The Promise And Pursuit Of Scientific Theories

Laurie Anne Whitt

Follow this and additional works at: https://ir.lib.uwo.ca/digitizedtheses

Recommended Citation
https://ir.lib.uwo.ca/digitizedtheses/1442

This Dissertation is brought to you for free and open access by the Digitized Special Collections at Scholarship@Western. It has been accepted for inclusion in Digitized Theses by an authorized administrator of Scholarship@Western. For more information, please contact tadam@uwo.ca, wlswadmin@uwo.ca.
The author of this thesis has granted The University of Western Ontario a non-exclusive license to reproduce and distribute copies of this thesis to users of Western Libraries. Copyright remains with the author.

Electronic theses and dissertations available in The University of Western Ontario’s institutional repository (Scholarship@Western) are solely for the purpose of private study and research. They may not be copied or reproduced, except as permitted by copyright laws, without written authority of the copyright owner. Any commercial use or publication is strictly prohibited.

The original copyright license attesting to these terms and signed by the author of this thesis may be found in the original print version of the thesis, held by Western Libraries.

The thesis approval page signed by the examining committee may also be found in the original print version of the thesis held in Western Libraries.

Please contact Western Libraries for further information:
E-mail: libadmin@uwo.ca
Telephone: (519) 661-2111 Ext. 84796
Web site: http://www.lib.uwo.ca/
NOTICE

The quality of this microfiche is heavily dependent upon the quality of the original thesis submitted for microfilming. Every effort has been made to ensure the highest quality of reproduction possible.

If pages are missing, contact the university which granted the degree.

Some pages may have indistinct print especially if the original pages were typed with a poor typewriter ribbon or if the university sent us an inferior photocopy.

Previously copyrighted materials (journal articles, published tests, etc.) are not filmed.

Reproduction in full or in part of this film is governed by the Canadian Copyright Act, R.S.C. 1970, c. C-30. Please read the authorization forms which accompany this thesis.

THIS DISSERTATION HAS BEEN MICROFILM ED EXACTLY AS RECEIVED

AVIS

La qualité de cette microfiche dépend grandement de la qualité de la thèse soumise au microfilmage. Nous avons tout fait pour assurer une qualité supérieure de reproduction.

S'il manque des pages, veuillez communiquer avec l'université qui a conféré le grade.

La qualité d'impression de certaines pages peut laisser à désirer, surtout si les pages originelles ont été dactylographiées à l'aide d'un ruban usé ou si l'université nous a fait parvenir une photocopie de qualité inférieure.

Les documents qui font déjà l'objet d'un droit d'auteur (articles de revue, examens publiés, etc.) ne sont pas microfilmés.

La reproduction, même partielle, de ce microfilm est soumise à la Loi canadienne sur le droit d'auteur, SRC 1970, c. C-30. Veuillez prendre connaissance des formulaires d'autorisation qui accompagnent cette thèse.

LA THÈSE A ÉTÉ MICRÔFILMÉE TELLE QUE NOUS L'AVONS RÉCU
THE PROMISE AND PURSUIT OF SCIENTIFIC THEORIES

by

Laurie Anne Whitt

Department of Philosophy

Submitted in partial fulfilment
of the requirements for the degree of
Doctor of Philosophy

Faculty of Graduate Studies
The University of Western Ontario
London, Ontario
September 1985

© Laurie Anne Whitt 1985
ABSTRACT

Recent research in philosophy of science suggests that the relationship which scientists have towards the artifacts of science, i.e., their theories, is considerably richer than many traditional accounts of scientific appraisal would lead us to believe. Problem-solving methodologists in particular, advocating a pragmatic account of scientific theories, argue that traditional methodologies have tended to focus exclusively on one modality of appraisal -- that of theory acceptance, and have advanced normative proposals which provide only for assessments of the empirical well-foundedness of scientific theories. As a result, it is held, these traditional accounts are unable to accommodate certain historical cases in which conceptual considerations have played a vital role in scientific appraisal, and in which the behaviour of scientists clearly indicates something less or other than theory acceptance on their part.

This essay explores a number of issues concerning the philosophy of science, the history of science, and the relationship between history and philosophy of science. A problem-solving methodology is adopted in the interests of contributing to some of this recent research in philosophy of science by critically assessing and further developing selected features of it. Two historical case-studies are presented in order to establish: (1) that conceptual considerations have figured significantly in the assessments which scientists make of their theories; (2) that scientists do something more than accept or reject their theories, they pursue them; and (3) that is defending
their decisions to pursue or not to pursue a theory, scientists typically do make reference to the promise, or lack of promise, of the theory. With the aid of these historical cases, specific normative proposals are developed which are intended to enrich our appreciation of scientific appraisal by providing an account of what it is to pursue a theory, of what it is for a theory to be promising, and of the role of conceptual considerations in supporting judgements of theory promise.

In Chapters One and Two a critical review of the project of historical philosophy of science, and of some recent contributions to the study of scientific theory appraisal which have emerged from it, is conducted. The two historical case-studies are then introduced. Chapter Three explores Newton's critique of the conceptual well-foundedness of Cartesian theory and his reasons for regarding that theory as unpromising and no longer worthy of pursuit. In "De Gravitatione et Aequipondio Fluidorum", Newton takes Cartesian theory to task on a number of grounds: for the vagueness and unclarity of its central concepts; for its conflation of the pivotal distinctions (e.g. between two senses of 'place' and of 'motion') which it has introduced; and for its commitment to a semi-relativistic theory of motion which was inconsistent with the absolute view of space it appeared to endorse. This case study also provides a valuable example of the interaction between conceptual and empirical problem-solving in theory development. Newton effectively demonstrates that the conceptual problems troubling Cartesian theory are responsible for that theory's inability to offer an adequate solution to the empirical problem posed
by the phenomenon of vortical motion. It is shown how Newton, as a result of his intimate acquaintance with the conceptual problems plaguing Cartesian theory, is led to conclude that a plausible contender to Cartesian theory would need to stand firmly committed to absolute space in order to resolve the empirical problem posed by vortical motion. Newton's own resolution of this problem in the scholium to the Principia is examined in an appendix.

The second case-study, considered in Chapter Four, focuses on the lingering and pronounced ambivalence of nineteenth century chemists towards the atomic theory, and their appraisal of its promise and pursuability. Initially, a survey of the central conceptual problems troubling Dalton's New System is undertaken. The atomic theory expounded there is shown to violate the definition of 'element' advanced some twenty years earlier by the eminent chemist Lavoisier; to endorse a theory of matter blatantly at odds with the prevailing Newtonian physics of the time; and to ignore the century-long preoccupation of chemical orthodoxy with problems of affinity together with that tradition's methodological proscriptions. Moreover, developments within the latter tradition served to establish it as a promising alternative to Daltonian atomism throughout most of the century. It is argued that the existence or reality of atoms was, never a central issue in the debates which waxed and waned over the century, much less — as is commonly held — one that was somehow decisively settled by the century's end in favour of 'atomists' and against 'anti-atomists'. While many chemists regarded Daltonian atomism as promising and worthy of pursuit, others continued to pursue affinityist
studies. The latter took issue not so much with Dalton's assertion of the existence of atoms but with what he had to say about their nature and centrality in chemical research. This was one aspect of a broader controversy over the appropriate problem-solving domains for chemical theories -- were theories of chemistry primarily concerned with the dynamics of chemical reactions or with the determination of atomic weights and the arrangement of individual particles of matter?

Such historical episodes underscore the fact that enriching our appreciation of scientific appraisal is not only desirable, it is vital if we are to do justice to the complexities and subtleties which characterize the relationship between scientists and theories. The philosophical morals to be derived from these case studies are drawn in Chapter Five where specific proposals for enrichment are advanced along the lines discussed in Chapter Two. An account of the nature and rationality of theory pursuit is presented, and a criterion is developed for appraising the promise of a theory in which assessments of conceptual well-foundedness figure importantly.
ACKNOWLEDGEMENTS,

I am grateful to John Nicholas for his guidance, example and encouragement. Also to T., whose patience and faithful support never wavered.
# TABLE OF CONTENTS

CERTIFICATE OF EXAMINATION .................................................. ii
ABSTRACT ........................................................................ iii
ACKNOWLEDGEMENTS ............................................................. vii
TABLE OF CONTENTS ............................................................... viii

CHAPTER ONE -- HISTORICAL PHILOSOPHY OF SCIENCE ............. 1
Introduction ........................................................................... 1
I. Foundational Problems: The Normative/Descriptive Paradox .... 5
II. A Critique of Laudan’s Proposed Resolution ......................... 10
III. Thagard: Towards a Normative/Descriptive Methodology .... 18
IV. A Pragmatic View of the Structure of Theories .................... 25
Footnotes ............................................................................. 33

CHAPTER TWO -- THEORY APPRAISAL IN HPS: A CRITICAL REVIEW 39
Introduction ........................................................................... 39
I. The Role of Conceptual Considerations ................................. 41
II. Theory Pursuit as a Modality of Appraisal ......................... 48
III. A Criterion for Theory Pursuit ........................................ 67
Footnotes ............................................................................. 79

CHAPTER THREE -- ABSOLUTE SPACE: FROM DESCARTES TO NEWTON 85
Introduction ........................................................................... 85
I. .......................................................................................... 87
II. ....................................................................................... 91
III. ..................................................................................... 99
IV. .................................................................................... 100
V. ..................................................................................... 104
VI. ................................................................................... 108
VII. ................................................................................... 111
Footnotes ............................................................................. 114

CHAPTER FOUR -- ATOMS OR AFFINITY? THE PURSUIT OF NINETEENTH CENTURY CHEMICAL THEORY 116
Introduction ........................................................................... 116
I. .......................................................................................... 118
II. ....................................................................................... 122
III. ..................................................................................... 130
IV. .................................................................................... 135
V. ..................................................................................... 141
VI. ................................................................................... 147
VII. ................................................................................... 153
VIII. .................................................................................. 159
IX. .................................................................................... 169
Footnotes ............................................................................. 177
CHAPTER ONE

HISTORICAL PHILOSOPHY OF SCIENCE

Introduction: An Irreverent Look at the Chalk-Line Game

Philosophy of science without history of science is empty; history of science without philosophy of science is blind.

Among the many revealing 'dare games' in which American children engage is the chalk-line game. The degree to which it is a game is debatable since its rules are as ambiguous as they are amorphous. This much, however, seems clear: it requires at least two players, but only one need actually decide to play it. Once this unilateral declaration is made, the other is ipso facto caught up in the game. The former (dub him 'Hans') initiates the proceedings by securing a suitable piece of chalk and drawing a line on the pavement in front of the latter ('Thomas' seems appropriate). Hans then steps back, glowers, and dares Thomas to step over it. Thomas of course, his own inclinations notwithstanding, must either step or not step across it. The winner is determined in a straight-forward manner -- by physical strength or by he who best manages otherwise to save face.

Happily though, there is some recourse for Thomas in his practical confrontation with the law of the excluded middle. He has been compelled to play -- to step or not to step -- but he can protest by refusing to play as Hans would have him. Thomas may, at his own
peril, choose to straddle the chalk demarcation. Alternatively, he may bend down and erase the line. (Clever, but ineffective, as Hans will respond by drawing another line and directing his dare to its erasure.) Or, in a rather more sophisticated maneuver, he may erase only part of the line and stand in the middle. The virtue of this third alternative is that, unlike the first, it effectively discards the game and, unlike the second, it denies neither the existence of boundary between the two players' spheres of influence nor Hans' right to point out this fact. Instead, it proposes that the boundaries be qualified: that their respective spheres, for all their distinctness, are not mutually exclusive. As the dare has been emptied of content, Hans and Thomas can turn to more productive play.

Since Reichenbach first introduced the distinction between the context of discovery and the context of justification, philosophers of science and historians of science have been developing their own variants of the chalk-line game. In the earliest of these, it was held that to cross -- or worse, to straddle -- the line is to sin twice over. Not only is this to court the genetic fallacy, it is also to conflate the is/ought distinction. The historian's sphere of influence is descriptive. S/he deals in case studies and empirical data, and in this teleological terms of reference may include all that is psychological, sociological and political in science. The historian's study yields results (significantly not recommendations) which illuminate the dynamics of theory change and the actual practice of science, i.e. the discovery, rise and fall of scientific ideas, theories, methodologies, etc. By contrast, the philosopher of science (with chalk in hand on
the other side of the line) addresses the logical structure of scientific theory. The mandate here is to investigate the evidential relations that hold between some body of data and the particular theory that these data are claimed to support. This is a normative enterprise: as the actual development and refinement of a theory may include irrational and arational elements, and as these are irrelevant to questions of justification, the philosopher deals not with the actual theory, but with some reconstruction of it: What this yields are recommendations as to how science ought to proceed, rather than observations about how past and present science does proceed. As Feigl notes:

It is one thing to ask how we arrive at our scientific knowledge claims and what ... factors contribute to their acceptance or rejection; and it is another thing to ask what sort of evidence and what general, objective rules and standards govern the testing, the confirmation, or disconfirmation and the acceptance or rejection of knowledge claims of science. Simply put, according to the Irrelevance Thesis, Hans and Thomas have little that is of telling value to offer one another:

the gap between history and philosophy of science is as broad as, indeed is illustrative of, the divide between matters of fact and matters of value. History is irrelevant to the philosopher because he is not concerned about what science has been, but rather how it should be. Philosophy is irrelevant to the historian because it is not his job to make normative judgments about the figures he studies.

Kuhn, in the nearly infamous The Structure of Scientific Revolutions, has been charged by his critics with both of the above sins of commission. These critics have construed his response to the Reichenbachian dare as an opting for the first alternative (the awkward
straddle with a foot in both camps) despite his declared decision for the second alternative (the erasure technique). Subsequently, a long line of philosophers and historians have entered the fray in an attempt to weigh the merits of the demarcation, to determine which items fall under which sphere of influence, and to assess what, if anything, the two disciplines have to say to one another. Although there is currently no consensus concerning the nature of their relationship (it has been regarded variously as "intimate" and "inextricable" on the one hand; and as "a marriage of convenience" on the other\(^5\)), that there is some such relationship is becoming clear. The Irrelevance Thesis no longer seems tenable in light of contemporary research demonstrating the role of philosophical assumptions in historical scholarship and illustrating the valuable contribution of historical case studies to the philosopher's accounts of scientific theory appraisal.\(^6\) Increasingly, something like the third alternative described above appears required, a response which acknowledges the distinctness of the two disciplines but which qualifies their boundaries by indicating important common ground between them.

Since my primary intent is to explore and to develop some of the work that is being done on this common ground, a careful engagement of this on-going debate concerning the relationship between the history and the philosophy of science is beyond the scope of this essay. Nevertheless, some discussion of the commitments and concerns typical of those doing work in historical philosophy of science, as well as of the problems raised by this developing research tradition, is in order. Such a discussion is conducted in Chapter 1 of this essay. Three areas
of neglect in standard studies of theory appraisal to which philosophers in this tradition are drawing attention will then be identified: the need to recognize (i) the role of conceptual considerations in theory-appraisal; (ii) the existence of a second modality of rational scientific appraisal, theory-pursuit; and (iii) the relevance of appeals to the promise of a theory-in-contexts of appraisal. Chapter Two examines some of the work underway in each of these areas by several historical philosophers of science, primarily problem-solving methodologists who employ a pragmatic account of scientific theories. It should be kept in mind, however, that the pragmatic approach which they adopt is assumed or presupposed in the following, and the fruits are explored. It will not be defended, or argued for, from first principles. The case studies of Chapter Three and Four are then invoked both to establish some of the consequences of this traditional neglect and, together with the discussion in Chapter 5, to refine and extend current efforts to remedy such neglect.

1. Foundational Problems: The Normative/Descriptive Paradox

The theory of science, on the one hand, cannot disregard the historical practice of science, while on the other it cannot take this practice to be a consistent and sufficient norm. The problem then is to find a defensible intermediate position. There is a danger, perhaps, in putting matters so; it sounds as though one is assigning relative weight to historical practice on one side and to logic/epistemology on the other in warranting a theory of science. Were this to be the project, it would have little chance of success. When the philosopher turns to the history of science, he does so for quite different reasons in different parts of the philosophy of science. And even
in the matter of theory appraisal, the two potential sources of warrant ought not be regarded as somehow independent of, or in competition with, one another.

I have spoken of historical philosophy of science as a research tradition in the philosophy of science. A word or two on the sense in which this is so is appropriate. Historical philosophers of science have in common certain commitments which guide and constrain their work in important ways. In addition to a shared commitment to the use of historical materials (often, full-blown case studies) in the construction of their theories of science, the entities to which they address themselves are dynamic and temporal. Scientific theories are not construed by historical philosophers of science as atemporal sets of propositions and in this, as well as in their methodological commitment to the use of historical data, they part company with the logical positivist tradition. Indeed many (if not all -- a short list would include Kuhn, Lakatos, Feyerabend, Laudan, McMullin, Thagard and Brown) have argued at some length that HPS is a response to the failures and inadequacies of the latter tradition. Certainly their efforts to take seriously the temporality of scientific theories and the dynamics of theory-change and theory-development have produced accounts of scientific rationality, progress and theory appraisal which diverge radically from those of the positivists.

Commitments such as the above serve to direct and to-delimit work in HPS. Norms for theory appraisal and assessment, for example, must be drawn from and tested against scientific practice. Since the goal is to generate from historical studies, methodological principles whose
sway can be extended normatively to cover the general practice of
science. HPS emerges as both an empirical and a normative
discipline. This brings to our attention the acutely problematic
foundations of HPS (alluded to above as motivating the Irrelevance
Thesis); it would seem to presuppose some resolution of the
normative/descriptive paradox. If one cannot derive an ought from an
is, and if prescriptive conclusions regarding proper scientific method
are to be drawn from descriptions of actual scientific practice, then
the entire project of HPS would seem ill-advised at best. Moreover,
however persuasively it may be argued that, on the one hand, the
history of science not only has not but cannot avoid making normative
assumptions and that, on the other hand, it is questionable in what
sense we are doing philosophy of science if our philosophical analyses
bear no resemblance to science as it is actually practiced, this does
not help matters. We must then confront the circularity problem:

If the writing of the history of science presupposes a
philosophy of science and if a philosophy of science is
then to be authenticated by its capacity to lay bare the
rationality held to be implicit in the history of science,
how can we avoid automatic self-authentication, since the
history we write will presuppose the very philosophy which
the written history will allegedly test?

Owing to such threatening circularity and to the perils posed by the
normative/descriptive paradox, the foundations of HPS would seem to be
conceptually troubled indeed.

Of course this fact has not escaped the notice of those doing
research in HPS. To the contrary, they are usually at some pains to
point it out, to arrive at some resolution of this conceptual problem
or at least to suggest the direction in which such a resolution might
lie. That this should be so is itself instructive, for it reveals something about the nature and role of conceptual problems which will be illustrated below in a different context, in the case studies of Newtonian and Daltonian theories. The conceptual problems raised by a research tradition (scientific or otherwise, one's own or a competing tradition) are not and cannot be easily ignored. They play a vital, and at times decisive, role in assessing work which is a product of the tradition. Moreover, contra Kuhn, conceptual problems are always an 'issue'; they do not conveniently disappear during Kuhnian periods of normal research. Although lack of a satisfactory resolution to them will not prevent work within the tradition from continuing (especially when the tradition is clearly dominant and its competitors few and feeble in comparison, as was the case with Newtonian theory in the 18th century), the weight of such problems and the frequency with which they are debated increases appreciably when the tradition is faced by serious and successful competitors (as 19th century Daltonian theory was). We will also see, contra Laudan, that the ability of a research tradition to resolve or dissolve conceptual problems can count in favour of a tradition — whether these problems are internal or external.

In the case of HPS, there have been two notable attempts to address and resolve conceptual problems generated by the tradition itself. While the second of these, by Thagard, seems more promising in a number of respects than the first, that of Laudan, my principal interest in the brief discussion which follows is not to establish this or to champion Thagard's resolution as a wholly satisfying one. The
latter would not only be premature as Thagard’s general account is still under development, it would also take us much too far afield. The broad concern of this study is to mine and to develop some of the significant work being done in HPS in the area of scientific theory appraisal, with the aim of contributing to and furthering that project. In the present section my intent is to observe and to illustrate one of the morals which will be drawn in the concluding chapter concerning scientific theories and theory appraisal: any assessment of the pursuitability of a research tradition must make reference to the conceptual problems which the tradition faces. We must persuade ourselves of its conceptual viability. A tradition which has demonstrated a high degree of such viability over time is not likely to have enjoyed a conceptually untroubled past and may not even enjoy an unproblematic present. But it will display considerable ability to respond to and to generate fruitful resolutions of conceptual problems. This, of course, cannot be done if the conceptually problematic is shrugged off or silenced, and, as we will see in the case studies examined below, scientists typically do not do so.

Nor, I think, should we. The foregoing moral applies as well to HPS. Any assessment of the contributions it has made to illuminating traditionally neglected features of theories of scientific appraisal must include some consideration of the conceptual problems it has faced and its response to them. The work of Laudan and Thagard, among others, demonstrates an awareness of and responsiveness to such problems. Ultimately, I think the overall assessment of HPS is encouraging. Not only is it proving itself to be a fertile tradition
by enriching our accounts of scientific theory appraisal, it is also proving itself a conceptually viable one.

II. A Critique of Laudan’s Proposed Resolution

Briefly then, Laudan proposes the following as "one possible way out" of the normative/descriptive paradox. On the basis of a crucial distinction between HOS1 (the chronologically ordered class of beliefs of former scientists -- "the history of science itself") and HOS2 (the descriptive and explanatory statements which historians make about science -- "writing about the history of science"), he claims that there is a set of widely held normative judgments (a small subset of the much larger set of all our beliefs about HOS1) which most scientifically educated individuals share. (Note that this is an empirical claim.) Some examples are: it was rational to accept Newtonian mechanics and to reject Aristotelian mechanics by 1800, and it was irrational after 1920 to believe that the chemical atom had no parts. These judgments are spoken of as normative intuitions, "our preferred pre-analytic intuitions about scientific rationality" (PI), and they function as "decisive touchstones" in testing a model of scientific rationality. They enable us to determine the degree of adequacy of any theory of scientific appraisal (which) is proportional to how many of the PIs it can do justice to.

This, for Laudan, suggests how it is that philosophy of science is both descriptive and normative, both empirical and a priori: it is
so with respect to different cases: It is descriptive with respect to the cases which constitute PI ("it aims to explicate the criteria of rationality implicit in our preferred intuitions about certain cases within HOSI") and it is normative with respect to the cases which constitute that set of beliefs about HOSI concerning which we have no strong, widely-shared, pre-analytic convictions (i.e., the complement of PI). What results is a normative model of rationality that is authenticated by and grounded within (the consensus of) our pre-analytic intuitions about scientific rationality. This strategy, for resolving the normative/descriptive paradox, then, on singling out sets of cases (or beliefs about HOSI) with respect to which a theory of scientific appraisal functions descriptively or normatively. (In terms of the irreverent tale told at the outset of this chapter, the philosopher of science plays Thomas with respect to the set of PI while playing Hans with respect to the complement of PI.) The procedure, Laudan observes, is analogous to that which we employ in ethics:

As in ethics, so in philosophy of science: we invoke an elaborate set of norms, not to explain the obvious cases of normative evaluation (we do not need formal ethics to tell us whether the murder of a healthy child is moral), but rather to aid us in that larger set of cases where our pre-analytic judgments are unclear.

This attempt to resolve the paradox is less than satisfying for several reasons. Consider Laudan's comment that the claim that at least certain specified developments in the history of science were rational is, however modest, "entirely a matter of faith." Since our criterion of rationality itself will take their rationality for
granted, we are unable to prove their rationality. Any such 'proof' would be circular or would generate an infinite regress. However, as we have seen, it is a matter of intuition(s) what these certain specified developments are. And, if Laudan's account is to be a workable one, the matter of reaching a consensus here would seem to be pressing and essential. But is unclear how this is to happen; given the apparent nature of the PI it is hard to see how they could be subject to rational disagreement. This would still be worrisome even if the consensus (regarding the set of PI) to which Laudan appeals actually does exist, but it is especially so if a consensus is lacking -- and we are given no good reason, indeed no reason at all, to suppose that it obtains. How is a consensus to be reached? One suspects that the procedures here will be little of an improvement upon those available to a Popperian scientific jury in arriving at its 'verdict' of 'basic statements'.23 No rational way of resolving a conflict of pre-analytic intuitions, or of modifying them in light of changes in our beliefs about HOS1, is indicated.

A further difficulty -- a rather slippery distinction between HOS1 and HOS2 -- compounds this one. Clearly the only access we have to HOS1 is through HOS2. The consensus we might reach about either the chronology of, or the beliefs which constitute, that chronologically ordered class of beliefs of former scientists (HOS1) depends upon our accounts of HOS2. We no more have direct access to such a class of beliefs than we do to "unvarnished facts"24 or theory-neutral observations. McMullin has noted that Laudan's term 'intuition' could be misleading if it led one to forget that normative judgments like PI
require a considerable sensitivity to history. They are not, he cautions, intuitive in the ordinary sense. This may well be, but then one wonders in what sense they are intuitive and what happens when we draw attention to their considerable sensitivity to history (HOS2). Most obviously, Laudan would then find it difficult to defend the claim that while the philosopher of science can dispense with HOS2, s/he cannot dispense with HOS1 when it comes to determining whether "would-be theories of rationality are, in fact, theories of rationality." Since the PI is a small subset of the larger set of all our beliefs about HOS1, it is subject to change just as is this larger set. Indeed, it must change if the scientifically educated individuals whose intuitions are consulted are to reach a consensus. Certainly when it does change it is as a result of changes in HOS2.

Nevertheless, Laudan's description of our procedure in philosophy of science, as well as of the 'analogous' procedure in ethics does not encourage one to suppose that the sense of intuition intended is of something other than what is incorrigible and wholly unmediated. From what he does say it appears that particular ethical judgments are incorrigible intuitions to which our ethical principles must conform. If this is so then we can neither revise them in light of the principles nor our principles in light of them. And if the normative judgments which constitute the PI are analogous to particular ethical judgments in this respect, then the same (unwelcome) consequence would seem to follow for them: the PI cannot revise, or be revised by, our theories of rational scientific appraisal. And even if one grants Laudan a sense of intuition here which does not imply being
incorrigible and wholly unmediated, one still wants more in the way of hints as to how they are corrigible, sensitive to history, and subject to change as a result of change within HOS2 as well as of the need to reach consensus.

The distinction between HOS1 and HOS2 seems uncontroversial if it is given ontological import. Of course the actual past of science, or history of science itself (HOS1) is distinct from writings about HOS1. On this reading of the distinction, the existence of HOS1 is a necessary condition of the existence of HOS2. But if the distinction is understood epistemologically, as it is in Laudan's resolution of the normative/descriptive paradox, knowledge of HOS2 is a necessary condition of knowledge of HOS1. (Here Laudan appears to endorse an epistemic optimism about our knowledge of HOS1, in contrast to his epistemic pessimism with regard to realism.)

If our beliefs about HOS1 (and so the cases constituting PT) are the product of what is written at the level of HOS2 (this includes the laboratory journals and lecture notes of scientists as well as, presumably, books like Laudan's), they should be responsive to, and corrigible in light of, changes in HOS2. Laudan not only does not give any hints as to how this might take place, but it is debatable whether he even thinks it possible. The PT function as decisive touchstones which 'test' our theories of rationality, and enable us to determine their degree of adequacy. Does this 'core' of intuitions (PT) change through time? Nothing that Laudan says suggests that it does. He might appeal to the type of response he makes in a number of other
being addressed. The selected cases which constitute PT differ in kind from the other cases. Thagard's selected cases differ only in degree.

The claim is also that we, human information-processing systems, typically do engage in such a reflective procedure in our reasoning in a number of areas. Ethics is another example where descriptive findings are seen to be relevant to normative issues. In establishing a set of normative ethical principles we must draw upon descriptive empirical matters, especially those having to do with the psychological limitations and abilities of moral agents. Nor is it the case here that we derive an ought from an is. Hans was right about this; it cannot be done. But if Daniels and Thagard are right, it is not what we do.

I have not pretended to assess the over-all merit of Thagard's proposed resolution here. Such an assessment would require a careful exploration both of the general model (FDN) and his responses to some of the problems it generates (such as the embracing of 'weak psychologism'). I have wanted to draw attention to the sense in which his work continues and contributes to attempts to resolve what is conceptually problematic in HPS. I have also wanted to suggest some of the encouraging respects in which this more recent account is preferable to the earlier one of Laudan.
IV. A Pragmatic View of the Structure of Theories

It was stated above that a characteristic feature of research in HPS is the commitment to regarding theories as dynamic, temporally-situated entities. Kuhn, Laudan and Thagard are typical in this respect. Another characteristic feature which their work shares is a commitment to a pragmatic, rather than syntactic account of the structure of theories. Their concern for how theories are used by scientists and for the kind of work which theories have historically done in scientific communities leads to a rejection of the logical positivist tradition's view of theories as sets of sentences in axiomatic systems. This concern is evident in Laudan's historically sensitive problem-solving model, in Kuhn's talk of puzzle-solving, and in his critically assailed but indefatigably mined concept of a paradigm, or disciplinary matrix consisting in part of concrete problem-solutions (the exemplars). It is also evident in Thagard's treatment of consilience as a central property of scientific theories. He argues that neither syntactic nor semantic accounts of consilience are as satisfying as a pragmatic account, and that the existence of consilience is a strong piece of evidence for a pragmatic conception of theories. However, as he notes,

the central defect of pragmatic conceptions is their unsatisfying vagueness: What is a paradigm, a field, a domain, a problem, or a problem-solution?

The vagueness of its central concepts is another serious conceptual problem troubling HPS. These are rich, complex concepts, desperately in need of some fine-tuning.
Thagard sets out to provide them with this by drawing on research and concepts employed in artificial intelligence and cognitive psychology. His frame-systems view of scientific theories is motivated by research in these two areas which indicates that if we are adequately to describe the representation and processing of information, new 'knowledge structures' are required. It is not my intent to endorse this view of scientific theories here. However, since Thagard's discussion of the issues to which this essay is addressed is couched in its term, it will be useful to sketch the central features of it.

A frame is a complex knowledge structure used by computers to store and process information. A theory is regarded as an information-processing, or frame, system: a set of data structures and procedures for operating on them. The information stored in frames can be represented by propositions but what differentiates them from mere sets of propositions are their procedural properties. Frames can be used to execute a variety of forms of reasoning, including inheritance, default, and analogical reasoning. These procedural aspects of frame systems suggest is the pervasive importance of procedural, as opposed to propositional, knowledge. According to research in AI by Winograd, a procedural representation of knowledge (consisting of a computer program for performing some task) is particularly effective in domains where much knowledge is "heuristic", and consists of neither simple facts nor general knowledge, but has the form "If you are trying to do X under conditions Y, then you should try Z." The relevance of AI to epistemology is
dependent upon the computational theory of mind, as Thagard notes, but he is encouraged by empirical evidence supporting the view that frames (or the 'schemas' of cognitive psychology) play a crucial role in the human information-processing system.45

This account of theories as frame systems can be used to dispel some of the vagueness clouding Kuhn’s discussion of problem-solving and paradigms. For Kuhn, the key to problem-solving lies not in the deductive application of formulas, but in the crucial preliminary step of achieving an understanding of the problem. The student must discover

a way to see his problem as like a problem he has already encountered. Having seen the resemblance, having grasped the analogy between two or more distinct problems, he can interrelate symbols and attach them to nature in the ways that have proved effective before.46

Such knowledge is tacit and is acquired by practicing on exemplars -- concrete examples of the standard problem solutions within a particular scientific community. In frame terminology, the key to problem-solving lies in the procedural knowledge required to map objects into appropriate frames in an effort to

match the characteristics of the objects in the problem at hand with the slots of frames of stereotypical objects already stored as a result of work on previous problems.47

Because we have little conscious access to the procedural mechanisms of our frame systems, much of this knowledge how to solve problems remains tacit. Students acquire a common frame system for solving problems by learning and applying the exemplars in their field. Trained scientists refine and develop such systems by assimilating new problem solutions. Construing theories as frame systems, Thagard notes, integrates the two
main senses of Kuhn's term "paradigm" -- the first sense, according to which a theory is a shared set of commitments about how to view certain phenomena, and the second sense according to which these commitments are built up on the basis of exemplars or standard problem solutions. A frame system can, then, by controlling the way in which we process information, function like a world view. But it does this by using frames and procedures which are acquired through problem-solving, and which function primarily for the solution of new problems.48

That a pragmatic view of the structure of theories should be characteristic of much research in HPS is not surprising, for it draws attention to how scientific theories are used in actual historical contexts rather than exclusively to their formal nature as axiomatic systems or their relation to the world. This pragmatic orientation of the methodologies of Kuhn, Laudan and Thagard is typically accompanied by some distinctive views regarding the nature of an explanation or a problem-solution, and the aims of scientific inquiry. Truth may be among the aims of scientific inquiry, but their methodologies are not committed to this. (A fact which is greeted rather differently in each case. Both Kuhn and his methodology are unsympathetic to realism.49 Thagard stresses the compatibility of a frame-systems view with either realism or non-realism and adds that he would mount a separate, independent argument for realism.50 While Laudan, who does not construe realism in terms of the aims of scientific inquiry, declares himself a semantic realist.51) Nor do they endorse the classic deductive-nomological model of explanation. We might consider briefly some of their claims about these important issues.
Kuhn suggests that if we want to account for "both science's existence and its success" it is neither necessary nor helpful to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal.

He proposes that we explain science's success or progress not as a process of evolution toward anything but as evolution away from current states of knowledge and problem-situations. However, he can also be seen to align progress with increasing puzzle-solving capacity:

Later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied.

Thagard will add that in this they show greater consilience, where Laudan speaks of greater over-all problem-solving capacity.

Laudan emphatically disassociates his view from the "epistemological optimism of bloated realism" and adopts a position of epistemic skepticism with regard to truth:

... on any coherent account of what rational behavior is, it is irrational to adopt a goal which (a) we do not know how to achieve, (b) we could not recognize if we had achieved, and (c), was such that we could not even tell whether we were gradually moving closer to achieving it. "True scientific theories" seems to be precisely such a goal.

as are theories which are "likely or verisimilar, or any of the other ersatz surrogates for truth." Like Thagard's, his methodology is fully compatible with non-realism with regard to the aims of scientific inquiry. Thagard, however, emphasizes the compatibility of the frame-systems view with the realist position as well. Yet realists are likely to be dissatisfied both with the fact that truth is not a direct
concern of theories on the frame-systems view and with the novel cognitive account of explanation which that view provides. The latter is similar to Laudan’s account of how problems are solved, especially in its departure from the strictures of the traditional D-N model of explanation.

According to this account, explanation is regarded not as an eternal property of sets of sentences, but as something we do, using scientific theories. Specifically, it is a process of providing or achieving understanding through analogical procedures of locating and matching, rather than through deductive procedures. The conceptual mapping amongst frames which is involved in explanation is a process of integrating representations of phenomena into higher level representations. We explain general phenomena by applying the appropriate frame or set of frames: the key to explanation and the key to problem-solving are the same. We should consider how this view removes the impediments which Laudan sees to talk of explanation rather than of problem-solving.

On the traditional D-N model a theory is a set of sentences or axioms from which an explanandum is deductively derived. The explaining theory must be true or highly probable and it must entail an exact statement of the fact to be explained. This is not very congenial to a problem-solving approach. If the problem-solutions which theories provide us are like D-N explanations, then they are not approximative, which they are for problem-solving methodologies. Also, since true theories are required, questions of truth and falsity would
be relevant to problem-solving. Yet such questions typically are held to be irrelevant by such methodologies. If explanation requires exact statements of fact then it is very unlike problem-solving. These are the impediments which Laudan sees to talk of explanation within a problem-solving methodology.

But as we have just seen, the frame system view of explanation is quite unlike this. Indeed, it departs from the D-N model in both these respects. On this view, explaining and problem-solving are the same process of achieving or providing understanding through reasoning procedures of locating and matching, not deduction. It requires skills in applying appropriate frames. Since frame systems are conceptual structures, rather than propositions, they cannot be said to be true or false. Explanation moreover, can be approximative:

1. Just as in the explanation of simple events much simplification and abstraction is needed to map actual objects into canonical object frames, so we can expect only an approximate fit between the frames for phenomena being explained and the frames of the theoretical system. The mathematical derivations can similarly enjoy a certain amount of slack.

This not only removes the impediments to talk of explanation rather than problem-solving; it also contributes significantly to the project of a pragmatically-oriented problem-solving methodology within HPS. Matters such as what it is to solve a problem, and the cognitive procedures it involves, are considerably less vague than they have heretofore been.

I have wanted to give some indication throughout the above of the considerable interest of the frame-systems account as a development
within, and contribution to, the problem-solving methodologies of HPS. But the interest of it is much wider than this. Thagard argues that an adequate theory of the structure and growth of knowledge, especially of scientific knowledge, must move beyond the propositional accounts that have been standard in contemporary analytic epistemology. His alternative account postulates units of knowledge larger than propositions whose central function is the processing of information. It is developed with the support of recent work in AI and cognitive psychology. As he notes:

If we are to establish normative principles for the structure and growth of knowledge, we need to take into account the nature of the inference systems which knowers actually possess.... For computational purposes, knowledge organization of the sort found in frame systems seems to be needed.

The general procedural properties of such systems are the principal feature singling them out as importantly different from familiar logistic systems. Thagard brings this research powerfully, provocatively as well as problematically to bear on two central conceptual problems in HPS by developing a normative-descriptive methodology and an account of problem-solving that is substantially richer in detail, complexity and interest than previous accounts; one which I do not presume to have done justice to here. Its commitment to weak psychologism and a computational theory of mind may, render it problematic; weak psychologism is defended, a computational theory, assumed. But it does much to establish the conceptual viability of the research tradition.
Footnotes


2. Reichenbach (1938), 6-7.

3. Feigl (1965), 472.

4. Laudan (1977), 156.

5. See the exchange between Giere (1973), McMullin (1974) and Burian (1977). This topic has spawned an extensive literature. For a useful guide to some of this see McMullin (1979), 80-83.

6. On the role of philosophical assumptions in historical scholarship see especially Grunbaum (1966) and Agassi (1963). The invaluable contribution of case studies to philosophical accounts, especially of theory-appraisal, has been explored by a long line of philosophers, most recently by Thagard (1983), (1982); McMullin (1976), (1979); Gardener (1979); Laudan (1977); Brown (1977) among others. Earlier work along these lines includes that of Whewell (1840), Hanson (1958), Kuhn (1970), Toulmin (1972), Lakatos (1978), and Feyerabend (1970), (1975).


8. McMullin (1979) speaks of "historical philosophies of science," whereas Laudan (1979) speaks of "historical methodologies". The expression "historical philosophy of science" is owing to Thagard (1982). I regard it here as a regress toward tradition in, roughly, the Laudanian sense of that term. As such, it plays a problem-determining, heuristic, and justificatory role vis-à-vis its constituent theories of appraisal which contrasts most notably and markedly with that of the logical positivist tradition.


10. Feyerabend's epistemological anarchism, of course, insists that there are no such norms to be drawn. Thagard refers to this as "post-positivist depression". The present essay explores and further develops an alternative to it.


13. Laudan (1977), 57. McMullin (1979) makes a similar point: The most serious problem facing the defender of T2 (the thesis that history of science provides an indispensable warrant for epistemological claims about the nature of science) is the apparent circularity of his reliance on historical episodes where the "successful" theory is assumed to be readily identifiable. (p. 69)
14. See the discussion in Chapter Two, section I, and in Chapter Five, section I below.

15. Thagard's (1983) is a rough draft, currently under revision. Early versions of several sections of this manuscript have been published in Thagard (1978), (1982a), (1982b), and (1984a). I attempt, wherever possible, to refer to the published work. Thagard's general account in (1983) draws centrally upon recent research in artificial intelligence and cognitive psychology. The normative/descriptive methodology which it defends (and an approximation of which it purports to describe) is also practiced. The general model (FDN) of moving from the descriptive to the normative is arrived at briefly as follows. Two cases of descriptively based development of normative principles -- HPS and WRE -- are examined and used to suggest a model for a third, less familiar case, that of deriving logical principles from psychological practice. The insights gleaned from these three cases are incorporated into a general model (FDN). The principal concern of Scientific Thinking is with the structure of the knowledge possessed by human scientists, and a normative/descriptive methodology is used to arrive at an understanding of this. The frame systems view of scientific theories is motivated by results in both AI and cognitive psychology which have convinced numerous researchers that in order to describe the representation and processing of information new "knowledge structures" are required. These results are due to the efforts of AI researchers to develop a way of representing knowledge for efficient use by computers and of cognitive psychologists to account for the ability of human beings to store and retrieve large amounts of information. The AI computational notion of a frame and the allied psychological notion of a schema are explored. As the former can be made more exact than the latter "irremediably vague" notion, it is adopted as more suitable for epistemological analyses. But Thagard is careful to point out that the relevance of AI to epistemology is dependent on the computational theory of mind.

This brief sketch of Thagard's over-all project can give no more than a rough indication of its scope, and of the types of issues which a defense of that project would need to address. Such a defense will not be attempted here.


17. Ibid., 158.

18. And an unsupported one.


20. Ibid., 162.
21. Ibid., 163.
22. Ibid., 161.
24. The expression is Gardener’s, in (1979).
27. In his (1978), Laudan rejects the realist view that the aim of science is true or ‘approximately true theories’ on the grounds that, since we would not know how to recognize a true theory if we had it, such a view would render science a utopian, and so irrational, activity. He marks his distance from the “epistemological optimism of bloated realism” and declares himself an epistemic skeptic, though a semantic realist.
28. See Laudan, (1977) 99, where he rejects the Lakatosian notion of a ‘hard core’. His claim is that there are certain sacrosanct elements of a research tradition whose rejection amounts to repudiation of the tradition itself. But contra Lakatos, the set of elements falling in this (unrejectable) class changes through time. See also p. 130 where Laudan suggests that although the very general characteristics of rationality are trans-temporal and trans-cultural, the specific parameters which constitute rationality are time- and culture-dependent
30. Ibid., 258.
32. The other “base case” is WRE in ethics.
33. My account here is based on that of Thagard (1983) and (1982) 27, but departs somewhat from it in format.
34. We must move beyond reflective equilibrium (Goodman’s fit between inferential practice and normative principles), according to Thagard (1982a), 40, since it is of no help in actually justifying normative principles. The problem, as he notes in (1983) is that an individual may reach reflective equilibrium while possessing an inferential system that is resoundingly non-efficacious. Instead, the justification of a set of normative principles is based on the place of the principles in a defensible inferential system. Such a system must be a coherent one and coherence is to be evaluated according to criteria (robustness, accommodation and efficacy) to which the achievement of reflective equilibrium is irrelevant. What really matters is not equilibrium, but progress, i.e. the
development of better and better inferential systems. Improvement of the latter, he suggests, may well come about through an oscillating process of richer and more efficacious principles, practices, theories and goals. Cf. Thagard (1983), 39-40.

35. Thagard (1982), 36.


37. Following Haack (1978), Thagard adopts a 'weak psychologism', according to which logic is prescriptive of mental processes. It is an alternative to 'strong psychologism' (which adds that logic is not only prescriptive but descriptive of mental processes) and 'anti-psychologism' (the view, held by Frege and Popper that logic has nothing to do with mental processes). 'Weak psychologism' uses empirical psychology as a starting point, since it presupposes an empirical account of what mental processes to be descriptive about, but goes beyond mere description of actual mental processes to consider what sort of inferential practices are normatively correct. Hence weak psychologism cannot escape the charge of subjectivism and relativism which is the chief motivation for resistance to admitting the relevance of psychology to epistemology. (Thagard (1983), 3).

This escape from relativism requires the model FDN, which offers an account of how descriptive empirical matters are relevant to but not constitutive of prescriptive concerns.

38. It also sets it apart from the recently popular semantic approach to theories à la van Fraassen (1980) and Stegmüller (1976). However, the pragmatic and semantic approaches are seen to provide mutual support for one another at certain points. One of these is in the explanation of general phenomena. On Thagard's pragmatic account, explanation essentially consists in the integration of a frame system describing low level phenomena at a more general level. This is achieved through matching and the establishment of an A-KIND-OF link. The latter, he notes, are based on a sort of homomorphism:

we match slots in the description of typical behavior of refracted light with slots describing typical behavior of waves or particles. Then the typical behavior of light in the particular case is seen as typical of more general behavior. Our original theoretical frame system is enriched by a new application, now conceptually integrated with it. The set-theoretic conception of theories captures much the same relation: when we say the phenomenon x is a P, we are saying that x is isomorphic to P in the sense that the model describing x is among the models characterized by P. (Thagard (1983), 117).


41. For a more detailed account, see Thagard (1983), 104-5.

42. These forms of reasoning are discussed in Thagard (1983), 47-87.

43. Winograd (1975).

44. Cf. Ibid., 190.

45. Notably, Brewer and Treyens (1981), and Chi et al. (1979).


47. Thagard (1983), 108.


49. In his (1970), Kuhn observes:

   One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that 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 populates nature and what is 'really there'.

   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 match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusory in principle. (p. 206).


55. See Laudan (1979), 24.
57. Ibid., 117.
58. Ibid., 113.
59. Ibid., 120.
60. Laudan (1977), 22-3.
61. Thagard (1983), 118.
62. Ibid., 47, 61.
CHAPTER TWO

THEORY APPRAISAL IN HPS: A CRITICAL REVIEW

Introduction

In the preceeding chapter, two distinguishing features typical of research in HPS were noted. One is that theories are regarded not as aperiodal and ahistorical sets of propositions, but as temporal entities. Another is that a pragmatic approach is typically adopted towards them. Characteristically, the concern of those who work in this tradition is not with the formal nature of theories as symbol systems, nor with their relation to the world, but rather with their use in particular contexts. Work in this tradition, then, draws upon, and elaborates, the sense in which theories are appropriately construed as temporal artifacts: constructed, developed and deployed by scientists in problem-solving contexts.

This construal of theories, as we have seen, is attended by a number of problems which bring into question the conceptual viability of HPS. Appraising theories so construed is rendered problematic. An effort is made to move from descriptive matters (selected historical cases) to normative ones, (methodological prescriptions), yet one cannot derive an ought from an is. Moreover, HPS theories of scientific appraisal are especially troubled by conceptual vagueness. Their fundamental units of analyses, such as the Kuhnian 'paradigm' and Laudanian 'problem-solving', have been disturbingly vague. This is all
the more telling a charge given the formal precision, strength and beauty of logical positivist accounts.

Typically, such vagueness is acknowledged, and attempts are made to overcome it. Kuhn, in his (1970) Postscript, in his (1970b) and (1972), is openly doing just this. Laudan appears willing to tolerate what is essentially a common-sense and unarticulated conception of problem-solving. But both he and his critics have drawn attention to the fact that much in his account is promissory in nature. He also acknowledges the troubles generated by the normative/descriptive paradox. His contribution to the resolution of the latter problem, as we have noted, is wanting in a number of respects. The promise of more recent efforts to address these two problems, such as Thagard's, is considerable. He offers a detailed account of a normative/descriptive methodology, of how descriptive matters can be relevant to, without being used to derive, normative principles. The approach to theories as inferential frame systems can be used to illuminate and bring needed clarification to Kuhn's talk of paradigms, and with Laudan, of problem-solving. The computational notion of a frame is precise; frames can be given a relatively exact characterization. A rich account of problem-solving, and of the procedural knowledge it requires, is provided as well as a novel view of explanation as problem-solving.

As a result of these developments, the conceptual viability of HPS seems much less in doubt. I want now to consider the matter of its fertility. I will argue that, especially with regard to the three
areas of traditional neglect (mentioned in the Introduction to Chapter One), HPS proves fertile indeed. In the next few sections I wish to establish this by looking at some of the work being done in these areas by historical philosophers of science (notably Kuhn, Laudan, McMillin and Thagard). The next few chapters apply, modify and develop some of that work: When scientific theories are regarded as temporal artifacts or dynamic inferential systems, theories of rational scientific appraisal have to address a number of concerns which traditionally they have not. The need to accommodate as rational appeals to conceptual and methodological, in addition to empirical, considerations in contexts of scientific appraisal is recognized, as is the need to acknowledge a second modality of appraisal -- theory pursuit -- and to provide a criterion for it which will enable us to appraise the promise of a theory. I turn now to an examination of these and related matters.

I. The Role of Conceptual Considerations

Certainly one of the most distinctive features of much recent work in HPS is the discussion of appeals to conceptual (including methodological) considerations in contexts of appraisal and of the role to be ascribed to such considerations in theories of scientific appraisal. Perhaps the most notable instance of this is the work of Laudan, though the existence of conceptual problems has been recognized by a number of philosophers, including Whewell, Toulmin, Popper and Lakatos. Laudan argues forcefully that there is a pressing
need for theories of scientific progress and rationality in which conceptual refinement and problem-solving are acknowledged as essential parts of theory construction and assessment. The dominant philosophies and historiographies of science over the past two centuries have been committed to the view that theory choice should be governed primarily, if not exclusively, by narrowly empirical considerations. As a result, they have tended to ignore conceptual issues altogether, or to dismiss them as "verbal disputes, quarrels of words, as unfortunate and certainly unscientific confusions which were nothing but symptoms of a relatively immature scientific mentality." Unlike Kuhn on the one hand, and Popper, Lakatos, Agassi and Koertge on the other, Laudan's problem-solving methodology grants a significant role to appraisals of the conceptual and methodological well-foundedness of scientific theories. Such considerations play a critical and evaluative, rather than a merely heuristic and inspirational, role. We might consider some of the principal features of this Laudanian account of the role of conceptual considerations in theory appraisal, and a few of the problems which it raises.

According to Laudan, the central cognitive test of any theory involves assessing its adequacy as a solution to certain empirical and conceptual problems. He describes empirical problems as first-order questions about the substantive entities in some domain, whereas conceptual problems are higher-order questions about the well-foundedness of the conceptual structures (e.g. theories) which have been devised to answer the first-order questions. Internal conceptual problems arise for a theory when it exhibits internal inconsistencies,
circularity, or ambiguity as regards its basic categories of analysis. External conceptual problems are generated for a theory (T) when it conflicts with another theory or doctrine believed by the proponents of T to be rationally well-founded. The latter may arise when two scientific theories from different domains are in tension, or when T is in conflict either with the methodological theories of the relevant scientific community or with any component of the prevalent world view.

Both types of conceptual problems, he notes, are characteristics of theories; they "have no existence independent of the theories which exhibit them, not even that limited autonomy which empirical problems sometimes possess." His central appraisal measure for scientific theories is 'problem-solving effectiveness. This involves the application of the following mini-max strategy:

the overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalies and conceptual problems which the theory generates.

Progress in some scientific domain can occur if and only if the succession of theories in that domain shows an increasing degree of problem-solving effectiveness.

There are at least two difficulties with Laudan's account to which I would like to draw attention here. Note that on the mini-max strategy outlined above, the problem-solving effectiveness of a theory is increased if conceptual problems are minimized. In other words, conceptual considerations would seem to play a negative rather than a positive critical role, despite Laudan's occasional claims to the
contrary. The fewer conceptual problems there are, the better off the theory is. This is in keeping with his view that such problems are characteristics of the theories or conceptual structures devised to solve first-order problems. If a theory or conceptual structure is problematic in this way, its overall problem-solving effectiveness is lessened.

The difficulty is that Laudan's appraisal measure is not sensitive, as it should be and as he apparently intends it to be, to an important feature of the theories or conceptual structures of science. This is that they are developed and refined at least partly in response to the conceptual problems they have spawned. The conceptual growth (or stagnation) of a theory turns in part upon the ability it has displayed over time both to resolve the external problems it has generated and to undergo internal conceptual refinement by eliminating the inconsistencies, circularity or ambiguities from which it suffers. We might regard this as an indication of the conceptual viability which the theory has demonstrated, i.e. the capacity it has shown to undergo conceptual growth and refinement. Laudan offers us no means of positively appraising such theoretical achievements, achievements which should, at least in some instances, stand to the credit of the conceptual structures devised to answer first-order empirical questions, increasing rather than decreasing their problem-solving effectiveness. He appears to want to make room for assessments such as these, but his mini-max strategy effectively rules them out. He notes, for example, in his discussion
of internal conceptual problems, that the well-foundedness of the conceptual structures generated by science can and often does increase over time through careful clarifications and specifications of meaning (which is, as William Whewell observed more than a century ago, one of the most important ways in which science progresses). He also observes that most theories, at their inception and for some time after, are riddled with conceptual problems. Yet his appraisal measure does not allow a positive assessment of the conceptual viability a theory may have displayed.

We do, of course, want the number and importance of the conceptual problems a theory faces at some time $t_1$ to tell against the theory in contexts of appraisal, and Laudan's measure allows for this. But we also want the capacity which a theory has shown prior to $t_1$ to undergo conceptual refinement -- its demonstrated viability -- to tell for the theory. Such assessments are particularly important in contexts of pursuit, as opposed to acceptance. The rational pursuitability of a theory turns, in part, upon the capacity it has displayed to undergo conceptual refinement and to resolve its conceptual problems (even if, in the course of doing this and of increasing its empirical problem-solving capacity, new conceptual problems have arisen). Moreover, as we will see in the case study of Daltonian theory presented below, appraisals such as this typically do figure in the decision of scientists to pursue a theory.
Yet, when we turn to Laudan's discussion of the context of pursuit (which includes a brief consideration of Daltonian atomism) 
this is neither recognized nor provided for. In the two cases which he cites as illustrative of the role which assessments of promise, 
feucndity or progressiveness have in determining the pursuitability of research traditions -- that of Galileo and Dalton -- reference is made 
only to the increase in empirical problem-solving which theories in these traditions have exhibited over time. He notes that it is always 
rational to pursue any research tradition which has a higher rate of progress than its rivals, even if its comparative problem-solving 
effectiveness is lower. This rate of progress is determined by 
identifying the changes in the problem-solving effectiveness, or 
momentary adequacy, of theories in the tradition during a specified 
time span \( t_1 \ldots t_n \). Once again, this is a matter of deducing the 
number and importance of the conceptual problems which the theory or 
thories face at \( t_1 \) and \( t_n \) from the number and importance of empirical 
problems solved.

This determination of the rate of progress a theory or theories 
may display does not adequately capture the role which conceptual 
considerations play in contexts of appraisal, and especially of 
pursuit. The synchronic determinations at \( t_1 \) and \( t_n \) do not fully 
capture the dynamics of conceptual refinement and problem generation, 
and so overlook an important consideration which figures in contexts of 
pursuit. Assuming certain empirical problem-solving abilities, a 
theory \( T \) may, according to Laudan, at \( t_n \) be a less effective problem-
solver than some rival \( T' \), yet still be worthy of pursuit if it
displays a higher rate of progress over \( t_1 \ldots t_n \) than its rivals. Using Laudan’s appraisal measure, one would involve a reduction in the number and importance of its conceptual problems at \( t_n \).

But consider Daltonian theory, for example, which eventually managed this, though not until late in the 19th century. Prior to that time, it was considerably more conceptually troubled than the rival affinitist theory, and for most of that time the empirical problem-solving abilities of the two were equivalent. Yet, it was rational to pursue Daltonian theory well before the late 19th century. At its inception early in the 19th century it was a bold, ambitious theory, vastly more conceptually troubled than its affinitist rival. Yet, for all its conceptual problems it quickly displayed impressive conceptual resources, proving itself an increasingly fertile and resilient theory. However troubled a conceptual structure, it guided fruitful empirical research and demonstrated a growing ability to explain many different classes of facts. It also demonstrated its viability, undergoing conceptual refinement and problem-resolution even though in the course of this and of its empirical problem-solving successes, new conceptual problems were generated. Considerations such as the above were frequently cited by 19th century chemists in appraisals of the theory, and they are important ones to take into account, particularly in contexts of pursuit. Laudan’s appraisal measure is not sensitive to them.
Laudan's account of the role of conceptual considerations in contexts of appraisal is wanting in a second respect. Although Laudan refers to conceptual problems as nonempirical, he notes that

In point of fact, there is a continuous shading of problems intermediate between straightforward empirical and conceptual problems.

He adds that for heuristic reasons, he has concentrated on the distant ends of the spectrum. And, indeed, we are offered no indication of how (or for that matter, whether) conceptual problems and empirical problems interact to influence the course of theory development. Clarification of this would greatly enhance our understanding of how problem-solving in science actually works. This, in turn, might serve to constrain and to guide normative methodological proposals. The first part of this task is undertaken in the Newtonian case study below, while some suggestions regarding the second part are offered in the final chapter. This case study provides us with a valuable example of 1) how the solution to an empirical problem can lead to a conceptual problem, and 2) how conceptual problems themselves contribute to the generation of empirical problems.

II. Theory Pursuit as a Modality of Appraisal

The commitment of logical positivism to the construal of theories as atemporal sets of propositions, to scientific theories which are (perhaps in several senses) 'finished' products, places considerable constraints on proposals regarding rational scientific appraisal. Scientific theory appraisal emerges as an essentially synchronic
affair, and theory acceptance as the sole modality of appraisal recognized. Moreover, accounts of rationality issuing from this tradition typically take the form of what Kuhn has called "the search for algorithmic decision procedures", the effort "to produce an algorithm able to dictate rational unanimous choice". By contrast, problem-solving methodologies in HPS have typically attempted to provide for rational diversity of action and, by so doing, for a second modality of rational scientific appraisal - theory pursuit.

Kuhn, for example, has contended that rational diversity of action is required in contexts of appraisal if theories are to be developed by scientists into acceptable problem-solvers. Defending the "looseness" of his five criteria of theory choice (i.e., the fact that, having invoked such shared criteria in appraising a theory, scientists may yet rationally disagree with one another), he claims that this is the source of their effectiveness as an indispensable means of spreading the risk which the introduction or support of novelty always entails.... Most newly suggested theories do not survive. Usually the difficulties that evoked them are accounted for by more traditional means. Even when this does not occur, much work, both theoretical and experimental, is ordinarily required before the new theory can display sufficient accuracy and scope to generate widespread conviction. In short, before the group accepts it, a new theory has been tested over time by the research of a number of men, some working within it, others within its traditional rival. Such a mode of development, however, requires a decision process which permits rational men to disagree, and such disagreement would be barred by the shared algorithm which philosophers have generally sought. If it were at hand, all conforming scientists would make the same decisions at the same time.
Laudan's approach, like that of Kuhn, would provide for rational diversity of action among scientists by allowing problem choice and the criteria of problem solution to remain sufficiently indeterminate. John Nicholas has recently drawn attention to the value of this, suggesting that it helps us to understand the diversity of opinion and action in different scientific communities. Moreover, he adds, it may enable us to account for the phenomena of rashness and tenacity which we find within scientific communities, arising from specialization of training and problem interest. Laudan stresses, in his discussion of the context of pursuit, that if we suppose that the context of acceptance exhausts scientific rationality and that it is rational for scientists to work with and explore only the theories they accept, the greater part of the history of science will be rendered irrational. In particular, we will be unable to offer a satisfying account of two frequent and well-attested historical phenomena: cases in which scientists investigate and attempt to develop the problem-solving abilities of newly emergent theories or research traditions, and cases in which scientists work alternately in two different, and even mutually inconsistent, research traditions.

The development of chemistry in the 19th century is an instructive example in all of the foregoing respects. As we will see in Chapter Four, the atomic theory was actively, and sometimes ambivalently, pursued for the better part of the century before it achieved and merited widespread acceptance. Throughout that time scientists with a variety of research interests worked to develop and extend the problem-solving abilities of Dalton's bold and controversial
New System of Chemical Philosophy and of successor theories in the atomic research tradition. A number of these scientists also contributed significantly to theories in the rival and conceptually incompatible affinitist research tradition. We will also see that the extensive, thorough-going disagreements within the chemical community over the merits of the atomic theory were the result of differences in research orientation and problem choice. Problem-solving by chemists in certain areas of chemical inquiry, such as organic chemistry, was more fruitfully advanced by atomic theories, whereas the problem interests of others, notably physical chemists, were better served by theories in the affinitist tradition. The pursuit of theories in both research traditions contributed to the specialization and methodological diversification which the 19th century chemical community underwent.

Whether or not it is rational for a scientist to pursue one theory rather than some other turns, then, in part, upon the types of problems with which s/he is concerned. More generally, it depends upon the problem context in which the scientist is immersed. The same is true of theory acceptance within the problem-solving methodologies of Laudan and Thagard. We might consider what their methodologies have to say about theory acceptance in the interests of articulating the merits of, and refining, the less familiar notion of theory pursuit.

On Laudan's account, theories are conceptual structures enabling scientists to solve empirical problems which are determined largely (though not exclusively) by the research traditions of which they are
part. In doing so, as noted above and explored more fully below, they generate conceptual problems. Some of these are present at their inception, while others arise as the theory is developed and deployed in empirical problem-solving contexts — those for which it was initially constructed and those to which it is extended. 24 Rational acceptance of such a problem-solving conceptual structure (and the research tradition in which it is embedded) does not require, nor does it consist in, the warranted assertion of the best established theory. 25 What then does it involve? What commitments does the scientist make when s/he accepts a particular problem-solving conceptual structure?

Because Laudan has more to say about the context of acceptance than he does about the concept of acceptance, it is difficult to be confident about the responses he would make to these questions. Given what he does say however; the following seems plausible, if problematic. So far as epistemic commitments go, there is no commitment to the truth (or some cognate thereof) of the theory. Laudan has argued forcefully 26 that we do not have any reason for believing that scientific theories are true. He rejects the view that scientific rationality presupposes a successive convergence on the truth and that warranted acceptability presupposes legitimate grounds for believing that which is acceptable. 27 The only belief involved in rational theory acceptance would seem to be the belief that the theory is the most effective problem solver. Yet acceptance of a theory and its related research tradition clearly involves substantial pragmatic commitments on Laudan's account. This follows in part from the
research-guiding role of research traditions. They specify the appropriate methods to be used for investigating problems and constructing theories in their domain.  

While Laudan's problem-solving approach differs in numerous respects from the semantic approach recently developed by Bas van Fraassen, there would seem to be significant agreement between them regarding the pragmatic dimensions of theory acceptance. While for van Fraassen the only belief involved in acceptance is the belief that the theory is empirically adequate, he stresses that more than belief is involved. A scientist who accepts a theory commits herself to using the conceptual resources of that theory in conducting her research activities:

To accept a theory is to make a commitment, a commitment to the further confrontation of new phenomena within the framework of that theory, a commitment to a research programme, and a wager that all relevant phenomena can be accounted for without giving up that theory. That is why someone who has accepted a certain theory, will henceforth answer questions ex cathedra, or at least feel called upon to do so. Commitments are not true or false; they are vindicated or not vindicated in the course of human history.

Pragmatic commitments such as these appear to follow from Laudan's account of theory acceptance as well. The nature and degree of such commitments are, as we will see momentarily, useful in helping us to determine what, for Laudan, distinguishes contexts of pursuit from those of acceptance.

There are a number of respects in which Laudan's account of theory acceptance is problematic. If theories are conceptual
structures which serve to direct and advance scientific problem-solving, they are not the type of thing which can be true or false. Yet Laudan espouses a semantic realism according to which scientific theories have truth values. He also suggests, without elaborating upon this, that to accept a theory is to treat it as if it were true. But of course if a theory is not the type of thing that admits of truth and falsity it would be unreasonable to treat it as if it were. Surely Laudan's point here is a pragmatic one. To accept a theory or research tradition is to commit oneself to using it to structure one's scientific activities, to draw upon its conceptual resources in contexts of problem-solving. It is also to believe that it is the most effective problem-solver.

If, as Laudan suggests, a scientist working in some problem domain D ought to accept that theory T which has demonstrated superior problem-solving effectiveness in D, then, given the pragmatic implications of his account of theory acceptance, a scientist who accepts T ought to conduct some significant portion of his or her research activities in accordance with T. Several consequences would appear to follow from this for Laudan's discussion of the contexts of acceptance and pursuit. One is that a scientist who failed to conduct any research in accordance with T could not be said to accept T, regardless of any declaration of allegiance to the theory (though this alone would not amount to the rejection of T). Another is that it would not follow from the fact that a scientist does indeed conduct a significant portion of his or her research in accordance with T that s/he accepts T.
Laudan's analysis does permit a scientist who accepts T to pursue as well some competitor T' in D. It will be rational for a scientist to do this, according to Laudan, provided that T' has demonstrated a higher rate of progress than T (and any other competitors in D) over some specified interval of time. Notice that it follows that, for some particular specified interval of time, there can be at most one theory which it is rational to pursue (though it may also be rational to accept some other theory on the basis of its overall problem-solving effectiveness). Notice too how crucial the matter of specifying a time interval becomes. A slightly shorter or slightly longer interval might well change the determination of which of two competing theories it is rational to pursue. This is particularly problematic in view of the purely quantitative criterion, (i.e. rate of progress) which Laudan employs to determine the rationality of pursuit.

On the Laudanian approach then, it would seem that a scientist who pursues a theory T' is committed only to the belief that T' has demonstrated a higher rate of problem-solving progress than has any other competitor. Are pragmatic commitments involved as well? And, if so, how do they differ from those which figure in theory acceptance? Given his account of theory acceptance, the reasonable response for Laudan to make, I think, is that there are and that they differ primarily in degree from those which are operative in contexts of acceptance. A scientist cannot be said to be pursuing T' unless s/he is conducting some research in accordance with it. Some wager that the theory will continue to demonstrate a high rate of progress, and that it will be able successfully to accommodate new phenomena, may be
involved. The extent of such commitments will turn in part upon the availability of an acceptable competitor in D. If there is no acceptable theory in D, and only two or more theories which are equally worthy of pursuit (as was the case in chemistry for most of the 19th century), then the pragmatic commitments involved will be more extensive. Laudan does envisage situations such as these despite the fact, noted above, that his discussion of the rationality of pursuit suggests that, for some particular specified interval of time, only one theory can be worthy of pursuit.

In contrast to Laudan, Thagard does not explicitly recognize pursuit as a modality of scientific theory appraisal. However, in view of what he has to say about 'theory-acceptance', such an account seems clearly needed. Unlike Laudan, whose position regarding both acceptance and pursuit neither probes nor benefits from his suggestion that theories are to be understood as conceptual structures designed to further the problem-solving aims of scientists, Thagard's position on acceptance is very much developed with an eye to what it is to accept theories understood as dynamic inferential, or frame, systems. Consequently, it has much to offer the project underway by problem-solving methodologies in HPS to enrich our accounts of theory appraisal. I will not attempt to outline here all of the important features of Thagard's account of theory acceptance, but to draw attention only to a few of these. Those explored are those most relevant to the present discussion of pursuit as a modality of scientific appraisal. I will argue that Thagard's discussion of acceptance not only lends itself to the recognition of pursuit as a
modality of appraisal, but assumes that such an account can be provided. Accordingly his position would gain considerably from a consideration of it.

When theories are construed along Thagardian lines as frame systems, normatively justified acceptance of, or inference to, a scientific theory is to be understood as the acquisition of a frame system by reliable means. The inferential process involved here is a form of inference to the best explanation, but two caveats regarding this are in order. The first is that explanation in the cognitive frame-based notion, consistent with a problem-solving approach, examined above. The second is that the standard view of inference is set aside as inadequate to account for the crucial procedural aspects of frame systems. On the standard sentential/propositional conception of inference, knowledge is increased by observation or by inferring new sentences or propositions from those which are already part of the existing body of knowledge, the goal being to derive true conclusions from true premises. At its simplest level, the procedural integration of new with existing knowledge required by the information-processing approach is achieved by a process similar to the addition of new sentences — a slot in a frame is filled. However, as Thagard points out, simple slot-filling will not suffice when we face significant advances in knowledge, such as those which attend the development of new theories. These require the construction of new frames and macrostructures. Hence Thagard adopts a broader, more-complex view of inference as a process by which existing knowledge structures are used to construct new knowledge structures. The result is a conception of
inference which is not consonant with standard notions of validity. Thagard believes this is not a great loss since no good account of inductive validity is at hand and since his main concern is to establish as normatively correct inferential procedures which are efficacious in accomplishing our cognitive goals.

To accept a theory then is to acquire a frame system with a great utility for processing information. This is far more complex than merely adopting an attitude toward a set of propositions: procedural connections with existing frames must be established. Thagard observes that a methodological conservatism follows from this. Such frame systems are not easily abandoned. A great utility is attached to their retention. While there are criteria which can establish one frame system as a better explanation than some alternative (Thagard's candidates are tenacity, simplicity, and analogy), no rational choice between the two is possible until the alternative has been learned and is in place.

All of this, of course assumes the existence of fully developed frame or conceptual systems, those in a position to be defended as providing the best explanation. Thagard does have something to say about theories whose careers are not so far advanced. His discussion, though, bears more on the context of discovery than it does on the context of pursuit, on the process and patterns of reasoning by which new theories are generated, rather than on the development and evaluation which may follow their initial discovery and precede their
eventual acceptance. What little he does have to say about the latter is of interest however, and merits our consideration here.

Thagard stops short of recognizing pursuit as a distinct modality of scientific theory appraisal. Indeed, he mentions the notion only in passing, indicating his agreement with Laudan\textsuperscript{35} that Peirce’s abductive pattern of reasoning is concerned not with the discovery but with the pursuit (or, as he adds, the weak evaluation) of theories already discovered. He speaks instead of the plausibility of a hypothesis, adopting this term from Peirce\textsuperscript{30} but construing it somewhat more widely. A theory is plausible when it explains some surprising phenomena or when it is of the right kind, i.e. when we have reason for expecting that the theory needed to explain some phenomena will be of that kind. The point which Thagard is most concerned to establish here is the following. While the reasons advanced for the plausibility (or implausibility) of a theory may not be sufficient to merit our accepting (or rejecting) it, the same sorts of reasons are relevant in each case. Specifically, considerations of consilience, simplicity and analog\textsuperscript{3} will be cited. He adds that considerations of dynamic consilience\textsuperscript{40} are especially relevant to plausibility since growth -- extension of the range of facts explained -- is a central feature of scientific theories. There is, then, no fundamental logical difference between claims for the plausibility of theories and claims for their acceptability. The only major difference between the two is that, unlike judgements of theory acceptability, judgements of theory plausibility need not be comparative.
Thagard's primary contribution to discussions of theory appraisal, within HPS clearly consists in his careful, detailed account of the nature of inference to the best explanation, and of the criteria relevant to appraisals of theory acceptability. It is, I think, both more satisfying and more workable than is Laudan's largely quantitative account of problem-solving effectiveness. However, if it is to be more satisfying and more workable as an account of problem-solving effectiveness, Thagard will need to clarify and refine it in a number of ways. Let me suggest some of the directions in which he might move and why he needs to do so.

Above all perhaps, Thagard should examine the matter of how contexts of acceptance (in which the objects of appraisal are well-established theories) differ from those of pursuit (in which the objects of appraisal are comparatively undeveloped theories, in need of substantial extension and modification if they are to realize whatever problem-solving potential they may have as conceptual structures or inferential systems). He has correctly and usefully called our attention to some of the important similarities between the two contexts, particularly to the fact that there is no special logic or pattern of reasoning peculiar to each and that the same sort of evaluative criteria are appropriately invoked in each context. Indeed, it may be his concern to stress just this which leads him to overlook or to underemphasize several fundamental dissimilarities between the two contexts of appraisal. While these differences are only differences in degree and in weight placed upon the varying criteria of evaluation,
they are, I will argue, significant enough to merit the recognition of two distinct modalities of appraisal.

He does address some of these dissimilarities. He notes, for example, that, unlike appraisals of theory acceptability, appraisals of theory pursuability (or 'plausibility') need not be comparative. Now we have already seen that for Laudan this is a respect in which the two modalities of appraisal are similar rather than dissimilar. We have also seen that Laudan's quantitative criterion of theory pursuit (according to which it is always rational to pursue that theory or research tradition which has demonstrated a higher rate of progress than any of its rivals) not only rests precariously and unconvincingly on the exact interval of time specified, it has as well the unfortunate consequence that, for any given interval of time, there can be only one theory in some domain which it is rational to pursue. Where Laudan's criterion of pursuability (highest rate of progressiveness) does not permit more than one theory to be worthy of pursuit for some given interval, Thagard's criteria would allow this. Such a result is valuable in helping us to account for a wide range of historical cases of theory pursuit, including that of 19th century chemistry in which scientists confronted two competing theories, neither of which were worthy of acceptance but both of which merited pursuit.

Thagard also observes that among the various criteria of theory appraisal considerations of dynamic consilience are directly relevant to assessments of theory plausibility. The merits of dynamic consilience as a criterion of theory appraisal are explored at greater
length in the next section of this chapter. My primary concern here is simply to draw attention to the respects in which Thagard's recognition of such a criterion, and the central role he assigns to it in assessing theory plausibility underscores the need to recognize a modality of appraisal distinct from that of acceptance as well as the inadequacy of construing this modality in terms of 'plausibility'.

The notion of static consilience, Thagard points out, is intended to serve as a measure of how much a theory explains, so that we can use it to tell when one theory explains more of the evidence than another theory. 14 If it explains more classes of facts than does \textit{T}, 15 Clearly such a criterion plays a central role in contexts of acceptance, in determining which theory provides the best explanation. Static consilience presupposes that a totality of classes of facts -- the total evidence -- is given. This is generally how it appears when a scientist presents the results of his her research. Arguments to the best explanation cite a range of facts explained.

Yet, he continues, since growth is a central feature of scientific theories, it is important in contexts of appraisal to assess how successful the theory has been over time in extending the range of facts explained. While the considerations relevant to determining the static consilience of a theory are purely synchronic ones, those relevant to determining its dynamic consilience are not:

a theory \textit{T} is dynamically consilient at time \textit{n} if at \textit{n} it is more consilient than it was when first proposed, that is, if there are new classes of facts which it has been shown to explain. 17
Thagard contends that this dynamic notion of consilience "must be taken into account in considering the acceptability of explanatory hypotheses."48

While it seems correct to maintain that the diachronic considerations which figure in assessments of dynamic consilience can appropriately be cited in contexts of acceptance, it also seems too strong to require that they be taken into account. Why should they be? In contexts of acceptance the concern is to determine which theory provides the best explanation. What is crucial to such a determination is that the theory be able to demonstrate a superior degree of static consilience. It is likely that any theory well-developed enough to be a serious competitor for acceptance will be able to demonstrate that it is considerably more consilient than it was when it was initially proposed. And if, comparatively, the theory is also able to demonstrate a superior degree of dynamic consilience, this fact ought to tell for the theory. But unlike comparative assessments of static consilience, assessments of dynamic consilience (comparative or otherwise) are neither decisive nor required in contexts of acceptance. At best, the criterion of dynamic consilience would seem to play only a supporting role in arguments for the best explanation.

As a criterion of theory appraisal then, dynamic consilience may be relevant to judgements regarding theory acceptability, but it is neither essential nor decisive in reaching such judgements. What of judgements regarding theory 'plausibility'? Assessments of dynamic consilience are, Thagard claims, "directly relevant" to these
judgements. Recall that, on his view, a theory is plausible if it has explained some surprising phenomena or if we have reason for expecting that it is the right kind of theory to explain some phenomena. Now it is possible for a theory to explain some surprising phenomena without its having demonstrated any dynamic consilience. The theory may have succeeded in explaining these phenomena when it was first proposed. Indeed, to judge a theory 'plausible' in this first sense is to make a purely synchronic appraisal of it. It is not to appraise the theory's dynamic consilience, i.e. the success it has had, over time, in extending its problem-solving range (the range of new classes of facts explained). The same holds true of judgements of theory 'plausibility' in the second sense, except that in this case dynamic consilience will be of no use at all in assessing plausibility since it makes no reference whatsoever to the theory in question being the right kind of theory to explain some phenomena.

The sense in which dynamic consilience is "directly relevant" to the judgements of plausibility which Thagard describes is, then, questionable. The problem, I would suggest, lies not so much with the notion of dynamic consilience as a criterion of theory appraisal (though in the next section I argue against its adequacy), as it does with that of theory 'plausibility'. Growth is a central feature of scientific theories: it is a feature which enters, appropriately, into the appraisals which scientists typically make of their theories. Such appraisals are fundamentally diachronic, developmental ones. They take into consideration both how the theory has fared so far and how it might be expected to fare in the future. The criterion of dynamic
consilience provides for these considerations. But to suggest that, in applying it, scientists are determining the 'plausibility' of a theory is in one case mistaken, and in the other, misleading. It is mistaken if 'plausibility' is construed in the second sense for such a judgement requires reference to reasons for expecting the theory is the right kind to explain certain phenomena and the criterion of dynamic consilience does not provide this. And it is misleading if 'plausibility' is construed in the first sense for, in applying the criterion, scientists are judging much more than whether or not, at \( t_n \), \( T \) explains some surprising phenomena. They are assessing the ability that \( T \) has shown, prior to \( t_n \), to extend its explanatory, or problem-solving, range.

What is needed, I submit, is the recognition of a second modality of appraisal, theory pursuit, which is distinct from that of acceptance in a number of respects. To determine whether or not \( T \) is acceptable, whether it provides the best explanation or is the most effective problem-solver, a comparative assessment is clearly required. Only one theory can be 'best' or 'most effective' in some problem-solving domain. The objects of appraisal, in contexts of acceptance, are theories well-developed enough empirically and conceptually, to be candidates for acceptance. Yet theories grow only if they are developed, and scientists must decide not only which theories are worthy of acceptance but which are worthy of pursuit and further development. To determine whether or not \( T \) is worthy of pursuit, whether it is a promising theory in light of what it has done and might be expected to do, no comparative assessment is necessary (though it
can usually be had and, given practical constraints and the need to determine research priorities, will often be useful. The typical objects of appraisal in contexts of pursuit are theories whose problem-solving abilities have yet to be well-developed, tested and explored.

If we are to include an appreciation of the dynamics of theory
development in our accounts of scientific theory appraisal, Thagard's notion of 'plausibility' is inadequate to the task. A second modality of appraisal, addressed to the rationality of pursuit, is required. Yet we can still acknowledge, with Thagard, that the same sorts of reasons or criteria guide theory appraisal in both contexts. Dynamic consilience and static consilience (as well as simplicity and analogy which we have not considered at all here) may appropriately be appealed to in judgements regarding either the acceptability or the pursuitability of scientific theories. However, if we are attempting to determine the rationality of acceptance, consideration of (comparative) static consilience is necessary, while consideration of dynamic consilience is not. Conversely, if we are concerned to establish whether a theory is worthy of pursuit, an assessment of its dynamic consilience is essential whereas an assessment of its static consilience is not.

In the next section I suggest that dynamic consilience, as Thagard formulates it, is not adequate as a criterion of pursuit, and propose in its place a criterion of fertility. Before doing this though, is worth pointing out another respect in which Thagard's own
account would benefit from the clear recognition of a second modality of theory appraisal. If, as he claims, to accept a theory is to acquire a frame system by reliable means, and if we cannot correctly speak of acceptance until the procedural mechanisms for processing frames (i.e. for enabling us to integrate and inter-relate new information with the old) are in place, then some account of the nature of appraisals made prior to this seems clearly in order. While Thagard does have something to say about how new theories or frame systems are discovered, he has little to say about the development they subsequently undergo and the criteria which guide this development. We need to know more about this if we are to understand theory acceptance in terms of the acquisition of a frame system.

III. A Criterion for Theory Pursuit

We have seen, in the preceding section that Thagard's notion of dynamic consilience has some merit as a criterion of theory appraisal. Unlike most traditional criteria, it does provide for the diachronic considerations which typically figure in actual cases of scientific theory appraisal. When scientists evaluate the merits of a theory they typically do take into account the actual performance of the theory over time, i.e. what it has done to advance their problem-solving efforts thus far. An appraisal of a theory's dynamic consilience, of the success it has had over time in adding to the classes of facts which it explains, will permit an estimate of the theory's future performance in this regard. Yet how adequate is it as a criterion?
which will enable us to determine whether or not a theory is promising, and so, worthy of pursuit?

In contexts of acceptance, where the concern is to determine which theory has established itself as the best explanation (or most effective problem-solver), scientists are interested primarily in assessing the theory's actual performance—what it has already proven itself able to do, rather than what it has the potential to do with further development. Appraisals of comparative dynamic consilience may be of some importance here, in determining the rationality of theory acceptance. But in contexts of pursuit scientists are primarily concerned to determine a theory's promise or potential, with what they occasionally refer to as the theory's fertility or fecundity, and are interested in the theory's actual performance—its career to date—principally insofar as it assists them in making that determination. We have already seen reason to doubt the adequacy of dynamic consilience as the sole criterion for such appraisals of a theory's potential: it cannot provide for assessments of theory promise or pursuability (for judgments of theory 'plausibility' in Thagard's second sense of that term) where the concern is to determine whether or not the theory is of the right kind to explain some phenomena. Nor is Laudan's 'rate of progress' any better suited to the task. And, while Kuhn would draw our attention to what he refers to as a less standard criterion—fruitfulness—which "deserves more emphasis that it has yet received," he unfortunately does little more with it than to note that it is of special importance to actual scientific decisions. Ernan McMullin, however, has offered an extended discussion of such
criterion which is important for us to consider here as it directly addresses the question of how a theory's promise or potential may be appraised. After presenting his account of a criterion of fertility, I will argue that it suffers from several serious defects and, in Chapter Five, attempt to develop a more satisfying criterion of fertility by means of which the promise of a theory may be assessed.

McMullin distinguishes between two types of theory-appraisal. In the first, an epistemic appraisal, we are primarily concerned to determine the acceptability of the theory. For McMullin, a realist, this is to concern ourselves with the truth-value of the theory:

To what extent has the theory been corroborated? Does it conform reasonably closely to the structure of the real? Is one warranted in accepting the existence of the theoretical entities it postulates?

To arrive at such a determination, a criterion of P-(proven) fertility is invoked. He stresses that this is a past, not a future, oriented criterion, which addresses the theory's performance or proven record, not its potential:

It is estimated by the actual success of the theory in opening up new areas, in meeting anomalies, and so forth. To estimate the P-fertility of a theory, one has to retrace its career and see how successful it has been in suggesting the right modification at the right time and in allowing the incorporation of new areas not originally foreseen.

When we "estimate" a theory's P-fertility then, it is with a view to epistemic appraisal. There is, however, another type of appraisal one might make of theory, a heuristic appraisal, in which a theory is subjected to a "second, and very different sort of demand":

what is its research-potential for the future? how likely is it to give rise to interesting extensions? Does it show promise of being able to handle the outstanding problems (inconsistencies, anomalies, etc.) in the field? Is it
likely to unify hitherto diverse areas, or perhaps open up entirely new territory?

The criterion to be invoked here is also quite different. Whereas P-fertility confirms the truth-value of a theory, the criterion of U-fertility determines its as yet untested promise. This is a "tentative, future-oriented affair" which involves examining the theory here and now, and estimating its imaginative resources (its 'neutral analogy' in Mary Hesse's phrase) for future extension and modification.

McMullin stresses that an estimation of U-fertility does not require one to trace the career of the theory over a period of time. He also adds that while the two sorts of appraisal are obviously not unrelated (since part of an epistemic appraisal is concerned with P-fertility and the latter can be understood as the way in which the original U-fertility of the theory proves out), they are nevertheless quite distinct. An estimation that a theory enjoys considerable U-fertility and so displays a high degree of heuristic promise tells us nothing of its epistemic status (i.e., its truth) though he notes, interestingly, that it may be enough to persuade us to invest our efforts in it. Conversely, an established theory may, in virtue of its P-fertility, have received a very positive epistemic appraisal but this tells us nothing of its heuristic potential for the future.

It should be clear from what has been said thus far that McMullin is offering not one but two, quite distinct, criteria of appraisal. If we are concerned to determine the rationality of theory acceptance, the criterion of P-fertility (which corresponds roughly to Thagard's,
dynamic consilience) must be invoked. This is a diachronic and wholly backwards-looking appraisal. It involves, McMullin claims, an "estimation" of the theory's past performance but not of its likely future performance. In light of whatever proven abilities it has displayed (or failed to display) to date. By contrast, a quite different criterion of U-fertility must be invoked if our concern is to determine the rationality of theory pursuit. Such an appraisal is synchronic and purely forward-looking. It involves an estimation of the theory's future performance which does not take into consideration the theory's achievements (or lack thereof) to date. While there is considerable interest in McMullin's account, I find these proposals regarding theory appraisal unsatisfying in a number of respects.

First of all, it should be noted that if McMullin is correct, the reasons that might be advanced for pursuing a theory will differ in kind from those we might advance for accepting a theory. Different sorts of reasons will be relevant in each case. Claims for the pursuitability of theories will be supported by appeal to the criterion of U-fertility, while appeals to P-fertility (and so to a theory's actual performance to date) will play no role in substantiating such claims. Conversely, an appraisal of P-fertility will be required to establish claims for the acceptability of theories, but appraisals of U-fertility (estimations of how the theory might be expected to perform in the future) will be irrelevant to supporting these claims.

Yet McMullin offers no evidence that scientists' reasoning about theory acceptability differs in this respect from their reasoning about
theory pursuability, nor does he present any argument to the effect that it ought so to differ. Such an argument would be difficult to mount since it would need to make a convincing case that the development which a theory has actually undergone ought not figure in any appraisal of its pursuability. It would also need to convince us that an estimation of a theory's potential to further extend our problem-solving ability in a field ought not affect our assessment of its acceptability. Neither conclusion would seem plausible. Surely, no matter how minimal its career, how a theory has fared under empirical testing and how it has fared with regard to the resolution of its conceptual problems are factors which are highly relevant to the rationality of pursuit, to determining whether or not the theory is promising and worthy of further investigation. Since such factors are irrelevant to an appraisal of U-fertility, it is hard to see how McMullin could defend his claim that an estimation of the U-fertility of a theory "may be enough to persuade us to invest our efforts in it," at least if we are to understand by this that it would be rational to do so. Similarly, any determination that a theory is likely (or unlikely) to give rise to further interesting extensions or to solve any unsolved (empirical or conceptual) problems ought to carry some weight (although it may not be decisive) in our reasoning regarding its acceptability. Moreover, as the case studies below demonstrate, such factors typically do enter into the appraisals which scientists make of their theories.

McMullin's account of these two criteria, moreover, is clouded by what appears to be a conflation of the distinction between 'appraisal'
and 'estimation'. When, for example, we appraise a theory's dynamic consilience, we compare its present to its past explanatory abilities in order to estimate its ability to continue to add new classes of facts to those which it explains. We appraise, then, something that is given or proven about the theory, so that we can get some sense of what we can reasonably expect of its future performance. Yet McMullin speaks of P-fertility (a criterion of appraisal which he stresses, tells us nothing about the theory's future, expected performance), as being "estimated" by the actual success of the theory, by its career to date. P-Fertility is, apparently, a criterion which 'estimates' the theory's past, what is given about the theory, but which cannot be used to provide an estimation about what can reasonably be expected of the theory in the future.

There are, then, two unwelcome consequences of McMullin's discussion: 1) in judging the pursuitability of theories we must bring to bear different sorts of reasons or criteria than we do in judging their acceptability; and 2) the rationality of pursuit requires no appraisal of a theory's past performance or career while the rationality of acceptance remains unaffected by any assessment of a theory's potential or likely future performance. To avoid these consequences, we might adopt a single criterion of fertility which is relevant to determining the rationality of pursuit as well as of acceptance. Such a criterion would provide an assessment of a theory's problem-solving potential, or expected future performance, based on its actual performance over a given period of time. Growth is a central feature of scientific theories and, if our normative proposals
regarding theory appraisal are to accommodate the dynamics of theory development something like a criterion of fertility is essential. In appraising the rationality of pursuit, it plays a crucial role for, in determining whether a theory merits further development, an estimation of its problem-solving potential, its likely future performance, is indispensable — and we cannot assess its potential as a problem-solver without attending to its past performance, to the career that it has enjoyed to date.

In addition to the theory's past performance, such an assessment might also draw upon the type of consideration which Thagard's second sense of 'plausibility' singled out, i.e. whether or not the theory is the right kind to explain certain phenomena, or to solve certain problems, P. We may, for example, have reason to expect that the theory needed to solve P will be of a particular kind K because other theories of kind K have successfully solved problems analogous in certain respects to P. If the theory under appraisal is of kind K, this fact will figure in our assessment of T's potential to solve P.

To appraise the fertility of a theory then, is to assess its problem-solving potential by considering both its actual performance over a given period of time and whether or not it is the right kind of theory.

Our interest in a theory's performance (past or future) will not, of course, be confined to its empirical performance. Part of what we are concerned with in considering the theory's actual performance over a given period of time is its empirical yield, i.e. the success it has
had in extending the range of facts explained, or of solved empirical problems, but we are also concerned with how it has fared conceptually. More particularly, we are concerned with what I will refer to as the theory's conceptual viability -- the capacity it has shown to undergo conceptual growth and refinement. Theories in the early stages of their development are often extremely conceptually problematic and, as the case studies below will show, they are developed and refined at least partly in response to the conceptual problems they have spawned. Initially, a theory is likely to have greater success at extending its range of solved empirical problems than at achieving an actual reduction in the overall number and importance of its conceptual problems. (That this is so is not surprising. To refine and clarify a theory internally -- by eliminating inconsistency, circularity, ambiguity and vagueness in its central theoretical concepts, and externally -- by bringing the theory into alignment with other established theories with which it conflicts or by bringing them into alignment with it, is a project which is neither readily nor easily completed.) Moreover, in the course of extending its empirical problem-solving range, the theory may well generate new conceptual problems which did not trouble it at its inception. Any assessment of a theory's potential which did not take into account its conceptual performance would be radically deficient. The fertility of a theory is a function not only of its empirical yield, but also of the conceptual viability it has displayed, over a given period of time.

It has already been argued above that Laudan's appraisal measure of problem-solving effectiveness and the mini-max strategy that he
adopts do not adequately capture the role which conceptual considerations play in theory appraisal. Theories undergo conceptual as well as empirical growth (or stagnation). The conceptual growth (or stagnation) of a theory, turns, in part, upon the ability it has displayed over time both to resolve the external problems it has generated and to undergo internal conceptual refinement by eliminating the inconsistencies, circularity, or ambiguities from which it suffers. Yet the capacity which the theory has shown to undergo such refinement -- its conceptual viability -- cannot, as we have seen, tell for the theory on Laudan's account. He offers us no means of positively appraising this theoretical achievement. The criterion of fertility proposed here allows for this for, in assessing a theory's potential as a problem-solver, we must address not only its empirical abilities but its conceptual viability. Since theory fertility determines theory pursuitability, the capacity a theory has shown over a given period of time \( t_1 \ldots t_n \) to undergo conceptual refinement and to resolve its conceptual problems can positively affect our appraisal of its pursuitability. Moreover, contra Laudan, it may do this even if the overall number and importance of its conceptual problems has not been reduced by \( t_n \).

While appraisals of theory fertility are essential to determining whether a theory is promising and worthy of pursuit, the criterion of fertility may also play a supporting role in arguments for the best explanation. It is, thus, relevant to judgements regarding theory acceptability, although, as was noted in the preceding section, in establishing the rationality of acceptance the criterion of static
consilience plays the most significant, or weighty, role. The value and relevance of the criterion of fertility in contexts of acceptance, as in contexts of pursuit, is due to the fact that it acknowledges growth as a central feature of scientific theories and provides us with a way of bringing diachronic considerations to bear in our appraisals of them. In judging which theory provides the best explanation at a given time $t_n$, it is appropriate to inquire as to how the theory has performed, conceptually and empirically, over time and how, on the basis of this past performance, we can expect it to perform in the future. In contexts of acceptance, by comparison to those of pursuit, we are likely to be more confident of such an assessment of the theory's potential, or expected future performance, as a problem-solver since, typically, the theories under assessment will already have relatively well-developed careers behind them. They will, in other words, have had more time to establish their problem-solving ability and our estimation of their future performance will be affected accordingly.

More must be said about fertility as a criterion of rational scientific appraisal, and more will be said below. The need to develop such a criterion, to recognize theory pursuit as a second modality of appraisal, and to accommodate as rational appeals to conceptual considerations in contexts of appraisal is pressing. Standard studies of theory appraisal by philosophers of science have failed to appreciate this, and some of the consequences of our traditional neglect of these matters will be considered in the case studies that follow. My central concern in this chapter has been to offer a
critical survey of some of the efforts to remedy this neglect, stemming from recent work in historical philosophy of science. It should be evident from the foregoing discussion that these efforts have been fruitful ones, extending our understanding of some important and long-overlooked dimensions of scientific theory appraisal. It should also be evident, however, that they stand in need of further investigation, clarification and development. That task, begin here, will occupy us for the remainder of this Essay.
Footnotes

1. Typical but not definatory. The two features mentioned are to be understood as neither necessary nor sufficient conditions, but as characteristics which hold typically rather than universally of research in HPS.

2. In his (1978), Laudan directly addresses this issue: Whether a problem-solving approach to science will ultimately prove to be sound depends on our collective capacity to make good on several promissory notes. We need a coherent account of how problems are generated. ...We need unambiguous criteria for individuating problems and for weighting their relative importance; Bayesian approaches may help here.... It would be nice as well, to have a heuristics for searching for problem solutions; work in artificial intelligence and cognitive psychology may be instructive here. ....In these and many other respects, what we have therefore is a sketch of a model of scientific growth rather than a fully developed model.

3. See, for example, McMullin (1979), Hull (1979) and Musgrave (1979).

4. The notion of 'fertility' is discussed below, Chapter Five, Section III.

5. See, for example, Kuhn (1970), (1977); Laudan (1977), (1979); McMullin (1979) and Thagard (1983), (1984).


7. Laudan (1972), 2.

8. While Kuhn gives at least some role to conceptual problems, Laudan regards his approach to them as more dismissive than illuminating since foundational disputes are, for Kuhn, characteristic of immature or pre-paradigmatic science. (For his criticisms here, see Laudan (1977), pp. 47, 150-1, 173-5). However, this dismissal of Kuhn may be a bit hasty, as it ignores what Kuhn has to say about-foundation disputes in revolutionary science (Kuhn (1970), especially Chapters vii-x). It also overlooks the fact that Kuhn's five criteria for theory evaluation (discussed in Kuhn (1970)), are broad enough to include in their scope conceptual and methodological considerations.

9. Laudan distinguishes his position on the role of conceptual considerations from that of these philosophers in his (1978).

11. Ibid., 68.


13. Conceptual viability is discussed at length in Chapter Five, Section I.


15. This criticism is explored fully in Chapter Five, Section I.

16. Ibid., 113.

17. This is argued below, Chapter Five. Note that, in discussing the context of pursuit, Laudan seems to have in mind primarily cases in which a scientist accepts one theory T, while pursuing another, incompatible, theory T'. He argues that this can be a rational strategy, and indeed, if T' enjoys a higher rate of progress than its rivals, this will always be a rational strategy according to Laudan. While I agree that this can be a rational strategy, I have argued that the appraisal measure which he uses to determine the rate of progress of T' and its rivals does not establish the stronger claim that such a strategy is always rational. By determining the problem-solving effectiveness of a theory over an interval t1, ..., t, as Laudan suggests, no account can be taken of the manner in which the theory achieves a possible increase in problem-solving effectiveness. All that the synchronic determinations at t, and t reflect is the change in the number and importance of the empirical and conceptual problems the theory has managed to solve, not how the change has come about.

T' might, for example, have decreased its conceptual problems (and so, have increased its problem-solving effectiveness on Laudan's account) by sacrificing fertility and consilience. Its degree of consilience may be low in comparison to that of a competitor. Its competitor may also have proven itself a more fertile, if more troubled, conceptual structure, one characterized by risk-taking and problem-generating extensions of its domain. If such is the case, it may be rational to pursue the competitor and it may not be rational to pursue T', despite the fact that, using Laudan's measure, T' will exhibit a comparatively higher rate of progress.

Conceptual considerations like those mentioned, then, are vital to weigh in contexts of pursuit. They may figure even more prominently in contexts such as those described in Chapter Four, contexts in which the empirical problem-solving abilities of T and T' are equivalent but neither theory is clearly worthy of full acceptance. In the absence of a clearly acceptable theory in some domain, it will be particularly important to understand why it might be rational for a scientist to commit his or her research time and resources to the pursuit of one theory rather than another.

19. McMullin has provided us with some of the most balanced and stimulating discussions of the consequences of treating theories as finished products in his studies of 'logistic' vs. 'historicist' approaches to theory appraisal. See especially his (1976) and (1979).


22. Kuhn (1977), 332. Newton-Smith charges, in his lively critique (1981) of Kuhn, that Kuhn over-reacts to the sound point that there is no algorithm for theory choice "for he seems to fail to appreciate that even if there is no rationally grounded algorithm to guide our decisions there may none the less be rational considerations which it is relevant to appeal to in justifying our decisions" (p. 116). Yet Newton-Smith's entire case for the alleged over-reaction rests on his reading of a single, somewhat cryptic, observation of Kuhn's that (t)hough the experience of scientists provides no philosophical justification for the values they deploy (such justification would solve the problem of induction), those values are in part learned from that experience, and they evolve with it. (1977), 324.

Moreover, as Newton-Smith himself notes, Kuhn does recognize (ibid., pp. 330-1) the existence of considerations which influence conduct without constituting binding rules when it comes to ethical decision-making, and Kuhn certainly intends this as a point of positive analogy with scientific decision-making.

23. Nicholas (1980), 228.

24. Some actual instances of this are explored in the case studies of Chapter Three. Recall too that for Laudan both theory acceptance and theory pursuit require comparative evaluations. Cf. his (1977), 120.

25. Laudan contends (1978) that if it did so science would remain forever irrational.


27. He also rejects a number of other "time-honored" linkages. See his (1978).


30. Even though one might argue, as Thagard does, that they may be used to make true or false claims about the world. See his (1983), 167-171.


32. Though once again one might argue that to accept a theory is to treat the assertions it can be used to make as if they were true or false. Such an approach might be open to Thagard but it would not seem to be for Laudan in view of his epistemic skepticism.

33. This is similar to the treatment of acrasia, or weakness of will, in moral philosophy where it is argued that if a person consistently fails to act in accordance with some moral code s/he cannot properly be said to accept that code despite any claims s/he might advance to this effect.

34. While I think this second point (or consequence of Laudan's account) is correct, I challenge the appropriateness of the first below, Chapter Five, Section II.

35. Most notably when he draws attention to the numerous historical cases in which the empirical problem-solving abilities of competing theories have been virtually equivalent. See his (1978), pp. 47-8. He does stop short of actually claiming that in these cases there was no acceptable theory to be had, though such a claim would seem to follow if the competitors in question were equally conceptually problematic. This, I argue below, was in fact the case during much of the 19th century in the area of chemical theory.


37. Laudan provides a useful distinction between these two contexts in his (1980). Interestingly, he describes the context of pursuit as "a nether region" between the contexts of discovery and of ultimate justification.


40. This notion is discussed below, in Section III of this chapter.

41. See especially Thagard (1978).

42. Ehrman McMullin (1979), among others, has raised doubts about the workability of Laudan's model.
43. As he stresses in his (1977): "All evaluations of research traditions must be made within a comparative context". (p. 120).

44. Thagard (1978), 79.

45. The more precise definitions are as follows:
    let T be a theory consisting of a set of hypotheses (H₁, ..., Hₖ); let A be a set of auxiliary hypotheses (A₁, ..., Aₘ); let C be a set of accepted conditions (C₁, ..., Cₙ); and let F be a set of classes of facts (F₁, ..., Fₖ). Then T is consilient if and only if T, in union with A and C, explains the elements of the Fₖ for k > 2.

    To get the comparative notion, let FT₁ be the set of classes of facts explained by theory T₁. Then we can choose between two different definitions of comparative consilience: (1) T₁ is more consilient than T₂ if and only if the cardinality of FT₁ is greater than the cardinality of FT₂; or (2) T₁ is more consilient than T₂ if and only if FT₁ is a proper subset of FT₂.
    Thagard (1978), 79.


47. Ibid., 83.

48. Ibid., 83.

49. This is argued at length below, Chapter Five, Section II.

50. The criterion of analogy plays an important role here. See Thagard (1978), 90.

51. Other criteria will need to be invoked as well.

52. Kuhn (1977), 32.


54. It should be noted that throughout his (1976), McMullin assumes realism, he does not argue for it. (The closest he comes to the latter is a passing observation to the effect that only a realist can apply notions like progress and growth to scientific change).

55. McMullin (1976), 422.

56. Ibid., 400-1.

57. Ibid., 423-4.
58. Ibid., 400.

59. Ibid., 428.

60. An assessment of its positive heuristic is also required. The various indices of fertility are discussed at greater length below, in Chapter Five.
CHAPTER THREE

ABSOLUTE SPACE: FROM DESCARTES TO NEWTON

Introduction

In "Absolute Space: Did Newton Take Leave of his (Classical) Empirical Senses?", I contend that the role of one of the central concepts in Newtonian theory --- absolute space --- was systematically misconceived by Newton's contemporaries and by numerous subsequent commentators on Newtonian theory. A methodologically-sensitized interpretation of the bucket experiment is tendered here in an effort to do justice to this role and to formulate an effective response to the question "What was absolute space and why was it necessary to do physics?". A satisfying response to this question is requisite if we are adequately to assess the conceptual well-foundedness of Newtonian theory. Against those who would offer what I refer to as an 'aeroplane' response to this question, I argue that Newton's postulation of absolute space was consistent with his methodological practice of classical empiricism and that, moreover, it did not even constitute a breach of the official Newtonian party line --- hypothesis non fingo. Absolute space, in other words, was not used in the scholium to explain, but to determine, the properties of things. It was a vital piece of theoretical equipment used in the description of the bucket phenomenon which served as an illustration of, rather than as evidence for, the theory of motion which Newton advanced in the Principia.
However, the conceptually problematic nature of absolute space has yet to be fully accounted for, and to do so we will need to consider in greater detail why it appeared to be necessary to do physics. My argument in the present chapter will be this. If Newton, in his dealings with the scholium's bucket, took the critical classical-empirical step of regarding "the Illustrations as an experimental basis", he also managed to use his idealized description of the phenomenon as an effective way of posing an empirical problem which his theory resolved. Absolute space was the bit of theoretical apparatus which provided the key to its solution. In the scholium, Newton was grappling with an empirical problem concerning vortical motion whose origins are to be found in the conceptual problems which troubled Cartesian theory, problems which Newton had painstakingly detailed in his early essay "De Gravitatione et Aequipondio Fluidorum". There, as we will see, Newton addresses himself to a fundamental internal inconsistency in Cartesian theory; while Descartes appeared to endorse an absolute view of space he offered an analysis of absolute motion which was at best semi-relativistic and compatible not with an absolute but with a relational theory of space. An investigation of the conflicting Cartesian sub-theories of space and of motion is conducted below in support of this.

The following case study, then, is intended to deepen our appreciation of the interaction between conceptual and empirical problem-solving in the development of scientific theories. Two of the most important results to which Newton is led as a result of his appraisal of the well-foundedness of Cartesian theory are: i) that the
conceptual problems which troubled the theory were responsible for its failure to provide a satisfying solution for an empirical problem, posed by phenomena undergoing vortical motion, in its domain; and ii) that if this empirical problem were to be resolved by a successor theory, such a theory would need to stand committed to absolute space. However, this case study also draws attention to the way in which the resolution of an empirical problem may itself generate conceptual problems for a theory. In the concluding section of this chapter we will see that although the postulation of absolute space was a central feature of the Newtonian solution to the empirical problem of vortical motion, it raised a conceptual problem for the theory of which Newton was no doubt aware, but which he was willing to risk.

I. Let us examine first the claim that, in the (principal) guise of the bucket experiment, the scholium poses the inquiry situation or empirical problem for which Newton, with the aid of absolute space (and given the definitions which precede the scholium as well as the laws of motion introduced immediately after) seeks to provide an adequate solution. Following Laudan, an empirical problem may be understood as a first-order problem, a substantive question about the objects which constitute the domain of any given science. What counts as an acceptable solution to an empirical problem may vary, since solution criteria (like the empirical problems themselves) are dependent upon the context of inquiry and subject to conceptual filtering. (For example, the empirical problem posed in a concrete fashion in the bucket experiment may have been solved by Newton's theoretically inspired interpretation but, come Mach and Einstein, have been re-
The quasi-relativistic theory of motion which Descartes fashioned would not only fail to grant this absolute generic space, or extended substance, any theoretical function, it would be inconsistent with it—a theory in which, as Descartes claims; there was no point "qui soit veritablement immobile" and in which, as Newton showed, "motion is not motion, for it has no velocity, no definition, and there is no space or distance traversed by it."\(^{26}\) The disenfranchisement of space or generic extension is accomplished by a relativistically conceived definition of place that brings Descartes' plenistic commitments to the fore. Place is not a "part of space which something fills evenly" (bodies do not occupy space they comprise it), nor does this definition even tacitly refer to generic extension or interior place. Place is "nothing but the surface of the surrounding bodies or position among some other more distant bodies."

The conceptual problems discrediting Cartesian theory compound and are responsible for that theory's inability to deal adequately with the objects in its domain and to resolve the first-order empirical questions it raises, notably about the phenomenon of vortical motion. Newton charges repeatedly in "De Gravitatione..." that the *Principes* tells us nothing (or very little) about what corresponds "to the nature of things" and far too much about what corresponds to or is the "product of our imagination."\(^{27}\) Specifically, Descartes has claimed that in order to determine the true and philosophical, unique and proper, motion of a body we must make appeal to the neighboring bodies that we consider and which seem to us to be at rest with respect to it, that we "arrestons en notre pensee." Thus, Newton complains,
inertial forces will be generated if the motion is relative: we might look at this motion as circular but non-vortical.

The effects which distinguish absolute from relative motion are the forces of receding from the axis of circular motion. For there are no such forces in a circular motion purely relative, but in a true and absolute circular (vortical) motion they are greater or less, according to the quantity of motion.

Here immediately follows a demonstration of such centrifugal forces generated in a bucket of water and thereby of absolute motion. The water, then, is undergoing absolute (accelerated) motion, motion relative to absolute space. Bucket phenomena such as that in the scholium, as well as the other phenomena listed under Definition V which suffer the effects of the counterpart centripetal force, testify to the absoluteness of space.

Regarding the bucket experiment as the concrete articulation of the type of empirical problem confronting Newton and his resolution of it (via a staging of it as a phenomenon) may serve to enhance our appreciation of Newtonian methodology — not the party line of fore-swearling hypotheses but the actual practice of classical empiricism. In particular, it underscores the effectiveness of the crucial methodological step of treating the illustrations of the theory as an experimental basis for it. Newton does not first point to a problem or raise a question then resolve or answer it. Rather, he presents us with a solution (the bucket phenomenon) which reveals the problem. Newton's experimentum crucis does not solve a problem and thereby increase the previous weight of the problem; rather "it is the solution which allows us to recognize the problem as a genuine problem at all."
Succeeding and competitor theories would be "expected either to solve it or to provide good grounds for failing to solve it."

Newton's contemporaries were flummoxed by the 'illustration' of absolute motion and sought to challenge his second inference from the existence of such motion to the absoluteness of space. They did not -- until Mach -- challenge the theoretically camouflaged first inference from the experimental data to absolute motion. They were unable to provide a relational theory which could resolve the problem and hence satisfactorily compete with Newton's theoretical resolution. The subsequent hegemony of Newtonian theory was not ended until Einstein and special relativity, for, as Earman points out:

What Mach offers ... is not a competing theory, but a hint of a competing explanation of the bucket experiment and some hints for constructing an alternative theory, e.g., the suggestion that an alternative theory which would explain the bucket experiment can be built on the notion that the inertial properties of bodies are not intrinsic but depend upon the presence and distribution of other bodies in the universe. ... Today Einstein's general theory is being challenged by competing theories, but without reference to such theories it is gratuitous to charge the adherents of his theory with a 'serious non sequitur' for postulating pseudo-Riemannian space-time. It is a measure of the contempt in which Newton is held by philosophers that such a charge could be made against his inference to absolute motion.

These philosophers (the previously discussed subscribers to the AR-2), unable to countenance absolute motion and absolute space within the framework of their empiricist epistemologies, have typically insisted that questions of truth and degree of confirmation are more important in theory evaluation than a theory's ability to provide adequate solutions to significant problems. This, together with a failure faithfully to assess Newton's methodological practice, has enabled
their premature and philosophically irresponsible dismissal of the Newtonian case for absolute motion and absolute space. They have, to paraphrase Reichenbach's own charge, hobbled (if not arrested) the analysis of the problems of philosophy of space and time.

II. If there are inertial forces, there is absolute motion. Newton has demonstrated that the water suffers the effects of such forces, and resolves the empirical problem by asserting that there is absolute motion. But, as his claim is also that if there is absolute motion, there is absolute space, the bucket phenomenon demonstrates as well that space is absolute. So the Newtonian solution also involves the recognition of the absoluteness of space.

This second link in his chain of reasoning however requires a more careful analysis for it provides a telling commentary on the origins and early beginnings of the empirical problem sketched above. Our claim thus far has been that, on the negative hand, absolute space was not postulated extra or post-theoretically as a hypothesis intended to resolve the empirical problem or first-order question posed in the guise of the bucket experiment by explaining the puzzling observation of the water's ascent. On the positive hand, it has been argued that absolute space is, rather, a theoretical construct — itself part of the theoretical apparatus set up by Newton in the Principia. The theory itself (not some mere hypothetical — adjunct to it) was needed to resolve the empirical problem. Further, the empirical problem — instantiated by a spinning pail of water — was designed to fit and was
couch* terms of the procrustean bed of the theory. The bucket experiment is staged, theoretically 'dished up'. (Hence, the apparent circularity detected by some in Newton's reasoning, a circularity endemic to classical empiricism.) Similarly, Newton's 'observations' in the bucket experiment are, insofar as the latter is handled as a phenomenon, theoretically motivated. In the scholiun to the Definitions a theoretical equivalence is set up with the aid of the experiment between 'suffering the effects of the forces of receding from the axis of circular motion' and 'undergoing absolute motion' and thereby with 'moving relative to absolute space,' (since to undergo absolute motion is to undergo translation with respect to absolute space).

It is a tribute to both the intricacies and the effectiveness of Newton's classical empiricism that an empirical problem is designed to fit, to be serviced by, as well as to help lay the foundations for his theory. But if the interpretation of the bucket experiment utus ushers us into Newtonian theory by providing a first and particularly illustrative instance of "how to collect true motions from their causes, effects and apparent differences" to be "explained more at large hereafter (in) the following treatise," it also is intended to persuade us out of Cartesian theory which could not come through with the quoted problem-solving goods. This is the issue to which we now turn.

One (among many) of the crucial deciding factors of inter-theoretic competition is the ability of a particular theory to solve a
significant (weighty) empirical problem. In the case at hand, an empirical problem is formulated in terms of the 'successor' theory and is designed to be solved by it. But as the bucket experiment is as much an experimentum crucis as it is a phenomenon, it is designed to be as well and at the same time decisive against a predecessor (Cartesian) theory. The solved empirical problem provides us with a means of, and reasons for, not only rejecting Cartesian theory but replacing it. Needless to say, although it was resolved in his own theoretical interests, the empirical problem did not spring full-blown into the head of Newton. It was in very large part the product of certain inadequacies and inconsistencies in a decaying Cartesian theory. (The use of 'decay' here is to underscore the progressive problem-solving impotence of Cartesian theory which Newton himself was 'focal in unmasking.) 'By resolving these,' Newton delivered the final coup de grâce to the decaying edifice.'

That Newton was fully conversant with the conceptual pitfalls and dead-ends of Cartesian theory is strikingly evident from a recently recovered essay of Newton's, "De Gravitatione et Aequipondio Fluidorum." This early essay (written some twenty years before the scholium) is impressive both as a philosophically acute analysis of the conceptual problems riddling the *Principia* and as an invaluable aid to a historically sound interpretation of the scholium. In a rich, revealing paper, H. Stein stresses the need for a reassessment of the scholium in light of the new data, a reassessment which goes some way towards exposing the astigmatic historical vision of the AR apologists.
Contending that Descartes was Newton's main philosophical target in the scholium, Stein insists that

to understand Newton's scholium on time, space, place and motion properly, it is necessary to realize that in it he was concerned to differentiate his theory not from Leibniz's but Descartes'. Establishing this difference was important to Newton for two reasons: Descartes' physics and cosmology constituted the most influential view in the scientific world at the time; and Descartes' mechanics was based upon a very confused semi-relativistic concept of motion, on which it would have been hopeless to build a coherent theory. In other words, the "prejudices" that Newton says his scholium was intended to remove were, in large part, those of the scientific community, and his aim was to establish a new technical terminology as the foundation for a coherent theory.

Near the end of this chapter we will briefly consider how it is that Newtonian theory, by resolving the pressing empirical problem outlined above, was heir to conceptual problems of its own. In the next several sections the task will be to support the claim that the empirical problem resolved in the scholium was itself generated primarily by the conceptual problems bounding Cartesian theory and with which Newton preoccupied himself most directly in his early essay.

The Cartesian origins of the empirical problem have already been unsubtly hinted at in the foregoing by the characterization of the bucket experiment and the other phenomena which figure under Definition IV as vortical-type situations. Such a characterization is borne out by a reading of "De Gravitatione ..." in which Newton tackles head on the consequences of Descartes' "absurd doctrine" regarding motion when applied to his theory of vortices and in particular to that exemplary case of a vortex—the solar system. Although the bucket has yet to
make its entrance', Newton offers a thought experiment which exhibits the same general structure and which is designed to deal with a similar, though more limited, blow to Descartes' theory of motion. Or, in other words, it provides support for the Newtonian claim that "physical and absolute motion is to be defined from other considerations than translation, such translation being designated as merely external" and more specifically that "only the motion which causes the Earth to endeavour to recede from the Sun is to be declared the Earth's natural and absolute motion. Its translations relative to external bodies are but external designations." The structural similarity to the bucket experiment is accomplished with the aid of a deus ex machina. As force is later applied to the bucket both to begin and to end its spin, Newton considers what would happen "if God should suddenly cause the spinning of our vortex to stop" as well as what would happen "if God urged the starry heaven ... with any very great force so as to cause it to revolve about the Earth." It is by means of this thought experiment that Newton hopes to demonstrate the absurdity of the Cartesian "consequence" that "motion can be generated where there is no force acting" and that there could be no motion even when the "greatest force was applied."

The entirety of this early essay documents in detail Newton's perception of the conceptual problems ensnaring the Cartesianians. Laudan describes such conceptual problems as higher order questions about the "well-foundedness of the conceptual structures (e.g., theories) which have been devised to answer the first-order questions" or to resolve the empirical problems to which the theory is addressed. In the case
of Cartesian theory (as Newton's "venture to dispose of his fictions" — and indeed any perusal of the Principes -- makes abundantly clear) we are faced with internal conceptual problems, problems arising not only from the "internal inconsistencies" which permeate the theory but also from the fact that its "basic categories of analysis are vague and unclear." Our discussion of these inconsistencies and ambiguities will take its cue from Newton's critical essay and attempt to show how, from an acute appreciation of the internal conceptual problems of the Principes, Newton was led to formulate the ("resolved") empirical problem of the scholium.

III. Descartes' concept of space, in marked distinction to Newton's, is perhaps the vaguest, most unclear category of analysis in the Principes. This, together with his recurrent conflation of the pivotal distinction initially set up between two senses of 'place' and of 'motion', is responsible for the crippling internal inconsistency in his theory -- his apparent commitment on the one hand to an absolute substantival theory of space and, on the other, to a theory of motion which is at best (and his own claims to the contrary notwithstanding) semi-relativistic.

Some brief preliminary remarks regarding Descartes' general philosophical method in the Principes will be useful. In Part I (Art. 61-62) two important distinctions are drawn which Descartes will later employ in his discussion of space, place and motion: "la distinction modale" and "la distinction qui se fait par la pensée".
The first of these, he observes, is between "le mode que nous avons appelé façon" ('façon' being literally 'the manner in which a thing is made') and the substance upon which the façon depends and which the façon diversifies. The distinction is significant in that, while we can have a clear and distinct idea of "la substance sans la façon" we cannot have a clear and distinct idea of "une telle façon, sans penser à une telle substance". As an example, he cites the distinction between figure or movement and the "substance corporelle" upon which they both depend. It should be noted that Descartes has claimed that we can have a clear and distinct idea not of une (a particular) substance but of la substance (which in this case would be rendered in English as substance in general) sans la façon. This is reaffirmed in Art. 63 where he notes that we can consider extended substance without thinking of some particular thing which is extended. ("Il y a quelque difficulté à séparer la notion que nous avons de la substance de celles que nous avons de la pensée et de l'étendue: car elles ne sont pas de la substance que par cela seul que nous considérons quelquefois la pensée ou l'étendue...")

The distinction of reason, or the distinction "qui se fait par la pensée," is to be contrasted to the foregoing. Descartes will get considerable mileage out of this distinction in Parts II & III, referring repeatedly to things which differ only "par notre pensée." The distinction of reason, according to Descartes, consists in the fact that we sometimes distinguish "une substance de quelqu'un de ses attributs" without which (attribute) it is impossible to have a clear and distinct idea of the particular substance. Duration is an example
of such an attribute which is distinguished from substance only "par la pensée" since any substance which "cessè de durer" ceases thereby to exist. There is one other point of note here, and that is that Descartes observes that the "extension du corps" (not extended substance) is an attribute which differs "du corps, qui nous sert d'objet" only because we confusedly think of such a particular extension without thinking of the body which is extended. Thus, apparently, while we can think of extended substance (a generic notion) without thinking of any particular thing which is extended, we cannot have such a clear and distinct idea of the extension of body (corps) unless we are thinking of a particular body which is extended.

The absolute view of space which Descartes appears to endorse (and indeed to argue for by maintaining the unintelligibility of a 'vuide' or bit of empty space) is one in which space is identified with extended substance (the generic notion just discussed) of which we do have a clear and distinct idea without thinking of any particular thing or body which is extended. (Sklar refers to this as "Descartes' 'plenum' view of space which ... suggest(s) the idea of space as the total 'stuff' of the world."!) It may be helpful to recall that Descartes is primarily concerned with the "principes des choses materielles" in Part II. Accordingly, in the first article he claims that there is a certain substance, extended in length, width and depth in the world and that this extended substance is properly called the body or the substance of material things. "Et cette substance étendue est ce qu'on nomme proprement le corps, ou la substance des choses matérielles." This permits him to pass from the generic notion of
extended substance to the more tractable and particular one of individual things or bodies and eventually to a discussion of two senses of 'place.' This passage is rather complete and was, in Newton's eyes, unfortunate, for Descartes will articulate his theory of motion in view not of his commitment to generic space but upon the basis of his distinction between two senses of 'place.'

It is upon the generic notion of extended substance which is distinct from particular bodies that the case for the Cartesian view of space as absolute or substantival must be made. Newton, at rate (and this suffices for our purposes here) clearly regarded Descartes as having committed himself, however tokenly, to the absolute view of generic space sketched above, "...Descartes himself had an idea of extension as distinct from bodies, which he wished to distinguish from corporal extension by calling it generic." He then cites the Principes, part II, Art. 10 "What space, or interior place is", 12 "How it differs from (corporal substance) in the way in which it is conceived," and 18 "How opinion about the vacuum, absolutely conceived, is to be emended." Moreover, he offers the concept of generic space repeatedly as the means by which Descartes might have avoided some of the more pressing 'absurdities' which trouble his theory. What Newton finds distressing, and damning insofar as it is it which backs Descartes into a quasi-relativism, is Descartes' failure to articulate his theory of motion by referring to "the generic space." That theory is instead grounded upon the distinction between two senses of 'place' and according to it there is no point in the universe "qui soit veritablement immobile" nor is there any place of any thing in the
world "qui soit fermé & arrêté" except insofar as we chose a frame of reference when "nous l'arrestons en notre pensée." (Art. 13) Consequently, the Cartesian 'unique and proper motion' is not an absolute but a relative motion: the "transport ... d'un corps du voisinage de ceux qui le touchent immédiatement, et que nous considérons comme en repos, dans le voisinage de quelques autres." (Art. 25). Newton however will insist that "...physical and absolute motion is to be defined from other considerations than translation, such translation being designated as merely external." 20 To appreciate why Newton so claimed, we must first look more carefully at the relevant passages in the Principes where motion is discussed and which fall under Newton's critical scrutiny.

IV. The first, or vulgar, sense of exterior place is introduced in Articles 13 & 14. The common usage of the term 'lieu' only draws our attention to the general situation of a certain body among other bodies, to how "il est situé entre les autres corps." If we adopt this sense of the term, we are able to lapse into saying that at any given moment an object both changes place and stays in the same place. As an example we are asked to consider a man seated in the cabin of a ship which is being carried out of port by the wind. This vulgar sense of exterior place would allow us to assert of him that at each moment he is both changing his situation — vis-à-vis the shoreline — and not changing his situation or place — vis-à-vis the cabin. 'Place' then, in sense 1, refers merely and loosely to the situation of a certain body among other bodies. There are no restrictions placed upon how
this situation is determined: we may appeal to the farthest fixed
star, to the nearest shoreline, to the ship's cabin, or to all three at
the same time.

Several passages further along, in Article 24, Descartes draws
our attention back to this example and to this sense of place in
describing the notion of motion prevalent in common usage. According
to this vulgar sense of motion as "l'action par laquelle un corps passe
d'un lieu en un autre," it is possible to say that a particular object
is at the same time both moving and not moving. And, although
Descartes will later maintain the same thing about the Earth, at this
point he clearly regards such a statement as attributable to confused
thinking and to be remedied by recognition of the proper and
philosophical sense of place.

This second philosophical sense of exterior place is sketched in
Art. 15. 'Place' in this sense is determined by the surface or surface
area (superficie) which surrounds a body. The superficie is not any
part of the body or bodies which surrounds a particular body, but only
the "extremité" between them. The distinction between the superficie
which surrounds a particular body and the body itself is a "distinction
modale." While we can have a clear and distinct idea of a body without
thinking of its surface, we cannot have any such clear and distinct
idea of a surface without thinking of a body whose surface it is.
However, when we think of surface in general (as opposed to the surface
of a particular body) it seems to us that it always remains the same —
provided that "elle est de mesme grandeur & de mesme figure."
Again, Descartes offers an example. Consider a boat which is carried along by the river's current but which is fighting a wind just strong enough to prevent it from making any headway downstream. In such a situation, the surface which surrounds the boat changes constantly because the bodies which surround it immediately are in movement with respect to it. Nevertheless, as the interior place or space which the boat occupies remains the same and is of a constant size and figure, the surface in general which surrounds it remains the same. And so, while the surrounding body (the river) "passe ailleurs avec sa superficie," we don't say that the body (the boat) which was surrounded by it changes place as well since it remains in the same situation with respect to the other bodies which we regard as unmoving. The boat "ne change point de situation à l'égard des rivages, (et) demeure en meme lieu."

Given this example, and in light of the previous man-in-the-boat-cabin example, used to illustrate the vulgar sense of place, what Descartes appears to be suggesting is this. The exterior place in which a given body is said to be is to be determined by the particular superficie which is between the surrounded body and its immediately surrounding bodies. This is acceptable only so long as the surrounding bodies are at rest with respect to the body surrounded, e.g. when the boat is simply being carried along downstream. However, when this superficie is constantly changing (as Descartes claims it does when "nous voyons que toute la superficie qui l' (le bateau) environne change incessamment") no one particular surface can be determined for the body surrounded since its immediately surrounding bodies are
constantly changing, e.g., the boat is fighting the wind and is unable
to make any progress downstream. In this case the exterior place of a
body is to be determined by "la superficie en general, qui n'est point
partie d'un corps plutôt que d'un autre" and which is between it and
the most immediately surrounding bodies with respect to which it is at
rest.

This is to place restrictions on how the situation or place of a
body is to be determined which are lacking in the vulgar sense of
exterior place. Our reference frames are not indifferent. When the
immediately surrounding bodies are at rest with respect to the
surrounded body the exterior place of that body is to be determined by
the particular surface which surrounds it. Properly speaking, we can
use only the ship's cabin as our frame of reference, not the nearest
shoreline or the farthest fixed star. However, when the surrounding
bodies are constantly changing, the exterior place of the surrounded
body can only be determined by adopting as our frame of reference the
most immediately surrounding bodies which we consider to be at rest and
with respect to which we can say the surrounded body is at rest.

It is this second sense of place—presumably designed to rule
out the (vulgar) sense in which we can say of a body both that it
changes place and that it does not change place—which underlies
Descartes' discussion of a second (true and philosophical) sense of
motion in Art. 15. Motion, properly speaking, is the "transport...
d'un corps du voisinage de ceux qui le touchent immédiatement, et que
nous considérerons comme en repos, dans le voisinage de quelques autres."
This true sense of motion is intended to permit the assignment of only one proper motion to a particular object by stressing that motion is always in the moving body and not in the body which causes it to move.

V. The distinction between two senses of motion which Descartes draws, and which is grounded upon his earlier distinction between two senses of place, is not, however, consistently respected in the subsequent Articles of Part II. He moves on to suggest (in Art. 31) that a particular body can have several diverse motions, and indeed, that it can participate in an infinity of other motions insofar as it is a part of other bodies which touch it and which move. Moreover, he contends that even a body's unique motion can be considered as a composite of several diverse motions.

A consideration of Part III reveals that this conflation has served a purpose. The cosmological scheme which he sets out there must allow him— if he is to stay in the good graces of Rome — to claim that the Earth is at rest, as well as to claim — if he is to maintain his own theory of vortices — that it is carried from place to place by the motion of the vortex of which it is a part.

The picture which emerges from Articles 24-29 is roughly this. The heavens are composed of liquid material which carry with them in a circular motion all the bodies which they contain — of which the Earth is one. Thus, while the heavens "transportent" all the bodies contained in them, the Earth and the planets are at rest just as a boat
drifting in the current is said to remain at rest in the middle of the sea. Properly speaking (sense 2) we cannot say that the Earth and the planets are moved (se meuent in the sense of être transporté) even though they are moved from place to place (se meuent in the sense (1) of être emporter). Similarly, properly speaking, we wouldn't say that the boat adrift is at rest "quoi que peut être le flux ou reflux de cette grande masse d'eau l'emporte insensiblement avec soi."

There is a succession of 'fudges' if not outright contradictions in these passages. In Art. 26 & 27 he claims that neither the Earth nor the planets allow themselves to be "transportées;" then in 28 he claims that although they are we cannot properly say they are moved. This flies in the face of his heretofore consistent practice of associating the true sense of motion with the verb "transporter." In the next Article he will assert that if we adopt the vulgar sense of motion we can speak of the planets, but only "façon improprement" of the Earth as moving. He notes that philosophers must understand place in its true sense and "le déterminer par les corps qui touchent immédiatement celui qu'on dit être mou" and not by those which are extremely distant, as are the fixed stars, from the Earth. This remarkable passage concludes with an admission that, at least as regards the Earth, he might not stick by his philosophical commitment to the true sense of motion, and this "pour nous accommoder à l'usage!"

In Newton's words, which are an understatement:

The philosopher is hardly consistent who uses as the basis of his Philosophy the motion of the vulgar which he had rejected a little before, and now rejects that motion as fit for nothing which, alone was formerly said to be true and philosophical, according to the nature of things.
Our discussion to this point offers no more than a rough-hewn sketch of the much more extensive and perceptive analysis of the highpoints (or lowpoints) of Cartesian theory which Newton provides in his remarkable essay. Much of the critique of these, which is also mounted there, convincingly anticipates both in spirit and content the more theoretically mature -- and devastating -- one of the scholium. It furnishes a striking portrait of the Empiricist bar none in the midst of a conceptual and philosophical 'prise' with the reigning (philoscientific) paradigm of his time. While the Descartes of the Principes has not yet gained the critical attention and full notoriety he deserves for his "undeniably confused conceptual scheme," Newton's early effort to untangle the ambiguities and unsort the inconsistencies of his frustrating, influential work is impressive.

Later, the high empirical yield of the Principia would demonstrate its progressiveness vis-à-vis the world view of the Principes. In "De Gravitatione..." Newton stages a critical first attack on the soon to be deposed paradigm by detailing in a convincing manner the sources for conceptual dissatisfaction with Cartesian theory. The most important conceptual 'result' to which Newton is led in that essay is that a plausible contender to Cartesian theory would need to stand firmly and effectively committed to absolute space in order to resolve the problem of vortical motion -- of the phenomenon which Descartes describes and which Newton dotes upon as the tendency of the Earth and the planets "to recede from the Sun as from a centre about which they are revolved (a tendency which would later be examined in bucket form), by which they are balanced at their due distances from..."
the Sun by a similar tendency of the gyrating vortex (a tendency which
would feature among the Definitions to the Principia). This result
was itself progressive not only for its promising resolution of the
empirical problem with which Newton was in the process of grappling --
and which would remain intractable for any theory which individuated
motions in the Cartesian manner -- but also because it avoided the
internal conceptual problems of that theory.

Newton confronted a theory which appeared to endorse an absolute
view of space yet which offered an analysis of absolute, true and
philosophical motion that was at best quasi-relativistic and compatible
not with an absolute but with a relational theory of space. This
fundamental conceptual inconsistency in Cartesian theory wherein the
two sub-theories of space and of motion conflict, commensurable though
they be, was avoided by Newtonian theory wherein the two subtheories
complement and indeed, with the masterful sleight-of-hand of classical
empiricism provide support for -- as illustrations of -- one another.
Openly added in "De Gravitatione..." by the vying, conceptual
commitments and "absurd (principally empirical) consequences" of the
Principes, Newton makes a significant move towards fashioning the
empirical problem whose solution in the scholium was exemplary of the
theoretical coup he then had the machinery to carry out. The bucket
experiment was a final formulation of the empirical problem which
Newton had begun to detect and to set up in his early criticism of
Cartesian theory.
VI. Descartes' pronouncements on space, place and motion served as the initial catalysts for Newton's own developing account of the system of the world. Given this influence, they also function as the historical backdrop against which an assessment of the Newtonian response to the 'why' and the 'what's of absolute space is to be made. What became of absolute space and of its role in physical theory as it passed from Cartesian to Newtonian hands? Consider 'Descartes' plenum-view of absolute space and his quasi-relativistic theory of motion. Substance and bodies are not in space (as they are claimed to be in the 'container' space often attributed to Newton); they are identified with space and the only vacuum in nature is that in Pascal's head. Absolute space is here understood as extended substance, a generic notion which can be clearly and distinctly conceived apart from the extension of particular bodies. But this generic conception of extended substance does not in any way provide a substantival mark against which the true and philosophical absolute motion of particular extended bodies - much less of the numerous corporeal particles of the vortices - is to be gauged. It does not furnish an 'anchor' for an account of motion, a failure Newton felt dearly in his insistence on mooring his theory of motion on "some motionless thing." To grant it such a theoretical role would have jeopardized Descartes' bid for theological orthodoxy - for the Earth would then not be vulgarly carried along (emporter) by the gyrating vortex but would be actually and truly, moved (transporter). It might also threaten his plenism by inviting the vacuums so abhorred by nature.
The quasi-relativistic theory of motion which Descartes fashioned would not only fail to grant this absolute generic space, or extended substance, any theoretical function, it would be inconsistent with it—a theory in which, as Descartes claims; there was no point "qui soit veritablement immobile" and in which, as Newton showed, "motion is not motion, for it has no velocity, no definition, and there is no space or distance traversed by it." The disenfranchisement of space or generic extension is accomplished by a relativistically conceived definition of place that brings Descartes' plenistic commitments to the fore. Place is not a "part of space which something fills evenly" (bodies do not occupy space they comprise it), nor does this definition even tacitly refer to generic extension or interior place. Place is "nothing but the surface of the surrounding bodies or position among some other more distant bodies."

The conceptual problems discrediting Cartesian theory compound and are responsible for that theory's inability to deal adequately with the objects in its domain and to resolve the first-order empirical questions it raises, notably about the phenomenon of vortical motion. Newton charges repeatedly in "De Gravitatione..." that the Principes tells us nothing (or very little) about what corresponds "to the nature of things" and far too much about what corresponds to or is the "product of our imagination." Specifically, Descartes has claimed that in order to determine the true and philosophical, unique and proper, motion of a body we must make appeal to the neighboring bodies that we consider and which seem to us to be at rest with respect to it, that we "arrestons en notre pensee." Thus, Newton complains,
...he seems to contradict himself when he postulates that to each body corresponds a single motion, according to the nature of things; and yet he asserts that motion to be a product of our imagination, defining it as translation from the neighborhood of bodies which are not at rest but only seem to be at rest even though they may be moving.

And,

I ask for what reason any body is properly said to move when other bodies from whose neighborhood it is transported are not seen to be at rest, or rather when they cannot be seen to be at rest.  

/In the absence of any consistent, empirically progressive relativistic theory, Newton is able to claim that if the story is to be told in accordance not with our ideas or imaginings but with the nature of things (in practice, the nature of phenomena), and if it is to offer an adequate account of such vortical phenomena as the tendency of the Earth and the planets to "receive from the Sun as from a centre, about which they are revolved," it is necessary that the definition of places, and hence of local motion, be referred to some motionless thing such as extension alone or space in so far as it is seen to be truly distinct from bodies.  

Yet this methodologically practical insight is shrouded in mysticism and mistakes if we heed the supporters of the AR. Nevertheless, Newton is not totally without blame. The three-dimensional Euclidean structure of the Principia absolute space was mystified by Newton himself with theological allusions to the 'sensorium of God.' However convenient such religious analogies were to Newton, and however comfortable he found himself with them, this talk only served to underscore the conceptual problems which his theory, despite its enormous empirical success and its considerable advance over the
VII. The initial conceptual discomfort experienced by many of Newton's contemporaries with absolute space—such as Leibniz and Berkeley—differed in degree but not in kind from that experienced later, after a Newtonian research tradition was well-established. Both the early and the subsequent conceptual unease expressed had its source in doubts about (and criticism of) the methodological well-foundedness of a theory which endorsed the 'existence' of absolute space. Leibniz rejected it as violating his own methodological principle of sufficient reason: "I say then, that if space were an absolute being, there would something happen for which it would be impossible there should be a sufficient reason. Which is against my axiom." For Berkeley it was equally a methodological faux pas, being an impossible "most abstract idea" which ran counter to the interests of his phenomenalism; "if we inquire narrowly, we shall find we cannot even frame an idea of pure space exclusive of all body."

Subsequent methodological objections would reflect the interests of the increasingly enforced line that the methods of science can and should be inductive and experimental. Indeed, this was the methodological party line as opposed to the methodological practice which Newton himself helped to pen. The empirically undetectable, theoretically required absolute space to which his practice committed him was at odds with the methodological norms he officially prescribed as governing scientific behaviour. Although the first-order empirical
questions were so handsomely and decisively answered by Newtonian
theory (and paradigmatically by the scholium's demonstration), their
resolution provoked second-order problems. Newton had made certain
claims about entities in the domain of his theory -- i.e., that they
underwent absolute motion -- which raised questions about the
methodological well-foundedness of the theory. His contemporaries were
unable to block the inference to absolute space. Those who followed --
such as Mach and Reichenbach -- would attempt to discredit his
reasoning by challenging the first premise of the argument; that is, by
rejecting the claim that the water in the bucket was suffering the
effects of absolute motion. They failed where Einstein succeeded since
no theoretical alternative was advanced, and no empirical progressiveness
secured. What they succeeded in doing was in attacking the
methodological soundness of Newtonian theory by exposing the phenomena
for what they were -- not what is seen but what is theoretically
filtered and 'selected for'.

This was to openly challenge the reasoning and the methodological
soundness of classical empiricism. Where Newton offered a theoretical
description of the phenomena of vortical motion, Mach attempted to
offer a 'competing' explanatory hypothesis according to which the
water's ascent was due to translation with respect to the fixed stars
-- or to the "coordinate system fixed at rest in the 'smoothed out'
total mass of the universe."34 But this alleged 'alternative
hypothesis' was competing not with another hypothesis, but Newtonian
theory and methodology. In fact, though under attack, Newtonian
absolutism would remain until the empirical progressiveness of
Einsteinian relativity displaced it. If and when general relativity is able to demonstrate a similar progressiveness, and absolute space — however non-Newtonian a version — appears to be necessary to do physics, the debate over the nature and 'respectability' of absolute space will acquire a new urgency.
Footnotes

1. Whitt (1982). See the Appendix at the end of this essay.


5. Laudan (1977), 33.

6. Ibid.


11. Ibid., 127.

12. Ibid.

13. Ibid., 128.


15. Ibid., 49.

16. Descartes (1644). References are to the French edition. They are numerous below, and will not be footnoted, though the relevant articles are noted in the text.


18. Descartes' treatment of the notion of grandeur (i.e. size or extent) and of the notion of number interestingly parallels what has been said earlier concerning the generic notion of extended substance. In Article 10, it serves that we can conceive of a "grandeur continuë", of ten, etc., without thinking of a particular substance. Similarly, we can conceive of the number ten without thinking of a particular thing because the idea we have of the number ten remains the same whether we are considering ten feet or any other group of ten.
20. Ibid., 128.
21. Ibid., 124.
22. Specifically, we have attempted to trace Newton's citations of Descartes back to their origin in the Principes.
25. Ibid., 131.
26. Ibid.
27. Ibid., 125.
28. Ibid.
29. Ibid., 128-129.
30. Ibid., 131.
32. Berkeley, as quoted in ibid., 307.
33. Ibid.
34. Sklar (1972) 306.
CHAPTER FOUR

ATOMS OR AFFINITY? THE PURSUIT OF NINETEENTH CENTURY CHEMICAL THEORY

Introduction

Early in the first decade of the nineteenth century a middle-aged meteorologist of humble provincial stock was invited to London by the Royal Institution for the purpose of lecturing a largely middle-class audience about the impact of science upon everyday life. One of the more notable gaps in his unsystematic education lay in the area of chemical training. Yet within five years, having achieved some modest distinction for his work on gases, John Dalton would present the scientific community with a New System of Chemical Philosophy. The atomic theory he expounded there would violate the definition of 'element' advanced twenty years earlier by no less a chemical eminence than Lavoisier. It would ignore the century-long preoccupation of chemical orthodoxy with the notion of 'affinity' as well as that tradition's methodological proscriptions. And it would endorse a theory of matter blatantly at odds with the prevailing Newtonian physics of the time.

Dalton's work, nevertheless, generated a debate that lingered — provoking the attention if not the research of virtually every chemist — for the better part of one hundred years. At the end of this
period, in his valuable account of 19th century European scientific
thought, the historian J.T. Merz recorded that

chemical research was governed all through the century by the atomic view of matter ... hand in hand with this ... went the absolute neglect with which questions referring to chemical affinity were treated ... of the dynamical changes that take place in time and imply the knowledge of forces or movements, few took any notice whatsoever.

Yet, in 1869 at the height of the debate, the new President of the
Chemical Society of London would draw attention to the skeptical
resistance to atomism which characterized virtually every decade of the
century:

I think I am not overstating the case when I say that, on the one hand, all chemists use the atomic theory, and that, on the other hand, a considerable number of them view it with mistrust, some with positive dislike.

In the following, we will probe this lingering and pronounced
ambivalence of chemists toward Daltonian atomism, exploring in
particular the host of conceptual problems with which -- it will be
argued -- the theory was fatally flawed. Other such probes have, of
course, been conducted. However, their neglect of affinitivist issues
has been, if not absolute, marked. Merz's observation, in any event,
seems more soundly applied to them than to actual developments in 19th
century chemistry. (Indeed, Merz's own 4 volume work is one of the
richest sources documenting affinitist activism throughout the
century.) It is a central contention of the present study that it is
only by appraising the affinitivist affiliations of the chemical
community at the turn of the century that we can begin to appreciate
the nature of the reception accorded to Dalton's startling theory.
Moreover, the continuation of certain affinitist lines of chemical
research throughout the remainder of the century provides an important key to understanding various developments in that community.

I. Throughout the 18th century the dominant chemical research tradition was based on the notion of 'affinities', and more narrowly on that of 'elective affinities' -- the differential tendencies of certain chemical substances to combine with others. The empirical problems recognized as legitimate within this tradition and upon whose solution a premium was set were concerned with the observable reactions of chemical reagents, with correlating and predicting how different chemical substances combine. The well-known instances of chemical affinity had been brought together for the first time in one place, and in a general fashion, by Newton in Query 31 of his 1706 Optics. Describing in attractionist terms a simple experiment in which iron displaces copper from acid solution, copper displaces silver, and so on for a number of metals, Newton asked

Does not this argue that the acid particles of the Aqua fortis are attracted more strongly ... by Iron than Copper, and more strongly by Copper than by Silver, and more strongly by Iron, Copper, Tin, and Lead, than by Mercury?

Experimental attempts to quantify chemistry along these lines began in 1718 with Geoffroy's construction of a table based on displacement reactions. In subsequent efforts to quantify and to establish a constant order of affinities a variety of methods were employed: the adhesion techniques of Guyton de Morveau, the rate of solution investigations of Wenzel, and the neutralization studies of Richter and Kirwan. Midway through the century, in his influential Éléments de
chymie théorique. Macquer expressed the conviction of the majority of his contemporaries that the study of affinity would "nous servira à rendre tous les phénomènes que fournit la Chymie, & à les lier ensemble." Similar assessments of the promise of affinity studies and of the consequences for chemistry of establishing a constant order of affinities were made early in the last quarter of the century by the Swedish chemist Tobern Bergman:

Will (this order) not, when once ascertained by experience, serve as a key to unlock the innermost sanctuaries of nature, and to solve the most difficult problems, whether analytical or synthetical? I maintain, therefore, not only that the doctrine desires to be cultivated, but that the whole of chemistry rests upon it, as upon a solid foundation; at least if we wish to have the science in a rational form, and that each circumstance of its operations should be clearly and justly explained.

Even Lavoisier, whose own work in the area was minimal, acknowledged that the science of affinities "is perhaps the best calculated part of chemistry for being reduced into a completely systematic body." Newtonian natural philosophy -- especially the form it took in the Optics and the "Queries" to the Optics -- profoundly influenced 18th century thought about the chemical elements, providing the affinitist tradition with its heuristic, a force ontology and empiricist methodology. Newton's speculations in the last Query were adopted as a veritable agenda from which chemists would not deviate en masse until Dallons New System began to take ambivalent hold. Nature, Newton had said there, was very simple and conformable to herself:

performing all the great Motions of the heavenly Bodies by the Attraction of Gravity which intercedes those Bodies, and almost all the small ones of their Particles by some other attractive and repelling Powers which intercede the Particles.... It seems to me farther, that these Particles... are moved by certain active Principles, such as is that
of Gravity, and that which causes Fermentation, and the Cohesion of Bodies. These Principles I consider not as occult Qualities, supposed to result from the specifick Forms of Things, but as General Laws of Nature, by which the Things themselves are formed: their Truth appearing to us by Phænomena, though their causes be not yet discover'd.

This analogy between celestial and chemical operations, between gravitational attraction and affinity served as a heuristic for the tradition. Dynamical interpretations of affinity were sought and extended by a succession of both chemists and natural philosophers, including Kant, Boscovich, Priestly, Buffon, Macquer and Bergman. In the 1778 edition of his *Dictionnaire de Chymie*, under the article "Affinité", Macquer made clear the prevailing conviction that there were no special, separate laws for chemical affinity. To the contrary

> les phénomènes chimiques, sont ... les effets d'une seule et même loi des plus grandes et des plus générales qu'on ait observées jusqu'à présent dans la nature; je veux dire de celle selon laquelle toutes les parties de la matière tendent les unes vers les autres.

Affinitist chemistry, then, was intimately tied to a dynamical natural philosophy which was inspired by Newton, governed by short-range forces between atoms, and whose ontology gave a decided priority to forces or powers rather than to matter. (Indeed, in some cases -- as with Boscovichian point atomism and Kantian force atomism -- the attempt was made to explain matter wholly in terms of force. This type of dynamical theory had a distinct chemical advantage over an unmodified Newtonian dynamics, for unlike the latter it could account for the phenomena of elective affinity and thereby of the specificity of chemical reactions.11) As Joseph Priestly observed:

> The principles of Newtonian philosophy were no sooner known, than it was seen how few, in comparison, of the
phenomena of nature, were owing to solid matter, and how much to powers.... It has been asserted... that... all the solid matter in the solar system might be contained within a nut-shell.

This nut-shell theory of matter carried to an extreme the Newtonian belief in the internally ordered structure and highly porous nature of all known chemicals (itself a consequence of the "inertial homogeneity" of matter\(^1\)). It was owing partly to this theory and partly to the inductivist and empiricist nature of their methodological commitments that chemists within the affinitist tradition began to suspect there was a deep gulf between chemical elements and the "primogenial particles of matter," i.e. the ultimate physical atoms whose non-redeeming features were their extraordinary minuteness and observational inaccessibility. As early as 1734 they expressed their readiness to leave to other philosophers the sublimer disquisitions of primary corpuscles or atoms.... the more intelligent among the modern chemists do not understand by principles those original particles of matter, of which all bodies are by the mathematical and mechanical philosophers supposed to consist.... Those particles remain indiscernible to the sense..., nor have their figures and original differences been determined by a just induction... genuine chemistry contents itself with grosser principles, which are evident to the sense, and known to produce effects.

In the second half of the century William Cullen and Joseph Black would lecture in a similar vein at the enormously influential universities of Edinburgh and Glasgow:

Elements are physical or chemical, the former are the real elements of bodies or as they are often called atoms, but these physical elements are rather imagined than actually known... the strict, precise meaning of element is, that which no human art can divide; these we call chemical elements but physical elements are those beyond which no power in our system can go...
And in France, the same distinction between medical elements and physical atoms would be stressed by Macquer and Gayton de Morveau who au lieu de spéculer ... sur la structure intime de la matière ... essayeront de rechercher quels sont les corps capables de se combiner ensemble pour donner un nouveau compose; quels son ceux qui déplacent les premiers pour prendre leur place. ... 16

All of this, finally, would culminate in Lavoisier's celebrated declaration of 1790 that if, by the term elements, we mean to express those simple and indivisible atoms of which matter is composed, it is extremely probable that we know nothing at all about them; but if we apply the term elements, or principles of bodies, to express our idea of the last point which analysis is capable of reaching, we must admit, as elements, all the substances into which we are capable, by any means, to reduce bodies by decomposition. 17

II. We have in hand now a rough sketch of the distinguishing features and commitments of 18th century affinitist chemistry. Such a sketch seems obviously essential to any assessment of the chemical community's reception of Dalton's New System. But it can also serve to illuminate and to put into perspective several of the more puzzling features of 19th century chemistry. According to many historians, there was an abrupt and protracted 'split' between chemistry and physics at the start of the century which was not mended until its close. 18 If a reason for the rift is advanced at all it is usually that during this time physicists had nothing useful to offer chemists whose research was inspired by Daltonian atomism. For example:

if chemists had had to wait until physicists had produced an atomic theory competent and detailed enough to explain the facts of chemistry, then the science could not have begun to make progress until at least a century after the time of Lavoisier. 19
This, however, seems to amount to little more than a restatement of the problem. We will suggest below that such observations are themselves partly the result of the neglect of the existence of a viable affinitivist alternative to Daltonian atomism, an alternative which actively drew upon developments in physics. This oversight is particularly acute among 20th-century historians who, unlike their 19th-century counterparts, have rarely raised the question of what became of the once so dominant affinitivist tradition. Was it indeed thrown into the lasting, total eclipse that their silence would seem to suggest? And if not (as we will urge), to what type of chemical research did the tradition contribute? Finally, the exploration of a positive 19th-century alternative to Daltonian atomism may help to clarify the identity and commitments of the notorious 'anti-atomists', who crop up so persistently but illusively in these historical studies, usually as a rather motley crew of sceptical detractors whose stubborn reluctance to embrace Daltonian atomism was based on quite diverse, often shifting, grounds. We will propose that many of these chemists had more in common than their skepticism and that, given the conceptual problems posed by Daltonian atomism, their critical grounds were somewhat sounder than has normally been supposed.

We might begin to address these issues by focusing on two of the most significant developments which took place in chemistry during the first few decades of the 19th century. First, the literal 'galvanization' of affinitist studies following Berthollet's devastating review of previous work in the tradition. And second -- in
light of this — the impact, promise and problematics of early
Daltonian atomism.

With the publication of Bergman’s affinity tables in 1778, the
reliability, chemical utility and orderliness of the scheme of elective
affinities was greatly advanced. Not only had he provided careful
experimental verification of the reactions on which previous tables had
been based, he had succeeded in extending those tables to include most
known substances and managed to reconcile the rules of elective
affinity with many reactions that had, theretofore, appeared
inconsistent with them.20 The few remaining inconsistencies he
attributed to inadequate data, a situation which could be remedied by
further experimentation. Prominent chemists of the last quarter
century such as Kirwan, Gayton de Morveau, Antoine Fourcroy and C.L.
Bertholet devoted themselves to supplying this data and to completing
the tables while they continued the search for a means of
quantitatively measuring affinities.

What they discovered however were further and apparently
irreconcilable inconsistencies. In the first decade of the 19th
century Berthollet brought out a series of works in which he critically
reviewed the basis and results of established affinity theory. By
showing that "the chemical action of bodies ... does not depend upon
their affinity exclusively, but also on their quantity"21 he hoped to
reorient affinity theory and to supplant Bergman’s determinations of
affinities with a better method. Indeed, the 1803 Eassai opens with a
declaration of allegiance to the heuristic which had guided the 18th century affinitists:

The forces which produce chemical phenomena are all derived from the mutual attraction of the molecules of bodies, to which the name affinity has been given, to distinguish it from the attraction of astronomy. It is probable that both are only the same property.  

But this Newtonian heuristic, together with the experimental programme if had inspired, would be effectively laid to rest by Berthollet's convincing demonstration that elective affinity could no longer be regarded as an invariable, uniform force. In order to arrive at the true measure of the relative affinities of two substances for a third "it would be necessary to discover in what proportion this third ... would divide its action," and there were "insurmountable obstacles that would be met with in the means that must necessarily be employed to prove this division of action." After Berthollet, chemists would no longer regard the forces of chemical affinity as analogous to, or as identical with, those of gravitation. As Whewell observed:

...there would be no objection to such an identification, if we could, in that way, explain or even classify well a collection of chemical facts; but ..., this has never yet been done by the help of such expressions. Till some advances of this kind can be pointed out we must necessarily consider the power which produces chemical combination as a peculiar principle, a special relation of the elements, not rightly expressed in mechanical terms.

At the same time that Berthollet was inadvertently undermining the guiding heuristic of previous affinitist studies, other chemists had begun investigations which would supply the tradition with a promising alternative heuristic for dynamical interpretations of chemical phenomena. The most notable figure among them was Sir
Humphrey Davy who was easily the most eminent, influential and flamboyant member of the English chemical community at the time when Dalton's atomic theory was just starting to attract attention. As a consistent and vocal opponent of that theory he was also one of the first in a long line of chemists to draw attention to the problems which militated against its acceptance (though not, as we shall see, against its pursuit).

From the beginning of his scientific career Davy was convinced that the concept of affinity was central to chemical science. He quickly seized on Volta's recently discovered galvanic pile as an important research tool for the affinitist tradition, appreciating its value as an instrument for overcoming the affinities of bodies and for helping chemists both to reveal the fundamental simplicity of their constitution and to determine the true elements. He undertook a series of galvanic experiments which eventually led him to conclude that the forces of chemical affinity and of electricity were intimately related. The Voltaic battery was, he said:

"an alarm bell to the slumbering energies of experimenters in every part of Europe, and it served no less for demonstrating new properties of Electricity and for establishing the laws of this Science, than as an instrument of discovery in other branches of knowledge; exhibiting relations between subjects before apparently without connection and serving as a bond of unity between chemical and physical philosophy."

This belief that chemistry and physics were ultimately united along dynamical lines would be shared by subsequent affinitists such as Faraday and Whewell. It was also characteristic of the tradition, as was Davy's commitment to the simplicity and homogeneity of matter. In
1806 he presented his paper "On some chemical Agencies of Electricity" to the Royal Society in which he advanced his electrical theory of affinity, a theory which drew together an astonishingly diverse array of previously unrelated phenomena — static and galvanic electricity, electrolysis, the forces of chemical affinity and chemical combinations, the heat and light of chemical reaction, affinity tables, and the electrochemical series. The paper was immediately and widely acclaimed as one of the most significant contributions to chemical theory of modern times.

Three years later he extended his electro-chemical theory to unite, in one comprehensive system, the two aspects of quantifiable chemistry: affinity and the law of equivalents, or Dalton's law of multiple combining proportions. While convinced of the truth of Dalton's laws of definite and multiple proportions, he would insist that their basis lay not in any "speculations upon the ultimate particles of matter" but more soundly in the mutual decompositions of neutral salts and "in the decompositions by the Voltaic apparatus, where oxygen and hydrogen, oxygen and inflammable bodies, acids and alkalies, etc., must separate in uniform ratios."26. Dalton's physical atomism was dismissed as hypothetical and as "contradicted by refined observation and opposed by the results of minute and accurate experiments."27

We must turn now to John Dalton and the New System which he was formulating during the first decade of the 19th century. One of the central features of the system that began as an "enquiry into the
relative weights of the ultimate particles of bodies ... a system, as far as I know, entirely new," as the one-to-one equation of the chemical elements with ultimate physical atoms -- the latter being understood as solid, indivisible particles. These chemical atoms were responsible for the initial problem-solving success of Dalton's theory: with the assistance of the balance and improvement of analytical techniques they would serve as readily quantifiable chemical units, inspiring a lengthy programme of weight determinations. And their appearance was, in this sense, timely, coming as it did on the heels of Berthollet's devastating critique of the previous century's efforts to quantify chemistry along affinitist lines. But they were a decidedly mixed blessing for the theory. The severity and duration of the conceptual problems they provoked were considerable.

Direct mention of "indivisible particles or atoms" occurs in an 1807 letter to Thomas Thomson where Dalton indicates it has been suggested to him that an explication of my latest experimental enquiries on the subjects of heat, elastic fluids, and chemical elements or atoms with their various combinations would not be unacceptable at Edinburgh.

In Lectures III, IV and V of that Edinburgh "Syllabus of a Course of Lectures on Heat, and Chemical Elements," under the heading of Chemical Elements, we find Dalton first publicly speaking of Elastic fluids conceived to consist of indivisible particles or atoms of matter, surrounded with atmospheres of Heat --

and of

Liquid and solid bodies conceived likewise to be composed of atoms surrounded with Heat ...
The following year, in Part I of his New System, Dalton underscores the solid, indivisible nature of his chemical atoms:

we might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen. All the changes we can produce, consist in separating particles that are in a state of cohesion or combination and joining those that were previously at a distance.

However, some of the confusion and ambiguity that would plague Dalton's theory is here ushered in for Dalton will describe both these separated or simple particles that enter into chemical combination and the joined particles that result from chemical combination as 'atoms.' 'Atom,' in other words, is used to refer to the smallest particle of both elements and compounds. Dalton does, at any rate, make it clear that

all atoms of the same kind, whether simple or compound, must necessarily be conceived to be alike in shape, weight, and every other particular.

And he did not hesitate to stress the fact that although it has been imagined by some philosophers that all matter, however unlike, is probably the same thing, and that the great variety of its appearances arises from certain powers communicated to it, and from the variety of combinations and arrangements of which it is susceptible,

it followed from his theory that there are a considerable number of what may be properly called elementary principles, which never can be metamorphosed, one into another, by any power we can control.

More exactly, his theory implied that there were more than 20 different kinds of matter, the chemical elements. This number would increase with each addition to the list of chemical elements.

Thus, not only did Dalton's theory violate the commonly accepted definition of the elements (perhaps best formulated by Lavoisier) which
stressed the importance of distinguishing them from indivisible physical atoms and of recognizing that it was the former, not the latter, with which chemistry was concerned. It also, by denying the homogeneity and internal structure of matter, violated the prevailing physics of the time. This would generate a fairly serious and lasting conceptual problem for the theory, since the competing affinitist theories did not run afoul of contemporary physics in these respects. It also helps to explain the comparative autonomy of atomistic chemistry from physics throughout much of the 19th century. When Dalton's theory was radically revised later in the century, partly as a result of developments in organic and stereochemistry, this conceptual problem would be resolved and chemists would be able to draw support from physical theories which assigned a complex structure to the atom and which implied the underlying unity of matter.

III. As we have been at some pains to point out, Dalton's theory made its appearance in a chemical community that was and had long been preoccupied with essentially dynamical concerns: chemical theories addressed themselves first and foremost to the forces of chemical affinity. The ontological commitments of Dalton's atomic theory, however, clearly lay elsewhere.

One of the more striking features of the theory is its total neglect of the forces of chemical affinity. From his earliest work on mixed gases Dalton had denied the existence of any sort of chemical affinity forces. There is scant mention in the New System of such
forces apart from his brief comment with reference to Berthollet's work that its study "is daily growing more obscure in proportion to the new lights attempted to be thrown upon it."\textsuperscript{33} As one commentator has observed:

"Beyond this notion that atoms were surrounded by envelopes of caloric, and that when they combined, the resulting molecules possessed a single envelope of caloric, and granted the reservation that chemical combination was governed by largely geometrical considerations, Dalton was little interested in the mechanism of chemical combination.\textsuperscript{34}

What little Dalton did have to say about how chemical substances combine and why they combine in the proportions that they do constituted, indeed, one of the most objectionable features of the theory. This account -- discussed in Chapter III of the New System, "On Chemical Synthesis," and visually captured by Plates 4 and 6 -- makes apparent Dalton's clear commitment to the actual existence of solid, indivisible, material atoms combining in accordance with simple mechanistic and geometric considerations.\textsuperscript{35} He observes that:

If there are two bodies, \textit{A} and \textit{B}, which are disposed to combine, the following is the order in which the combinations may take place, beginning with the most simple: namely,

1 atom of \textit{A} 1 atom of \textit{B} 1 atom of \textit{C}, binary.
1 atom of \textit{A} 2 atoms of \textit{B} 1 atom of \textit{D}, ternary.
2 atoms of \textit{A} 1 atom of \textit{B} 1 atom of \textit{E}, ternary.
1 atom of \textit{A} 3 atoms of \textit{B} 1 atom of \textit{F}, quaternary.
3 atoms of \textit{A} 1 atom of \textit{B} 1 atom of \textit{G}, quaternary.
&c., &c., &c.

And he proposed the following general 'rule of simplicity' for the purpose of calculating atomic weights on the basis of the combining weights obtained by experiment:

The following general rules may be adopted as guides in all our investigations respecting chemical synthesis.
1st. When only one combination of two bodies can be obtained, it must be presumed to be a binary one, unless some cause appear to the contrary.

2nd. When two combinations are observed, they must be preserved to be a binary and a ternary.

etc. In the plates, which "exhibit the mode of combination in some of the more simple cases":

The elements or atoms of such bodies as are conceived at present to be simple, are denoted by a small circle with some distinctive mark; and the combinations consist in the juxtaposition of two or more of these; when three or more particles of elastic fluids are combined together in one, it is supposed that the particles of the same kind repel each other, and therefore take "their stations accordingly."

Elaborating on this view of chemical combination elsewhere, Dalton explained that although there is "no mechanical reason" why one atom of A should not combine with "as many atoms of B as are presented to it, and can possibly come into contact with it," what limits the process is "the repulsion of the atoms of B among themselves."

Now this repulsion begins with 2 atoms of B to one of A, in which case the 2 atoms of B are diametrically opposed; it increases with 3 atoms of B to 1 of A, in which case the atoms of B are only 120° asunder, with 4 atoms of B it is still greater as the distance is then only 90° and so on in proportion to the number of atoms.

From this mechanical view of combining ratios he readily derived "the relative weights of the ultimate particles, both of simple and compound bodies ... which it is one great object of this work to shew the importance and advantage of ascertaining."

Dalton's chemical contemporaries were quick to appreciate and to accept what they regarded as the empirical portion of his theory -- the laws and ratios according to which the elements combined. Once these had been placed on an even sounder empirical basis following the
accurate researches of Wollaston and Thomson on the oxalates in 1808; they were widely recognized as an important move toward the quantification of chemistry. When Davy, as President of the Royal Society, presented Dalton with a Royal Medal in 1826 he underscored the significance and value of this contribution:

With respect to the weight or quantity in which the different elementary substances entered into union to form compounds, there was scarcely any distinct or accurate data. Persons whose names had high authority differed considerably in their statements of results; and statistical chemistry, as it was taught in 1799, was obscure, vague and indefinite, not meriting the name of a science. To Mr. Dalton belongs the distinction of first unequivocally calling the attention of philosophers to this important subject ... thus making the statistics of chemistry depend upon simple questions in subtraction or multiplication, and enabling the student to deduce an immense number of facts from a few well-authenticated, accurate, experimental results.

But the great majority of chemists were just as quick to reject what they would increasingly refer to as the hypothetical part of the theory -- Dalton's attempt to provide a causal explanation of these laws in terms of solid, indivisible material atoms. Where Dalton would cast himself as the Newton of chemistry, noting that definite proportions were "like the mystical ratios of Kepler, which Newton so happily elucidated," these chemists could cast him as no more than the Kepler of chemistry. And like Kepler, who had attempted to explain the motions of the planets in terms of magnetic forces, they believed Dalton had erred in advancing an hypothesis to explain his laws. Moreover, not only did this explanation suffer from conceptual ambiguity (as a result of Dalton's use of 'atom' to refer to both simple and compound particles) it violated the prevailing matter theory
in physics and the accepted definition of 'element' in chemistry. It also failed to address the forces of chemical affinity which had dominated the domains of dynamical chemical theories for a full century. Worse still, even in the estimation of the more sympathetically-inclined chemists like Wollaston and Berzelius, the mechanistic atomism which it tendered instead was inadequate and unsatisfying as an account of the process of chemical combination. Since both of these influential, widely-respected chemists initially regarded the atomic theory as worthy of pursuit and contributed significantly to its empirical problem-solving abilities -- but ultimately found themselves unable to accept it -- their work is of considerable interest to us here.

In the same paper in which he published his important results on the oxalates (alluded to above), W.H. Wollaston stated that:

when our views are sufficiently extended to enable us to reason with precision concerning the properties of elementary atoms, we shall find the arithmetical relation alone will not be sufficient to explain their mutual action, and ... be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension.

He undertook to develop such an atomic geometry in 1813, a programme which was rapidly abandoned a year later along with any further serious pursuit of the atomic theory on his part. Indeed, he soon formulated a "synoptic scale of chemical equivalents" which separated the practical part of (Dalton's) doctrine from the atomic or hypothetical part. Noting that "Mr. Dalton conceives that we are estimating the aggregate weights of a given number of atoms ... when we estimate the relative weights of equivalents," Wollaston pointed out that it was hopeless
to seek true atomic weights since experiment could not determine whether a given compound were really 'binary' or 'ternary'. What experiment revealed was the proportions of elements in compounds and the ratios in which elements combined, and these analytical values—the equivalents—could be used by chemists in their research without implying any commitment to Dalton's mechanistic atomism.

Wollaston's 'equivalents,' based directly on experimental evidence, would be favourably received by a great number of chemists in subsequent decades. This was especially true following the distrust and uncertainty surrounding the determination of atomic weights which developed in the 1830's, largely as a result of Dumas' work and the anomalies which were discovered through the use of different methods of weight-determination. In the 1840's, with Faraday's discovery of electro-chemical equivalences and with the open advocacy of 'equivalents' by the prestigious continental chemist Leopold Gmelin, their currency and value as a stoichiometric tool increased. Finally, when the members of the Karlsruhe Conference of 1860 met to discuss the various problems of chemical nomenclature and symbolism which had become acute over the previous years, they accepted after a vote that 'equivalents' were more empirical than (and so to be preferred to) 'atoms' and 'molecules.'

IV. The figure of Jons Jacob Berzelius is particularly relevant at this point. Our concern thus far has been to indicate the nature of the various conceptual problems raised by Daltonian atomism and to
stress the vigorous presence of an alternative affinitist research tradition which could embrace the laws of multiple, definite and combining proportions while rejecting the hypothetical, billiard ball atoms from which they deductively followed. Berzelius' stature in the chemical community was lasting and considerable. His early pursuit of the atomic theory resulted in valuable contributions to its empirical problem-solving abilities. But he found himself ultimately unable to accept it, on both conceptual and empirical grounds. In its stead, he formulated an alternative electrochemical theory which, for all its stress on the role of individual atoms, lay clearly within the opposing affinitist tradition and dominated most chemical research in the second quarter of the century.

As we have already seen, showing the importance of determining atomic weights was regarded by Dalton as one great object of his *New System*. The accurate determination of such weights was the most immediate empirical problem confronted by the theory. While the work of Wollaston and Thomson on the oxalates was an initial contribution towards its resolution, the work of Berzelius was even more significant in this respect. He considerably advanced experimental techniques and set new high standards of accuracy and comprehensiveness with the publication of an extensive set of atomic weights in his 1814 "Essay on the Cause of Chemical Proportions." Nevertheless, in this essay and in publications the following year, Berzelius conducted a detailed discussion of the empirical and conceptual problems Dalton's theory faced. The formulae of certain metallic oxides, for example, indicated the presence of half-atoms of oxygen. Thus the already ambiguous
Daltonian 'atom' was further troubled by what Berzelius had described in an earlier letter to Dalton as "la difficulté de concevoir un demi-atom."

And, his important first effort to extend the range of Daltonian theory by analysing 13 organic compounds had met with only a qualified success for, from the data he obtained, he concluded that although formulae could be given for them in accordance with Dalton's theory, the law of definite proportions did not seem applicable.

Berzelius also objected to Dalton's conception of diverse geometrical configurations for different atoms, regarding it as unnecessary and as a flight of fancy since there was no way of distinguishing between the atom per se and an atom in its spherical heat envelope. Accordingly, he proposed a new system of chemical symbolism, intentionally designed to replace Dalton's and eventually successfully in doing so. The vivid, pictorial symbolism of Dalton's 'billiard-ball' circular signs with their empirically unsupported suggestions of structure were gradually replaced by Berzelius' alphabetical notation in which "the chemical sign expresses always one volume of the substance."

It was partly owing to these difficulties that Berzelius found himself unable to accept Daltonian atomism, contending that Dalton's hypothesis of atoms "can neither be adopted nor considered true." Drawing instead upon Gay-Lussac's valuable empirical generalization in the chemistry of gases -- which showed that gases combine in simple ratios by volumes -- Berzelius elected to express Dalton's laws of chemical proportions in terms of 'volumes' rather than 'atoms':

What in one theory is called an atom is in the other theory a volume. In the present state of our knowledge the theory of volumes has the advantage of being founded upon a well-constituted fact, while the other has only a supposition for its foundation. Berzelius did not, however, regard Dalton’s theory as unworthy of pursuit, noting that "it would be rash to conclude that we shall not be able hereafter to explain these apparent anomalies in a satisfactory manner." And, in his 1815 "An Address to those Chemists Who Wish to Examine the Laws of Chemical Proportions," he pointed out that when I endeavoured to draw the attention of chemists to the difficulties in the atomic theory it was not my intention to refute the hypothesis, I wanted to lay open all the difficulties of that hypothesis that nothing might escape our attention calculated to throw light on the subject.

In the same address, Berzelius cites yet another telling reason for his non-acceptance of Dalton's theory. The account of the 'mechanism' of chemical combination which that theory tendered was inadequate. It wholly neglected what Berzelius regarded as the essential factor to be considered in the search for an explanation of chemical phenomena -- the powers or forces associated with individual particles of matter:

For my own part, in considering a corpuscular theory of chemistry, I conceived that it should constitute the fundamental theory of the science; and instead of being occupied with a part of the phenomena, ought to embrace the whole. But when we treat atoms in a chemical theory, we ought to endeavour to combine researches respecting the cause why atoms combine with researches into the cause why they combine only in certain proportions.

From the very beginning of his career, Berzelius had been swept up in the exciting new researches in 'galvanic' and electro-chemistry, and frequently collaborated with Sir Humphrey Davy. Like Davy, Berzelius believed that the key to chemical understanding lay with the
study of the forces of chemical affinity. And like Davy he believed that the key to understanding the latter lay with the new electrical studies. Both chemists were early proponents of what Faraday would later refer to as

the beautiful idea, that ordinary chemical affinity is a mere consequence of the electrical attractions of the particles of matter.\(^{54}\)

According to Berzelius' dualistic electrochemical theory of affinity, each of these particles of matter had unlike and specific polar charges, which were the seat of affinity. Chemical combination involved neutralization of charge. Berzelius applied his theory to account for the phenomena of elective affinities and to explain all types of chemical reaction: mechanical cohesion, dissolution, electrolysis and so on. The theory would figure prominently in chemical investigations for the next two decades, until the work of Dumas and Faraday.

Michael Faraday -- whose career began under the tutelage and encouragement of Sir Humphrey Davy -- became, in many senses, Davy's successor. Theirs were among the primary contributions in the first half of the 19th century to what we have been speaking broadly of as the affinitist or dynamist tradition. Some of the central commitments of that tradition in the second quarter of the century are best seen in Faraday's work. Like Davy, Faraday was very much a 'natural philosopher', believing that physics and chemistry were unified along dynamical lines through the study of the forces or powers of matter. Chemical affinity and the laws governing its operation were the foundations of chemical science, and the conviction that chemical,
electrical and mechanical forces were ultimately the same served as the
heuristic which guided Faraday's effort to understand these
foundations. In his earliest lectures he stated that:

the science of chemistry is founded upon the cohesion of
matter, and the affinities of bodies; and every case,
either of cohesion or of affinity is also a case of
attraction. It is, therefore, of the utmost importance
that we should become acquainted with attraction in general
before we descend to particular instances. When, also, I
have informed you that the powers which cause the cohesion
of similar matter, and the combination of dissimilar
matter, are actually those which we have been considering
under the names of attraction of aggregation and of
electrical attraction, it will immediately be seen that I
have not entered into a detail of irrelevant and
superfluous matter, but have been employed in giving first
principles for the consideration of the powers immediately
productive of chemical phenomena. That the attraction of
aggregation and chemical affinity is actually the same as
the attraction of gravitation and electrical attraction I
shall not positively affirm, but I believe they are.

In these early lectures, we also find Faraday emphasizing what
would be a life-long commitment to the simplicity and unity of matter,
urging chemists in 1818 "to realize the once absurd notion of
transmutation." Amid the rapidly proliferating number of chemical
elements (and the Daltonian consequence of which he rejected —
namely, the rapid proliferation of many different kinds of matter) he
protested that

At present we feel impatient, and to wish for a new state
of chemical elements. For a time the desire was to add to
the metals, now we wish to diminish their number.

Faraday too would embrace the law of definite proportions, which he
described as "the teaching of Dalton, that chemical powers are...
definite for each body," while rejecting Dalton's solid, indivisible
billiard-ball atoms. His dynamical theory of matter led him,
eventually, to defend a version of Boscovicéan force atomism according to which individual particles are regarded not as solid, indivisible bits of matter, but as centres of force.

Faraday's principal contribution to the affinitist research tradition came as a result of his quantitative measurements of the chemical effects produced by electricity. In the process of electrolytic dissociation, particles are formed which he referred to as 'ions'.

They are combining bodies; are directly associated with the fundamental parts of the doctrine of chemical affinity; and have each a definite proportion, in which they are always evolved during electrolytic action. He proposed to call the numbers representing the proportions in which these evolved 'electrochemical equivalents', noting that these "coincide, and are the same, with ordinary chemical equivalents." The correlation between definite proportions and definite electrochemical action which followed from his results introduced great harmony into the associated theories of definite proportions and electrochemical affinity. According to it, the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity, or have naturally equal electric powers; it being the ELECTRICITY which determines the equivalent number, because it determines the combining force.

V. The Atomic Debates of the 1860s were a fitting, if somewhat frustrated, tribute to the confusing conceptual ferment in chemistry during the first 50 years of the century. Thus far, in drawing attention to certain features of that ferment, the concern has been to
single out those which have been unemphasized and often wholly neglected in the few, standard studies of 19th century chemistry which have been undertaken. On the whole, those studies have failed to attend to the theoretical and research alternatives to Daltonian atomism which the chemists of this period enjoyed. As a result the 'anti-atomist' opposition which they do address tends to emerge as a rather motley group of skeptics and critical detractors united largely by their denial of the existence of atoms.

The foregoing account would suggest that this depiction of the opposition is misleading in at least two respects. First of all, there was a viable and well-established alternative to Daltonian atomism -- a research tradition whose ontological, methodological and heuristic commitments had evolved from those which were predominant throughout the preceding century. Broadly speaking, 19th century chemists in this tradition sought to investigate and to quantify the forces which modify matter, and regarded chemistry as ultimately united with physics along dynamical lines. They shared Newton's wariness of hypotheses (as well as the acrasia in this regard to which Newton and the Newtonians were subject), and were concerned, à la Lavoisier, to distinguish chemical elements from ultimate physical atoms. Behind the chemical scenes of this tradition, commitments to the simplicity and homogeneity of matter and to some variety of force atomism were common.

Though plagued in its course with a good measure of empirical and conceptual problems (only a few of which we have alluded to here) this tradition was not thrown into a sudden eclipse by the appearance of its
Daltonian rival as the silence of historians on its fate would seem to imply. Its continued viability was due partly to the severity of the conceptual problems generated by Daltonian atomism, partly to its ability to accommodate Dalton's valuable empirical laws, and partly to the promise of its newly 'galvanized' heuristic. While the attention of the chemical community was indeed drawn by Dalton to more static concerns, many prominent chemists continued to pursue their affinitist studies. These 'anti-atomists' then, were provoked not by Dalton's assertion of the existence of atoms, but by what he had to say about their nature and centrality in chemical research. This was but one aspect of a much broader controversy over the issue of appropriate problem-solving domains -- were theories of chemistry primarily concerned with the dynamics of chemical reactions or with the determination of atomic weights and the arrangement of individual particles of matter?

The latter controversy, attenuated by the fact that the professional community in the first half of the century was still small and composed of generalists, contributed to the subsequent diversification and specialization of that community. It continued, in the second half of the century, along the very lines of chemical specialization which it had itself fostered: while inorganic and physical chemists would, in their explanations of chemical phenomena, give primary emphasis to the role of forces in chemical reactions, organic chemists would stress, that of the spatial arrangements of individual particles of matter. To suggest, as most historical treatments of this period have, that the debate which Dalton's theory
triggered was decisively settled against 'anti-atomism' and for 'atomism' (Ostwald's and Mach's notorious 'conversions' by Perrin's work are commonly cited in this respect\(^6\)) is to oversimplify in the extreme. By contrast, we still suggest below that this century-long debate in chemistry achieved a resolution at the end of the century only in a very qualified sense. It resulted, in effect, in a draw—two fundamentally different approaches to chemical problem-solving had emerged as equally valid. Before turning to these events, however, we must consider the tumultuous events of the 1860's.

Dalton's one-to-one equation of the chemical elements with solid indivisible physical atoms generated, as we have already seen, serious external conceptual problems for his theory by placing it at odds with accepted physical theories and in particular with the belief in the homogeneity and internal structure of matter. It figured as well as a source of internal conceptual problems for the theory since the smallest particles of compounds which Dalton also referred to as 'atoms' were physically divisible. These were the beginnings of a conceptual mare's nest in which chemistry and Daltonian atomism became increasingly ensnared and which may well be unrivalled in the history of science. 'Atoms', (elementary and compound), 'half-atoms', 'molecules' (elementary and compound), 'half-molecules', 'equivalents' and 'volumes'\(^6\), all were used in different senses and often interchangeably, as were 'atomicity', 'quantivalence' and 'equivalence'. In 1869 a congress of European chemists was convened in Karlsruhe for the purposes of bringing about some order and agreement in the terminological morass which often left them talking past one another.
and of discussing various problems which lack of agreement on 'atomic' or 'equivalent' weights had produced. The aims of the meeting were:

Définition of important chemical ideas, such as those expressed by the words: atom, molecule, basic. Examination of the questions of equivalents and chemical formulae. Establishment of a uniform notation and nomenclature.

And it was the formulae and nomenclature with particular respect to organic chemistry that the convenors were concerned. The first order of business dealt with the precise meaning to be given the terms 'atom' and 'molecule', but on this -- as on the matter of chemical formulae -- the congress adjourned without having obtained substantial agreement, although after a controversial vote it was decided that 'equivalent' was to be accepted as more empirical than 'atom' or 'molecule'.

Before the conference actually adjourned however, there did occur one event which would with time prove instrumental in resolving some of the conceptual ambiguity which bedeviled the entities of Dalton's New System. Those chemists who pursued Daltonian atomism during the first five decades of the century had been preoccupied primarily with arriving at an accurate determination of atomic weights from the equivalent weights which chemical analyses yielded. The variety of methods used for doing so had led to conflicting, discordant results and this confusion was of course transferred in the assigning of chemical formulae. Berthelot, for example, pointed out that four different empirical formulae had been assigned to acetic acid, while Kekulé managed to collect 18 different rational formulae which had been assigned to it. In Karlsruhe, Cannizzaro distributed copies of a paper in which he pleaded for the adoption of a system of atomic weights
based on work in the combining volumes of gases which had been accumulated several decades before. In 1811 Amadeo Avogadro had explained the volume relations of gases during chemical combination, which Gay-Lussac had already observed, by assuming that equal volumes of gases contain equal numbers of particles. In the case of elementary gases such as oxygen and hydrogen he postulated the existence of 'molecules constituents' -- particles which separated or divided during chemical reactions into the 'half-molecules' (atoms) of which they were composed. Cannizzaro began by considering some of the reasons why chemists had found Avogadro's hypothesis unacceptable and argued that these had lost their cogency. He then applied it to determine a system of atomic weights which was preferable to those yielded by other methods for reasons of simplicity, consistency and adequacy.

Upon their return from Karlsruhe, many of the chemists studied Cannizzaro's pamphlet and found his arguments convincing. A system of atomic weights along the lines he had proposed was soon adopted. With this, agreement and consistency in the assignment of chemical formulae became possible and concerted attention could be focused on such phenomena as isomerism whose very recognition had been problematic as long as disagreement over chemical formulae was acute. Analyses of various organic compounds had suggested that differences in properties between certain such compounds could not be adequately accounted for simply in terms of the kind and proportions of the elements present since their composition was, apparently, identical. The tendency to interpret chemical formulae structurally and to account for such differences in terms of differences in molecular arrangement was
encouraged as a result both of the adoption of a system of weights based on Avogadro's work and of the enthusiastic pursuit of valence theories by organic chemists throughout the '60's.

VI. It was in the debates of the Chemical Society which took place between 1866 and 1874 that the troubled state of chemistry 50 years after Dalton had 'struck' with his New System can most readily be ascertained. Of particular concern to us here is the 1869 address of that Society's President -- A.W. Williamson -- "On the Atomic Theory" and the lively discussion which followed it with Benjamin Brodie in the Chair. Williamson begins with a survey of the confusion which was so apparent and so rife with respect to the atomic theory in the chemical textbooks of the time. The considerable interest of this merits the following somewhat extensive quotations:

There are considerable differences, not to say discrepancies, between the statements made by different chemists on the subject of the atomic theory. In some textbooks of the science the replacing values of so-called equivalent weights of elements are described as being the atomic weights of those elements, while in the same and in other books, statements are made respecting the principles of the determination of atomic weights which lead to different numbers from those representing the replacing values.

Williamson goes on to offer a series of illustrative examples from these texts, which reveals something of the nature and the degree of dissatisfaction with the atomic theory:

"The question whether matter is infinitely divisible, or whether its divisibility is limited, remains, at the present day, at the same state as when it first engaged the attention of the Greek philosophers, or perhaps that of the sages of Egypt and Hindostan long before them."

Another distinguished author describes --

"The law of definite proportions;"

"The law of multiple proportions; and"

"The law of equivalent proportions."
He subsequently describes the "hypothesis of the atomic constitution of matter", the word hypothesis being no doubt intended to indicate an opinion on his part that the atomic constitution of matter is open to more doubt than the so-called law of multiple proportions, &c. Again, in another able book we find it stated that to each element is assigned a "particular number, termed its proportionate number, which expresses the least indivisible proportion of the element that is found to enter into a combination," hydrogen being taken as the unit, — a very intelligible description of the atomic weight of the element. In illustration of these so-called proportional numbers, the author gives Gerhardt's table of atomic weights, which were in use at the time his work was written. He avoids the word atom in describing his indivisible proportions, although later on he occasionally falls into the use of common words "atomic weight," and atom. When he comes to explain molecules and equivalents, there is no more talk about proportional numbers... Another author says: --

"The law of multiple proportions being founded on the experimental facts, stands as a fixed bulwark of the science, which must ever remain true; whereas the atomic theory by which we now explain this great law may possibly, in time, give place to one or more perfectly suited to the explanation of new facts." 37

Williamson then observed of his contemporaries that it sometimes happens that chemists of high authority refer publicly to the atomic theory as something which they would be glad to dispense with, and which they are rather ashamed of using. They seem to look upon it as something distinct from the general facts of chemistry, and something which the science would gain by throwing off entirely.... I think I am not overstating the fact, when I say, that, on the one hand, all chemists use the atomic theory, and that, on the other hand, a considerable number of them view it with mistrust, some with positive dislike. If the theory really is as uncertain and as unnecessary as they imagine it to be, let its defects be laid bare and examined. Let them be remedied if possible, or let the theory be rejected, and some other theory used in its stead, if its defects are really as irremediable and as grave as is implied by the sneers of its detractors.

The burden of Williamson's address is directed to underscoring the great explanatory value of atomic theory in accounting for a variety of experimental observations, and he discussed these at great length, arguing that the atomic theory is
a simple theory which explains in a consistent manner the most general results of accurate observation in chemistry, and is daily being extended and consolidated by the discovery of new facts which range themselves naturally under it."

In it, however, and in the subsequent discussion of it, we find considerable attention given to the numerous problems which had been troubling the conceptual foundations of the theory since its inception. The majority of the assembled chemists conceded the utility of the theory but expressed marked reservations as to the theory's acceptability. The status and ambiguous nature of its fundamental entity continued to trouble them: what was the atom? was it indivisible or did it have structure? and was it really necessary to do chemistry? Those present, whether critic or proponent, balked most decisively at any commitment to the limited divisibility of matter (i.e., to the indivisibility of the Daltonian atom). This is as true of those who regarded themselves as proponents of the theory as it was of the critics. Williamson, for example, is at pains to emphasize that the question whether our elementary atoms are in their nature indivisible, or whether they are built up of smaller particles, is one upon which I, as a chemist, have no hold whatever, and I must say that in chemistry the question is not raised by any evidence whatever.

...A philosopher disbelieving in the existence of atoms, would point out that whereas the divisibility of matter is infinite, we find that by the chemical processes available for the removal of carbon from carbonic oxide, the carbon divides into equal portions, one remaining in the oxygen, the other leaving it; and his statement that this half of the carbon is added on again when the process is reversed, is as consistent with the evidence as that of the believer in atoms, who asserts that in forming carbonic acid, oxygen is added in quantity equal to the oxygen in the carbonic oxide, while the formation of carbonic oxide from the acid consists in taking away half the oxygen.

So determined is Williamson to separate the issue of commitment to an atomic theory from that of commitment to 'indivisibles' that he alludes
to Dalton's inconsistent usage of the term 'atom' to refer to the smallest particle of both elements and compounds — the source of so much of the confusion which surrounded the atomic concept — in laudatory terms in the following remarkable passage:

It was a great step to extend the use of the word atom to groups of elements known to hold together only under certain limited conditions. For by including in one term, atom, the smallest particles of the elements, and the smallest particles of these compounds which behave like elements, we deprive the word atom of the only objectionable peculiarity of which it might have been accused. It is no longer an absolute term; and in its application to the elements it denotes the fact that they do not undergo decomposition under any conditions known to us. If anybody uses the word in its absolute sense in its present applications, he is guilty of manifest inconsistency.

Benjamin Brodie, presiding over the discussion of Williamson's lecture, accurately sums up the latter's position as follows:

Whether the smallest particles of matter have a spherical form or not, whether they are not in their nature indivisible; whether they are in reality the ultimate atoms of matter, or like the planets of this system, he (Williamson) knew not, nor did such questions exist for him as a chemist. He therefore thought it wise to exclude them, important as they were, from the actually existing atomic theory.

Brodie, for his part, found this attempt at problem resolution wholly unconvincing. The atomic theory, as he understood it, was that any given portion of matter is made up of a number of finite particles, the aggregate of which could be divided and subdivided until at last only one indivisible particle would remain, and this view might be derived from the perusal and common assent of every work on chemistry that defined the atomic theory. He thought that such a view as to the physical indivisibility of matter must be separated from the facts and basis of chemistry.

Brodie was particularly baffled at statements made by various of his colleagues to the effect that they were willing to use the theory although they did not believe it to be true: "He could not understand
using a theory and denying it at the same time." Frankland, for instance, who "considered it impossible to get at the truth, as to whether matter was composed of small and indivisible particles, or whether it was continuous -- the question belonged to what metaphysicians termed 'the unknowable'" had very readily acknowledged the importance of the fullest use of the theory as a kind of ladder to assist the chemist in progressing from one position to another in his science." Among the reasons cited for not accepting the theory as true was the fact that it failed to illuminate, and indeed hindered, the study of the dynamics of chemical reactions. Citing Faraday's work he notes that:

Any attempt to realize by its (the theory's) help the action of attraction and repulsive forces upon matter was excessively difficult; indeed to realize such an action through a perfectly void space was to him quite impossible ... if matter be assumed to be thus composed of solid particles separated by a void space, then, in considering the phenomena of electricity in connection with that view of matter, this space existing between the atoms must be either a conductor or a non-conductor of electricity. If a conductor, such a thing as an insulator was an obvious impossibility; if it was a non-conductor, such a thing as a conductor was equally inconceivable. Frankland concluded that while "he did not wish to be considered a blind Believer in the theory," he thought that "no one could blame him for not making a sufficient use of it." Brodie had, for some time, been convinced that it was nonsense for chemists to employ a theory they were willing to deny while exploiting its successes to the full. Two years earlier, in 1867, he had presented to the Chemical Society his Calculus of Chemical Operations. In his address, Brodie was careful to stress what he
regarded as the central virtues of the Calculus. Unlike Dalton's, Brodie's new system of chemical notation deployed symbols which were accurately defined, not intended in any way to represent the objects symbolized, and which did not raise the question of whether or not matter was atomic. Its aim was to free the science of chemistry from the trammels imposed upon it by accumulated hypotheses, and to endow it with the most necessary of all the instruments of progress, an exact and rational language.

Brodie had recently seen an advertisement for a set of balls and wires to construct molecular models, described as 'glyptic formulae.' The fact that chemistry should have resulted in such a "thoroughly materialistic bit of joiner's work" proved, he protested, that chemistry had gone off the rails -- such a "bathos" could only be the culmination of a whole series of errors and misconceptions. Atoms, he contended, were not only unnecessary but confusing, since they were not subject to any particular rules and could be manipulated at will. His new symbols, unlike those currently in use, had agreed and distinct meanings and could be combined under definite laws. Moreover, they took into account the laws of chemical combination without lending themselves, as equivalents did, to an atomic interpretation.

Other chemists contributing to these debates shared many of Brodie's misgivings about the confusing and ambiguous nature of the fundamental concepts of the atomic theory. Colin Wright declared:

I fail to see the cogency of the reasons which lead a great number of modern chemists to the impression that matter can only be viewed as being made up of "atoms" of some sixty-five essentially different kinds; these atoms, when connected together in certain ill-defined ways,
constituting the "molecules" of which the innumerable compounds now known are conceived as being made up.

And Wright, too, charged that atoms were unnecessary. The atomic theory had only been

a mechanical conception suited, doubtlessly, to an age when an accurate knowledge of facts was only beginning to exist, these facts being of a nature difficult to grasp without some material aid, but not possessed of such advantages when the knowledge of these facts and their correlations becomes somewhat more extended ... it is unnecessary to express any facts ...

Mills extended this general line of attack, pointing out that chemical discoveries had cast no light upon the question of whether atoms were indivisible; adding that atomic theories had neither given any satisfactory explanation of affinity nor made any headway in elucidating isomerism:

Isomerism is not ... explained by assertions about indivisibles, which have neither been themselves discovered nor shown to have any analogy in the facts or course of nature -- nor by explicit statements about a "structure" which has never been seen -- nor by the use of a phrase, to which no clear definition has been, or can be, attached.

He also argued that the atomists had not explained isomerism since space and position could only be relative, and to talk of position in empty space was to talk nonsense.

VII. If any one development were to be singled out as central to the resolution of the controversies which were generated by the appearance of Dalton's [New System] at the beginning of the century and which waxed to a frustrated climax some 60 years later on the floor of the Chemical Society, it would have to be the radical changes in the nature of the chemical community during the last third of the century. The role of
these changes in securing the détente reached by the century's end (as well as the fact that the outcome was indeed a détente and not some clear-cut victory of 'atomists' over 'anti-atomists') has been obscured by the persistent tendency to regard these debates as turning primarily upon issues of ontological commitment.

In contrast to such standard historical treatments, the present study will offer a sketch of a resolution to the controversy in which neither of the two research traditions traced thus far definitively lost or won. Rather, a quite different view of the outcome of the debates presents itself when they are construed within the broader context we have been urging here, i.e. as turning primarily upon the issue of appropriate problem-solving domains — of whether theories of chemistry were properly concerned with the dynamics of chemical reactions or with the determination of atomic weights and the arrangement of individual particles of matter. Understandably, this disjunction had not been read inclusively by a small and 'generalist' chemical community. But the possibility and desirability of such a reading would become apparent with the rapid growth and specialization of that community in the last third of the century. Eventually a resolution to the longstanding controversy would be secured by the attendant methodological diversification of that community and two fundamentally different approaches to problem-solving in chemistry would emerge as equally valid.83

One of these approaches — which we will refer to as the particulate approach — can be seen as the contribution of chemists
(notably of organic chemists) working within what might be regarded as the tradition of Latter-Day atomism, substantially modified and influenced by the development of the kinetic theory. The other referred to here as the bulk approach was the contribution of chemists (notably physical chemists) working within what we might regard as Latter-Day affinitism, a research tradition which would benefit considerably from work in thermodynamics. On the bulk approach, unlike the particulate, chemical reactions need not be pictured in terms of atoms, molecules and their motions. No underlying model of the mechanism of chemical change is postulated and while one may, in employing it, view matter as continuous rather than discrete one need not make any commitments in this regard. Instead, by considering the energy changes involved in chemical reactions, the bulk approach deals with the system as a whole in terms of empirical macroscopic relationships (between, for example, the thermodynamic coordinates, pressure, volume, temperature, etc.). Before we examine this approach and how it provides the Latter-Day affinitists with the elusive and long-sought true measure of 'chemical affinity,' we need to explore developments within Latter-Day atomism — the laying to rest of the conceptually troubled Daltonian atom by a much less problem-riddled successor together with the forging of the particulate approach and the modifications induced by the application of kinetic-molecular theories in physics.

The burst of activity in organic chemistry during the last third of the nineteenth century was stimulated by a variety of factors. The value and usefulness of the earlier proposals of Cannizzaro regarding
the construction of a consistent system of atomic weights began to be appreciated, as did the work of Kekulé in structural and stereochemistry. The latter had given considerable impetus to organic research by introducing the idea of the tetravalency or fourfold saturating capacity of carbon. Kekulé's ground-breaking experimental work served as an important first step in expanding the problem-solving domain of Latter-Day atomist theories. In addition to weight relations, such theories would address themselves to the geometrical arrangements of particles of matter in space. And fruitfully so, for as Merz -- writing at the end of the century -- noted, 'Kekulé's capacities of saturation or valencies appeared very early as points of saturation, and the saturation itself as a linkage.... One of the most remarkable instances of the use of linkages to explain the difference of a series of organic compounds, all closely connected with each other, is the theory of the so-called aromatic compounds, derived from benzene, which we owe to Kekulé. It has stood the criticism of more than a quarter of a century, and has led to the most wonderful practical knowledge of a large number of old and new compounds. It is not astonishing if, in the face of these remarkable strides which geometrical symbols have led to, an attempt has been made to form an actual conception of the geometrical figure and grouping of the atoms of which chemical molecules and compounds are made up.

This attempt was a principal concern of numerous organic chemists during the last third of the century. In 1874 Van't Hoff and Le Bel accounted for the phenomenon of optical isomerism, which had proven problematic for Kekulé's earlier structural formulas, by proposing a tetrahedral carbon atom. When, in 1887, the power of this three-dimensional view of bonding was demonstrated by Wislicenus as it was soon extended by other chemists such as Meyer and Pope beyond the carbon
atom. Merz heralds these developments as the fulfillment of an 80-year-old "prophecy." In 1808 Wollaston, stimulated by but discontented with Dalton's New System, had observed that "the atomic theory could not rest contented with a knowledge of the relative weights of elementary atoms, but would have to be completed by a geometrical conception of the arrangement of the elementary particles in all the three dimensions of solid extension."^{35}

While these developments within structural and stereochemistry served to expand the problem-solving domain of Latter-Day atomist theories they also served to relieve some of the befuddledness which had so burdened the concept of the atom throughout the century. What they suggested in particular was the ultimate unacceptability of an indivisible, unstructured Daltonian atom. As one contemporary chemist noted, the three-dimensional view of bonding of the "chemistry in space"

necessarily leads, ... to the assumption that polyvalent atoms can no longer be thought of as material points, but that parts of atoms can be distinguished from which an effect is exerted on other atoms....

To this Wislicenus replied: I completely agree ... that our views on the structure of molecules make it impossible to suppose that atoms are 'material points.' It would be nearer the mark to regard them as spatial structures ... and to suppose the chemical activity units in polyvalent atoms to be located at various points in these structures. Nothing stands in the way of the view, so long as we regard so-called elementary atoms not as 'atoms' in the strict sense, but as made up of groups of primary atoms of simpler type.... This idea is neither startling nor new. Most chemists nowadays who take an interest in such matters would agree with it.
Indeed, "this idea" had begun to acquire a secure foothold some 15 years earlier, in 1870. Lothar Meyer, who shared the Nobel Prize for the periodic table with Mendelev, had opened his Annalen paper of that year with the declaration:

That the until-now undecomposed chemical elements are absolutely undecomposable materials is for the present very improbable. It seems more probable that the atoms of the elements are not the ultimate but only proximate parts of molecules both of the elements and of their compounds. Thus the molecules are particles of the first order, the atoms a second order which are composed of particles of a third and higher order.

Two years later, Mendelev had noted in another Annalen paper that

The discovery of such relations (those of Dumas) led to the comparison of the members of the various groups with those of homologous series and further to the chemico-mechanical conclusion on the compound nature of atoms, which is held to be most probable by chemists.

The modifications induced by work in organic chemistry that we have been considering are, however, only part of what distinguishes Latter-Day atomic theories from those which prevailed earlier in the century. Even more significant perhaps is the fact that they finally began to be buttressed by and formulated under the influence of recent developments in physics, in particular of kinetic-molecular theories. These theories, which were to provide a powerful new heuristic to chemists working in the atomic research tradition, made use of a model which explained the physical behaviour of gases in terms of a rectilinear translatorymotion of the molecules. They received their initial impetus from Joule's work on the mechanical theory of heat during the 1850's, and underwent considerable refinement and extension over the second half of the century in the hands of Clausius, Maxwell,
William Thomson, van der Waals and Boltzmann. A detailed account of the evolution of kinetic-molecular theory is obviously beyond the scope of the present study. But there are several aspects of it which need to be singled out as of particular importance to the chemical controversy addressed here. These include certain developments in thermodynamics with which the evolution of kinetic-molecular theory is entwined. In the final section of this chapter, we will consider the significant impact which these developments in thermodynamics had on the work of chemists who continued to contribute to the affinitivist research tradition.

VIII. Having abandoned the idea that the particles of a gas enjoy a rotatory or vibrating motion, James Joule, in a series of experiments undertaken during the 1850's, adopted instead the suggestion of Bernoulli that they are in a natural state of rectilinear motion — changed only by their impact with other particles on the walls of the containing vessel. In 1857 he succeeded in calculating the velocity with which a hydrogen particle, at ordinary temperature and pressure, must be moving under the assumption that the atmospheric pressure is equilibrated by the rectilinear motion and impact of the gas particles on each other and the vessel walls. The historian Merz, writing at the end of the century, stresses the great importance of Joule's results, describing them as "the first attempt to explain the physical properties of matter by giving a numerical value to a molecular, not molar, quantity." It was, he observes, through the acquisition of this and further such numerical data regarding molecular size and
composition that the scientific community of his time began to take the atomic view of nature in real earnest.

Yet it was primarily through the work of Rudolf Clausius that chemists in that community began to appreciate the significance of kinetic-molecular theory for their own research along atomic lines. In his celebrated paper of 1857 on specific heats, Clausius showed that it was possible to account for a troublesome discrepancy between expected theoretical values and actual experimental results by assuming that the molecules of a gas underwent an internal, as well as a rectilinear translational, motion. He suggested that the heat absorbed by a gas may have the effect not only of increasing the velocity of the molecules but of causing the atoms of which they are composed to vibrate, or to rotate around a common center. The energy which brought about these latter effects he termed "atomic energy", and he distinguished it from the "molecular energy" which caused an increase in the velocity of the gas molecules. As a result of Clausius' work the new (kinetic) ideas became still more exactly defined; they included the conception familiar to chemists of compound atoms or molecules. The smallest individual particles of matter in the free state were themselves not simple bodies, but systems of still smaller particles; they were molecules composed of atoms; the symbols of chemists became descriptive of real physical conditions; the vague notions of radicles, types, or compound atoms began to acquire geometrical and mechanical definiteness.

During the 60's and 70's the basic equations of the kinetic theory were established through the efforts of Clausius, Maxwell, Boltzmann and van der Waals. Correlations were made with Boyle's and Charles' laws, with Graham's law of gaseous diffusion, and with
Avogadro's hypothesis. In 1867, during the course of the atomic debates, Maxwell pointed out that chemical atomic theory was based on an assumption -- Avogadro's hypothesis -- which could not be deduced "from purely dynamical considerations (based on) the supposition advocated by Professor Clausius and others, that gases consist of molecules floating about in all directions and producing pressure by their impact." He suggested (as did an increasing number of other scientists of the time) that when two distinct theories, based on quite different evidence, physical and chemical, lead to the same conclusion, it would seem to be good evidence for their soundness. Chemists, during these decades, gradually began to apply the kinetic theory to chemical problems and to cite the successes of the kinetic theory in support of their own atomic views. C.A. Wurtz, for example, in 1879, maintained that "As heat itself is a mode of movement, it results that the thermochemical facts are perfectly adapted to the atomic hypothesis." And Cannizzaro, discussing the kinetic theory, noted that

In this manner alone can all that relates to the molecular and atomic constitution of bodies be deduced from a single principle, which is, indeed, that most natural passage from physics to chemistry. Is it necessary for me here to declare that I do not admit that difference between the physical molecule and the chemical molecule which some persons have wished to introduce?

It would be difficult to overstate the importance of these developments in kinetic-molecular theory to the progress of Latter-Day atomism. Not only did they relieve the long-standing tension between chemical and physical reasoning, ushered in by Dalton's New System and described by so many commentators of the time as producing a great
divide' between the two communities, they "brought about a union of
the researches of chemists and physicists." Latter-Day atomists were
now able to enlist the support of an independent, physical line of
reasoning for their chemical theorizing and, moreover, to draw upon it
for help in clarifying the confusion which had so persistently attended
the fundamental concepts of 'atom' and 'molecule'. And, by providing
the first reliable estimates of atomic dimensions, it greatly
strengthened the belief that appeal to such concepts was not merely
convenient, but necessary. Further, the rise of kinetic-molecular
theory permitted the forging of a particulate approach to chemical
problem-solving which greatly enhanced the empirical problem-solving
domain of atomic theories. It became possible for Latter-Day atomic
theories, in contrast to those which preceded them earlier in the
century, to offer an account of chemical processes. Chemical reactions
could be pictured

in terms of atoms and molecules and their motions. In this
mechanistic, kinetic aspect a definite picture of the
mechanism of chemical change is possible, limited only by
the scientists' ignorance of the laws governing the motions
of atoms and molecules.

Merz, in the following passage, draws attention to a number of the
above respects in which kinetic-molecular research impacted upon and
contributed to the efforts of chemists working along atomic lines:

The great differences exhibited by larger portions of
different kinds of matter -- i.e. the chemical differences
or qualities -- were reduced to the actual composition and
qualities of the molecules and atoms themselves. Chemists
and physicists were now alike compelled to venture on some
more definite hypothesis, descriptive of the great variety
of constitution which the molecules of chemically distinct
substances exhibit.... The manner in which these
molecules enter into, and again separate out of,
combinations and compounds always regaining and showing
their original characteristics, forced more and more upon
natural philosophers the conviction that compounds were
merely geometrical arrangements of individually independent atoms, and that these atoms must possess geometrically different forms and figures, enabling them, without loss of their individuality, to enter into varying configurations.

The kinetic-molecular programme, however, was not without its difficulties and critics, and both of these influenced the evolution of atomic theories during the last two decades of the century. The trouble came from the rapidly developing field of thermodynamics — specifically from the clash between classical thermodynamics and the kinetic-molecular theory of heat. In the final section of this chapter we will consider how the application of thermodynamics to chemistry advanced the programme of Latter-Day affinitism and was responsible for the forging of the second, "bulk", approach to chemical problem-solving described above. For the remainder of this section our attention will be confined to the course of the controversy over kinetics and thermodynamics.

We have already noted that, during the 60's and 70's, kinetic-molecular theory was successfully applied to numerous physical and chemical problems. By the 80's however, this early, rapid rate of empirical problem-solving progress began to slow considerably. A spate of critical attacks on the kinetic theory of gases was published, especially during the last decade of the century, drawing attention to this and raising serious doubts about the conceptual well-foundedness of a theory that was incompatible with classical thermodynamics. Van't Hoff, for example, observed in a letter to Arrhenius:

"You write about the anti-kinetic utterances of Ostwald. I, too, must say that with a fairly large expenditure of
mathematical development the kinetic theory barely gives the current 4% interest on capital, and I think that even this theory should be measured by its fruits.

And Planck, writing of the kinetic theory in his 1897 textbook on thermodynamics maintained that

Obstacles, at present unsurmountable, however, seem to stand in the way of its further progress. These are due not only to the highly complicated mathematical treatment of the accepted hypotheses, but principally to essential difficulties... in the mechanical interpretation of the fundamental principles of Thermodynamics.

According to the Second Law of Thermodynamics, energy is subject to irreversible dissipation in all natural transformations. This property is not shared by a mechanical system of particles obeying Newton's laws of motion. This incompatibility with the absolute validity of thermodynamics became one of the central arguments against the atomic-kinetic programme.

Those who supported kinetic-molecular theory argued that the Second Law of Thermodynamics was essentially statistical in nature. The efforts of Maxwell to apply statistical methods to the kinetic theory were followed by those of Boltzmann, whose H-theorem placed the statistical interpretation on a more quantitative basis. These attempts to resolve the apparent contradiction between macroscopic irreversibility and the fundamental reversibility of Newtonian mechanics faced certain obstacles however. The two fundamental problems were posed by the 'reversibility paradox' and the 'recurrence paradox.'
The 'reversibility paradox' arises from the seeming contradiction between the irreversibility which the H-theorem predicts for a system of many molecules and the reversibility of individual collisions which is one of the basic premises in the derivation of the theorem. As Kelvin, who first discussed the problem in 1874, put it: "How can irreversibility result from molecular motions and collisions which are themselves (according to Newton's laws of motion) strictly reversible in time?"\textsuperscript{100} Boltzmann's response to this problem when it was brought to his attention in 1876, was, as Stephen Brush points out, quickly to convert "the apparent difficulty into a new conceptual advance."\textsuperscript{101} He explained the tendency of systems to pass from ordered to disordered, rather than from disordered to ordered, states as due to the far greater number of disordered states. The great majority of individual molecular states correspond to 'disorder' rather than 'order'. This explanation led him to clarify the theretofore ambiguous concept of entropy as a measure of disorder; the tendency toward increasing entropy is to be understood as a tendency toward increasing disorder. Later, in response to detailed technical criticism of the H-theorem raised in 1894, Boltzmann recognized that a much more specific postulate of molecular disorder was needed in the proof of the H-theorem if the 'reversibility' objection were to be fully met. And he agreed with Burbury's suggestion that in order to derive the principle of irreversibility from the classical kinetic theory it was necessary to assume—that molecular motions are randomized after each collision.\textsuperscript{102}
In 1896, Zermelo and Poincaré pointed to another conceptual problem which was generated by the kinetic theory in connection with the statistical interpretation of the Second Law of Thermodynamics. Zermelo argued that Poincaré's 'recurrence theorem' in dynamics, according to which any mechanical system confined to a finite space with fixed total energy must eventually return arbitrarily close to its initial state, makes any mechanical model such as the kinetic theory incompatible with the Second Law of Thermodynamics. If a gas really is a mechanical system composed of atoms obeying Newton's laws, then it cannot go irreversibly toward an equilibrium state but must return to its initial non-equilibrium state. In short, entropy must -- contrary to the Second Law -- eventually decrease. Since he, together with numerous other scientists of the time, regarded the Second Law as a strictly valid induction from experience, he urged, with them, that the contradiction be resolved by abandoning the assumption that a gas is a deterministic mechanical system obeying Newton's laws.\textsuperscript{103}

Boltzmann, in reply, insisted that the Second Law of Thermodynamics was only statistically, not absolutely, valid. He indicated, moreover, that the recurrence property is completely in harmony with the statistical interpretation of the Second Law, for the probability of a statistical fluctuation that would return the system to its initial state is so small that one would have to wait untold eons of time before observing a recurrence of the initial state. Consequently, the fact that such behaviour of gases is never observed in the laboratory is perfectly consistent with the kinetic theory. Boltzmann noted that:
when Zermelo concludes, from the theoretical fact that the initial states in a gas must recur — without having calculated how long a time this will take — that the hypotheses of gas theory must be rejected or else fundamentally changed, he is just like a dice player who has calculated that the probability of a sequence of 1000 one's is not zero, and then concludes that his dice must be loaded since he has not yet observed such a sequence.

It did not, however, prove to be necessary to wait untold eons of time before a physical situation which violated the Second Law of Thermodynamics was observed in the laboratory. In 1905 Einstein presented his theoretical treatment of the problem of Brownian motion, and made clear its significance in settling the dispute between those advocating a statistical-atomistic theory and those who supported the absolute validity of the Second Law of Thermodynamics:

In this paper it will be shown that according to the molecular-kinetic theory of heat, bodies of microscopically-visible size suspended in a liquid will perform movements of such magnitude that they can be easily observed in a microscope, on account of the molecular motions of heat. It is possible that the movements to be discussed here are identical with the so-called 'Brownian molecular motion'. If the movement... can actually be observed (together with the laws relating to it that one would expect to find), then classical thermodynamics can no longer be looked upon as applicable with precision to bodies even of dimensions distinguishable in a microscope; an exact determination of the actual atomic dimension is then possible. On the other hand, had the prediction of this movement proved to be incorrect, a weighty argument would be provided against the molecular-kinetic conception of heat.

During the next few years Jean Perrin conducted a series of experiments which confirmed Einstein's predictions and solved the long unsolved problem of Brownian motion.
In his discussion of the ambiguous status of unsolved problems, Laudan points out that because it is sometimes not clear to which domain of science an unsolved problem belongs such a problem is often recognized as a genuine problem only when it has been solved. The transformation of unsolved into solved problems is one of the means by which theories demonstrate empirical progressiveness. In the present case, the phenomenon known as Brownian motion was first discussed by Robert Brown in 1828. But for over seventy-five years it was unclear whether it was a genuine or important problem and so long as it remained a scientist could ignore it by claiming that it was not a problem which a theory in her or his field should be expected to solve. With its solution in the first decade of the twentieth century, the problem of Brownian motion became "one of the triumphal successes of the kinetic-molecular theory of heat" and at the same time "one of the core anomalies for classical thermodynamics . . . due to the presence of an alternative research tradition which could solve it." 106

Brownian motion was only one of a number of significant empirical problems for which theories within the atomic-kinetic research tradition had, by 1908, proven themselves to be adequate problem-solvers. Scientists such as Crooke, Arrhenius, J.J. Thomson, Rutherford and others had used atomic theories in their research to solve a wide array of empirical problems. Their efforts, moreover, led to the abandonment of the hard, stable and indivisible Daltonian atom and with it to the resolution of many of the conceptual problems raised against atomic theory in the debates of the 60's and 70's. The now structured atom and the complex molecule were, as Perrin observed in
Les Atoms in 1913, a focus of unification for an extraordinary diversity of phenomena: the stoichemical laws, substitution, chemical valency, and stéreochemistry; diffusion, Raoults law, Arrhenius ion theory, and osmotic pressure; kinetic theory, Brownian motion and micro-fluctuations; quantum theory and black-body radiation; cathode rays and X-rays, gas ionizations and positive rays; radioactivity, and the probabilistic interpretation of the Second Law of Thermodynamics. 107

IX. In the previous two sections we have seen how developments in organic chemistry and the application of kinetic-molecular theory to chemistry contributed greatly to the refinement of Latter-Day atomic theories and the forging of the particulate approach to chemical problem-solving. We must turn now to consider how research in physical chemistry and the application of thermodynamics to chemical processes influenced the evolution of Latter-Day affinity theories with their attendant bulk approach to chemical problem-solving. In what follows it will be important to keep in mind that two different strains of affinitism can be distinguished in the closing decades of the century. Some of those who adopted a bulk approach to chemical problem-solving in their affinitist research also endorsed the sweeping, substantial claims of the energeticist programme, while others did not. It is the latter, non-energeticist, strain of affinitism that is of central importance in establishing the claims made earlier 108 regarding the nature and resolution of the century-long controversy engendered by Daltons New System. — viz., that with the increasing specialization
and methodological diversification of the chemical community the resolution achieved by the century's end is appropriately construed not as a victory of 'atomists' over 'anti-atomists,' but more along the lines of a 'draw' in which two fundamentally different approaches to chemical problem-solving emerged as equally valid. Nevertheless, the energeticist strain is of interest and some consideration of it is in order here.

Studies of the physical agencies which were at work in chemical processes, and in particular of the importance of energy in chemical reactions, figured prominently in the research of an increasing number of scientists during the latter part of the 19th century, giving rise to a new branch of chemistry, physical chemistry. Although the establishment of physical chemistry as a separate discipline is usually traced to a specific date and event -- 1887 and the publication of the first number of Ostwald and Van't Hoff's "Zeitschrift für physikalische Chemie" -- fundamental contributions to the field had been made well before this date by both chemists and physicists. It was through the integration of this earlier work in thermochemistry and thermodynamics that the efforts of physical chemists towards the end of the century led to the clarification and long-sought quantitative determination of the forces of chemical affinity which had eluded affinity theorists during the first half of the century. As one commentator, surveying these developments and writing in the midst of them, notes:

'It has been one of the greatest performances of the last twenty years of the century to have approached the all-important question, "What is chemical affinity, and how is it to be measured?" in a comprehensive spirit, and to have brought it to the verge of solution.'
The year 1887... can also be considered as the epoch when the new science of physical chemistry was fairly launched into existence... From that period the physical properties of chemical substances have received systematic mathematical, and exact treatment, guaranteeing something like continuity and completeness, and leading on to the solution of the great remaining question, What is chemical affinity?110

The thermodynamic concept of affinity at which physical chemists arrived was in terms of free energy: affinity was regarded as a measure of whether or not a certain reaction would proceed. Since this view approaches chemical reactions by considering the energy changes involved and deals only with the observable, measurable behaviour of large quantities of substances it requires no assumptions about the nature of the constitution of matter and there is no need to postulate the existence of atoms or molecules or even the kinetic theory of heat. It is, however, usually convenient to make use of the language of atomic and molecular theories.111

Indeed, a number of those who contributed to these developments in Latter-Day affinitism also contributed to work in kinetic-molecular theory and Latter-Day atomism -- most notably, perhaps, Clausius, J.J. Thomson and William Thomson (Lord Kelvin). Others, however, were among the most vocal opponents of the latter theories, and regarded it as a virtue of this bulk, thermodynamic, approach to chemical problem-solving that it is independent of atomic and kinetic reasoning. As Merz comments, such

opponents of the kinetic, mechanical, or material views of natural phenomena have always existed: in the early years of the century they described their view by the word "dynamic". At that time it was the atomic theory they principally objected to. ... The more recent critics of the mechanical interpretation of physical phenomena, among whom I will mention only Prof. Ostwald of Leipzig, Prof. G. Helm of Dresden, and Prof. Ernst Mach of Vienna, ... maintain that their exact treatment is not arrived at by introducing
hypothetical quantities such as the atomic and other theories are founded upon, but by contenting themselves with measuring such quantities as are presented directly in observation, such as energy, mass, pressure, volume, temperature, heat, electric potential, &c., without reducing them to imaginary mechanical or kinetic quantities.

The research which led to this thermodynamic concept of affinity was extensive and varied, and, until the publication of Ostwald's great two volume textbook, Allgemeine Chemie (1885-87), scattered and fragmented. The early thermochemical investigations of Berthelot and J.J. Thomson laid the foundation for much later work. Thomson sought to develop the chemical side of the mechanical theory of heat by applying it to thermochemical processes. In his 1861 memoir on the "General Nature of Chemical Processes, and on a Theory of Affinity Based Thereon" and in his four volume Thermochemische Untersuchungen (1882-86), his aim was to establish -- through quantitative thermochemical investigations -- the dynamical laws relating to chemical processes. As a result of his work, it became clear that thermal methods were valuable in studying the problems of affinity. Berthelot, who contributed to these efforts to measure chemical forces by means of thermal quantities (and particularly, with his "bomb calorimeter"), to the improvement of techniques of thermochemical measurement), succeeded in demonstrating that there were marked similarities between chemical and physical equilibria. In 1873 he announced his principle of maximum work or third law of thermochemistry, the refinement and qualification of which would absorb the labours of subsequent researchers such as Helmholtz and Van't Hoff.
Significant early work in chemical thermodynamics was conducted by Horstmann, Gibbs and Helmholtz. Horstmann, in 1868, was the first to apply thermodynamics to chemical change; in 1875 Gibbs showed how thermodynamical principles could be applied to heterogenous systems (Van't Hoff and Le Chatelier would later do the same for homogenous systems); and Helmholtz, in 1881, introduced the concept of free or available energy as the measure of chemical reaction. These contributions, however, were appreciated mainly by physicists and it was not until the work of Van't Hoff, beginning in 1884, that the importance of applying thermodynamical principles to chemical processes became apparent to chemists. Van't Hoff did this by applying thermodynamics to chemistry with special reference to ideas of affinity:

The question of equilibrium was very closely associated with the problem of affinity, and so I took up first the simple phenomenon of affinity, which finds expression in the attraction of water.

Following Guldberg, Waage and Berthelot, he provided an analysis of equilibria in terms of 'mass action' which rendered unnecessary the use of parameters other than 'active mass,' temperature, pressure, number of phases, equilibrium constants and other heat-related quantities. He showed, moreover, that on the basis of the second law of thermodynamics a measure of affinity is to be found in the maximum external work that can be done by a chemical reaction "without admitting anything concerning ... the matter wherein affinity is supposed to reside." Finally, when Nernst, in his 1906 "Calculation of Chemical Equilibrium from Thermal Measurements," added a third theorem to the first and second laws of thermodynamics, it became possible to calculate the free
energy or the maximum work of a chemical reaction from purely thermal measurements.

The first edition of Ostwald's *Allgemeine Chemie* had the great merit of consolidating much of the above research and uniting "the disjecta membra of physical chemistry, notably of the theory of affinity, into a systematic whole." But the concepts of physical chemistry encountered resistance in certain quarters, notably among organic chemists. This resistance increased markedly with the publication of the second edition of Ostwald's work in 1893. There he introduced the concept of 'energetics', making it the foundation of the doctrine of affinity, and coupling it with strong criticism of atomic and kinetic theories. (The term, invented by Rankine, had been revived and extended into a far-reaching principle by Helm in 1887, but did not receive much attention until Ostwald adopted it in a somewhat modified form.)

By generalizing on classical thermodynamics and applying thermodynamic concepts in both physics and chemistry, Ostwald's energetics programme was an attempt to re-establish the whole of physics and chemistry on non-atomic 'energetic' foundations. Energy, according to Ostwald, was the fundamental "indeed the only real thing in the so-called outer world," and all the phenomena of nature were manifestations of energy and its transformations. The programme of energetics could not have been more extreme or ambitious. As Stephen Brush observes, it claimed "to be the only legitimate scientific theory, sufficient unto itself" and could find no place in either
chemistry or physics for atomic and kinetic reasoning — which was castigated as 'materialistic', 'hypothetical' and 'unnecessary'. (Consider, for example, the polemical title of Ostwald's 1894 Lübeck address — "Die Überwindung des wissenschaftlichen Materjalismus," and his later assertion that the early stoichiometrical laws of chemistry could be arrived at through thermodynamic, experimental considerations of phase equilibrium:

Chemical dynamics has ... made the atomic hypothesis unnecessary for this purpose and has put the theory of stoichiometrical laws on a more secure ground than that furnished by a mere hypothesis.¹¹⁹

It is likely that it is this anti-atomist energeticist strain of affinitism which many commentators have had in mind when they have construed the resolution of the 19th century atomic debates as a victory of 'atomism' over 'anti-atomism'. This verdict, as we indicated earlier, has been further encouraged by the widespread tendency to regard these debates as turning primarily upon issues of ontological commitment and to overlook the presence of a viable, alternative, affinitist research tradition. The case study offered here has attempted to demonstrate that such a construal is not only a gross oversimplification, but a distortion, of a far more complex and interesting controversy — one in which conceptual and methodological considerations played a vital role.

We have suggested that the century-long controversy engendered by Dalton's New System is misleading and inaccurately construed in terms of victory and defeat. Rather, the modifications which both Latter-Day
atomic and affinitist theories underwent in the closing decades of the century and which contributed to their conceptual refinement as well as to their empirical adequacy, together with the increasing specialization and methodological diversification of the chemical community resulted by the century's end in a 'draw' in which two fundamentally different approaches to problem-solving in chemistry emerged as equally valid: the bulk approach adopted by many physical chemists pursuing research along non-energetic affinitist lines and the particulate approach utilized primarily, though not exclusively, by organic chemists. Since the former, unlike the latter, addresses chemical reactions through a consideration of the energy changes involved, it confines itself to observable, measurable quantities and does not stand committed to any particular theory of the nature or constitution of matter. The achievements of Latter-Day affinitism which the bulk approach helped to secure are not incompatible with those won by Latter-Day atomism through a particulate approach. Which approach it was appropriate for chemists to adopt would turn in part upon the kind of chemistry they were doing and in part upon which aspect of chemical processes was being addressed:

Chemical reactions present two aspects to the investigator, change in form or distribution of matter and change in form and distribution of energy. . . . chemists (who paid) . . . heed to the latter of these changes . . . saw the ultimate merger of chemistry and thermodynamics. . . . fruitful answers to scientific problems have often been found by viewing matter as continuous or even ignoring entirely the question of 'discrete versus continuous' matter. Thermochemistry and chemical thermodynamics are good examples of this latter approach to problem-solving. . . . Chemistry, the periodic table, the laws of combining weights neither confirm nor contradict thermodynamics, but thermodynamic results and data govern the phenomena that chemistry interprets in its laws.
Footnotes

1. Dalton (1808).
2. Merz (1904), 419.
3. Williamson (1869), 331.
4. Dalton and the reception which his New System received have, of course, received a great deal of attention. Of the vast number of studies published I will here mention only a few: Thackray (1966a), (1966b), (1968a), (1970); Knight (1967); Brock and Knight (1967); Nye (1976a), (1976b); Cardwell (1968); Gardener (1979); Greenaway (1968); Guerlac (1961), (1968); Merz (1904); Buchdahl (1960).
6. Macquer, in his 1749 Eléments de chymie théorique, as quoted by Guerlac (1968), 89.
8. Lavoisier, in his 1789 Traité Elémentaire as quoted by Thackray (1968b); 108.
10. Macquer as quoted by Metzger (1930), 61.
13. For a discussion of the nut-shell theory of matter, see Thackray (1968b).
14. Peter Shaw in his 1734 Chemical Lectures, as quoted by Thackray (1968b), 48.
15. William Cullen’s 1762 views, as recorded by one of his students and quoted in Thackray (1968b), 48.
17. Lavoisier, in his Elements of Chemistry, as quoted by Thackray (1968b), 49.
18. See, for example, Merz’s discussion of “The Atomic View of Nature” in his (1904), and Knight (1967). Also Brock and Knight (1967).
22. Berthollet in his 1803 Essai de statique chimique, as quoted by Thackray (1968), 105.
24. Whewell (1847), 388.
26. Ibid., 48.
27. Ibid., 48.
31. Dalton (1808), 212.
32. Dalton, in an 1810 lecture, in Roscoe and Harden (1896), 112.
33. Dalton (1808), 142.
36. Dalton (1808), 213.
37. Ibid., 214.
38. Ibid., 216.
40. Dalton (1808), 213.
41. Davy, as quoted in Knight (1967), 19.
42. Dalton in Roscoe and Harden (1896), 159.
43. Wollaston, as quoted by Knight (1967), 24-5.
44. Wollaston, in ibid., 10.
45. Wollaston, as quoted in Brock and Knight (1967), 5-0.
46. See the discussion of Ihde (1964), 152-53.
47. Berzelius in Roscoe and Harden (1806), 101.
49. Berzelius (1813), 450.
50. Ibid., 450.
51. Ibid., 450.
52. Berzelius (1815), 127.
53. Ibid., 122.
54. Faraday, in his 1838 Experimental Researches in Electricity, as quoted by Levere (1971), 87.
56. Faraday, as quoted by Crookes, cited in Scott (1959), 64.
58. Faraday, in ibid., 87.
60. See, for example, Nye (1976).
61. See Crosland's discussion in his (1978), 344.
62. Contributors to the Chemical News of 1869, in a lively series of letters under the heading of "Equivalence and Quantivalence", repeatedly take up, and propose their solutions to, this befuddling problem. Consider as an example, the following excerpt from F.O. Ward in the April 30 issue:

Sir, -- In your report of the last meeting of the Chemical Society appear some valuable remarks of the President, Dr. Williamson, on the injurious confusion which is introduced into chemical science by the indeterminate significance, and interchangeable use, of the two words atomicity and equivalence. Dr. Williamson deprecated the shifting and uncertain sense which had been attached to these expressions by various speakers during the evening's debate. "He would not," he added (according to your report), express any opinion as to whether the atomicity
theory was right, or the equivalence theory, but they were different."
He then attempts to offer
in the concisest language at my command, exact definitions
of equivalence and atomicity; or, as I prefer to term the
latter, quantivalence. ...As for the alternative
expression, atomicity, its meaning, as marked by its
derivation, seems to be "indivisibility": a meaning
altogether inappropriate, otherwise than by a purely
arbitrary convention, to express the idea of chemical value
in exchange, which quantivalence, on the contrary, aptly
denotes. Atomicity is, indeed, a weak neologism into which
chemists seem to have drifted, for want of a better word,
at the time when chemical philosophy was beginning, to
undergo its great modern transformation. let us hope that
so defective a term may soon give place, by common consent,
to the more significant and appropriate term quantivalence.

63. As quoted in Crosland (1978), 344.
64. Knight (1967) discusses the various experimental and theoretical
objections to Avogadro's hypothesis. See pp. 90-94 especially.
65. ...Rather than as memoranda of the reactions of substances or as
recipes for their syntheses and analyses, an interpretation which
had been encouraged by the theory of types. Cf. Crosland (1978),
319-337, for a discussion of empirical, rational and structural
formulæ.
66. Williamson (1869), 328.
67. Ibid., 328-29.
68. Ibid., 328, 331.
69. Ibid., 350-1.
70. Ibid., 365, 338.
71. Ibid., 343.
72. Brodie (1869), 434.
73. Ibid., 440.
74. Ibid., 440.
75. Ibid., 435.
76. Ibid., 435.
77. Ibid., 435.
78. Brodie, as quoted in Knight (1967), 108.
79. See Brock and Knight (1967), 12.
80. Wright, as quoted in Brock and Knight (1967), 28.
81. Ibid., 27.
82. Mills, as quoted in Knight (1967), 124.
83. In the discussion of this below, I adopt and extend for the purposes of this case study a number of the points made by Schelar in her valuable (1966). She does not describe the two approaches as "bulk" and "particulate", nor does she consider them in -- or examine their significance for -- the broader context under discussion here. I am greatly indebted to her lucid and stimulating suggestions.
84. Merz (1904), 449.
85. Ibid., 451.
86. As quoted in Farrar (1968), 65-66.
87. Meyer, as quoted in Scott (1959), 66.
88. Mendeleev, in ibid., 66.
89. Merz (1904), 435.
90. Ibid., 436.
91. Maxwell, as quoted in Brush (1976), 196-97.
92. Wurtz, as quoted in Nye (1972), 14.
94. See, for example, Merz (1904), 435.
95. Ibid., 437.
97. Merz (1904), 440-1.
98. Van't Hoff, as quoted in Nye (1972), 18.
99. Planck, in _ibid._, 18.

100. Kelvin, as quoted in Brush (1976), 83.


102. For a discussion of this see _ibid._, 95.

103. See _ibid._, 95-96.

104. Boltzmann, as quoted in _ibid._, 632-633.


108. See this chapter, sections vi and viii.


110. Merz (1912), 165-66.

111. Schelar (1966), 123.

112. Merz (1912), 183-4.

113. Schelar (1966), 147. For further discussion of these developments, cf. her article.

114. Berthelot, as quoted in _ibid._, 121.

115. Merz (1912), 152.

116. See _ibid._, 183.

117. Ostwald, as quoted in _ibid._, 187.

118. Brush (1968), 197.

119. Ostwald, as quoted in Nye (1972), 17.

120. Schelar (1966), 123-4.
CHAPTER FIVE

THEORY PROMISE AND THEORY PURSUIT

Introduction

In Chapter One of this essay we considered an account of the normative/descriptive methodology which can be employed to enable us to arrive at normative methodological conclusions about the practice of science with the aid of descriptive, historical studies. On that account, no attempt is made to derive the former from the latter; rather, through a systematic and critically reflective reasoning procedure which integrates normative and descriptive elements (the historical/methodological loop described above), selected historical case studies are used both to generate and to revise normative principles. The criterion of selection, as we have seen, is based on the impact of the cases on the development of science. Subsequent events in the history of science must suggest that they have significantly furthered scientific inquiry and research. Newton's Principia and Dalton's New System certainly qualify as such "landmark scientific investigations." They impacted upon and contributed to subsequent work in physics and chemistry to a degree that may well be unrivalled in those disciplines. What we must now ask is what we can learn from them about the nature of actual scientific theory appraisal, and what normative proposals in this regard we might advance with their aid. The case studies of Newtonian theory and 19th century chemistry developed here draw attention to three different, and heretofore
largely neglected, features of scientific appraisal for which our normative accounts must provide.

I. Scientists typically are concerned not only with how well theories fare empirically, but with their conceptual well-foundedness. Conceptual considerations have figured significantly in the assessments scientists make of their theories. The case studies presented here have repeatedly drawn attention to this crucial dimension of theory appraisal. Newton's "De Gravitatione..." is perhaps especially instructive in this regard, for here the conceptually problematic foundations of Descartes' *Principes* are subjected to a thorough and searching critique. Cartesian theory is taken to task on a number of grounds: the vagueness and unclarity of the central concepts of that theory — most notably that of 'absolute space' — is underscored; pivotal distinctions (e.g. between two senses of 'place' and of 'motion') which are introduced are then conflated; and the commitment to an 'absolute', substantival view of space is shown to be inconsistent with the semi-relativistic theory of motion tendered.

An awareness of internal conceptual problems such as these also marked the reception of early Daltonian atomism. Dalton's use of 'atom' to refer to both simple and compound particles, for example, prompted charges that the fundamental concept of the theory was ambiguous and confused. This case, however, also provides an especially rich source documenting scientists' appreciation of, and response to, the external conceptual problems which a theory may generate. Much of the criticism directed against early Daltonian
atomism was prompted by the fact that it endorsed a theory of matter at odds with the prevailing physical theories of the time and violated the heavily inductive and empiricist methodological commitments of the contemporary chemical community. We have also seen how, later in the century, conceptual difficulties were raised for kinetic-molecular theory as a result of its conflict with classical thermodynamics.

These are only some of the respects in which the case studies examined here testify to a concern, on the part of scientists, that a theory be conceptually as well as empirically well-founded. Conceptual considerations do enter significantly into scientists' assessments of theories, yet this important dimension of theory appraisal has been largely neglected by philosophers. Our normative methodological proposals need to address more than the narrowly empirical dimensions of theory appraisal: we must find some way of accommodating at rational the appeal to conceptual as well as empirical considerations if our accounts of scientific appraisal are to be adequate.

2. Scientists typically do something more than accept or reject their theories, they pursue them. Throughout the greater part of the nineteenth century chemists worked on and developed theories in the atomic research tradition, but it was not until late in the century that such theories were able to gain something like general acceptance in the chemical community. Numerous chemists during this time, as we have seen, declared themselves willing to make use of the atomic theory in their research, but reluctant or unwilling to accept it. Like Frankland, many acknowledged "the importance of its fullest use as a
kind of ladder to assist the chemist" but "did not wish to be considered a blind believer." And frequently it was the conceptual difficulties facing the theory which were cited as reasons for non-acceptance — in particular its non-empiricist methodology and commitment to the discontinuity of matter.

If it is rational for scientists to work with only the theories they accept, then we will be forced to regard the behaviour of the chemical community throughout most of the century as irrational. Indeed, since most theories require considerable development before they can be regarded as candidates for acceptance, we will be hard-pressed to make sense of much of the history of science if theory-acceptance is taken to exhaust rationality. Our accounts of scientific rationality, then, need to recognize a second modality of scientific appraisal — theory pursuit. Our normative methodological proposals must be extended to the rationality of pursuit.

3. In defending their decisions to pursue or not to pursue a theory, scientists typically do make reference to the promise, or lack of promise, of the theory. The present inadequacies and difficulties of the theory which prevent its acceptance may be acknowledged or, as in the case of Berzelius, probed in great detail. Yet the theory may be held to be unworthy of rejection, on the basis of claims about its fertility, i.e., about how it has furthered and is likely to further research. Such assessments were repeatedly made during the nearly century-long pursuit of Atomic theory. Problematic, under steady siege, but undeniably useful and productive in guiding research.
activity, theories in the atomic research tradition were continually subjected to assessments of promise and fertility. Similar observations can be made of early kinetic-molecular theory. And, in both cases, the estimates of promise and the efforts of scientists were rewarded: the theories were developed to the point where they could command, and indeed gained, general acceptance within the relevant communities.

Appeals to the promise, or lack of promise, of a theory, then, have figured prominently in scientists' decisions regarding theory pursuitability. However, standard accounts of theory appraisal have had little or nothing to say about how one is to assess the promise of a theory, or to arrive at estimates of fertility which can be used to support judgements of pursuitability or non-pursuitability. Our normative accounts need to be enriched along these lines, to provide a way of understanding and evaluating the promise of a theory, to address the need for a criterion of appraisal which can yield estimates of a theory's fertility.

Although these dimensions of scientific appraisal have been largely neglected by traditional philosophy of science, we have seen that historical philosophers of science — and particularly problem-solving methodologies in HPS — have recognized and granted a role to them in their accounts of rational theory appraisal. In Chapter Two of this essay, some of their proposals were critically examined. They will be subjected to further criticism in this chapter, which undertakes the task of refining, extending, and, where necessary,
reformulating these contributions to provide a more adequate account of scientific theory appraisal.

I. The Conceptual Dimensions of Theory Appraisal

We considered, in Chapter One, some characteristic features of how theories are construed in HPS, and particularly by problem-solving methodologies in HPS. It is typical for research in this tradition to take very seriously the temporality, or historical nature, of scientific theories and to adopt a pragmatic, rather than a syntactic or semantic, approach to them. Theories are devices, constructed, developed and deployed by the scientists who must use them in particular contexts, to explain, or resolve empirical problems about, the natural world. Given this construal of scientific theories, it is hardly surprising that accounts of theory appraisal in HPS have, as we have seen in Chapter Two, been concerned to provide for criteria which permit us to appraise how a theory has fared over time and which might be used to support judgments regarding the rationality of theory-pursuit.

We have also seen that such accounts grant a significant role to conceptual considerations in theory appraisal. That they should do so, and that they are especially well-suited to do so, can be better appreciated in light of another characteristic feature of how theories are construed in HPS. Whether they are regarded along Thagardian lines as information processing systems, along Laudanian lines as conceptual structures which facilitate problem-solving, or along Kuhnian lines as
disciplinary matrices furnishing a scientific community with its exemplars, theories are characteristically taken to provide scientists with the conceptual resources which guide empirical research activity. The explanatory success, or empirical adequacy, which a theory may enjoy is very much a function of the conceptual resources which it makes available for the doing of science. The principal resources it supplies are an ontology and a methodology. Once theories are so construed, it is evident not only that there is a role to be played by conceptual considerations in theory appraisal, but that this dimension of theory appraisal is a fairly significant one. Indeed, as I will argue more fully below, when theories are construed in this way it becomes difficult to understand how one could go about conducting a purely empirical appraisal of a theory.

Whatever the details of the specific accounts then (theories as sets of data structures and of procedures for manipulating them, as disciplinary matrices, as problem-solving conceptual structures), there would seem to be much agreement among them regarding what scientific theories are and what they do. Scientific theories serve as conceptual repositories; they provide scientists with the conceptual resources needed to investigate and understand the natural world, to carry out and interpret empirical research. Given this view of the nature of theories, we must ask now how one is to appraise theories, so construed: How are we to go about satisfying ourselves that a theory is well-founded? to decide that it is worthy of our pursuit or acceptance? How are we (as Van't Hoff would have it) to measure a
theory by its fruits" and (as Van't Hoff did) determine whether or not it "gives the current 4% interest on capital?" 

The case studies of Chapters Three and Four suggest that when scientists appraise a theory they have two different concerns: conceptual and empirical. They make assessments of the conceptual, as well as of the empirical, well-foundedness of their theories. (The case studies also suggest that these assessments take two different forms; scientists make decisions both to pursue and to accept theories. These issues are taken up in the following section.) In appraising the conceptual well-foundedness of a theory, they address themselves primarily to the conceptual resources which the theory supplies for empirical problem-solving, raising questions about the ability the theory has demonstrated to undergo conceptual growth and refinement (in Whewell's terms, about its "explication of conceptions"), about the consistency, vagueness or ambiguity of its central concepts; about its conflict, compatibility with, or reinforcement by, other scientific theories; about the methodology which it provides. In appraising the theory's empirical well-foundedness, scientists are concerned more with its empirical yield, asking how well its conceptual resources have furthered important research; how well they have enabled us to explain the natural world; how well they have succeeded in resolving significant empirical problems.

The studies of Newtonian and of 19th century chemical theory developed above offer several examples of both of these dimensions of theory appraisal. Newton was appraising the conceptual well-
foundedness of Cartesian theory in "De Gravitatione...". There he indicated the numerous respects in which the conceptual resources of that theory were wanting: the two sub-theories of space and of motion were inconsistent; the concept of absolute space suffered greatly from vagueness and ambiguity; and important distinctions introduced (between two senses of 'place' and of 'motion') were repeatedly conflated. Nineteenth century chemists, appraising the conceptual well-foundedness of early Daltonian theory, were also able to cite a number of respects in which the conceptual resources of that theory were wanting: the concept of atom was ambiguous (Dalton had applied the term to both simple and compound particles); the theory violated the accepted definition of 'element' as well as the accepted methodology of the existing chemical community; and it endorsed a theory of matter inconsistent with the prevailing physical theories. Examples of these chemists conducting appraisals of the empirical well-foundedness of 19th century chemical theories include: Berthollet's critique of established affinity theory in the first decade of the century in which he examined the chemical reactions inconsistent with the rules of elective affinity; Davy's influential 1806 paper, in which he drew together the diverse array of previously unrelated phenomena for which his electrical theory of affinity was able to account; Berzelius' 1814 essay, where he carefully elaborated both the empirical support for Daltonian theory, as well as the empirical difficulties with which it was faced; and Williamson's 1869 address, where he details at some length "the vast body of evidence of the most various kinds, and from the most various sources" which supports his claim that the "atomic
theory is the consistent general expression of all the best known and best arranged facts of the science.⁶

In appraising a theory then, scientists do, and ought to, assess its conceptual as well as its empirical well-foundedness. We have seen something of what is involved in such assessments, of the concerns and questions that might be raised, and of the features of the theory addressed in each case. In section III of this chapter, where a criterion of fertility is developed, we will have more to say about the empirical dimensions of theory appraisal. For the remainder of this section, we will concentrate on the conceptual dimensions of theory appraisal. How does one go about deciding whether a theory is, or is not, conceptually well-founded? How can a theory demonstrate, or fail to demonstrate, that it is conceptually progressive? Larry Laudan has dealt with these issues more directly and more extensively than anyone else and, in Chapter Two, his position was subjected to a preliminary critique. That critique will be extended here and an alternative analysis — which attempts to provide for a more adequate account of the conceptual dimensions of theory appraisal — will be advanced.

We have already noted above some of the difficulties which cloud Laudan's discussion of conceptual problems.⁷ One is that he has apparently conflicting things to say about the nature of conceptual problems, and how they differ from empirical problems. He claims that conceptual and empirical problems can be arranged on a spectrum, and that there is, between the distant ends of the spectrum (on which, he observes, he has focused for heuristic reasons), a continuous shading
of problems intermediate between the straightforwardly empirical and conceptual. Yet this claim that the difference between conceptual and empirical problems is one of degree is undermined both by his characterization of them and by various comments he makes concerning them. Empirical problems are described as first order questions about the objects in the domain of a given science. Conceptual problems, by contrast, are described as characteristics of theories; they are higher order questions about the conceptual structures that do the empirical problem-solving. Moreover, he refers to them as nonempirical and notes that they have no existence independent of the theories which exhibit them, not even that limited autonomy which empirical problems sometimes possess.

While I think Laudan is correct in maintaining that "the formulation (of empirical problems) will be influenced by our theoretical commitments," I think he is mistaken in supposing that for this reason the difference between the two types of problems is one of degree (and it is the only comment I have been able to find in his discussion which might be regarded as a reason for maintaining this). I would suggest that the difference between conceptual and empirical problems is one of kind: they are, after all, about different kinds of things and they are raised in response to different kinds of demands that scientists make of theories. Conceptual problems are meta-level questions about the network of conceptual resources supplied for empirical problem-solving, and they are raised in response to the formal demands which scientists make upon their theories (e.g. that they be internally consistent, that they not conflict with other well-
established theories, that their central concepts be clear and well-explicated, etc.). Empirical problems are questions about the objects in the domain of a given science, raised in response to the empirical demands which scientists make of theories as they are deployed in research (e.g. that their solutions be precise and accurate, that they extend the range of their problem-solving abilities, etc.). A conceptual problem does not exist independently of the particular theory which exhibits it because it is a meta-level question about that particular theory; that theory is wanting with respect to some formal demand (for consistency or clarity) that has been made of it. Since an empirical problem is a question about some object or objects in the domain of a given science, it can exist independently of the particular theory which addresses it, i.e. other theories may address it as well. Whether or not a specific empirical problem does indeed enjoy such 'limited autonomy' would seem to be a function of the degree to which the central concepts of a theory enter into its formulation. For example, the empirical problem posed in a concrete fashion by Newton's bucket experiment took the form of the following question: "With what sort of motion (true and absolute or apparent and relevant) are we confronted in the bucket experiment and more generally, in similar situations of vortical motion?" This question could be addressed by both Cartesian and Newtonian theories. Similarly, early in the 19th century, questions concerning the proportions of elements in compounds and the ratios in which elements combined could be studied using either Dalton's atomic theory or Davy's electrochemical theory of affinity. However, the central empirical problem posed by Dalton's theory -- the
determination of atomic weights — could be addressed only by the former, not the latter.

I have suggested that there are at least two sorts of formal demands which scientists make upon the network of conceptual resources supplied by a theory for empirical problem-solving — demands of consistency and of clarity. A theory will be conceptually problematic if it is unable to respond satisfactorily to either of these demands. The demand for clarity will not be met when the central concepts of the theory suffer from vagueness, ambiguity, or circularity. The theory may fail to meet the demand for consistency either internally (when it is self-contradictory) or externally (when it is logically incompatible with some other well-established theory). Such conceptual problems are clearly characteristics of particular theories that arise from the failure of those theories to meet the formal demands placed upon them. Although their presence will affect a theory’s empirical problem-solving abilities, and although empirical problem-solving may contribute to their resolution (or dissolution), they are not addressed to the natural world and do not raise substantive questions about the objects in a given domain. It might be useful here to look more closely at several examples of conceptual problems drawn from the case studies above.

The internal inconsistency in Cartesian theory sprang from Descartes’ apparent commitment on the one hand to an absolute, substantival view of space and, on the other, to a semi-relativistic theory of motion which was compatible not with an absolute, but a
relational view of space. The absolute view of space which Descartes appeared to endorse, and indeed to argue for by maintaining the unintelligibility of a 'void' or bit of empty space, was one in which space was identified with the generic notion of extended substance. Descartes had claimed that there is a certain substance, extended in length, width, and depth in the world and that this extended substance is properly called the body or the substance of material things. We can have a clear and distinct idea of extended substance without thinking of any particular thing which is extended. Descartes distinguished this idea of an extension that was distinct from particular extended bodies from corporeal extension (of which we cannot have a clear and distinct idea unless we are thinking of a particular body which is extended) by calling it generic. While this generic notion of extended substance would appear to commit him to an absolute, substantival view of space, his theory of motion committed him to a relational, rather than an absolute, view of space. That theory was articulated not by reference to the concept of absolute or generic space, but in terms of a distinction between two senses of 'place'. According to it there was no point in the universe "qui soit veritablement immobile" nor is there any place of any thing in the world "qui soit ferme & arresté" except insofar as we chose a frame of reference when "nous l'arrestons en notre pensee". (Art. 13)

We have seen in Chapter Four that Dalton's theory failed to meet the formal demands for consistency and clarity, and so was conceptually problematic, in a number of respects. The concept of atom was troubled by ambiguity because Dalton had described both the separated or simple
particles that enter into chemical combination and the joined particles that result from chemical combination as 'atoms'. The term 'atom', in other words, was used ambiguously by Dalton to refer to the smallest particle of both elements and compounds. Moreover, since the latter particles were divisible and structurally arranged and since one of the central features of Dalton's New System was the one-to-one equation of the chemical elements with physical atoms which were solid, indivisible and unstructured particles, the theory was troubled by an internal inconsistency as well. This feature of Dalton's New System also generated two external inconsistencies for the theory. It implied that there were more than 30 different kinds of matter, the chemical elements, thus rendering it incompatible with the prevailing physical theory of the time which was committed to the homogeneity and internally ordered structure of matter. And by equating the chemical elements with physical atoms, it brought the theory into direct conflict with the accepted definition of 'element' according to which there was a sharp distinction between chemical elements and the "simple and indivisible atoms of which matter is composed."

The conflicting claims which Laudan makes about how conceptual problems differ from empirical problems is one respect in which Laudan's discussion of conceptual problems is wanting. Another is his virtually complete neglect of what seems to be a very important question: what relationship is there, if any, between conceptual and empirical problem-solving? Do conceptual and empirical problems interact with one another in any interesting way in the course of a theory's development to, say, secure (or undermine) its progressiveness?
Laudan's problem-solving effectiveness appraisal measure suggests -- if it does not ensure -- that they do not. Consider what we must do in applying it. Three separate calculations are made: the number and importance of solved empirical problems are added up; the number and importance of the anomalies and conceptual problems are added up; and the latter sum is subtracted from the former. There is nothing in this which indicates, or indeed permits, any interaction between conceptual and empirical problem-solving which can be seen to secure progressiveness. Does empirical problem-solving contribute in any way to the resolution of conceptual problems, and thereby to over-all theory progressiveness? Laudan's appraisal measure does not allow for this. Nor does what he has to say about how conceptual problems are resolved give us much reason for thinking this. While he addresses the issue of empirical problem solution at some length, his comments about conceptual problem solution are comparatively brief and somewhat overshadowed by his (illuminating) treatment of conceptual problem generation.

There is one passage though in which Laudan hints that there may be an interesting relationship between a theory's conceptual problems and its empirical problem-solving abilities:

It may even be true that some small measure of ambiguity is a positive bonus, since less rigorously defined theories can often be more readily applied to new domains of investigation than more rigid ones.

Conceptual problems, in other words, may actually contribute in some way to a theory's empirical problem-solving abilities, to what I have referred to above as its empirical yield. This is a nice insight
which, unfortunately, I think Laudan loses along the way. It is not just that he does nothing more with it. It is seriously at odds with his account of the role of conceptual considerations in theory appraisal. With the passage quoted above in mind, let us re-consider why this is so.

How can we, using Laudan's appraisal measure, determine whether or not the conceptual standing of a theory T has improved over some period of time, $t_1 \ldots t_n$? What we need to do, and all that we need to do, is to count up the number and importance of the conceptual problems from which T suffers at $t_1$ and at $t_n$. If the total at $t_1$ is greater than the total at $t_n$, then T's conceptual standing has improved. If the total at $t_1$ is equal to or greater than the total at $t_n$, then T's conceptual standing has either gone unchanged or has deteriorated over this interval. When scientists appraise the conceptual dimensions of their theories then, they need do no more than this. The improved conceptual standing which a theory may acquire over time is a function, and is only a function, of the reduction it has achieved in the number and importance of conceptual problems from which it suffers. There is a sense in which, for Laudan, conceptual problems are blemishes, warts on the theory which need to be removed if the theory is to improve its conceptual standing. This is, moreover, the only way in which a theory can improve conceptually and increase, thereby, its progressiveness.

Laudan's problem-solving effectiveness appraisal measure does not, and cannot, provide for any sense in which "some small measure of ambiguity" might count as a "positive bonus" for the theory. Such
ambiguity counts against the theory at \( t_1 \) and, if it has not been eliminated, at \( t_n \) as well. It is true that if the theory is "applied to new domains of investigation" the empirical component of his appraisal measure will capture this achievement. But it is also true that the appraisal measure isolates this achievement as a purely empirical one, which in no way reflects positively on the conceptual resources of the theory. It offers no way of tying an increase in the theory's empirical yield to (or of seeing such an increase as the result of) the ambiguity of theoretical concepts at \( t_1 \). Although Laudan, in this passage, appears to acknowledge some positive role for conceptual considerations in theory appraisal, his appraisal measure can grant them no more than a negative role -- the only way in which a theory can improve its conceptual credentials is by reducing the respects in which it is conceptually problematic. The conceptual achievements of a theory can be no more than a matter of 'wart' elimination. Not only does his appraisal measure provide no way of recognizing that a theory's conceptual problems may affect its empirical problem-solving abilities, it seems to suggest that the conceptual and empirical dimensions of theory appraisal are quite independent of one another.

The analysis of the conceptual dimensions of theory appraisal which I am proposing here as an alternative to Laudan's takes its cue from Laudan's work but departs from it in several substantial ways. My main criticism of Laudan's analysis has been that it provides an inadequate account of the role of conceptual considerations in theory development and in theory appraisal. All that his appraisal measure
can and does reflect by way of the conceptual improvement which theories may enjoy as they are developed is the successful resolution or decrease in number and importance of the conceptual problems which trouble them. And it seems to me that if the case studies explored above are any indication, the story here—both about the conceptual development which theories undergo and about how scientists (are to) appraise this—is far more complex. There are a number of different points to be made here.

The first is that I am in only qualified agreement with Laudan’s claim that, when a theory is successful in eliminating or reducing the conceptual problems from which it has suffered, it has thereby demonstrated conceptual improvement and shown that it is (at least conceptually) progressive. The reason for the qualification has to do with a point already noted above.\textsuperscript{12} Laudan regards the former as a sufficient condition for the latter, and I do not. Laudan’s problemsolving effectiveness appraisal measure, or mini-max principle, is a purely quantitative criterion of appraisal. The synchronic determinations it would have us make at $t_1$ and $t_n$ reflect only the change in the number and importance of the conceptual and empirical problems the theory has managed to solve, not how that change has come about. It asks, in other words, "how many?" without also asking "how?" or "how well?" There are a variety of different ways in which a theory might actually eliminate or reduce its conceptual problems, some of which should at least make us question whether or not the reduction thereby achieved is appropriately regarded as an improvement in the conceptual standing of the theory, whether it really contributes to the
theory's (conceptual) progressiveness. Arbitrary or ad hoc conceptual adjustments, for example, might be made, or certain empirical problem-solving abilities of the theory might be sacrificed to effect such a reduction in conceptual problems. The theory's consilience or fertility might be compromised; that "small measure of ambiguity" which enabled the theory to "be more readily applied to new domains of investigation," might be given up. Or the theory might refuse to appropriate the conceptual resources of other valuable, but conceptually troubled, theories. Latter-Day atomic theories, for instance, might have, in view of the conceptually problematic nature of early kinetic-molecular theory, failed to draw upon the valuable resources it supplied for chemical research. By appropriating these resources, they acquired a new and weighty conceptual problem. What I want to emphasize here is this. We cannot conclude, from the fact that a theory has managed to decrease its conceptual problems over some interval of time, that it has thereby bettered its conceptual credentials or contributed to over-all theory progressiveness. If we are adequately to appraise the conceptual standing of a theory, and how this may have altered over time, we need to know more than the simple fact that the theory has succeeded in reducing its conceptual problems. We must consider as well how this change has come about. And for that we require a qualitative criterion of appraisal, such as that of conceptual viability proposed below.

There is a second, related point to be made here concerning the conceptual development which theories undergo, and how we are to appraise this. The case studies explored above suggest that theories
undergo conceptual growth and refinement as, and partly in response to the way in which, they undergo empirical growth and refinement. They become more resilient, they are fine-tuned conceptually, they may align themselves with, and appropriate, the conceptual resources of other 'friendly' theories. In short, there are a number of different ways in which they may enhance the conceptual resources they provide for empirical problem-solving (and of course a number of different benefits may thereby be secured for their empirical problem-solving abilities ... valuable extensions to other domains may result, entirely new areas of research may be opened up, and so on). But as they do all of this, they may well generate new conceptual problems, not present at the earlier stages of their careers. This is just what happened in the case of 19th-century atomic theories. Some of the conceptual problems from which early 19th-century atomic theories suffered had been resolved by the time; later in the century (circa 1875), they began to align themselves with, and to appropriate the conceptual resources of, kinetic-molecular theories in physics. Yet as they did this they became heir to the conceptual problems plaguing those theories — notably the conflict with classical thermodynamics. There is an important sense in which they underwent conceptual improvement; although they continued to be conceptually troubled.

Laudan's appraisal measure however, is unable to capture such conceptual improvement. As a quantitative criterion, it addresses only the number and weight of empirical problems the theory has solved, and anomalies and conceptual problems it has left unsolved. Consequently, it is unable to reflect the dynamics of conceptual development: in a
'curious sort of way, it 'looses' the actual history of the theory. If the number and weight of conceptual problems at \( t_1 \) (say 1810) equals that at \( t_1 \) (say 1875), then we would have to say that, so far as conceptual development was concerned, theories in the atomic research tradition remained unchanged. The conceptual dimension of theory appraisal is reduced to a matter of counting up the wartish conceptual problems which disfigure the theory at \( t_1 \) and of subtracting therefrom the number of those which disfigure it at \( t_n \). Yet, as the case studies suggest, scientists, in appraising theories, typically are sensitive to, and concerned to assess, the kind of conceptual growth and refinement they have undergone as well as whether, and how, this has affected their empirical abilities in significant ways.

Let me return to some observations made earlier regarding the conceptual development to which theories are subject. Theories, it was noted, undergo conceptual growth (this includes the refinement of concepts, the resolution of existing conceptual problems, and the generation of new ones) as, and partly in response to the way in which they undergo empirical growth. The conceptual and empirical dimensions of a theory are thus intimately bound up with one another: in order to solve empirical problems, the conceptual resources of a theory must be drawn upon; as the theory is deployed in empirical problem-solving, its conceptual resources are refined and modified (i.e., old conceptual problems may be put to rest, new ones may take their place). It was also noted that there are several different ways in which theories may enhance the conceptual resources which they provide for empirical problem-solving. They may become more consilient; their central
concepts may be 'fine-tuned' or, in Whewellian terms, explicated; they may align themselves with, and appropriate the conceptual resources of, other theories. As theories undergo conceptual development in these ways they may succeed in resolving some of their old conceptual problems, but they may also generate new ones. (For example, as the ambiguity of a theory's concepts is lessened it may become clearer that the theory is in conflict with some other established theory. Or, as happened in the case of Latter-Day Agnosticism, when a theory aligns itself with, and appropriates the conceptual resources of a theory in a different domain, it may inherit that theory's conceptual problems.)

If we are adequately to appraise this sort of conceptual development, a qualitative criterion such as conceptual viability is needed. The degree of conceptual viability of a theory is a function of the capacity it has displayed to undergo conceptual growth and refinement, to enhance its conceptual resources in any or all of the above ways. Conceptual viability is one of the features of a theory which we need to appraise to provide an optimal estimate of the theory's fertility. (Other features of the theory must be consulted as well to arrive at such an estimate. These are considered in the discussion of fertility below, section III.) It should be clear that a theory may demonstrate conceptual viability over some interval of time t₁ ... tₙ, even though it may not have succeeded in reducing, by tₙ, the number and importance of its conceptual problems. The reason for this is that it may well have generated new ones during the interval. What it must have done, however, if it is to justify its claim to being a conceptually viable theory, is to have succeeded in resolving, or made
noticeable progress towards the resolution of some of the conceptual problems which troubled it at \( t_1 \). The criterion might be stated somewhat more formally as follows: Let \( T \) be a theory and let \( CP \) be a set of conceptual problems generated by \( T \). \( T \) demonstrates conceptual viability over \( t_1 \ldots t_n \) if and only if some of the elements of the \( CP \) at \( t_1 \) are not elements of the \( CP \) at \( t_n \) (i.e. \( T \) has resolved some of its conceptual problems over this interval) or the severity or weight of some of the elements of the \( CP \) at \( t_1 \) has decreased by \( t_n \) (i.e. \( T \) has managed to reduce, by \( t_n \), the severity of some of the conceptual problems which troubled it at \( t_1 \)).\(^{13}\) This more formal statement of the criterion has one short-coming: it presents the notion of conceptual viability solely by reference to the conceptual problems generated and resolved by the theory. The criterion can be understood in these terms and it may be useful to have this more precise statement of it in hand. However, the description given of conceptual viability in the opening of this paragraph provides a richer, if somewhat less precise, formulation of the criterion by making reference to the ways in which a theory may undergo conceptual growth and refinement and thereby enhance the conceptual resources which it supplies empirical problem-solving. I would like now to examine more closely some of the ways in which a theory may demonstrate conceptual viability -- through the fine-tuning or explication of its concepts, through the achievement of greater consilience, and through the appropriation of the conceptual resources of theories in other domains.

The process of fine-tuning or explicating concepts is as central to the conceptual development of a theory as that of securing precise
and accurate solutions to empirical problems is to a theory's empirical development. Whether it is quickly achieved, as it was in the case of absolute motion and space in Newtonian theory, or is drawn out over many years, as it was in the case of the concepts of affinity and atom in chemical theory, it is, and is regarded by scientists as, a desideratum -- primarily for the clarity which it yields. When it is lacking or slow in coming, as it was in the case of 19th century atomic theory, it can be the occasion for great frustration and confusion among scientists and one of the main barriers to theory acceptance. The Atomic Debates of 1869, and the controversy and near-exasperation which marked them, are an excellent example of this. After some 60 years of fairly extensive use of the atomic theory in chemical research, chemists not only found it necessary to convene a congress for the purpose of defining "important chemical ideas, such as those expressed by the words: atom, molecule..." but they had to adjourn without obtaining any substantial agreement. Perhaps what emerges most clearly from the lively, often heated, discussion reported in the Chemical Society Journal is that, while a good number of these chemists recognized the empirical abilities of the atomic theory and were willing to employ it in their research, they found themselves unable to accept it on conceptual grounds -- one of these being, in Mills' words, the confusion generated by "the use of a phrase to which no clear definition has been ... attached." 

Yet, as Whewell has noted:

discussions and speculations concerning the import of very abstract and general terms and notions may be, and in reality have been, far from useless and barren ... the progress of a science depends upon such disputes and
speculations as give clearness and generality to its elementary conceptions ... the distinctness of Conceptions ... is a real requisite of scientific progress.... The Conceptions must be, as it were, carefully unfolded.... This is one of the processes by which our knowledge is extended and made more exact; and this I shall describe as the Explication of Conceptions.\textsuperscript{16}

In the case of atomic theory, the clarity which results from the explication of concepts was slowly and painfully won: the ambiguous, solid, billiard-ball chemical atoms of Dalitagen theory were replaced by the complex, ‘structured’ molecules and atoms of Latter-Day atomic theory.\textsuperscript{17} This achievement, as we have seen in the case study above,\textsuperscript{17} was secured largely through the use of the theory in empirical research, especially in organic chemistry. The contribution which empirical problem-solving may make to the fine-tuning or explication of concepts can be seen in another instance above.\textsuperscript{18} Research by chemists in the last quarter of the 18th century, undertaken in an attempt to reconcile the rules of elective affinity with many reactions that were inconsistent with them, led only to further and apparently irreconcilable inconsistencies. Surveying these results in his critique of established affinity theory, Berthollet concluded that, since it was necessary to take into account the relative proportions of the reactants, the concept of elective affinity could no longer be regarded as it had been — as an invariable, uniform force analogous to gravitation.

Concepts may be fine-tuned or explicated by other means. Whewell mentions the use of definitions, propositions and axioms:

\textit{a right definition of a Term may be a useful step in the explication of our conceptions; but this will be the case only when we have under our consideration some Proposition in which the Term is employed. For then the question}
really is, how the Conception shall be understood and defined in order, that the Proposition may be true.... To unfold our Conceptions by this means of Definitions has never been serviceable to science, except when it has been associated with an immediate use of the Definitions ... (we may also proceed by means of Axioms), stated in addition to, or in preference to, Definitions.

And he stresses how, by these means, we may achieve the desired clarity of concepts:

The Explication of Conceptions ... is the process by which we bring the clearness of our Ideas to bear upon the Formation of our Knowledge. And this is done ... not always, nor generally, nor principally, by laying down a Definition of the Conception; but by acquiring such a possession of it in our minds as enables, indeed compels us, to admit, along with the Conception, all the Axioms and Principles which it necessarily implies, and by which it produces its effect upon our reasonings ... the person shall see the necessity of the Axioms belonging to each Idea; — shall accept them in such a manner as to perceive the cogency of the reasonings founded upon them.

Newton's explication of the concepts of absolute space and motion provides a classic example of a scientist securing clarity for the central concepts of a theory by proceeding in these ways: Rather than define the concepts of absolute space and motion, in the scholium, he offers a number of propositions in which these terms are used: "The effects which distinguish absolute from relative motion are the forces of receding from the axis of circular motion."; "Absolute motion is the translation of a body from one absolute place into another."; "Absolute space ... remains always similar and immovable ... no other places are immovable but those that ... do thereby constitute immovable space,"

We have also seen how his methodology of classical empiricism supplied what Whewell refers to as "ready exemplifications ... of that which is to be proved or disproved concerning the ideas." 21 The bucket and rotating globes experiments are used to exemplify, or illustrate the
existence of, absolute motion and consequently (given the theoretical equivalence set up between 'suffering the effects of the forces of receding from the axis of circular motion,' 'undergoing absolute motion,' and 'translation with respect to absolute space') of absolute space itself. The conceptually problematic nature of Newtonian theory had to do not with doubts about the clarity of its central concepts, but with doubts about its methodological well-foundedness.

There is an example of yet another means by which the explicating or fine-tuning of concepts may come about, in Chapter Four. When a theory aligns itself with, and appropriates the resources of a theory in another domain, it may not only enhance the conceptual resources which it provides for empirical problem-solving (as happened when Latter-Day atomists began to draw on kinetic-molecular theory and apply it in chemical research), but also further the explication of its concepts. The application of thermodynamics to thermochemical theories of heat is an instance of this. It led to the clarification of a thermodynamic concept of affinity in terms of free energy: affinity was regarded as a measure of whether or not a certain reaction would proceed.

The process of explicating or fine-tuning a theory's concepts is obviously one means by which a theory may resolve its conceptual problems. This includes not only internal conceptual problems such as ambiguity, vagueness, and inconsistency but external conceptual problems as well, such as conflicts with other, well-established theories. Boltzmann's clarification of the ambiguous concept of
entropy, for example, permitted a resolution of the 'reversibility paradox' and thereby helped to reduce the conflict between kinetic-molecular theory and thermodynamics. A second means by which a theory may resolve its conceptual problems, and demonstrate conceptual viability, is available however in the achievement of greater consilience. I turn now to a consideration of this.

I have been arguing that a criterion of conceptual viability is needed if we are to provide a more adequate account of the role of conceptual considerations in theory appraisal. I have suggested that the explication or fine-tuning of its concepts is one type of development a theory may undergo which results in an improvement in its conceptual standing and so testifies to its viability. But in what way is the achievement of greater consilience relevant to an appraisal of the conceptual well-foundedness of a theory? Clearly it is relevant to assessments of a theory's empirical well-foundedness. The achievement of greater consilience indicates that the range of the theory's problem-solving abilities has been successfully extended to new kinds of phenomena, that the theory has improved its ability to explain the natural world by extending itself to new classes of facts. And, in section III below, I will make dynamic consilience as one of several indices to be consulted in evaluating theory fertility. At present however, I want to draw attention to the respect in which the achievement of greater consilience can be regarded as a conceptual achievement as well, one which reflects favourably on the structure that does the empirical problem-solving. A theory which has, in the course of its development, become increasingly consilient, has
succeeded in enhancing the conceptual resources which it supplies for empirical problem-solving in a very effective way.

To see this, consider what results for a theory when it manages to increase consilience. Whereas the explication of concepts is valued primarily for the clarity which it yields, the achievement of greater consilience is valued primarily for the simplicity and increased generality which it provides. In the following passage, Whewell describes the way in which consilience contributes to the simplification of the theory:

... all the additional suppositions tend to simplicity and harmony; the new suppositions resolve themselves into the old ones, or at least require only some easy modification of the hypothesis first assumed: the system becomes more coherent as it is further extended. The elements which we require for explaining a new class of facts are already contained in our system. Different members of the theory run together, and we have thus a constant convergence to unity.

The Consilences of our Inductions give rise to a constant Convergence of our Theory towards Simplicity and Unity.... These two Characters are, in fact, hardly different; they are exemplified by the same cases. For if these Inductions, collected from one class of facts, supply an unexpected explanation of a new class ... there will be no need for new machinery in the hypothesis to apply it to the newly-contemplated facts; and thus we have a case in which the system does not become more complex when its application is extended to a wider field....

He then goes on to suggest that consilience secures not only simplicity for the theory but greater generality as well:

... both these cases of the extension of the theory ... may be conveniently considered in yet another point of view; namely as successive steps by which we gradually ascend ... to a higher and higher point of generality. For when the theory, either by the concurrence of two indications, or by an extension without complication, has included a new range of phenomena, we have, in fact, a new induction of a more general kind, to which the inductions formerly obtained are subordinate, as particular cases to a general proposition.
We have in such examples, in short, an instance of successive generalization.

On this Whewellian account of consilience, a theory which achieves greater consilience by extending its empirical problem-solving abilities to new domains, or by explicating new classes of facts, does so without increasing its complexity. It is important to stress this since a theory might extend its empirical problem-solving abilities in this way (or, for that matter, solve an anomalous problem) through ad hoc maneuvers, i.e., by making assumptions which do not receive further empirical support as the theory is developed or which do not serve to uncover new facts which they help to explain. The theory might, that is, increase its empirical yield at the cost of introducing greater complexity into its network of conceptual resources, into the structure which does the empirical problem-solving. As Whewell observes:

The new suppositions are something altogether additional; -- not suggested by the original scheme, perhaps difficult to reconcile with it. Every such addition adds to the complexity of the hypothetical system, which at last become unmanageable, and is compelled to surrender its place to some simpler explanation.

Such a theory may increase its empirical problem-solving abilities, but it has not, on the account I am urging here, increased its consilience.

It may be instructive to contrast my position here with that of Laudan. Laudan has some interesting comments to make on the subject of adhocness, but once again I find myself in no more than a qualified agreement with him and am forced to conclude that his account of the role of conceptual considerations in theory appraisal is an inadequate one. Laudan is concerned to counter the traditionally pejorative
connotations which afflict the notion of 'ad hocness'. He argues that ad hoc modifications are, by their very definition, empirically progressive. His claim is not that ad hoc theories are invariably better than non-ad hoc ones, but that an ad hoc theory is preferable to its non-ad hoc predecessor (which was confronted with known anomalies). To believe otherwise is to deny a vital aspect of the problem-solving character of scientific inquiry. ... The detractors of ad hocness have yet to show that the emendation of a theory to preserve its problem-solving capacity and to save it from an anomaly requires any less theoretical imagination or serendipity than the construction of a new theory from scratch. And he adds an historical worry to this philosophical one:

Those modern philosophers and scientists who wish to make ad hocness a debilitating handicap for any theory which exhibits it must explain why the most "successful" theories of the past (his examples include Newtonian mechanics, Daltonian atomism, and Darwinian evolution) were also highly ad hoc.

But he does espy a grain of truth in the concerns raised about ad hocness, and suggests that to locate it we must direct our attention to the conceptual rather than the empirical level. He suggests that the empirical gains of an ad hoc theory may be more than offset by its conceptual losses. The ad hoc modifications made might generate acute internal or external conceptual problems, such as internal inconsistency or conflict with other acceptable theories. The result would be an over-all reduction in the theory's problem-solving effectiveness:

I am sympathetic to the suggestion that an ad hoc modification can be a virtue rather than a vice in that it increases the empirical yield or progressiveness of the theory. But, as I have already
indicated, such a move also increases the theory's complexity, and this is a conceptual loss which Laudan's account is unable to reflect. The modification introduced may not generate any acute conceptual problems such as internal or external inconsistencies. It may not introduce any of the conceptual problems that Laudan recognizes at all. But it will introduce greater complexity, and the conceptual component of Laudan's appraisal measure cannot register this as any sort of a 'loss' which detracts from the theory, or weakens the theory's conceptual standing in any way. Similarly, it is unable to register an increase in consilience as a conceptual 'gain' for the theory, reflecting an improvement in its conceptual (not only in its empirical) standing and indicating that the conceptual resources which it supplies for empirical problem-solving have been enhanced in a significant way.

As for Laudan's historical worry, I would suggest that what is so impressive about, and one of the great virtues of, theories such as Newtonian mechanics and Daltonian atomism (et al.) is the increased consilience which they achieved. And, once again, this stands not only to their empirical, but to their conceptual, credit: it was an achievement which introduced greater simplicity and generality into the available explanations of the natural world. Newton was able to show that three different, and theretofore isolated, phenomena were of a similar kind: the motions of bodies in the solar system, of the tides, and of balls rolling down inclined planes were mechanical in nature, operating under the force of gravity. The conceptual viability of Daltonian (i.e. pre-Latter-Day) atomism was due not to any success in the explication or fine-tuning of its concepts (Daltonian atomism, as
we have seen, was remarkably unsuccessful in this), but to the achievement of greater consilience. Early Daltonian theory was of unique value, in that it introduced greater simplicity and generality into the explanation of weight relations in chemical combination: it was able to explain two laws already known -- the law of definite proportions (a compound contains fixed proportions by weight of its constituents) and the law of equivalent proportions (the ratio of the weights of A and B which react with a given amount of substance C is independent of C) -- and to extend this explanation to the law of multiple proportions (the weights of element A which combine with a fixed weight of element B are in the ratio of small whole numbers) advanced by Dalton in 1804. Daltonian atomism moreover, became increasingly consilient as it was employed in empirical research over the first half of the century, leading Williamson to praise it as:

\[ a \] simple theory which explains in a consistent manner the most general results of accurate observation in chemistry ... daily being extended and consolidated by discovery of new facts which range themselves naturally under it.\]

However the most striking gains in consilience for theories in the atomic research tradition came with the achievements of Latter-Day atomism. Latter-Day atomic theories extended themselves to new domains, addressing not only weight relations, but fulfilling Wollaston's 'prophecy' by providing "a geometrical conception of the arrangement of the elementary particles in all the three dimensions of solid extension" as the result of research in structural and stereochemistry. They were also able, unlike their Daltonian predecessors, to offer an account of chemical processes, by aligning
themselves with, and appropriating the conceptual resources of, kinetic-molecular theories. I have noted above that such appropriation is another means by which a theory may demonstrate its conceptual viability, but it is possible to regard it as a special, and particularly impressive, case of the achievement of greater consilience, since this is in fact what often results. Both of these processes too, like that of explicating concepts, serve as means by which a theory may resolve its conceptual problems. When Latter-Day atomic theories successfully uncovered and provided explanations for new phenomena in the field of organic chemistry, and appropriated the resources of kinetic-molecular theory for chemical research, a number of the most vexing internal and external conceptual problems of Daltonian atomism were laid to rest.

It should be evident in the foregoing that, on the analysis of the conceptual dimensions of theory appraisal proposed here, a crucial role is played by empirical research in the resolution of the conceptual problems troubling a theory, and so in the improvement in the theory's conceptual standing. Since conceptual problems are metalevel questions or doubts raised about the structures that are used to answer empirical questions about the objects in a given domain, it is appropriate that, in the actual use of these structures in empirical problem-solving, our doubts and questions about them may be eased — or exacerbated. Perhaps the most impressive example of the former, in the case studies above, is the use of the kinetic-molecular theory to solve the empirical problem of Brownian motion. This empirical achievement eased the higher-level doubts that had been raised about kinetic-
molecular theory — namely its apparent inconsistency with the Second Law of thermodynamics. In effect, it dissolved this external conceptual problem by demonstrating that the Second law was only statistically, not absolutely valid as had been supposed. As for the latter — the exacerbation of higher-level doubts through lower-level empirical problem-solving, a nice example is provided by Berzelius' work on the metallic oxides. His research revealed that the formulae of certain metallic oxides indicated the presence of half-atoms of oxygen. Given Daltonian claims about the indivisibility of atoms, and the ambiguity surrounding the term, the "difficulté de concevoir un demi-atom" only added to the growing confusion at the time regarding the central concept of Daltonian theory.

The point made in the preceding paragraph can be stated somewhat differently. I have claimed that, in appraising the conceptual well-foundedness of a theory, scientists address themselves primarily to the conceptual resources which the theory supplies for empirical problem-solving, raising questions about the ability a theory has demonstrated to undergo conceptual growth and refinement, i.e., about its conceptual viability. And I have argued that a theory may demonstrate its conceptual viability in at least three ways: through the explication or fine-tuning of its concepts, through the achievement of greater consilience, and through the appropriation of the conceptual resources of theories in other domains. A theory cannot hope to display conceptual viability unless it is actually used by scientists to guide and direct their research activity, unless scientists draw upon the network of conceptual resources which it supplies for investigating the
natural world. In the examples provided, theories actually demonstrating conceptual viability in each of these three ways, we have seen that the role of empirical research in securing or contributing to such viability has been a crucial one. This was the case even where it might least have been expected — in the explication of concepts through the use of definitions, propositions, and axioms. Axioms are, themselves empirical laws and the definitions supplied must, if they are to be "serviceable to science" be used in propositions. These propositions are, in turn, exemplified, or illustrated, by phenomena. And we have seen Newton using the bucket experiment to do just this.

We have also seen another example of the interaction between conceptual and empirical problem-solving, and the way in which such interaction may contribute to a theory's progressiveness, in the Newtonian case study. Appreciation of the conceptual problems which trouble a predecessor theory, and which limit or thwart its ability to present adequate solutions to the empirical problems in its domain, may serve to clarify precisely what conceptual resources a successor theory will require if it is satisfactorily to resolve these empirical problems. Well-versed in the conceptually problematic Cartesian theory, Newton's critique of it in "De Gravitatione..." was an early formulation of the more theoretically mature one provided in the scholium to the Principia. His careful appraisal of that theory led him to conclude that a successor to it would need to define 'true and philosophical' motion from considerations other than translation. In the scholium, Newton 'anchors' his account of motion on absolute space and is able successfully to resolve the empirical problem of vortical
motion while avoiding the internal conceptual problems generated by the Cartesian 'solution'. The fundamental conceptual inconsistency in Cartesian theory wherein the two sub-theories of space and of motion conflict was, as we have seen, avoided by Newtonian theory wherein the two sub-theories not only complement, but provide support for -- as illustrations of -- one another.

Let me conclude this section with a few further comments regarding how the analysis of the conceptual dimensions of theory appraisal proposed here differs from that of Laudan. I have wanted to stress that the difference between conceptual and empirical problems is a difference of kind, not of degree. In keeping with a pragmatic view of theories, I have focused not so much on what theories are as on what they do. And what a theory does is to supply scientists with a network of conceptual resources which guides and directs their research, and with which they investigate the natural world. In appraising the conceptual well-foundedness of theories, scientists raise meta-level questions about the conceptual resources supplied for investigating the natural world, about how well the theory has been able to undergo conceptual growth and refinement as it has been deployed in -- and in response to -- empirical research. In appraising the empirical well-foundedness of theories, scientists raise questions about the empirical yield of the theory, about how well the resources supplied have met the demands made by the natural world, and empirical research in it. Empirical problems are lower-level questions about the natural world which must be answered by deploying the conceptual resources of a theory in empirical research. Conceptual problems are higher-level
questions about the network of conceptual resources supplied to meet the demands of empirical research, about the conceptual structures that must do the empirical problem-solving. To raise the lower-level questions, scientists must to some degree draw upon the conceptual resources of the theory, and to answer the higher-level questions they must to some degree draw upon empirical research conducted by means of the theory. These two sorts of questions differ not so much in the means by which they are answered, but in what they are a response to. Lower-level empirical questions are raised in response to the demands which the natural world make upon a theory. Higher-level conceptual questions are raised in response to the demands which scientists make upon the conceptual resources supplied by a theory for investigating the natural world -- demands that the theory be consistent, that its central concepts be clear and well-explicated, that the theory increase its consilience and so introduce greater simplicity and generality into scientific explanations of the natural world.

Whether empirical or conceptual, such problems are obstacles which the theory must overcome if it is to function effectively and adequately. So I am in agreement with Laudan that conceptual problems, per se, are 'wartish'; they are blemishes on the theory which need to be eliminated. And, perhaps unlike Laudan, I am willing to grant this even of that "small measure of ambiguity" which may serve as a "positive bonus" for the theory in the long run. But the suggestions I have made here about the way in which theories develop conceptually, and the appraisals to be made of the way in which their conceptual resources have been deployed and developed over time, do not commit me
as (I have argued) Laudan's appraisal measure commits him, to saying that the only way a theory can demonstrate (conceptual) progressiveness is by freezing off its warts or eliminating its blemishes. If we appraise the conceptual viability a theory has displayed over the course of its development, it seems to me we are better able to accommodate both a kind of assessment which scientists typically make of their theories and a kind of development which theories actually undergo. So, while even a small measure of ambiguity is, per se, wartish, what we are interested in when we appraise theories is how (and whether) this ambiguity has contributed to, or hindered, theory growth in significant ways.

II. The Pursuit of Theories

We have begun to consider what it might be to, in Van't Hoff's words, "measure a theory by its fruits" and determine whether or not it "gives the current 4% interest on capital." Part of what we need to do is to appraise the theory's conceptual viability. Conceptual viability is one of several indices of theory fertility, a feature of the theory we need to assess if we are to determine whether or not the theory is promising. This last will be discussed more fully in the third section of this chapter. For the present, I will consider not how promise is to be determined, but what it is for a theory to be promising. When Van't Hoff measured the kinetic-molecular theory by its fruits in the 1880's, he judged that it barely gave the then current 4% interest rate, and so was an unpromising theory, worthy of pursuit. When Frankland (and a good number of other 19th-century chemists) declared
that he did not want to be considered a "blind believer" in the atomic theory and could not "accept it as true," but that he had been — and planned to continue — using it "as a useful ladder," he had arrived at a decision shared by many of his colleagues that while the theory was not acceptable, it was promising and worthy of pursuit. "Scientific estimations of promise or lack of promise lead to scientific decisions to pursue or not to pursue theories. What is it to pursue a theory? What happens to a theory when it is pursued? And what, if anything, happens to scientists and the scientific community? These are some of the questions examined in this section.

Let us look first at some cases of theory pursuit drawn from Chapter Four: Berthollet's pursuit of affinity theory; and Berzelius and Wollaston's initial pursuits of Daltonian atomism. I have already noted\(^35\) that Berthollet's critical review of the basis and results of established affinity theory, during the first decade of the 19th-century, opened with a declaration of allegiance to the positive (Newtonian) heuristic which had guided the 18th-century affinitists, according to which the forces of affinity were regarded as analogous to gravitational force. Yet his examination of the results of that theory, including the apparently irreconcilable inconsistencies of many chemical reactions with the rules of elective affinity, suggested that this positive heuristic had been played out. His work was clearly within the affinitist tradition, the aim of it being to show that "the chemical action of bodies ... does not depend upon their affinity exclusively, but also on their quantity" and to replace Bergman's determinations of affinities with a better method. What he wanted to
do was to reorient affinity theory. (And, thanks to the advent of the galvanic pile, it was reoriented — or galvanized — by its newly acquired positive heuristic.)

Berzelius, as we have seen, was instrumental in furthering the empirical problem-solving ability of the atomic theory: he considerably advanced experimental techniques and set new high standards of accuracy and comprehensiveness with his 1814 publication of an extensive set of atomic weights. However, we have also seen that in that and the subsequent year he presented a detailed review of its empirical and conceptual shortcomings. He stressed that it was not his intention to refute the atomic theory; and that it would be rash to conclude that we shall not be able hereafter to explain these apparent anomalies in a satisfactory manner. His intent was to lay open all the difficulties of that hypothesis that nothing might escape our attention calculated to throw light on the subject. 36

Wollaston's work on the oxalates was also a valuable contribution to the empirical problem-solving ability of early Daltonian atomism. But in the same paper in which he published his results on the oxalates, he expressed the need for an atomic geometry (what many commentators have referred to as his 'prophesy'). By doing so, he was fingerling an important inadequacy in Daltonian theory, one perceived by a number of his contemporaries. In order to arrive at a calculation of atomic weights from empirical data on combining weights, Dalton had proposed his rule of greatest simplicity, 37 justifying it by appeal to the "mutual repulsion of atoms among themselves." The resulting
diverse geometrical configurations of atoms were criticized as fanciful since experiment could not determine whether a given compound were really 'binary' or 'ternary' -- as Wollaston would point out later, having abandoned a year-long project to develop an atomic geometry and, with that project, any further serious pursuit of Daltonian atomism.

The cases of Berthollet, Perzelius and Wollaston may take us some way towards answering the three questions raised above concerning what scientists do when they pursue theories, what happens to theories as they are pursued, and what happens to scientists when they pursue theories. One of the things that happens to scientists is that their research and problem interests are shaped by, as they help to shape or develop, the theories on which they are working. The theories undergo development in accordance with the research and problem interests of the scientists, and this development is both empirical and conceptual. So there are at least two things that happen to theories: the first is that their empirical problem-solving abilities are critically probed and refined; the second is that the conceptual problems of the theory may be addressed and efforts made to 'treat' them. Since it is the work of scientists which is responsible for these developments of the theory, these are also the two things which scientists do when they pursue theories. Before moving on to examine each of these responses to the three questions posed at the outset, we might pause to see how they are exemplified by the cases.

Turning first to how scientists' problem interests are shaped by, as they help to shape or develop, the theories they work on, consider
Wollaston and Berzelius' pursuits of early Daltonian atomism. Dalton's theory, as we have noted, was valuable in explaining the weight relations in chemical combinations. His chemical atoms inspired a lengthy programme of weight determinations: they served — with the assistance of the balance and the improvement in analytical techniques — as readily quantifiable chemical units. Recall Davy's comments, in tribute to Dalton in 1826:

With respect to the weight or quantity in which the different elementary substances entered into union to form compounds, there was scarcely any distinct or accurate data. Persons whose names had high authority differed considerably in their statements of results; and statistical chemistry, as it was taught in 1799, was obscure, vague and indefinite, not meriting the name of a science. To Mr. Dalton belongs the distinction of first unequivocally calling the attention of philosophers to this important subject ... thus making the statics of chemistry depend upon simple questions in subtraction and multiplication, and enabling the student 'to deduce an immense number of facts from a few well-authenticated, accurate, experimental results."

The extensive set of atomic weights produced by Berzelius and Wollaston's early research on the oxalates, as well as the latter's year-long project to develop an atomic geometry, clearly indicate that the problem interests of these chemists not only had been shaped by, but contributed to the development of Daltonian theory. Moreover, when they did abandon their pursuit of the theory, their problem interests were modified without being wholly abandoned. Wollaston would continue to study the proportions of elements in compounds and the ratios in which elements combined — producing a "synoptic scale of equivalents" which allowed chemists to draw upon the valuable empirical laws of definite, equivalent and multiple proportions without implying any commitment on their part to Dalton's mechanistic atomism. This also
reflected the failure of his year-long attempt to develop an atomic geometry which could improve upon Dalton's inadequate and empirically unsupported account of the simple geometrical and mechanistic considerations explaining why "atoms combine only in certain proportions." Berzelius too came to regard as inadequate the 'mechanism' of chemical combination proposed by Dalton. He thought it important to "combine researches respecting the cause why atoms combine with researches into the cause why they combine only in certain proportions." In the dualistic electrochemical theory of affinity which he developed, the role played by individual atoms was an important one. But the theory was an attempt to provide for what Dalton's theory had neglected, and what Berzelius had come to regard as the essential factor to be considered in the search for an explanation of chemical phenomena -- the powers or forces associated with individual particles of matter. Berzelius' problem interests had been modified along affinitist lines.

Turning next to how theories are developed, empirically and conceptually, and the related issue of what scientists do when they pursue a theory, it is evident that the empirical problem-solving abilities of Daltonian theory were probed, extended and defined by the accurate experimental research of both Wollaston and Berzelius. When the empirical problem-solving abilities of a theory are exercised in this way at least two things happen to the theory (this is even more apparent if we look not at the individual pursuits of Wollaston, Berzelius and other chemists, but at the cumulative effect of such pursuits on the atomic theory over the course of the century): new
empirical problems confront the theory and the domain of application of
the theory is itself 'carved out,' probed and extended. Berzelius made
an important first effort to extend the range of Daltonian theory by
analysing 12 organic compounds, yet, as he noted, the success of the
theory here was a qualified one -- from the data he obtained he was
forced to conclude that although formulae could be given for them in
accordance with Dalton's theory, the law of definite proportions did
not see applicable. Berthollet's effort to reorient affinity theory by
showing that "the chemical action of bodies ... does not depend upon
their affinity exclusively, but also on their quantity" was an effort
to extend the problem-solving domain of affinity theory (though that
extension was not successfully secured along these lines until the law
of mass action was formulated after, 1864). Berthollet's documentation
of the inconsistencies between the rules of elective affinity and many
chemical reactions demonstrated, however, the degree to which the
positive heuristic that had been guiding affinitist research had been
played out: it was no longer successfully securing empirical problem
solutions or advancing the attempts of chemists to quantify their field
along affinitist lines.

We can see in all of these cases that when scientists pursue a
theory they are careful to draw attention to its various empirical, as
well as conceptual difficulties with the intent (to paraphrase
Berzelius) not of refuting the theory, but of furthering work on it.
The conceptual problems faced by the atomic theory were openly
addressed by Wollaston and Berzelius. Both chemists were acutely aware
that the mechanistic atomism which Dalton had tendered was inadequate
and unsatisfying as an account of the process of chemical combination, and this contributed to their decisions to abandon its pursuit. Both of them developed alternatives to the theory which permitted them to benefit from Dalton's valuable empirical laws without committing themselves to the conceptually problematic chemical atoms. Berzelius would do this by expressing Dalton's laws in terms of 'volumes' rather than 'atoms', noting that "in the present state of our knowledge, the theory of volumes has the advantage of being founded upon a well-constituted fact, while the other has only a supposition for its foundation," while Wollaston would confine himself to the analytical values, or 'equivalents', yielded by experiment.

We can return now to our three questions, using these examples and others to further probe, and offer some more general observations regarding, each of them. What do scientists do when they pursue a theory? What happens to theories when they are pursued? And what happens to individual, and to the community of, scientists? We will then turn more directly to the question of the rationality of pursuit.

When scientists decide to pursue a theory, they are deciding to work on it. The converse is also true: when scientists decide to work on a theory, they are deciding to pursue it. But neither is true of theory acceptance. A scientific decision to accept a theory is not also a decision to work on the theory ... although the latter decision may also be made. Nor is it the case that a scientist who had decided to work on a theory has also thereby decided to accept it. To work on a theory is to engage or to make use of it in some portion of one's
research activities, and theories may be worked on in laboratories, in armchairs, or even in professional meetings. There are thus, fairly substantial pragmatic commitments involved in theory pursuit, and I am claiming that, in this, it differs from theory acceptance. My position here on the pragmatic implications of theory acceptance contrasts markedly with that of Van Fraassen and possibly with that of Laudan examined above.42 The only pragmatic implications of theory acceptance, I suggest, are a readiness to defend the theory and a willingness, at least, to work on it. In this respect, the acceptance of a scientific theory differs from that of a moral code. Strong pragmatic commitments follow from the latter: one must at least try to live by the moral code one finds most worthy of acceptance.43 But one’s research life need not be lived in accordance with the scientific theory one regards as most worthy of acceptance. The reason is that there may be other promising theories available, theories that deserve to be developed.

There are extensive commitments involved in theory acceptance, but they are epistemic ones, such as: the belief that the theory provides the best of all available explanations; the belief that it is empirically adequate, or the most effective problem-solver in some domain; and, if one is a realist, the belief that the theory is true and that the theoretical entities posited by it exist. By contrast, the epistemic commitments of theory pursuit are minimal; epistemically, pursuit need involve no more than the belief that a theory is promising in some domain.
The matter of how one rationally arrives at and defends one's belief that a theory is promising is taken up in the next section. The claims I have made regarding the epistemic commitments involved in theory acceptance are fairly standard. But the claims I have made regarding the limited pragmatic implications of theory acceptance may be contended. Van Fraassen certainly would find them contentious, and Laudan might as well. After all, if a scientist accepts some theory $T$, acknowledging that $T$ provides the best explanation of, or is the most effective problem-solver for, the phenomena in some domain, doesn't it seem odd, if not less than rational, for her not to make use of $T$ to guide her research and problem-solving activities? I will not attempt to mount a full defense of the claim I have made regarding theory acceptance here. This would be a lengthy endeavour and my principal concern at present is to develop an account of theory pursuit. But I can make a few remarks by way of response to these possible objections.

If it were the case that scientists, in accepting a theory $T$, thereby committed themselves "to the further confrontation of new phenomena within the framework of that theory," then other promising theories would fail to be developed (or, at least, we would have to regard their development as less than rational). One does not have to have strong convictions about the importance of theory proliferation to scientific progress to find such a scenario disturbing. (Even Kuhn would (now) agree that rival theories play a role in promoting puzzles into the crisis causing anomalies that may end the hegemony of the accepted theory.) If scientific progress has anything at all to do with the generation of better theories, and if we claim that when
scientists accept a theory they must confront all new phenomena within the framework of that theory, then it would seem that, once an acceptable theory in some domain had been found, 'progress' would be restricted to the kind of "mopping up" and puzzle-solving activities described by Kuhn as characteristic of Normal Science. The hegemony of T would be ensured, challengeable only by less than rational behaviour. We would, moreover, be forced to regard the behaviour of scientists throughout much, if not most, of the history of science, as less than rational.46

But my claim regarding theory acceptance has been a somewhat stronger one. I have maintained not only that a scientist may accept a theory T without working exclusively on that theory, but that she may do so without working on it at all, without thereby being committed to using T to guide or structure any portion of her research activities. My main reason for maintaining this is that I don't see any reason for ruling it out. Provided that there are other promising theories available that deserve to be developed, why should it be a requirement of theory acceptance that a scientist devote some of her research time, effort and money to the accepted theory? I think that it is desirable, and indeed is very likely, that some significant portion of the scientific community devote their research activities to the accepted theory. But I don't see any good reason for requiring of the individual scientists who accept T that they spend at least some of their limited time and resources working on T, when there are other promising theories available and worthy of development. I have said that they should be ready to defend T and should be at least willing to
work on it, but I don't believe that the pragmatic implications of theory acceptance are, or need be, any stronger than this. With this modest defense of my claims concerning theory acceptance in hand, let me return to the issue of theory pursuit. I noted that when scientists decide to pursue a theory they are deciding to work on it. I want to look more closely now at what the latter decision amounts to, at some of the things that scientists actually do when they work on a theory.

The cases of theory pursuit considered earlier in this section have taken us some way towards understanding what scientists do when they are engaged in theory pursuit. When scientists pursue, or work on, a theory they will typically make efforts to develop the theory in two ways. They attempt to refine and extend its empirical problem-solving abilities, and to enhance the conceptual resources which it provides for empirical problem-solving. Let us examine first the contributions they may make to the conceptual development of the theory. An important initial contribution which scientists make here is the frank acknowledgement of the theory's conceptual problems. If a theory is conceptually troubled, ignoring or denying that it is so troubled will only hinder, not contribute to, its conceptual development. We have seen that numerous chemists who pursued the atomic theory readily acknowledged its problematic conceptual standing. They also made attempts to improve this standing through each of the means described in section I of this chapter, that is, by further explicating or fine-tuning its concepts, by increasing its consilience, and by appropriating the conceptual resources of theories in other domains. (Since these efforts have already been discussed in section
I, they will not be reiterated here.) They were attempting, in other words, to establish the conceptual viability of theories in the atomic research tradition, and they were successful in doing so. These then are some of the things that scientists may do when they work on, or pursue, a theory. But scientists will also devote considerable research time and effort to developing the theory empirically, to refining and extending its empirical problem-solving abilities.

When scientists work on a theory in order to refine and extend its empirical problem-solving abilities they customarily proceed by addressing the positive heuristic provided by the theory. The notion of a positive heuristic can be usefully contrasted with that of a methodology. A methodology is supplied by, and is part of what characterizes, the research tradition in which a scientist works. It provides the scientist with general directives about how, and how not, to go about doing science, with general norms which guide and constrain scientific conduct and theory construction: do (or don't) feign hypotheses; do (or don't) posit unobservable entities or processes. And, as Laudan has noted, the methodology of a research tradition will serve, at least broadly and partially, to delimit the domain of application of the theories within the research tradition. It does this by indicating what count as legitimate empirical problems for theories within a particular research tradition. The inductivist and empiricist methodology of 19th-century affinitist chemistry, for example, specified as legitimate empirical problems those concerned with the observable reactions of chemical reagents.

Thus, to ask how this acid and this base react to form this salt is to pose an authentic problem. But to ask how atoms
combine to form diatomic molecules cannot conceivably count as an empirical problem because the methodology of the research tradition denies the possibility of empirical knowledge of entities the size of atoms and molecules.\textsuperscript{47}

Given the non-inductivist, non-empiricist methodology of 19th-century atomic chemistry however, questions about the combining properties of certain entities not directly observable could be, and were, recognized as legitimate empirical problems.

Positive heuristics, by contrast, are supplied not by the research tradition itself, but by the specific theories which constitute it. Like a methodology, a positive heuristic provides scientists with research directives, but unlike a methodology, the research directives a positive heuristic provides are fairly specific. A positive heuristic singles out -- from the set of authentic, or methodologically-legitimated empirical problems addressable by the theory -- certain problems or types of problems, targeting these as primary research problems and relegating the compliment of this subset to a lesser or secondary importance. (In Lakatos' terms,\textsuperscript{48} a "research policy, or order of research, is set out" by the positive heuristic, enabling scientists to live with or ignore certain anomalies while setting a premium on the solution of others.) It may also specify a means by which the primary research problems targeted are to be solved. Thus it guides or directs the research of scientists along specific lines, or towards a specific type of empirical problem, and indicates the resources that are available for developing the theory along these lines or for solving that type of empirical problem. The positive heuristic of Dalton's theory, for example, directed the research of
scientists towards the determination of atomic weights, and it specified the means available for arriving at such determinations, viz. Dalton's "rule of greatest simplicity."

The ways in which theories express their positive heuristics, or the forms which their positive heuristics take, may vary. Lakatos speaks of a positive heuristic as a model, simulating reality which one knows is bound to be replaced with further development of the theory, but he also suggests that it can be formulated as a "metaphysical" principle, such as "the planets are essentially gravitating spinning-tops of roughly spherical shape." Kuhn seems to be describing something very similar to this in his discussion of one of the components of the disciplinary matrix, i.e. the shared commitments to, or beliefs in, heuristic or ontological models such as: the molecules of a gas behave like tiny elastic billiard balls in random motion; heat is the kinetic energy of the constituent parts of bodies. He observes that all such models have similar functions:

Among other things they supply the group with preferred or permissible analogies and metaphors. By doing so they help to determine what will be accepted as an explanation and as a puzzle-solution; conversely, they assist in the determination of the roster of unsolved puzzles and in the evaluation of the importance of each.

The positive heuristic of a particular theory may be partially modified or replaced by subsequent theories in the research tradition. The former may happen, as it did in the case of Dalton's theory, when the means specified for resolving the targeted, primary research problems are found wanting and ineffective, or objectionable in some
way. The latter may happen when the positive heuristic has been played out or exhausted, when it has, as Lakatos puts it, 'run out of steam.' This will usually be signalled by the accumulation of anomalies among the primary research problems targeted by the positive heuristic. Berthollet's effort to re-orient affinity theory, for example, was a response to the build up of such anomalies in Bergman's theory of elective affinity. A new positive heuristic may be introduced partly as the result of impressive developments in certain experimental techniques. An instance of this is the 'galvanization' of affinitist studies in the first decade of the 19th-century by the voltaic pile: a new positive heuristic was introduced according to which the forces of affinity were regarded as analogous to those of electricity. Later in the century, affinitist theories would acquire yet a different heuristic: with Berthelot's bomb calorimeter and with the improvement of measurement techniques in thermochemistry, thermal methods were used to solve problems of affinity.

I have already noted that when scientists work on or pursue a theory they will also attempt to extend its empirical problem-solving abilities to new kinds of phenomena, to use it to explain new classes of facts, and, if possible, to do this in a way which increases the consilience of the theory. Clearly, one way this might happen is by departing from the policy or order of research specified by the positive heuristic. Wollaston's project to develop "a geometrical conception of the arrangement of the elementary particles in all the three dimensions of solid extension" might be regarded as such a departure, albeit a fruitless one. But it is important to realize that
such extensions of the theory may be won through efforts to develop it along the lines specified by the positive heuristic. This is in fact what occurred in the case of 19th-century atomic theory. Efforts to arrive at an accurate determination of atomic weights were finally rewarded around 1860 with the adoption of Cannizzaro's method of calculating atomic weights, and a system of atomic weights based thereon. With this, agreement and consistency in the assignment of chemical formulae became possible and concerted attention could be focused on such phenomena as isomerism whose very recognition had been problematic as long as disagreement over chemical formulae was acute. Chemists were then able to devote themselves to questions concerning the relation of atoms and molecules, to use atomic theory to address structural problems and to develop accounts of chemical bonding.

We are now in a position to respond more fully to a question examined earlier: what happens to theories when they are pursued? And to scientists when they pursue theories? Nineteenth-century chemical theory presents an especially interesting case of theory pursuit. For roughly an entire century, theories in two different research traditions were actively pursued. It was quite possible, as we have seen in Chapter 7, to do chemistry at the beginning of the century without incurring the ontological and methodological commitments of Daltonian atomism. And, although the community had transformed remarkably during this time from a small community of generalists many of whom could do competent work in any area of the science to a large community of specialists, it was still possible to engage in chemical research at the end of the century without incurring the ontological
and methodological commitments of Latter-Day atomism. The tendency to
categorize this century as one of atomists vs. a motley crew of skeptical
detractors and nay-sayers is unfortunate. Some, if not most, of these
'skeptics' had other commitments and problem-interests.

One result of the pursuit of both atomic and affinitist theories
was the development of two different approaches to chemical problem-
solving -- bulk and particulate. The former, unlike the latter, is
consistent with both a continuous and a discrete view of matter. Which
approach it is reasonable to use will be influenced by the kind of
chemistry one is doing and by the aspect of chemical processes being
addressed (chemical reactions undergoing change in form and
distribution of energy, or undergoing change in form or distribution of
matter). Similarly, early in the century, whether it was reasonable to
pursue affinitist rather than Daltonian theory turned in part upon
one's problem interests. If one were interested in studying the forces
in chemical reactions it would not have been reasonable to guide and
direct one's research activities in accordance with Daltonian theory.
However, if one were interested in studying combining weights, the
proportions of elements in compounds and the ratios in which elements
combined, it would have been reasonable, but it would not have been
necessary, to guide one's research in accordance with Daltonian
atomism.

We have also seen that a number of chemists throughout the
century contributed to theories in both research traditions. Berzelius
is of particular interest here. His case provides an instructive
instance of how the research or problem interests of scientists figure in their decisions about which theory or theories they should pursue, and of how, by pursuing these interests, the domain of proper application of theories may be carved out. I have noted above that although Berzelius granted a considerable role to individual atoms in the electrochemical theory of affinity, that theory lay clearly within the affinitist research tradition. The controversy in chemistry for a good part of the 19th-century, as I have characterized it, was over the issue of appropriate problem-solving domains, over whether theories of chemistry were primarily concerned with chemical processes and the forces involved in them or with the determination of atomic weights and the arrangement of individual particles of matter. What disturbed chemists like Berzelius about Daltonian theory was not Dalton's assertion of the existence of atoms, but what he had to say about their nature and centrality in chemical research. Berzelius' first contributions to chemistry were in the area of galvanic research. His early studies of the effects of electricity in organic and inorganic nature led him to collaborate with Davy in an investigation of the nature of ammonia amalgam. Combining proportions proved useful as analytical tools in this investigation, and it was this which first aroused his interest in Dalton's New System. Yet, after contributing work which provided important, if qualified, empirical support for Daltonian theory, Berzelius abandoned it — although he would continue to make use of atomistic concepts in his electrochemical theory. Central among his reasons for doing so was the failure of Daltonian theory to address problems of affinity and the inadequacy of its account of the simple mechanical and geometrical considerations
governing chemical combination. The theory failed to address what he regarded as essential,

being occupied with a part of the phenomena, (when it) ought be embrace the whole ... when we treat atoms in a chemical theory, we ought to endeavour to find out the cause of the affinity of those atoms ... to combine researches respecting the cause why atoms combine with research into the cause why they combine only in certain proportions.

For Berzelius, "the ideas on the relation of the atoms to their electorchemical properties ... constitute(d) an essential part of" chemical theory:

The different relations of bodies to electricity will henceforth be the basis of all chemical systems... [This problem] will soon become the general object of our researches, and gives us reason to hope for a new dawn in chemical theory.

Electricity was the key to chemical affinity, and experiment had shown that electricity seemed to obey the same quantitative laws as ponderable matter in chemical combinations. Berzelius presented a detailed argument demonstrating that the law of definite combining proportions was compatible with important earlier work in the affinitist tradition -- specifically with Berthollet's mass law. Berzelius had hoped to secure a consilient explanation of these laws, but his model of chemical combination was unable to do no more than assume them successively. Berzelius' own electrochemical theory did offer a considerably more detailed account of chemical affinity than had previous affinitist theories. Perhaps too much so, for the degree of specification which it provided of the electrical mode of chemical combination was such as to make later modification difficult. The stress which his theory placed upon the role of electrically polarized
atoms resulted from his attempt to extend the laws of combining proportions to the realm of organic chemistry. The complex chemical constitution of organic compounds posed particularly serious difficulties for Dalton's account of chemical combination. On Berzelius' theory, it was the electrical natures of individual atoms, rather than their arrangement, which accounted for molecular properties.

This controversy between Berzelius and Dalton over the appropriate problem-solving domains for chemical theory would continue throughout much of the century. Attenuated by the fact that, in the first half of the century, the chemical community was still small and composed of generalists, it would contribute to the subsequent diversification of that community. Latter-Day affinitists were primarily inorganic and physical chemists who, in the explanations of chemical phenomena, gave principal emphasis to the role of forces in chemical reactions. To address these research interests, they developed a bulk approach to chemical problem-solving. Organic chemists, pursuing Latter-Day atomic theories, gave principal emphasis to the spatial arrangement of individual particles of matter in explaining chemical phenomena and employed a particulate approach to chemical problem-solving to further these research interests. In this way, over the course of the century, the differing research or problem interests of chemists eventually led to the development of two different approaches to chemical problem-solving. Moreover, as a result of their pursuit of theories in both research traditions, the domains of proper application of atomic and affinitist theories were
slowly carved out. The particulate approach of Latter-Day Atomism was able to provide an account of chemical processes as involving changes in form or distribution of matter and of the arrangement of atoms and molecules in the three dimensions of space. This was especially valuable in elaborating and resolving structural problems such as isomerism. The bulk approach of Latter-Day affinitism was able to provide an account of chemical processes as involving changes in form and distribution of energy. This was of particular value in investigating chemical equilibria and allowed chemists to address the observed behaviour of large quantities of substances independent of any reference to the atomic or molecular constitution of the substances.

The foregoing observations regarding the way in which the problem or research interests of scientists figure in their decisions to pursue particular theories, and the way in which the appropriate problem-solving domains of theories are delineated or carved out as they are pursued, may help us to address more profitably the question of the rationality of theory pursuit. In Chapter Two I argued that appraisals of pursuitability, unlike those of acceptability, need not be comparative. I would like both to qualify and to clarify this claim in light of the considerations advanced above concerning what scientists do when they pursue theories and what happens to theories when they are pursued. When scientists pursue a theory, they work on it in the belief that it has promise as a problem-solver in some domain, or with respect to a certain set of problem-interests which the scientist may bring to the theory, and which figure in that scientist's decision to pursue the theory. Since acceptance involves belief that the theory
provides the best explanation, and since only one among the theories in competition can be accepted as best, appraisal of theory acceptability must be comparative. Should there be a tie, we can only say that two of the competing theories are equally worthy of acceptance; we cannot rationally accept them both. However, since pursuit involves belief that a theory is promising (in the domain or problem-relative way indicated above), and since several competing theories may be able to demonstrate promise in this respect, appraisal of theory pursuitability need not be comparative; a theory may be determined to be promising irrespective of how it fares against the competition. Should there be a tie, we can say that two theories are equally promising, and so equally worthy of pursuit. We can, moreover, rationally pursue them both.

There is, however, a troublesome ambiguity in the preceding paragraph. "We" may be understood collectively, as referring to what it is rational for a particular community of scientists to do. Alternatively, it may be a kind of royal "we", to be understood individually, as referring to what it is rational for an individual scientist to do. The ambiguity is interesting because it suggests the need for a distinction between individual and community rationality. To see this need, consider what happens when we introduce the distinction. If we are dealing with the rationality of acceptance, community rationality can be seen to distribute over individual rationality. That is, if it is rational for a given scientific community to accept T because it provides the best of all available explanations, then it is also rational for each individual scientist to
accept T as the best explanation. Community rationality does not distribute over individual rationality though, when it comes to the rationality of pursuit. If it is rational for a given scientific community to pursue several theories $T_1 \ldots T_n$ because they are promising, it does not follow that it is rational for each individual scientist to pursue several theories $T_1 \ldots T_n$. (Or at least it does not follow if "rational" is understood here as a requirement rather than as a permission. And it is understood as a requirement in the case of acceptance; if T is worthy of acceptance, then an individual scientist would be behaving irrationally by rejecting it.) If several different theories are worthy of pursuit in a community, we do not want to make it a requirement that each member of that community pursue several different theories; a scientist would not be behaving irrationally by pursuing only $T_1$.

This, then, is another respect in which theory pursuit differs from theory acceptance. In the case of theory acceptance, what it is rational for the community to do -- e.g., accept T -- is also rational for the individual scientist to do, and to be rational the individual scientist must accept T. But in the case of theory pursuit, it does not follow from the fact that it is rational for the community to pursue several theories, that an individual scientist must, to be rational, pursue several theories. Having introduced the distinction between individual and community rationality, we might return to the earlier difference noted between appraisals of pursuitability and of acceptability (namely, that the former, unlike the latter, need not be comparative), and consider whether the claim here needs to be qualified.
in light of this distinction: are comparative assessments of the
promise of theories required at either the individual or the community
level? Once again, there seems to be no reason to require comparative
assessments at the level of the community. To simplify matters,
suppose a given community C at $t_n$ is faced with 7 candidate theories,
some of which are competitors in the same problem domain, some of which
are not. The question is, which of these theories are promising and
so, worthy of pursuit? To determine which are promising, the criterion
of fertility (discussed below, section III) is applied to each. This
will winnow out some of them — those unable to establish clear claim
to being promising as problem-solvers in their respective domains.
Suppose that leaves us with a set T of 4 theories, only two of which
(T1 and T2) are competing in the same problem domain. This then is the
set of theories which it is rational for C to pursue at $t_n$. Since
community rationality does not distribute over individual rationality,
what this means presumably, is that an individual scientist S who is a
member of C must, to be rational, pursue only theories which are
elements of T and not pursue any of those which have been winnowed out
as unpromising.

But which theory (or theories) in T ought S to pursue? What
makes it rational for S to pursue one rather than another of the
promising alternatives? Clearly here a comparative assessment seems
required. And, I think, we can provide it in light of the discussion
earlier in this section regarding how the problem or research interests
of scientists figure in their pursuit decisions. What makes it
rational for different members of a scientific community to invest
their time and resources in the pursuit of one theory (or of certain theories) rather than another are the differing problem or research interests which they bring to their work. These differences among individual members of C may arise from a number of factors: the training they have undergone; the previous work they have done (including the types of problems on which they focused, who they have worked with, the experimental techniques in which they have become skilled); and the problem contexts in which they are currently immersed as a result of their most recent research. The contextual differentiation of a scientific community is the result of such factors. They were, in the case of the nineteenth-century chemical community, largely responsible for the transformation of that community from a small group of generalists many of whom were able to do competent work in different areas of chemistry to a diverse community of specialists many of whom confined their research efforts to specific areas (e.g., organic, rather than physical, chemistry).

Keeping in mind the relevance to pursuit decisions of the problem interests which scientists bring to their work, we can return to S who must reach a decision about which of the 4 promising theories in T to pursue. We have said that only two of these theories are competitors, that is, they are both promising in the same domain, or with respect to the same problems. Suppose further that these are the problems which interest S, those which, as the result of the various factors mentioned above (her training, previous work, current research), she is most concerned to address and hopes to resolve. Clearly S could rationally defend her decision not to pursue T3 and T4, by acknowledging that
while those theories are promising in their respective domains, the
problems in those domains are not among her primary problem interests.
(Perhaps S is a late 19th-century organic chemist, and T1 and T2 are
theories in the atomic research tradition, while T3 and T4 are theories
in the affinitist tradition. Unlike her contemporary, Van't Hoff, her
problem interests are fairly narrow, structural ones, confined say to
problems of geometric isomerism. Given these problem interests, she
has rationally defended her decision not to pursue T3 and T4, for,
while the latter may be promising theories, they are not promising in
the domain of problems of most interest to her. And it would not be
rational for her to use those theories to pursue her problem interests.)

This leaves S with T1 and T2, both of which are promising in the
domain of problems on which she is interested in working. Is a
comparative assessment of T1 and T2 needed at this point? Must S, to
be rational, pursue only one -- the more promising -- of the two?
There seems to be no reason to require such a comparative assessment,
to insist that she behaves rationally only if, after appraising their
fertility, she works to develop the more promising of the two, the one
able to demonstrate a higher degree of fertility. If both theories
have been able to establish themselves as promising problem-solvers in
the domain of problems which interests her, then she may be well-
advised to pursue them both. With further development, the theory
which was at t1, the less promising of the two, may prove itself after
all the more effective problem-solver in that domain. If we are to
give theories, especially new theories, a chance to develop and to
prove their empirical problem-solving abilities we cannot (contra Laudan) insist that there is, at $t_n$, only one theory which it is rational for $S$ to pursue (the one with the highest rate of progress, or degree of fertility).

It is important to realize though, that while $S$ is not rationally required to choose, say, $T_1$ rather than $T_2$, she could rationally defend such a choice. She may wish to concentrate all of her research efforts on one theory, and she might defend her choice by arguing that, upon appraisal, $T_1$ has proven itself the more fertile, hence more promising, theory. To complicate matters for her here, we might give her a colleague $C$, who shares the same problem interests as $S$ and who similarly wishes to concentrate his research efforts on one theory. $C$, however, having appraised the fertility of both $T_1$ and $T_2$, has determined that $T_2$ has displayed a greater degree of fertility. Thus he will defend his decision to pursue $T_2$ by arguing that it is the more promising of the two. $S$ and $C$ then, are both able to defend their pursuit choices but they disagree as to which is the more promising theory.

I have introduced this complication to make a final point regarding the rationality of pursuit. Before making it let me summarize the discussion thus far. I have argued that the contextual differentiation of a scientific community is the result of the differences in research orientation and problem interests which are to be found among the individual members of that community, and which arise from a variety of factors that permit and encourage the
specialization of research efforts. At any given time it is likely that there are a number of different theories which it is rational for such a community to pursue and comparative appraisals of theory promise are not required to determine these. However, since some of these theories may be promising in different domains or with respect to different problems, and since the problem interests and research orientation of scientists differ, some comparative assessment is required by an individual scientist who must select among the theories that are promising in the community those which, given her problem interests, it is rational for her to pursue. Once these have been determined, no further comparative assessments of promise are required of the scientist. However, while rationality does not require that she make a further comparative assessment and pursue only the most promising of the competing theories, it certainly permits her to do so. And she may rationally defend her choice by arguing that it is the most fertile, hence most promising theory. The final point I have wanted to make here, by introducing a colleague who challenges her choice, is that there is room for rational disagreement among scientists as to which theory is most promising in some domain. To paraphrase Kuhn, when scientists do decide to pursue, from among several competing theories, only that theory which is most promising, they may nevertheless reach different conclusions even though they are fully committed to the same criterion of fertility.

The reason for this, I suggest, is due to individual differences in the weighting of the various indices of fertility which themselves arise from the type of differentiating factors noted above (training,
problem-concentration, previous work, etc.). As a result, one scientist may place considerable weight on the ability the theory has shown to clarify its concepts. Another may weight more heavily the kind of explanation which the theory affords. Another may stress its positive heuristic, or its dynamic consilience, and so on. That scientists committed to the same criterion may rationally disagree as to which of the competing theories being appraised is most promising, is a welcome consequence. Such comparative assessments need not be made, but they often are made: scientists often do decide to concentrate their research efforts on developing the theory they have determined to be the most promising. The different conclusions they may reach as a result of according different weights to the various indices of fertility helps to insure that a number of different promising theories will be further developed and be given a chance to demonstrate the nature and extent of their empirical problem-solving abilities.

III. The Promise of Theories

How can we tell when a theory is promising, and so worthy of pursuit? A host of nineteenth century chemists, as we have seen, made such judgements regarding both atomic and affinitist theories. Thus far we have seen something of what is involved in making these judgements -- namely assessments of the conceptual well-foundedness of a theory, and something of what they commit scientists to, epistemically and pragmatically. For the purpose of appraising the
conceptual well-foundedness, I have proposed a criterion of conceptual viability. We will consider now what else appears to be involved when scientists like van't Hoff attempt to "measure a theory by its fruits" and determine whether or not it "gives the current interest on capital." The conceptual viability of a theory is only one of the indices which need to be consulted to estimate the fertility of a theory and determine whether or not it is promising and worthy of further development. We must also appraise the empirical well-foundedness of the theory -- the capacity it has shown to undergo empirical growth and refinement. Appraisals of conceptual and empirical well-foundedness are temporal indices of theory fertility, addressing how the theory has developed over time. But to arrive at an optimal estimate of theory fertility, there are at least two other formal indices which must be consulted as well. We must appraise both the positive heuristic and the kind of theory which is under consideration. The first of these four indices of theory fertility has already been examined in Section I of this chapter. In this section we will address the remaining three and complete our response to the question which opened this paragraph.

I have suggested above that when scientists appraise a theory's empirical well-foundedness they are concerned with its empirical yield, i.e. with the capacity it has demonstrated to undergo empirical growth and refinement. In assessing empirical well-foundedness, scientists raise questions about how well the theory has responded to the empirical demands that have been made of it in research, and thereby enabled us to explain the natural world; how well the conceptual
resources provided by the theory have actually succeeded in furthering important empirical research. A theory can demonstrate its capacity to undergo empirical growth and refinement in at least three different ways: by increasing the accuracy of its problem solutions and the number of problems it solves in a given domain; by extending its problem-domain (i.e. by adding new classes of facts to the domain of facts which it explains); and by increasing its dynamic consilience (that is, by extending its problem-solving domain without the addition of ad hoc hypotheses). In assessing the empirical yield of a theory then, the capacity it has displayed to undergo empirical growth and refinement in these three ways, our concerns are both quantitative and qualitative ones. We are asking not only how much the theory has been able to explain, or how many problems it has been able to solve over some interval, but how well it has done so: has the accuracy of its problem solutions improved? Has it extended its domain in a dynamically consilient or in an ad hoc way?

Beginning with the first, let us look briefly at each of these three means by which a theory may increase its empirical yield. A theory may undergo empirical refinement and growth by improving the accuracy of its problem-solutions and increasing the number of problems it solves in a given domain at any stage of its career, though it is perhaps the most reliable means by which a young and little-developed theory may impress and rapidly increase its empirical yield. Focusing their efforts on the primary research problems targeted by the positive heuristic of a theory (on what Kuhn refers to as "that class of facts that the paradigm has shown to be particularly revealing of the nature
of things"), scientists can significantly advance its empirical problem-solving abilities by sophisticating experimental methods and techniques of measurement, or by contributing a new (or improving and adapting an existing) technical apparatus for use in increasing the precision and scope of problem-solutions in the targeted research area. Berzelius, for example, devised well-planned gravimetric procedures and employed carefully purified reagents to perform analyses which were patiently repeated until he arrived at values for combining weights of the elements which could be refined no further. Davy explored and improved techniques for utilizing the voltaic cell in chemical research. His success in decomposing a number of salt solutions and solid compounds contributed to the early empirical problem-solving abilities of the electrochemical theory of affinity, according to which the chemical attraction between elements, responsible for compound formation, was held to be electrical in nature. Later in the century, thermal methods, used to solve problems of affinity, were greatly advanced with the development of Berthelot's bomb calorimeter and with the improvement of thermochemical measurement techniques. Similarly, in the first half of the 19th century, the development of numerous methods to arrive at accurate determinations of atomic weights helped to further the empirical abilities of atomic theory: Dalton's rule of greater simplicity; Gay-Lussac's law of combining volumes; the law of Petit, and Dulong on the relationship between specific heat and atomic weight; Mitscherlich's law of isomorphism which established the relationship between chemical composition and crystalline form. Each of these specific methods contained limitations which often led to ambiguous and discordant results; and the accurate determination of
atomic weights would not be fully secured until, as a result of Cannizzaro's pamphleteering, the general method proposed by Avogadro and based on vapor densities was adopted.

Both of the remaining two ways in which a theory may increase its empirical yield, and so demonstrate a capacity to undergo empirical growth and refinement, have to do with extensions of the theory's problem-solving domain. They differ in the manner in which these extensions are achieved -- through the addition of ad hoc hypotheses or through an increase in dynamic consilience. I have already argued that when a theory manages to extend its problem-solving domain in a dynamically consilient way it enjoys an important, conceptual as well as empirical achievement by securing greater simplicity and generality for the network of conceptual resources it provides for empirical problem-solving. Ad hoc extensions, by contrast, introduce greater complexity into this network of resources; new assumptions are added to the theory in order to account for a particular fact or class of facts, and these assumptions do not receive further empirical support as the theory is developed or do not serve to uncover new facts which they help to explain. Berzelius, for example, was unable to provide a consilient explanation of the law of definite combining proportions using Berthollet's theory of affinities. He had simply added the assumption that those parts of bodies that combined do so in definite proportions; any excess of the various substances would remain in an equilibrium determined by their antagonistic forces. Later he would use his own electrochemical theory of affinity to provide an explanation of isomerism which was ad hoc in nature. He assumed the existence of
different atomic states to account for the different properties of substances which had the same chemical composition. Neither of the above assumptions served to uncover new facts which they then helped to explain, though both added to the empirical problem-solving abilities of their respective theories. A consilient explanation of isomerism was, however, provided by atomic theory. Wollaston had early taken up and attempted to develop Dalton's claims regarding the geometrical considerations governing chemical combination, arguing that the atomic theory could not rest contented with atomic weight determinations but would need to provide geometric models of the arrangement of atoms in space. With the development of such models, and of three-dimensional views of chemical bonding in the third quarter of the century, atomic theory was able to explain the differences in properties of substances with the same composition in terms of structural differences. Another notable example of a theory extending its problem-solving domain without the addition of ad hoc assumptions or modifications of the theory is to be found at the end of the 19th-century in the dynamically consilient explanation provided by kinetic-molecular theory of the phenomenon of Brownian motion.

Appraisals of a theory's empirical yield, like those of its conceptual viability, provide scientists with temporal, or diachronic, indices of theory fertility which may be used to determine whether or not a theory is promising and worthy of pursuit. As such, they are highly sensitive to the specific time-interval selected: determinations of the empirical yield or conceptual viability of a theory may vary markedly if the interval over which the theory is being evaluated is
lengthened or shortened. When scientists assess the promise of a
theory at some time \( t_n \), they do not always consult the full career of
the theory -- how it has developed from its inception at, say, \( t_1 \), but
often emphasize how it has performed most recently -- say from \( t_4 \)...
\( t_n \). A nice example of this is at hand in the diverging assessments
which scientists made of the promise of kinetic-molecular theory at the
end of the 19th-century.

After a series of impressive achievements from roughly 1850-1880,
kinetic-molecular theory lapsed into a period of stagnation which
lasted until 1905 and during which it came under considerable critical
attack. We have seen in Chapter Four some of the assessments which
scientists made of it in the 1890's. Van't Hoff observed that

```
with a fairly large expenditure of mathematical development
the kinetic theory barely gives the current \% interest on
capital, and I think that even this theory should be
measured by its fruits.
```

And Planck, in 1897, noted that

```
Obstacles, at present unsurmountable, however, seem to
stand in the way of its further progress. These are due
not only to the highly complicated mathematical treatment
of the accepted hypotheses, but principally to essential
difficulties ... in the mechanical interpretation of the
fundamental principles of Thermodynamics.
```

Both Van't Hoff's and Planck's assessments of kinetic-molecular theory
as less than promising resulted from emphasizing the recent (post-1880)
history of the theory -- what it had done or been unable to do lately,
and the difficulties it had generated. Boltzmann, by contrast, in
assessing the promise of the theory in 1896 and 1899, judged it to be
promising in light of its earlier (pre-1880) achievements. He
acknowledged the point that the theory had not done much for scientists lately:

The mathematical part of the gas theory ... pursues mainly the purpose of further development of mathematical method, for the valuation of which immediate practical utility was never decisive.

and complained that the theory's critics "had inferred from the small current yield of molecular theory to its decline." Citing the important results which had been achieved through the use of molecular ideas, he denied "the alleged barrenness of atomism" and argued that, for his part, its "earlier attainments" meant that it should be cultivated further. 02

Like the differences among scientists regarding the weighting of the various indices of fertility (noted at the end of the last section), these differences in the selection of an appropriate time interval over which to assess theory fertility may help to provide for rational diversity of action in the scientific community. Individual scientists may reach different conclusions regarding the empirical yield and the conceptual viability of a theory because they differ in the significance which they grant to the recent performance of the theory. But these temporal indices of fertility are not the only ones which must be consulted. There are at least two formal or synchronic indices which figure importantly in appraisals of theory fertility; assessments need to be made of the positive heuristic and of the kind of theory which is under consideration. Such indices can help us to understand why it might be rational for a scientist to stick tenaciously by a theory through a long period of stagnation, or to work
on a theory which is new and little developed, and where the temporal
indices of fertility may accordingly be only marginally applicable.

In the previous section I indicated that a positive heuristic
provides scientists with research directives by singling out -- from
the set of authentic, or methodologically-legitimated empirical
problems addressable by a theory -- certain problems or types of
problems and targeting these as primary research problems while
relegating the compliment of this subset to a lesser or secondary
importance. I noted that a means by which these targeted problems are
to be resolved may also be specified by the positive heuristic, and
that the positive heuristic may receive partial expression in the form
of a model, analogy or metaphor. Drawing on Whewell's discussion in
another context, we might say that the acquisition of a positive
heuristic supplies scientists with a "bond of unity by which the
phenomena are held together," leaving

the subject ... open to further prosecution; which, ulterior
process may, for the most part, be conducted in a more
formal or technical manner. The first great outline of the
subject is drawn; and the finishing ... demands a more
minute pencilling.... In the pursuance of this task, rules
and precepts may be given, and features and leading
circumstances pointed out, of which it may often be useful
to the inquirer to be aware.

According to the positive heuristic provided by Dalton's theory,
for example, atoms were analogous to a pile of shot: the atoms of a
given element were all alike with respect to their specific properties
(e.g. weight, size and number per unit volume), while the atoms of
different elements differed from one another in weight, size, and
number per unit volume. (One consequence of this was that the atoms of a compound gas would occupy a greater volume than the atoms of an elemental gas; hence, contra Gay-Lussac and Avogadro, equal volumes of gases could not contain equal numbers of atoms.) An important property characterizing the atoms of the different elements was their relative weights, and, indeed, Dalton had stated that "one great object" of his work was to show "the importance and advantage of ascertaining" these relative weights. Thus, the determination of atomic weights was singled out as a primary research problem by the positive heuristic of Daltonian theory, and a means for resolving such problems was provided in the form of Dalton's rule of greatest simplicity. The equivalent weights of the elements (the weights that combine together to give definite compounds) could be determined directly by experiment. Data on combining weights could then be used to calculate atomic weights if one knew how many atoms of one element combined with a single atom of another. Since there was no means of estimating such combining numbers of atoms, Dalton assumed that combination would always be of the simplest type. Discrepancies between predictions and experimental results could be and were attributed by chemists working on the theory to inaccuracies resulting from insufficiently refined analytical techniques. When Berzelius, for instance, encountered such discrepancies he assiduously repeated and modified his procedures.

Enlightened by the knowledge of my own errors, and with the aid of better methods, I finally found a great accord between the results of the analyses and the calculations of the theory.
The question we must now consider is how one is to appraise such positive heuristics. There are at least three features of a positive heuristic which need to be addressed: the analogy or model it provides; the means it specifies for resolving the targeted research problems; and the results to which the application of this means have led. Appeals to the first of these three features -- the analogy or model which the positive heuristic provides -- can play an important role in supporting judgements of the promise or future fertility of a theory. When a theory's positive heuristic is based on a model or analogy which is familiar and well-established, it is in a position to make a stronger case for being worthy of pursuit. Scientists often do cite such a model or analogy as figuring among their reasons for believing a particular theory to be promising. Boltzmann, for example, in defending his pursuit of the kinetic-molecular theory, argued that it deserved to be "cultivated further" in part because "experience teaches that one will be led to new discoveries almost exclusively by means of special mechanical models." The mechanical model of billiard balls provided a familiar and well-understood basis for studying what was less familiar and less well-understood -- the behaviour of the molecules of a gas. Similarly, throughout much of the 18th century, affinitivist chemistry was guided by a positive heuristic according to which the forces of chemical affinity were analogous to gravitational forces. Since the latter were, as the result of Newton's work, familiar and well-established, it was believed they would prove fruitful as an analogical 'key' for solving the problems of chemical affinity.
The conceptual resources which a theory supplies for empirical problem-solving are enhanced by connection with those of an already well-established theory: the use of such models and analogies provides scientists with a way of drawing upon the resources of another successful theory in order to resolve the primary research problems targeted by the positive heuristic of the theory on which they are working. They may increase their understanding, and improve their explanations, of the phenomena which they are addressing by regarding these phenomena as like phenomena which are already well, or are better, understood. Of course the model or analogy does not itself provide an explanation of the phenomena in the targeted research area, or solve the primary research problems. What it does do is to suggest the kind of explanation which is needed and is likely to succeed, leaving to scientists working on the theory the task of developing the specific, detailed explanations that are required in each case. In Whewell’s terms, it leaves the subject "open to further prosecution", by drawing "the first great outline" which then demands finishing through "a more minute pencilling" in of detail. Moreover, points of disanalogy may play a role here in ruling out a certain kind of explanation or problem solution. On the positive heuristic of Berthollet’s affinity theory, for example, the forces of chemical affinity were analogous to gravitational forces in that they varied according to the masses, or quantities of the reacting substances, whereas this was a point of disanalogy between the forces of affinity and those of gravitation according to the positive heuristic of Bergman’s theory of elective affinities. Thus, the former theory suggested that the kind of explanation which would prove fruitful in
accounting for particular chemical reactions would need to make reference to the relative quantities of the reacting substances. This kind of explanation was ruled out by the latter theory: the products of a reaction were to be explained by reference only to the relative intensities of the affinities of substances, the quantities entering into the reaction being irrelevant.

A second feature of the positive heuristic which needs to be consulted is the means specified for resolving the targeted research problems. To see how such a means may be critically evaluated, we might consider the objections that were raised against Dalton's rule of greatest simplicity as a means for determining atomic weights. Recall that, since there was no means of estimating the numbers of atoms which combined to form compounds, Dalton had simply assumed that such combination would always be of the simplest type. A number of chemists (including Wollaston, Berzelius, Gay-Lussac and Lavoisier) were acutely dissatisfied with this assumption and regarded it as an arbitrary conjecture. The assumption was not arbitrary in that Dalton gave no reason for it: he held that binary compounds are more likely than ternary, and etc., since the mutual repulsions of atoms of the same element limit the number which can combine with those of the other element. But it was conjectural in that there was a lack, or an insufficient variety, of evidence for it. While the rule could be used, in conjunction with Dalton's theory, to arrive at correct predictions regarding combining proportions, there were quantities which appeared in the rule, namely the combining numbers of atoms,
which could only be determined by using the rule itself. Thus there was no independent way of testing the rule. As Berthollet objected:

We have no means of determining the number of atoms which combine in this manner in each compound; we must therefore have recourse to conjectures... Can such presumptions serve as the basis for the determination of the elements of chemical combination? Are we not accepting the vaguest speculations of metaphysics...

Berzelius too held that Dalton's assumption was not "fully warranted by facts" and initially made use instead of Gay-Lussac's empirical law of combining volumes to determine the combining numbers of atoms. The ratios of the weights of the combining volumes of elementary gases were taken to represent the ratios of the weights of the atoms of those elements. Later, he drew on the DuLong-Petit law as well, which permitted the calculation of atomic weights on the basis of measurements of specific heats. Neither of these two methods for determining atomic weights relied on the rule of greatest simplicity, and as a result of their use it was possible to calculate atomic weights in more than one way and to provide a variety of evidence for an hypothesis as to atomic weight; such an hypothesis could be tested by measuring combining proportions and then re-tested by measuring specific heats via the DuLong-Petit Law.

What this case suggests is that the promise of a positive heuristic is affected by the variety of evidence that can be cited for any hypotheses which occur in it. However, the means which a positive heuristic specifies for resolving the targeted research problems need not involve the use of hypotheses. Sometimes the means specified will involve the use of a technical apparatus or device. The promise of a
positive heuristic may be greatly enhanced when it directs scientists to the use of a powerful new experimental technique or apparatus for resolving empirical problems in the targeted research area. The positive heuristic of Davy's (as well as of Berzelius') electrochemical theory of affinity, for example, directed scientists to decompose compounds into their constituent elements by means of an electric current supplied by the voltaic battery. The great power of the latter (which Davy had described as "an alarm bell to the slumbering energies of experimenters in every part of Europe"), suggested that it would be possible to decompose substances such as the alkalies, which were believed to be compounds but which had resisted every effort to break them down into their simpler substances.

In appraising the promise of a positive heuristic, it is important to address not only the analogy or model it provides and the means which it specifies for resolving the primary research problems, but also the results to which the application of this means has led. Questions may be raised about the actual ability which the positive heuristic has demonstrated to secure adequate solutions to problems in the targeted research area. There are two examples of this which we might examine here. The first is Wollaston's criticism of the positive heuristic of Dalton's atomic theory, and the second is Berthollet's criticism of the positive heuristic of Bergman's elective affinity theory. In both of these cases, the criticisms were directed not so much to the means specified for resolving the primary research problems targeted, as to the results achieved by employing these means. Wollaston, for instance, pointed out that the means for determining
processes, or mechanisms. Consequently, they are able to survive changes in, for example, the prevailing theory of matter and will remain unchallenged should doubts be raised about some specific postulated mechanism held to be responsible for the integral effects or molar phenomena to which regulative theories are addressed. The likelihood that they will be subject to intertheoretical conflicts is also lessened since they may be compatible with competing matter theories and with different accounts that may be proposed of underlying entities, processes or mechanisms. Maxwell, for instance, cited this as a virtue of his 1865 "Dynamical Theory of the Electromagnetic Field." There he followed Thomson and Halt in arguing that the energy of the electromagnetic field could be specified without also specifying a particular underlying mechanical structure for the system, without constructing an account of "the nature of the connexions of the parts of the system." To do so, he drew on Lagrange's generalized equations of motion, maintaining that the power of this method lay in the fact that "the final equations ... are independent of the particular form of (the mechanical) connections," and recommending it as "presenting to the mind in the clearest and most general form the fundamental principles of dynamical reasoning." The formalism of analytical dynamics employed by Lagrange also provided the basis for George Green's 1838 theory of the elastic solid ether. Green too, in defending the promise of this theory, argued that it is preferable to assume a general "physical principle as the basis of our reasoning, rather than assume certain modes of action which, after all, may be widely different from the mechanism employed by nature."
the affinity between two substances is constant under similar conditions, and so is independent of the masses of those substances. Thus chemists, in arriving at determinations of relative affinities, were directed to discount as relevant the quantities of the reacting substances, and to attend solely to the relative intensities of the affinities of substances. Elective affinity was, then, a constant, invariable force which alone determined the direction of a chemical reaction.

In application, this positive heuristic was, for a considerable time, enormously successful. Chemists, by proceeding in accordance with the means it specified for resolving the targeted research problems were able to determine, and to establish the order of elective affinities. Bergman had managed to extend his affinity tables to include most known substances and to reconcile the rules of elective affinity with a number of reactions which had seemed inconsistent with them. But as chemists continued working with this positive heuristic in order to fill out the omissions in Bergman's tables and to dispose of the remaining exceptions to his rules, they found themselves unable to fit the reactions they discovered into his ordered displacement series without inconsistencies. They repeatedly uncovered what they termed anomalous reactions, i.e. ones which did not fit the predicted order. This forced them to devise an increasing number of ad hoc explanations to account for these anomalies. Fourcroy, for instance, in 1801, declared that

all anomalies were the result of unaccounted, special circumstances such as the physical state of the substances, the heat or cooling employed, imaginary substances, and
particularly haste and carelessness on the part of the investigator.

Clearly then, by the beginning of the 19th-century, the positive heuristic of Bergman's theory had been played out, or as Lakatos would put it, had "run out of steam." In other words, the means it specified for resolving the targeted research problems was no longer able to achieve its ends effectively. The assumption that elective affinity was a constant force which alone determined the direction of a chemical reaction and which was independent of the relative quantities, or masses, of the reacting substances was no longer fruitful in securing problem solutions in the targeted research area.

It was just this exhaustion of the positive heuristic provided by Bergman's theory of elective affinities which led Berthollet, in 1803, to regard that theory as unpromising and no longer worthy of pursuit. He attempted to re-orient affinity theory by supplying it with a new positive heuristic which directed chemists' attention to the importance of considering not only the affinities, but the relative quantities of the reacting substances, as well as the properties which influenced the direction of a reaction. According to the positive heuristic provided by Berthollet's theory, the forces of chemical affinity were analogous to gravitational forces in that they were proportional to the relative masses of the reacting bodies. In this way Berthollet was able to explain a number of reactions of alkalies and alkaline earths with acids which were anomalous under Bergman's theory.
Having explored the three features which need to be consulted in appraising a positive heuristic, we can look now at the second of the two synchronic indices of theory fertility. When scientists estimate the promise or future fertility of a particular theory, they sometimes make reference to the kind of theory which it is. I will present some examples of this momentarily, but before doing so it will be helpful to consider first a distinction, introduced by Merz, between two different kinds of theories. Merz contrasts a constructive theory with a regulative theory:

one which allows us to deal with the grand total or outcome -- mathematically called the integral -- of physical processes and changes without necessarily possessing a detailed knowledge of the minute elements or factors -- mathematically called differentials -- out of which they are compounded. Inasmuch as what we actually observe are always integral effects -- i.e., summations or aggregates of great numbers of individual and unobservable processes -- this line of reasoning is not infrequently very useful, and has been in many cases applied to arrive at important conclusions...

He adds that whereas a regulative theory exerts a control and enables us to check the correctness of results, (b)oth in chemistry and physics other (constructive theories) are required for extending -- not merely correcting -- our knowledge.

Unlike constructive theories, regulative theories address themselves only to such "integral effects" or "molar phenomena" and do not attempt to construct them by appealing to underlying entities, processes, or mechanisms.

This distinction between two different kinds of theories was expressed in Chapter Four in terms of a distinction between two different kinds of problem-solving approaches. Latter-Day affinitist
theories were regulative theories which employed a bulk approach to chemical problem-solving. They dealt with the observed behaviour of large quantities of substances independent of any reference to the atomic or molecular constitution of the substances. Chemical reactions were studied by considering the energy changes accompanying reactions: the system as a whole could be examined without requiring the construction of any underlying models or commitment to any particular theory of matter. Latter-day atomic theories, on the other hand, were constructive theories which employed a particulate approach to chemical problem-solving. Using this approach, chemists attempted to provide accounts of the behaviour of matter in quantity by reference to the behaviour of individual particles. Chemical reactions were studied in terms of atoms, molecules and their motions: a mechanism for chemical change could be provided, based on the laws governing the motions of atoms and molecules. These theories were committed to a discrete, particulate view of matter.

How are considerations of theory kind relevant to an appraisal of the promise or future fertility of a given theory? Merz's discussion suggests that the great virtue of constructive theories is that they are capable of extending our understanding of the natural world in significant ways. A constructive theory often does this by provoking a great deal of both experimental and theoretical research activity, by opening up new problem areas or by generating extensive programs of research for which it, some modification of it, or a new theory to which it gives rise, serves as the basis. His comments with respect to the atomic theory, and Newton's theory of gravitation -- two of his
primary examples of constructive theories -- bear this out. As
Newton's theory
had given rise to a surprising activity in physical
astronomy, to a long series of exact measurements, and to
theoretical deductions of a purely mathematical kind, so
the atomic theory of Dalton in the early years of the
century fixed the task of chemists for a long time ahead.80

Regarding the atomic theory, he draws attention to
the extension which was gained in the domain of actual
facts ... the great harvest of actual knowledge of the
things and processes of nature which was collaterally
gained, whilst chemists were trying to prove or to refute
existing opinions.

One of the reasons which Boltzmann cited in support of his assessment
of kinetic-molecular theory as promising and worthy of pursuit, singled
out this ability of a constructive theory to extend knowledge by
opening up new problem areas for research. He argued that the "more
boldly one goes beyond experience ... the more surprising the facts one


can discover,"81 He acknowledged the usefulness of regulative theories
(those of Latter-Day affinitism), but maintained that

a theory which yields something that is independent and not
to be got in any other way, for which, moreover, so many
physical, chemical, and crystallographic facts speak, must
not be combated but further developed.

It would be a mistake to conclude that since regulative theories
do not, by contrast, serve to extend scientific research and to uncover
new domains for investigation in the way that constructive theories do,
they are therefore inherently less promising as theories. Their virtue
lies in their potential to control and regulate subsequent research.
This is a powerful function which is possible in part because regulative theories do not offer accounts of underlying entities,
processes, or mechanisms. Consequently, they are able to survive changes in, for example, the prevailing theory of matter and will remain unchallenged should doubts be raised about some specific postulated mechanism held to be responsible for the integral effects or molar phenomena to which regulative theories are addressed. The likelihood that they will be subject to intertheoretical conflicts is also lessened since they may be compatible with competing matter theories and with different accounts that may be proposed of underlying entities, processes or mechanisms. Maxwell, for instance, cited this as a virtue of his 1865 "Dynamical Theory of the Electromagnetic Field." There he followed Thomson and Tait in arguing that the energy of the electromagnetic field could be specified without also specifying a particular underlying mechanical structure for the system, without constructing an account of "the nature of the connexions of the parts of the system." To do so, he drew on Lagrange's generalized equations of motion, maintaining that the power of this method lay in the fact that "the final equations ... are independent of the particular form of (the mechanical) connections," and recommending it as "presenting to the mind in the clearest and most general form the fundamental principles of dynamical reasoning." The formalism of analytical dynamics employed by Lagrange also provided the basis for George Green's 1838 theory of the elastic solid ether. Green too, in defending the promise of this theory, argued that it is preferable to assume a general "physical principle as the basis of our reasoning, rather than assume certain modes of action which, after all, may be widely different from the mechanism employed by nature."
These two synchronous indices of theory fertility provide a valuable complement to the temporal indices discussed earlier. Their merit, I would suggest, is this. First of all, they enable us to see how a newly emergent and little-developed theory, which may be able to register no more than a meager claim to fertility under the temporal indices, may nevertheless be judged promising and worthy of pursuit. The ability to accommodate cases like this is an important one if we believe it can be rational for scientists to invest their time and resources exploring and developing the empirical problem-solving abilities of new theories. Secondly, such synchronous indices may help us better to understand, and to account for, the phenomena of rashness and tenacity which we find in scientific communities. They suggest why it might be rational for a scientist to pursue tenaciously a 'stagnating' theory, i.e. one in which considerable time and resources have been invested, but which nevertheless has fared only minimally vis-à-vis the temporal indices of fertility. Conversely, they permit us to appreciate how it can be rational for a scientist to abandon pursuit of a theory which, according to the temporal indices is relatively fertile, and 'rashly' pursue some other theory which is, according to these same indices, much less fertile.

IV. Concluding Remarks

My intent in this essay has been two-fold. I have wanted to establish that there is a pressing need to enrich our normative methodological proposals by providing an account of the conceptual
dimensions of theory-appraisal, of pursuit as a modality of appraisal, and of a criterion for theory-pursuit. I have also wanted to move to meet that need by developing such an account. My task here has been to address the question of what it is to pursue a theory and of what it is for a theory to be promising, and to stress the role of conceptual considerations in supporting judgements of theory promise. The reason for this latter emphasis is due partly to the fact that traditionally the role played by such considerations has been either overlooked or acutely understated, and partly to the fact that their role in theory appraisal is most in evidence when scientists are attempting to determine whether or not a theory is promising, and so worthy of pursuit. Throughout much of "De Gravitatione..." for example, Newton raised grave doubts about the conceptual viability of Cartesian theory as he argued that it was unpromising and no longer worthy of pursuit. And throughout most of the 19th-century, appraisal of the promise of the atomic theory repeatedly made reference to its conceptual standing. Conceptual viability, however, is only one of several indices which must be consulted to provide an optimal estimate of the future fertility, or promise, of a theory.

It seems fitting, in these concluding pages, to raise a final question: is the present account itself a promising one? It will come as no surprise that I am prepared to maintain that it is. But here, as in the scientific context, this is to claim both that it is worthy, and in need of further development. And this in turn is to acknowledge that it has certain abilities, but also certain limitations or shortcomings. Accordingly, I will briefly draw attention to what I
take to be the merits of the foregoing account as well as to some of
the respects in which it is wanting and requires further work.

At the outset of this essay, I identified three problem areas
neglected by traditional philosophy of science, and argued, with the
aid of the case studies, that this neglect is a serious one, which
greatly constrains our ability to accommodate as rational much of the
actual history; and practice, of science. I then examined some of the
attempts that have been made, primarily by problem-solving
methodologists in historical philosophy of science, to address these
problem areas. While their contributions are valuable, they indeed have
directly informed my research here, they were found to be inadequate in
various respects. My own efforts to contribute to problem-resolution
in these areas have built upon their work but, in order to overcome
these inadequacies, I have found it necessary to depart from their
analyses in some significant ways. Laudan's work is an instructive
case in point both because my own work is clearly indebted to his and
because he has done more than perhaps anyone else to advance research
in each of these three areas. Drawing on the case studies, I
contended: that his problem-solving effectiveness appraisal measure
does not adequately capture the role which conceptual considerations
play in theory appraisal; that his claim that appraisals of pursuit are,
essentially and necessarily comparative has the unfortunate consequence
that at any given time in a particular scientific community there can
be only one theory which it is rational for scientists to pursue; and
that his suggestion that the 'rate of progress' which a theory has,
enjoyed is sufficient to determine whether or not it is promising and worthy of pursuit is unsatisfying on several grounds.

If the proposals developed here regarding how to capture the role of conceptual considerations in theory appraisal and how judgements of theory promise and pursuitability are to be secured have merit, it must lie in the degree to which they have moved us beyond the inadequacies of earlier analyses and placed us in a better position to account for actual cases of theory appraisal such as those presented above. Further development of these proposals might proceed along a number of different lines. From the consideration of further cases of scientific appraisal we could learn whether or not extending the range of application of these proposals will require some modification of them. This too might lead to a needed refinement and clarification of certain features of the proposals themselves; e.g. of the distinctions between individual and community rationality, and between regulative and constructive theories, of how the appropriate problem-solving domains of theories are 'carved out' as they are pursued and of the consequences of this for theory appraisal, and of the nature, function and evaluation of the positive heuristics of theories. The implications of these proposals for several more general topics might also be explored. On the Laudanian account for example, the aim of science (or rather, of those who engage in the activities of science) can be seen to be the development of theories which are better, 'more' effective problem-solvers. The present account does not take issue with this, indeed it attempts to understand how this may come about, i.e. through the pursuit of theories which are promising. It does take
exception however, to the claim that the promise of a theory is solely a function of its 'rate of progress' as determined via Laudan's problem-solving effectiveness appraisal measure. It proposes instead a criterion of fertility for estimating the promise of a theory, the weighting of the indices of which may vary among individual scientists. It also suggests that the question "Is it rational for scientist S to pursue T at t_n?" cannot be readily answered (as it can be for Laudan), since the problem interests of scientists in a given community may differ: some specification of S's problem-interests is required if we are to say in which theory or theories it would be rational for her to invest her efforts and resources.

Now there are two points that could be made about the foregoing issues. The first is that I have been assuming throughout what might be regarded as 'legitimate' problem interests, that is, I have not meant to imply that any or all problem interests deserve to be addressed. Admittedly, there is a great deal more that needs to be said about what constitute legitimate problem interests and the types of factors which determine this. A second, related point is this. It might be objected that the various ways in which the present account attempts to provide for rational diversity of action within a scientific community leads to the Feyerabendian extreme of anarchism according to which it can be rational to pursue any theory. While the account offered here does invite considerable latitude in this regard, I do not think the objection can be sustained. Application of the criterion of fertility may not ensure unanimity in estimations of the promise of a theory, but it does enable us to establish, in particular
cases, that a theory is no longer promising and so is unworthy of further pursuit, as well as that it would be irrational for an individual scientist with a specific set of research or problem interests not to pursue certain theories. If, for example, one were studying problems of isomerism in the last quarter of the 19th-century it would have been irrational not to pursue theories in the atomic research tradition. These theories had amply demonstrated their promise in furthering research in this and related areas, and isomerism did not fall within the problem-solving domains of Latter-Day affinitist theories. Similarly, if one wished to continue studying problems of elective affinity after 1803, it would not have been rational to do so using Bergman's theory. Berthollet's devastating review of the latter had clearly revealed that it was no longer promising as an approach to the study of chemical affinity.
Footnotes


2. See pp. 151 above.

3. Some of the most explicit examples of this in the case of the atomic theory are to be found in the discussions reported in the Chemical Society Journal, 1868-69, excerpts of which can be found in the above, Chapter Four, Section VI.

4. See Chapter Four, Section IV.

5. Or, more exactly, the research tradition of which it is part. A positive heuristic or heuristics will also be supplied. I consider the latter in section III of this chapter.


7. See especially p. 43 and ff. above.


10. Ibid., 48.

11. Ibid., 48.

12. In Chapter Two, Section III. See especially fn. 476.

13. Thus, however desirable, it is not a necessary condition of conceptual viability that the theory have succeeded in reducing the overall number of conceptual problems from which it suffers.


15. See above, p. 155.

16. Whewell (1847), 7, 10, 15, 6.

17. See especially pp. 157-59 above.


20. Ibid., 18.

21. Ibid., 10. See also the discussion of exemplification by Huxley in Giere and Westfall (1973), 73-76.
22. See the Appendix, Section II especially.


25. Whewell (1847), 68, 74, 73.

26. Ibid., 74.

27. Ibid., 68-9.

28. His definition is as follows: "a theory is ad hoc if it is believed to figure essentially in the solution of all and only those empirical problems which were solved by, or refuting instances for, an earlier theory". See Laudan (1977), 115.


30. Ibid., 118.

31. See p. 149 above.

32. Cf. p. 156 above.

33. See p. 166 above.

34. Cf. p. 136 above.

35. See pp. 124-25 above.

36. See p. 138 above.


38. Cf. p. 133 above.

39. See p. 138 above.

40. See p. 126 above.

41. Cf. p. 137 above.

42. Cf. pp. 52 and ff. above.

43. I have argued this in my (1985).

44. See p. 53 above.

45. For a discussion of this, see Nicholas (1982).
47. Ibid., 87.
49. Cf. Ibid., 135-36.
51. See above, p. 136.
52. Levere (1971), 147.
53. Ibid., 143.
54. Cf. Ibid., 146.
55. Cf. Ibid., 166-67.
56. Kuhn (1977), 324.
57. See Section I of this chapter.
59. See Chapter Four, Section VIII.
60. Ibid.
63. Whewell (1847), 46. Whewell is here discussing the "colligation of facts".
64. See p. 122 above.
66. Quoted in Gardener (1979), 12.
67. For a useful discussion of analogy as a criterion for inference to the best explanation, see Thagard (1983), 144-49.
69. Gardener makes this point in his (1979), 14-23.
70. Quoted in *ibid.*, 15.
71. See Muir (1869), 17.
73. See above, p. 127.
74. Cited in Gardener (1979), 16.
75. Holmes (1962).
76. See especially Merz (1912), 146, 152. Merz notes that the "whole of chemistry has been revised for teaching purposes from this point of view by Hans Januschke, *Das Prinzip der Erhaltung der Energie*, Leipzig, 1897."
79. Merz (1904), 395-96.
81. Quoted in Gardener (1979), 12.
82. Cited in Merz (1912), 186.
83. Quoted in Klein (1972), 69a.
84. In *ibid.*, 69-70.
85. In *ibid.*, 70.
APPENDIX

ABSOLUTE SPACE: DID NEWTON TAKE LEAVE OF HIS
(CLASSICAL) EMPIRICAL SENSES?

I. It is in the scholium of the Principia on time, space, place and
motion that Newton delivers what is—arguably—a reluctant kiss of
betrayal to empiricism. Right there, "in the main body of his chief
work," as E.A. Burtt observes, the deed is done: "When we come to
Newton's remarks on space and time...he takes personal leave of his
empiricism."¹ Reichenbach registers the event less charitably,
dismissing the "crude reification of space that Newton shares with the
epistemologically unschooled mind in its naive craving for realism."²

Injury is then added to insult as Reichenbach holds Newtonian mechanics
to task for arresting the analysis of the problems of space and time
for more than two centuries.

What Reichenbach and like-minded company (to be recognized below)
are roughly agreed upon is that the question "Why was absolute space
necessary to do physics?" is mistaken. Absolute space not only was
unnecessary, it was a lapse on the otherwise brilliant physicist's
part. Given the fact that it is superfluous, the question "What was
absolute space?" tends to be ignored as unintelligible babble by the
verificationist-minded or to be passed over in appropriate silence
because untestable. Consider as an instance of the former, Phillip
Frank.
We could not understand the logical structure of Newton's physics if we ignored the fact that he used in his law of inertia an "organismic" or, as we may say in this case, a "theological" element. In the late eighteenth and early nineteenth centuries, when the effort was made to purge physics of all theological elements, Newton's physics became illogical. "Absolute space" became a mere word without the slightest trace of operational meaning.

J.J.C. Smart, on the other hand, in route to his conclusion that absolute space is "scientifically otiose" contends that:

Since the special theory of relativity, we now also know that a velocity with respect to absolute space cannot be determined by any means whatever... A verificationalist or operationalist philosopher would therefore deny all meaning to talk of absolute space. Nevertheless, if we are not verificationalist about meaning..., then we can allow Newton's assertion of the existence of absolute space to be both meaningful and consistent with his assumptions. We might still, however, regard the assertion of the existence of absolute space as pointless, as an assertion which does not make an interesting (because testable) scientific hypothesis.

The consensus view is that Newton was playing Judas to his empiricist self in the notorious scholium, lapsing into the postulation of absolute space. This acrasia response (AR) takes one or both of two forms. In the AR-1 the lapse is a supernatural one; in the AR-2 the lapse is a logical one. Both versions seek primarily to seize upon an excuse for Newton's behaviour, rather than to ferret out good reasons for it. Both amount to a refusal to give absolute space a responsible, historically sensitive consideration. Neither is able: 1) to draw sufficient support for its claims from the testimony of the scholium in question; 2) to take into account the significance of certain historical factors; 3) to place the postulation of absolute space in the context of the Principia as a whole.
Newton the weak-willed, the first version of the AR reads, indulged his "naive craving for realism" and consortcd with "a crude conception of a substantial space which ... cannot be correct." Or, Newton the "mystic and dogmatic" succumbed to his "supernaturalistic tendencies": he begins with "very precisely formulated empirical statements (presumably a reference to Definitions I-VIII which precede the scholium) but adds a mystical superstructure (i.e. 'absolute space and time').' The foregoing are Reichenbach's rabid renditions of the AR-I. In the same spirit as Frank and Smart, Whitrow chalks up absolute space to a metaphysical-theological belief.

Newton's conception of the world-process was basically "metaphysical", but he believed that its actual course can be ascertained only by its effects on observable phenomena. Unfortunately, Newton became entangled in a serious difficulty. He interpreted his famous experiment with a rotating bucket as empirical evidence, supporting his metaphysical-theological belief, derived from More, that space is absolute.

I do not intend to dispute here Ted McGuire's contention that the basic concepts of Newton's natural philosophy "can be ultimately clarified only in terms of the theological framework which, guided so much of his thought." Even if McGuire is correct in this, it would lend no support to the peddlars of the AR-I. The latter have not cited theological considerations to clarify one such basic concept and its role in the Principia, but to dismiss it. Newton has given way (or perhaps vent) to his theological scruples and has, for the brief span of this scholium, gone off the (empirical) rails. He returns quickly to the straight and narrow, but irreparable damage has already been done: He has gone on record as endorsing the empirically undetectable.
This empiricist-backed campaign to purge absolute space from Newtonian mechanics is suspect for several reasons. Too readily convinced of the absurdity of Newton's 'lapse', it seeks to write off what needs to be illuminated. It may be granted that Newton's theological convictions were influential in his commitment to absolute space. But this is not to suggest, as the AR-1 would have it, that the postulation be regarded as no more than a regrettable concession to Newton's supernaturalism. Moreover, this view foresewars any serious effort to read the scholium as an integral part of the Principia. It emerges, rather, as an unexpected and out-of-place growth on an otherwise empirically sound text.

The AR-2, which in many cases is propounded concurrently with the AR-1, regards the postulation of absolute space as a logico-philosophical lapse on Newton's part. Once again, it is Newton's alleged departure from empiricist methodology which prompts the charge. E.A. Burtt, for instance, impressed by Newton's claim that "in philosophical disquisitions, we ought to abstract from our senses, and consider things themselves, distinct from what are only our sensible measures of them," ends up excusing Newton as an "uncritical, sketchy, inconsistent, even second-rate" philosopher, although a "marvelous genius" of a physicist. The motif remains essentially unchanged: absolute space is scientifically otiose and must be explained away. Mach's interpretation of the bucket experiment is wielded with a heavy and historically insensitive hand to sustain the change. Whitrow, arguing that Berkeley anticipated "in all essentials" the Machian criticism of Newton's analysis of rotational motion, concludes that:
In his discussion Mach made the same point, remarking that
the only experimental test that could be imagined for the
falsification of the idea that rotational motion is
relative (with respect to the universe as a whole) would be
to compare Newton's experiment as he performed it with one
in which the bucket is left undisturbed and the universe is
made to rotate around it. The test is impossible to carry
out and so we are not compelled to accept Newton's
interpretation of his experiment.

Ernest Nagel is another who cites Mach's criticism as decisively
capturing the Newtonian logical lapse.

But let us examine Newton's interpretation of the bucket
experiment. Newton's argument was severely criticized by
Ernst Mach, who showed that it involved a serious non
sequent. Newton noted quite correctly that the variations
in the shape of the surface of the water are not connected
with the rotation of the water relative to the sides of the
bucket. But he concluded that the deformations in the
shape of the surface must therefore be attributed to a
rotation relative to absolute space. However, this
conclusion does not follow from the experimental data and
Newton's other assumptions, for there are two alternative
ways of interpreting those data: the change in the shape of
the water's surface is a consequence either of a
rotation relative to absolute space or of a rotation
relative to some system of bodies different from the
bucket.

The promised examination of Newton's interpretation of the bucket
experiment is conducted in light of Mach's alternative interpretation,
i.e., with the corrective and indiscriminate assistance of historical
hindsight. It also conveniently overlooks the fact that the relativistic
cartesian theory whose confused inadequacy Newton was successful
in demonstrating ran into conceptual problems precisely by moving
beyond the most immediately surrounding bodies (e.g., the bucket) to
some different and indefinite reference system to account for (what it
claimed was) absolute motion. Indeed, Newton had, well before the
scholium, considered the hypothesis that the "tendency to recede from
the axis of rotational motion" was due to "a motion relative to the fixed stars." However, what was needed to 'compete' with Newton's so-called interpretation (and more properly called theoretical demonstration) was an alternative theory. None of the relativistic theories of this time, and most decidedly not the influential one of Descartes, could offer an equally convincing account of the experiment.

The basic claims that the AR (a joint effort of both versions) can be seen as making are, therefore, three-fold: first, that Newton's postulation of absolute space was an abandonment of his empiricism; second, that it is attributable to religious 'influences'; third, that the reasoning which leads to its postulation in the scholium, and which supports the conclusions drawn from the bucket experiment, is indefensible. Scientifically otiose and thus disgraced, absolute space emerges as a regrettable and unintelligible (since metaphysical) stain on the Newtonian record — a groundless prejudice from which Einstein's special theory of relativity liberated physics.

II. The empiricist-minded backers of the AR have presented an account of absolute space which is confused, inaccurate, and untenable. It is by filtering Newton through the lenses of subsequent scientific advances that they have arrived at the alleged 'lapses.' That absolute space was otiose and that Newton in postulating it was playing Judas to his empiricist self are charges of which he should be considered innocent until proven guilty, and not the reverse. The fact that more recent events — such as the rise of relativity theory — suggest that
absolute space is not necessary to do physics cannot be used as evidence that Newton was dealing in the otiose.

The confusion which riddles the AR is apparent even in so mollified a rendition of it as Max Jammer's. At one point, Jammer notes that absolute space is a necessary prerequisite for the validity of the first law of motion and claims that its introduction into Newton's system of physics "did not result from methodological necessity only."¹³ Later, however, he makes a very different claim. Not only is absolute space not a methodological necessity, but its postulation violates the Newtonian methodology. Although Newton "succeeds in convincing himself that he has proved the reality of this concept by physical experiment ... [H]e was not aware that his procedure violated the very principles of the methodology he professed."¹⁴ Similarly, the existence of an absolute reference system is at times regarded by Jammer as an "indeensible assumption" and at times as demonstrated or proven by the bucket experiment.

In an attempt to fashion a more illuminating account of Newton's behaviour and a more satisfying response to the 'why' and 'what' of absolute space, we might examine the suggestion (implicit in the AR, but never productively formulated by it) that its postulation was a violation of Newton's official methodological dictum "hypotheses non fingo." This second proposal — that absolute space was a hypothesis — has the merit of being consistent with the well-founded appraisal of Newton as violator par excellence of his own methodological commitment and is thus able to marshal some support both from the Newtonian party.
line (what he says he does, or does not, do) and from occasional Newtonian practice (what he actually does — namely, feign the odd hypothesis). But for all its prima facie plausibility, this second proposal does not stand up to careful scrutiny.

The argument that absolute space is offered by Newton as a hypothesis focuses on the passage from the existence of absolute accelerations (as 'established' by the bucket and the rotating globes experiments) to the existence of absolute space. Why was it necessary to postulate it? Well, it was necessary as any other feigned hypothesis was necessary for Newton — presumably to explain the properties of things. Consider what Newton has to say on the subject of hypotheses in his reply to Pardies' second letter.

In answer to this it is to be observed that the doctrine which I explained concerning refraction and colours, consists only in certain properties of light, without regarding any hypotheses, by which those properties might be explained. For the best and safest method of philosophizing seems to be, first to inquire diligently into the properties of things, and establishing those properties by experiments and then to proceed more slowly to hypotheses for the explanation of them. For hypotheses should be subservient only in explaining the properties of things; but not assumed in determining them; unless so far as they may furnish experiments.

The 'thing' in the present case is motion, accelerated motion, and the property of it which Newton seeks to explain by the hypothesis of absolute space is its absoluteness. Newton has, first, diligently inquired into the properties of things, and established that there are absolute accelerations by means of the bucket experiment. "...[T]he proper method for inquiring after the properties of things is, to
deduce them from Experiments,\textsuperscript{16} He then proceeds with an explanation. As one backer of this second proposal has put it:

All in all, the following hypothesis is plausible: There is an object, homogenous, isotropic and infinite in extent, acceleration relative to which is the unique cause of the existence of inertial forces ... the object exists everywhere at everytime, and is everywhere and always the same. It is invisible, massless, totally penetrable, frictionless, etc. In fact, its sole properties seem to consist of its being the substantival mark against which absolute accelerations are to be gauged.\textsuperscript{17}

Among Newton's methodological prescriptions, such hypotheses are to be distinguished from theories and phenomena. A theory is arrived at not by "deducing it only from a" conflation of contrary suppositions, but by deriving it from Experiments, concluding positively and directly."\textsuperscript{18} As Feyerabend observes in his study of Newtonian methodology:

The nature of things, insofar as it shows itself in experiment, and their behaviour, is settled first, and in a unique way. Reasons for this behaviour and this nature are given later, and with the help of hypotheses.... No hypotheses are needed if the problem is to establish a theory, or a phenomenon.... 19

Thus, if this second proposal is correct, the absolute space hypothesis does not serve to establish any theory or phenomenon: rather, it is designed extra-(if not post-) theoretically to explain or provide reasons for the water's ascent up the sides of a bucket and the tension in a cord joining two spinning globes. Newton, consistent in his inconsistency, has disappointed in his promise not to feign hypotheses elsewhere, and his commitment to absolute space in the scholium is but another instance of the same.
The allure of this proposal is powerful if it is regarded as the only alternative to the AR. For under it, the introduction of absolute space may be regarded as a departure from Newton's empiricist methodology but as neither a pointless Newtonian quirk or aberration nor an indefensible otiose assumption. It is a hypothesis (however well or poorly founded) needed by Newton to explain the bucket experiment. But this alternative will not do. Its basic claim that absolute space was a hypothesis and a violation of Newton's empiricism is incorrect and its account of why absolute space was necessary is inadequate. That the support for this second proposal is marginal and unsatisfactory, that it is undercut both by an appreciation of the scholium itself and of its function in the Principia as a whole, as well as by other historical considerations, and that it fails thereby to illuminate faithfully Newton's actual procedure are claims which will be defended by the development of a third, alternative, proposal. This alternative suggests a perception of la belle bête and of its role in the Principia in which Newton is seen as consistently sticking to — rather than enjoying a respite from — his schizophrenic classical empirical senses. Moreover, it lays the groundwork for a philosophical analysis of absolute space which does a greater justice to, by taking more complete advantage of, the historical record.

III. The case for this third proposal as to the nature and necessity of absolute space rests primarily upon three issues, each of which will be considered in progressively greater detail. The first has to do with the general structure and content of the Principia itself. This
will be noted only in the broadest fashion and will serve principally as a contextual backdrop for the second issue — i.e. the function of the scholium within the Principia. The influence of Descartes on Newton will be of particular interest here. The third issue focuses upon the testimony of the scholium itself on the subject of absolute space.

In the Preface to the Principia, Newton states that his aim is, "to cultivate the science of mathematics, as far as it relates to philosophy." The whole difficulty of philosophy, he continues, "seems to consist in investigating the powers of nature from the phenomenon of motion, and in demonstrating other phenomena from these powers. And to this end the general propositions (of Book I and II) are directed." He then embarks upon what might be regarded as foundational concerns, laying down the definitions of such words (e.g. the absolute, accelerative, and motive quantities of centripetal force) as are less known and explaining the sense in which he "would have them to be understood in the following Treatise." The scholium to the definitions undertakes some conceptual clarifications of notions such as time, space, place and motion which he does not define as they are "well known to all." The axioms or laws of motion are then introduced, immediately following which — throughout the entirety of Books I and II — Newton sets out in detail his theory of mechanics. Book III then deals with a specific example — the explication of the system of the world — an investigation of the "forces of gravity ... (which) are derived from celestial phenomena, by propositions mathematically demonstrated in the former books."
Let us turn then more carefully to the scholium itself. Why does Newton concern himself with the clarification of the notions of time, space, place and motion? The most immediate reason he gives is that:

... the vulgar conceive those quantities, only from their relations to sensible objects. And thence arises certain prejudices, for the removing of which it is proper to distinguish them into absolute and relative, true and apparent, mathematical and vulgar.

This, together with the structure of the Principia as a whole, reflects an acute familiarity with and sensitivity to Descartes' Principes and the mare's nest of conceptual problems therein. The way has been paved for Part III of the Principes, as for Book II of the Principia, and their respective explications of the system of the world by, in each case, a detailed theory of motion. Descartes' consideration of the concepts of space and place were indisputably crucial to the relativistic or quasi-relativistic theory of motion which he developed. They were essential parts of the theoretical apparatus by means of which he fashioned his account of motion. A similar observation, I submit, is to be made of Newton (particularly of a Newton wary of Cartesian relativism and the ambivalent stance toward absolute space which helped to generate it... as Westfall states: "To avoid the Scylla of relativity, Newton embraced the Charybdis of absolute space.")

Absolute space is not a hypothesis which is irrelevant to establishing the theory of mechanics set out in the Principia, playing no more than an extra- or post-theoretical explanatory role. It is an integral part of Newton's theoretical apparatus.
This last proposition requires elaboration. At the close of the scholium, just prior to introducing the laws of motion, Newton reveals that

... how to collect true (that is, absolute) motion from their causes, effects, and apparent differences; and, on the contrary, from motions, either true or apparent, to collect their causes and effects, shall be explained more at large hereafter. For to this end I composed the following treatise.

While the determination of absolute motion and of the distinction between absolute and relative motion is, as Newton claims here, the effort of the Principia as a whole, the effort of the scholium is in part -- as we shall see below -- to establish the existence of absolute motion. Unlike Descartes, Newton makes explicit early on his position both on the absolute vs. relativism debate, and on absolute vs. relational views of space. That the latter has been historically the more 'visible' and discussed debate is perhaps unfortunate and a misleading emphasis. Central in Newton's postulation of absolute space was his desire to avoid Cartesian relativism in his theory of motion. Thus, absolute space became a crucial piece of theoretical equipment. The tendency to see Newton as pitted against Leibniz has perhaps obscured this, acting as a red herring to deflect attention to the issue of ontological commitment and of the objective existence of an empirically undetectable entity.

IV. Two related claims have been made that merit further attention. These are that the scholium was, in good part, designed to illustrate that there is absolute motion and that absolute space was requisite in
this illustration. Our examination of these will involve us in a reconsideration of the bucket experiment and is intended to advance the case for the third proposal.

There are two important inferences in the bucket experiment. One is from the data to absolute acceleration and the other is from absolute acceleration to absolute space. The first inference was unchallenged by Newton's contemporaries and was not seriously questioned until Mach. Opponents of absolute space were preoccupied with the second inference and with how to block it. As Sklar puts it:

"...they believe, if there are absolute accelerations they must be accelerations relative to something. But then, uniform motion relative to that something is absolute uniform motion and place relative to that something is absolute space. And, they believe, if these latter are mere metaphysical chimeras, then so must be absolute acceleration as well. But the water still sloshes in the bucket, the weights fly out on the string, and the moon falls to fall into the earth. ...it is only because the earlier (that is, pre-Machian) pure relationalists, Leibniz, Huyghens, and Berkeley go so far as to accept Newton's arguments for absolute acceleration that they end up perplexed."

While the second proposal argued that absolute space was a (extra- or post-theoretical) hypothesis used in "explaining the properties of things" — the bucket experiment — "but not assumed in determining them," the precise opposite is asserted by the third proposal. Absolute space is not used in explaining but in determining the properties of things — the absoluteness of the water's motion. Newton has taken, here as in the ray theory of light, the crucial classical empirical step of regarding the "illustrations as an experimental basis."
Feyerabend observes that an important identification is involved in this step: the experimental result is identified with what Newton refers to as a phenomenon, an "idealized description of the result that uses the terms of the theory under review" whose features correspond point for point to the peculiarities of the theory to be proved. Thus it serves as an illustration of, rather than as evidence for, the theory.

There are two aspects to such an idealized experiment. One is its 'canonized' status: it is not on all fours with other experiments but is purposely singled out as an exemplary experimentum crucis. The second is its purified, purified nature. It is cleansed of its shortcomings. The peculiarities of each single experiment and those features of it which do not allow for an immediate description in terms of the theory are omitted.

Consider the following passage from the scholium which lends support to the claim that absolute space is not a mere explanatory device but part of the theoretical apparatus Newton sets up to deal with motion:

Wherefore entire and absolute motions cannot be otherwise determined than by immovable places; and for that reason I did before refer those absolute motions to immovable places. Now no other places are immovable but those that from infinity to infinity, do all retain the same given position one to another, and upon this account must ever remain unmoved and do thereby constitute immovable space.

Newton now quickly moves to relate a crucial experiment, the bucket phenomenon, which offers a selected illustration of the existence of both absolute acceleration and absolute space. We are faced with a
phenomenon however, an idealized description of the experimental result whose features correspond to the peculiarities of the theory to be proved. He begins by claiming that "the effects which distinguish absolute from relative motion are the forces of receding from the axis of circular motion." With this criterion in hand he then turns directly to a description of the experiment and applies the criterion:

... the vessel, by gradually communicating its motion to the water, will make it sensibly begin to revolve and recede by little and little from the middle, and ascend to the sides of the vessel ... this ascent of the water shows its endeavor to recede from the axis of its motion; and the true and absolute circular motion of the water ... becomes known.

A few sentences later he repeats that the ascent of the water toward the sides of the vessel "proved its endeavor to recede from the axis." This endeavor does not depend upon any "translation of the water in respect of the ambient bodies; nor can any true circular motion be defined by such translation." This endeavor, and any such absolute acceleration depends upon "translation" with respect to absolute space.

Newton's description of his experimental result has, then, been couched in the terms of the theory under review. The bucket phenomenon illustrates the presence of centrifugal forces, and ipso facto, of both absolute acceleration and (for Newton at least) absolute space. If the description is loaded, how is it idealized? Does Newton, in other words, redescribe what is seen in order to turn it into a physically useful phenomenon as he did, for example, in that classic "case of the disappearing pyramids"? (Recall that in the latter optical case, cited by Feyerabend, we find Newton making no response to Linus' critical observation that the spectrum is capped by sharp cones or pyramids
rather than the semicircular ends corresponding to the circular aperture. Instead of offering, in the Opticks, some explanation of why such pyramids are in fact observed — even though the physical light terminates in a semicircle — Newton elects to redescribe what is seen in terms of the theory in question and thus the pyramids 'disappear.' This demonstrates the way in which, for Feyerabend's Classical Empiricist, the 'phenomena' serve as selected and idealized experiments whose features correspond point for point to the peculiarities of the theory to be proved.)

I think that there is a sense in which Newton has 'neglected' certain features of the experiment which would not facilitate his theoretically-loaded description of it. But in this instance two important points must be noted: 1) we do not have a Linus or a von Helmholtz actually reporting such 'pyramids' until Mach, and 2) Newton's negligence was justifiable given his methodological practice. The points are related in that Mach's report constituted, in effect, an attack upon Newton's classical empiricism.

Looking at the first point we might recall the failure of Newton's relativistically-committed contemporaries (Leibniz, Berkeley, et al.) to challenge the first inference in the bucket experiment and Newton's illustration of a system of bodies undergoing absolute acceleration. (This might be owing to the mesmerizing effects of Newton's 'demonstration,' noted by Sabra. It is precisely this inference which Mach challenges, and in doing so he is blocking the crucial classical empirical step of regarding the phenomena or
illustrations as an experimental basis. Newton's description of the bucket experiment is a description of a phenomenon, namely of what sort of motion the water in the bucket is undergoing at various stages. It is not a description of what is seen, and what is seen is simply a change in the shape of the surface of the water. This change might be regarded as the consequence of rotation relative to something, but no more absolute space than the fixed stars. In Mach's words:

There is no decision about relative and absolute with which we can possibly meet, to which we are forced or from which we can obtain any intelligence or other advantage. ...Newton's experiment with the rotating vessel of water simply informs us, that the relative rotation with respect to the sides of the vessel produces no noticeable centrifugal forces, but that such forces are predictable by its relative rotation with respect to the mass of the earth and the other celestial bodies.

Mach is here nipping classical empiricism in the bud by taking a different methodological tack -- a hypothesis is advanced which offers an explanation for the change in shape of the surface of the water in a rotating bucket. But this relativistic hypothesis is weak competition for an absolutistic theory. It is not until the emergence of special relativity and the appearance, not of a methodological alternative and the 'hint' of a competing theory of motion, but an empirically progressive theoretical alternative that Mach's objections acquire the peculiar forcefulness they seem to have in the minds of many philosophers. And even then, the conclusion he sets out above -- "there is no decision about relative and absolute with which we can possibly meet" (though Mach clearly makes one!) -- can only be reached after a prior decision has been made to accept Mach's alternative methodology which, in contrast to Newton's, seeks to explain or to provide reasons for the water's ascent.
Looking more briefly however at the second point, it must be noted that some twenty years before Newton's discussion of the bucket experiment in the scholium, he had entertained the idea that the tendency of a body to recede from the axis of circular motion could be explained relativistically by appeal to rotation relative to the fixed stars — or to innumerable other bodies. But this is rejected as it leads to an incoherent theory of motion (Descartes') and all mention of fixed stars 'disappears' from the scholium.

For unless it is conceded that there can be a single physical motion of any body, and that the rest of its changes of relation and position with respect to other bodies are so many external designations, it follows that the Earth'...endeavours to recede from the centre of the Sun on account of a motion relative to the fixed stars, and endeavors the less to recede on account of a lesser motion relative to Saturn and the aethereal orb in which it is carried, and still less relative to Jupiter... (etc. through Mars' aetherial orb and others.)

According to the relativistic theoretical alternative with which Newton was faced, and given the manner in which it individuated motions, we have no reason to regard the 'fixidity' of the fixed stars as any greater than any other system of bodies: they certainly do not provide the guarantee of an immobile reference point which absolute space does and which Newton quite soundly considered requisite for the account of true and philosophical motion that Descartes had claimed to offer.

Of course, Linus' pyramids and Mach's fixed stars are not on quite the same footing. In the former case, Newton was forced to redescribe what he saw in order to turn it into a useful phenomenon. We are not faced with this clear-cut a discrepancy in the latter case. Newton does, nevertheless, in the scholium, choose to neglect those
aspects of the experimental data of what he saw, which would run afoul of his theory-laden description of the bucket phenomenon. What Newton saw was a mere change in the shape of the water's surface; what the bucket experiment illustrated was a system of bodies undergoing absolute motion. What he neglects to consider is that the change of shape he observes could be described as due to motion relative to the fixed stars by some more sophisticated relativistic theory which did not individuate motions in the Cartesian manner, and which did not purport -- as Cartesian relativism did -- to be describing true and philosophical motion. Mach's fixed stars and his suggestion of such a sophisticated relativistic theory could not be so readily ignored as Descartes', but they would have to be dealt with somehow.
Footnotes

1. Burtt (1954) 244.
2. Reichenbach (1959) 53.
3. Frank (1957) 118.
5. Reichenbach (1959) 60.
6. Ibid., 59.
10. Whitrow (1953) 44.
17. Sklar (1972) 296. It is not clear if Sklar consistently endorses the second proposal.
22. Stein in (1967) provides a careful analysis of the technical issues involved in what might be regarded as a defense of the third proposal referred to above. This essay is indebted to his stimulating analysis and suggestions, to some of which I will return below.

23. Sklar (1972) 297, 305.


25. Ibid., 161, 163.

26. Ibid., 161-2.

27. Ibid., 162, footnote 9.


29. Mach (1960), 279, 284.

BIBLIOGRAPHY

Agassi, J. 

Alexander, H. 
The Leibniz-Clarke Correspondence, Manchester: Manchester University Press, 1956.

Berzelius, J. 


Brewer, W. and Treyens, J. 

Brock, W. and Knight, D. 

Brown, H. 

Brodie, B. 

Brush, S. 

The Kind of Motion We Call Heat, New York: North Holland, 1976.

Buchdahl, G. 

Burian, R. 

Butts, E. 

Butts, R. 

Cardwell, D.S.I. (ed.) 

Chi, M. et al. 

Crosland, M. 


Giere, R. "History and Philosophy of Science: Intimate Relationship or Marriage of Convenience?", British Journal for the Philosophy of Science, 24, 1973.


Hanson, N. Patterns of Discovery, Cambridge: Cambridge University Press, 1958.


Newton, I. Opticks, Bk. III, 1730, excerpted in Thayer (1953).


"Queries on Light and Colours" in Cohen (1975).


Space, Time and Spacetime, Berkeley: University of California, 1974.


"From the Descriptive to the Normative in Psychology and Logic," Philosophy of Science, 49, 1982a.

Cognition and the Growth of Science, rough draft, 1983.


"Consilience and the Structure of Theories," paper presented at the spring conference on philosophy of science at the University of Western Ontario, 1984b.


Whitrow, G. "Berkeley's Philosophy of Motion," The British Journal for the Philosophy of Science, 4, 1953.


