of a chemical theory.¹⁵¹

Doppelt also endorses the view that the incommensurability of standards is illustrated by "the shift from pre-Daltonian to Daltonian chemistry."¹⁵² Thus, Doppelt states that,

Kuhn considers the transition from the pre-Daltonian to the Daltonian paradigm of chemistry to be among our best examples of scientific revolution ([1], p.133). His account of this transition stresses the alleged fact that the pre-Daltonian chemistry of the phlogiston theory and the theory of elective affinity achieved reasonable answers to a whole set of questions effectively abandoned by Dalton's new chemistry. The old chemistry was able to explain the observable qualities of chemical substances - e.q. why the metals were so much more alike in their observed metalline qualities than their ores, and

150 Kuhn, Structure, 106.

151 Ibid., 107..
152 Siegel, "Latest Form," 108.

also the qualitative changes they undergo during chemical reactions, such as the formation of observed acidic properties. For example it explained the common properties of the metals as due to their possession of phlogiston, lacking in their ores ([1], In effect, the new 'quantitative' pp.99-100). chemistry of Lavoisier and Dalton abandoned any concern for theses questions and these observational whose treatment constituted data the main achievement of the earlier model of chemistry. Thus, the new paradigm 'ended by depriving chemistry of some actual and much potential explanatory power' ([1], p.107) - Though it brought in its wake the capacity to treat a whole range of data and problems (concerning weight relations and proportions in chemical reactions) only accorded minimal recognition Yet, as Kuhn sees the matter, what has before. occurred in this transition is 'a change of standards'; because 'During much of the nineteenth century failure to explain the qualities of compounds was no indictment of chemical theory' ([1], p.107) even though this capacity constituted one of the main criteria of explanatory adequacy within pre-Daltonian chemistry.

It has been maintained that this shift from qualitative to quantitative standards was the hallmark of the chemical revolution. In general it is claimed by Doppelt and others, that phlogiston chemistry, "from the very start, is a qualitative explanatory system."¹⁵⁴ It can be argued that "Lavoisier even in the beginning was somewhat antiparadigm insofar as he showed interest in quantities."¹⁵⁵ In the study of chemistry he uses "his balance and balance sheet, and in fact soon produces the very first chemical equation. Chemistry thus gains a quantitative power because of

¹⁵³ Doppelt, "Kuhn's Epistemological Relativism," 122.

¹⁵⁴ Gale, Theory of Science, 135.

^{155&}lt;sub>Ibid</sub>.

Lavoisier's new ideas. But his first move does not produce only profits. Something was lost from chemistry as well."¹⁵⁶

In this connection we are reminded that Kuhn had argued that one of the reasons for the incommensurability of rival paradigms was a loss of data, and he had specifically claimed that a consequence of the demise of phlogiston was that "Lavoisier's chemical theory inhibited chemists from asking why the metals were so much alike, a question that phlogistic chemistry had both asked and answered. The transition to Lavoisier's paradigm had . . . meant a loss not only of a permissible question but of an achieved solution."¹⁵⁷

The question then naturally arises - does the new paradigm compensate for these alleged 'losses'? This raises another issue which must be addressed - whether or not the chemistry represented a clear-cut improvement(i.e. new progress) in comparison with the phlogiston theory. Some scientific historians argue that "we must always be careful not to simply equate paradigm change with progressive increase in explanatory scope."¹⁵⁸ On the other hand, it is maintained that paradigm change

always involves some perceived improvement (otherwise, why change?), but there is always some losses as well. When paradigms change, so do the implied questions, problems, and solutions which occupy scientists. Usually involved in the situation is some question, problem, or solution whose loss represents a sacrifice, at least in the intellectual component of the science. But notwithstanding the

- 156_{Ibid}.
- 157 Kuhn, Structure, 148.
- **158**Gale, <u>Theory of Science</u>, 135.

loss, 'science marches on' in some important sense during paradigm change. It is no different in this present case, as we shall see.¹⁵⁹

Before evaluating these issues we must look at the phlogiston theory and see just what led to its eventual In order to fully understand the phlogiston overthrow. theory and the events leading up to its downfall, it will be useful to examine the state of chemistry during and just prior to the phlogiston period. The science of chemistry had been delayed in its development or 'modernization' by two In the first place the ancient view of chemistry factors. was still prevalent in the eighteenth century even though break-throughs had already occurred during the maior seventeenth century in physics, mechanics and astronomy. From the time of Aristotle scholars had believed that "airs, earths, fires, and waters were the ultimate qualitative categories into which substances were arranged."¹⁶⁰ During the eighteenth century many chemists still "regarded the vast multitude of different substances that we see in the world around us as consisting of only . . . four elements."¹⁶¹ For example, one could conjecture that a particular substance which "burned more vigorously than another was therefore supposed to contain a higher proportion of the element fire; and one that was more fluid than another was similarly supposed to contain a higher proportion of the element

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^{159&}lt;sub>Ibid</sub>.

¹⁶⁰ Ibid., 119.

¹⁶¹ Douglas McKie, "The Birth Of Modern Chemistry," in The History Of Science; Origins And Results Of The Scientific Revolution: A Symposium(London: Cohen And West Ltd., 1951): 97.

water."¹⁶² The second factor which hindered the birth of modern chemistry was the phlogiston theory itself. This was "a broad conceptual scheme into which could be fitted most of the chemical phenomena of the mid-eighteenth century."¹⁶³ It was based on a theory of combustion which had been formulated in the seventeenth and eighteenth centuries by two German chemists, Becher and Stahl. In their view, all combustible and inflammable substances were assumed to contain a common principle of inflammability which Stahl named phlogiston. When a combustible substance was burnt, phlogiston was thought to escape from it in the form of fire and flame. According to this theory "when a match is struck or a candle burns, some 'fire-stuff' is released from each of them - and so like wise for other kinds of burning."164

The phlogiston theory permitted the co-ordination of many previously isolated facts. For instance, phlogiston

was the substance emitted during combustion and the calcination of metals, the 'food of fire' or 'inflammable Principle'. The complete, or almost complete, combustion of charcoal, sulphur, complete, phosphorus, etc. demonstrated that these bodies were very rich in phlogiston: while the formation of sulphuric acid, phosphoric acid, etc. from the the fumes produced by solution of combustion demonstrated that the substances themselves actually consisted of nothing but acid joined the to phlogiston (ie. sulphur minus phlogiston --> sulphuric acid: therefore sulphuric acid + phlogiston = sulphur). When a metal was heated, the phlogiston

163 James Bryant Conant (ed.), "The Overthrow Of The Phlogiston Theory; The Chemical Revolution of 1775-1789," in Harvard Case Histories In Experimental Science, 1, ed. James Bryant Conant and Leonard K. Nash (Cambridge: Harvard University Press, 1948): 70. 164 McKie, "Birth of Modern Chemistry", 98.

^{162&}lt;sub>Ibid</sub>.

given off left a calx behind (therefore calx + phlogiston = metal). Conversely, by heating the calx with charcoal, phlogiston was exchanged and the metal restored. Many reactions became comprehensible when interpreted in terms of an exchange of phlogiston, so that often where a modern chemist sees a gain or loss of oxygen, Stahl saw an <u>inverse</u> loss or gain of phlogiston.

As we shall see, this view ultimately proved to be It is useful to give a brief summary of the inadequate. types of events the phlogiston theory was designed to explain before giving a more in-depth analysis of the successes and failures of the phlogiston theory and the impact of Lavoisier's revolution. In the first place the

common-sense view of combustion is that something is driven out of the burning object, leaving only ashes behind. By the eighteenth century, this 'something' had a well-established name, PHLOGISTON-the fire stuff. Assuming that combustible material contains phlogiston explains most of the obvious facts about combustion. Heating drives off phlogiston into the air ; cooling makes it less volatile; smothering The well- known fact that a burning holds it in. candle placed in an enclosed container soon goes out was explained by saying that the enclosed air gets saturated by phlogiston so that the phlogiston remaining in the wax has nowhere to go.¹⁶⁶

In the second place, phlogiston

accounts not only for combustion but also for the very important process of smelting. This is the process by which crude ores are turned into more refined metals. Generally this is done by carefully heating the ores, together with a measured amount of charcoal, to a controlled temperature. It was claimed that the charcoal contains an excess of phlogiston which, at moderately high temperatures, leaves the charcoal and combines with the ore to form the metal. This hypothesis was substantiated by the fact that further heating at higher temperatures

¹⁶⁵A. Rupert Hall, The Scientific Revolution: 1500-1800. The Formation of the Scientific Attitude (Toronto: Longmans Canada Ltd., 1962): 329. ¹⁶⁶Giere <u>Understanding Scientific Reasoning</u>, 114.

returns the metal to its original state. The phlogiston is driven out of the metal by the higher temperature. Even rusting was explained as the result of the phlogiston slowly escaping from the metal.¹⁶⁷

In conclusion, these "claims may be taken as the 'laws' that define PHLOGISTON MODELS. Such models lay behind many hypotheses about systems undergoing combustion, rusting, or the process of smelting. The PHLOGISTON THEORY was the general hypothesis that this sort of model fits most cases of combustion, smelting, rusting, and so on."¹⁶⁸

Let us now turn our attention to a more detailed examination of the phlogiston theory and the developments which led to its downfall. The chemists of the eighteenth century were particularly interested in determining the qualities of metals, and what happened to metals when they were heated. For example, when

a metal, such as copper or lead, is heated, it turns into a powdery substance and its metallic properties are lost. (The same thing happens in the familiar rusting of iron, but there without the application of heat.) The chemists of that time explained this by saying that a metal was a kind of combustible and that, when heated, it lost its 'phlogiston', leaving the powdery residue, which they called a calx.

Furthermore, they knew that "if this calx was heated afresh with charcoal, it was converted back again into metal; and charcoal, since it would burn away almost entirely, was held to be very rich in 'phlogiston'."¹⁷⁰ Thus it was concluded that what had happened was that the "heating of the calx with

^{167&}lt;sub>Ibid</sub>.

¹⁶⁸ Ibid., 115.

¹⁶⁹ McKie, "Birth of Modern Chemistry", 98.

^{170&}lt;sub>Ibid</sub>.

charcoal had therefore restored enough 'phlogiston' to the calx to reconstitute the original metal."¹⁷¹ In addition, it was maintained that "a metal was a compound of its calx and 'phlogiston'; and the process of heating a metal to give its calx, called calcination, was a decomposition, a kind of combustion in which 'phlogiston escaped from the metal."¹⁷²

Chemists of the past had been particularly interested in the practical aspects of chemical theory, such as metallurgy. For example, according to the phlogiston theory "a metal . . . is more complex than is the corresponding oxide. In particular it accounted for one of the simplest chemical processes then employed for practical ends, namely, the preparation of metals from their ores."¹⁷³

It was held at the time that the

transformation of an earthy substance into a metal in the smelting process appeared to be much the same whether the metal was iron, or tin, or copper. What could be more plausible than to assume that in each instance the ore, when heated with charcoal, took up a 'metallizing principle' which conferred upon the earth the properties of a metal? If one called this hypothetical substance phlogiston, an 'explanation' for metallurgy was at hand.

"Metallic Ore + Phlogiston---> Metal "175 (An Oxide) Plus from Charcoal

The fact that charcoal would burn by itself when heated indicated to the founders of the phlogiston theory that the phlogiston escaped in the process and became combined with the air. In general, substances

171 Ibid. 172 Ibid., 98-99. 173 Conant, <u>Harvard Case Histories</u>, 70. 174 Ibid. 175 Ibid.

that burned in air were said to be rich in phlogiston; the fact that combustion soon ceased in an enclosed space was taken as clear cut evidence that air had the capacity to absorb only a definite amount of phlogiston. When air had become completely phlogisticated it would no longer serve to support combustion of any material, nor would a metal heated in it yield a calx; nor could phlogisticated air support life, for the role of air in respiration was to remove the phlogiston from the body. Everything fitted together very well. 176 fitted together very well.

Today we understand that air

is primarily a mixture of two gases, oxygen and nitrogen. Combustion and respiration involve chemical reaction between carbon compounds and oxygen; the products of these reactions are water and carbon dioxide, except in the case of charcoal, when carbon dioxide alone is formed. When a metal is heated in air, it forms an oxide by combining with the oxygen; the product was known to the chemist as a 'calx' and the process as 'calcination'.¹⁷⁷

In addition, when

many oxides of metals are heated with charcoal, the oxygen combines with the charcoal forming carbon dioxide ('fixed air' to the chemists of the eighteenth century) and the metal. Mercury oxide, a red powder, also known as red precipitate or mercurius calcinatus per se, has the unusual property of being converted into the metal mercury and oxygen when heated quite hot without charcoal.¹⁷⁸

According to the modern atomic formulations of these

reactions with respect to the metallic element mercury:

" Calcination:

2Hg Mercu Metal	+ Iry Plus	O ₂ Oxygen Gas	Heated > Yields ()	2HgO Oxide of Mercury Red Powder)
<u>Decom</u> 2HgO Oxide Mercu	position of hea of very ry> Yie	Oxide. ted 2He hot Merce meta lds	g + ury Plu al	0 ₂ s Oxygen Gas
176	• •			

176 Ibid. 177 Ibid., 68.

178 Ibid., 68-69.

<u>Reduction with addition of</u> <u>Charcoal (also called reduction</u> <u>with phlogiston)</u>.

2HgO С CO_2 + Heated 2Hq +Plus Charcoal Oxide of --> Mercury carbon Mercury (Carbon) YieldsMetal dioxide or fixed air"**179**

Let us look more closely at the second of these experiments (decalcination of mercury) in order to see just how the development of quantitative methods created difficulties for the explanation of calcination according to the phlogiston theory. When in

the 1770's, utilizing techniques first developed by Priestley, Lavoisier performed a number of careful experiments with mercury. In one of these experiments he floated a precisely measured amount of mercury on a liquid and covered it with a glass jar, thus enclosing a known amount of air . . The mercury was then heated using the rays of the sun focused by a powerful magnifying glass (a burning glass). In such circumstance, as Lavoisier well knew, a red powder, or ash, forms on the surface of the mercury. Some of the mercury undergoes a controlled burning. (see figure., 1 on page 64)

If we apply

the phlogiston model to this experiment, one would expect two things. First, the resulting mercury plus red ash should weigh LESS than the original sample of mercury alone. This is because some phlogiston must be driven off, leaving the ash behind. And the volume of air inside the jar should INCREASE since it now contains the phlogiston that was driven out of the mercury. This means that the level of the liquid inside the jar would drop to make room for the additional 'air'.

179 Ibid., 69.

¹⁸⁰ Giere, <u>Understanding Scientific Reasoning</u>, 115-116. 181 Ibid., 116.



Figure 1. Priestley's method for liberating oxygen from the red oxide of mercury by using a burning lens to decompose the compound at high temperature and collecting the gas released over liquid mercury as depicted by Conant.¹⁸²

182_{Conant, Harvard Case Histories},
In fact, the results of this experiment proved to be exactly opposite to what would have been anticipated on the basis or thephlogiston model. At the completion of the experiment, the water level in the container had gone up rather than down, and the mercury/ash residue weighed more than the mercury alone had weighed when the experiment had begun. Of course, with greater heat (the second reaction in the above set of chemical equations), this outcome was reversed as the calx lost weight and the oxygen released from it caused the fluid level to go down.

Some of the other problems for the phlogiston theory were that "no one had ever seen phlogiston, or could mention a single one of its properties save that it departed on combustion It was, therefore, a hypothetical substance devised for a single purpose. This, however, troubled no one."¹⁸³ Another problem confronting it was "that air is required for combustion . . . and must have been generally known to anyone who could successfully build a fire."¹⁸⁴ However, this problem was alleviated by the claim that "the phlogiston did not simply go away in combustion; it united with the air or some portion of it. If there was no air present the fire went out because the phlogiston had nothing with which to combine."¹⁸⁵

¹⁸³J.F. Moore, <u>A History of Chemistry(New York: McGraw-</u> Hill Book Company, Inc., 1931): 33. 184Ibid. 185Ibid.

Nevertheless it has been claimed that qualitatively, the satisfactorv "phlogiston theory was а framework to accommodate the chemical phenomena known in the 1770's. Even some quantitative changes could be accounted for"186 bv phlogiston. For example, when a candle was burned in a confined space of known volume, it was observed that the candle would burn for a particular amount of time, and the candle's flame would get dimmer and dimmer. The candle went out when common air became 'saturated' or 'loaded' with phlogiston. Thus, the air was no longer able to absorb any more phlogiston and the candle went out. It was also observed that the quantity of air was reduced. It was maintained that the "diminution in bulk of the air. . . is a consequence of the phlogistication of the air."187

Nevertheless, one persistent anomaly of the phlogiston theory was that "when a metal was calcined, the weight of the residual calx or powder was greater than the original weight of the metal taken. But how could the weight increase, since something material, namely 'phlogiston', had been lost from the substance of the metal?"¹⁸⁸ Once Lavoisier had shown that "phosphorus, sulphur and several metals increased in weight on combustion or calcination how could chemists still accept the idea of phlogiston being given off?"¹⁸⁹

186Conant, Harvard Case Histories, 72. 187Ibid. 188McKie, "Birth of Modern Chemistry", 99. 189Maurice Crosland, "Chemistry and the Chemical Revolution," in The Ferment of Knowledge: Studies in the

However, in spite of what has already been said "such considerations as weight had not been considered vital or even relevant by many earlier chemists, who had been more concerned with essences than physical attributes. but Lavoisier helped to create a new chemistry in which number, weight and measurement were basic parameters."190 Thus, it has been argued that this "sometimes resulted in a situation in which 'traditional' chemists and the new 'physical' chemists argued past each other with no common frame of reference, a classic case of 'incommensurability'."¹⁹¹

Different historians give different evaluations of the importance of the weight gain anomaly, if we could legitimately call it an anomaly at all. But if weight gain during calcination was the only problem facing the phlogiston theory it might be plausibly argued that this was not enough by itself to produce the chemical revolution. There is a great deal of disagreement concerning the importance of the question of weight gain during the combustion of metals etc. Thus, we should be cautious not to assume that this was the only factor involved in facilitating the chemical revolution. Nevertheless, there is little doubt that the problem of weight gain helped spark the chemical revolution. However, if the solution to this problem was all the new chemistry would

<u>Historiography of Eighteenth-century Science</u>, ed. G.S. Rousseau and Roy Porter (New York: Cambridge University Press, 1980): 406. **190**Ibid. **191**Ibid.

accomplish, it could be argued that there might not have been a revolution at all.

As we have seen, one possible solution to the weight gain anomaly for those who supported the phlogiston theory was to maintain that "'phlogiston did not gravitate as other matter, but levitated- that it naturally rose upwards to the heavens whereas other substances naturally tended to fall to the earth - that it had a negative weight , as we might say."192 Thus, some attempted to argue that phlogiston, "unlike all other substances . . . is not attracted to the center of the earth but is repelled from it. Hence the more lighter it is!"193 phlogiston a substance contains the Furthermore, we should take note that there is, "nothing inherently absurd in the idea of something not amenable to the attraction of gravitation, but that iust this hypothetical substance should be the only one to show the property might have set some men thinking."194

Let us see how these arguments evolved during the period in question. Some have argued that at least in the beginning, the phlogiston theory's treatment of weight gain or loss in combustion did not present its supporters with a serious anomaly against it. For example, one could explain "the increased weight of a calx in a way that nowhere conflicted with the idea of phlogiston. Until such a time as gases were collected, and the gain or loss of weight due to

¹⁹² McKie, "Birth of Modern Chemistry," 99.

¹⁹³ Moore, <u>A History of Chemistry</u>, 34.

^{194&}lt;sub>Ibid</sub>.

the participation of a gaseous element in a reaction could be correctly estimated by means of the balance. it was impossible to achieve a balance of masses in a chemical equation."¹⁹⁵ This was at a time (before 1775) when chemical mathematics was such that "there was no palpable absurdity in the conception of phlogiston as a material fluid."¹⁹⁶ Thus one could argue that you could not

expect that chemistry should be able to present you with a handful of phlogiston, separated from an inflammable body; you may just as reasonably demand a handful of magnetism, gravity, or electricity . . . There are powers in nature which cannot otherwise become the objects of sense, than by the effects they produce; and of this kind is phlogiston.¹⁹⁷

Eighteenth-century scientists readily admitted the "existence of weightless, impalpable fluids such as electricity and caloric."¹⁹⁸ Thus, it could be maintained that the "purely logical objection which has often been raised against the phlogiston theory is therefore of small value."199

Nevertheless, one might wonder how this contention could do away with the anomaly confronting the phlogiston theory in gain of weight which was observed during view of the calcination. Even if it was true that there were weightless substances or substances that are not subject to forces such

¹⁹⁵ Hall, The Scientific Revolution, 329.

^{196&}lt;sub>Ibid</sub>.

¹⁹⁷ Richard Watson: Chemical Essays, I (London, 1782): 167, as cited in A. Rupert Hall, The Scientific Revolution: 329-330. 198 Hall, The Scientific Revolution, 330.

^{199&}lt;sub>Ibid</sub>.

as gravity, this might account for no loss of weight, but it certainly doesn't seem to explain how there could be an actual gain in weight during calcination unless additional ad hoc assumptions are made. Accounting for the weight gain in terms of giving phlogiston a negative weight is at least more consistent with the facts even though it is far fetched.

In spite of the various objections that might be raised against the handling of the weight gain anomaly, some have argued that Stahl's phlogiston theory did not "check the progress of chemistry as an empirical science; rather his views provided а useful provisional scheme for the explanation of many experiments."200 It is asserted that "Lavoisier himself was at first far more keenly aware of the need to scrutinize experimental data, than of any implausibility inherent in the phlogistic doctrine itself."²⁰¹ Some of the merits of the phlogiston theory were that it

enabled a consistent interpretation to be given to experiments on combustion, and many others involving oxidation and reduction. Chemists gained a valuable insight into a number of reactions in phlogistic terms, and so learnt to treat natural substancessulphur, carbon, salts, alkalis, acids, metals, earths, and so forth-as the really active participants in their processes.²⁰²

An important prerequisite for the advances in chemical theory which took place in the latter part of the eighteenth

200 Ibid.

²⁰¹ Ibid.

²⁰² Ibid., 331.

century was the improved techniques for collecting the gases evolved during chemical reactions and for quantifying their volume and weight. It was the discovery of oxygen, and the recognition that ordinary air was a mixture of gases, that helped bring about the downfall of the phlogiston theory, and destroyed the concept of there being only four basic elements. It should be kept in mind that the "concept of the gaseous state, which it is so easy to take for granted today, was not achieved without considerable difficulty."²⁰³

Long before the chemical revolution began (1660-89)²⁰⁴ Boyle had proposed that "material substances existed in three states: solid, liquid and air."²⁰⁵ Furthermore, Boyle suggested that each of these states was "dependent upon the relative density of the underlying atoms."²⁰⁶ However, Boyle's view was essentially ignored due to its reliance on quantitative properties, such as how many 'corpuscles' were packed into a given region of space. It was only when quantitative criteria became more acceptable that Boyle's ideas in this regard became incorporated into chemistry.²⁰⁷

Moreover, whereas "it is not difficult to arrive at the concept of solid or liquid as a generalization from experience of several distinct species of solids or liquids, the idea that there could be several distinct species of gas

203 Crosland, "Chemistry and The Chemical Revolution," 398.
398.
204 Gale, <u>Theory of Science</u>, 120.

206 Thid.

 $207_{\rm For}$ a discussion of Boyle's theories in regards to the various states of substances see Gale, 119.

was one of the great achievements of natural philosophy of chemistry in the eighteenth century."208 In fact the "understanding of gases was to be crucial to the chemical revolution of Lavoisier, both the understanding of the relation of gases to other forms of matter and the study of one particular gas which Lavoisier called oxygen."²⁰⁹

On the other hand, increasing knowledge concerning the properties of gases created problems for the phlogiston theory. More than 50 years before the chemical revolution Stephen Hales (refer to Hall p.329) made significant contributions to chemistry in that he "examined 'airs' produced in a variety of chemical processes, particularly in order to discover the quantity evolved from a given weight of materials, but he drew no new qualitative distinction between them. From his experiments he concluded that 'air' was capable of being fixed in substances as a solid."²¹⁰

Later the term 'fixed air' was applied to the gas which was evolved during the reduction of a calx in the presence of charcoal. This gas would not support combustion and readily dissolved in water. It was later identified as carbon Joseph Black was the first chemist to attempt to dioxide. identify 'fixed air' '(1756, or see Hall p.330)' and "at the time discovering its function in a number same of

²⁰⁸ Crosland, "Chemistry and the Chemical Revolution," 398. 209
Ibid., 399-400.
210
Hall, The Scientific Revolution, 331.

reactions."²¹¹ It is maintained that Black's experiments had helped to show an important "linkage . . ., for many 'airs' had been roughly identified in the past (e.g. as inflammable, or extinguishing flame), but none had been clearly described as being distinct in species from common air, nor had any function been ascribed to them as participants in chemical processes."²¹²

Thus, it is asserted that the "most striking part of Black's work . . . was his proof that quicklime was lime deprived of 'fixed air': the quantitative relation was completely established."²¹³ Thus, Black argued that certain chemical reactions involved the 'exchange' of 'fixed air'. In addition, Black's "experiments could be explained without phlogiston, and he resisted all attempts to argue that phlogiston was involved in them."²¹⁴

Another source of trouble for the phlogiston theory was the discovery of the composition of water by Henry Cavendish in 1783²¹⁵. In general it

had been easy for supporters of the phlogiston theory to explain how 'inflammable air' could arise from the action of a metal on a dilute acid but not for Lavoisier. It was Cavendish's experiments on the combustion of inflammable air which gave Lavoisier the vital clue in 1783. He was able to synthesize water from 'inflammable air' (hydrogen) and 'vital air' (oxygen) and he could now explain the

211 Ibid. 212 Ibid., 331-332. 213 Ibid., 332. 214 Ibid. 215 Crosland, "Chemistry and the Chemical Revolution," 406. inflammable air evolved in the action of dilute acids on metals as coming from the water.²¹⁶

According to Lavoisier's 'broad working hypothesis,'

The increase in weight of metals on calcination was, however, a quantitative observation that presented great difficulties to those who thought in terms of phlogiston. After the discovery of the compound nature of water [1783]²¹⁷, an explanation was contrived, but it had only a short life, for the phlogiston theory was then going to pieces rapidly.²¹⁸

Thus, it is maintained that not until

the discovery of the common gases-when hydrogen was taken to be pure phlogiston, nitrogen to be phlogisticated air, oxygen to be dephlogisticated air, etc., - did phlogiston become a serious impediment to the interpretations of experimental work; only then did its usefulness as a hypothesis become really significant.²¹⁹

In analyzing the importance of the phlogiston theory's role in promoting experimental research it has been claimed that the phlogiston theory did not significantly retard the advance of chemistry, at least in the first three-quarters of the eighteenth century. For example, it has been maintained that phlogiston "was undoubtedly a useful concept until about 1765."²²⁰

On the other hand, it is frequently argued that the phlogiston theory did help to delay the scientific revolution that eventually provided the basis of modern chemistry. Nevertheless, it is worth taking note that during the

²¹⁶ Ibid., 406-407.

²¹⁷ See Conant, <u>Harvard Case Histories</u>, 109. 218 Conant, <u>Harvard Case Histories</u>, 72. 219 Hall, <u>The Scientific Revolution</u>, 331. 220 Ibid., 330.

revolutionary phase, 1772-1789,²²¹ there were times when the phlogiston theory was not a serious impediment for the further development and progress of chemistry, as opposed to a later stage where the persistence of the phlogiston theory might be viewed as more of an obstacle to advancement. Tn addition, different historians give a different emphasis as seriousness of certain developments to the for the conditional acceptance of the phlogiston theory. However, in this paper I am attempting to give a consistent view of the main issues, and at the same time do justice to the diversity of opinion presented.

One of the lessons to be learned from the phlogiston theory is that "a theory is not necessarily true because it can explain a great number of facts."222 Some have even argued that the phlogiston theory was "false to the verge of the ludicrous . . . yet coordinated most facts familiar to the chemists of the day and enabled them to use their knowledge efficiently for the solution of new problems. The phlogiston theory was, therefore, well fitted for its position as a great working hypothesis, and this gave it universal credit in spite of faults so glaring that it is now hard to see why they were not patent to every thoughtful observer."223

Thus, in appraising the role of the phlogiston theory it has been argued that what was of primary importance for

223_{Ibid}.

²²¹ See Conant, <u>Harvard Case Histories</u>, 68. 222 Moore, <u>A History of Chemistry</u>, 33.

three-quarters of the eighteenth century, was the development and design of fruitful experiments and the collection of The phlogiston and anti-phlogiston theories were data. crucial for explaining experimental results rather than a necessary component for gathering hard data. Thus, regardless of whether or not chemists accepted the phlogiston theory, they pursued their experimental investigations and continued to accumulate factual information which their theories were required to explain. Most scientists would concede that а in which the further progress of a branch of "situation science is directly dependent upon an adequate matching of theoretical concepts and experimental facts is by no means uncommon. "224 In spite of this one can also maintain that even though "such a matching of fact and theory is always useful it is far from being invariably essential."225 Thus. it has been concluded that the "empirical attitude of the great experimenters was in reality far more important than their theorization: it is therefore the less likely that any plausible modification of the doctrines prevailing through the first three-quarters of the eighteenth century would have had much influence on the course of events"226.

Tn time chemists came to realize that Lavoisier's "interpretation of the phenomena was far superior to that of phlogiston theory: it was one upon which the ultimate

226_{1bid}.

²²⁴ Hall, The Scientific Revolution, 333. 225 Ibid.

advancement of chemical knowledge depended."²²⁷ However The revolution in chemistry was not due solely to Lavoisier's ideas, and "it is also perfectly clear that the inventive empiricism of his contemporaries was just as necessary for this as his own logical, interpretative intellect, and that, moreover, the rapid progress of chemistry in the nineteenth century owed a great deal to developments, such as electrochemistry and the atomic theory, to both of which Lavoisier's own insight into the nature of chemical reactions contributed nothing."²²⁸

It is argued that Lavoisier was more ingenious in his interpretation of experiments than as an "author of new experiments."²²⁹ His contribution was that he was one of the "first to realize their full significance."²³⁰ Thus, none of his "most famous experiments was new: the element of originality in them was limited to Lavoisier's insistence upon paying heed to the teachings of the balance."²³¹

A review of the subsequent evolution of the debate between the adherents and opponents of the phlogiston theory can help us to understand how it came to be replaced. "Lavoisier's new system of chemistry seems to have started with his pondering on the very large increase in weight when phosphorus was burned in air."²³² He had "discovered that

²²⁷ Ibid. 228 Ibid., 333-334. 229 Ibid., 334. 230 Ibid. 231 Ibid. 232 Conant, <u>Harvard Case Histories</u>, 72.

sulfur in burning, far from losing weight, on the contrary, gains it, it is the same with phosphorus; this increase of weight arises from a quantity of air that is fixed during combustion and combines with the vapours."²³³ Furthermore, he believed that the results which he had established by his experiments, were decisive, and they led him "to think that what is observed in the combustion of sulfur and phosphorus may well take place in the case of all substances that gain in weight by combustion and calcination; and . . . that the increase in weight of metallic calxes is due to the same cause."²³⁴

It is contended that we "seem to have here the flash of genius that puts forward a bold working hypothesis on a grand scale without much evidence to support it. Yet there is no doubt, as Lavoisier always claimed, that the essential idea in this theory was contained in this note; something was taken up from the atmosphere in combustion and calcination. This was exactly opposite, be it noted, to the phlogiston doctrine."²³⁵

Lavoisier was convinced that science must "lay it down as an incontestable axiom, that, in all the operation of art and nature, nothing is created; an equal quantity of matter exists both before and after the experiment; the quality and quantity of the elements remain precisely the same; and

^{233&}lt;sub>Ibid</sub>.

²³⁴ Antoine Lavoisier, in a note to the Secretary of the French Academy, 1772 cited by Conant, <u>Harvard Case Histories</u>, 73.
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²³⁵ Conant, Harvard Case Histories, 73.

nothing takes place beyond changes and modification in the combination of these elements."²³⁶ For example, Lavoisier drew generalizations and made predictions on the basis or his experimental observations. He

"discovered that air was 'fixed' in phosphorus pentoxide and sulphur dioxide (made by burning phosphorus and sulphur), bringing about an increase in weight. He predicted that the same would be found of all combustibles, and that the increased weight of metallic calces was due to a similar fixation of air. This last prediction he confirmed (1772), by reducing lead oxide to lead with charcoal: a large quantity of air was involved."²³⁷

It was Lavoisier's strongly held conviction that by the use of quantitative methods and by exploring the process of combustion and calcination he could explain these processes in more precise terms without relying on phlogiston as a necessary component of such an explanation. Thus, he "embarked upon 'an immense series of experiments' intended to reveal the properties of different 'airs' involved in chemical reactions, which seemed 'destined to bring about a revolution in physics and chemistry.'"²³⁸

In order to fully understand the chemical revolution and Lavoisier's and Priestley's contribution to the discovery of oxygen we must look at some of Priestley's experiments with 'nitrous oxide'. In 1772²³⁹ Priestley had developed "a 'nitrous air test' for the purity of air."²⁴⁰ Priestley had

²³⁶A. Lavoisier, 'Elements of Chemistry' trans. Robert Kerr (Edinburgh, 1790): 130 as cited in Hall, <u>Scientific</u> <u>Revolution</u>, 334. 237Hall, <u>The Scientific Revolution</u>, 334-345. 238Ibid., 335. 239Conant, <u>Harvard Case Histories</u>, 113. 240Ibid., 74.

"prepared an oxide of nitrogen . . . nitric oxide (NO) . . . called . . . nitrous air."²⁴¹ Priestley already "knew that this colorless gas which is insoluble in water, when mixed with air produced a red gas ('red fumes') that was soluble in water."²⁴² In addition he also discovered that "air in which a candle had been burned until the flame went out would not produce soluble red fumes with nitrous air'."²⁴³ And the "reason for this, we now know, is that the reaction is between the nitrous air' and the oxygen:

2N0	+	0 ₂	>	2NO ₂	
'nitrous		oxÿgen		'red fumes' that	
air,				dissolve in water "244	

The product of this reaction fails to appear when the oxygen had previously been completely removed from the sample to be tested. As a result, "it is evident that when the two gases are mixed over the water there will be a diminution in volume."²⁴⁵ Priestley had discovered that he could use the 'nitrous air test' to determine the 'goodness of common air'. Priestley would blow a quantity of "nitrogen 'airs' - this time the odorless, colorless, water-insoluble one [ie. NO]-into the sample of air he wished to check."²⁴⁶ Furthermore, he would measure both the quantity of nitrous air and the quantity of the original sample to be tested for its

²⁴¹ Ibid. 242 Ibid. 243 Ibid. 244 Ibid. 245 Ibid., 74-75. 246 Gale, <u>Theory of Science</u>, 246.

'goodness'. Thus, if "the air in the sample was 'good' (respirable, supportive of combustion, etc.), then there would be an immediate reaction: A red gas would be formed, and this gas would go into immediate solution with the water in the base of the collection jar."²⁴⁷ In addition, he "then would measure the volume of the gas which remained after the red fumes had dissolved, and this would provide a measure of the 'goodness' of the air involved."²⁴⁸

For example, it was found that "if only the two gases . . [nitrous air and oxygen] . . . were at hand and the volumes were chosen correctly, there would be no residual gas left and the reaction would be complete."²⁴⁹ However, since "air is only about one-fifth oxygen, there will always be a large amount of residual gas when nitrous air' and common air are mixed; . . the nitrogen does not react and is only very slightly soluble in water."²⁵⁰

Furthermore, depending upon the amount of nitrous air used, and whether or not it was mixed with common air or air containing more or less oxygen, it was observed that likewise the residual volume of the gases varied, depending upon the initial volumes and purities of these gases.²⁵¹ Thus, the

249 Conant, <u>Harvard Case Histories</u>, 75.

²⁴⁷ Ibid.

²⁴⁸ Ibid.

^{250&}lt;sub>Ibid</sub>.

²⁵¹ In experiments where the gases released during chemical reactions were collected over water, the measurement of their volumes was complicated by the capacity of water to dissolve various gases to different degrees. This difficulty was overcome by 1774 after Priestley had introduced the technique of collecting gases over mercury (refer to Hall, 329).

more NO₂ that was produced in the reaction the more of this gas dissolved in water and as a result we get a proportional reduction in the volume of the gases left over. In short, it is evident "that 'nitrous air' and pure oxygen react, and when the reaction is carried out over water a large contraction in volume occurs."²⁵²

In addition, if the "standard procedure for testing good air is followed, one volume of 'nitrous air' will be added to two volumes of oxygen."253 What is important here is that under these conditions "all the 'nitrous air' is used up but a large amount of oxygen is left over. The actual diminution in volume will be deceptively similar to that found when common air is at hand, but the residual gas instead of being nitrogen is oxygen."²⁵⁴ However, there is only "a small but significant quantitative difference", 255 depending upon whether or not you are using common air or pure oxygen. When you use "common air the resulting gas- the residual- occupies only 1.8 volumes if 2.0 volumes are initially employed; with pure oxygen the final volume is nearer 1.6 volumes."²⁵⁶ However, when either common air or oxygen is mixed with nitrous air over water, what is more significant than the discrepancy in the volumes of the residual gases is the difference in their properties. For example, when common air is mixed with 'nitrous air', the

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252Conant, Harvard Case Histories, 75.
253Ibid.
254Ibid.
255Ibid.
256Ibid.
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oxygen is removed and the nitrogen in the common air and some of the 'nitrous air' is left. On the other hand when oxygen is mixed with 'nitrous air' the residual gas still consists entirely of oxygen. Evidently, in the two cases the residual gas left over will have very different properties. Thus, the

residue in the one instance will support neither combustion nor animal life, nor will it react further with 'nitrous air'. In the other, the residue has all the properties of the original sample. Any one of the simple tests will at once make this striking difference apparent; a lighted candle, a live mouse, or the addition of 'nitrous air' will convince anyone that the two samples of residual gas were totally different.²⁵⁷

After collecting a sample of the gas given off from heating red mercury calx, Priestley tested this gas to see if it was combustible. Priestley was extremely surprised that the candle burned brightly, for the "candle should not have burned. When the goodness experiment was run on normal air, it completely exhausted the respirable principle of the sample. Candles put into the remainder went out quickly, burning splints immediately extinguished, and living animals quickly went unconscious."²⁵⁸

However, when Priestley tested the gas he collected from heating the red calx "the candle did not die: rather, it lived happily for a long period of time. . . . The air given off from the calx of mercury was better, of a higher degree

²⁵⁷ Ibid., 76.

²⁵⁸ Gale, Theory of Science, 247.

of 'goodness,' than garden-variety air."²⁵⁹ Furthermore, when he "pumped in his nitrogen air until the red fumes quit forming. . . the calx-of-mercury air was able to absorb four to five times as much nitrogen air as did ordinary air. Thus, the calx's air was four to five times purer than ordinary air."²⁶⁰

However, both "Priestley and Lavoisier overlooked the clue offered to them by the somewhat larger diminution in volume of the new gas when subjected to Priestley's test for 'goodness.'"261 its Priestlev, did not abandon the phlogiston endeavoured to explain theory, but these observations in terms of phlogiston. Thus, he noted that combustion and respiration "cease when the air reaches its saturation level of phlogiston. This is usually after about 20 percent of the available air has been used up."²⁶² Furthermore, he maintained that "ordinary air is about 80 percent saturated with phlogiston in its natural state."263 Priestley still had to account for the gas produced when heating the red calx. He argued, that inasmuch as the gas produced when heating the red calx was four times as pure as ordinary air "this could be interpreted as meaning that it was air which was 100 percent unsaturated by phlogiston."264 Priestley, "then went conservative and stayed entirely within

259 Ibid. 260 Ibid. 261 Conant, <u>Harvard Case Histories</u>, 75-76. 262 Gale <u>Theory of Science</u>, 247. 263 Ibid., 248. 264 Ibid. his paradigm; he made the observable facts consistent and coherent with the phlogiston conceptual system by his very naming of the substance; it was 'dephlogisticated air.'"²⁶⁵ Thus, we can credit Priestley with naming the new gas "in a very coherent, phlogistonian manner; the air given off during smelting of mercury calx was to be called 'completely dephlogisticated air.'"²⁶⁶

On the other hand, Lavoisier at this time (1775) misidentified this gas as 'common air' rather than oxygen. It was Priestley who was responsible for setting Lavoisier straight on this score by using his 'goodness test' for gases to show that the product was superior to ordinary air. But unlike Priestley, once Lavoisier appreciated his mistake in identifying this gas with common air he corrected it. Lavoisier finally came to the conclusion that "this 'respirable air' (the 'dephlogisticated air' of Priestley) combining with metal formed a calx, and that the same air combining with charcoal gave Black's fixed air (carbon dioxide)."²⁶⁷ Thus, by "1778 the elements of his new theory of oxidation were quite firm, and in it phlogiston had no part for he had proved that the phlogiston-concept was the inverse of the truth."268 Once he realized that he was dealing with a discrete new entity, Lavoisier "took the stuff and made it the central element in his new theory. He named

^{265&}lt;sub>Ibid</sub>.

²⁶⁶ Ibid.

²⁶⁷Hall, <u>The Scientific Revolution</u>, 336.

^{268&}lt;sub>Ibid</sub>.

it 'oxygen,' and worked up a wholly new conceptual systemcalled 'oxygen theory' naturally in opposition to theory."269 phlogiston Nevertheless, Priestley's stubbornness continued, and he would never accept the oxygen It can be argued that Priestley was justified for a theory. time in his position concerning the phlogiston theory. However, at a latter stage it can be argued that he was no longer justified in his stance as the weight of evidence was against him.

Initially, neither Priestley nor Lavoisier realized the difference between common air and the gas produced when a mercury calx is heated at high temperature because they failed to do the proper tests. It is claimed that it was only by accident that Priestley came "to examine what was left over when 'nitrous air' had diminished his new air from red oxide of mercury!"²⁷⁰ It is maintained that the "fact that both investigators took the wrong turn in the road at a critical point in a study of the first importance illustrates how much more complicated is the advance of science than 'collecting the facts, classifying the facts, formulating laws, and elaborating from the laws adequate theories.'"²⁷¹

Furthermore, it is claimed that one of the reasons that both Priestley and Lavoisier failed to realize the significance of their experiments was that both men "made the same mistake . . . unconscious assumptions invalidated

271_{Ibid}.

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²⁶⁹ Gale, <u>Theory of Science</u>, 248.

²⁷⁰ Conant, <u>Harvard Case Histories</u>, 76.

their chains of reasoning."²⁷² Thus, for a time Lavoisier failed to realize that he was missing the meaning of his "experiments with the red precipitate of mercury (oxide of mercury)."273 He still identified the 'new gas' produced as common air, rather than oxygen. However, by 1776 he had "prepared an 'air' from a sample of red oxide of mercury and found it to be considerably 'better' than common air by the test with 'nitrous air.'"274 Moreover, by "May 1777, Lavoisier read to the Academy a paper on the respiration of animals in which he makes clear that air is a mixture of two gases, one 'highly respirable,' the other unable to support combustion or respiration. By 1778 there was no doubt in anyone's mind that a new gas, not common air, was produced on heating red oxide of mercury."275

<u>Decompositi</u>	<u>on of Oxide</u> .	•		
2HgO	heated	2Hg	+	0, '
Oxide of Mercury	very hot > Vields	Mercury metal	Plus	Oxygen Gas
	ITETUS			

For Lavoisier, his "broad working hypothesis . . . was that 'something' was taken up from the air when a metal was calcined."²⁷⁶ However, he first thought that "this 'something' might be fixed air",²⁷⁷ but, he was not able to prove this experimentally. It is argued that his working hypothesis was too broad to yield any predictions that were

272 Ibid. 273 Ibid. 274 Ibid., 77. 275 Ibid. 276 Ibid., 87. 277 Ibid.

easily tested, but when he substituted "the words 'fixed air' for 'something,' yielded deductions that were not confirmed by experimental test."278 Due to some comments offered by Priestley, he concluded that "he had a calx which on heating yielded a gas that behaved like common air."279 substituting "common air for the 'something' in his Thus, broad working hypothesis yielded a deduction that appeared to be confirmed."280 However, we must keep in mind that "deductions from broad working hypotheses are never directly confirmed or negated."281 In other words, a "specific experiment must always be related to the deductions by one or more limiting working hypotheses."282 It is at this point that Lavoisier encountered difficulties. He wanted to identify the 'something' released when the mercury calx was heated. For this purpose in 1775 he carried out a series of six experiments in order to determine the nature of the gas produced from heating mercury oxide alone and seeing how it compared to the gas produced when mercury oxide was burned in the presence of carbon. The six experiments he performed showed

(1) that it was not susceptible to combination with water upon shaking;(2) that it did not precipitate lime water;(3) that it did not combine with fixed or volatile alkalis;(4) that it did not at all diminish their caustic qualities; [these first four tests were designed to show whether the gas was in whole or part 'fixed air' as Bayen had reported; obviously it was

278_{1bid}. 2791bid. 2801bid. 2811bid. 2821bid. not;] (5)that it could be used again for the calcination of metals;(6) that is was diminished like common air by an addition of a third of nitrous air; finally, that it had none of the properties of fixed air: far from causing animals to perish, it seemed on the contrary more suited to support respiration; not only were candles and burning objects not extinguished in it, but the flame increased in a very remarkable manner and gave much more light than in common air.²⁸³

It is pointed out that with each of these tests "a limiting working hypothesis was implicit, an 'if...then' type of reasoning was employed."²⁸⁴ The first four experiments gave convincing evidence that the gas was not fixed air. Moreover, the "fifth and sixth tests, together with the experiments with the candle and with animals, seemed to provide conclusive evidence that the gas was common air."285 By supposing that the gas was common air, Lavoisier "could say, 'if I perform the following manipulations, then the result will be such and such'."286 This type of statement "is a limited working hypothesis that is confirmed or negated by test."287 However, scientists were faced with the "question whether another substance could also behave in this manner; of these tests the nitrous air test appeared to be the most specific and must have appealed to Lavoisier because it was at least roughly quantitative."²⁸⁸ Thus, by use of the nitrous air test it was shown that the gas released by

283Antoine Lavoisier, "Memoires de l'Academie des Sciences 1775, p.520, as revised in 1778, cited by, Conant, <u>Harvard Case Histories</u>, 82-83. 284Conant, <u>Harvard Case Histories</u>, 87. 285Ibid. 286Ibid. 286Ibid. 288Ibid. 288Ibid. heating a calx was not common air by seeing how great a diminution took place when the air was mixed with nitrous gas. By 1776 Priestley and Lavoisier "would both agree that a new air was present when the calx of mercury was heated."²⁸⁹

But in spite of this agreement they would disagree as to what broad working hypothesis one should accept. On one hand, "Priestley stuck to the conceptual scheme in which phlogiston was the determining factor in calx formation."290 On the other hand, "Lavoisier saw his broad working hypothesis now made more specific by substituting the words 'a constituent of the atmosphere which supports combustion' for his 'something.'"²⁹¹ And Lavoisier's "working hypothesis on a grand scale was about to attain the status of a new conceptual scheme."292

We should take time to point out that Priestley had a strong allegiance to the phlogiston theory even when it was in conflict with his own experimental results. For example, we have seen

that Priestley called the gas he had discovered dephlogisticated air, his idea being that this was the component of the atmosphere with which the phlogiston united when it emerged from a burning substance. He called nitrogen 'phlogisticated air,' and this nomenclature would seem to imply that he considered it a product of such union. If so nitrogen should sometimes appear as a product of combustion, but this contradiction was overlooked,

- 289_{Ibid}. 290_{Ibid}. 291_{Ibid}.
- 292_{Ibid}.

like every fact which told against the phlogiston theory.²⁹³

Furthermore, "Priestley missed entirely the real significance of his discovery."²⁹⁴ Priestley and several of this contemporaries were

so sure that something was always given off in combustion that they had lost the power to believe that the burning body united with one of the gases of the atmosphere even when they saw the latter disappear before their eyes. Such blindness was really less pardonable in Priestley than in the others, for he not only could not draw the correct conclusion from his own experiments, but all the work of Lavoisier a little later failed utterly to convince him, and he defended the theory of phlogiston to the last.²⁹⁵

There are a few things to take note of in the way that the experiments of Lavoisier and Priestley helped to "illustrate а "number of general principles in the science."296 development of For example, some of "the difficulties of chemical experimentation are exposed very clearly; the difficulties are sometimes those of interpretation of what is observed, sometimes the failure to try what now seems an obvious further experiment, often the failure to have homogeneous materials at hand."297 In addition, "the role of accidental discovery is almost glorified by Priestley."298

Furthermore,

²⁹³ Moore, <u>A History of Chemistry</u>, 49. 294 Ibid., 49-50. 295 Ibid., 50. 296 Conant, <u>Harvard Case Histories</u>, 104. 297 Ibid. 298 Ibid.

repeated use of the limited working hypothesis is evident. For example, every time a chemical test is Priestley or Lavoisier is essentially applied, saying, 'If I do so and so, such and such will happen.' Priestley's original faulty identification laughing gas and his of oxygen as failure to interpret the 'nitrous air test' correctly shows how involved hidden assumptions are many in the interpretation of experimental results.

important be learned Another, lesson to is that "Priestley's blind adherence to the phlogiston theory in spite of his own effective discovery of oxygen and in spite of its obvious faults (such as the failure to account for the increase in weight on calcination) shows the hold that one scheme mind conceptual may have on the of an investigator."³⁰⁰ And finally, by 1778 we see in the story of the overthrow of the Phlogiston theory "the transformation of a broad working hypothesis into a new conceptual scheme . . . of revolutionary importance."301

We should not lose sight of the conditions that helped to precipitate a chemical revolution such as: "(a) the improvement in communications among scientific men, which made science more and more of a cooperative effort; (b) the accumulation of quantitative studies in physics that made unsatisfactory the concept of phlogiston, which implied a substance with a negative weight; (c) the accumulation of a century's work on the materials, apparatus, and techniques of chemistry."³⁰²

299_{Ibid}. 300_{Ibid}. 301_{Ibid}. 302_{Ibid}., 104-105. Thus, in summing up, the "discovery of oxygen . . . was the central event in the overthrow of the phlogiston theory. But it must be remembered that it was the discovery that oxygen was a constituent of the atmosphere which provided the key to the riddle of combustion."³⁰³ As it turned out, the "method of preparing the red oxide of mercury was an essential link in Lavoisier's argument . . . the red powder was a true calx; it was formed when mercury was heated in air. In this process the gain in weight was due to combination of either air or a constituent of air with the mercury."³⁰⁴

By 1783, Lavoisier forcefully directed his attack against the phlogiston theory and "marshaled the evidence for the new ideas and showed that the concept of phlogiston was not only unnecessary but self-contradictory."³⁰⁵ Thus, we are heading toward the "final collapse of the phlogiston theory."³⁰⁶

The cumulative effect of these researches lead to the sudden conversion of most French chemists at about 1785, and the new ideas were firmly fixed by the publication of Lavoisier's great textbook 'La Traite Elementaire De La Chimie' in 1789. The change was well called the Chemical Revolution, for it inverted completely the chemical point of view. The mysterious hypothetical substance, phlogiston, which did not obey the law of gravitation, and changed its properties arbitrarily as theoretical considerations dictated, was banished from the science and the law of conservation of mass vindicated once for all.³⁰⁷

303_{ibid.}, 105. 304_{Ibid.} 305_{Ibid.}, 109. 306_{Ibid.} 307_{Moore}, <u>A History of Chemistry</u>, 56.

As we have seen another important contribution to chemistry at this time ('1783', see Conant, 113) was that "the composition of water was established by experiments of Henry Cavendish . . . which were immediately repeated by Lavoisier."³⁰⁸ Subsequently, with the discovery that water was formed when hydrogen was

burned in air, Lavoisier's scheme was complete. Water was clearly the oxide of hydrogen. Lavoisier at once proceeded to test an obvious deduction from this extension of his conceptual scheme, namely, that steam heated with a metal should yield a calx and hydrogen. It did. (The converse was likewise demonstrated at about the same time.)

Hydrogen + Oxygen ---> Water Steam heated with metal ---> Calx + Hydrogen (oxide)

After the relationship between water, hydrogen, oxygen, metals and oxides' was established, it "would seem to leave no ground for the supporters of the phlogiston theory to stand on."³¹⁰ However, this is not what immediately happened, but rather, "for a few years the new knowledge had the contrary effect."³¹¹ Those who believed "in the phlogiston were at least able to explain why a calx weighed more than the metal."³¹² This was accomplished by a modification of the phlogiston theory as illustrated by the following table.

- 308 Conant, <u>Harvard Case Histories</u>, 109.
- 309_{Ibid}. 310_{Ibid}.
- 312_{Ibid}.

Modified Phlogiston Theory (about 1785)

Hydrogen = phlogiston (often carrying water); Oxygen = dephlogisticated air; Water = dephlogisticated air + phlogiston; Nitrogen = completely phlogisticated air' Common air = partially phlogisticated air carrying water; Metal = calx + phlogiston - water; Calx = base of a pure earth + water; Charcoal = phlogiston + ash + water.³¹³

Once again we see that the "story of the last days of phlogiston theory is of interest, . . . in illustrating a recurring pattern in the history of science."³¹⁴ This recurrent pattern is that it is "often possible by adding a number of new special auxiliary postulates to a conceptual scheme to save the theory - at least temporarily."³¹⁵ However, such a modified theory of conceptual scheme does not always have a 'long' or 'fruitful' life, but, "sometimes, as in the case of the phlogiston theory after 1785, so many new assumptions have to be added year after year that the structure collapses."316 Furthermore, it needs to be pointed out that most of the "illustrations of this pattern. concern concepts and conceptual schemes of far less breadth than the phlogiston doctrine."317

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³¹³ Ibid., 109-110. 314 Ibid., 111. 315 Ibid. 316 Ibid.

^{317&}lt;sub>Ibid</sub>.

In this connection, it is interesting to note that Lavoisier also did considerable work in providing an explanation of the nature of acids. He "spoke of acids as containing 'air'and he later coined the term oxygen from two Greek words meaning 'acid-producer' because he was convinced that oxygen was the principle of acidity. Furthermore, in spite of Lavoisier's error that all acids are oxygen based (in fact some acids contain no oxygen), his theory concerning acids had superior explanatory power"³¹⁸, over the phlogiston theory. Thus, the "oxygen theory of acidity is sometimes treated as an embarrassing mistake, but it is probably more it as a valid useful to consider theory of limited applicability; it certainly did have some predictive value. In so far as he was describing oxy-acids Lavoisier had an adequate theory."319 However, when others corrected Lavoisier's mistake by introducing "the class of hvdracids . . . this was presented not as a revolution but rather as a small adjustment within Lavoisier's new system". 320

It is important that we do not lose sight of the fact that "the oxygen theory, whether of combustion, caloric, acidity or the more general concept of oxidation, has diverted attention from another equally important achievement of Lavoisier, that of reinterpreting chemical composition."³²¹ For example, "Joseph Black was able to

³¹⁸Crosland, "Chemistry and the Chemical Revolution,"
408.
319Ibid.
320Ibid., 415.
321Ibid., 408-409.

discuss the chemical revolution without mentioning the overthrow of the phlogiston theory, referring instead to 'discoveries. . . relating to the constituent parts or principle of natural substances.'."³²²

A metal calx (metal oxide) was no longer thought of as a simple substance but as a compound.

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" Metal ----> Calx + Phlogiston "323
(calx + Phlogiston) "323
was replaced by
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"Metal + Oxygen Mental Oxide + caloric (Oxygen Principle ----> (Metal + Oxygen + caloric) Principle) **" 324**

fairness to the historical record. In and as the previously cited chemical equations reveal, in spite of Lavoisier's antiphlogiston position he was not able to do away with the notion of phlogiston entirely and reintroduces a somewhat phlogiston-like idea which he called 'caloric'. Although, Lavoisier's concept of caloric was more limited in scope than that of phlogiston, nevertheless, Lavoisier was reluctant to totally surrender the notion of phlogiston. One of the reasons for this may have been that at this time heat was not very well understood, and fire often was still thought of as being like a liquid substance or as some kind Thus, Lavoisier used caloric to retain the of 'principle'. notion that something was given up during combustion.

³²² Ibid., 409.

³²³ Ibid.

³²⁴ Ibid.

One of Lavoisier's greatest accomplishments was the part he played in helping to distinguish and to identify elements from compounds, which more often than not, the phlogiston theory had gotten wrong. Long before Lavoisier's time Bovle had advanced the postulate that the atom was an entity that could not be further reduced or decomposed into simpler parts by chemical means. Although this idea did not lead at the to the concept of a chemical element, it did imply an time idea of chemical analysis as consisting of the decomposition or dissociation of a compound substance into its simpler parts.

Before Lavoisier's contributions the concept of chemical composition was often confused. If a metal was to be considered as a compound of its calx with phlogiston, yet if the compound weighed less than the sum of its component parts, then the meaning of words such as element and compound were ambiguous. In 1787 Lavoisier was responsible for helping to introduce a new or altered nomenclature. Following Boyle's lead, he identified elements as those substances that cannot be further decomposed³²⁵, at least not by ordinary chemical means. Thus in his 1790 table of elements he included "the elementary gases, oxygen, hydrogen and nitrogen along with heat and light."326

Lavoisier's substitution of oxygen for phlogiston made possible a revised distinction between what was to be

³²⁵ See Moore, <u>A History of Chemistry</u>, 58. 326 Ibid.

considered as simple or complex, which substances were to be classified as elements and which belonged to the category of compounds. At issue was whether or not the process of combustion was actually a decomposition where phlogiston was thought to be given off, rather than an interaction in which one substance united with another to yield a more complex combination of the two. In short when Lavoisier

substituted reduction for phlogistication and oxidation for dephlogistication it was only natural that the newly discovered element oxygen should usurp the position of exaggerated importance from which phlogiston had just been displaced. This is exactly what happened. Every element found its position in the system of Lavoisier according to its relation toward oxygen. Metals had hitherto been compounds of bases with phlogiston. They now became the elements which united with oxygen to form bases.³²⁷

Lavoisier established the principles of quantitative analysis and introduced the idea of writing chemical equations. 328 He maintained that scientific methodology should endeavor to break down substances into their constituent parts and also be able to make the substance from its parts. He advised that "in general it ought to be considered as a principle in chemical science, never to be satisfied without both these species of proofs."³²⁹ For example, he claimed that we "have this advantage in the analysis of atmospheric air; being able both to decompose it, and to form it anew in the most satisfactory manner."³³⁰ By

³²⁷ Ibid., 63.

³²⁸ Ibid., 56.

³²⁹ Conant, <u>Harvard Case Histories</u>, 106.

^{330&}lt;sub>Ibid</sub>.

1785 Lavoisier had shown that the gas remaining from air after its ability to support the calcination of mercury had been exhausted "was no longer fit either for respiration or combustion . . . this gas was commonly called 'mephitic air'. Lavoisier named it azote. In English the name nitrogen was introduced."³³¹ Lavoisier concluded "that atmospheric air is composed of two elastic fluids (gases) of different and opposite properties . . . if we recombine these fluids . . . we reproduce an air precisely similar to that of the atmosphere."³³² We should observe the caveat that the "distinction between a mixture and a chemical compound was not yet quite clear."³³³ However, only by the "assiduous use of the 'principle of the balance sheet' by hard-working investigators in the next two decades was it finally shown that elements unite in definite proportion to form a compound."334

Thus, Lavoisier "was able to present chemistry in a logical order starting from the elements before considered as compounds of these elements. This was all the more necessary in so far as substances which had previously been thought of as simple were now considered compound and vice versa."³³⁵ For example, a metal had always been viewed as a compound and the calx as a simple substance, due to the fact that

^{331&}lt;sub>Ibid.</sub>, 107. 332_{Ibid.} 333_{Ibid.}, 108. 334_{Ibid.}

³³⁵Crosland, "Chemistry and the Chemical Revolution," 109.
'something'(phlogiston) was believed to have been given off during combustion. Furthermore, Lavoisier revealed that chemical composition was a good deal more complex than had been previously believed, when everything had been thought to be composed of some combination of one or more of the four basic elements, fire, air, water, and earth. Chemists of the phlogiston period attempted to explain chemical phenomena in terms of mixtures or absorptions of these elements rather then in terms of chemical reaction or bonding. Thus, Lavoisier finally did away with the notion that chemical reactions could be explained in terms of these four elements. Moreover, he "destroyed the status of air as an element, showing that it was a mixture of gases, one of which took an active part in combustion. Similarly the Aristotelian element of water became in the new chemistry a compound of hydrogen and oxygen."³³⁶

We should not lose sight of the fact that Lavoisier gave a particularly important role to the balance for weighing substances and the careful measurement of volumes of gases. This technique served as a diagnostic tool that would subsequently facilitate the ability of chemists to make subtler distinctions between different kinds of gases, elements and compounds. Previously for example, carbon monoxide was often confused with hydrogen, as both gases share in common the quality of inflammability. Lavoisier's work gave impetus to the search for additional elements and

336_{Ibid., 409-411}.

laid the foundation for the further development of chemical theory by Dalton and Avogadro during the early years of the nineteenth century. 103

IV. Evaluation of Doppelt's Argument.

In this section I will examine a variety of proposed external paradigm-neutral standards in an attempt to show that they did indeed play a role in bringing about the chemical revolution which is contrary to the Kuhnian thesis of incommensurability and epistemological relativism put forward by Doppelt. I will also dispute the claim that the chemical revolution was characterized by a significant loss of data or by failure to represent genuine cumulative progress in scientific knowledge.

In any discussion of the external standards that can help to guide the framing of hypotheses we should not lose sight of the fact that the heart of science is observation. One of the primary roles of a hypothesis is to explain past events or observations and to predict future ones. In other words, the hypothesis both explains and predicts, when it implies "the past events that it is supposed to explain, and . . future ones."³³⁷ And most importantly, when a hypothesis fails to predict future observations, then questions are reopened.

Generally speaking, one can "adopt or entertain a hypothesis because it would explain, if it were true, some things . . . already believed. Its evidence is seen in its consequences."³³⁸ We would also expect that a successful hypothesis would be "a two-way street, extending back to

³³⁷W. V.Quine and J.S. Ullian, <u>The Web of Belief</u>, 2nd ed.(New York: Random House, 1970): 80. 338Ibid., 66.

explain the past and forward to predict the future."³³⁹ At least this is an ideal condition for hypotheses to satisfy, even though it must be admitted that both retrodiction and prediction are not always satisfied by an accepted hypothesis or theory. However, what "we try to do in framing hypotheses is to explain some otherwise unexplained happenings by inventing . . . a plausible description or history of relevant portions of the world."³⁴⁰

The most important question that we have to answer concerns, the nature of the evidential warrant necessary for justification, just what evidence is available, and the extent to which that evidence should count in favor of justifying a hypothesis. Various hypotheses will be held more tentatively than others, depending upon the degree of evidential warrant that is brought to bear in support of a particular hypothesis. Thus, in order to fully understand the nature of evidential support, and subsequently the strength of justification for a hypothesis or theory we must look at the conditions or 'virtues', that can supply incremental support for justifying a hypothesis. Particular importance should be attached to those virtues which are independent of the particular theories or paradigms being compared and which therefore deserve to be classified as external paradigm-neutral standards.

339_{Ibid}. 340_{Ibid}.

One virtue, or external standard which а qood scientific hypothesis may possess is 'conservatism'. Α hypothesis that is designed to explain certain events, may come into conflict with other beliefs. When faced with such a situation it is necessary to reject some part of one's overall 'web of beliefs' in order to once again have a consistent set. In other words, acceptance of a hypothesis "like acceptance of any belief in that it demands is rejection of whatever conflicts with it. The less rejection of prior beliefs required, the more plausible the hypothesis - other things being equal."³⁴¹ Thus, the external standard of conservatism has us "sacrifice . . . as little as possible of evidential support, whatever that may have been, that our overall system of beliefs has hitherto been enjoying."³⁴² In addition, conservatism is a good strategy for pursuing science, because the less we have to revise the less likely that we will make a radical mistake.

Another closely related external standard is 'modesty'. This criterion asserts that we should make only as small a change as possible in our system of beliefs in order to account for some new phenomenon. Even when some new hypothesis is completely compatible with our past beliefs so that conservatism is maintained, there is still room for exercising modesty. In general a "hypothesis A is more modest than A and B as a joint hypothesis . . . one

³⁴¹Ibid., 66-67. **342**Ibid., 67.

hypothesis is more modest than another if it is more humdrum: that is, if the events that it assumes to have happened are of a more usual and familiar sort, hence more to be expected."³⁴³ In short, it is a good policy to assume as little as possible that "will suffice to account for the appearances."³⁴⁴ Like conservatism, modesty helps to ensure that we take the smallest amount of risk in making a serious error, when we account for new phenomena within our system of beliefs.

It is pertinent to ask whether or not the criteria of conservatism and modesty were satisfied when the chemical revolution took place. The loss of data argument is relevant to this issue. Thus, according to Doppelt and Kuhn, the "Daltonian revolution should not be viewed simply as the expansion of chemical theory to include the phenomena which the old chemistry accounted for, as well as the phenomena which the new chemistry accounted for, . . . because the new chemistry in fact lost much of the ability to account for the for."³⁴⁵ phenomena the old chemistry could account Furthermore, they claimed that "the new chemistry ceased to be fundamentally interested in the problems the old chemistry took to be basic; the new chemistry instead relegated those problems to the back burner (or indeed, declared them 'unscientific'), and took as central to chemical inquiry a set of problems ('quantitative' aspects of chemical new

343_{Ibid., 68.} 344_{Ibid.}

345 Siegel, "Latest Form," 108.

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reactions) regarded as crucial less than by the old chemistry."346

But if we examine the shift that occurred from the phlogiston theory to the oxygen theory and the subsequent development of chemistry, it is hard to see that there was loss raw observational any actual of data, including observations concerning the qualities of substances. The major difference between the paradigms was the interpretation of data in terms of the explanatory causal mechanisms that ultimately held to be responsible for the observed were chemical interactions. We can agree with Doppelt that the scientific revolution in chemistry, and its subsequent development including Daltonian chemistry, did adopt the view that " 'quantitative problems concerning weight relations and proportions' were deeper, and more basic to chemical theory, than the qualitative questions addressed by pre-Daltonian chemistry."347 However, as we have already seen, Lavoisier showed that in order to understand the basic causal mechanisms responsible for chemical interactions, it was necessary to account for the weights of the component substances, both before and after chemical interactions, as well as to identify and describe the qualitative properties of the initial substances and of the products produced in chemical interactions. In many ways, Lavoisier's reliance on the balance ultimately helped the chemists of the period to

^{346&}lt;sub>Ibid.</sub>, 108-109. 347_{Ibid.}, 111.

make qualitative distinctions between various compounds and elements, such as 'fixed air' (CO_2) and oxygen. It may be the case that chemists to some degree placed attempts to explain the qualities of substances on the 'back burner,' but there is little in the historical record to show that chemistry was not concerned to account for qualitative properties when adequate explanations which were compatible with the available evidence could be developed.

What was temporarily lost during the early stages of the chemical revolution was some explanatory power, but the evidence in support of the abandoned explanations was rather thin. For example, Doppelt argues that once the phlogiston theory was rejected, chemistry could no longer account for the similarities of metals, by claiming that their common natures resulted from their all containing phlogiston, or that metals were shiny because they all were composed of some 'fiery stuff'. Furthermore, when the original four elements were done away with, it was no longer possible to account for the physical state of substances by maintaining that all liquids contained water, or that all solids were such by virtue of their containing earth, etc.

We must admit that when there is a change of this magnitude we would expect there to be some gaps in the new chemistry's ability to explain everything the old chemistry had explained immediately after its adoption. Even if we grant that a temporary loss of explanatory power may result in the early stages after a shift from one paradigm to

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another, I see no reason to suppose that this necessarily there is any loss of genuine observational implies that data, or that any actual loss may not be compensated by a larger body of data being explained by the new theory (or which it promises to explain). As has already been pointed out, what the revolution amounted to was а different interpretation of the data concerning chemical processes in the light of new observations which the old theory found difficult to explain. In addition, the decision of chemists to shift allegiance from one paradigm to another and the debate that took place between adherents of these different paradigms can be explained at least to some degree by appeal to the virtues of conservatism and modesty. However, what is crucial, is that in the long-run the justification of the new chemistry can be shown to increasingly satisfy a number of paradigm-neutral external standards.

Furthermore, we can maintain that in time, at least in the long-run, the change in the relative importance attached to the qualitative and quantitative aspects of chemistry, a change which has been described as a shift in internal standards, was justified by compelling reasons based upon external paradigm-neutral standards of evidence. It can be persuasively argued that if we actually compare the historical development of chemistry (as with other domains of science), we will see that if a new theory is to have a long life, it must in the long-run continue to grow in its ability to produce and explain more and more observational data then did its predecessor. And when we compare contemporary chemistry with that of the phlogiston theory we can see, at least in the long-run, how much this has proved to be the No one would now argue that the phlogiston theory case. explained some data in a more adequate or comprehensive way than does modern chemistry. Thus, it appears that contemporary chemistry certainly has recouped any loss of data or explanatory power that might have occurred in the short-run after the change from the phlogiston to oxygen theory. Although it is true that Doppelt's position does not rule this possibility out, he does leave one with the impression that it is still questionable whether or not even contemporary theory has recouped the data it initially lost. Thus, it can be argued that as data accumulates in support of theory, justification can become compelling as it а new superiority over its its demonstrates predecessor by providing a more thorough or comprehensive explanation of the overall evidence. Paradigm-neutral standards are crucial in determining which theory is to win out over its rivals.

Another, important external standard or virtue is 'simplicity'. Like both conservatism and modesty, simplicity is considered to be a methodologically sound strategy in science, as it also helps to insulate us from error, and aids in giving us a system of beliefs that is more manageable. This helps to ensure a science that is able to make more testable predictions than would be the case if simplicity was not preserved. There "is a premium on simplicity in any

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hypothesis, but the highest premium is in the giant joint hypothesis that is science, or the particular science as a whole."348 In other words, we "sacrifice simplicity of a part for greater simplicity of the whole when we see a way of doing so."³⁴⁹ For example, a scientist should even be prepared to favour a more complex hypothesis than some alterative, other things being equal, if by so doing one can more complex hypothesis subsume the under an alreadv established set of laws thus creating a unified and simpler science or branch than was previously possible. The external standard of simplicity thus helps to work towards the unity of science, and this virtue is also applicable to the chemical revolution.

As we will see, Lavoisier's oxygen theory was compatible with Newtonian physics and the phlogiston theory was not. It can be argued that science as a whole was simpler by virtue of adopting the oxygen theory inasmuch as the oxygen theory provided for greater unity then would have been the case if phlogiston theory had been retained. the In time. Lavoisier's oxygen theory also proved to be simpler than the phlogiston theory when it became necessary for the phlogiston theory to make a multitude of revisions in order account for accumulating observations to the which subsequently were satisfactorily embodied in the oxygen theory. In other words, the phlogiston theory became more

^{348&}lt;sub>Quine, Web Of Belief</sub>, 69. 349_{Ibid}.

complex than the oxygen theory in order for the former to retain as much generality as the latter.

However, it must be acknowledged that the evaluation of theories often involves an element of subjectivity in deciding what hypothesis or system of beliefs is simpler. In mathematics simplicity is much more clear cut than in other areas of science. Debates as to just what constitutes the most plausible hypothesis may result when it is not clear which of alternative hypotheses is the simplest. But, in spite of these difficulties the standards of simplicity and economy of ideas are still a strong quide in the pursuit of science. Often there is not a great deal of difference between modesty and simplicity. It is often hard to separate standards into neat categories as they usually overlap with one another.

conclude that We can conservatism, modesty and simplicity all are good strategies for pursuing science, because the "longer the leap, . . . the more and wider ways of going wrong. And we have seen that what recommends simplicity is that the, "the more complex the hypothesis, the more and wilder ways of going wrong; for how can we tell which complexity to adopt?"350 Thus, simplicity like conservatism and modesty, limits liability³⁵¹ and therefore is sound strategy. All three can help us to decide between

^{350&}lt;sub>Ibid.</sub>, 72. 351_{Ibid}.

two or more theories that account for the same facts, as illustrated by the chemical revolution.

Generality is another external standard which provides additional evidential warrant to support a hypothesis or theory. Generality, is concerned with the scope of а hypothesis' application. The "plausibility of a hypothesis depends largely on how compatible the hypothesis is with our being observers placed at random in the world"³⁵² and this can help to protect us from a hypothesis being confirmed merely by coincidence. In other words, the "more general the by which hypothesis is we account for our present observations, the less of a coincidence it is that our present observation should fall under it . . . to confer plausibility."353 We should also keep in mind that there might often be a trade-off between standards, such as modesty and generality. However, "generality is desirable in that it makes a hypothesis interesting and important if true."³⁵⁴ Thus we can say, that when "a way is seen of gaining great of simplicity, or generality with little loss great simplicity with no loss of generality, then conservatism and modesty give way to scientific revolutions."³⁵⁵

As we shall see, the discovery and characterization of oxygen, and the methods that were used to clarify the various reactions in which it participated, served as an example and

^{352&}lt;sub>Ibid.</sub>, 74. 353_{Ibid}.

^{355&}lt;sub>Ibid., 75.</sub>

model which in the course of ensuing decades were successfully applied to permit the identification of many other elements and compounds and the explanation of their interactions, thereby satisfying the virtue of generality.

Conservatism lays emphasis on the lack of disagreement or conflict between a new hypothesis and prior beliefs, so that the need for theoretical revision is minimized. But this is only one aspect of the ways in which various components of our 'web of beliefs' may relate to one another. The degree to which a new observation or hypothesis brings prior data and concepts into better agreement is a property that has been described as coherence.³⁵⁶ A distinction may be drawn between internal and external forms of coherence. These

two different types of coherence . . . are both described rather nicely by physicist Richard Feynman's statement . . 'I know that the hypothesis is a good one if it ties together and makes sense out of stuff I knew earlier but couldn't quite understand.'. . The tying 'together' that Feynman is talking about is of course coherence, and it can be two kinds. . Internal tying together refers to the coherence within a specific field which is contributed by a new hypothesis; external tying together refers to the coherence which obtains between the specific field and other fields.³⁵⁷,

Coherence also includes the contribution to this consistency which is made by the new hypothesis. Thus, a new theory is most acceptable if it is both internally consistent and externally fits in well with other aspects of

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³⁵⁶ Gale, <u>Theory of Science</u>, 223.

³⁵⁷ Ibid., 224.

its own discipline as well as with the theoretical framework of other fields of investigation and the background concepts that comprise prevailing scientific tradition.

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Given that "the historical facts of discoveries clearly exhibited . . . both conceptual innovation and conceptual conflict³⁵⁸ we must account for both of these factors in doing justice to 'sound' scientific practice and the historical record. Α specific decision about the acceptability of a certain hypothesis "requires that judgements be made concerning the amount of consistency as opposed to the amount of conflict."359

However, we should point out, that not all aspects of a particular paradigm are of equal importance. A significant aspect of scientific paradigms "is that the concepts are structured into layers."360 For example, some "hypotheses are more hypothetical - believed more tentatively than laws; and principles are held to be more accepted and necessary than laws; and so on."³⁶¹ Thus, it is necessary to keep in mind that when one encounters questions of consistency, when for example a new hypothesis is in conflict with some other elements of a broader theory, it is always necessary to look at which levels of the paradigm are consistent with the new hypothesis and which levels conflict with it in order to decide just what should be rejected and what should be

³⁵⁸ Ibid., 226. 359 Ibid. 360 Ibid., 224. 361 Ibid.

retained. Hypotheses may be viewed more favourably even if contrary to other hypotheses which are under they are evaluation if at the same time are in better agreement with the more basic laws and principles.³⁶² Presumably further investigation will be required to resolve the issues which remain in dispute. Thus, we can conclude that a hypothesis is tenable if it is consistent with the prevailing paradigm. Moreover, in "cases where conceptual conflict occurs between the prevailing paradigm and the new hypothesis, deeper-level coherence can make up for conflicts between higher-level hypothesis."363 and the new For concepts example, Lavoisier's hypothesis postulated that in the process of calcination the metal combined with something in the air, and this was able to account for the weight gained by the calx. This hypothesis conflicted with the phlogiston theory which claimed that in calcination phlogiston was given up from the metal in order to form the calx, and this suggested that phlogiston had a negative weight. However, this aspect of the phlogiston theory contradicted Newton's law of universal gravitation. Thus see, that although we Lavoisier's hypothesis conflicted with the phlogiston theory at a higher level, it was in agreement with Newton's law at a deeper level and therefore gained favour. Of course, the preferred situation is one in which both internal and external types of consistency are completely satisfied.

^{362&}lt;sub>Ibid., 225.</sub> 363_{Ibid., 226.}

Another type of coherence is 'predictive coherence'. This kind of coherence takes place when the facts of some "discovery were in no way logically inconsistent with the body of science"³⁶⁴, but indeed could be anticipated as a consequence of the theory being proposed. Thus, the "function of coherence in this kind of case is easy to understand, and presents no real problem. . . . The new discovery coheres simply because it is, for all practical purposes logically predictable from the paradigm."365

realize that the concept It is important to of coherence involves more than consistency between the separate components of a theory but also includes its agreement with the empirical evidence which is necessary to provide support for the thesis in question, such as observational data or identification any objective entity postulated of to participate in its mechanisms. In order for widely held beliefs to add justification for theory acceptance, those empirical evidence beliefs must have and/or logical significance in order for consistency or coherence to count as justification for a theory. Thus, counter to Laudan, the fact that a hypothesis is consistent with well entrenched beliefs, such as religious precepts, is not in itself adequate for justified acceptance, even though it might at one time have served for some as a sufficient criterion for actual acceptance. We cannot lose sight of the need for

364_{Ibid.}, 223. 365_{Ibid}.

adequate empirical evidence as the crucial prerequisite to justify acceptance of any theory.

For example, Lavoisier was faced both with the question of coherence and with the ontological question of whether or not he could isolate and identify an entity that had the particular properties that his hypothesis postulated. We can understand that the ontological question, as applied to establishing hypotheses, similarly "involves questions about objects and properties."³⁶⁶ In other words, the question in "its most essential formulation, . . . asks . . . 'Does the hypothesis commit me to any particular beliefs about objects or properties in the universe?'"367 Another aspect, in the justification of a theory or hypothesis is whether or not the "new objects or properties . . . can be fitted into consistent causal pictures of the observational world."368 We can assert that the acceptance of a new hypothesis was more justified when it is the case that the objects it posits to exist are similar in kind to our ordinary conceptions of objects, such as that they have mass, can be located in space and in time, and can be isolated from other objects and identified as distinct entities. In other words, a general axiom of human cognition is that it must "reduce the flux of perception - the never-ending sequence of ever-changing sights, sounds, feels, smells of the physical world - to a stable order of objects. . . . via its use of the concepts of

^{366&}lt;sub>1bid.</sub>, 209. 367_{1bid.} 368_{1bid.}

objects existing through time, stable in their causal interactions."³⁶⁹

general, science begins with observations of In phenomena which are "precisely formulated, and thought is turned to the question 'What sort of an object, with what sorts of specific qualities, could be responsible for the features I observe?"370 Thus, given that "Lavoisier brought back reports of a strange new substance which was contained in the air,"³⁷¹ we could ask whether or not such an object with the requisite properties could be isolated and identified? One answer to this question is that, in keeping with the virtue of modesty, we could "catalog our various previous kinds of objects in an effort to find something whose behaviour is at least similar to what is going on in the situation at hand."³⁷² To reason from "well-known, past of objects to present unfamiliar situations theories requiring new sorts of objects and behaviors, is called 'analogy' or 'modelling.'"³⁷³ And furthermore, the "new construct which we invent to explain the wondered-about observations is called an analogy or model. . . Then, once our concepts about the model are relatively developed . . . we go out into the world (i.e., set up experiments in the lab) and attempt to find and bring back alive one of the new

³⁶⁹ Ibid., 210-211. 370 Ibid., 211. 371 Ibid., 210. 372 Ibid., 210. 373 Ibid., 212.

beasts postulated in the model."374 In other words, "the model - the new hypothesis - refers to a 'hypothetical mechanism' which at least has the status of a candidate for existence."375 If a hypothetical object seems necessary to provide a good explanation for a candidate theory and if ultimately it offers the probability that it can be captured, then the hypothesis stands a good chance of being acceptable. Finally, if the object is actually observed, then the new entity can be said to exist and the hypothesis is at that point strengthened. Thus, in this partial account of the scientific process which began the chemical revolution, it can be claimed that two important goals of science were For example, one was achieved "since the satisfied. hypothetical mechanism was originally invented in order to be responsible for producing the wonder, the hypothesis explains. That is, it tells us why and how the surprising situation occurs"³⁷⁶ Furthermore, "given that it explains well, the concept of the hypothetical mechanism will motivate researchers to attempt to find it. Finally, if the new beast is found and brought back alive, then certainly the second science, prediction and control, will goal of be satisfied."³⁷⁷ As we will see, oxygen was ultimately isolated and clearly identified as a separate entity. Unlike oxygen, phlogiston was postulated, but never isolated and identified

374_{Ibid}. 375_{Ibid}. 376_{Ibid}. 377_{Ibid}. as a distinct entity from other substances. In one of the various versions of the phlogiston theory, phlogiston was identified with 'inflammable air' (hydrogen). But this was satisfactory suggestion because'airs' "rich а in not phlogiston were supposed to inhibit combustion, yet pure When things burn they are supposed to phlogiston burns! release phlogiston, so it would seem when phlogiston burns it is released from itself!"³⁷⁸ Thus, this hypothetical entity not only failed to display the predicted properties, but instead gave rise to apparent contradictions, thereby failing the test of internal consistency or coherence.

Let us now pursue our comparison of the phlogiston and oxygen theories in order to ascertain how consistent each was with the known facts of chemistry, as well as how each chemical theory is consistent with other conceptual schemes, such as the physics of the time. For example, not only was Lavoisier's oxygen hypothesis not internally consistent with the prevailing phlogiston paradigm "and indeed contradicted it, but at the same time, exhibited an extremely deep level of coherence to the physics of his time. Thus it was that oxygen, considered as a substance with positive gravitational mass, fit very closely to the fundamental law of gravitation

³⁷⁸Alan Musgrave, "Why Did Oxygen Supplant Phlogiston? Research Programmes In The Chemical Revolution," in <u>Method</u> <u>And Appraisal In The Physical Sciences: The Critical</u> <u>Background To Modern Science, 1800-1905</u>, ed. Colin Howson (New York: Cambridge University Press, 1976): 190.

in physics while phlogiston with its negative weight could not be matched into the physical paradigm."³⁷⁹

Nevertheless, it has been suggested that philosophers of science "who think that the evidential support of a theory depends solely upon the timeless logical relations between theory and evidence will have to say that 1784 phlogiston theory had as much evidential support as 1784 oxygen."³⁸⁰ For example, it can be said that both theories "explained the main facts about combustion and calcination (and both faced some outstanding anomalies)."³⁸¹ However, this was not the decision that was made by "chemists of the late eighteenth century."382 This is because there were important reasons for maintaining the superiority of the oxygen theory over the phlogiston theory. One reason is that scientists recognized the fact that "phlogiston theory merely accommodated known facts, many of which had been discovered by testing predictions made within the oxygen programme."³⁸³ Moreover, scientists could see that the "1784 phlogiston theory was inconsistent with the previous version, and marked a return to the imponderable [or weightless] phlogiston of Stahl."³⁸⁴ Thus, we can "contrast . . . this incoherent development with the smooth development of the various versions of the oxygen programme."³⁸⁵ In addition, the oxygen theory "developed

379Gale, A "Theory of Science, 225-226.
380Musgrave, "Why Did Oxygen Supplant Phlogiston?" 205.
381Ibid.
382Ibid.
383 Ibid.
384Ibid.
385Ibid.

coherently and each new version was theoretically and empirically progressive, whereas after 1770 the phlogiston programme did neither."³⁸⁶

In considering what external standards of evidential warrant there are for the justification of theory choice, it is important to realize that more is involved in defining adequate standards for theory evaluation than simply being able to account for the facts. Let us recall Lavoisier's use of quantitative methods in his examination of the calcination of mercury on the surface of a liquid in a closed container (see figure 1., page 64) in order to see how the application of at least some of the paradigm-independent standards of evidential warrant and the use of logical analysis could provide a good test of the merits of the phlogiston theory.

Before, proceeding with our analysis of this particular experiment, it will be profitable to first examine some of the conditions that are necessary for an experiment to be a 'GOOD TEST' of a hypothesis or theory.

In the first place,

A GOOD TEST of a theoretical hypothesis is an circumstances organized set of involving the HYPOTHESIS, INITIAL CONDITIONS, and a PREDICTION. components These must satisfy following the conditions.

(1) The prediction is logically DEDUCIBLE from the hypothesis together with the initial conditions.

(2) Relative to everything else known at the time (excluding the hypothesis being tested), it must be IMPROBABLE that the prediction will turn out to be true.

386_{Ibid}.

(3) It must be possible, at the appropriate time, to VERIFY whether the prediction is in fact true or not.

In a nutshell, a good test of a theoretical hypothesis requires initial conditions and а prediction which is (1) deducible, (2) improbable, and (3) verifiable.

In justifying theoretical hypotheses, scientists follow a SIMPLE INDUCTIVE RULE.

If the prediction is successful, the hypothesis is

If the prediction fails, the hypothesis is refuted.³⁸⁷

Furthermore, it is important to point out that the

simple inductive rule . . . does not guarantee a true conclusion, but it does make it very likely that the conclusion reached is in fact true. What we have yet to see is the reasoning behind the rule set out explicitly in argument form with premises and conclusion.

If the experiment is a GOOD TEST of the hypothesis, then the prediction can be deduced from the hypothesis together with appropriate initial conditions. 388

Given that, "'H' stands for Hypothesis, 'IC' for initial conditions, and 'P' for the prediction,'"³⁸⁹ our first condition for a good test is "Condition 1: If (H and IC), then P."³⁹⁰ The next major requirement of a

GOOD TEST was that the prediction be something known to be IMPROBABLE when the test is designed. In order capture this requirement in a conditional to statement, we must explicitly take account of the knowledge used to justify the claim that the prediction is indeed improbable. Such knowledge is often quite diverse. Let us therefore abbreviate all this knowledge by the letter 'B,' for BACKGROUND knowledge. The required conditional statement, then

³⁸⁷ Giere, <u>Understanding Scientific Reasoning</u>, 105. 388_{Ibid., 107.} 389_Ibid. 390_{Ibid}.

may be written as: Condition 2: If (Not H and IC and B), then very probably not P.³⁹¹

It needs to be pointed out that 'not H' in condition 2 is not known to be the case. Condition 2 is intended to impose the requirement that if the hypothesis does not hold, then the occurrence of P is unlikely. If P was likely to occur, regardless of whether or not H was the case, then P's verification would offer little if any support for the hypothesis being tested.

Let us now apply this model to the experimental results of Lavoisier's test where mercury oxide was heated in a closed jar, and then determine just what conclusions it would be reasonable to draw from the results. We can characterize the experiment thus:

H: The experimental setup roughly fits a phlogiston model of combustion.
IC: The various facts describing the experiment are as outlined above.
P: The remaining mercury and red powder together weigh LESS than the original sample of mercury alone. And the level of the liquid inside the jar should go DOWN.

But in fact the reverse of the anticipated result was observed. The solid residue weighed more than the starting material and the fluid level in the jar rose as the volume of the gas it contained decreased. Thus, the prediction failed to occur. Applying the simple inductive rule we conclude immediately that the predicted outcome failed to occur, weakening the theory upon which it was based and in fact

³⁹¹Ibid., 107-108. **392**Ibid., 116.

rendering it highly improbable. "To construct the ARGUMENT justifying our conclusion, we combine Condition 1 with the result of the experiment as follows:

If (H and IC), then P. Not P and IC. Thus, Not H. "393

An alternative for 'IC' is the 'auxiliary hypotheses' which together with the hypothesis under test entail a prediction, or which may have been tacitly assumed to be a part of the background conditions. Before continuing with our discussion of what constitutes a 'good test' of a new hypothesis, we must acknowledge that the preceding model is in some respects an over-simplification of the scientific For example, the auxiliary or initial conditions of process. any experiment are not always known to be the case. It is important to bear in mind that no theory, laws or hypothesis entail any predictions without the augmentation of some conditions that are believed to describe the initial state of and where the hypothesis under test is only one the system, the components of the system. Scientists must of make assumptions as to which conditions actually obtain in а particular experiment, along with the hypothesis under test. Thus the hypothesis alone entails no specific outcomes or predictions without the further addition of auxiliary When a prediction made by a hypothesis and its conditions. initial conditions fails to occur we are forced to make a revision in either the hypothesis under test or in the other conditions. The negative result of the experiment doesn't tell us where to make the revision. We could even question whether or not the measurement of the experimental result was subject to error and that this might have been responsible for failure of the prediction to take place.

The degree to which auxiliary conditions are known may vary depending upon the particular case. The conditions that are postulated are often held to be simplifications of the auxiliary conditions that actually pertain, and thus in this respect may be said to be questionable or false. Some of the background knowledge or conditions might also be more firmly established than other auxiliary hypotheses. These conditions would be less subject to revision than others assumed to be operative in the experiment, when the prediction made by the hypothesis fails to occur. In addition, it is often assumed that other auxiliary conditions may be so insignificant in their influence on an experiment that they may be disregarded or deemed irrelevant to the situation at hand. We will return to this question when we discuss the virtue of 'refutability'. Let us now continue with our discussion of what constitutes a 'good test' for a hypotheses or theory and how it pertains to the phlogiston theory.

One conclusion that we can be drawn is that if

the PHLOGISTON THEORY includes the general hypothesis that ALL combustion-like processes fit phlogiston

models, then Lavoisier's experiment refuted the theory as well. But no defender of the phlogiston theory interpreted it so broadly. What in fact happened is that members of the THEORETICAL TRADITION based on phlogiston models MODIFIED their models to accommodate Lavoisier's results. This is а scientifically sound strategy for dealing with unwelcome facts. But the strategy does not pay off unless these new models yield justifiable hypotheses. Merely coming up with a revised model that fits the known results is not enough. Justification requires full-fledged tests that satisfy condition 2 as well as condition 1. Phlogiston theorists were not able to do this successfully.

Some of the revised phlogiston models attributed a NEGATIVE MASS to phlogiston. This put phlogiston chemists at odds with Newtonian physicists, for whom all particles have POSITIVE mass. Some models postulate that phlogiston is lighter than air and thus exhibits a buoyancy effect-like the bladder in a fish or the hot air balloons that were then popular in France. Other models specified that the escaping phlogiston is replaced by water vapor, which has greater mass and greater volume than phlogiston. This accounts both for the increased mass of the mercury sample and the decreased volume of the air. But none of these models were justified in applications to further experiments. They kept on being refuted. By 1789 the phlogiston tradition was effectively dead, even though Priestley himself defended it in a text published as late as 1796.

It can be concluded that, the " objectivity of SCIENCE, imperfect as it is, is not a function of the objectivity of SCIENTISTS. It is a function of the 'logical' rules of the game. These are embodied in the specification of a good test, and thus in Conditions 1 and 2."³⁹⁵

Let us recall (page, 42) Doppelt's contention that, at least in the short-run, new theories are not more reasonable to accept than the ones they replace and are not in fact accepted by most scientists on the positivist criterion of increasing cumulative empirical adequacy. In reply to

³⁹⁴Ibid., 116-117.

^{395&}lt;sub>Ibid.,</sub> 117..

Doppelt, it can be argued that what has led particular scientists to abandon one paradigm and adopt another in the early stages of the transition between them has historical interest but is of no epistemic importance and properly belongs to the domain of psychology and sociology, and not to the philosophy of science. For example just what led Lavoisier to choose to pursue an alternative to the phlogiston theory has at best marginal epistemic importance. And whether or not it is rational for an individual scientist to cling to an old theory while it is being undermined by new observations, as Priestly did with respect to phlogiston, also is beside the point. However, what is important is whether or not the decisions to accept new paradigms at the made by the bulk of a scientific time when they were such as the choice of most chemists to abandon community, the phlogiston theory in favour of oxygen, were in fact justified on the basis of shared external paradigm-neutral standards which apply uniformly to successive paradigms, and which transcend the internal standards of particular paradigms.

Another lesson to be drawn from the above analysis of Lavoisier's experiment in which he decomposed the oxide of mercury is that, the "rules of the game ensure that the harder one tries to get a good justification, the greater the risk of refutation-unless the hypothesis is indeed on the right track."³⁹⁶

396_{Ibid}.

This leads us to another key external standard, 'refutability'. Refutability requires that "some imaginable event, recognizable if it occurs, must suffice to refute the hypothesis. Otherwise, the hypothesis predicts nothing, is confirmed by nothing, and confers no earthly good beyond perhaps a mistaken peace of mind."³⁹⁷ Again, we must be careful not to over-simplify the scientific process and acknowledge Pierre Duhem's point that theoretically one can maintain that just about any hypothesis "can be unrefuted matter what, by making enough adjustments no in our beliefs."³⁹⁸ This was the course of action pursued by those who persisted in their support for the phlogiston theory after it had already been abandoned by most chemists. However, in spite of the correctness of Duhem's assertion, we must bear in mind that saving a hypothesis from being refuted will bv experimental counter-evidence involve some In other words, the degree to which a incremental costs. hypothesis can be said to be refutable "is measured by the cost of retaining the hypothesis in the face of imaginable events."399 We must measure the degree in terms of "how dearly we cherish the previous beliefs that would have to be sacrificed to save the hypothesis. The greater the sacrifice the more refutable the hypothesis."⁴⁰⁰ Thus, we can draw the conclusion that in science there are often times when saving

³⁹⁷ Quine, Web of Belief, 79.

³⁹⁸ Thid.

³⁹⁹ Thid

⁴⁰⁰ Ibid.

a hypothesis would require too great a cost by forcing us to give up or change many of our firmly held beliefs which had been logically sound, well founded according to our external standards, and well confirmed by the evidence. This would demand too large a sacrifice for us reasonably to make. For example, we would not want to sacrifice contemporary physical theory in order to retain some hypothesis that was not confirmed by experimental test (such as the recent effort to salvage the notion of 'cold' fusion in spite of contrary evidence), because the cost of maintaining such a hypothesis would deprive us of a theoretical system with much explanatory power.

In general, theories that have enjoyed much success in both explanatory and predictive power have proved to be highly resistant to falsification. But, as we have already pointed out, when a theory does encounter contrary-evidence we are faced with a choice of abandoning the hypothesis, ignoring the outcome by attributing it to experimental error, making some revision of the hypothesis, or modifying the original auxiliary conditions. We must acknowledge that if a particular hypothesis has been highly successful in making confirmed predictions we would be less justified in abandoning it than might otherwise be the case. And, this is especially true when there is no other alternative hypothesis capable of, or potentially promises to which is either explain both the old and the new data. Without an alternative theory, scientists would effectively abandon

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their field of science if they rejected a well-tested theory on the basis of an anomaly or experimental counter-evidence, no new equally plausible theory or paradigm when is available. But, in spite of what has already been said, this does not mean that the counter-evidence can simply be ignored. In fact scientists must try to find a satisfactory means of accounting for this counter-evidence, even in the light of proposed alterations to the auxiliary conditions of the hypothesis. In addition, there might be varying degrees of evidence in favour of certain conditions pertaining to the situation, so that revision is not a purely arbitrary choice, or one without limits. And it is for this reason that the virtue of refutability still retains its teeth in requiring that there be some event(s) that could ultimately over-throw a new hypothesis unless it can be saved by the adoption of reasonable auxiliary hypotheses. However, revisions of the auxiliary hypotheses must eventually be justified by some further evidence showing that at least some aspect of one's assumed IC's were in some respect mistaken. We need to be careful in making this stipulation. We are not claiming that theories must be highly falsifiable, but that well all established theories are those which have been adequately tested and found to be resistant to falsification in contrast to theories that have not yet established themselves as the result of their past success.

At this point it will be profitable to compare the history of Newtonian physics and its attempt to account for the astronomical deviation of Uranus' orbit within its theoretical framework, with the attempt which was made to introduce the concept of negative weight in order to reconcile the weight gain during calcination with the theoretical system which postulated the loss of phlogiston in this process.

When the orbit of Uranus was found to deviate from the course predicted by Newtonian physics, the theory was subject either to 'revision or refutation'. However in this instance conservatism prevailed as "one is loath to revise extensively a well established set of beliefs, especially a set so deeply entrenched as a basic portion of physics."401 Thus, given the fact that "Uranus had been observed to be as much as two minutes of arc from its calculated position, what was sought was a discovery that would render this deviation explicable within the framework of accepted theory. Then the theory and its generality would be unimpaired, and the new complexity would be minimal."402 Given the success of Newton's laws of gravitation in predicting the orbits of the other planets in the solar system, some counter-evidence such as Uranus's deviation in its predicted orbit would hardly falsify a theory that had so much explanatory and predictive As we now know, an additional planet, Neptune, success. was the discovered cause for Uranus' deviation from its predicted orbit.

401_{Ibid., 77.} 402_{Ibid.} However, before the discovery of Neptune was made, it "would have been possible in principle to speculate that some special characteristic of Uranus exempted that planet from the physical laws that are followed by other planets. If such a hypothesis had been resorted to Neptune would not have been discovered; not then, at any rate."⁴⁰³

However, there may be good reasons not to evoke this type of hypothesis in order to accommodate counter-evidence. At this stage we can distinguish between two types of 'ad hoc' hypotheses. An ad hoc alteration of some particular IC may be reasonable in view of counter-evidence. On the other hand, some other kinds of ad hoc hypotheses may be much less reasonable to make, as for example, when some kind of special or unique force is postulated to save a theory in anomaly. Revision of one's spite of an IC is an unreasonable ad hoc amendment when such forces only apply to that specific situation. Thus, some 'ad hoc' hypotheses are more reasonable to make than others. It may be sensible to assume that the initial conditions were mistaken in the light of counter-evidence to an otherwise successful theory. In these circumstances one may be able to save a particular hypothesis or theory from falsification. As Putnam points out, "an alteration in one's beliefs, may be ad hoc without

403 Ibid., 77-78.

being unreasonable. 'Ad hoc' merely means 'to this specific purpose'."404

The deviation of the orbit of Uranus from that expected in accordance with Newton's law of universal gravitation and the composition of the solar system insofar as it was known at that time led astronomers to suggest an addition to their set of auxiliary assumptions (Giere's initial conditions), namely the presence of another as yet undetected planet taken together these premises enabled them to predict the orbit which this planet should follow so that they would know where look for it. A hypothesis that accounted for the to deviation of the orbit of Uranus by postulating that this particular planet was not subject to the laws that the other planets in the solar system are subject to, is the type of ad hoc hypothesis that can be said to be unreasonable because this type of hypothesis lacks both the virtues of simplicity and generality. Thus, we can draw the conclusion that ad hoc hypotheses of this pejorative type are those which "purport to account for some particular observations by supposing some very special forces to be at work in the particular case at hand, and not generalizing sufficiently cases."405 bevond those In these circumstances, the likelihood of obtaining further evidence which can offer support for the new auxiliary hypothesis is very remote.

 ⁴⁰⁴ Hilary Putnam, "The 'Corroboration of Theories," in Scientific Revolutions, ed. Ian Hacking (Oxford: Oxford University Press, 1981): 76.
 405 Quine, Web of Belief, 78.

However, the undesirability of adopting such a hypothesis varies in degree. For example, the "extreme case is where the hypothesis only covers the observations it was intended to account for, so that it is totally useless in prediction. Then also it is insusceptible of confirmation, which would come of our verifying its predictions."⁴⁰⁶

In fact, the hypothesis that phlogiston had negative weight, which was introduced in order to account for the gain in weight during calcination is a fairly extreme case of an ad hoc hypothesis that lacks any general application beyond the situation for which it was devised to account. As already mentioned, we should be suspicious of just one substance exhibiting this special property without any independent evidence for this supposition outside of the fact that if true the anomalous weight gain in calcination would no longer count against the theory. Moreover, even if we were willing to allow this move in principle, it is contradicted by those instances in which phlogiston apparently cannot be assigned a negative weight. Furthermore, the fact that Lavoisier was the weights of all of the components able to account for both before and after various chemical reactions, and the fact that the initial and final weights of the reactants were equal in all testable situations, offers strong evidence against the notion of any substance having a negative In other words giving phlogiston a negative weight weight. does not provide an intelligible mechanism to explain the

406_{Ibid}.
gain in weight during calcination. In general, it can be maintained that a hypothesis "strikes us as giving an intelligible mechanism when the hypothesis rates well in familiarity, generality, simplicity."⁴⁰⁷ Moreover, we "attain the ultimate intelligibility of mechanism, no doubt, when we see how to explain something in terms of physical impact, or the familiar and general laws of motion."⁴⁰⁸

In general it can be argued that it is easier to refute a hypothesis than to justify it. However, to some degree this may be an over simplification of the scientific process. scientist might try to save a hypothesis when its Α prediction was not realized during experiment by modifying or revising the initial conditions of the experiment. In theory, nothing is immune from revision, although in practice the resulting revisions must result in a working theory that is not continually refuted by further experiment, and the also be able to make further fruitful should theory predictions that can be experimentally confirmed. In some cases it would be absurd to make wholesale revisions to save a hypothesis as this would deprive us of much explanatory power. The more revisions required to save a hypothesis or theory, the more likely the theory will lose its ability to make further predictions that can be experimentally verified. Thus, a theory might collapse under the weight of the necessary revisions. In the case of the phlogiston theory,

407_{ibid}. 408_{Ibid}. as we shall see, it can be argued that so many revisions became necessary that it was progressively less able to accommodate new data and make profitable predictions, and as a result lost much of its ability to explain the relevant phenomena.

Another questionable type of hypothesis is one that is evoked "to save some other hypothesis from refutation by systematically excusing the failures of its predictions."⁴⁰⁹ Like an ad hoc hypothesis, this type of saving hypothesis "shares the traits of insusceptibility of confirmation and prediction."410 uselessness in We have seen that phlogistonists attempted to save their theory from being refuted by using additional post hoc hypotheses to insulate the theory from being refuted. However, the addition of such saving hypotheses produces a theory that can no longer make accurate and testable predictions, which as a result must deprive it of evidential warrant. If this was not the case it would be possible to salvage almost any theory. An infinitely elastic theory would be entirely useless and could not explain anything in terms of an intelligible mechanism. What could explain anything, explains nothing.

The history of the chemical revolution shows that adherents of the phlogiston theory repeatedly revised their system in an effort to keep it compatible with the observational data and the various anomalies that it faced.

409_{Ibid}. 410_{Ibid}. Under the phlogiston theory some "well-known facts about combustion . . . were not explained: why does combustion soon cease in an enclosed volume of air, and why is the volume of air reduced by it; why won't things burn at all in a vacuum? Worse still, other well-known facts seemed to refute the theory; why, if calcination is the release of phlogiston, do calces weigh more then the original metals?"411 We might try to solve the first of these problems by adding auxiliary hypotheses. Thus, it was argued that conditions or phlogiston "must be carried away from a combustible by the air, and a given volume of air can only absorb a certain amount of it"⁴¹² in order to explain the observed facts that "nothing will burn in a vacuum, and combustion soon ceases in a confined space."⁴¹³ The problem of why the volume of air is reduced after absorbing phlogiston could be resolved by assuming that "air saturated with phlogiston ('phlogisticated air') takes up less room than ordinary air (just as cottonwool saturated with water takes up less room than ordinary cotton-wool)."414

However, the weight gain in the formation of a metal calx still proved to be a problem for the theory that phlogiston is given off during calcination. But again, as an example of the Duhem thesis, phlogiston theory alone "does not entail that calcination will lead to a weight loss. . .

413_{Tbid}.

⁴¹¹Musgrave, "Why Did Oxygen Supplant Phlogiston?" 188. **412**Ibid.

^{414&}lt;sup>-</sup>Ibid.

To derive such a prediction we need the following additional premise: phlogiston has weight, nothing weighty is added to the metal as it calcinates, and if something weighty is removed in the process, and nothing weighty added, then the result will weigh less than the original."415 It is clear "observed weight increase contradicts that the the phlogiston theory with these additional conjunction of premisses."416 Lavoisier solved this problem by rejecting the phlogiston theory, but this was not the only option open to scientists. As we have already mentioned one solution to the problem was to give phlogiston a negative weight, although few serious scientists found this a desirable option Another means of solving the puzzle was to to choose. suggest as Boyle had in 1673 "that the weight of calxes was augmented by 'fire particles' "417 that were somehow absorbed into the 'pores' of the calx while it was losing phlogiston. As early as 1630 Rey had proposed that the increase in weight "comes from the air, which in a vessel has been rendered denser, heavier, and in some measure adhesive, by the vehement and long continued heat of the furnace: which air mixes with the calx,"418 just as sand becomes heavier on absorbing water. According to Priestley, this role could be played by the 'phlogisticated' or 'fixed' air formed during calcination.

415_{Ibid}. 416 Tbid.

- 417 Ibid., 189.
- 418_{Tbid}.

However, in 1772 Lavoisier considered it unlikely that in calcination "the 'fixing' of a quantity of air is to explain both the burning and the weight increase" 419 and furthermore, carried out additional experiments that would seem to make it more and more difficult to account for the weight gain of the calx by some type of augmentation from the outside. For example, he tested "his theory . . . against which been adopted Bovle's theory, had by some phlogistonists. On Boyle's theory, if a metal is calcinated in a closed container the weight increase comes from outside the container- on Lavoisier's, it comes from inside the container."420 However, unlike Lavoisier, Boyle "had not weighed the entire container and its contents before and after the calcination, but only the metal and the calx."421 According to Musgrave, Lavoisier had discovered that there was "no overall weight increase: what augments the calx must come from inside the container. This was a success for Lavoisier, and a defeat for one version of phlogistonism."⁴²²

One other criterion of a plausible hypothesis is the precision with which key terms or concepts are defined. However, in order to "preserve its [the phlogiston theory's] coherence, phlogiston has been rendered a vague concept, one which cannot satisfy the strict demands of scientific definition."⁴²³ For example, one of the problems for the

⁴¹⁹ Ibid., 191. 420 Ibid. 421 Ibid., 191. 422 Ibid., 191-192. 423 Gale, <u>Theory of Science</u>, 250.

phlogiston theory was that the concept of 'phlogiston' itself was not precisely defined, and was even given contradictory properties in order to make adjustments for the failure of the theory. The vagueness of its definition, which allowed phlogiston to be given contradictory properties, made the phlogiston theory 'logically inconsistent,' which ultimately counted against its being coherent. In other words, "a fundamental and mortal sin against the principle of coherence"⁴²⁴ is that:

"Chemists have made a vague principle of phlogiston which is not strictly defined, and which in consequence accommodates itself to every explanation into which it is pressed. Sometimes this principle is heavy and sometimes it is not; sometimes it is free fire and sometimes it is combined with the earthy elements; sometimes it passes through the pores of vessels, sometimes they are impenetrable to it. . . It explains at once causticity and non-causticity, transparency and opacity, color and the absence of color. It is a veritable Proteus which changes its form every minute."⁴²⁵

One of Lavoisier's main points in his 1783 memoir is

that

"phlogiston apparently has contradictory properties, e.g., weight and no weight. That is, phlogiston is '(Wp. - Wp).' Second, not in itself but as an explanatory concept, phlogiston implies - i.e., produces - contradictory properties in observable substances it is involved with."⁴²⁶

^{424&}lt;sub>Ibid</sub>.

⁴²⁵ Antoine L. Lavoisier, "Reflections on Phlogiston, Develop the Theories Serving to of Combustion and Calcination," in Oeuvres de Lavoisier. Tome II. Memoires de <u>Chimie et de Physique</u>. 1862, 640, cited by Musgrave, "Why Did Oxygen Supplant Phlogiston?" 203. **426**Gale, <u>Theory of Science</u>, 250-251.

In some cases contradictory properties (such as anomalies) can lead to a search for an explanation which in time may allow the contradictory properties to be reconciled. When this does not prove to be possible it becomes necessary to abandon the theory entirely unless an appropriate revision into bring it agreement with, the can apparently contradictory observations. Thus, in his argument Lavoisier is utilizing "a tried and true rule of modern philosophy and logic of science that if a concept explains in the same way both a property and its opposite, then the concept is unacceptable."427

These considerations indicate that precision is indeed an important external standard of science. Precision also "conduces to the plausibility of a hypothesis. It does so in an indirect fashion. The more precise a hypothesis is, the more strongly it is confirmed by each successful prediction that it generates. This is because of the relative improbability of coincidences."⁴²⁸

In relation to Lavoisier's oxygen theory, his continued reliance on the balance in his chemical experiments added precision to his theory's predictions and to his experimental results that far outstripped the precision of the phlogiston theory's predictions and experimental results. This is because precision "comes mainly with the measuring of quantities"⁴²⁹ and since Lavoisier was able to predict and

⁴²⁷ Ibid., 251. **428** Quine, <u>Web of Belief</u>, 98. **429** Ibid., 99.

account for all the weights, volumes etc., of the various substances both before and after a chemical reaction he gained a precision that was unequalled by advocates of the phlogiston theory.

In his rebuttal to Siegel's criticism, Doppelt has argued that good reasons "can be brought to bear on the different standards internal to rival paradigms because the exponents of each paradigm recognize that certain problemsolving capacities it lacks accrue to the other only if the latter's standards are embraced."430 In support of this view, Doppelt asserts that if "the pre-Daltonian can see that if he embraced the standards of the new Daltonian chemistry, certain achievements result which are lacking in his own research program. . . . These achievements constitute good reasons for accepting the standards implicit in the new quantitative model of chemistry."431 However, even after acknowledging the problem-solution successes of its rival paradigm, the new paradigm's acknowledged successes "need not be rationally compelling to the pre-Daltonian, because the different achievements made possible by his (pre-Daltonian) standards of chemical theory count as more important (better, more compelling reasons) then the Daltonian achievements relative to his standards."432

However, it can be argued that the forgoing argument fails to be convincing because both phlogistonists and their

⁴³⁰ Doppelt, "Reply to Siegel, 121. 431 Ibid. 432 Ibid.

adversaries were interested in essentially the same major problems, such as the mechanisms responsible for combustion calcination and respiration. To the extent that the new chemistry did tackle other problems there were good and compelling reasons to expect that the solution of these problems would provide a deeper understanding of chemical properties and processes. One may admit that the oxygen immediately account theory could not for important properties of metals, such as why they were shiny. However, it can be argued that metals have many properties in common as well as being shiny, like their ability to conduct electricity, and to react with acids to release hydrogen as well as being malleable. It would be strange if all these properties could be explained by the common possession of some hypothetical substance which to this point it had not yet been possible to identify and isolate. In time through advances in both physics and chemistry it became possible to provide good explanations for all of these as well as other properties of metals. Thus, in the long-run chemistry can be shown to have recouped any temporary losses it had suffered in the early going.

It also is important to discuss the nature of confirmation and refutation and the crucial role that external standards play in determining just how hypotheses are confirmed or refuted. In the first instance, "no matter how much data we have there will still be many mutually

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incompatible hypotheses each of which implies those data."433 Thus it is clear that what "confirms one hypothesis will confirm many; the data are good for a whole sheaf of hypotheses and not just one."434 In general without some further criteria of theory choice, theories or hypotheses are under-determined by the data or evidence found in their support. We must keep in mind that a hypothesis doesn't imply anything by itself, but "what does the implying is the whole relevant theory taken together, as newly revised by the adoption of the hypothesis in question."435 Furthermore, in general, a hypothesis makes 'conditional predictions,' which means that certain initial conditions or auxiliary hypotheses must be satisfied before the predicted events can be expected However, when predictions "come out right . . . we to occur. gain confirmatory evidence for our hypothesis. When they come out wrong, we go back and tinker with our hypothesis to make it better."436

As we have already mentioned, to use various types of 'ad hoc' and 'post hoc' hypotheses in order to save a hypothesis or theory is a questionable practice as it severely limits the evidential warrant for the theory's In addition, we also justified acceptance. have stressed the view that the predictability of future events or observations, which would be unlikely to happen if the

^{433&}lt;sub>Quine</sub>, <u>Web of Belief</u>, 97. 434_{Ibid}.

Ibid., 80.

⁴³⁶ Ibid., 81.

hypothesis was incorrect, is a key factor in determining the for acceptance of a particular epistemological warrant theory. When a theory or hypothesis is unable to make such predictions, it loses any chance of being supported. Moreover, a hypothesis that is only based on a single experiment provides "very little confirmation for the hypothesis; further tests, in varied circumstances, . . . would either have brought added confirmation or shown the hypothesis to be mistaken."437 We also have pointed out the importance of external coherence as an important factor for deciding between theories.

Thus, another external standard for determining the degree of evidential warrant in favour of a particular theory is that post ad hoc explanations are not as good as predictions.438 Even if we grant that the phlogiston theory may have been able to accommodate the same facts as the oxygen theory we still would have good reason to prefer the oxygen theory over the phlogiston theory as the oxygen theory was predicting new facts before they were discovered, while the phlogiston theory was merely 'post hoc' in trying to accommodate these new observations within its theory. Furthermore, the oxygen theory was progressive in that it was possible to produce successful predictions even though some revisions in the theory were necessary, whereas the phlogiston theory became unable to make successful

437 Ibid., 97.

438See Musgrave, "Why Did Oxygen Supplant Phlogiston?" 204. predictions and became less and less able to accommodate new facts while remaining consistent. The more revisions that were made, the more complex and unwieldy the phlogiston theory became. We are now in a position to conclude that there are good reasons for preferring one theory over both accommodate another, even when the same facts. Evidential warrant can involve more than just being able to accommodate facts, and gives us a valid criterion for deciding between empirically adequate theories.

We have seen that the development of improved quantitative methods played a key role in the discovery of oxygen as well as the identification of other gases. It has been claimed that the overthrow of the phlogiston theory was accompanied by a transfer of allegiance from qualitative to quantitative standards of explanatory adequacy. This shift in internal standards has been held to be responsible for the incommensurability of the phlogiston and oxygen theories. It is true that one of Lavoisier's major contributions was his stress on "increased reliance on quantitative procedures. important here was not the mere tabulation of What was weights and measures. . . but rather use of measure for constructive purposes, to arouse or to answer questions."439 What was equally important was that rigorous quantitative methods "were only useful in proportion as they brought about a sharper juxtaposition of fact and theory."440

⁴³⁹Hall, <u>The Scientific Revolution</u>, 339. **440**Ibid.

However, to understand Lavoisier's crucial contribution to chemistry and the chemical revolution involves more than simply trying to justify the claim that suddenly quantitative standards became important in analyzing chemical phenomena. essential reason that quantitative standards became The important was that Lavoisier was able to show that what had once been identified as a compound (a metal) was simpler in composition (and later came to be recognized as a chemical and vice versa, and to show that common air which element) had been regarded as elemental was in fact a mixture of gases (see page 97-98 in section II). Furthermore, he went on to show that there were more elements than previously believed. the experimental evidence that Thus, it was promoted adherence the quantitative criterion for studying to By means of the methodology of the balance chemistry. Lavoisier was in time able to produce а variety of observations which did much to justify the desirability of being able to quantify chemical reactions as an important standard for understanding their underlying internal mechanisms as well as explaining the weight gain anomaly concerning the processes taking place in combustion.

It can be argued that one of the "basic features of observation is measurement. Modern science simply did not exist until man learned to measure precisely such quantities as distance, volume, weight, temperature, and time."⁴⁴¹

⁴⁴¹Fred C. Hess, <u>Chemistry Made Simple</u> (London: W.H. Allen, 1955): 3.

these quantities not only enabled Moreover, "measuring scientists to gather quantitative data, but it also permitted the use of mathematical ideas in getting real meaning from their observations."442 Thus, for "chemistry the invention of the balance was a critical development. With it, the most fundamental fact about chemistry yet uncovered could be demonstrated, namely, that all changes in Nature from one form to another take place on a definite weight basis. Until this was shown, there simply was no science of chemistry."443

However, it would be a mistake to maintain that the only factor that heralded the chemical revolution was Lavoisier's ability to resolve the weight gain anomaly, or that suddenly viewed from Lavoisier's perspective, phlogiston's problem concerning weight gain was abruptly seen for the first time as a serious anomaly for the phlogiston theory. It was the particular accomplishments of Lavoisier's use of the balance and the results it produced that finally showed that the phlogiston theory had to be wrong, and this fact was primarily responsible for initiating the revolution in chemistry. What produced the chemical revolution was not simply the new paradigm's being able to provide a solution to the weight gain problem, but the fact that Lavoisier's new system of chemistry could produce results that could no longer be explained or accounted for by phlogiston chemistry.

442 Ibid. 443 Ibid., 3-4.

On the other hand, Lavoisier's growing experimental output was in time explainable by means of the new chemistry.

Let us review Lavoisier's use of the balance and carefully examine what else Lavoisier was able to accomplish through its use. His experiments on the decomposition of the oxide of mercury played a key role in the development of his ideas. The properties of mercury oxide were unusual in the sense that at low temperatures mercury will oxidize (be combined with oxygen), but at higher temperatures it will give up its oxygen.

Decomposition of Oxide.

2HgO	heated	2Hg	+	02
Oxide of	very hot	Mercury	Plus	Oxygen
Mercury	> Vields	metal		Gas
	TTCTUD			

He could also compare the results of heating mercury oxide with those of other metals where the presence of carbon was necessary in order for the metal oxide to give up its oxygen which then combined with the carbon to produce 'fixed air' or carbon dioxide. In addition, Lavoisier paid attention to the qualities of the gases that were produced when heating a metal or its calx, both with and without the presence of carbon. But he also was careful to measure the weight of the substances he started with as well as the weights of the various by-products, such as the metal, calx and/or the gases that were produced. It was by means of these experimental procedures that Lavoisier was able to unravel the puzzle of chemical combination and to realize that chemistry could not

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be adequately understood in terms of the original four basic elements, fire, earth, water, and air. Moreover, he was able to fulfill his standard for an adequate chemical explanation by showing that he could both create an identifiable product from its chemical components, and then decompose it into its elements.

Let us take a closer look at these results. For example, "Lavoisier, in a series of experiments with red calx, had been extremely careful to account for all the weights of the substances involved. Moreover, he had run the smelting both with charcoal and without charcoal, and in each case accounted for the weights of all substances before and after the interaction."444 In the first instance Lavoisier performed the experiment "using charcoal. The results are exactly as conceived in phlogiston theory: The calx and charcoal were entirely consumed leaving only metallic mercury and fixed air in the final product. The beginning weights and the ending weights were exactly identical."445 (Although heat had been added no one had succeeding in showing that heat alone could contribute weight to any components of a chemical reaction). However, Lavoisier did not stop here, but he "proposed that the fixed air was not a simple element; rather, he said, let us conceive that fixed air is a compound of charcoal plus one of the substances that compose the calx."446 It should be clear that Lavoisier's hypothesis is

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444Gale, <u>Theory of Science</u>, 245.
445Ibid.
446Tbid.
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"in complete and absolute contradiction to the phlogiston theory."⁴⁴⁷ The reason for this is that counter to the phlogiston theory "Lavoisier was in fact proposing that the calx was not a simple element, but rather was compound in nature - an interpretation just the exact opposite of the phlogiston conception."⁴⁴⁸ He further reasoned that "if the calx was a compound, then the mercury metal was a simple element"⁴⁴⁹ (or at least was simpler than the calx). And he drew the conclusion that "the reaction was not a combination reaction between the calx and phlogiston, but actually was a dissociation reaction in which some underlying substance was stripped away from the calx."⁴⁵⁰

Lavoisier's conclusion was based on a further experiment in which he did the same thing except this time he did not add any charcoal, but heated the mercury oxide to a high temperature, whereupon he "then weighed the resulting metal and gas. The weights added up neatly."⁴⁵¹ The significance of this result is "that nothing, no mass, was added to the reactants during the experiment."⁴⁵²

Furthermore, "although phlogiston theory predicts that phlogiston is added to the calx in order to smelt it to the metal, Lavoisier's results indicate that the metal weighs less than the calx (which implies that phlogiston has a

- 450_{Ibid}.
- 451 Ibid.
- 452 Ibid., 246.

⁴⁴⁷ Ibid.

⁴⁴⁸ Ibid.

⁴⁴⁹ Ibid.

negative weight), and most importantly, that the weight lost by the calx is just exactly identical to the weight of the residual gas."453 As "the final clincher, the gas which is evolved in the smelting done without charcoal is most definitely not fixed air. . . . He showed that the gas would support combustion, it would support animal respiration, it did not turn limewater cloudy, it was insoluble in water, etc."454

These kinds of observations were responsible for Lavoisier's eventual attack on the phlogiston theory. In the first place, Lavoisier points out that chemists "have made phlogiston a vague principle, which is not strictly defined and which subsequently fits all the explanation demanded of it. #455 Thus, "phlogiston does provide the coherence 'demanded of it' in explanation of all the relevant observational phenomena; but at what cost?"456

Finding this cost too great, Lavoisier directly attacked the existence of phlogiston "and his attack is based upon the logic of coherence."⁴⁵⁷ Lavoisier wrote that:

My only object in this memoir is to extend the theory of combustion that I announced in 1777; to show that Stahl's phlogiston is imaginary and its existence in the metals, sulphur, phosphorus, and all combustible bodies, a baseless supposition, and that all the facts of combustion and calcination are explained in

453_{Ibid}.

454_{Ibid}.

455 Antoine Lavoisier, "Reflections on Phlogiston," A Memoir to the French Academy (1783) quoted by Gale, Theory of Science, 250, who cited Douglas McKie <u>Antoine Lavoisier</u> (New York: Collier Books, 1962): 110-112. 456 Gale, <u>Theory of Science</u>, 250.

457 Ibid., 251.

a much simpler and much easier way without phlogiston than with it. 458

Thus we can summarize the main thrust of Lavoisier's argument. He poses two premises (P.1 and P.2) and then draws a conclusion (C), as follows:

P.1 'All the facts of combustion and calcination are explained in oxygen theory, without use of the phlogiston concept.

P.2 'The oxygen explanation is simpler . . .[than the phlogiston theory] . . . (and 'much easier,' which perhaps means more 'efficient' or more 'elegant,' although I am not sure what it means).'

C. 'Phlogiston is imaginary, its existence is a baseless supposition.'⁴⁵⁹

It appears that Lavoisier is arguing against the existence of phlogiston from "logical deficiencies in the concept to nonexistence of the substance named in the concept."⁴⁶⁰

Thus between 1777 and 1783 Lavoisier endeavored to "explain the well-known facts using no reference to phlogiston, but only his concept of 'eminently respirable air' (ERA). He also developed new facts, particularly quantitative measurements of a delicate order of accuracy. Needless to say, his attempts were successful."⁴⁶¹

For example, recall that Lavoisier conducted experiments in which he would heat a metal in a closed vessel with a measured amount of 'common air' until a metal calx was formed. At this stage both the calx and the

458_{1bid}. 459_{1bid}. 460_{1bid}. 461_{1bid}., 252. 155

remaining gas in the vessel were weighed. He found that the calx had "gained an amount of mass identical to the amount lost by the common air. Moreover, the common air is no longer common air; it will not support combustion or respiration, nor does it pass the goodness test."⁴⁶² He also, "comes to call this residual air - the remainder from common air after its 'eminently respirable' part has been removed - moffet, which means 'an asphyxiating gas.'"⁴⁶³ Lavoisier produced the following equation for calcination, "Metal + ERA --> metal calx."⁴⁶⁴

It should be noted that this equation expresses a chemical process that is exactly opposite to the phlogiston In addition, "thinking of the ERA as a discrete, theory. independent physical object which can move about during the reaction now further allows Lavoisier to come to a notion about the smelting, both with and without the addition of charcoal."465 Thus in the case of 'simple smelting' the reaction would be as follows:" Metal calx --> metal + ERA."466 However, when the smelting is done with the addition of charcoal, "the reaction produces fixed air which Lavoisier now conceives as being a compound produced by movement of the ERA from its location in the calx into some sort of union with the parts of the charcoal. The reaction is fairly straightforward. If Fixed air = charcoal + ERA

462 Ibid. 463 Ibid. 464 Ibid. 465 Ibid. 465 Ibid. . . . then, normal smelting . . . Metal calx + charcoal --> metal + fixed air."⁴⁶⁷

Furthermore, Lavoisier was able to account for all the parts involved in the chemical reaction and "most importantly . . . he can also account for all the weights. Indeed, he can use the initial weights of the reactants on the left side of the ---> sign to predict the weights of the reactants on the right side."468 And an important consideration of the superiority of Lavoisier's oxygen theory over the phlogiston theory is that the oxygen theory has the ability to predict precise amounts which is "highly significant when compared to phlogiston theory, which can do nothing similar."469 Thus, the oxygen theory was shown to fully satisfy the criterion of precision as well as that of internal consistency, without vaqueness of concept which we have seen was the characteristic of the various versions of the phlogiston theory.

Another important aspect of precision is that it can be increased by the redefinition of terms.⁴⁷⁰ In other words, we "take a term that is fuzzy and imprecise and try to sharpen its sense without impairing its usefulness."⁴⁷¹ As we have already mentioned, phlogiston was a very vague concept that was so elastic that it seemed to fit whatever

⁴⁶⁷ Ibid., 252-253.
468 Ibid., 253.
469 Ibid.
470 Quine, Web of Belief, 99.
471 Ibid., 99-100.

role it was required to play and at different times contradictory properties were attributed to the term. Although, Lavoisier did not sharpen the meaning of phlogiston, but in fact did away with the concept altogether, he was able to introduce new terms into chemistry, such as oxygen that were given a much more precise meaning, and a more limited role to play than did phlogiston. Thus, Lavoisier's chemistry contained much more precise terms than did the phlogiston theory.

Lavoisier's methodology and analysis and the conclusions that he drew from them clearly implied the notion that conservation of weight was an attractive principle and surely contributed to the strengthening of this concept. This additional cogent source of provided an support for Lavoisier's views, because the oxygen theory exhibited a degree of 'external coherence' that the phlogiston theory did The argument in support of this claim will be not possess. finish analyzing after we Lavoisier's made clearer experimental results and the subsequent conclusions he drew, but we should point out that the superiority of Lavoisier's oxygen theory is in part dependent upon his use of equations for predictions which "necessarily presupposes the principle of the conservation of matter"472 which was inherent in Newton's concept of the relation between gravity, weight and mass. Before fully developing this point let us first look at his results on the process of respiration. Lavoisier

472 Gale, Theory of Science, 253.

describes the process of respiration as follows: "Thus, if Common air = ERA + moffet [i.e. oxygen and nitrogen] then, Respiration: Body's fuel + common air ---> fixed air + moffet"⁴⁷³

Lavoisier postulated that in the process of respiration

there is something in animal bodies which acts as fuel, just as does charcoal in combustion. When the common air enters the body, the ERA become attached to the fuel, producing the fixed air compound which is then exhaled. Respiration, according to this conception, uses up the ERA of common air, leaving fixed air and moffet as residuals in the exhalation. To back up this analysis, Lavoisier returns to the calx-of-mercury reaction, and applies the very same notions in a more detailed experimental interpretation.⁴⁷⁴

Let us look at the chemical equations involved in these processes. As usual Lavoisier is careful to measure the volumes of the gases he starts out with"

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"Mercury + common air ---> mercury calx + moffet
(1 volume) (5/6 volume)"<sup>475</sup>.
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He arrives at the conclusion that "the ratio of ERA to moffet is about 1:5; that is, ERA is about one-sixth of common air."⁴⁷⁶ However, Lavoisier goes on to reverse this process by "smelting calx of mercury back to its original state of metallicity. In so doing he reconstitutes the common air that he originally started with. The equation is

in two stages.

473_{Ibid}. 474_{Ibid}. 475_{Ibid}., 254. 476_{Ibid}. Stage 1.

Mercury calx ---> mercury + ERA Stage 2.

ERA + moffet = common air "477

In the first stage of the experiment Lavoisier was able to separate "the ERA and metallic mercury; in the second stage, he takes the original 5/6 volume of moffet which remains after the ERA is absorbed during calcination, adds it to the ERA given off during the first stage, and produces the original starting 1 volume of common air."⁴⁷⁸ This shows that "the ERA hypothesis can be used to render a completely consistent and coherent account of the entirety of facts surrounding calcination, combustion, and respiration."479 Furthermore, Lavoisier is able to account "for the compound nature of common air and provides an explanation for the evolution of fixed air during both combustion and respiration."480 In "1779 Lavoisier coins the name 'oxygen' for his new gas . . . ERA takes on an independent life of its own as a specially named object."⁴⁸¹ Thus, we can draw the conclusion that there is "no question about the logical virtues of this account."⁴⁸² Lavoisier is now in a position to argue for "his system's logical coherence and consistency,

477 Ibid. 478 Ibid. 479 Ibid. 480 Ibid. 481 Ibid. 482 Ibid. and at the same time, attack the logical inconsistency and lack of simplicity of the phlogiston theory."⁴⁸³

Thus, in summing up of the accomplishments of Lavoisier's oxygen hypothesis, it can be maintained that it "clearly eliminated any thoughts about the existence of a zero) weight."484 substance with negative (or, . . . Lavoisier's oxygen hypothesis was a powerful force as it was justified in part by its success "in weighing oxygen quite accurately, and had traced its mass throughout its reaction. Moreover, his use of the balance had permitted the development of precise quantitative equations. Given these features, there is no doubt why his system appealed to the physicists: Its formal quantitative style, as well as its substantive concepts, were squarely in line with the best physics of the day."485

Moreover, there were additional elements in his system, that had strong appeal for physicists and mathematicians.⁴⁸⁶ For example, his "early adoption of the principle of the conservation of matter fitted rather nicely into the numerical schemes of physics, in which various quantities such as momentum (mv), . . . and kinetic energy (mv²) were all conserved entities. The numerical equations that he developed provided strong evidence of conservational laws, which was a new aspect of chemistry."⁴⁸⁷ It is clear that

⁴⁸³ Ibid., 255. 484 Ibid., 255-256. 485 Ibid., 256. 486 Ibid. 487 Ibid.

"Lavoisier's program was a telling blow against the nonconservative entities which function in the phlogiston system, non-conservative entities which physicists would find somewhat repugnant."488 In short, "all these features, positive mass, quantitative formalism, precise numerical and conservational entities prediction, were verv attractive to physicists and mathematical scientists. Thus, what we must see here is a growing external coherence between the new chemistry and the prevailing physics."489

Furthermore, Lavoisier and Laplace "did experiments which built further bridges between the new chemistry and the older, well-established physical paradigms. . . . a final example of the growing external coherence provided by the new hypothesis."⁴⁹⁰ This experiment "concerns respiration and heat."⁴⁹¹ The experiment was designed to "measure heat by the amount of ice which could be melted by the hot body. Although this procedure provides no absolute measure of heat, it does give a clean, clear, relative value which can be used to compare two or more bodies."⁴⁹² The assumption was made that "two bodies which each melt two cubes of ice have the same amount of heat: one body which melts only one cube has only half the heat of either of the first ; and so on."⁴⁹³ They placed "a quinea pig in a chamber, and measured how much

488 Ibid. 489 Ibid. 490 Ibid. 491 Ibid. 492 Ibid. 493 Ibid., 256-257.

ice the animal could melt with his body heat over a measured They also collected the fixed air respired period of time. by the animal, and measured its volume."494 Lavoisier had already formulated a theory of respiration where animal bodies "have a charcoal like fuel, which is slowly combusted with the oxygen they breathe in; fixed air is exhaled as a result of the reaction. Thus, the amount of fixed air respired is directly related to the amount of fuel which is burned in the animal's body."⁴⁹⁵ However, Lavoisier goes on to make the "bold hypothesis: The heat given off during the slow-speed combustion in animal respiration should be closely related to the amount of heat which could be generated by burning an identical weight of charcoal."496 We must also point out, that this "new prediction follows strictly logically from Lavoisier's concepts; but it is a completely physics of his new notion as far as the time is concerned."497 The main problem for Lavoisier was "to figure how much fuel the animal burned during the time out period."498 Lavoisier had already measured the "volume of exhaled air. So what he does now is to burn enough charcoal to produce an amount of fixed air identical in volume to that exhaled by the pig."499 Then the next step for Lavoisier was to measure "how much charcoal was burned in

⁴⁹⁴ Ibid., 257. 495 Ibid. 496 Ibid. 497 Ibid. 498 Ibid. 499 Ibid.

order to produce that volume of fixed air."⁵⁰⁰ As a final step "he takes an identical amount of charcoal, burns it, and measures how much ice it melts."⁵⁰¹ Lavoisier discovered that the "amount of heat produced by burning the charcoal is exactly identical to the amount produced by the pig during respiration."⁵⁰² By this experiment, Lavoisier "completes the circle and, in so doing, makes a firm link bridging pure chemical concepts such as 'fixed air,' 'oxygen,' etc., and the physical concept of 'heat,' not to mention the biochemistry and physiology involved in respiration."503 In conclusion we again get large amounts of external coherence. 504 Thus, by '1784', the "phlogiston theory was completely doomed; oxygen theory was assured the ascendant position."505

- 500_{Ibid}.
- 501_{Ibid}.
- 502 Ibid.
- 503_{Ibid}.
- 504 Ibid.
- 505_{Ibid}.

V. Conclusions

The major argument to be made against Doppelt's version thesis with respect to relativism of Kuhn's and incommensurability is that there are, indeed. external paradigm-independent standards, or virtues which constitute the criteria for determining explanatory adequacy, and which ultimately govern the decisions made by the scientific community when new theories replace their predecessors. When these criteria are applied it is the cumulative weight of the evidence which ultimately determines the growing acceptance of new theories when scientific revolutions take place. One caveat to keep in mind, is that these standards are ideals, which are not always completely satisfied. However, the more a hypothesis or theory satisfies these standards the more evidential warrant there is in support of a particular theory or hypothesis and the wider the acceptance it gains within the scientific community. As we have seen, some individual scientists (such as Priestley) may cling to theories after they are beset with problems and have outlived their usefulness, whereas others, (such as Lavoisier) may chose to adopt a new working hypothesis before it has become established by a sufficiency of evidence because it offers the promise of fruitful opportunities for exploration and In this sense, some short-term relativism cannot be advance. denied. However, this short-term relativism does not undermine a theory of scientific progress that is cumulative with respect to genuine observational data or the degree to

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successful theories are cumulative by virtue which of satisfying paradigm-neutral external standards governing the degree of evidential warrant which supports them. Thus, there is non-relativistic knowledge and cumulative scientific It may be true that at an early time in the progress. history of а successful theory the evidence mav be insufficient to make the choice to adopt the new paradigm rational or compelling. However, if the new paradigm makes successful predictions whereas its rival fails, the new paradigm will gain more and more support which in time can become overwhelming.

In addition, we characterize rationality in terms of whether or not new paradigms and theories are adopted or accepted on the basis of their satisfying more and stronger evidential warrant external standards of then their We are not arguing that when Lavoisier first predecessors. proposed the anti-phlogiston paradigm he then had compelling reasons in favour of his choice. However, in time, reasons based on meeting neutral external standards did become compelling. In the final analysis it is evidential warrant that decides the superiority of one theory or hypothesis over another, thereby undermining any notion of long-term relativism. During a scientific revolution, internal standards of a specific paradigm may undergo change, but the acceptability of these internal standards is determined by paradigm-independent standards that, contrary to Doppelt, are not in themselves influenced by the internal standards of a particular paradigm. Ultimately, paradigm-independent standards determine what is to count as evidential warrant for establishing sound theory choice.

In the analysis presented here it has been argued that the phlogiston and anti-phlogiston theories were not incommensurable. In the first place there was no disagreement as to what counted as observational evidence. Instead, their disagreement was focused on the underlying causal mechanisms that were ultimately responsible for explaining these observations. It is in this sense that they Furthermore, independent paradigm-neutral were rivals. standards provided a means by which to compare the evidential theory, undermining warrant for each the case for incommensurability and allowing us to provide an adequate notion of scientific, progress at least in chemistry. то the extent that the chemical revolution is typical of paradigm replacements, it provides historical evidence against Kuhnian epistemological relativism even in Doppelt's weaker version of this thesis.

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