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LINDHOLM, Lynn Marie, 1945-
THE ASSUMPTION OF CLARITY-- A
CRITIQUE OF SOME INFLUENTIAL VIEWS ON
THE RELATIONS BETWEEN SCIENCE AND
METAPHYSICS.

City University of New York,
Ph.D., 1977
Philosophy

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LYNN MARIE LINDHOLM

1977

T H E A S S U M P T I O N O F C L A R I T Y

--A Critique of Some Influential Views on the
Relations Between Science and Metaphysics

by Lynn Lindholm

A dissertation submitted to the Graduate
Faculty in Philosophy in partial fulfillment
of the requirements for the degree
of Doctor of Philosophy, The City University
of New York.

1977

This manuscript has been read and accepted for the Graduate Faculty in Philosophy in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

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A B S T R A C T

The Assumption of Clarity

Advisor: Professor Peter Caws

This dissertation is an investigation into the debate on relations between science and metaphysics; it focuses on an assumption -- labelled "the assumption of clarity" -- which has been an important but often unnoticed factor in this debate. In brief form, the assumption of clarity is the view that metaphysical ideas or systems important in the development of science are necessarily clear and consistent views. The assumption includes the concession that metaphysical views do indeed play a role in science; however, it is a different claim: namely, that all such influential metaphysical ideas and systems are coherent, clear, intellectually respectable ideas -- of the kind, one could say, associated with classical modern metaphysicians such as Descartes, Leibniz, or Spinoza when they interpreted in the best light.

I have argued below 1) that the assumption of clarity is a powerful view which has influenced many theories of science and theories on the interaction between metaphysics and science; 2) that the assumption is not only powerful but often held implicitly which makes its influence harder to identify and to criticize; 3) that the assumption is false so that its implicit adoption leads to a distorted streamlining of the interaction between metaphysics and science and makes the fact of an interaction appear trivial instead of philosophically

significant as it truly is.

To elaborate the above claims: I have tried to reveal the extent to which the assumption of clarity has affected debate on the relations between metaphysics and science by affecting the work of some of the most sophisticated commentators in the field and by affecting evaluations of their work. The dissertation includes discussion of the history and rationale behind the assumption of clarity which brings out the important point that the assumption is supported by valuable historiographic and philosophic traditions. These supports have allowed the assumption to affect the historically oriented reports of the relation between science and metaphysics particularly. Also, I have tried to explain how the assumption has influenced several contemporary and classical modern philosophical theories of science and theories of the connection between science and metaphysics; part of this affect is due to a traditional bias against confusion in science; part of it is due to a traditional bias against metaphysics in science.

With respect to the above claims, two areas of debate of thought on interactions between metaphysics and science have been taken as central: that surrounding the works of E. A. Burtt, Alexandre Koyré, and their followers who emphasize historical views of science and metaphysics; and that surrounding the philosophic commentators Karl Popper, his critical disciples Joseph Agassi and W. W. Bartley, and their major opponents including Thomas Kuhn and Stephen Toulmin. These commentators represent some of the most sophisticated, sympathetic, and liberal opinions on the role of metaphysics in science; yet, there are inadequacies in their views and a weakness in the impact of their

views which can be explained by implicit and uncritical deference to the assumption of clarity.

The investigation also includes an extended case study into the work of the early scientist, William Harvey, who discovered the theory of the circulation of the blood (1628). The study reveals the plurality of roles which metaphysical ideas have played in Harvey's research and reviews the inadequacy of commentaries on that interaction.

Finally, the dissertation closes with the outline of an alternative philosophic view of the relation between metaphysics and science; one which reflects the surrender of the assumption of clarity and which thereby comes closer to capturing the true critical character and scope of the interaction.

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INTRODUCTION TO THE ASSUMPTION OF CLARITY AND OUTLINE OF THE ARGUMENT

(a) Background

This investigation began some years ago with an intriguing historical puzzle. Recent research seemed to show that William Harvey, who discovered the theory of the circulation of the blood (1628), had held several general views on the relation of the body to the world, the potential of the body to perfect itself, and the nature of life, which were decidedly alien to modern notions and which appeared odd, unclear and confused in their own right. However, these views also appeared -- on examination of his research activities and reports -- central to his scientific achievement in a number of ways. As just one example, the microcosm-macrocosm theory of the body, that all important aspects of the body have a parallel in the structure of the world, suggested to Harvey the theory of circulation in which the blood circulates in the body to return to the heart just as the planets circle the heavens and receive heat from the sun.

Yet, these general views on the broad arrangement and essential nature of things -- these metaphysical views -- were rarely mentioned in accounts of Harvey's scientific work. When they were, they were commonly dismissed, ridiculed, apologetically excused; alternatively, they appeared in streamlined and simplified form. Although some accounts were of course better than others, none adequately reported the intellectual condition of the metaphysical views nor captured the rich and varied roles they seemed to play in Harvey's research.

Moreover, the accounts which resulted from excusing or streamlining the metaphysics often seemed, in a curiously insidious way, to reinforce the moral that only clear and streamlined views could be useful to productive scientific thinkers.

The puzzle described above deepened into a serious and captivating intellectual problem because the inadequacy in the treatment of the broad metaphysical views and their functions infiltrated -- to a greater or lesser degree -- the works of the most careful, sophisticated, and sympathetic historians. The classic works of E. A. Burtt, Alexander Koyré, and the noted studies of their followers such as I. B. Cohen and Frances Yates are examples. Moreover, one could see the same puzzling incongruities between the apparent importance of some odd metaphysical views and reports of them in the accounts of the work of other great researchers as well -- Kepler, Galileo, and Newton to name just a few. The significance of the phenomena lies here to a large extent in the wide spread nature of the problem and the unquestionable high quality and integrity of the works which it affected. This clearly indicated that if there was a systematic distortion of events, it was not only unintentional but it illustrated the ^{role} of subtle and extremely important intellectual forces.

The investigation into the assumption called here "the assumption of clarity" is an exploration into those subtle and extremely important intellectual forces which have prevented an adequate account of the role which metaphysical views of many kinds have played in science.

(b) The Assumption of Clarity

The assumption of clarity itself is the view that all metaphysics which is influential in science must be basically clear and consistent. After the resurrection of the status and character of metaphysical views in science by Burt, Koyré, and others, the assumption of clarity is a central obstacle ~~to~~ a more accurate understanding of the role of metaphysics in science; but although pinpointed as a focus of criticism, the assumption itself represents and is supported by several important philosophical and historiographic theories which will be described later.

However, before I go on to describe the problem created by the adoption of the assumption of clarity, I want to give a fuller and clearer statement of it. The assumption of clarity includes the view that metaphysical ideas have indeed influenced scientific thought; in this respect the assumption of clarity enters the debate on relations between science and metaphysics after some connection is conceded. However, the assumption of clarity is the view that all such influential views -- views which have played a role in the development of science -- have been intellectually refined ideas. They have been clear, consistent, coherent, not confused or contradictory. This is the literal reading of the assumption.

One might add that the intent of many advocates of the assumption seems clearly to establish the claim that some metaphysical systems or views which have had a place in the history of scientific thought have been suggestive, essential, enlightening, informative rather than ridiculous and nonsensical; the assumption

of clarity is mistakenly assumed to be a message coextensive with that summary.

But, let me say more about the assumption of clarity itself. I have put forward the assumption as a philosophic view not solely as an historical one. That is, the assumption is more than the claim that the metaphysical views which have had a role in science happen to have been clear and consistent; it includes the suggestion that metaphysical views or systems which influence science would have to have been clear and consistent. Or -- in other words -- all metaphysics which has a role in a science is clear and consistent; this is the assumption of clarity.

To elaborate the assumption more fully, one must add this question: Is the assumption the view that ~~only~~ clear metaphysics influences science beneficially? Or, is it the alternative claim that only clear metaphysics has ever interacted with scientific thinking? To put this same question in another way: Does one assume -- in adopting the assumption of clarity -- that all metaphysical views which have come into the realm of science and which were helpful were in turn necessarily clear, that only the clear parts of the metaphysics were helpful, or only clear views entered what we call science or is it the view that ~~only~~ clear views were considered by scientists.

In both the above cases, the second alternative is defended by no one now -- except in as much as it can be interpreted in the sense of the first claim: That only clear metaphysics ~~is~~ helpful in the development of science. And that is what is understood here. When I state the assumption of clarity as the view that all metaphysics influential in science is clear, I can be understood to be saying, "Beneficially influential" -- or "influential in Science"

meaning "in the production of the valuable work that we honor as scientific today;" these two are equivalent for our purposes. The point is the same: all metaphysics which can be imagined to have assisted in or to have been necessary in the production of the intellectual achievement called science cannot have been confused and unclear, incoherent or inconsistent. The claim is the same in all these variants.

I must warn readers in an introductory way that the assumption of clarity is an assumption -- not often an explicit claim. As stated here, it is the articulation of a theory which seems to be assumed by many commentators -- some of whom state nearly opposite opinions--and a reconstruction which is part of an explanation: the adoption of the assumption explains a certain streamlining and smoothing of views which is otherwise left unaccounted for. But more of this below.

(c) Claims

The major claims of the dissertation are 1) that the assumption of clarity has a wide influence in debates on the relations between metaphysics and science, 2) that it is held implicitly even by commentators who would defend it if it were spelled out, 3) that it is false and thereby its adoption -- particularly its implicit adoption -- inhibits an accurate understanding of the role of metaphysics in science.

To elaborate, the assumption of clarity inhibits an accurate understanding of the role of metaphysics in science because powerful and influential metaphysical views were regularly -- as in the case of Harvey -- less clear and

consistent than they are presented to be. Even commentators who depart from the assumption of clarity, such as Burtt and Koyré, are affected by it; their own more accurate presentations of science are partly obscured because of its force in the tradition and among their followers.

To be more precise, the classic contributors Burtt and Koyré -- and also Walter Pagel in his work on Harvey -- declare themselves against the assumption of clarity in certain important respects. Nevertheless, a certain vagueness and a tempered quality in their presentations of the role of metaphysical views in science can be noticed and can be explained by the operation of the assumption of clarity. The view is extremely difficult to abandon because it is powerfully connected to valuable historiographic policies and to theories of rationality. Also, scholars who follow Burtt and Koyré -- e.g., I. B. Cohen, Francis Yates, Thomas Kuhn, J. E. McGuire and P. Rattansi, --- have consistently failed to appreciate their presentation of unrefined metaphysics and its role in the development of science because, it is argued, the assumption of clarity reasserts its obscuring influence in their understanding of these works. This influence extends both to the fact that Burtt and Koyré have presented unclear metaphysics in important connections with science and to the significance of any connection between science and metaphysics. Again, the resiliency of the assumption of clarity is testimony to its plausibility and its powerful support in the form of fruitful historiographic policies, theories of explanation, and theories of rationality.

In sum, the dissertation is in general an investigation into the question,

how do metaphysical views and scientific views interact? It is more precisely an exploration into how one particular tradition -- the bias toward clarity embodied in the assumption of clarity -- has affected debates on this issue. It is also a critical argument against the implicit influence and use of the assumption. More precisely still, it is a discussion of how and to what extent the assumption of clarity affects the works of particular writers and how it is supported by major philosophic and historiographic theories.

(d) *Outline of the Argument*

The major arguments which are developed in the exploration of the above problems and claims may be outlined as follows: Chapter I is directed largely to a discussion of the significance of the claim that the assumption of clarity affects -- in direct and indirect ways -- even the work of E. A. Burtt and Alexandre Koyré and the appreciation of that work. That is, Chapter I is directed toward revealing the uniqueness and special value of the view on science and metaphysics put forward by Burtt and Koyré; it is argued that their views cannot be reduced to traditional ideas nor to those of other contemporary philosophers such as Pierre Duhem. The general background to the question of the possible interaction between metaphysics and science comes out of this review as part of the exposition of the position on these matters put forward by Burtt and Koyré. The attempt to outline the distinctiveness of their position is an attempt to make the significance of the connection between science and metaphysics more apparent. When it is more apparent, the significance of a more radical claim in the same direction -- that is, that not only intellectually

refine metaphysics, but also confused metaphysics, has been part of science -- becomes clear. To be more precise, the significance is that a new idea of what constitutes scientific thinking and rationality is required -- one which is not required by a tempered view of a connection between science and metaphysics.

The position of Burtt and Koyré on the question of the relation between metaphysics and science is, in a word, that science develops through the guidance of and the conflict of ideas and that the most important of these ideas are metaphysical theories developed prior to the science they foster and prior to the establishment of the facts which that science explains. Both the metaphysical and the scientific theories involved are meaningful, non-tautological theories of the world -- not meaningless pseudo-views nor empty heuristic devices.

This position is presented as the most adequate and sophisticated account of the relation between metaphysics and science to date -- barring the philosophic views presented as part of the new solution below. In this introductory chapter, the value and distinctiveness of the Burtt-Koyré contribution is discussed by comparing them to other important commentators in the area. The discussion shows that the more revolutionary and important parts of their message have consistently been ignored, qualified, absorbed only partially and with hesitation.

The second chapter is an attempt to put forward an explanation of the resistance to the more radical aspects of the Burtt-Koyré position on science and metaphysics. The explanation includes an analysis of certain aspects and inadequacies both of their own presentations and in the tradition of research in

the history of science which they fostered -- not^{withstanding} the superior, indeed invaluable, quality of both. In this respect, one can mention factors such as the stress the classic authors must put on the power and comprehensiveness of the metaphysics behind science, the tendency to emphasize the absence of justification rather than any other weakness of the metaphysics, and a certain vagueness concerning the actual "nature" of the connection between metaphysics and science. All these characteristics invite or support the assumption of clarity indirectly and weaken the impact which on their own evidence they could make in establishing a role for metaphysics in science.

The discussion in Chapter II goes on to include an account of how impressive scholars who follow in the footsteps of the classic pioneers *succumb* to the weakness of their presentations in a way that can be explained by deference to the assumption of clarity. This account is, to be precise, an argument in support of the claim that the assumption of clarity is powerful and has a widespread, important effect in debates on metaphysics and science; I do not want to allege that the assumption is in it^{self} a dominant influence. I do claim that a streamlining and distorting tendency which would have to be summed up by a theory close in content to the assumption of clarity can be seen to have affected presentations and debates in this area, that this affect is important, and so is identifying it and criticizing it. The way in which these followers of Burt and Koyré manifest this tendency is explained in some detail and I hope quite clearly -- whether readers disagree or not -- in the appropriate section of Chapter II.

In addition, the Chapter II includes an explanation, in some detail, as to

why the assumption and its attendant weaknesses in historical presentation are extremely hard to surrender or avoid; they are curiously invited by the same fruitful historiographic policies which have made ~~this~~ research superior -- e.g., the policy of trying to avoid hindsight. To be clear, the explanation of how the assumption of clarity is part of the historiographic policy of avoiding hindsight is part of an argument to show that the assumption infiltrates the historiographic tradition. The details of this explanation appear in Section 2.3 and 2.4.

To speak again to the significance of these arguments, the policy of avoiding hindsight is widespread and honored; this is fairly uncontroversial. Thus, connecting the assumption of clarity with that policy shows that it is widespread; the possibility that this widespread assumption of clarity is a distortion as well as part of a fruitful policy makes pointing this out significant -- as obviously significant as any argument which shows that in accepting what we thought was a boon, we are accepting a disadvantage at the same time without quite realizing it.

Chapter III is an extended historical case study of the incongruency between the character of Harvey's metaphysical views, their seemingly intimate and essential functioning in his scientific research, and the hostile, apologetic, and/or incomplete accounts of both in historical and philosophic literature. To be clear, this chapter presents evidence for the conclusion that the assumption of clarity is false and that its adoption distorts presentations of scientific achievement. The evidence consists -- as explained at some length in this chapter -- of chronic failure to note inconsistencies in his views, neglect of the credit he

himself gives to his metaphysics, his constant references to what we call metaphysical views, neglect of conflicts and failings which can be explained with reference to his metaphysical position -- and the appearance of some of these failings in even the more sophisticated historical commentators. This evidence is part of an argument that some powerful view is operating; and a reasonable though mistaken deference to the assumption of clarity provides an explanation. To summarize the chapter in other words which may make its argumentative function more clear.

Although historical examples appear in other sections of the investigation, the Harvey review is intended to illustrate more thoroughly the multifaceted role of metaphysics of all kinds in science -- a role which is not limited, e.g., to irrational inspiration. It also is designed to show that the intellectual condition of Harvey's metaphysics cannot be adequately represented in any presentation of his science which uses a streamlined or refined version of his actual views. The precise arguments are, among others, that if that is attempted, many of Harvey's problems, many of the more exciting episodes in his scientific career, and many of his most profound achievements, have to be overlooked or distorted.

Chapter IV is a presentation of arguments concerning the philosophical traditions and positions which support the assumption of clarity and reinforce its armour of reasonability. To be clear, the general argument is that strong philosophic views -- connected with rationality -- reinforce the view that any productive thinker must, in his productive capacity, be clear. This view is questioned here; it is in fact denied. But, the significance of the argument lies in making the position explicit and in showing that it has often been

adopted implicitly, not merely in arguing that it is false. A brief survey of traditional views on science and rationality is given to bring out particularly the common identification of both with clarity and to note the strong (and reasonable even if mistaken) bias against confused ideas. This strong and consistent tradition supports the assumption of clarity by making it appear so obviously reasonable that it is not often articulated or criticized. This general philosophic bias against confusion and the reasonable, though not critical, preference for clear ideas is followed into the works of contemporary writers on science and rationality. Even there it can be seen that the most liberal identify reason and clarity and indicate -- when they do not claim outright or consistently -- that confused ideas do not have a place in science. This is another argument which supports the view that the assumption of clarity is powerful and widespread in the philosophic tradition.

The argument is continued via an examination of the vagueness exhibited in the work of the most sophisticated commentators on the precise character of the relations between metaphysical views and scientific ones. It is argued that this vagueness -- like the assumption of clarity -- represents an attempt to attenuate the theoretical consequences of establishing a connection between metaphysics and science. The way a retreat to this vagueness and to the assumption of clarity can be combined in various permutations in order to soften or avoid the conflict between an interaction and the accepted theories of rationality is described in outline. To be precise, it is argued that the vagueness exhibited can be accounted for -- in these articulate and careful commentators -- by the reasonable -- if mistaken -- determination not to come into

conflict with most present theories of rationality; theories which also support the assumption of clarity. In sum, the reviews together with citations from various philosophers support the conclusion that no commentator leaves room for a rational connection between a confused metaphysics and proper scientific thinking.

Finally, the dissertation includes the outline of an alternative view of the relations between science and metaphysics which does not depend on the assumption of clarity. The theory is an augmented version of a position put forward by Joseph Agassi -- a position which is itself a critical revision of Popper's theory of rationality. In this new view, the relation between science and metaphysics is critical interaction; interaction to outline and focus problems; sometimes to clarify; sometimes to refute or make unsatisfactory. Each discipline -- science and metaphysics -- plays an important role in the rational debate which comprises the other; the rationality of each intellectual enterprise often depends, in fact, on taking account of views and arguments from the other. That is, on the one hand, debates on metaphysical issues often focus interest on certain problems in science, thus promoting a concentration of effort which makes criticism possible. Moreover, scientific theories are often developed because they are views which reflect a metaphysical position important in such debates; the metaphysical conflict provides not only motivation but, material by which to interpret theory, develop criticism and judge criticism. On the other hand, metaphysical frameworks and views provide only partial interpretations of theories and incomplete standards of criticism; thus, science develops independently to a significant degree. The development may show the

inadequacy of some metaphysics by involving the refutation of a metaphysically-interpreted scientific theory and it may show the potential of other world views through the fruitfulness of theories which partially instantiate them. Thus, science is part of the critical, rational debate in metaphysics just as metaphysics has an important function in the rational conduct of science. Confused metaphysical frameworks or views may and have played all these roles; the rational endeavor of many thinkers has included the struggle to refine and clarify the metaphysics they used at the same time that they used it in the ways described. If this view is accurate, rationality is more comprehensive and less fragile than tradition has allowed.

CHAPTER 1. THE CONTEXT OF CONTEMPORARY DEBATE ON THE
RELATION BETWEEN SCIENCE AND METAPHYSICS.

Chapter I. The Context of Contemporary Debates on Relations Between Science and Metaphysics

Any investigation which deals, as this one does, with broad areas of great intellectual interest will require some preliminary discussions before the major claims and positions can be appreciated. Here the question is, how do metaphysical views--and confused metaphysical views in particular--function in science? The subjects involved--science, metaphysics, the importance of clear thinking, and the possible rationality of confused ideas are very differently understood in different contexts. Hence, how they are used in any investigation will affect its orientation and result.

Some of the required preliminaries are presented directly in this introductory chapter--e.g. see the sections on the "Burr-Koyré" position and the "Metaphysics and Confused Metaphysics". But, in the main, the issue is clarified and meanings are restricted through a presentation of the background and distinguishing features of the work of two writers--the philosopher E. A. Burr and the historian Alexand Koyré. The view of science and its relation to metaphysics which is developed in their works is taken here to be one of the most accurate and sophisticated answers in the literature to the question at hand. Thus, the Burr-Koyré view is taken also as the point of departure for the rest of the argument and the context of the investigation is spelled out through a discussion of the problem situation which makes their position valuable and superior.

The major claims made in this chapter are that Burr and Koyré have developed a position on science and metaphysics which is revolutionary in its

stress on metaphysics as simultaneously speculative, non-tautological, and at the same time helpful, even essential, in development of classical science. The superiority of their views is not often appreciated to the full. Later, it is argued that the implicit adoption of the assumption of clarity--that all influential metaphysical views must be clear and consistent--is responsible to a significant degree for the chronic underestimation of their message. But only when the merit and distinctiveness of the Burtt-Koyré position is understood does the fact that it is partly undermined by the assumption of clarity appear significant.

Of course, any investigation into relations between science and metaphysics would have to make some room for the classic works of Burtt and Koyré. They are often spoken of jointly as groundbreakers who revolutionized the history of science and reinforced the move away from a narrow vision of both science and of metaphysics which was ensconced in the philosophic tradition of positivism. It is also widely acknowledged that each made his mark in these areas by revealing with great historical empathy and skill the major role of ideas--particularly of broad, conjectural, metaphysical ideas--in the development of the classical scientific system of the 17th century.

Even the name of Burtt's renowned study, The Metaphysical Foundations of Modern Science, indicates its relevance to the central question at hand--how do metaphysics and science relate to each other? Koyré's major works present themselves superficially as basically historical researches. For example, consider Études Galiléennes, Newtonian Studies, and The Astronomical Revolution. However, they are persuasively presented critical interpretations of

historical events; they deal in an argumentative manner with the importance of metaphysical ideas in scientific development. The title of the collected essays, Metaphysics and Measurement, reflects this orientation.

To summarize, many commentators--historians and philosophers alike--credit the pioneering works of these two authors with establishing a new standard in historical appreciation mainly by rescuing and celebrating the importance of theoretical, metaphysical ideas in the development of science. Thus, their place in any reasonably comprehensive study of relations between science and metaphysics would be assured even were they not considered representatives of a superior view as they are here.

However, there is an additional and rather curious reason why the view of Burtt and Koyré has received special attention in the present study; to wit, their contribution is far more often alluded to and honorably mentioned than articulated or discussed. Characterizations of the position they have developed on the nature of science and its relation to metaphysics are very rare, largely superficial, and inadequate--particularly in distinguishing their contribution from that of other important figures in the field, notably the French philosopher Pierre Duhem. Duhem, as professional readers will know, is also noted for declaring science unavoidably and basically theoretical.

One could almost put forward as a universal truth that an influential and honored view which is not stated and only formally discussed is both a gold mine of interesting problems and is quite chronically undervalued. Both judgments apply to what is put forward below as the "Burtt-Koyré" position. Exploring this largely unexamined position allows one to spell out explicitly

many, conditions which mark an adequate theory of relations between science and metaphysics which are now only marginally deferred to through the formal notice taken of their work. The role assumptions, such as the assumption of the essential clarity and coherence of influential metaphysical views, in the failure to appreciate the distinctive merits of the Burtt-Koyré position also begins to become apparent. In all these ways, the background of the Burtt-Koyré contribution is the background to the philosophic debate at hand.

Section 1.1. A Note on "Burtt-Koyré"

The position on relations between science and metaphysics which is referred to in this study as the "Burtt-Koyré view" may be summarized as follows:

Science is an enterprise motivated and guided by ideas and the conflict of ideas--not by economic forces, practical needs, facts, or experience. The ideas most influential in the creation of scientific systems are broad, powerful metaphysical theories, i.e. speculations concerning the true nature of the world--as opposed to previously collected reports of empirical data or established empirical theory. Scientific views are developed which incorporate the language and ideas of the metaphysical framework and which conform to it in some way. Both the metaphysical views and the scientific ones are considered meaningful, contentful theories about the world--not pseudo-expressions nor empty heuristic devices.

Sensitive readers may yet find it curious that a study on relations between science and metaphysics should so prominently feature and associate the works of these two writers, Burtt and Koyré. Burtt and Koyré did not work together; neither is in the mainstream of contemporary philosophy of science; Koyré is known in fact as an historian. Although their works are often associated, the practice is by no means universal. In fact, many scholars who know one quite intimately might not mention the other. Koyré, for example, has disciples in the history of science who do not acknowledge the work of Burtt. Moreover, each author has attitudes and opinions on the matters at hand which are not shared by the other. For example, Burtt shares some of Pierre Duhem's tendency to see metaphysical views as methodological policies; whereas Koyré emphasizes the conflict of ideas more than Burtt in describing the development of science in interaction with metaphysics. For all these reasons, it may be

somewhat misleading to refer to the "Burtt-Koyré" position.

In support of the policy, let me say straightaway that the "Burtt-Koyré" position is an artificial creation, one of the contributions of the present study. As explained below, neither Burtt nor Koyré nor their many followers articulate the position cited above. That these views are the distinctive features which make their works valuable is partly a discovery and partly a claim which is, again at least in part, the product of the present investigation. Credit must be given to the philosophers Popper and Agassi, as well. (This is done in Chapters II, IV.)

The fact that this study has had its impetus in some interesting incongruities in the history of science--the work of William Harvey in particular--partly explains the concentration on Burtt and Koyré and the association of their work. Both Burtt and Koyré deal extensively--almost exclusively--with historical matters. They have expanded the research which illuminated Copernicus' devotion to circular motion, Galileo's assumption of the geometrical nature of things; Kepler's sense of the propriety of motion circumscribed around conic sections. These theories, like the views on the connection between the body and the world which influenced William Harvey in his scientific endeavors, seemed more important than they were traditionally acknowledged to be.

Moreover, the views of Burtt and Koyré on science and its relation to metaphysics are indeed similar enough that they share the position described above. Also, their works made an impact at the same time and in the same direction--namely, away from positivist views of science as defined solely by empirical, positive, collections of data or heuristic generalizations. One can

add that although their studies are now classics, none of their many successors has developed views of science as ^wpowerful as that which can be abstracted from their works and which is summarized above.

Section 1.2. A Note on "Metaphysics" and "Confused Metaphysics".

Philosophers understand different things under the category "metaphysics". Pierre Duhem, for example, understands as metaphysical any theoretical view which purports to describe essence of things in the real world; in Kant's work, "metaphysics" refers in the main to views about God and the soul, whereas theories concerning absolute space and causality which we would regard as metaphysical are considered by him the fundamental axioms of science. For the contemporary commentator Thomas Kuhn, metaphysics may mean any view which functions instrumentally or heuristically and thus not realistically; whereas, for Koyré the category indicates exactly the opposite role. In some of the discussions concerning Kaballistic metaphysics in Chapter II, metaphysics is a category which includes pseudo-ethical views concerning the necessity for purification as a precursor of enlightenment. The philosopher of science Karl Popper is now both famous and infamous for considering metaphysical any isolated singular statement affirming existence.

For the most part, these differences in categorization are discussed as they arise; and, in other cases the context makes the meaning of the term "metaphysical" unproblematic. In no case, I would argue, does a point rest on an equivocation about it. When not under discussion, metaphysics may be understood in the traditional sense to refer to theories about the world: the organization of the universe, the structure of and character of existing things, the character of the laws by which events are governed, and variations on such subjects.

As examples of such traditional metaphysical views, one may mention the

Aristotelian theory of substance, the Platonic theory of Forms, Thales' view that all is water, or Descartes' theory that the world is made of three different substances--body, mind, and God. The theory that the world could not have been created from nothing, the (Keplerian) idea that the stars would have to be placed isometrically in an infinite universe, Descartes' view that all change of motion results from action by contact, all are classical metaphysical positions.

William Harvey adopted an Aristotelian metaphysical preference for certain hierarchical arrangements in the body; e.g., he preferred to see the heart, as the source of heat, in the center of the bodily functions and therefore disliked the theory of his predecessor Galen which featured a concentration on the liver.

Metaphysical theories on the character of knowledge^d itself may also be included in the category "metaphysics" since they were traditional and since such views often do contain reflections on the structure of the world.

These distinguishing marks of all these views are that they are substantive speculations concerning the world--synthetic not analytic in the traditional sense--and they are speculations--neither derived from an authoritative source nor verifiable, not subject to empirical test in fact.

The question, what is the difference between metaphysics and science may appear to give some difficulty since both metaphysics and science are taken by some philosophers to be non-tautological speculations about the world. Each may be judged true or false. However, the criterion used to distinguish them here--namely, that metaphysical views are not now in fact testable--is sufficient for clarity. It may prove inadequate in some more definite or elaborate con-

texts, but they would have to be spelled out.²

Neither should it be necessary to stipulate why the metaphysical views discussed here are indeed metaphysical in the sense of not subject to empirical test. Perhaps they are too vague, perhaps they are not embedded in a sufficiently-developed theoretical system to allow tests to be carried out, perhaps they function more as methodological rules or ethical principles than as statements about the world so that tests of them are not developed, although they could be. If they are developed, but ignored, then one can speak of a scientific theory which is held as a dogma or which is put forward as a conventional classification; one need not call such basic hard-core theories metaphysical simply because tests are not regarded.³

Likewise, the question, can there be a science without a metaphysics, is only marginally important here in spite of its general interest and relevance since the one says yes or no, theories of the interaction between science and metaphysics remain both important and problematic. Although we may agree that there is no science without metaphysics, that does not mean that former thinkers held this opinion; nor that when developed, the idea is compatible with other views we have of science--with theories of explanation, for instance; nor does it enable us to describe the relation between the two.

In fact, I do think that there can be a science without metaphysics although the investigation here stresses the significance and the interesting problems connected with other scientific developments. Nevertheless, Pierre Duhem, for one, describes the various theories--formulae--which allow for prediction of the paths of light rays; they developed and changed in part and could be

"understood as descriptions of the paths of light rays without postulating any metaphysical view or even theoretical explanation of what a light ray is."⁴ To come at this from another angle, there may be many scientific generalizations and effects which would not be noticed except for a background metaphysical problem but which can not be stated or explained as part of a world view until a new metaphysics is developed. Some of the experimental results Faraday achieved come in this category according to Joseph Agassi.⁵ One has to call Faraday's results scientific; but is this science with a metaphysics or without one?

One could answer yes, this is definitely science with a metaphysics, but the sense of this declaration differs markedly from any traditional acknowledgment of the relation; traditionally, metaphysics dictated or determined science; there is no idea or metaphysical conflict encouraging science as criterion. However, Agassi mentions several important areas of scientific work which are not dictated by metaphysical views but focused upon because of metaphysical problems in the area. Not every major concentration and achievement in science is the result of such background metaphysical debates, he warns; but many are. I think it should also be clear in what follows that I do not claim that all good science has metaphysical sides much less that all good science has a confused metaphysical background--just that some can and has.⁶

There are some theories which will be referred to as metaphysical in the dissertation which some commentators would not classify as metaphysical.

These are views which combine ethical, epistemological and cosmological positions such as the theory that all mistakes are the result of conspiracies reflecting the sin of hubris. It is of some importance that such theories not be excluded apriori from the arguments concerning relations between metaphysics and science; or, at least, it will be necessary not to artificially prune the system which contains them before they are considered.

Neither will the metaphysical views referred to in the discussions below be limited to those mentioned in the more technical studies of ontology or theory of explanation which have occupied many twentieth century philosophers of science.⁷ They will feature broad cosmological theories--not refined, mathematical or logical positions. The debate on the status and interpretation of theoretical entities in scientific systems undertaken in a number of works by positivists and logical empiricists from Mach to Hemple is a debate which overlaps the work of Burt and Koyré; but, it is not the same as the one they undertake. The debate on theoretical entities is more abstract and refined; it is not conducted in an historical setting.

Moreover, Burt and Koyré consider metaphysical theories and systems--not entities and their nature; the systems with which they are concerned are explanatory not essentialistic or definite and in force and they are far more broadly interpreted than the theoretical entities which disturb some modern commentators. Finally, the more positivistically oriented debate on theoretical entities does not often directly confront the possibility that the theories involved are parts of metaphysical systems. The point is that a resolution of the debate on the status of theoretical entities would do nothing to resolve the

problem of the influence on science of the cosmological systems which Burtt and Koyre discuss because this former debate takes an even more refined starting position for metaphysics than Burtt and Koyré do. The "theoretical" aspect of theoretical entities is often never referred to as metaphysical at all.⁸

To emphasize the general point, the view of metaphysics upheld here on behalf of Burtt and Koyre is a more historical, broad view; metaphysics as they understand it includes ontology, cosmology, some epistemology, at the very least. The view of metaphysics upheld by many twentieth century philosophers of science is a far more restricted one. It is confined almost entirely to ontology and to limited questions concerning ontological status. One can wonder in what way is this difference important? The point is this: it is not the redefinition of metaphysics as a broad cosmology rather than as refined ontology which makes it possible for Burtt and Koyré to argue that broad metaphysical systems influence science in important ways. (If anything, the broader definition makes their claim more difficult to prove.) Rather it is the further argument based on historical evidence and debate which gives power to their position. The claim they make is interpreted substantively by insisting, as Burtt and Koyré do, that definitions apart, the views they take as metaphysical are the views which have been historically important in major scientific developments--not the refined ontological position discussed by modern theorists on the empirical side nor the refined, but empty mathematical views pushed by Duhem and his followers. It was the fully developed metaphysical systems which encompassed cosmology, ontology, epistemology, and other fields of thought which affected scientific development. Thus, settling

the weaker claim concerning theoretical and mathematical entities would not settle the stronger claim for the influence of such broad metaphysics; the debate is not the same.

In the same way, in making the claim in later chapters that even confused metaphysical views influence scientific developments benignly, I do not merely introduce a broader notion of metaphysics, nor am I simply relying on, or simply pointing out, the fact that many of the thinkers discussed had a broader notion of metaphysics than many philosophers do now. Nor, in fact, is it simply the case that because they had a broader notion of metaphysics, it is easy to claim that what they called metaphysics was influential and that it was confused. Rather, I am saying that whatever one wants to call such views, it was not only those views which were broadly metaphysical in the sense of cosmological, ontological, epistemological and/or ethical that influenced science, but also confused views of the same broad sort. I move another step away from refinement--a step farther than Burt and Koyré have been understood to make--and they are in their turn a step farther removed from a refined idea of metaphysics than many twentieth century modern commentators. In all cases the important claim is that that kind of metaphysics, the kind designated, was important and influential--not a more refined kind.⁹

To say a few more clarifying remarks on the subject of confused metaphysics: A view is not confused just because there are many interpretations of it--cosmological, ontological, ethical, etc. It is often confused if, for example, the different interpretations are not held distinct. That is, an interpretation of atomism as a theory about physical matter is a distinct theory from an atomistic psychological theory. If one calls both theories atomic or even thinks that two interpretations, one physical and one psychological, have some underlying identity which makes them connected without spelling out how--then ones ideas may well become confused.

One might suspect that the categorization of ideas into ethical epistemological, ontological--is a serious problem; that is, perhaps one needs some secure categories in order to talk about views which can indeed be separated into them and interpreted in many different levels. But, the categorization is not problematic if nothing depends on it. One need not, for example, claim that there are views which are entirely ontological but not in any sense epistemological; one can even claim that all ontological views have epistemological consequences or collolaries and still have unproblematic categories. In the cases here, for example--as with the microcosm-macrocosm theory which William Harvey believed--its categorization as partly ethical, partly epistemological partly metaphysical in the sense of cosmology, is traditional.

I can add that a theory may be more powerful if it can be interpreted on many levels as I say many "confused" views discussed below were. The theory of gravitation, for example, was considered more powerful when it was found to

apply not only to planets but to bodies on earth and in fact to any masses then discovered. This, one may say, is a theory interpreted on many levels; but one means it applies literally in many circumstances--or is a broadly interpreted theory. Even on present criteria, this is a wholly admirable characteristic. The coherence of levels achieved by the metaphysical views discussed in the dissertation as confused does not approximate this kind of coherence; the application of the theories cannot be carried out literally or thoroughly in the various levels. The microcosm-macrocosm theory only vaguely or inconsistently can be applied to bodies, seasons, kingships, and heavens; Kepler's unification by means of physical forces, harmonies, and theological rightness, also can only be pushed in a confused and non-literal fashion. Nevertheless, such theories can be powerful and the power is not unrelated to this coherence of levels--epistemological, ontological and cosmological or whatever. Most of them, in my view, gain great power partly from the (mistaken) sense of universality they offer and the (mistaken) suggestion that they have a coherence of levels, and can actually be connected explicitly as in the gravitational theory. In summary, I think a theory which can be interpreted on many levels is often indeed considered more powerful and it may be more powerful in many cases; but that does not mean it meets present standards of clarity or explicitness. To separate these is important for the purposes here. I claim below, for example, that it was the power of the microcosm-macrocosm metaphysics popular in the Renaissance which explains its wide effect on important thinkers: its power and not its intellectual propriety was enticing.¹⁰

On confusion generally, the point made not that a metaphysics must be

clear in order to have a rational connection with science. Rather the point is that many traditional views on rationality include this condition--which is why the assumption of clarity is so difficult to drop: one must choose between it and the assumption of irrationality. My point is nearly the opposite here: that one cannot assume that rationality requires clarity, that this assumption is uncritical, too limiting, and in fact, false.

As to the further question, what is a rational connection between science and metaphysics, in other chapters I give alternate views of what are at least the necessary conditions for the rationality of any connection or influence of one view and another. Many traditional views imply that the only rational connection is one of deductive entailment of one view by another. The advanced theorist Karl Popper includes contradiction: two views are rationally related if one is logically incompatible with the other. His followers Agassi and Bartley say that two views can be rationally related if one is a question and the other an answer. My point in this introduction is merely to point out that on almost any criterion one can articulate or report now, it is difficult to say that scientific views and confused metaphysical views can be part of the same discussion or that any discussion with such elements can be a rational debate.¹¹

As to the question, is confusion attached to metaphysics as an inherent property of the system or it is relative - one can avoid both conclusions: it is a matter of fact whether a view or person is confused--I hardly know what to say about a system which would be "inherently" confused instead of "in fact" confused.

To restate this in summary, no categorization itself is of importance; one

could easily call the theory of original sin an ethical or religious theory rather than a metaphysical theory or part of a metaphysical system, one could call it confused or unconfused. Then, however, the argument presented would have to be rephrased to accurately represent the issue. One could say, for example, that Koyré's work presents science as partly mixed with and directed by metaphysical views which were adulterated with eclectic components of a vague and confused sort as described above. But, then one must add that the general impact of his work does not reflect this claim in spite of his best efforts: viz. that no such adulterating elements affect the metaphysics and the science involved.

This lack of impact is the result of implicit and uncritical deference to the assumption of clarity--that all metaphysics influential in science must be basically clear and coherent--on the part of Burtt and Koyré themselves to some degree--as well as by their successors. And, although extremely reasonable, even seemingly inescapable, the assumption of clarity is false and misleading. This is the major position taken there with respect to the effect of the assumption of clarity on theories of the relation of the affect of the assumption of clarity on theories of the relation of metaphysics and science. Whether one calls the views which adulteratize the influential metaphysics "metaphysical" or not does not change the issue. One can call them "confused" or not, as long as they are identified and noted as the kind of views previously excluded from science on theoretical grounds.

Section 1.3 The Distinctive Burt-Koyré Contribution.

The connection between science and metaphysics is often taken to be obvious and not particularly significant nowadays. Discounting those commentators who would mention a connection between science and metaphysics only to dismiss the relation as a source of error and pseudo-science, one may still conclude that a great many thinkers now take the connection as an obvious matter of fact. As a consequence, Burt and Koyré hardly seem unique in their view that there is an important interaction between science and metaphysics; moreover, the claim does not seem remarkable, unique or not.

The force of the main arguments and theses presented below will remain obscure until the distinctive character and value of the Burt-Koyré position on relation between metaphysics and science is recognized, at least as it is here understood. In this connection, one must note that although many prominent philosophers and historians of science in recent times have noted a close connection between science and metaphysics, yet it is not easy to place these comments in any single framework.

A commentator may stress a close relationship between metaphysical views and scientific ones for a number of different reasons. For example, one may wish to emphasize the fact that views taken to be metaphysical, such as Medieval scholastic tenets, are part of a continuous tradition which leads to scientific theories, as Pierre Duhem has done. Alternatively, one may note the similarity between theories of the two types in order to point out that scientific theories develop under the influence of metaphysical views, as Burt and Koyré

argue. With different concerns altogether, one may stress a similarity in order to make a case for the ultimate unverifiability of scientific theories, as Thomas Kuhn often does; or, with the opposing purpose to argue that a clear criterion of falsifiability rather than verifiability is needed to separate science from other disciplines, which is Karl Popper's aim. All the authors mentioned may appear to agree that there is a close connection between metaphysics and theoretical science; yet, each theory of the relation, the question the theory answers, the function it plays in the philosophy of the individual thinker is different from its counterpart in another thinker's view. The point is that these commentators who all seem to share the innocuous opinion that science and metaphysics are connected, quickly become vigorous opponents when any detail of their views is exposed. Thus, one must go into the details of such positions to reveal the character and strength of the debate and the Burtt-Koyré contribution to it, when possible, I have tried to use the general categorizations consistently and in as traditional a manner as possible in the presentation of positions on metaphysics. For example, an inductivist is someone who is an empiricist and who thinks scientific theories are true and that the truth is either received or validated by an inductive procedure. This procedure might be the process of collecting facts with an unprejudiced mind; or it might be the process of basing theories on facts through some logical association. A characterization of an inductivist can be created which is more specific and definite than this --here, in the tradition, or nearly any discussion one could name.¹² But the categorization is specific and consistent enough for the purposes at hand; namely, the inductivist can be distinguished from

the theorist *who* held that science was validated by some source prior to, or in other ways independent of, the facts, here called the apriorist. He can also be distinguished from the theorist who holds that scientific theories--no matter what their method of discovery or validation--do not reach the status of "true" but merely of "useful" or "beautiful"; here called the conventionalist.

As noted, these epithets are not used idiosyncratically and as long as they are *critically as well*, it does not matter that their scope of reliability is limited.

However, there is one term among the commonly used ones here which is important and which may be used in a way which borders on the novel or the idiosyncratic and which therefore requires additional comment; that is the category "realist". Readers will see that I think Koyré, and Burtt to some extent, describe people acting in a way that makes them "realists" even though the actions do not fit traditional rubrics. For example, thinkers are presented as scientists and religious thinkers and metaphysicians, but they seek the truth in science in all these capacities.

One could say a realist is one who presents his theories as true or false and takes reasonable steps to show which they are. One could also say a realist is simply someone that actually holds what it sounds as if he holds judging from his assertions. For example, Copernicus is a realist if he believes the sun actually does sit in the center of the world and planets actually do circle it; a Medieval Aristotelian is a realist if when he speaks of the realm above the moon as perfect and unchangeable he means that it is unchanging

and perfect. If either hold these views as poetic analogies, imaginary models which are convenient, classification devices, or methods of prediction, then they are not taking a realist stand.

All this is quite traditional but for the claim concerning Burtt and Koyré's historical descriptions. The claim is that they describe people breaking, ~~that~~ given rules for being a realist, but being "realists" all the same. I am in part exploring through them what it means to be a bona fide realist--someone who makes a reasonable attempt to explain the world truly. Thus, here the word realist is sometimes used as a convenience meaning not a conventionalist, not a positivist, not a traditional realist, but some thing more, a kid of realist yet to be described; other times the word has the less innovative conventional meaning.

(a) Burtt and Koyré contrasted to historians of ideas.

The works of Burtt and Koyré may easily be seen to be part of the Lovejoy tradition¹³ in history of ideas, and this association can contribute to an under-estimation of the philosophic contribution which, it is held here, their work makes. To philosophers who stress the a-historical nature of the discipline, the strong concentration on historical events in the works of Burtt and Koyré seem clearly to distinguish them from the mainstream of philosophy. Yet, they deal with concepts of philosophic importance;¹⁴ so it is difficult to classify them among general historians. "Historian of ideas" seems a good alternative. The problem is that the argumentative and abstract philosophic potential of their works

tends not to be appreciated under the classification, "historian of ideas".

On the other hand, historians of science of any traditional bent and other commentators on science per se, see that Koyré and Burtt do not interest themselves solely in those aspects of history, even of intellectual history, which are clearly marked off to be scientific. For example, Koyré discusses Kepler's arguments against the infinity of space based on the necessary isotropism of such an infinity and on the proper arrangement of the heavenly bodies based on their necessary relations to conic sections Burtt discusses Galileo's view of the tides. The historian Walter Pagel who follows in their footsteps spends large sections of his book on Harvey tracing the historical career of the microcosm-macrocosm theory. None of the views discussed are part of the modern scientific view on the subjects at hand. Most are not even considered scientific by these historians in the time they were developed.¹⁵

Thomas Kuhn describes the traditional orientation in doing history of science as the wish to discover, illuminate, or celebrate true science, and/or its method:

Condorcet and Comte are only the most famous of the philosophically inclined writers who, following Bacon's lead, attempted to base narrative descriptions of true rationality on historical surveys of Western scientific thought.¹⁶

Both propagantists for science and amateurs pursued this type of history which did not concern itself with any aspect of events except those clearly understood to relate to successful scientific activity as judged by modern standards; no failures or blind alleys, sometimes no ideas as opposed to discoveries of fact, were explored--except as objects of ridicule or as obstructions. The historical context or contemporary understanding of ideas was considered irrelevant.¹⁷

As examples of such historians one could mention Charles Singer, and Henry

Guerlac--the latter is still considered an eminent contributor to the field. Joseph Agassi, in an extended discussion of such matters, uses Douglas MacKie as a paradigm case of an historian of science who understands science--and therefore its history--to concern only those views which have reached the modern text.¹⁸

To such historians of science or to those who saw history of science as that sort of activity, the work of Burtt and Koyré seemed out of place. They deal with scientists but they are clearly concerned to capture some of the actual historical character of the period--at least in its intellectual aspect. As just noted, this historical tenor may overwhelm the analytic message--which concerns "science"--for philosophic observers. I. B. Cohen the historian, in his eulogy of Koyré, notes that a characteristic of his work is its "stress upon scientific ideas, understood in their own terms and in relation to the living background in which they were imbedded".¹⁹ They often deal with ideas that were failures and constituted by-ways in the development of science.²⁰

Thus, to observers, Burtt and Koyré may not seem to be studying "science" but something more general; and the "history of ideas" is a designation which seems made to order. They do seem to be doing the history of ideas, not the history of ideas specific to science. This summary of their work, of course, misses its central point, viz. that science is more the history of these other ideas than the traditional history of science with its stress upon fact, techniques, or modern notions, would suggest. Koyré says as much in a passage which gives his argumentative position:

This scientific and philosophic revolution--it is indeed impossible to separate the philosophical from the purely scientific aspects of the process: they are interdependent and closely linked together...²¹

Koyré refers of course to the revolution of the seventeenth century which may again suggest placing him among historians of ideas.

However, to historians generally, and to some historians of science of a more modern variety as well, Koyré and Burtt are far too philosophical to be considered standard representatives of their discipline. They do not concern themselves with the detailed texture of historical, activity nor with what intellectual discoveries or movements meant to the mass of people. Burtt and Koyré deal only with certain intellectual events in history and only with certain intellectual participants in the events.²² Thus again, from a different angle, they seem to be historians of ideas not historians or historians of science.

The label, historian of ideas, unfortunately seems to suggest uncontroversial work or an uncontroversial, non-argumentative position at least to some ears. To explore this somewhat, Crane Brinton characterizes the Lovejoy school of intellectual historians as those:

Concerned with what can be called the cartography of ideas or (perhaps) semantics. ... Lovejoy identified as a "cluster of ideas" such very common terms as "nature", "reason", "Romantic". His major task was that of analyzing the constituent elements of these clusters of ideas.²³

Although Brinton does contrast this type of intellectual history with the more documentarian type concerned with establishing the existence, location, and publication of "intellectual" sources, he adds that even this slightly more adventurous work is "concerned rather more with establishing than with evaluating or synthesizing facts".²⁴

I. B. Cohen conspires in the conflation of Koyré's work with that of less controversially-minded historians by referring to Koyré's historical method as that of

"conceptual analysis".²⁵ Cohen follows Koyré and Kuhn in naming the philosophers Meyerson and Cassirer as the predecessors of this approach or "method", not any historian of ideas. Still, the overtones behind the characterization suggest a subdued point.

Moreover, Cohen's evaluation of the scope and force of Koyré's work is somewhat unsteady even though there is no doubt he did value it genuinely. He describes the change made in the history of science by Koyré as "radical" while referring to his new "approach" which others initiated and which Koyré simply used more thoroughly and with better examples; and he calls the contribution a new "emphasis."²⁶

Henry Guerlac shows a similar lack of consistency in his praises. He notes that Burtt and Koyré,

go farther [than Duhem] and demonstrated the close relationship, during the formative era of modern science, between science and religious thought.²⁷

This is not much for an "immense" contribution and a "great debt". In both the above cases--Cohen and Guerlac--one suspects the commentators have felt the impact of Burtt and of Koyré more than their explicit acknowledgements suggest. The oversight at hand here can be illuminated by mentioning a few examples of differing interpretations. Duhem reported a close relationship between Medieval religious scholarship and late Renaissance scientific thought. He mentioned the theories concerning the impossibility of creation from nothing and the theory of motion as impetus. He claimed that such ideas--developed in a religious framework--were developed quite continuously through to Galileo. Burtt also emphasizes that views connected with religious discussion--e.g. substance--were interwoven in physical views but, he sees the new metaphysics as conflicting with the old, There is a religious framework but a

new, replacement "religion" completely incompatible with the old. This is not "going farther".

However, both Burtt and Koyré contribute to this evaluation of themselves as unpretentious analysts, at least to some extent. Koyré describes his Newtonian Studies as the attempt,

to define the structural patterns of the old and the new world views...their central theme is illustration by means of conceptual analysis of the way in which fundamental scientific ideas are at the same time related to the main currents of philosophizing and determined by empirical controls.²⁸

Indeed, in a few pages of detailed and illuminating discussion of the differences in his concern with the history of science from that of others, Koyré puts off "explanations" with some force saying that the changes in outlook must be described and understood before they can be explained.²⁹ He adds a remark which shows he thinks such an explanation might not be a coherent goal in any case, so the remarks must be taken with a grain of salt.

All the above representations, one may argue, give the Burtt-Koyré programs much less credit for its argumentative power and potential than it deserves. Both Burtt and Koyré, as Agassi for one has argued at length,³⁰ develop a position on the changes in world view in the seventeenth century which they mean to be competitors to other, specified, interpretations. In the same way, much of Koyré's work and many of his most careful historical "analyses", are not attempts to "define" any pattern in isolation from a definite problem or controversy; they are pointedly directed refutation of rival views--particularly those of the positivist and Marxist sociologists--on the factors important in the development of science. A glance at the first pages of the works in question reveals this immediate awareness of controversy.

Whether the historian of ideas is philosophically underdeveloped or philosophically overdeveloped is a curiously variable judgment. Whereas philosophers may see such studies as too pedestrian, with insights confined to historical periods remote from modern problems, other commentators such as Ernan McMullin, see this same history of ideas as too interpretive and creative rather than genuinely, historically bound:

The history of ideas can easily become a logical and analogical development whose dynamism lies in the ideas themselves and in the creativity of the person constructing it, rather than in the partial records of the free decisions and semi-opaque mental constructions of men long dead. The connectives of history are not always those of logic or analogy.³¹

These remarks are intriguing; at first blush they seem eminently sensible and perceptive; but one soon begins to wonder if there are, in fact, connectives of history which are not "of logic or analogy" or something equally abstract. The question involves discussion of history as written theory or as past events which are taken up again below; it also involves the question of what other sorts of connectives could be of interest intellectually--a question which appears repeatedly throughout these discussions.

McMullin's remark makes it clear that even should Burtt and Koyré be deemed historians rather than philosophers, their position *vs. a vis. an* interpretive and argumentative comment would not be clear. However, enough has been said here to indicate that Burtt and Koyré are not taken to be historians of ideas in the sense of uncontroversial analysers of concepts in these discussions. And, even to their peers, they seem to straddle fields.³² The point to notice here is that the work of Burtt and Koyré had made an impact which has been difficult to absorb and appreciate. Hidden

intellectual obstacles--such as the assumption of clarity--will be offered as the reasons for this difficulty.

(b) Burtt and Koyré contrasted to traditional realists--empiricist and apriorist.

The contributions of Burtt and Koyré on the relations between science and metaphysics must be distinguished from those of seventeenth century philosophers, whether of the inductivist-empiricist variety or deductivist-apriorist type. Burtt and Koyré did their historical work in a philosophic atmosphere of positivism; i.e. in an atmosphere excessively hostile to speculative metaphysics of any kind and it may not be clear that their claims would be novel in an earlier context. That is, it is largely agreed now that the hostility to metaphysics and the claims for a positive, theoretically uncontaminated science which were connected with it, fostered by twentieth century positivists following Mach were naïve and extreme; few commentators on science would now support the idea that science is and must be totally isolated and separated from metaphysics; in fact, many acknowledge that there is a clear connection, at least historically, between metaphysics and science. How could there not be, it is reasonably argued, given the well-known work of Descartes and Leibniz who explicitly connected their metaphysics and science? Even Bacon, it will be seen, the arch-empiricist, believed science would produce the true metaphysics. Thus it may be argued that Burtt and Koyré are not unique in going back, with many others, to a more reasonable position. This argument is very reasonable, but misleading. Let me explain.

It is true that both Burtt and Koyré speak to an audience affected by unsympathetic and extreme positivism and they are critical of this framework; however, their position cannot be taken to be a return to any position presented by any philosopher or commentator before them. They are not simply reestablishing the more

reasonable and indeed obvious view that metaphysicians were often scientists and that the people we call scientists often held metaphysical views. As explained in some detail below, Burtt's and Koyré's work is novel in maintaining that speculative metaphysics as speculative and metaphysical plays a role in science, and that the science is still an empirical and critical enterprise as we have--at least in some approximation--identified it to be. Traditional seventeenth century thinkers never associated what they meant as science--secure knowledge--with what we see as metaphysics--speculative thought; only in as much as it could be secured could metaphysics come into their science. When it did, science often lost part of its empirical character and most of its critical character (in as much as it had any). The view that speculative metaphysics--as untestable but synthetic theory--has a role in a critical empirical science is new; particularly in that Burtt and Koyré elaborate on this role in ways which make the metaphysics an essential part of the scientific enterprise.

Yet, the systems do not thereby approach aprioristic axiomatic systems of the seventeenth century theorists. Burtt, Koyré, the seventeenth century empiricists, and seventeenth century apriorists are all realist philosophers in the sense that they have held that scientific theories are, or should be, true explanations--or, to say this another way, that science is the search for truth not merely for prediction or classification. In this they are to be distinguished from many other commentators and classed together; but on other grounds--as summarized here and explained below--their positions differ markedly.

First, the Burtt-Koyré position on metaphysics and science may easily be distinguished from 17th century--and succeeding--empiricist positions; yet, even here

the extent of the divergence is often underestimated. Traditional empiricism has often incorporated one of two views: that scientific theories in some way arise out of facts or that they are in some way based on facts. Let me call the first of these the "methodologically-oriented view." The rather simpleminded but powerful methodologically-oriented view of induction--that in science theories ~~arise~~ from facts and only after a collection of facts or an immersion in them--was advocated by Bacon and popular even through the time of Faraday. Although it was tempered and criticized many times, it is still advocated by many historians of science. Burt and Koyré attack this view. If indeed science is taken to be an intellectual endeavor related to that engaged in by Copernicus, Kepler, Galileo, or Newton, their critique is devastating. Koyré's criticism of the inductivist view, e.g., includes reports on Galileo's reliance on ~~thought~~ experiment, the importance of assumptions on the applicability of geometry to nature, and the incongruency between theory and experience in the new science.

Of course, there are many views of science which are quite unrelated to Burt's and Koyré's, quite distinct from any claim concerning metaphysical influences on science, but which include the acknowledgement that theories in science often make their appearance before the facts which they explain. Boyle, Whewell, indeed Descartes himself, may be mentioned.³³ Thus, the fact that Burt and Koyré show sensationally that scientists had theories before facts is unremarkable. However, the Burt-Koyré confrontation with traditional views of science is not exhausted against the methodological-oriented position. The fact that theories which precede facts are often metaphysical in their view is of considerable importance here. Both Boyle and Whewell are noted for admitting with some ease that working hypotheses were essential

in science; yet, science was characterized by a commitment to countenance those hypotheses which could then be based on or justified by confirmation facts. Against this view, Burtt's and Koyré's work may yield two powerful arguments, as follows:

First, scientists gained a major element of the content of their hypotheses from the Burtt-Koyré view, from independently developed metaphysical sources. A crucial point is, in Koyré's view:

...the rise and growth of experimental science is not the source but, on the contrary, the result of a new theoretical, that is metaphysical, approach to nature that forms the content of the scientific revolution of the seventeenth century...³⁴

From this point, Burtt and Koyré can also stand against the view that science only has a place in science in as much as it is based on fact in the end. If hypotheses were not ideas developed ad hoc simply as working hypotheses in the scientific situation (narrower situation) in mind in the rather innovative, made-to-order fashion the characterization might suggest. That is, the "course-there-are-working-hypotheses" view may imply that the theories are developed by scientists solely with limited scientific difficulties in mind, that these theories have no intellectual connection beyond the immediate situation in science. The Burtt-Koyré position contradicts this sanguine view by presenting these hypotheses (which appear prior to the fact) as inspired and influenced by broad metaphysical world views. That a theory accepted because it explains the facts it explains is connected to a metaphysical view or system may be more dangerous than the "working hypothesis" of tradition.³⁵

The second criticism of advanced empiricist claims extends the o

a more concrete manner. It is that the metaphysically informed theoretical positions taken by scientists prior to production of the facts are not merely prior to the facts in a methodological sense; they are also epistemologically prior to the facts. They are epistemologically prior to the facts in the sense that the language the metaphysics supplies allows those facts to be articulated. Thus, although Koyré acknowledges that experimentation is a way of interrogating nature, he adds,

For Galileo, as we know well, it was in curves and circles and triangles, in mathematical or even more precisely in geometrical language--not in the language of common sense or in that of pure symbols--that we must speak to Nature and receive her answers. Yet, obviously the choice of the language, the decision to employ it, could not be determined by the experience which its use was to make possible. It had to come from other sources.³⁶

An even stronger version of the critical point here is the repeated emphasis on the example of metaphysics leading to the criterion of acceptance--specifically, of exact measurement--for Kepler and Newton.³⁷ Moreover, Koyre notes that resistance to parts of the new metaphysics led to scientific misjudgment, not only in the case of scholastics who refused to look through telescopes,³⁸ but also in that of Galileo's³⁹ work and Huygens.⁴⁰ Thus, in a strong sense, the theory is that not only prior to the facts it then explains, but also, it cannot be independently validated by those facts; at least not in any traditional, secure manner. Thus, seventeenth century empiricists cannot approach the statement on relations between science and metaphysics Burtt and Koyré present.

It is not clear how far one can push this dependence of fact on metaphysical language in Burtt and Koyré. The passage cited above is not equivalent to the

view that metaphysical positions determine facts. Kuhn's and Feyerabend's incommensurability and irrationalist conclusions, discussed later, would be distasteful to them to be sure; but, it is not easy to articulate their strong claims for metaphysical influence in a way which does not yield such a position. Burtt and Koyré do not discuss this point explicitly, of course, since the problem arose in part out of their work and not before it. But, it must be stressed that Burtt and Koyré do want to say that in some ways the fact that a new metaphysical framework was adopted makes the facts of science possible and in part makes them what they are.⁴¹

For the contrast attempted at the moment, one need only note that the Burtt-Koyré view differs from traditional empiricists' positions not merely in including the point that theory in science may enter prior to facts, but also the point that the theory involved has metaphysical connections. This implies its source and rational--at least originally--are quite independent of empirical justifications.

From this point it is easy to conclude that the Burtt-Koyré view is an advanced statement of traditional apriorism;⁴² it is in fact described in this way on occasion. Dudley Shapere, for one, understands the Burtt-Koyré message to be a defense of Platonistic apriorism,

The beginning of modern science, far from being an empiricist reaction against dogmatic Aristotelianism, had its deepest roots in a revival of Platonism...⁴³

Shapere is not definite on the issue of what Platonism or apriorism means to him; but, he criticizes Burtt and Koyré for defending it as the essential aspect

of seventeenth century science. The several historians who read Burt and Koyré rather vaguely to have reintroduced philosophy into science also give the impression of considering their view a reintroduction of apriorism.⁴⁴

Descartes would, in fact, have agreed with a great deal of the Burt-Koyré theory as it was described above; and this is another source of confusion as to the distinctive character of the claims to be made on their behalf. Descartes and Leibniz certainly, one would think, held that metaphysics informs and underlies scientific theory, that theory often arises before the facts (although not always or necessarily), and is validated independently of them. If these earlier advocates of metaphysical bases of science are not credited, then surely Kant should be acknowledged to have a claim as the theorist who made the metaphysical underpinnings of science apparent. From this angle, it may seem that Burt and Koyré should get credit for filling in historical details on particular scientists and particular metaphysical views, but no further philosophic credit.

Yet, Burt and Koyré take a position clearly distinguishable from that of these realist philosophers who trace scientific theories to authoritative sources independent of facts--even Kant. In contrast to Descartes' and Leibniz' position, they do not hold that scientific views are deduced from not entailed by metaphysical principles,⁴⁵ nor that the metaphysics which informs scientific theory is authoritative or necessary. One of Burt's major concerns is to undercut the authority assumed by science on the strength of its supposedly exclusively-empirical, or exclusively justified, entrenchment;⁴⁶ and this critique hits the apriorist philosophers such as Descartes and Leibniz as soundly

as empiricist philosophers. Both Descartes and Leibniz emphasized broad cosmological, ontological principles where essential foundations of science--if not indeed the starting points of science; but these principles were anxious in the sense that they were validated, justified, secure theories--first philosophy. They were not speculations and in as much as they were, they were not science. Koyré and Burtt understand metaphysical views in the modern sense of unverified speculations not doctrines validated by derivation from an apriori source nor susceptible to a apriori validation.

It is indeed difficult not to defer to Kant's tremendously subtle treatment of what we now call the relationship between metaphysics and science. He discusses the metaphysical foundations of thought with references to a fully developed science. His philosophy includes an acknowledgement of the undeniable existence and overwhelming importance of the framework of thought while at the same time he tries to incorporate empirical fact into the verification of science. Neither of these steps was taken by earlier apriorists with at all the same sensitivity.⁴⁷

To be more specific, Kant recognizes that there are and must necessarily be principles the acceptance of which is required to make "experience" possible and that therefore such principles must precede experience. In this doctrine, Kant is a precursor of both Burtt and Koyré and all sophisticated thinkers in this area in a real sense; the citation from Koyré above (p.^{n.36}) recalls Kant's idea that the "possibility" of experience depends on the previous adoption of certain metaphysical views. However, in finishing that statement, I

have been forced to some anachronisms which reveal the difference between the commentators. That is, in Kant, one does not "adopt metaphysical views!" which allow the possibility of experience; one "discovers" what "the foundations of knowledge" must be. The principles involved for Kant - e.g. Euclidean geometry--are established because they--and not others--are necessary to explain the existence of science. They are certain whereas the principles of Koyré and Burt are clearly not. Moreover, their adoption allows for the possibility of "knowledge" in the sense of demonstrable, unchanging truth not simply the establishment of the theoretical systems we now take as science. The "metaphysics" of Burt's and Koyré's picture of science is his "scientific" framework of thought; he would not have admitted "metaphysics" in the sense of doubtful speculation or religiously colored theory into science at all.⁴⁸ Likewise, Descartes himself would have disparaged--and did--any connection between science and metaphysics if the latter had been understood to be unverified and doubtful theories. Thus, Burt and Koyré, although realists and anti-inductivists, are not to be identified in their claims with traditional seventeenth century thinkers of the apriorist position on questions of the relation between metaphysics and science; no traditional apriorist philosopher can be taken to have put forward the same view of science and its relation to metaphysics that they do.

Readers familiar with Francis Bacon's own views as opposed to those of more ardent and extreme followers will know that Bacon was not antagonistic to metaphysics--only to metaphysics as prejudice embraced before empirical support was available.⁴⁹ In the end, he held, science itself would produce the true metaphysics and this would in fact be the crowning glory of the enterprise.

In a similar spirit, many traditional empiricist commentators may speak of

the philosophy and even the metaphysics connected with science; they indicate those cosmological or philosophic positions which seem to be suggested or abstracted from scientific results--a kind of "scientific metaphysics".⁵⁰ This is not at all the same sort of acknowledgement of a connection between metaphysics and science as that which Burtt and Koyré imply, in fact an inversion of it. Such commentators do not usually see the metaphysics as independently developed and on a different epistemological level but as an offshoot of the same epistemological status as the science it is abstracted from. The connections established are not taken to affect the character of scientific views.⁵¹ Also, these metaphysical connections are not at all involved in the activity known as scientific; they do not participate in science as a developing process. The connection with science is less intimate in fact and less independent in theory; it is the fact that Burtt and Koyré reverse this usual, traditional relation that makes their view both distinctive and problematic.

A last word on the distinction between the views of Burtt and Koyré and those of traditional theorists on science is necessary to set the proper context for their contribution. Although traditionally metaphysics as doubtful speculation could have no relation to science proper, metaphysics could indeed influence events in the history of science. Metaphysical views in the sense of doubtful speculations could be (and were often) cited as the ^usource of obscurity and confusion, as obstacles to the development of ideas, as corrupting influences on potentially scientific thinkers and their ideas. Metaphysics could foster pseudo-science and dogmatism. The black and white histories of science which Duhem and Koyré attack feature such views prominently.

Burtt and Koyré do not resurrect metaphysics in the unfavorable context men-

tioned above. They claim both to establish relations between metaphysical views and scientific ones and to make no apologies for the science which is produced. It is the classical, paradigmatic science which they claim has close connections with metaphysics not a corrupted outdated, or in some other way inferior, brand.

Likewise, the traditional, usually positivistic, exclusion of metaphysics from contact with real science was coupled with the idea that metaphysical views--doubtful speculations--were often incoherent, muddled, intellectually dishonest, wispy, and uninformative ideas. The metaphysics which Burtt and Koyré claim contributes to the development of real science is not such a metaphysics; it is powerful, coherent, intellectually awesome and tremendously informative body of thought - despite being doubtful speculation.

Thus, Burtt and Koyré depart from the classic modern thinkers views on relations between science and metaphysics in several regards: They disagree on the fact of a connection--traditionally, there is none but an obstructive one whereas for Burtt and Koyré the relation is an essential fact about science. They disagree on the effect of any influence--traditionally, metaphysics infects ideas making them less scientific whereas for Burtt and Koyré the effect is a favorable one. They disagree finally on the nature of metaphysics--traditionally, metaphysics as speculation was not intellectually respectable whereas for Burtt and Koyré it is admirable, although still not respectable in the sense of proven. The main common ground they share - against later positivist commentators - is the view that science aims at the truth and that theory - contentful views which go beyond the facts - are necessarily part of science.

(c) Burt and Koyré contrasted to Duhemian Conventionalists.

Were Burt and Koyré the first or the only commentators to dispute the inductivist view that science derived from the facts alone and was completely divorced from metaphysical views, there would be no difficulty in pointing out the value of their contribution. However, Pierre Duhem's forceful and revolutionary criticism of the traditional ~~realists~~ of all types⁵² preceded that of Burt and Koyré. Duhem's contribution to the renewed appreciation of theoretical views in their relation to science is both central and complex; it can easily be confusing and misleading; he resurrected a connection between metaphysics and science through his historical work while philosophically he was hostile to it. Moreover, and quite apart from the above, Burt and Koyré may easily be mistaken for disciples of Duhem rather than identified as the (appreciative) opponents they actually are.

For both above-stated reasons, it will be important to review Duhem's contribution to the issue at hand with special regard to differences between his views and those of the Burt-Koyré "school". It is especially misleading to speak of the "school" in this context since Burt as well as some disciples of Koyré follow Duhem philosophically and Koyré historically; the necessary qualifications are made as required below.⁵³

Duhem's major contribution to a revival of the debate on the relation between metaphysics and science was his criticism of traditional radicalism.⁵⁴ His criticism of inductivist radicalism is the more widely appreciated, although his critique undercut apriorism as well. To briefly restate the now well-known position, Duhem held that the classic scientists of the seventeenth century were not revolutionaries who disdained and rejected tradition and in that way created modern science from nothing --

out of the facts or by clear thinking. In recovering Galileo's and Kepler's predecessors in theory of motion and mechanics, Duhem hoped to show persuasively that a long tradition of thought and experiment, embedded in what had been held to be an anti-scientific Aristotelian (metaphysical) framework, led to their achievements. He also argued that absorbing and honoring this tradition, not radically rejecting it, was essential to the success of the early scientists. Further, he held that their success itself was best described as a refinement of, or a complement to, this long and slowly evolving tradition, not the overthrow of it.

Duhem himself did not intend his criticism of radicalism to contribute to acknowledgement of a more intimate connection between metaphysics and science. Quite the contrary. Nor, it is to be noted, is the conservatism with which he replaced this radicalism at all amenable to a sympathetic consideration of the role of metaphysics in science; it is not shared by Burtt and Koyré who follow Duhem in his critical views.

To take this last point first, Duhem connected his defeat of inductivism with the view that early scientists were not radicals, qua scientist. Science depends on preserving tradition; metaphysicians, being interested in the truth as regards the essence of things, are likely to disregard developed and important traditional views merely on the ground that they do not reflect this essence. Good scientists are therefore not metaphysicians. They do not dismiss Aristotelianism, for example, in their capacities as scientists no matter what misguided claims they may make in other roles. As scientists, their job is to save the phenomena not reach the essence of things. The Renaissance scientist, like Copernicus, according to Duhem, did not understand

this as well as earlier thinkers.

One powerful and fruitful principle promulgated and observed by the Parisian Scholastics deserves special mention--hypotheses ...are geared toward a single end--to save the phenomena.... This limpid conception of the nature of physical hypotheses... received its greatest set back precisely at the time when astronomy and physics were making new and rapid progress. The greatest artists are not necessarily best at philosophizing about their art.⁵⁵

The Parisian Scholastics Duhem praises determined to save the phenomena rather than opt for mystic irrationalism; the latter was tempting since according to their metaphysics, no physical law could possibly govern both celestial and terrestrial domains.

Yet, they had an alternative--namely, to surrender the metaphysics which denied the unity of nature. Duhem downplays this alternative or denigrates it; Burtt and Koyré describe the conflict which led to it and celebrate it as decisive. That is, contrary to Duhem, both Burtt and Koyré emphasize how literally the early scientists took the metaphysical interpretation of physical views--both the views they favored and those of their predecessors which they wanted to replace. Moreover, they emphasize how essential this commitment to a literal understanding was in the creation of the new science. One could call this literal stand "realism": the commitment to take theories as true explanations of actual events in the world.⁵⁶

To expand, admitting the inconsistency between the old metaphysics and the new science--impossible if one doesn't take a literal, metaphysical stand with respect to hypotheses--led to adopting a new world view which eliminated the barrier between celestial and terrestrial worlds; this was exactly the step essential to the new science in Burtt's and Koyré's interpretation. Without taking seriously this metaphysics--without interpreting the science metaphysical or "realistically"--the

science would not be what it is; the events of the Scientific Revolution would not, in a strong sense, have been possible. Agassi summarizes and elaborates this point in the following way:

The most interesting and critical studies in the whole field of the history of science are those of Koyré and his followers, which, I presume, have developed in reaction to Duhem's continuity theory, and which emphasize the immense role played by the logical conflict between Aristotle and Archimedes in this history of Renaissance mechanics.⁵⁷

The point is that the logical conflict only results when the theories in question are interpreted as explanations of actual events in the physical world.

To be clear, I do not claim here that Duhem denied that scientists often believed their metaphysical views to be true. Bruno would not have gone to the stake for a "heuristic device"; on this, Duhem and I agree. But Duhem does say that when scientists were caring about the truth of their views--the metaphysical truth and whether or not their theories captured it--they were not being scientists. They may have thought their views were true; they may have thought that it was important that they were true, but they were good scientists in spite of this, if at all, not because of it.

Likewise, Duhem did not say that no Medieval scholastic genuinely believed any theory^{e.g.} that only perfect motions existed in the heavens. Most certainly many believed this as literally as they held any theory. But, when Duhem admits such beliefs--as that on perfect motion--into science, he admits them only in as much as they function heuristically--not as a substantive view about the nature of motion; they are part of science only through saving the phenomena not through revealing reality. In the section of Aim and Structure cited above, Duhem says explicitly that

even Copernicus and Galileo believed their metaphysical views and believed that science concerned these metaphysical truths; but they were good scientists for other reasons. In fact, when they began taking seriously their metaphysical preferences-- instead of understanding that the theories are only meant to save the phenomena-- they did a disservice to actual science according to Duhem.⁵⁸

I am sure the difficulty here is not just one of language or vague expressions. Of course, Duhem did say that the theoretical system approaches a "natural classification"; this implies that it comes "closer to the truth or to describing the actual state of affairs". But this part of his philosophy is not consistent with the rest of it. Or, perhaps, it is better to say that the faith that the system will approach a natural classification is just that: a faith. No steps can be taken in that direction in science. Criteria of improvement make no reference to such aims.⁵⁹

Perhaps the difficulty is that I say Burtt and Koyré held that metaphysical theories were taken "seriously" implying that the psychological sincerity or earnestness or enthusiasm was the important element. This is misleading; "taken seriously" mean, rather, taken literally as explanation of events--as true or false not as analogies, approximations, or merely convenient models in which to incorporate facts.

Thus, in their view the adoption of a new world view did happen in the Renaissance and it was part of what made the new science possible, and what made the new science what it was; moreover, the change was in Duhem's sense revolutionary. These views promoted by Burtt and Koyré are in the most direct conflict with the Duhemian views on science and metaphysics although the latter are also taken to contribute to a better appreciation of a relation.⁶⁰

To cover the field, it seems necessary to review the place of Koyré's contri-

bution, particularly his emphasis on realism, in the context of the work of other post-Duhemian historians. Many later-day commentators on science seem to appreciate metaphysical influences yet their appreciation - through the influence of Duhem - is equivocal at best, Thomas Kuhn is one example. To explain, in showing how much the achievements of more noted workers depended on that of lesser predecessors, Duhem necessarily recovered and emphasized the importance of theories, problems, and points of view which did not achieve scientific status in many cases. The recovery of tradition gives necessarily a more developed conception of the problems of the day, the theories as they were held by the thinkers who developed them, and contemporary difficulties. All this makes Duhem's approach to the character of science more historical than analytical. The point is that the recovery of tradition leads at one and the same time to a treatment of theories with a greater historical sensitivity and to a treatment of theories which will be seen as metaphysical by many observers.⁶¹

Thus, since many historians have taken Duhem for their model due to the more genuinely historical approach his views encourage, they have inherited an orientation toward out-of-date theories which appear metaphysical as a matter of course. They attend to theories which are or have been seen as metaphysical simply because they follow this anti-radical tradition of searching for and valuing predecessors. The approach necessitates exploring false leads, incomplete and underdeveloped theories, unsuccessful hypotheses--all views which may be seen as metaphysical on traditional criteria.

Yet the appearance of an appreciation of a relation between metaphysics and science which this historical stance gives may often be merely an appearance--par-

ticularly pale when contrasted with the views of the connection developed by Burt and Koyré. What to outside observers--outside Duhem's philosophy--is metaphysics, is often not acknowledged as metaphysics by Duhemians.⁶² The point is that one does not know, often, how far any historian or commentator who used Duhem's historical approach will follow him in his denial of metaphysics qua metaphysics. Koyré's far more unequivocal position on this point--the metaphysics he associates with science is definitely meaningful, contentful, theory about the world--is one remarkable contribution to the debate; it helps reveal the valuelessness of other positions.

There is another Duhemian curiosity to record in order to reveal the value of the Burt-Koyré position. Since Duhem only emphasizes the ideas of predecessors in order to rescue conservatism, he must necessarily overlook one invaluable way in which thinkers may interact with their tradition to the benefit of their endeavor: namely, by criticizing it. Duhem presents tradition only as that which a thinker follows and to which he contributes his more or less significant bit. He cannot--without surrendering his position--present tradition as that against which one may react strongly as opposed to that which one merely dismisses or disregards.

Again Burt and Koyré come much closer to doing justice to this sort of relation between science and metaphysics; they emphasize theory which comes into conflict with tradition as criticism of previous frameworks.⁶³ This makes their treatment of historical episodes far more appreciative and critical in many cases. Koyré's emphasis on the difference between the theory of prism diffraction offered by Newton and that incorporated in the folktales of generations before is a beautiful and typical example. Everyone knew the prism split light into colors; Newton not only measured the angles

of diffraction precisely but--more importantly--his theory was a theory of white light being split into constituents which were there previously rather than changing qualitatively into something else. This new content makes the theory as Newton spells it out differ from previous observations; and, the theoretical content makes the measurement meaningful if not actually possible.⁶⁴

(d) Burt and Koyré contrasted to socio-economist and "externalists".

There is another notable movement to explain the development of science which differs markedly from that of Burt and Koyré; the school may be taken to be similar since each endeavors to expand the boundaries of "science" as understood traditionally. These commentators stress the importance of economic, technical, or other "external" factors in the development of science. Thomas Kuhn, Joseph Agassi, Henry Guerlac, and Arnold Thakray⁶⁵ have discussed the divergence of approach and opinion involved in these two positions and their comments will be reviewed and summarized here.

For the record, "external" history is that which emphasizes motives and justifications which are acknowledged to be independent of considerations of truth or falsity. Economic motives or technical problems may give rise to scientific problems or lead to the discovery of theories but these factors are not intellectual. Koyré and Burt also bring in factors "external" to traditional science when they introduce metaphysics; but, metaphysical theories are intellectual and, in their view, true or false. They are not discontinuous therefore with the scientific theories they influence and are internal to the enterprise. Koyré's view stated below emphasizes this point.

Koyré's own position against what he calls the psychosocial historian is prominent in several of his major works. One statement of it is the following, which I will give at length since it indicates Koyré's arguments as well as his position.

The psychosociological explanations of the rise of modern science present us with a mixture of two by no means equivalent theories. (1) modern science is the off-shoot of the technical development of the sixteenth and seventeenth centuries... (2) modern science is made by scientists who, because of the increasing importance of technics and the growing importance of industry, started thinking about problems they had neglected...

Both these theories seem to me to miss (1) the role of purely theoretical interest in mathematics which led to--and was maintained by--the rediscovery of Greek science...and (2) the overwhelming importance and the autonomous evolution of astronomy, promoted much less by practical needs... than by theoretical interest in the structure of the universe.⁶⁶

The text to this extended not includes the powerfully pro-metaphysics view cited earlier (p.). Koyré may be seen to argue also that it was not the craftsman who taught the scientists as much as it was scientists like Galileo who taught the craftsmen.

Koyré is stating his views in response to a series of commentators, listed in the note from which the above passage is taken, who emphasize factors external to the logical and rational area of culture and life.

To emphasize the main point, these factors are "external" not in a factual genetic sense that they "come from" outside science; in this sense, metaphysical views are also external. One of the points made by Burtt and Koyre is that metaphysical views influential in science are not derived from previous scientific views but often conflict with them. External to science means parts of realms or disciplines which cannot be judged by the same standards. To take a crude example, if Galileo's class origin is a factor in his development of or preference for Copernicanism, this factor cannot be analysed in the same terms as a view of the world which is true or false. As noted, there are a number of different factors which are significant; some commentators emphasize that sociological and economic factors--such as changing class status, the drive to improve material conditions, or the economic interest in better time keeping and navigational devices--have to be introduced to understand why modern science arose when it did, and that these factors "explain" its appearance

more in the sense that they explain interest in the area.

As Koyré notes, it is often not clear whether the factors mentioned by externalists serve as motivation for intellectual activity, as actual determinants of research programs, or as validation criteria. Each view can and is often combined with the view that a technological innovation preceeded and encouraged certain theoretical advances and that they must be referred to in any explanation of how advances in science are made.⁶⁷ The improved clocks and telescopes of Galileo are the most common example again; the point of the claim is that it is these innovations, not logical thought or experimentation, which are the crucial factors in theoretical development.

This externalist approach was often Marxist or pseudo-Marxist oriented; a further point in the argument based on it was that, really, science was not the isolated, ivory tower, unconcerned intellectual activity portrayed in traditional histories. In claiming that sociological and economic factors influence or determine scientific advances, these commentators thought they had captured the broader and more realistic nature of the enterprise.

Crane Brinton, for example, in explaining the special characteristics of an historian as opposed to a specialist working the history of a field explains that the intellectual historian must deal with the effect of specialist ideas on the masses, and the reverse effect of more mass developments on ideas. Some historians of ideas, he says, referring to Roquian, disdain the "idealist" philosophies of intellectuals who believe ideas were more instrumental than "hard, specific, grievances" in historical developments such as the French Revolution.⁶⁸ That the specific, more earthy problems are the driving forces of history is a view which may be applied even to the

history of theoretical science; early attempts in this direction were extreme and uncritical, a version of the view, however, is advocated by Thakray and has been approached by Kuhn.

Both the above authors--Thakray and Brinton--seem to share Thomas Kuhn's view that the new historians such as Koyré, although they are far superior to former, anti-historical writers on science, are deficient in not dealing with the broader cultural settings of and influences on science. Kuhn, for example, says,

The discovery of medieval science and its Renaissance role has disclosed an area in which the history of science can and must be integrated with the more traditional type of history.⁶⁹

The traditional history emphasizes the hard grievances and more primitive motives and purposes than those science is usually taken to represent. Kuhn goes on to note that it is unfortunate that most of the workers in the new history ignore these factors:

Its best practitioners have, like Koyré, usually minimized the importance of non-intellectual aspects of culture to the historical developments they consider.⁷⁰

Like Thakray and a few others, Kuhn side-steps without comment the possible conclusion that Koyré is "one of the best" simply because he minimizes the non-intellectual aspects. It is a policy which is hardly the result of neglect or obliviousness on Koyré's part although Kuhn comes close to unfairly suggesting the opposite. On the contrary, it is the result of a well-developed critical view of science - stated at the beginning of this chapter - and reflected in the quotations earlier.

The sociological-economic commentators, of course, have important traditional aspects of the origins of science to which they can appeal. Bacon had, after all, emphasized that "knowledge is power"; he publicized the view that only through theoretical advance could material well-being be securely achieved and made it

well-known that material well-being was one pre-eminent goal that should motivate men to pursue science. Bacon, in fact, being no radical in ethical and spiritual matters emphasized that truths in these areas were to be left to traditional sources of religion and morality; this enforces the view that science leads merely to technical understanding and material improvements not to real enlightenment.

Of course, Bacon never suggested that proficiency in mastery of the world be a criterion of the acceptability of scientific theory. Yet, his attitude, and the general Puritan orientation toward hard work and solid achievement on earth--as Merton is famed for noting--accompanied both the development of middle-class capitalist culture and characterized the orientation of many early scientists as well.⁷¹

Koyré is particularly concerned to argue against this sort of interpretation of the rise of science. His view is that a connection between science and metaphysics, even religious metaphysics, is not complementary to this sociological-economic view but in direct conflict to it. Although both views of the process of scientific advance stress outside influences of a kind--external to traditionally conceived rationality--Koyré's outside influences are intellectual whereas the others are not. Even Merton's Puritan motives are psychological not intellectual.

There is a vast difference in the claim that science is guided either in prior motivation or in determination of criterion for theories by economic desires and the claim that science is guided by the concern for certain cosmological plans. In Koyré's view of the matter, the motives and criteria are still centered on the truth of theories about the world; they are still ivory tower concerns in the sense that it is theories, not activities nor external goals which reveals the center of science. One of the most pointed expressions of his position appears in this reply to Henry Gueŕlac:

Thus, it seems to me--and if it is idealism, tant pis--that science, the science of our epoch, like that of the Greeks, is essentially theoria, a search for truth, and as a result of this fact it has, and has always had, value as an end in itself, and an inherent and autonomous--although not always regular and logical--development, such that it is only by a study of its own problems, its own history, that it can be understood by historians.⁷²

This stance makes Koyré's position in introducing metaphysics into science more distinctive than it would otherwise be. He introduces a new element into the realm of science but one which he does not view as changing the activity from something autonomous, from a search for truth, to an irrational activity. That this "external" factor is intellectual makes this position more coherent. At the same time it makes his position a clear--or more clear--threat to traditional "rationality". Other externalist views (not touching the rationality of the development of ideas) do not threaten "rationality" itself but the claim of science to the status.

Indeed, the extent to which Koyré denies external influences may be reflected through historians who do not agree with him although they are followers. In Koestler's Sleepwalkers the main characters of the scientific revolution seem barely aware of the outside world or the effect, practical or theoretical, they may be making on it through indulging their intellectual impulses. Their concern is stubbornly other-worldly.⁷³

The kind of factor introduced by the socio-economic commentators may easily be combined with a Duhemian conventionalism; since Duhem stresses that the metaphysical truth of the theoretical system is not the concern of the scientist, he may well welcome the introduction of other motives to explain the choice of problems and intellectual purposes. Here again Koyré's work was meant to refute exactly such

⁷² "The scientific revolution is a purely intellectual activity, and it is not a social activity." Koyré, *Philosophy of Science*, p. 10.

⁷³ Koestler, *Sleepwalkers*, p. 10. "The scientific revolution is a purely intellectual activity." Koestler, *Sleepwalkers*, p. 10.

Claims by pointing out how literally metaphysical and physical concerns were taken at exactly the points at issue and that they fulfilled just the functions mentioned.

Although the debate undertaken on this point is one of great interest, the arguments adduced against Koyré's position do not seem at all convincing. Kuhn himself, who joins the latter-day forces, gives a direct and pointed analysis of why the Merton thesis, as he calls it, could not be developed profitably: the view of science which it incorporated--of science as based on empirical data collection and hard work--connected to material achievement--was being discredited by other historical work even as Merton publicized it.⁷⁴

As to Henry Guerlac's contribution to the debate, it seems also quite unsatisfying. Guerlac speaks of the need to avoid specialist studies of a discipline if one means to be a genuine historian. That one must not hope to advance history by studying some selected and isolated aspect of human endeavor, which may thereby be "remote from crucial human problems".⁷⁵ These problems must indeed again refer to hard, actual grievances which have a close connection to struggles of mass audiences. Yet, surely by now, it is not in order to think that only the lack of bread--or some similar unintellectual event--qualifies as a genuine problem in human life; and, the indication that Guerlac knows so surely that the crucial problems are not those Burtt and Koyré discuss seems slightly out of place and old-fashioned.

Likewise, naïveté seems to characterize Guerlac's easy talk about a "greater synthesis" to which historical work must defer; and, his emphasis on the importance of the "larger cultural fabric"--larger than that indicated by Burtt and Koyré--seems uninformative when not positively mysterious. The same may be said of the "general

flow of history" which he takes as important; and again one must assume it is something distinct from Koyré's historical points, which misses the point of conflict precisely.⁷⁶ Guerlac would have to indicate the nature of the "general flow of history" much more precisely in order to show that he can avoid the criticism of such references which many commentators such as Popper could make.

Koyré appeals to the limits of human capacity in response to Guerlac's nostalgia for the general flow or synthesis.⁷⁷ His appeal is proper but could well be enforced in other ways. It would, in fact, be better not to concentrate on the technical difficulty which clearly interferes with Guerlac's ideal; the intellectual mistake involved is more central and more interesting. The idea that there is a general synthesis or proper line of development in history rather than alternative answers to particular question and selected different points of view and interests is the mistake. Koyré indicates at the end of his paper that he is both a fallibilist and a pluralist on this point in spite of his concentration on the appeal to limited human capacity.⁷⁸

Indeed, the half apologies made on behalf of the need to do selected history are distasteful; even Agassi, ordinarily anything but deferential to the conventions of historical work--makes them.⁷⁹ It seems bizarre to think that any commentator would want to rescue the whole of "past actuality" and attempt to record it; such an enterprise hardly seems valuable or desirable even if possible. It would involve an actual "reliving" of the past. Yet, those who longed for authentic "experience" in their historical writing could as easily be satisfied with the actuality of their own present. Whereas, if possessed of an inquiring and abstracting mind, a person faced

with the possibilities of occupation would hardly choose to have a flood of undifferentiated "past actuality" available. Anything of the sort would be irrelevant; or it would be a positive obstruction to the goal. The craving for access to a full past to which the half-apologies so common in this discussion refer is on the same level as crying for the moon; a posture not contemptible merely nor mainly because the goal yearned for is impossible, but rather because the program is clearly so badly, uncritically, thought out that one suspects disingenuousness.⁸⁰

Moreover, it is not clear that the desire for past actuality is coherent. Koyré approaches the criticism when he responds to Guerlac that although "reality is one" our thought is "abstract and analytical," and thus ipso facto selective and pluralistic.⁸¹ In the face of such considerations it is difficult to appreciate the relevance of our human limitations in doing history.

Section 1.4. Summary: On the Value of the Burtt-Koyré View.

In this chapter, I have wanted simply to develop the problem situation surrounding the question, how do metaphysical views--particularly confused metaphysical views--interact with scientific ones; what role do they or can they play in science? Toward this end, I have given some of the history of the issue, noted the more famous answers to the question, the inadequacies of these answers and replacement of them. This has been done here mainly by arguing that the Burtt-Koyré view is a superior and distinctive answer to this question--a judgement not articulated often but widely shared. It has been argued that the failure to appreciate Burtt and Koyré shows that this problem situation has not been well or critically understood; an illumination of their merit in contrast to those of other positions fills in the context of the contemporary problem of relations between science and metaphysics. This is the case regardless of whether or not readers agree that the position distinguished is indeed a superior one.

First, the critical and argumentative character of Burtt and Koyré's work is not captured by associating their work with that of historians of ideas; they may have been mistaken but they both see themselves to be doing exposes or urgent rescue jobs on the ideas they treat, not narrations.

Against many traditional theorists of science who take an empiricist line, they hold that theoretical views of a strong, directive character were developed before the body of scientific data and generalizations which have come to reflect them. These viewpoints were metaphysical; they provide the language and frameworks of the science which develops. All this suggests that classical science was not created

from an independent empirical base or because of the commitment to such a base; at least, not if the empirical base is something independent of a theoretical viewpoint. The metaphysical world view of scientists is an essential part of their work as they develop science.

However, the new world view which has played this essential role is not seen to have apriori validity; indeed it is this which makes the Burtt-Koyré presentations of science developing intimate touch with metaphysical ideas so startling. Metaphysics in their 20th century post-positivist accounts is not the system of authoritative first principles of Aristotle or Descartes; but the unfounded and unproven speculation which had a very unfavorable reputation in connection with science. Thus, the metaphysics Burtt and Koyré have described in connection with science have not been put forward as the validated base of science--not the first principles of nature which traditional rationalists (apriorists) postulated as the foundation of all science. The metaphysical systems involved in the Burtt-Koyré view remain not only unverified but independent of the science which they inform and guide.

However, neither are the metaphysical views which influence science so powerfully, nor the theories they influence, held by Burtt and Koyré to be conventional, heuristic devices. Rather their status and functioning as realistic views--non-tautological claims put forward as part of the true explanation of actual events--is emphasized in contrast to the position taken by Duhem and his followers. In this vein, the development of modern science is put forward as a revolutionary event; the theories of the new science were held as realistic views, and thus their adoption required the change to a new world view also held realistically. The new world view was needed not only because it provided the new science with content but with its

essential methodological characteristics; that is, it not only allowed the formulation of the new theories but made possible their judgement and refutation on the essential new lines. This position is not completely clear in the works of Burtt and Koyré; it can be abstracted from their historical pictures of the role of metaphysics in the science of the seventeenth century and by reconstruction out of the refutations they supply to alternative views of the nature of science.

Although it needs clarification and elaboration philosophically, the essentially new and previously unaccountable relation between science and metaphysics which they suggest should be apparent. Certainly, revolution based on metaphysical orientation not on continual refinement of established tradition is a keynote of their view; but that this orientation does not prevent corrections and refutations--on the contrary it makes them possible--is also part of the distinctive picture they create and it is particularly valuable.

Finally, Burtt and Koyré do introduce novel elements into the picture of scientists thinking and working as scientists but they insist that these elements were internal and intellectual not practical or external to theory. The motives and criteria for scientific decisions still, in their views, center on theories which purport to explain the world truly not on psychological, sociological, economic wishes or necessities. Thus, although their views do confute many traditional theories of the constituents of rationality in scientific thinking, they are known as "rationalists" and "idealists" who stress the fundamental autonomy of science as the search for truth.

To summarize then, the Burtt-Koyré endeavor has changed the problem of the relations between science and metaphysics. The superiority of their views makes it clear that any satisfactory answer to the problem need not be either empiricist nor

apriorist nor conventionalist; that there is an alternative. One can say that they describe a realistic endeavor--an effort to explain the actual events of the world truly--which does not fit traditional criteria for such endeavors for the several reasons mentioned above. They rescue metaphysics, extol the power of world views, and explain how intimately they infiltrate classical science. Their message is that science did not develop independently of such ideas. Thus, both the idea of science and the idea of metaphysics which emerge from their works are broadened, liberalized and enriched. Some of this enrichment has been overlooked and squandered--as evidenced by the failure to notice the more revolutionary character of their contribution--by themselves and their followers. This oversight is due, in part, to the effect of the assumption of clarity and its supporting traditions--an argument developed in Chapter II.

FOOTNOTES

N.B. All references are given in full once; thereafter works are referred to by abbreviations formed from the series of capital letters in the title. A complete list of the abbreviations used preceeds the bibliography.

- 1 Burt, The Metaphysical Foundations of Modern Science, (London: Routledge and Kegan Paul, 1932); Koyré, From the Closed World to the Infinite Universe, (Baltimore: The Johns Hopkins University Press, 1968; orig. publ. 1957); Études Galiléennes, (Paris: Herman et Cie., 1935-1937); The Astronomical Revolution, (Ithaca, New York: Cornell University Press, 1973, orig. 1961); Metaphysics and Measurement, (London: Chapman and Hall, 1968), Newtonian Studies, (Chicago: Phoenix Books, Chicago University Press, 1968).
- 2 Some of the contexts in which the question, what is the difference between science and metaphysics, would need a more elaborate answer are discussed in Chapter IV; Section 4.2b and Appendix B to that Chapter particularly.
- 3 The question of the refutability of metaphysical views also recurs throughout the investigation and is taken up explicitly in Section 4.2 and Appendix B of Chapter IV. The comments here are designed to dissociate my position from Karl Popper's and from Thomas Kuhn's; also to acknowledge a problem for further discussion.
4. Duhem, Pierre, Aim and Structure of Physical Theory, (New York: Atheneum, 1974; orig. publ. Princeton University Press, 1954), p. 119, for electricity without electric fluids; pp. 187-189 and other places for Fresnel's interpretation of Foucault's experiment--an interpretation that made "metaphysics" irrelevant.
5. Agassi, Joseph, "Scientific Problems and their Roots in Metaphysics" in The Critical Approach to Science and Philosophy, ed. by Mario Bunge, (New York: The Free Press, MacMillan, 1964), pp. 208-209. Also, Agassi, Michael Faraday as a Natural Philosopher, (Chicago: University of Chicago Press, 1976), pp.
- 6 Ibid., "SPRM", pp. 189; 210-211. Also, n.9; and text which ends this section.
- 7 Although it will not be discussed in the text, the work on metaphysics and science done by W. O. Quine deserves mention; it represents--at least in part--one twentieth century position on the role of metaphysics in science which is widespread and which does have connections with Burt's and Koyré's work. To explain, Quine is interested in the nature and existence of mathematical entities; thus, to a certain extent, he is interested in the

nature of entities taken for granted in scientific frameworks. He concerns himself with the question of the real metaphysical existence of numbers, relations, or sets; and in this respect his studies are a specialized case of the debate on the status of theoretical entities in scientific theories. (This summary reflects no more than a superficial appreciation of his position as discussed, for example, in the two opening articles in his From a Logical Point of View, (New York: Harper and Row, 1961).

- 8 Of course, the debate on the status of theoretical entities did come directly out of a positivist plan to demarcate science from metaphysics. The 20th century philosophers of the Vienna circle hoped to define science in a way which would in particular exclude all metaphysical views from consideration within that realm. The philosophers associated with that movement, Moritz Schlick, Friedrich Waismann, Rudolf Carnap, Victor Kraft, Herbert Feigl, among others, made an effort to develop a criterion which would show metaphysics to be different and separated from science, they rejected obviously metaphysical systems and terms--which had no established framework in today's science--straightaway. For example, the microcosm-macrocosm theory of the body would not have had a chance to enter this debate. The question of the empirical founding of theoretical terms and the corresponding claims which could be made for the existence or not of the entities to which they referred, concerned terms and entities important to science now; and this argument does not open the possibility of a full fledged metaphysics playing a role in a science as Burtt's or Koyré's studies do. The present debate ranges over two possibilities--that the terms needed and used by scientists are empirically grounded and the entities are therefore established as scientific or

the terms are not empirically grounded and are therefore necessarily heuristic only and have no metaphysical--ontological--referent. The possibility that the terms may refer to real existents but the theories cannot be empirically grounded is taken most seriously by Karl Popper who then comes to the opinion that all scientific theories are conjectural--never established; of the twentieth century thinkers from this camp who have commented in this area, he comes the closest to an adequate theory of science from the point of view of the debate at hand. Therefore I have taken him as the best representative of the entire tradition of the Vienna Circle. See also Ch. IV below.

Pap, Arthur, "Does Science Have Metaphysical Presuppositions?", in Readings in the Philosophy of Science, ed. by Feigl and Scriven, (New York: Appleton-Century Crofts, Inc. 1953). Pap gives a representative account of the question from a neo-positivist point of view. The "metaphysics" he deals with are principles which form no independent system--such as the principles of causality. He attempts to explain them all as either merely psychological motivators or "leading principles" which means

methodological devices; Popper's attempt is more thorough in Logic of Scientific Discovery, and, as noted, even if successful which the attempt has not been, it leaves aside principles such as the microcosm-macrocosm theory, the belief that essential properties are unchanging, etc.--i.e, more contentful metaphysics.

- 9 See the discussion under 1.3b; 4.2; and 4.5 below.
- 10 See Chapter III; footnote 59, and Chapter II, n. 34.
- 11 See result Section 4.6, Ch. IV below.
- 12 Some of the characterizations of the various positions on relations between metaphysics and science which are given here may seem abstract and general to an excessive degree. There are many intrinsically interesting illustrations of these general positions in the history of science; I have alluded to several in this introductory discussion but hardly more than alluded. Lack of space makes more extensive illustrations from the history impracticable. The Harvey case study which occupies Chapter II and the discussion of Newton's and Kepler's more esoteric interests in Chapter II may help readers who have this difficulty.
- 13 Arthur O. Lovejoy, The Great Chain of Being, (New York: Harper and Row, 1960); the work is widely considered the inaugural study in modern history of ideas.
- 14 In the inverse position, Kuhn claims that historians of science were made aware of the power of more historical approaches which avoided parochial hindsight when philosophers such as Lange and Cassirer dealt with ideas and thinkers commonly associated with the history of science but dealt with them as important in the philosophic tradition. Thomas Kuhn, in his article on "History of Science" under "History" in The International Encyclopedia of the Social Sciences, Vol. 14, ed. by David L. Sils, (New York: MacMillan, the Free Press, 1968), p. 75.
- 15 Koyré, it should be noted, does not share the uneasiness expressed here that reviewers will not value the philosophic content of his work.

"Il n'est-heureusement-pas nécessaire aujourd'hui d'insister sur l'intérêt de étude historique de la science. Il n'est même pas nécessaire-apres l'oeuvre magistrale d'un Duhem, d'un Emile Meyerson, apres celles M. Cassirer and de M. Brunschviog-insister sur l'interêt et la fécondité philosophique de cette étude."

- 16 Kuhn, "HS", p. 75
- 17 Koyré, FCW, p. 2. Also Kuhn credits Agassi with a "brilliant parody" of the old-fashioned tradition in history of science; "HS", p. 75.. See Agassi's monography, Towards an Historiography of Science, in History and Theory, Beiheft 2, 1963, Sections 1-3.
- 18 Agassi, ibid., Sections 1 and 2.
- 19 I. B. Cohen, "Commemoration", Isis, Vol. 57, Part 2, No. 188, Summer 1966, p. 16.
- 20 See Agassi, THS, Sections 13-16 for a discussion of the difficulty of avoiding hindsight and the philosophic implications behind it; also extensively discussed in Section 2.4 in this text. Also see Koyré, AR, pp. 10-11; EG, p. 91 on Bennedetti; also the last pages of his "Commentary", a reply to Henry Guerlac in Scientific Change, ed. by A. Crombie, (London: Heinemann, 1963) pp. 856-857. (Also U. of Wisconsin Press).
- 21 Koyré, FCS, p. 2, also p. vii.
- 22 Crane Brinton in his "Intellectual History" under "History" in IESS, p. 463; also, Henry Guerlac, in SC, pp. 801, 809-812; and for others, one can refer to Arnold Thakray's contribution, "Science: Has Its Present Past a Future?" to Historical and Philosophical Perspectives on Science, which is Vol. 5, of the Minnesota Studies in the Philosophy of Science, pp. 112-113. These matters are taken up also in Section 1.3d of this text.
- 23 Brinton, p. 465.
- 24 Ibid., p. 465.
- 25 Cohen, p. 158.
- 26 Ibid., p. 158.
- 27 Guerlac, p. 809.
- 28 Koyré, NS, p. vii.
- 29 Ibid., pp. 5, 6. One must note that Koyré held "explanations" of events such as the scientific revolution to be incoherent aims at times. They were too general and indistinct; he also, I suspect, suspected them of incorporating references to laws of history.

- 30 Agassi, THS, especially pp. 93, 57, 58.
- 31 Ernan McMullin, "The History and Philosophy of Science: *A Taxonomy*", in MSPS, Vol. 5, p. 17. (pp. 12-67, entire article).
- 32 I argue, following Agassi, that the difficulty reflects a philosophic lack-- of an adequate theory of novelty and of the usefulness of errors; this discussion is continued in Sections 2.4 and 4.1, 4.5 below.
- 33 Agassi, "SPRM", records these debts; Laurens Laudan, in his comment on Thakray's paper in MSPS, Vol. 5, objects to the criticism of this naive position as beating a dead horse; p. 128. Yet, Koyré, Burt, and Popper had to emphasize the point against repeated objections. Agassi, THS, p. 37, for criticism of the myth of "continuity in history of methodology". Also Agassi, "SPRM", pp. 111, 194-196. There is more function to criticizing the simple position than may be immediately apparent, as I try to argue in the text.
- Buchdahl, Gerd, Metaphysics and the Philosophy of Science, The Classical Origins Descartes to Kant, (Oxford: Basil Blackwell, 1969). Buchdahl takes a fairly traditional approach in emphasizing that the development of science influences the direction and problems of certain philosophic systems in a major way; the metaphysics consists in justificatory postulates to support theories of science and its possibility. pp. 3-4.
- 34 Koyre, NS, p. 6.
- 35 Agassi point out that the significance of the "prior to fact" requirement is the fact that it is related to the doctrine of prejudice; this view is that any idea once acknowledged, is extremely likely to become a prejudice since it will be defended whatever its proponent later discovers concerning its truth; this is especially to be feared in the case of metaphysical views since more is at stake in such broad world views. And, this is the point, these considerations affect justification; one crosses over into the area of validation of theory (whatever its source) without retreating to the conflation of proof and discovery. It is simply the case that traditional justification did not rely completely on abstract formal considerations but also on more vague considerations such as "openmindedness" as well. And, Agassi, "Unity and Diversity in Science", Boston Studies in the Philosophy of Science, ed. by R S. Cohen and M. Wartofsky, Vol. 4, 1969, pp. 463-522; see also, IHS, pp. 33, 46, 62, and no. 125 for comments on informal, semi-ethical considerations in traditional methodology, and the doctrine of prejudice.

- 36 Koyré, MM, p. 19. See also the vivid and related descriptions on p. 3, and references to Meyerson in the notes on pp. 18 and 19, of the same volume.
- 37 NS, pp. 4, 5, 7; Burt, MFMS, p. 226.
- 38 MM, p. 18; The resistance may be perfectly compatible with Galileo's own metaphysics--just as there were no real frictionless planes to testify to free fall, one would not bring a telescope to discover geometrical truths about the heavens. But this was not the reasoning of the scholastics.
- 39 MM, p. 2, Galileo did not reach the law of inertia due,
 "only (to) his reluctance to draw, or to admit, the ultimate consequences--or implications--of his own conception of movement, his reluctance to discard completely and radically the experimental data for the theoretical postulate he worked so hard to establish, that prevented him from making the last step."
- 40 NS, p. 117. Newton achieved the law of universal attraction and Huygens did not because Huygens could not in good conscience surrender the metaphysics of Descartes which outlawed it as an explanation; Newton shared the metaphysics but bent it.
- 41 See above, p. 21, the passage cited there.
- 42 I am stuck, like many commentators, with the confusions of the tradition in which words like "apriorist" mean many things. To some, it means anyone who is not an empiricist; to others the list must at least be restricted to philosophers who lived after, say, Montaigne. Apriorist may mean "arrived at before empirical input" or "innate" or "guaranteed by a source independent of experience". Koyré and Burt are called apriorists occasionally because they speak of frameworks arrived at before the facts which the frameworks handle; but they do not claim any authoritative status for the outlooks which make up the framework or the facts. Thus, they are not apriorists in the traditional sense.
- 43 Dudley Shapere, Galileo, (Chicago, Univ. of Chicago Press, Phoenix book, 1974), p. 130. Shapere asks, "Can the 'Platonic', 'rationalistic', interpretation of Koyré's be borne out"? But, he does not discuss the issue beyond the labels. One does not quite know what rationalist amount to--apriorism or simple anti-inductivism, for example. He does not clarify his own position much by saying the "platonic" means "exhibiting the chief tenets of Plato's own thought". (p. 130). One can easily conclude that shapere will be criticizing Koyré only after simplifying his position to a

traditional, apriorist one. Indeed, he has what I take to be an unhelpful discussion of Galileo's view on the certainty of knowledge and the sources of truth--it misses entirely Koyré's point that Galileo's Platonism was his orientation toward anti-inductive methodology. (p. 133, but for qualifications to this, pp. 134, 145).

- 44 E.g., C. O. Gillispie, The Edge of Objectivity, (New Jersey: The Princeton University Press, 1960), p. 523: Koyré's "writings have revealed to me wherein the intellectual value of the history of science consists". This is an example of the vague acknowledgements of the value of Burt's and Koyré's work.
- 45 Agassi, "UD", discusses Kant's attitude toward metaphysics in a slightly different way from that followed here. (p. 521.)
- 46 Burt, MFMS, pp. 19-20, and last chapter.
- 47 There is a chronic difficulty due to an important equivocation on just this point in Burt's and Koyré's work: does the metaphysics entail the science? No, but is it separated from the science? No. Does it direct the science? Yes. How does it do that without entailing scientific positions? This issue is given consideration in Section 4.2 below.
- 48 Kant, Immanuel, The Metaphysical Foundations of Natural Science, with an intro. by James Ellington, (New York: Bobbs-Merrill Co., 1970, orig. publ. 1786.) pp. 4 and 6, for Kant's declaration of the need for certainty in science proper and his declaration that science necessarily involved metaphysical principles. The translator comments on the fact that Kant's metaphysics is not doubtful speculation, pp. x-xii. See Critique of Pure Reason, trans. by Norman Kemp Smith (New York: MacMillan and Co., 1929), Chapter III, Sections 3, 7, in the Transcendental Dialectic; and pp. 659ff., 664-665.

See also, Popper, Conjectures and Refutations, (New York: Basic Books, 1962), pp. 26-27, 47-48ff., and Chapter 7.
- 49 Agassi, THS, p. 6 and "SPRM", p. 194; for alternative logical relation between science and metaphysics; also "SPRM", pp. 204-206.
- 50 Errol E. Harris, Foundations of Metaphysics in Science, (New York: Humanities Press, 1965) Harris approaches such a view but his metaphysics is that of Whitehead which is not the established metaphysics of traditional science; thus, he advocates reform but no new claim for metaphysical influence on science--metaphysics still emerges as true from science.

- 51 Harris, FMS, This characterization is not quite accurate; Harris verges toward irrationalism when he does not maintain the more traditional view. Yet, this irrationalism is based on an aprioristic attitude toward relations between metaphysics and science--metaphysical dictates science--and therefore the status of the two fields remains equivalent and to him this means no critical relation hold between them.
- 52 Duhem, his multivolumed history, La System du Monde, (Paris: 1913-1958). Also ASPT, see No. 40 above.
- 53 Kuhn, "HS", refers to the "new historians" carte blanche and alludes to Koyré loosely as a follower of Duhem, (p. 76). Many historians, as noted above, acknowledge their debts too vaguely to be useful.
- 54 Agassi, THS, Section 9, pp. 31-33, for an extended explanation of this major point in Duhem's critique of traditional history of science.
- 55 Duhem, To Save The Phenomena, (Chicago: Univ. of Chicago Press, 1969), p. 60; also pp. 61, 63.
- 56 Agassi, THS, p. 33, for a cryptic but important note on why early scientists' realistic beliefs were important in a criticism of Duhem in spite of clear alternative of reinterpretation cited above. The passage is referred to in the text below.
- 57 THS, p. 57. And see Koyré, NS, p. 7, n. 3; AR, pp. 122, 123 on Kepler's realism.
- 58 TSTP, pp. 59-60. PT, pp. 224-226 for Copernicus' radicalism as responsible for the unhealthy overthrow of important medieval theory of weight; and for he and Galileo being led astray on the tides via their metaphysics, p. 239f. Also, pp. 113, 115, for their sincere but mistaken beliefs in metaphysics rather than experimental laws.
- 59 ASPT, Ch. II, Section 4; also pp. 334-335; and see DeBroglie's acknowledgement of this faith in his introduction: pp. ix-x.
- 60 See NS p. 57, and n. 3 on page 4 for a good example of Koyré's divergence from Duhem precisely on the importance of realistic interpretation of theory. It is not clear how Burtt or Koyré might label their view of early scientists to distinguish it from the philosophic alternatives; that is, since early scientists were not inductivists, not radical, not conservative, and not conventionalist, one needs a different characterization. Popper would want to say that they are "critical rationalists" who criticized their tradition and replaced parts of it with bold conjectures criticized in their turn. Koyré rejects this view explicitly:

"What the founders of modern science, among them Galileo had to do was not to criticize and to combat certain faulty theories or to replace them by better ones. They had to do something quite different. They had to destroy one world view and replace it by another. They had to reshape the framework of our intellect itself; to restate and to reform its concepts and to evolve a new approach to Being..."
MM, p. 90.

Kuhn, on the other hand would want to say these early thinkers were defending a paradigm which they adopted through a gestalt switch. Feyerabend says they are clearly hippie anarchists doing their thing, which includes (Galileo) brainwashing, but there is no position that can be systematized. Shapere has been quoted to say they are Platonistic apriorists with more hope; Koestler calls them "sleepwalkers"; Agassi describes their position as that of "radical reactionaries" who do want revolution of a radical nature but only so as to wipe out everything and go back to the pure sources. I prefer something on the order of the last position, although Shapere's when spelled out comes close to it as well; but, it is not necessary to take a stand here.

61 Kuhn, "HS", pp. 70, 81; Agassi, THS, Section 8, and pp. 32, 41.

62 Kuhn, *ibid.*, pp. 69-70, for example, introduces Meyerson's, Duhem's, and Burt's, contributions without mentioning the word, metaphysics. Agassi, THS, pp. 29, 31-33. Shapere discusses Galileo's platonism and Koyré's claims about it without introducing "metaphysics"! See Shapere's Galileo, pp. 128-133.

63 Agassi, THS, p. 33 quoted above in the text; p. 24, for Koyré acknowledging and building on controversy; pp. 54, 64, 75, for the relations of advances in this regard to philosophic liberalization of views on scientific justification. Also, Koyré, NS, p. 5, n. 2:

"Philosophers are often inclined to misjudge the situation of contemporary philosophical positions and--when dealing with the past--to forget that as often as not and even more often than not philosophic and religious teachings are not so much expressing as opposing the prevailing trends of their time."

64 NS, p. 3, n. 2.

65 Kuhn, "HS", pp. 78-79; Agassi, THS, Section 7, pp. 23-28; Guerlac in SC; Thakray, "SHIR" in MSPS, Vol. 5.

66 Koyré, NS, pp. 6-7, n. 3; and, MM, p. 17.

- 67 Agassi analyses and ridicules the uncritical attempts to use the need for "technological invention as an excuse to explain mistakes or delays in the progress of science, or in explaining successes: THS, p. 45, and no. 145.
- 68 Brinton, p. 465.
- 69 Kuhn, "HS", p. 76.
- 70 Ibid., p. 76.
- 71 Robert Merton, "Science, Technology, and Society in Seventeenth Century England," (New York, Harper Torchbooks, 1970).
- 72 Koyré, "Commentary", in SC, p. 856.
- 73 Arthur Koestler, The Sleepwalkers, (N.Y.: Grossett, Dunlap, 1963).
- 74 Kuhn, "HS", p. 79.
- 75 Guerlac, p. 811.
- 76 Ibid., pp. 811-812.
- 77 Koyré, "Commentary", in SC, pp. 857-852.
- 78 Ibid., p. 857.
- 79 Agassi, THS, p. 48, although this is quite mild. Guerlac: p. 797: "Written history can only be highly selective, at best a mere sketch..."

It is still necessary to acknowledge the noncontemptible thought that one's own special interest may introduce a significant bias in one's history--even if one thinks this is inevitable and not to be struggled against. In fact, one object as strongly to the relativistic position that each point of view has its truth and that therefore there is no such thing as parochialism as to attempts to reconstruct "past actuality". The parochialism of one's personal bias--or impersonal intellectual bias--may rob one of valuable insight into history. The intellectual bias against the possible rationality of considering some confused metaphysical views part of rational research in science is an example of such a bias. The point is only that trying to recover the past interferes with the goal of trying to avoid significant biases as much as possible. (See my criticism of G. Witteridge quoting sources, Ch. III note 3.

- 80 See G. B. Shaw's preface to Back to Methuselah for the argument outlining this criticism against the desire to live forever.
- 81 Koyré, "Commentary", p. 851, n. 3.

CHAPTER II. BURTT, KOYRÉ, AND THE ASSUMPTION OF CLARITY

Chapter II. Burtt, Koyré, and the Assumption of Clarity

This chapter is directed to an explanation of the fact that an entire introductory chapter has been necessary to describe the unique and revolutionary message concerning science and metaphysics which can be seen in the work of Burtt and Koyré but which has been obscured and neglected -- partly through characteristics of their own presentations. In more detail, the problem is the following: E. A. Burtt and Alexander Koyré were sophisticated, sympathetic commentators who not only stressed the role of metaphysics in science but also stressed the conflicts, obstacles, and difficulties in the construction of the new science.

Yet, in spite of these merits of their works, latter commentators have neglected the conflicts and emphasized the impressive defense and smooth reconstruction of the metaphysics. The rough edges on the metaphysics of scientific thinkers like Harvey, Kepler, and Newton have hardly been acknowledged until recently and only with a shrug or a qualification which destroys their force. The rough edges -- even some of the wildness and inconsistencies in the metaphysics influential in science -- appear on the pages of Burtt's and Koyré's work; but these classic commentators themselves hardly pause over the passages. Certainly, their successors have not absorbed the fact that their work puts on the agenda the extremely interesting and challenging problem, how can a confused view -- a confused metaphysical view at that -- rationally contribute to the development of science? Why hasn't this question established itself?

One answer, of course, is that a shrug is all the problem of the relation

of a confused metaphysics and science deserves. However, another explanation is developed in this chapter. In part, aspects of the Burtt-Koyré presentations themselves contribute to the underestimation of the force of their work. They do not concentrate on the most extreme incoherencies and confusion in early metaphysics. Also, they encourage the assumption of clarity by stressing the power and important influence of the neo-platonic metaphysics of the late Renaissance; this stress includes the idea that it functions as a preformed and considerably independent factor in scientific developments.

Secondly, the prevailing philosophical atmosphere was positivistic and hostile to any connection between science and metaphysics; it directed Burtt and Koyré, and their interpreters, to concentrate on the point that metaphysical theories actually did beneficially interact with scientific views and that these metaphysical ideas were not non-sensible, meaningless, pseudo-views but intellectually potent and powerful systems. In this enterprise of resurrection, their statements indicating that the metaphysics influential in science was not entirely unproblematic -- easy enough to underestimate -- were simply slighted. The fact that metaphysical theories had to be taken seriously as an unjustifiable and as parts of science was disturbing enough.

In fact, as a third and most influential factor, the absence of a way to describe science as including unempirical and unjustified elements and as irrational, made even the plain interaction between science and metaphysics difficult to absorb. Any further indications that this metaphysics was confused or inconsistent would involve greater difficulties; e.g., in contradicting important historiographic policies. This is explored in the latter part of the chapter.

In Burt's and Koyré's presentation, the scientific systems are unified and impressive; the metaphysical systems were developed prior to the science and gave rise to important elements in the science; the science manifests or "conforms" to the metaphysics.¹ Quite naturally one can get the idea that the metaphysics under discussion is, as a matter of course, clear and coherent. One must emphasize that neither Burt nor Koyré claim that a clear metaphysics gives way to science. Each author is noted for exposing obstacles, difficulties, and previously unappreciated intellectual conflicts in the development of modern science. The impression of an unproblematic metaphysics working as a guide in the background of science arises as a stubborn unintended by-product of their work and the view itself can only be fully attributed to some of their successors. The assumption survives their critical approach in part because the difficulties they discuss can be interpreted as technical difficulties in applying or interpreting the metaphysics; also, neither classic author discusses explicitly the problem of how to connect a confused metaphysics rationally to a clear science. As a result, each author's work -- with its stress on the powerful influence and aprioristic development of the metaphysical base -- leaves an impression of the intellectual respectability vivid enough to invite inaccurately streamlined images of scientific development.

This invitation was taken by later commentators who adopt the assumption of clarity fully and implicitly as part of a very reasonable, superior, but mistaken, historiographic policy. This view of influential metaphysics is too powerful to be adopted implicitly as it has been by these later commentators -- such as

Rattansi, McGuire, Cohen, Yates and Pagel; moreover, the policy protects a distorted, overly smooth, view of the scientific process.

Section 2.1. Burtt's Foundations and the Assumption of Clarity

E. A. Burtt, in his classic work, The Metaphysical Foundations of Modern Science,² drew up a revised picture of the development of science; he pictured it not as a narrow, technical, positivistically-based enterprise but as the intellectual effort to institute a new metaphysics -- a new world view which had many alien qualities and many difficulties connected with it. This view of science as involving comprehensive and powerful metaphysical views was more accurate, more exciting, more noble than the view of science as a narrow, technical discipline. Moreover, his work includes a careful and critical examination of that metaphysics and its establishment at the foundation of science.

Yet, although Burtt worked toward a more realistic view of science, the information modern scholars give us concerning the Renaissance metaphysical arena from which science arose reveals that his picture of science is too streamlined, smoothed, not representative enough. Burtt's work, actually, can be seen to have encouraged, in part, the viability of the assumption of clarity -- the view that any metaphysics influential in science must be clear and consistent. One can see, in fact, that many commentators familiar with Burtt's work understand it to be fully consistent with the assumption of clarity, not a refutation of it. How can this be explained?

The question is not easy to answer; particularly in light of the splendid critique Burtt gives of the metaphysical base of modern science. Yet, there are explanations which reveal how his study protects and promotes the assumption despite its great merits. This explanation brings out "weaknesses" in his study

which could not have been seen before his study was done; they illustrate its value rather than lessen it. An explanation which accounts for both the facts of Burtt's critical attitude, sophistication, and sympathy for metaphysical influence, and the streamlined view of the development of science which his work presents, consists of the following points:

Burtt does not focus on the esoteric, mystical, or occult parts of the metaphysics he deals with; one can say his problem made them irrelevant to some degree. Burtt discussed the role of metaphysics in science in a philosophic atmosphere of positivism -- which made the unsubstantiated and apriori character of the metaphysics a challenging enough point: clarity did not have to be considered. In fact, the positivistic philosophic climate included the traditional theory that metaphysics is meaningless pseudo-science which only interacts with science to the detriment of the latter. So, Burtt had to emphasize the coherence and meaningful content of the metaphysics to make his -- true -- point: that this metaphysics was the intellectual groundwork of the impressive achievement we call modern science.

Also, Burtt never gives an unqualified criticism of the internal coherence and clarity of the metaphysics which fostered science; his strongest attacks are so closely bound to his points that the metaphysics is liable in being empirically unproven and inimical to a favorable philosophy of man that no other independent claims -- against the coherence and clarity, e.g. -- can be separated from them. In a few ways, in fact, even Burtt's attacks enforce the idea that the scientific world view was based on a clear and coherent metaphysics: e.g., it is a pervasive and inescapable depressent. Without clear and unqualified disclaimers,

it is hard to reach any conclusion but that the metaphysical groundwork at issue must be a clear consistent system of thought -- which is the assumption of clarity.

Let me review these points somewhat more slowly.

First, it is necessary to recall the major message of Burtt's classic work since that message itself encourages the assumption of clarity. The general message of The Metaphysical Foundations of Modern Science is that a powerful and unsubstantiated metaphysical component was injected into science at its creation through such ideas as force, mass, primary quality, atom. Burtt is at one and the same time establishing metaphysics per se as a true, undeniably present and active, intellectual force in the development of science and exposing that foundation as devoid of the invulnerability which traditional and positivist thinkers of many varieties claimed for it. He emphasized both the power and content of the metaphysics and the fact that the ideas existed independently of and prior to the empirical achievement which we honor as science.

Much of this character of Burtt's analysis can be gleaned from the following passage which describes Newton's importance in the creation of a metaphysical basis for science:

... Newton is an exceedingly important figure... He not only found a precise mathematical use for concepts like force, mass, inertia; he gave new meanings to the old terms space, time, and motion, which had hitherto been unimportant but were now becoming the fundamental categories of men's thinking. In this treatment of such ultimate concepts, together with his doctrine of primary and secondary qualities, his notion of the nature of the physical universe and its relation to human knowledge (in all of which he carried to a more influential position a movement already well advanced) -- in a word in his decisive

portrayal of the ultimate postulates of the new science and its successful method as they appeared to him, Newton was constituting himself a philosopher rather than a scientist as we now distinguish them. He was presenting a metaphysical groundwork for the mathematical march of mind which in him had achieved its most notable victories. Imbedded directly and prominently in the Principia, Newton's most widely studied work, these metaphysical notions were carried wherever his scientific influence penetrated, and borrowed a possibly unjustified certainty (my emphasis) from the clear demonstrability of the gravitational theorems to which they are appended as Scholia. Newton was unrivalled as a scientist -- it may appear that he is not above criticism as a metaphysician.³

A careful look at this passage -- supplementing it with other points from Burt's work--can reveal several aspects which would direct readers to the assumption of clarity and deflect attention from the fine introduction to the less refined parts of scientific metaphysics he does include, e.g., on Kepler and Copernicus discussed later. First, the metaphysical groundwork he describes Newton refining is a groundwork -- that is, it is done before the empirical work, as part of a philosophic movement which preceeds the science; the science follows the metaphysical groundworking. To add to the emphasis above one can cite the question with which Burt chooses to open his study:

Why did Copernicus and Kepler, in advance of any empirical confirmation of the new hypothesis that the earth is a planet revolving on its axis and circulating around the sun, while the fixed stars remain at rest, believe it to be a true picture of the astronomical universe? This is historically the most convenient question with which to open our attack.⁴

The emphasis on the prior ^{-to-}fact status of the metaphysics which was influential in science has a few functions in Burt's critical analysis. Most importantly, it represents his attack on the traditional and the later positivistic

view of the justification of science, the empirical entrenchment of its world view, and generally the indubitability of its assertions, particularly its view of man in the world. Burt is concerned to attach the authority claimed by the scientific world view -- not its claims to clarity and coherence.

To repeat, the alleged justification of the world view established in science not the clarity of scientific metaphysics, is the center of the attack. The point is so central to this analysis that it needs to be illustrated further. In the following passage Burt again indicates that it is the proper establishment of the metaphysics which is part of science, not its clarity, that is in question:

Modern thinkers have been so unanimous and so vigorous in their condemnation of the manner in which large propositions were imposed on innocent minds by external authority that it has been rather easily taken for granted that the propositions themselves were quite untenable, and that the essential assumption underlying the new principle of freedom, the manner in which knowledge was successfully sought with its support, and the most general implications about the world which seem to be involved in the process, are thoroughly well grounded. (my emphasis)⁵

This emphasis on justification, not clarity, is reenforced in many ways. Specifically, Burt's main message can hardly be articulated without emphasis on the coherence and power of the metaphysics he is talking about; that it was a metaphysics -- not stray intuitions of scientists or odd glimpses into ultimate foundations -- which formed the foundations of science; this is important to his point. Moreover, in drawing out the presuppositions of the scientists as a metaphysical system which was powerful and pervasive, he gives strong indirect support to the idea that this metaphysics was a coherent and clear system.

For example, consider the inclusiveness and fundamental character of the concepts with which Newton concerned himself -- as noted -- in the passage cited at the beginning of this section: force, mass, inertia, space, time, motion. These are complementary concepts in the new metaphysics; the point is stressed and thereby the coherence of the system -- although not its justification -- is strongly suggested. Even when Burtt is presenting Kepler's odd combination of mystic and scientific views, he must for his point refer to the inclusiveness of the new definitions as well as their character as apriori and religiously connected views.⁶ The point is that *in saying* there was a performed metaphysics which became the groundwork for science -- the mold or pattern of which the new science followed -- one ipso facto directs attention to the character of the metaphysics as clear and coherent. A message is nearly unavoidable.

Another closely related emphasis encourages this line of thought: namely, science is indeed consistent, clear and coherent. Any metaphysics which it followed, which provided *its* groundwork, would have to have been equally clear and consistent if it really did follow its grounding. This is another conclusion, the premises for which dot the pages of the Foundation. Burtt praises modern science in many places -- as in our first citation in which he mentions the "successful method" of the new science, the "clear demonstrability of the gravitational theorems," and Newton's status as an "unrivalled" scientist.⁷ To see how far he actually goes in this position, one can note his summary of the phenomena:

The actual achievements of the new science were undeniable; furthermore, the old set of categories, involving, as it appeared the now discredited medieval physics, was no longer an alternative to any competent thinker. In these circumstances it is easy to understand how modern philosophy might have led into certain puzzles which were due to the unchallenged presence of these new categories and presuppositions.⁸

None of them (the critics) subjects the whole system of categories (my emphasis) which had come to its clearest expression in the great Principia to a critical analysis. It may be that their failure to construct a convincing and encouraging philosophy of man is due in large part to this untested remainder.⁹

Both features of his point are apparent here: he is "forced" to emphasize the systematic character of the categories and call attention to the fact that they were successful to explain why they were accepted: thus he is forced to say the science was successful. The point is that it is quite easy for his own readers to assume that any system which can provide a sufficient number of intermeshing concepts which join in the production of an elaborate and successful physical theory such as Newton's, must necessarily be a cohesive body of thought.

One can see three other lines of argument which are part of Burt's attack but which enforce the focus away from the lack of clarity toward other flaws; namely, Burt's belief in the elemental force of metaphysical views, his ideas that the modern metaphysics has an nearly inescapable depressive force, and the fact that its creators emphasized clarity and consistency as a sine qua non.

To explain the first point, Burt is an exceptionally uncompromising believer in the force of metaphysical world views as controlling influences on

thought; a belief which refers to their elemental and fundamental nature quite apart from their connection with any successful science. In the opening pages of his book, he notes that metaphysical concepts far outflank other popular concepts in directing thought and that our scientific age has such a systematic and controlling substratum:

It may very well be that the truly constructive ideas of modern philosophy are not cosmological ideas at all but such ethicosocial concepts as "progress," "control," and the like. These form a fascinating key to the interpretation of modern thought and give it a quite different contour from that which it assumes when we follow up its metaphysical notions. But with that aspect of modern thinking we are not concerned in the present treatment. In the last analysis, it is the ultimate picture which an age forms of the nature of its world which is its most fundamental possession. It is the final controlling factor in all thinking whatever. (my emphasis) And that the modern mind clearly has such a picture, as clearly as any previous age that one might wish to select, it will not take us long to see.¹⁰

The metaphysical world view is a "controlling factor in all thinking" indicating that it has no cracks in it; such a blanketing effect implies (not logically but intuitively) that the metaphysics is coherent and clear -- or how could it keep such a pervasive grip? Later, Burtt points to the repeated attempts of philosophers like Berkeley to temper the cold, inhuman metaphysical world view behind modern science as testimony to the wholesale force of the view as

... to the

The constant renewal of these attempts (to reinstate man with his high spiritual claims in a place of importance in the cosmic scheme) and their constant failure widely and thoroughly to convince men, reveals how powerful a grip the new view they were attacking was winning over people's minds...¹¹

This again is a testimony to the metaphysics which is an essential ingredient in our scientific and social Burt's argument and which keeps impressions of its lack of clarity or coherence at arms length. Moreover, it indicates the second factor which recurs in his work and which leads to a strong impression of the coherence of the metaphysics; namely, its power as a depressive agent. The famed opening section of Burt's book feature Bertrand Russell's myth of Mephistopheles creating the meaningless world for his own amusement and destroying it for as little reason.¹² This extensive alienating quality of modern scientific picture of the *world* is featured throughout the text as it is in the citation above; and it is strong indirect testimony to the force and coherence of the metaphysics which -- in Burt's view -- gives the new science its characteristic orientations.

The same indirect force attaches to the fact that the scientists and thinkers studied themselves promoted, with some fanfare, the view that all fruitful opinions are clear and consistent. Galileo, Descartes, Newton, are well known to have closely associated if not identified, acceptable theories with true theories and also with clear theories. Their standards in these matters were markedly more severe than those of their predecessors and in some cases even of their successors.¹³ The mere fact that these thinkers cultivated certain metaphysical ideas lends force to the impression that the metaphysics in question must have been unexceptionable with respect to its clarity and consistency.

Of course, there is no logical necessity here; Galileo could have been mistaken in thinking his neoplatonic metaphysics -- which recommended clarity -- was clear itself. In saying that Galileo and other early thinkers who advocated clarity as a sine qua non were influenced by metaphysical views,

Burtt cannot be taken by strict logic to be declaring these later ideas clear. Yet, in the absence of an extended discussion or plain disclaimer, the mere fact that such thinkers took the ideas in question very seriously and found them helpful in their endeavor -- as Burtt unquestionably does argue -- reinforces the idea that they were in fact clear.

Burtt does extensively question the "validity" of the metaphysics of early scientists and this must be acknowledged. From my own perspective, indeed, I find it curious that his work did not make more radical a philosophic impact given his splendid discussion of the role of Kepler's mystic~~o~~-pythagoreanism in his science and of its dual character as modern and mystical is excellent.¹⁴

One can criticize Burtt's account as tending toward inconsistency and ambivalence on a few points: *Are* there really theoretically barren facts or not -- and this point is discussed in Section 4.2 -- and can the success of this metaphysics really be credited to serendipity as he implies?¹⁵ However, one could hardly ask for more by way of a presentation, without softening, of the intellectually off-color aspects of Kepler's metaphysical positions. The coherence of the new world view and its "accidental" success is captured very well in the passage:

Kepler early realized that the admission of validity to the Copernican world-scheme involved a radically different cosmology, a cosmology which could rest upon the revived Neo-Platonism for its general background, would find its historical justification in the remarkable developments in the sciences of mathematics and astronomy, and which could lay bare a marvelous significance and a new beauty in the observed events of the natural cosmos by regarding them as exemplifications of simple underlying numerical relations. The task involved revising to this end the

traditional ideas of causality, hypothesis, reality, and knowledge; hence Kepler offers us the fundamentals of a metaphysic based in outline upon the early Pythagorean speculations, but carefully accommodated to the new ideal and method. Fortunate indeed it was for Kepler's historical importance that his venture proved pragmatically successful. The acquisition of further empirical facts in astronomy by Galileo and his successors showed that the astronomical and physical universe was enough like what Copernicus and Kepler had dared to believe for them to become established as fathers of the outstanding movement of human thought in modern times, instead of being consigned to oblivion as a pair of wild-minded apriorists.¹⁶

In the case of Kepler in particular,

... his outlook and method were as fully dominated by an aesthetic as by a purely theoretic interest, and the whole of his work was overlaid and confused by crude inherited superstitions which the most enlightened people of his time had already discarded.¹⁷

Many commentators on metaphysics and science and on Kepler ignored or dismissed Burtt's presentation here for some time; and one can explain why by noticing that to a large degree Burtt ignores the philosophic problems generated and prefers to emphasize the lack of justification of the metaphysics and its incompatibility with a comfortable and "sensible" concept of self and knowledge rather than to discuss its function of unclear metaphysical views in the developing science. Both Copernicus and Kepler appear in an introductory capacity.

Burtt does not dwell on lack of clarity and most of his references to it are tempered by these other two concerns. For example, Burtt approached a criticism of the internal coherence of Newtonian metaphysics in an offhand remark on the aether in his concluding summary.¹⁸ He notes such confusions of

Galileo and the unsophisticated metaphysical outlook of Newton generally due to the latter's dose of positivism.¹⁹

Newton receives extensive criticism as a metaphysician; he is unpracticed, not thorough, and oblivious. He did not think out the metaphysical consequences -- (N.B. not contradictions) -- of his new language and categories.²⁰ Burtt does not say his views were unclear or incoherent; he stresses that they were uncritically, thoughtlessly adopted and misguided from the point of view of a "convincing and encouraging philosophy of man," as noted in the first citation in the section. Not accidentally, the criticism of Newton the metaphysician which is seen to be allowable in that passage refers to this problem of self and mind, not to clarity or consistency.

This is how focus on the nature of the metaphysics in science as confused is tempered and somewhat obscured even by Burtt himself who appreciates it fairly fully. It is not surprising that in the positivistic atmosphere in which he wrote the work, commentators did not search out the problems connected with a confused metaphysics and found themselves with challenge enough with an unsubstantiated metaphysics leading to a peculiar philosophy of mind.

Likewise, the discussions in the concluding critical analysis of Burtt's book -- discussions which might seem to suggest that the previously unnoticed and unjustified metaphysics can, when examined, be seen to be incoherent -- actually refer to inadequacies in the philosophy of mind and self. Let us take a look at an extended statement:

... when, in the interest of clearing the field for exact mathematical analysis, men sweep out the temporal and spatial realm all non mathematical characteristics, concentrate them in the lobe of the brain, and pronounce them the semi-real effects of atomic motions outside, they have performed a rather radical piece of cosmic surgery which deserves to be carefully examined. If we are right in judging that wishful thinking in the interest of religious salvation played a strong part in the construction of the medieval hierarchy of reality, is it not an equally plausible hypothesis to suppose that wishful thinking of another sort underlay this extreme doctrine of early modern physics -- that because it was easier to get ahead in the reduction of nature to a system of mathematical equations by supposing that nothing existed outside of the human mind that was not so reducible, naturalists proceeded at once to make the convenient assumption? And there is a certain pre-emptory logic in this.

... This once done, all the other features of their cosmology followed as naturally as you please. It has, no doubt, been worth metaphysical barbarism of a few centuries to possess a modern science.²¹

The message that the metaphysics is weak is so closely connected with the view that it is unproven and not amenable to a more reasonable philosophy of man that it is very difficult to separate any further message about lack of clarity or coherence. Burt talks so directly, it seems, to a positivistic audience that one could conclude that he uses their view that all unproven theories are ipso facto meaningless and thus Galilean-Newtonian metaphysics is suspect and vulnerable -- but on no weaker criterion. That the metaphysics of science was maintained by "wishful thinking" does not mean it was maintained by, or in spite of, inconsistency or confusion; in fact, the passage indicates the opposite -- there is a logic to the ideas. Also, when Burt says the rest of the cosmology

follows "naturally" from certain assumptions, he clearly is saying it follows clearly and consistently: the assumption of clarity. ¶When they are ^{not} put in the context of a traditional or positivistic criterion of knowledge as remarked above, the impossibilities, gaps, and inadequacies in the new world view analyzed in Burtt's final chapter are connected with a meaningful concept of self. This point has been made a few times in the citations presented in this section and deserves to be emphasized: Burtt's references to "metaphysical barbarism" and allusions to unsatisfactory character, even nonsensical aura, of the scientific metaphysics are presented as reflecting his commitment to a "meaningful order" in the universe and the need to have a metaphysics consistent with what he presents as a reasonable philosophy of mind. The metaphysics is not internally inconsistent, but inconsistent with various attempts to "construct a convincing and encouraging philosophy of man" and to "reinstate man with his high spiritual claims to a place of importance in the cosmic scheme;" it is not clear whether Burtt wants to, or is able to, make a stronger point against the metaphysics of classical science on a less psychological criterion of inconsistency.

Of course, there are reasons beyond the positivistic tradition which repressed *the notice of* problems of unclear metaphysics in science; there is the problem that it is difficult to maintain historical integrity, paradoxically, without assuming that a thinker was clear. This view is discussed in Section 2.4 below. Moreover, philosophically, it is difficult to describe a thinker as unclear and yet rational; a problem which also appears later in this chapter and is addressed directly in Chapter IV. Although not specific to Burtt's work or its impact, these two general reasons why, despite his work, the assumption of

clarity was encouraged and protected, are extremely important.

Section 2.2 Koyré and the Assumption of Clarity

Koyré's view on the relationship between metaphysics and science is more developed than Burt's; it also presents the relationship more as a critical interplay. His presentation of the conflicts, obstacles and difficulties emphasizes the intellectual problems which spurred on modern science; he also describes inconsistencies and difficulties -- as in the theory of the void and of action at a distance -- which took generations of effort by the finest minds to overcome. These characteristics of Koyré's work on the development of science make it superior: he does not beautify and refine the activity into the automatic production of a method or of the technical requirements of society.

Yet, his work has not been noticed as an attack on the assumption of clarity. The philosophers who notice it stress that he showed that metaphysics as metaphysics, not as pseudo-science nor as empty formalism, informs science; his presentations are not understood to be criticisms of the assumption of clarity -- that the metaphysics which interacts with science is clear and consistent and intellectually respectable; and, his disciples, although among the best and most critical historians of science, do not see and follow this more revolutionary aspect of his view of science.

As with Burt the explanation of this phenomena lies in part in the work of Koyré itself. Koyré, for one thing, is no philosopher; there are few general, abstract discussions of the relationship between science and metaphysics; Koyré does not try to spell out in detail which theories of science or of rationality might be debilitated by his vision of science, nor does he articulate a general

theory just what the interaction between science and metaphysics is in his view. More than in Burt's case, the absence of a general philosophic discussion of his findings weakens their force.

Moreover, Koyré's work does share most of the characteristics which make Burt's Foundations a source of the impression that the metaphysics in science is clear and coherent. That is, Koyré argues against positivism and so stresses the aprioristic aspect of influential metaphysics; he also argues against positivistic views that metaphysical ideas are flimsy and nonsensical in some way. In fact, since he does not share Burt's poor opinion of the philosophy of mind provided in the world view of science, his presentation of the metaphysics does not have even the allusions to incoherence which Burt makes on behalf of a more hopeful image of man and knowledge. Rather, Koyré stresses more than Burt the intellectual power -- the undeniably rich content -- of these metaphysical views. This is a major reason why his work seems to allow the assumption of clarity to remain in the background.

Also, Koyré discusses at length the new high standard of clarity and intellectual honesty adopted by the early scientists -- largely as a result of the metaphysical beliefs they held; in this too, he goes somewhat farther than Burt. In addition, one must note that many of the intellectual conflicts and battles he discusses concerned difficulties in translating the comprehensive and extremely powerful metaphysical view into a scientific system; it is often not apparent whether or not clarifying the metaphysics itself was a problem -- and if so how much of a problem. Moreover, neither determination of his position on this issue seems compatible with all the rest of his views. As with Burt,

this allows the suggestion of clarity and consistency in a pre-developed metaphysics which sets a foundation for science to follow to make the stronger impression than any idea of confusion and incoherence. Finally, in large part, the resiliency and vitality of an assumption of clarity must again be traced to the strong historiographic and philosophic nourishment it receives -- much more than to the oblique suggestions Koyré's presentations allow.

To review these factors: I think one has to emphasize, in any argument that Koyré's work has or can support the assumption of clarity, that he is magnificently elaborate in his insistence that the metaphysics of science came first, before the empirical achievement and its substantiation (which we are wont to call "science"), and that the revolution directed by this metaphysics was extremely, impressively comprehensive. Let us dwell with Koyré on his idea of the scientific revolution:

Let us dwell for a moment upon this revolution, one of the deepest, if not the deepest, mutations and transformations accomplished -- or suffered -- by the human mind.²²

I shall ... characterize this revolution by two closely connected and even complementary features: (a) the destruction of the cosmos and therefore the disappearance from science -- at least in principle if not always in fact -- of all considerations based on this concept and (b) the geometrization of space.²³

... the real world is no more seen, or conceived as a finite and hierarchically ordered, and therefore qualitatively and ontologically differentiated, whole, but as an open, indefinite, and even infinite universe, united not by its imminent structure but only by the identity of its fundamental contents and laws ... all its components appear as placed on the same ontological level ... identified and unified, in which astronomy and physics become interdependent and united because of their common subjection to geometry.²⁴

It is easy not to understand why classical science substituted a world of quantity for that of quality: ... there are no qualities in the world of numbers, or in that of geometrical figures.

And even more, ... it is easy now to understand why classical science substitutes a world of being for the world of becoming and change: just because ... there is no change and no becoming in numbers and in figures. But in doing so, it was obliged to reframe and to reformulate or rediscover its fundamental concepts, such as those of matter, motion and so on. ...

If we take into account the tremendous scope and bearing of this so deep and radical revolution, we shall have to admit that, on the whole, it has been surprisingly quick.²⁵

It is a thrilling story, the story of the successful and unsuccessful efforts of the human mind to formulate these new and strange ideas, to build up, or as Spinoza has so pregnantly said, to forge, the new tools and patterns of thinking and of understanding.²⁶

This comprehensive passage reveals most strongly Koyré's emphasis on the power and scope of the revolution brought about by the new metaphysical orientation. He puts in a footnote his qualification to the conclusion that the metaphysics consistently excluded "purpose" related to "cosmos" from the vision of the world, saying that Newton's world picture included it (before LaPlace) and that explanations which referred to purpose were used by Kepler. This remark reveals a break in the scope of the revolution; however, here it is parenthetical and by the way and has only lately been the focus of concerned attention.²⁷ In his beautiful rendition, the thoroughness of the revolution, the unified nature of the result (more unified by law as required by the metaphysics), the comprehensive character of the change required by logical consistency with the geometrization of space, and the thoroughgoing reformulation of language --

all these are vivid testimony to the scope and consistency of the metaphysics; as he notes, Koyré, himself, seems to conclude in the emphasized paragraph.

The final paragraph in the citation above in which Koyré calls the development of science according to this metaphysics a "thrilling story" also mentions not just successful but "unsuccessful" efforts to "formulate" these ideas. The perceptive will pause at this point to surmise that this comprehensive metaphysics is not completely preformed and ready to follow; however, he immediately refers to "patterns of thinking and of understanding" which the science would certainly follow -- when possible; thus, the impression of weakness is immediately attenuated. Readers looking for indications that the metaphysics of science might not be clear could find them here; but it is not clear how they can be melded to the vision of comprehensiveness and coherence of the world view which is also stressed.

The idea that the metaphysics of science is preformed is also important; it provides one of the most consistent and strongest suggestions that it must also be basically clear and coherent. On this point Koyré is also definite and unqualified. The passage cited in Chapter I on this point may be expanded and augmented here:

And as for experience and experiment -- two things which we must not only distinguish but even oppose to each other -- I am convinced that the rise and growth of experimental science is not the source, but, on the contrary, the result of the new theoretical, that is, the new metaphysical approach to nature that forms the content of the scientific revolution of the seventeenth century, a content which we have to understand before we can attempt an explanation (whatever this may be) of its historical occurrence.²⁸

This stress on the anti-inductive nature of the methodology of early scientists is repeated often, for example, in The Astronomical Revolution:

It is rather strange that the astronomical revolution, not only as regards its origin -- the observational data of Copernicus are almost the same as those of Ptolemy -- but also as regards its development, was almost entirely independent of the development of observational astronomy. Kepler, tells us that the eight minutes difference between theory and Tycho Brahe's observations are at the root of his Astronomia Nova ... but, in fact, this difference was of importance to him only because he was trying to explain it within the framework of an astronomy which he describes as that is to say, within the framework of a celestial physics conceived long before he had access to Tycho Brahe's observations. Interpreted within the framework of astrokinematics -- the framework of Tycho Brahe -- the difference in question would have had no serious consequence.²⁹

These powerful passages from the point of view of the preeminent role of a metaphysics that is not created by the facts but exists prior to their establishment as significant -- determining in part which are significant. The point that the metaphysics is not produced by facts suggests strongly that it exists preformed, and that the scientific development followed the metaphysical outline. Even famous examples of a seeming commitment to empirical accuracy as the Kepler incident above are explained as devotion to a previously conceived and accepted world view rather than any rejection or tempering of such commitments.³⁰ Pointing this out with the thoroughness and emphasis that made it an unavoidable feature of the debate on science and metaphysics is one of Koyré's major contributions.

Again, as in the case of Burtt, these arguments that metaphysical

world views preceeded and directed scientific progress, tend to encompass the idea that metaphysics was clear and coherent. Koyré does not say this; in fact, he mentions several theoretical problems which the metaphysics itself forced on the scientists attempting to follow it -- e.g., the difficulties with the void, of absolute space, of action at a distance -- which will be discussed below. But these problems, for one thing, do not necessarily imply confusion in the metaphysics and they are easy to slight in a rendition which so strongly and properly emphasizes the strength and power of the metaphysics despite its problems in the development of science. Finally, many of Koyré's claims are hard to make coherent on any other assumption besides that of the prior development of a metaphysics; specifically, his strong claim that the metaphysics colors the language of research and makes it possible only make sense when one assumes the prior development of the metaphysics and something at least of its consistency, and as noted, prior development suggests clarity and smooth application.

To further illustrate how the assumption of clarity can seep up through Koyré's critical discussion of problems -- and to approach a report of his direct discussion of the obstacles made for scientists by metaphysics -- one can refer to his account of the development of the Newtonian science as a "synthesis" of the two earlier opposing metaphysical trends, the mathematical Galilean view and the Corpuscularian view of Boyle. This abbreviated account gives an accurate impression of two recurring aspects of Koyré's analysis: that of the power of the metaphysics and that of its challenge.

The physico-mathematical current I have just been sketching is certainly the most original and most important trend of seventeenth century scientific thought. Yet, parallel to it there runs another one, ... this current is inspired not by the Platonic idea of the mathematical structure and determination of being, but by the Lucretian Epicurean, Democritean conception of its atomic composition ... Looking at things from this perspective, Newton presents us with a synthesis of both trends, of both views. For him, just as for Boyle, the book of nature is written in corpuscular characters and words. But just as for Galileo and Descartes, it is a pure mathematical syntax that binds them together and gives its meaning to the text of the book. The introduction of the void -- with its correlative attraction -- into the world view of Newton -- in spite of the tremendous physical and metaphysical difficulties involved by this conception (action at a distance, existence of nothing) was a stroke of genius and a step of decisive importance ... The cautious corpuscular philosophy did not really know what it was doing. But as a matter of fact, it had been only showing the way to the Newtonian synthesis of mathematics and experiment.³¹

In the pages summarized in this passage, Koyré so impressively reconstructs the production of the Newtonian world view -- and makes it an impressively coherent synthesis -- that one can hardly help but assume that the metaphysical views involved were intellectually respectable in all significant respects.

The qualifications and problems mentioned here and in other reconstructions will be discussed below. Their existence means that one can by no means conclude that Koyré himself holds the view that there was here a development based on a clear, preformed metaphysics; but his phrases both here and in other places, and his ambition to show the power of the metaphysics, allow that interpretation. In a tradition which treasures the assumption of clarity, his works could be made innocuous and kept out of the stronghold by a stress on them.

Yet, to get any accurate idea of the relation of Koyré's work to the viability of the assumption of clarity, one must review and examine at least a few of the many instances in which he reports conflicts, obstacles and difficulties, of a theoretical kind, in the development of science. It can hardly be emphasized enough that this quality makes Koyré's work what it is: a cut above most commentators in the field. He not only reports difficulties but intellectual difficulties; not only intellectual difficulties and problems but those that involve metaphysics. His vision of the history of science is more rich, humane, lacking in a great deal of the artificial smoothness of many idealized pictures drawn previously. The criticism made here will still be that he does not go far enough; but his criticism could hardly have been made without the example of his own work on which to base it. Yet, in stressing the reality of the intellectual battles, he made them theories which produced them a bit too real, too preformed, too ready on their own, to contribute to science. But let us look first at how much Koyré avoids the assumption of clarity.

Many passages from Koyré's major works cited already in this section express specific denials -- or ideas close enough to denials that they must be noted -- of the assumption of clarity. For example, Koyré mentions both "successful and unsuccessful efforts of the human mind to formulate these new and strange ideas" (emphasis mine). On the same page, Koyré begins his account of the "conflicting" metaphysical orientations which led into the Newtonian synthesis, the Platonic and Corpuscularian. Here, of course, is a

representative and important example of Koyré presenting the "creation" of the new world view in a way that stresses it was not a preexisting ready guide; "creation" of the world view in this sense immediately opens the possibility that the metaphysics was both helpful and part of the project at hand. That the early scientists had to form and forge (as cited above) the new metaphysics is the strongest argument Koyré can bring against any charge that he represents the development of science as smooth and unproblematic. The significance of the point is that the problems involved in creating and forging the new world view are problems which lie beyond the ordinary theoretical difficulties of resolving inconsistencies between theory and fact. The history of science, in this view, suddenly has a far rougher and obscure terrain; scientists have not merely to resolve logical conflicts between theory and fact; they have to create the world view that made those conflicts apparent.³²

This emphasis is revolutionary; but, it is a serious puzzle as well -- for, it conflicts with other strong suggestions Koyré must make concerning the development of science; namely, the necessity of adopting a metaphysics to provide a language, standards and a pattern for scientific development.

There is a real conflict here between the "problem" of creating a metaphysics and the argument that a metaphysics formed and created the science. Koyré simply does not discuss this conflict. Perhaps it can be explained as merely apparent but he does not do it. Also, one may be able to abstract from his historical reconstructions and arguments a theory which reconciles these two important claims; nevertheless, it seems clear that most of Koyré's followers

have preferred to abstract a rather innocuous and streamlined version of the history of science which ignores these problematic points as much as possible.

The most apparent and important difficulties with which Koyré deals at great length are those involved in the Newtonian "synthesis." As cited above, Koyré does say that the admirably coherent world view synthesized from Platonism and Corpuscularianism involved serious metaphysical and physical problems -- in its introduction of the void, of action at a distance, of attraction. The difficulties mentioned are classical metaphysical problems: just the sort which positivists claimed made meaningless vagueries of world views. Koyré goes on to discuss the facts -- well known at the time -- that several "features and implications of the Newtonian world view [were on generally accepted criteria] unclear and unintelligible,"³³ that they progressed from, as Mach put it "uncommon incomprehensibility (to) common incomprehensibility"³⁴ and they were "obscure."³⁵ Koyré, in fact, explains at some length the drastic change introduced through these concepts:

We have to dwell for a moment upon the more general and more diffuse consequences of the universal adoption of the Newtonian synthesis, of which the most important seems to have been the tremendous reinforcement of the old dogmatic belief in the so-called "simplicity" of nature, and the re-introducing through science into this very nature of very important and far-reaching elements of not only factual but even structural irrationality.³⁶

Much of the discussion in the whole of Newtonian Studies is a closely argued reconstruction and analysis of the "irrationality" *made* to bring out the real problems and intellectual, theoretical battles over what *one* calls confusion and

inconsistency which made up the development of science. One can only admire the vigor, honesty, and vitality in these accounts; why the radical change they implied in the rationale and methodology of science was not more widely appreciated is not easy to understand.

To add an equally strong further indication of Koyré's developed and subtle orientations, one must note that Koyré openly acknowledges confusion and lack of clarity in his discussion of Benedetti's contribution to the development of the law of free fall. He not only mentions confused views in this case but declares his appreciation of them in general -- in direct contradiction to the assumption of clarity.

On pourra dire, sans doute, que la question que nous venons de poser n'a, en elle-même aucune importance: la pensée de Benedetti étant obscure, et même assez confuse, ses glissements, ses inconsequences, s'expliquent par la même d'une manière suffisante ...

Or, certes, la pensée de Jean Baptist Benedetti n'est rien moins qu'un modèle de clarté. C'est néanmoins une pensée vigoureuse et honnête. Il ne faut pas oublier, en outre, qu'une pensée -- en général et aux époques de transition en particulier -- peut être obscure et confuse, sans que pour cela, elle perde toute valeur. Bien au contraire, ainsi que l'a fortement affirmé Duhem, et ainsi que l'a admirablement montré Émile Meyerson, c'est dans l'obscurité vers le clair, on ne va pas du clair au clair, ainsi que le voulait Descartes.³⁷

This is a strong departure from the assumption of clarity and should have -- with the other discussions of obstacle and conflict -- inhibited its adoption by Koyré's followers. Koyré speaks here of thought in general, and although the context concerns Benedetti's physics, his acknowledgement of the presence of

important and (at the same time) confused ideas clearly has general applications in his outlook.

However, just how much this statement on the confusion of Benedetti's thought and the overly sanguine views of Descartes on clarity represent a stand on Koyré's part is not easy to determine. For one thing, referring to the transitional nature of an era, as Koyré does here, is a standard way to imply that the presence of confusion has no greater philosophic significance. Koyré mentions the transitional character of Benedetti's time but he seems also to want his acknowledgement of the latter's trouble to be taken as a general acknowledgement of confused ideas. Moreover, one does not know from the comment if Koyré means to call Benedetti's thought "obscure" in his own terms or merely in modern, unsensitized contexts. It seems nearly certain that Koyré does mean to call it unclear both in our terms and in Benedetti's own time; but there is enough ambivalence to accommodate either interpretation which obscures the conclusion in favor of a role for confused thought.

One must come to a similar conclusion in reference to Koyré's many remarks on the creation of the new metaphysics with much effort by the classical scientists. These remarks are clear and definite in a considerable number of cases; yet, they do not stand out sufficiently given the aspects of Koyré's work which emphasizes the power and coherence of the metaphysical forces. His successors who developed the Burtt-Koyré approach to relations between science and metaphysics certainly noted the conflicts due to confusion much less than they registered the underlying impression of clarity.

Thus, each of the instances above is important in understanding Koyré's

position. But, it can be argued, an examination of his work, its context, and their contexts, shows that most of them are not quite the staunch pillars against the assumption of clarity that they seem to be. Koyré's position is just tempered enough, and in some respects just ambivalent enough, to allow the assumption of clarity room to enter the treatment even though one cannot conclude that Koyré uses it -- at least not consistently.

Most importantly in the creation of that flexibility, Koyré often indicates that the difficulties involved in the development of the new science under the influence of the new metaphysics were technical. They involved, for example, the arduous tasks of developing mathematical tools and of reforming conceptual images to fit the new view. Some of these tasks -- indeed most -- are not technical in the strong instrumental sense of the word -- of requiring technical invention. They are technical difficulties in the sense that they do not involve major changes in the metaphysics; rather they are puzzles set by the attempt to follow the metaphysics into a fully developed science.

Koyré, of course, does not go very far in this direction and is, in fact, known for his avoidance of any such fault in the larger scheme: his work is a rejection of the view that technical or practical needs rather than intellectual problems were essential to the theoretical development of science (as discussion above in Section 1.3(d)). But, in discussion the difficulties inside the framework of following out a metaphysics, Koyré is hesitant enough at saying the metaphysics is ever in jeopardy or in the process of major change that he can give the impression that the difficulties he discusses are technical in this important sense.

For example, Koyré's remarks on the two conflicting metaphysical trends which make up the background to the Newtonian synthesis is not as much an acknowledgment of competing cosmologies as it might at first appear. Rather, he gives the conflict of a small part of a world view devoted to the possibility and nature of knowledge with the larger and more powerful image which neo-platonism offered. The hostility to sweeping generalizations, which Koyré brings up as part of the empirical tradition, would significantly disturb the picture of a smooth development on neo-platonic guidelines only if the overwhelming power of that opposing framework were not so clearly emphasized.³⁹

Also, one must note that many difficulties and achievements which Koyré mentions come up in the overcoming of the old world view while the development of the new science follows fairly unproblematically from the new metaphysics. This indeed is the most accurate description of most of Koyré's presentations of the history of the scientific revolution. That is, obstacles are real and intellectual, not pseudo-problems, practical or technical problems; but they concern replacing the old world view with the new one -- how to substitute mass for substance, let us say, or how to develop a mathematics which allows one to identify and calculate the new, essential properties of matter and no others.⁴⁰

Such external difficulties do not clearly and decisively cancel the impression of a development of the new science using a clear new metaphysics. There is, specifically, no stress on conflicts between the new science and the new metaphysics; and few difficulties in the internal consistency or clarity of the new metaphysics which gives rise to these tasks. So again, in spite of his

own recognition of the fact that much new thought is confused and helpful simultaneously, Koyré can report critical debate and clarifying efforts without destroying at least the underlying impression of the coherence of the operative metaphysics. Koyré in the end does not give a consistent view that the problems and conflicts he reports are important in contrast to the preexistence of the metaphysics as a guide, and readers may well overlook the significance of his treatment of them.

In conclusion, it will be appropriate to explain that Koyré would not have to celebrate confusion or contradiction in order to avoid the suggestion of underlying and basic clarity. Praising confusion as such is not necessary -- and it is not demanded or favored here -- in order to put forward a confused metaphysics as influential in early modern science. That is, to avoid a distorting suggestion of clarity, it is not necessary to claim that confused views are helpful in their confusion or because of their confusion; nor need one claim that contradictory views are helpful or influential in their very contradictions or because they are contradictory.⁴¹

One might think Koyré had to meet such a requirement in order to be free of the criticism that he allows an impression of clarity in the metaphysics to survive; but this is not so. One need say that the metaphysics involved was in fact confused and that at the same time or in the same form it was helpful and influential. It may have been influential in spite of its confusions, because of its confusions, or independently of its confusions; this can be left undetermined. All that is necessary is to say the influential view was in fact confused. This much, however, needs to be said explicitly and clearly without

the implications that the confusion was of course regrettable. Koyré comes closer than any commentator to this position, but he is not fully clear or consistent, and his work did not have the impact against the assumption of clarity one would expect.

Of course, as with the work of Burtt, strong traditions which make it difficult to claim that a view may be at once helpful and confused have worked against the full appreciation of Koyré's work. The strong theories which reinforce the assumption of clarity as an historiographic necessity are reviewed below; the philosophic traditions in Chapter IV. Both these influences are greater factors in the explanation of why Koyré's work has in part encouraged the assumption rather than destroyed it than any flexibility in his own presentation.

Section 2.3. Developments of and Departures from the Burtt-Koyré View.

Work done by followers of Burtt and Koyré reveals both the important influence and the inadequacy of their treatments of science. Commentators such as J. E. McGuire, P. Rattansi, Frances Yates, and I. B. Cohen follow Burtt and Koyré in their concentration on the ideological side of science and on seeing a contribution in its contemporary setting. This has led them, however, to a recovery of more eclectic, unsystematic, and obscure metaphysical views; those of the light-mystic, Hermetic-Kaballist tradition are examples. The extent to which these more obscure metaphysical views are the center of focus is a new and significant development. It constitutes a departure from the original Burtt--Koyré position since the classic authors makes the resurrection of a respectable traditional and objective metaphysics the central feature of the development of science. However, although their own research seems quite obviously to contradict an assumption of clarity, these scholars do not note or criticize the assumption; they take it for granted in a more powerful way. The assumption is not at all suited to the presentation of the less refined views they discuss and one can see that its adoption limits the effectiveness of their arguments and presentations. A report on some important representative samples of this work illustrates these points.

(a) The assumption of clarity in "Newton and the Pipes of Pan".

"Newton and the Pipes of Pan" by J. E. McGuire and P. Rattansi is a notable scholarly paper which concerns the interaction between metaphysics and

science.⁴² The "Pipes of Pan" deals with the general Scholia which Newton appended to the Principia and in which he presents quite extravagant religious beliefs, alchemical interests, conjectures as to the source of the pure wisdom of the ancients--the prisca sapientia of ancient sages. Not surprisingly, these concerns and opinions have been largely disregarded by admirers of Newton's science. When noticed, they are commonly disparaged as momentary inexplicable lapses, dismissed as insincere, or described as apologetics directed to traditional religious thinkers.

Rattansi and McGuire undertake to lay bare the opinions and the problems of the Scholia in terms of the contemporary traditions of their day. They intend to show not only that Newton was seriously concerned with the issues and opinions he expressed there but also that he meant seriously his view that they were an integral part of his scientific enterprise. By reconstructing the rich context which supported these concerns, Rattansi and McGuire intend to show that they are considerably more substantial than one would think prima facie.

To take the argument slowly, the stated aim of the authors, which they fulfill admirably, is to show that Newton took the more esoteric researches of the classical Scholia seriously: that he quite genuinely "considered the arguments and conclusions of the Scholia an important part of his philosophy".⁴³ To go further, the authors argue that this position was not "curious and anachronistic" except when it is seen in the context of an inappropriate and alien world view such as that "of the generally accepted view of the intellectual milieu in the late seventeenth and early eighteenth century England".⁴⁴ This

generally accepted view is the mistaken one that the time was secular, positivistic, or otherwise inimical to spiritual concerns. But when Newton's Scholia research is seen "in the intellectual environment of his country" accurately delimited, it can be seen to be a "legitimate task".⁴⁵

The criticism of the "Pipes of Pan" which is important to the argument at hand is this: besides the stated aims listed above, the authors consistently work toward another unstated but powerful aim, namely, to show that Newton's esoteric problems and opinions were in fact as reasonable in themselves and as consistent with his scientific world view as he and his contemporaries thought they were. In other words, Rattansi and McGuire state outright that Newton genuinely took the concerns of the classical Scholia seriously. He genuinely thought they were a coherent part of his intellectual world view which included the views and standards of the Principia. These two sets of concerns seem disparate to us but to Newton they did not. Rattansi and McGuire do not state that they think Newton was right, but they take great pains to suggest that he was. Their efforts include consistent care to present the views and opinions in these esoteric fields as clear and consistent, to present the broadened philosophic field which included them in the same way, and to refrain from any discussion of this presentation or possible qualifications to it.

As a more concrete example of this suggestion, one can examine the statement below.

The apparent contradiction between such a traditional Neo-Platonic philosophy [in the Scholia] and the stem inductivism of the Principia, dissolves when we examine more closely how Newton modified the "mechanical" philosophy of nature which was current earlier in the century.⁴⁶

In other words, Newton seems to be making a mash of his scientific world view only if one assumes he was a strict mechanist of the earlier Galilean-Cartesian variety. However, the closer examination which McGuire and Rattansi provide reveals (in their view) that the contradiction between Neo-Platonism and inductivism was dissolved because Newton "restricted" the mechanical philosophy "especially in its pretensions to knowledge of the natural world".⁴⁷ This amounts to the claim that Newton surrendered a large part of his inductivist philosophy totally--namely its claim to realism; it is a drastic path to resolve the contradiction. Yet, unless that is how the restriction is understood, the contraction does not dissolve.

In the same line of argument, the authors state that Newton failed to solve the ontological problem of causation but that this failure was "less significant than his attempt to investigate it through a unique combination of methods". This unique combination of methods was:

rigorously inductive philosophy, using controlled experiment and elaborate mathematics, complemented by an historical approach, reconstructing the prisca sapientia of the laws of God's agency in the world.⁴⁸

The point is that one cannot tell if the authors wish to say that this program was sensible as well as "unique". They seem to agree with Newton in his opinion that this attempt--the search through prisca sapientia of the laws of God's agency in the world with mathematical methods in order to solve the ontological problem of causation was well conceived, that there was no intellectual confusion or muddle in the combination. Yet this opinion is never

stated; much less discussed.

One might think that Rattansi and McGuire prefer to call Newton's methods "unique" because they want to avoid labelling them "irrelevant" or "ridiculous" or "not modern" according to the naive anachronistic standards that were traditionally used. But the label in fact allows them to suggest that Newton's methods were unexceptionable not only on the standard of the time but on their own standards as well, without stating this opinion. This is plainly unsatisfactory but is, I claim, the major function of the word. A similar significant ambivalence in position is reflected in the choice of the word "legitimate" in their statement that "in the intellectual climate of his century, [taking the Scholia program as part of his overall intellectual research program with his science], was a "legitimate" task."⁴⁹ [my emphasis]. Rattansi and McGuire do not say "correct", "admirable", or even "reasonable" here. This is not accidental. They do not want to make explicit comments of that sort; yet their presentation invites just such conclusions.

To review the situation, Rattansi and McGuire are caught in a dilemma which encourages them to present a distorted picture. They wish to make Newton's opinions understandable and fully appreciated not to dismiss them simply because they are strange and unfamiliar in the context of modern concerns and standards. This leads to their extensive efforts to reconstruct the opinions and traditions of the day which show the bases and background of those seemingly odd opinions. It also leads to their consistent care not to express any negative view of their own in the case. The combination gives the impression that they have a positive view. Yet they do not state a

positive view.

I have tried to show that Rattansi and McGuire hesitate to say that they find Newton's expanded world view reasonable but there are a number of alternative explanations of this hesitance. One might argue that it is their sense of the irrelevance of their own modern standards that keeps them from expressing any opinion on this point. This view however, is not compatible with the fact that the presentation as it stands quite consistently although implicitly suggests an opinion: namely, that Newton was right to view his Scholia researches as he did.

A more important argument is that the impression that McGuire and Rattansi agree with Newton is simply the by-product of their accurate reconstruction of his views. That is, they give a presentation of Newton's concerns which quite consistently shows them to be clear and consistent, reasonable views. They were such views; this impression is not the result of an implicit suggestion built in by the authors, although they would agree with a statement that Newton was correct in this matter because he was correct. This explanation will also not do. Both in the case of their attribution of "uniqueness" to Newton's methods and in the case of their attribution of "legitimacy" to his research program, the authors refrain from more positive expressions because they do not believe the positive evaluations. That is, they do not in fact think Newton was correct in his evaluation of his research program nor do they think the Scholia views were as clear and consistent as he found them to be.

This conclusion is supported by the fact that Rattansi and McGuire's own research as they report it uncovers the inadequacies of the Scholia ideas and

problems. The views Newton put on his agenda were, in their own terms, not coherent or open to rational consideration in the way his other scientific opinions were: To explain, Rattansi and McGuire themselves recognize that men have indeed often taken seriously philosophies they could not in principle expect to find comprehensible. As they point out, Newton's

harmony of the spheres rests on the belief that the true system of the world was known to the ancients but has been turned into 'a great mystery' which only initiates could penetrate.⁵⁰

The true meaning of the Old Testament would only become clear in retrospect, in the light of historical experience. ...Newton's textual analysis of ancient natural philosophy ...for him, represented a deeper penetration into prisca sapientia possible only when the preliminary work had been accomplished through experience.⁵⁷

To repeat the critical point, the authors avoid any suggestion of the need for criticism of this program and rather stress how unproblematically integrated this philosophy was--in Newton's mind--with his scientific program. They thereby suggest that it is similarly unproblematic to them that they find it easily integrated into his scientific program. But this is a position they would not, I claim, support if stated outright. Therefore, it is doubly unsatisfactory that their presentation transmits an implicit message on the point: the message is implicit and it is the opposite of the one they would hold.

To summarize, that Rattansi and McGuire agreed with Newton can not be taken seriously. They do not, therefore, avoid stating their own opinion of his concerns and programs because that opinion is irrelevant to the historical reconstruction. Their reticence represents not the irrelevance of their own standards of clarity and unreasonability but a conflict between their standards

and Newton's--a conflict they do not know how to handle and thus do not want to state. They do not know how to report their conflict without undermining their original program--to show how Newton's concerns and plans were appreciated in their own time.

I will argue below that this problem can not be solved unless one develops a theory of how to combine the cultivation of confused and inconsistent views with rational thinking; until this is done, commentators like Rattansi and McGuire who do not want ideas and programs dismissed uncritically as old fashioned or ridiculous will be "forced" to present them as clear and coherent in order to argue that a reasonable man took them seriously. The work of Frances Yates outlined below increases the problem in this area; Rattansi and McGuire are dealing with the concerns of Newton which are at the end of the period of Hermetic-Kabbalistic influence in the late Renaissance. At that period the tradition was more refined. Yates, however, deals with it in connection with science in its hey day.

(b) Frances Yates and the Early Hermetic-Kabbalist Tradition.

One of the more notable works on the Hermetic-Kabbalist tradition in its relation with science is Frances Yates' work, Giordano Bruno and the Hermetic Tradition.⁵² In the preface to that work, Yates describes the increase in interest in the Hermetic-Kabbalist tradition in the Renaissance. She does not mention Burtt or Koyré since she is not a historian of science; instead she credits P. O. Kristeller who developed a more accurate view of Renaissance thinkers such as Marcilio Ficino and Pico della Mirandola.⁵³ These thinkers

were formerly considered part of the undifferentiated humanist tradition. However, Kristeller showed that their interests and beliefs were really quite distinct. They were far more cosmological and philosophical than literary, more neo-platonic than either Aristotelian scholastic or Roman.

At the same time that Kristeller was uncovering the neo-platonic orientation of some Renaissance figures, other scholars were discussing the peculiarly vital tradition of Renaissance Kabballism. Yates connects this neo-platonic tradition with the Hermetic-Kaballist orientation in writing of Giordano Bruno--a figure important in the history of science traditionally. It is in this way that her study becomes part of the argument at hand.

Burt and Koyré do not treat Renaissance Kabballistic influences at any length; Burt, in fact, does not mention the particular brand of metaphysical thinking. Koyré does mention it but has no extended discussions. Rattansi and McGuire, who use Yates' work and the same sources she does, are exploring the last effects of the Hermetic-Kaballist movement in Newton's cosmological explorations. But, Yates' book on Bruno is a more significant development for it suggests that this philosophic-cum-religious tradition impinged on the science quite extensively at a time when the metaphysical tradition was not refined. Bruno was credited in many traditional histories with a lightened belief in the Copernican theory and with martyrdom to the modern theory of the infinite universe. To understand the true context of his beliefs makes him a rather wild philosophical magician seeking the proper cosmological pattern in which to work mystico-cosmological spells in a startling and significant change. Yates herself considers her portrait of Bruno revolutionary.⁵⁴

From the point of view defended in this dissertation the research Yates presents is a serious contribution to the debate on relations between science and metaphysics generally. From it, one can see more clearly than before that the metaphysics which surrounded classical science in its development was not the refined systematic body of theory discussed by many philosophers today and emphasized by Burt and Koyré. Rather the metaphysics was a set of vague, confused notions, not solely philosophical or cosmological but partly religious, mystic, even magical. Bruno is revealed in Yates' book to be a Magus, a mystical magician in the vein of alchemists and astrologers. His metaphysical views were blended with utopian hopes and plans for magical operations. His was just the sort of metaphysics which was most castigated by traditional philosophers of science. Koyré and Burt rescued metaphysics in its connections with science but only by emphasizing the more refined and powerfully developed systems which were impressive in the seeming coherence. Yates' work reveals the underside of those same systems and reveals them to be far more problematic as guides to science.

Yates work has consequences beyond those for the understanding of Giordano Bruno alone, of course; her books feature debates between Robert Fludd, a friend and associate of William Harvey, and Johannes Kepler. They are joined by D. P. Walker's further studies of Fludd and of other late Renaissance figures on the borderline between science and philosophy;⁵⁵ and several other scholars have investigated this metaphysical tradition and its connections with science.⁵⁶

It is a serious question whether or not Yates' study of Renaissance

Hermeticism has consequences in the larger history of science of that time and whether or not it affects the philosophic conception of science or methodology, Neither is obvious even when her results are accepted without demur.⁵⁷ Unfortunately, Yates also projects an ambivalent view of the nature of the metaphysical theories she explores. She wants to reveal the Hermetic-Kabalist world view as one significantly different from that previously attributed to respectable Renaissance thinkers and yet she also projects an implicit claim that the world view was coherent and clear. The policy is a particularly inappropriate one for her to follow. It is also, however, particularly inviting; for, besides the dilemma which pushes Rattansi and McGuire to a distortion of the true picture, Yates is working with much more incomplete and fragmentary sources. In such cases, the tendency to rebuild a tradition in a clear and consistent way is hard to overcome; one might claim in fact that such a reconstruction is the only fair one to make. In a similar way, one is tempted to assume that any lack of clarity or inconsistency is the result of gaps in the historical record rather than evidence of the nature of the world view.⁵⁸

The ease with which one may by-pass the apparent inconsistencies or the lack of clarity in a tradition is an important feature of Yates' work; it shows that the influence of the assumption of clarity goes beyond the option allowed for it in the works of Burtt and Koyré themselves.

(c) Cohen on Koyré on Historiographic Policy.

The assumption of clarity may or may not affect Koyré's own presentation of relation between science and metaphysics in a harmful way, but it is very

sensibly in the background both in estimates of his work and in the works of commentators who follow in his path. The Rattansi-McGuire article provides one example; I. B. Cohen's eulogy of Koyré⁵⁹ illustrates from another angle a crucial equivocation which derives from Koyré's approach - as Cohen understands Koyré's approach.- The equivocation concerns whether one can do credit to a thinker's ideas without crediting them with particular merits apriori.

In describing Koyré's distinctive attack on the history of science, Cohen links seeing an argument in its own time with making arguments coherent and valid. He does not equate the two explicitly but his report suggests it, he does not deny the equation, and no alternative is apparent. After saying that Koyré refused to give black and white judgements of theories by seeing them in terms of modern textbooks, Cohen describes Koyré's alternative as follows:

Professor Koyré always attempts to find that the basis might possibly be of the particular argument, theory or set of concepts which he encountered. ...Professor Koyré was interested [not like those who judge on modern standards] first and foremost to find out the degree to which men of science of the past may have been "right" in terms of their own times, not ours.⁶⁰

This is close to a direct statement of the point of view I find mistakenly supporting the assumption of clarity; that is, that to understand a thinker in his own time, one must understand him to be "right" in his own time. But there is no direct statement, only a vagueness that reveals an equivocation. The vagueness concerns whether "right in their own time" is to be contrasted merely to "right in our time" or it is also to be contrasted to "wrong (although perhaps reasonable) in their own time" or even "unreasonable (although perhaps understandable) in

their own time". This is precisely the difficulty which arose with respect to the "legitimacy" of Newton's concerns discussed above; here however, one is dealing not with a study which follows the Burtt-Koyré approach but an analysis of that approach itself.

Cohen is vague at exactly the significant point which would be the explication of what "right in one's own time" would amount to. At one time, he suggests strongly that if his view were ever spelled out, it would be that one does have to find the basis of an argument that makes it reasonable before one can be said to have appreciated it in its own time and not in an anachronistic modern terms. At other times, he suggests a more critical position. For example, in the paragraph following that cited above, Cohen reports that when Koyré analysed and reconstructed historical events, he tried to find out "whether or not" a particular idea "followed or did not follow" [my emphasis] from contemporary views.⁶¹

The "contemporary views" referred to above, however, include "either the explicit or implicit assumptions of the author or general propositions of the age in which that author lived."⁶² The first part of this quotation, in the paragraph above, indicates that several critical conclusions concerning the coherence or consistency of an idea are available; the second part of it makes the possibility of reaching them extremely remote. One can hardly fail to find some predecessor from which a particular idea follows or to which it coheres if one can resurrect implicit as well as explicit assumptions of the author and general propositions of the age at hand. Combine this with Cohen's further note that "in many cases it was necessary to study hard and deep to discover

such preconceptions"⁶³ and it becomes clear that in the fashion of medieval scholastic policies, these rules give one sufficient latitude in time and space that one need never declare a theory unreasonable, unclear, or without coherent connections.⁶⁴

Cohen caps the review of Koyré's policies by noting that the latter's analyses were often summarized with congratulations to the thinker in question for being "right".

Readers of Koyré's writings are familiar with his oft repeated comment after such an analyses: And he was right!⁶⁵

Koyré's sense of the importance of conflict prevented his misinterpreting the "right" in this phrase to the extent to which it is capable; but his followers such as Cohen do not have an interpretation of it which avoids serious equivocations.

However, to give credit to Cohen's more critical understanding of Koyré's policies, one must note that he quotes Koyré to say one must place works studied in their intellectual milieu and resist the temptation to make the thoughts of past thinkers more accessible by putting them into modern language which clarifies them but deforms them at the same time. Koyré is then quoted referring to "la pensée souvent obscure, malhabile et même confuse des anciens".⁶⁶ This is, one must argue, a warning which indicates both authors are aware of the dangers of beautification through the adoption of the assumption of clarity.

Yet, even this direct warning is equivocal; one is left with no answer to the question, are the thoughts Koyré refers to confused only to those of us

who see them outside their full intellectual surroundings? That is, are they confused only to those who are unfamiliar with them and therefore tempted to read modern meanings into them? Or, were the ideas understood in their confused sense at the time; were they perhaps confusions in the minds of the very thinkers who advocated them? The warning quoted does not indicate that the ideas in question must have been clear in the proper setting; but, it does not explicitly allow the possibility that they were either. The modern interpretation makes the ideas more clear to us than they would be to us; one still does not know if it is possible to conclude that the views explored were confused at the time.

In the same vein, Cohen goes immediately on to note that Koyré's studies of Newton according to the historical approach, "preserved some of Newton's original ambiguity, where we today would restrict him to the narrow confines of our own current technical expressions".^{b7} This is one of the clearist statements of the possibility of uncovering more confusion rather than less by placing a thinker in his own time. Yet, this is also not a secure indication that Cohen departs from the assumption of clarity. For, the modern technical expressions make the meaning clearer to the modern reader than the historical expressions would otherwise be to him—that is, than they would otherwise be given his uneducated, uninstructed sense of their context. But, Cohen does not say that the views themselves were unclear once that context is provided; he does not indicate that they remain ambivalent or unclear to our instructed understanding of them nor to the thinkers of the day. Also, if he had not made reference to "technical or non-technical" substitutes for the true understanding but had

compared the clear modern interpretation to a confused contemporary one, the message would have been clearly against the assumption of clarity. As it stands, it is not.

Thus, several of Cohen's remarks seem more critical than they are; the sensitivity of the Burtt-Koyré school of historians to historical accuracy leads one to be skeptical of any claim that they distort the ideas they discuss. Yet, it is argued here, all their critical awareness sidesteps criticism of the assumption of clarity; it is the one assumption which is regularly allowed to escape, despite appearances to the contrary.

(d) Holton on Kepler's Odd Metaphysics

In an extremely enjoyable article, Gerald Holton has made a study of Kepler's very unusual and active metaphysical orientation.⁶⁸ It is a conscious attempt to give a more accurate and full account of his world view and its importance in his scientific work than previous commentators have done. In fact, Holton aims to resurrect Kepler's metaphysics in the same way Rattansi and McGuire attempt to recover Newton's alchemical views and Walter Pagel to rescue Harvey's "biological ideas" (as described in the next chapter)

Since Holton acknowledges the oddity of Kepler's metaphysics and champions its rightful claim to be part of his science, hopeful readers might expect an open account of how a confused metaphysics influences science. In part, they will get such an account; Holton's study does show, in my view, that Kepler's metaphysics was confused and that it simply cannot be relegated to a pre-scientific limbo without relegating his entire achievement to the same limbo. Even more than this, Holton describes the function of Kepler's strange metaphysical views in some detail and so gives an account of just how such views can be helpful; his account corresponds in several particulars to the one I give in Harvey's case below.

However, for all this, the Holton article is a serious disappointment. It suffers from the same deficiencies as Rattansi's and McGuire's, and as Pagel's work below; namely, although the evidence he presents himself seems clearly incompatible with the assumption of clarity (and although he never makes the assumption explicitly), he tries to uphold it by presenting Kepler's metaphysics as unfused despite the great difficulties and awkwardness this forces upon him.

Like Pagel and the others, Holton dislikes the portrayal of Kepler as a weird schizophrenic who happened to have been a modern physicist part of the time but was a medieval astrologer at others.⁶⁹ This is a distortion of Kepler's thought which, he claims, fits neither category and which, besides that, was significantly unified. Holton wants to put forward Kepler's view as a unified and integrated melding of metaphysics, physics, geometry, and theology, not an arbitrary and insulated juxtaposition; the science and the philosophy were part of the same enterprise of comprehending the world. However, Holton not only wants to avoid calling Kepler's world view regressive, modern, or schizophrenic; he also wants to avoid calling it confused. And this, in my view, is impossible. The attempt nearly vitiates some of his best descriptions and renditions.

The question is, was Kepler's world view really or only apparently confused? Holton tries to consistently give the impression that it was only apparently confused--he even said as much. Unfortunately, this conflicts with his other major message that Kepler's world view was unified and integrated with his science.

I will review these points as well as the excellent insights Holton gives into the nature and function of Kepler's metaphysics. I can only treat these excellent points much less fully than they deserve.

Holton gives several excellent summaries of Kepler's genuine passion and ecstasy over his investigations and discoveries; he describes Kepler's openness in narrating his mistakes and blindneses, he reports with some vividness how closely Kepler associated his science with his love of God in passages that are not only enlightening but moving.⁷⁰ The points which are most important for the discussion

here are these: Kepler's outlook differs from ours in radical ways; it cannot be separated into modern categories; and it contributed to his scientific feats in fundamental ways.

First, although Holton qualifies this insight in ways I will criticize below, he does recognize that Kepler's intellectual outlook is hard to understand not merely because he had different problems and concerns than we do; he had a world view that included different standards of intellectual theory--it separates him not merely from us but from Galileo and Descartes. In Holton's own words:

Even in comparison with Galileo and Newton, Kepler's writings are strikingly different in the quality of preoccupation. He is more evidently rooted in a time when animism, alchemy, astrology, numerology, and witchcraft presented problems to be seriously argued. His mode of presentation is equally uninviting to modern readers....Nor is this impression merely the result of the inevitable astigmatism of our historical hindsight. [my emphasis]. We are trained on the ascetic standard of presentation originating in Euclid as reestablished, for example, in Books I and II of Newton's Principia, and are taught to hide behind a rigorous structure the actual steps of discovery....⁷¹

Holton is directing attention to the fact that Kepler's standards concerning what is sensible--and how to present it--differ from ours, even from his contemporaries. Unfortunately, Holton emphasizes the difference in presentation habits rather than the other standards although his account reveals both.⁷² This *comment* is exceedingly important; it is a declaration that the differences cannot be resolved merely by recovering details of the context; Kepler's view will not be ours no matter how much we understand of his context.

However, nearly as soon as Holton takes this enlightened step--as I consider it--he qualifies and soft-pedals it. Although he has just said historical *insight*

will not reconcile us to Kepler, he sums up his unusual standards of presentation by saying, "They mirror the many-sided struggle attending the rise of modern science in the early seventeenth century."⁷³ The summary is true but functions here as an implicit suggestion that the context makes them acceptable. Also, Holton emphasizes the divergence between our expectations and Kepler's manner of presentation as if our disagreements do not extend to deeper standards - yet his account reveals the opposite.⁷⁴ We do not simply hide the steps to discovery, our discoveries must meet different criteria,

Another excellent aspect of Holton's study is how persuasively it shows that Kepler's science and his metaphysics are thoroughly integrated. The following passage reveals not only the unusual nature of the metaphysics but this integration:

We shall see that when his physics fails, his metaphysics come to the rescue; when a mechanical model breaks down as a tool of explanation a mathematical model takes over; and at its boundary in turn there stands a theological axiom....

Kepler set out to unify the classical picture of the world, one which was split into celestial and terrestrial regions, through the concept of a universal physical force; but when this problem did not yield to physical analysis, he readily returned to the devices of a unifying image, namely, the central sun ruling the world, and of a unifying principle, that of all-prevailing mathematical harmonies.

In the end he failed in his initial project of providing the mechanical explanation for the observed motions of the planets, but he succeeded at least in throwing a bridge from the old view of the world as unchangeable cosmos to the new view of the world as the playground of dynamic and mathematical laws. And in the process he turned up, as if it were by accident, those clues which Newton needed for the eventual establishment of the new view.⁷⁵

Holton's summary is wonderful (except for its unfair and misleading conclusion). It seems clear that this is a metaphysics which is strange--in fact confused-- but which allowed Kepler to keep the important aim of unifying the world view on his agenda. Holton's account emphasizes both that Kepler's metaphysics "comes to the rescue" by keeping important problems from being abandoned prematurely, and in allowing scope for the passion to unify disparate visions of the world. These two functions - one can claim - are invaluable ones which even a confused metaphysics can perform; and this is one of the main conclusions of the dissertation, which Holton's study supports.

However, Holton does not declare that Kepler's vision--as described above-- is confused ~~or~~ inconsistent; nor is he consistent even on the point that it is not compatible with what we would consider acceptable in these areas. Like Pagel, Rattansi, and McGuire, Holton remains vague on the question whether he thinks Kepler's views were as sensible as Kepler thought they were. In introducing the world view in the passage cited above, Holton reveals his hesitance:

Conceptions which we might [my emphasis] now regard as mutually exclusive are found to operate side by side in his intellectual make up. A primary aim of this essay is to identify those disparate elements and to show that in fact much of Kepler's strength stems from their juxtaposition.⁷⁶

The "might" is significant here; it is inexplicable, I would argue, except on the theory that Holton does not want to say that Kepler's vision--for all the strength it gives his research--is incompatible with our conceptions; it is in fact confused and inconsistent. Does Holton really think it is a mere "possibility" that we hold

explanations in terms of physical force to exclude explanations in terms of theological proprieties?

Holton, of course, wants to insist that this world view was helpful, essential, in no way extraneous to, Kepler's scientific endeavor; Kepler's science simply cannot be extracted out from these overarching ideas that are alien to our standards. In this I would agree with him wholeheartedly; in fact, in this dissertation, it is argued that studies like Holton's on Kepler make the proposal that confused metaphysics has a role in science undeniable. But this is not Holton's conclusion.

Holton tries to maintain, in spite of everything cited here, that Kepler's world view was clear. He does not state this intention or view but his summaries are unmistakable. Consider the concluding and perhaps the best of Holton's descriptions of Kepler world view. In this passage, Holton claims at one and the same time that Kepler's vision was a unified, melded one in which physical causes, mathematical harmonies, and metaphysical priorities were on a level and interchangeable, and yet that this unified vision was not one that was confused:

So intense was Kepler's vision that the abstract and concrete merged. Here we find the key to the enigma of Kepler, the explanation for the apparent [my emphasis] complexity and disorder in his writings and commitments.

In one brilliant image, Kepler saw the three basic themes or cosmological models superposed: the universe as a physical machine, the universe as a mathematical harmony, and the universe as a central theological order. And this was the setting in which harmonies were interchangeable with forces, in which a theocentric conception of the universe led to specific results of crucial importance for the rise of modern science.

Holton claims here that there is no problem in thinking harmonies are interchangeable with forces; as he says, the disorders and complexity of the view are only "apparent". Yet, his own account is a vivid one of Kepler's struggles and battles in developing and using this complex and disordered view; struggles with three basically incompatible themes. It seems clear that for Kepler they were melded not merely held juxtaposed and in distinct compartmentalized versions; but that they were melded into a clear and consistent view in which the "confusion of incongruous elements--physics and metaphysics, astronomy and astrology, geometry and theology" was only "apparent"⁷⁸ seems as clearly false.

To summarize Holton's difficulty, if the different elements in Kepler's world view are kept distinct, it is easier to say he is not confusing one kind of view with another; but then, of course, he is a schizophrenic. If they are melded, they are part of his work, his science; but also, of course, he is very likely confused. Holton wants to say both that Kepler's views were melded and unified but that they were not confused. His essay wavers unsatisfactorily from one to the other and loses much impact because, I propose, this cannot be said.

The process Holton summarizes is one of an intellectual odyssey--of a great adventure and a great struggle. His hesitance to present Kepler's world view was unclear and confused partly obscures the difficulty of the adventure as well as the character and extent of his accomplishment. He cannot show as clearly as he otherwise would, that in as much as Kepler made great achievements, they were the result of an impassioned and heroic determination to understand the world, not to give up the search for a unified vision; and that in this

effort, his metaphysics was an invaluable but problematic and confused tool--not a smoothly prefashioned instrument. A great part of his achievement was his willingness to risk working on that confused metaphysical level for the sake of the questions it allowed him to handle and his ability to do as much there as he did. That his metaphysics was essential to his achievement does not mean that struggle with its disorder and complexity was not part of the problem. Holton's suggestions that the disorder and complexity were apparent not real forces him to misrepresent somewhat the nature of the job and to underrate the struggle and the accomplishment.

Moreover, Kepler's world view can be presented as part of a rational research program as interesting and powerful, without thereby having to be presented as clear, and Holton does not therefore have to substitute one unrepresentative reconstruction for another.

As a summary, let us ask directly how much Holton can tell us about the function of a confused metaphysics in science. Does his account help to give a more definite idea that confused metaphysical views do help in science and what kind of idea does his account present?

In this summary, it must be remembered that to argue that the assumption of clarity is false--to say that a confused metaphysics can have a role in science--it is not enough to show that some thinker held confused metaphysical views and was a scientist at the same time. For, the confused metaphysics may be interpreted as incidental to the scientific development, as a mere motivation for the development, or indeed as an obstacle which had to be put aside in order

to make the development; one can alternatively argue that the confusions in the metaphysics are merely "apparent". The arguments in this dissertation are directed to showing that none of these interpretations can be made to fit important cases without serious distortion of events; and despite Holton's own stand, his account supports this conclusion.

Holton gives a detailed and appreciative enough account of Kepler's views to show they do not meet present standards of clarity and consistency. Acknowledging that the metaphysics was confused, we can get a great deal of enlightenment about the role of confused metaphysics from Holton. His account makes the theory that the metaphysics was incidental laughable; the metaphysics supplied Kepler with most of the tools of his science--beyond motivation. And, as noted, he does say that the metaphysics helped Kepler by encouraging and allowing scope to his passion to comprehend the world as a unified picture--a scope he would not have had if he had abandoned all unclear metaphysics.

Thus, Kepler's case--like that of Harvey below--is even on a superficial review an prima facie example of a metaphysics which is confused being beneficial--essential--in a scientific development; when several intelligent attempts to explain this prima facie conclusion as mistaken fail, the confusion gains force. Also, the arguments which complete this study include explanations of how it is possible for a confused metaphysics to influence science beneficially in the sense that such an influence might be rational. Without such an explanation, or without at least criticism of theories which declare the impossibility, even specific historical evidence is hard to credit.

Section 2.4. The Policy of Avoiding Hindsight Supporting the Assumption of Clarity

There is an important reason why the assumption of clarity is not criticized even by historians undoubtedly concerned with accuracy such as I. B. Cohen or by scholars who deal with prima facie obscure metaphysical ideas such as Rattansi, McGuire and Yates. The historiographic policy of avoiding hindsight--seeing a theory in its own terms or in its own time, not being wise after the fact--seems to make the assumption inescapable. The assumption is part of an independent test of an historical interpretation. It is extremely difficult to drop it in the absence of a theory of rationality which allows a thinker to be confused and reasonable simultaneously.

To see how this is so, one must reconstruct the situation of the critical historian in some detail. He is faced, let us imagine, with a theory or problem which appears strange and unrelated to the successful efforts in a particular area. He knows that many advances in the field have occurred since the theory in question was being debated. These advances may have been made with the help of the theory but they still obscure the problems its proponents faced, the difficulties which loomed large, the advantages of the view at the time. Knowing this in general, the historian does not credit an initial opinion that a theory is odd, inconsequential, uninformative or confused. He tries to reconstruct the problem situation of the thinkers who invented or advocated the theory.

When he does this, the critical historian must use some standard to judge whether or not he has been successful at reconstructing the contemporary problem situation accurately. That is, he must decide how he will tell if his efforts to

overcome the parochial and unfavorable modern view of a theory have been sufficient to do credit to the view in question. A ready standard in this endeavor is the attainment of clarity and consistency: when the theory is seen to be clear, informative, a coherent part of the thinker's world view and consistent with his other beliefs or with popular views, one has certainly done one's best for it.

By the above route, the need for an independent standard transforms the aim of "seeing a theory in its own time" into "seeing a theory as clear and coherent in its own time". To be clear, an historian sensitive to the liability of being wise after the event can try to find background theories, problems, or traditions which make a seemingly unclear theory clear. But to succeed in the reconstruction of the view as clear and coherent easily becomes the test of success in overcoming one's modern point of view. When this happens, the assumption of clarity becomes part of policy and is beyond empirical criticism.

Thus, thinking along the lines outlined leads directly to uncritical policies in spite of the original intention. One must assume that any inconsistency or incoherence in a theory is merely apparent and will vanish when the historian does his homework adequately and thereby rescues the appropriate context. Whenever a seemingly unclear or inconsistent view presents itself, the critical historian is led most irresistibly to the view that the weakness is one in his reconstruction rather than in the theory. If every case of obscurity or contradiction is considered the result of the historian's inadequacy, it becomes impossible to attribute a confused or incoherent view to any thinker.

Of course, one might argue that no historian takes the extreme literal

position that the commentator is always at fault for not recovering the proper context whenever a view seems unclear; and further, that the moderate position--to take seriously the possibility that the view is clear, to assume it prima facie and look for the context--is very sound policy. This moderate position is superior but only when combined with clear standards on when one reverses the prima facie assumption; unless a standard is provided, the moderate position becomes the extreme one in spite of the historian's better intentions. (That such a standard can not be provided because there is no way to desert the assumption without declaring a view irrational is the argument developed below.)

One can see that the additional standard is needed by considering the particularly difficult cases of reconstruction when the traditions involved are incompletely documented or especially alien; the Hermetic-Kabbalistic view of the Renaissance fit both categories. In such cases, presenting that part of the theory which is clear and considering the rest as "in need of further study" seems both conscientious and fair; for, it may well be true that the part of the theory which appears unclear or incoherent will not seem so once more information or familiarity is achieved.⁷⁹

However, the "further study", which seems so reasonable here, may be prolonged indefinitely, especially if the need for a "deep and lengthy" search, which Cohen referred to, is considered appropriate. During this time, one is presenting an image of a view as completely clear--since the public parts of it are clear and the rest is merely alluded to as the object of research; whereas, what one has on hand in fact is, quite the contrary, a prima facie confused and

incomplete view which may never reach any other status. Thus, the moderate position is likely to produce the greatest distortion in those cases of odd traditions concerning which it is most tempting to adopt it. Yate's, McGuire's, and Rattansi's work are cases in point.

To go more deeply into the problem, it should be noted that the rule of thumb for good historical explanations--to find the contemporary context which makes a view clear and consistent--is extremely hard to resist. For one thing, the views which are tackled in such situations are propounded by thinkers who often are eminently successful in some other way. Newton, Galileo, and Harvey are examples. Were this not the case, their less current beliefs would very likely not be the subject of historical inquiry. Thus, one is dealing with thinkers who have given evidence of ability to reach fruitful, valuable, powerful theories which are also clear and coherent on modern standards. It makes some sense that the more minor, less famous views of these thinkers would live up to the same standards. If they seem prima facie not to do so, it is easy to conclude that further investigation will reveal the context which in turn reveals their clarity and coherence. For example, if Newton's Scholia researches appear obscure and silly, since the Principia shows Newton to have been a powerful and rigorous thinker, it is quite reasonable to believe that the other views he took seriously were judged by those same rigorous standards.⁸⁰

The point which must be emphasized is that this assumption is not uncritical hero worship. One does not assume the clarity of Newton's less familiar views just because he is a renowned thinker and had other respectable views. Rather, one tends to that assumption to avoid appealing to the ad hoc

theory that Newton was an intellectual schizophrenic--that is, that he used, quite unaccountably, two conflicting standards in judging his intellectual concerns.

To explain, the alternatives seem to be these: One can explain why Newton cultivates certain prima facie confused or obscure views by showing they can be explained as clear on further investigation; they do not constitute a lapse. Or, one can explain his interest by saying he had a blind spot or prejudice so his usual high standards did not apply. The latter attribution, that Newton was prejudiced, is not merely derogatory and hostile to him personally, it is also unexplanatory. Prejudice, like schizophrenia, indicates an inexplicable stance or split; it is as much a label as an explanation. That these two--the attribution of hidden clarity or the attribution of prejudice--seem to be the only alternatives strengthens the tendency to adopt the assumption of clarity as one way toward a more informative treatment.⁸¹

Similar reasons lead Rattansi and McGuire to emphasize that many contemporary thinkers would have found Newton's physico-theological interests quite unsurprising and unexceptionable. The important point they want to make is not that one can trust the opinion of the crowd--if they shared the view it must be clear or reasonable or valuable or whatever; but rather that this shows Newton's interests and beliefs did not reflect merely his idiosyncrasy. His apparently strange interest had an intersubjective status which shows in itself that they were more meaningful than the explanation via personal prejudice allows.

Thus, the strong point supporting the assumption of clarity is that it provides an interpersonal, non-ad hoc criterion for the accurate reconstruction of a

thinker's position. Clarity and consistency in the context of the thinker's other known theories and/or the contemporary world view is a standard that is independent both of the modern anachronistic point of view and of the individual historian's personal preferences or bias. When one has recovered enough contemporary background to explain a view as part of a coherent ideology of the thinker or as reflecting the climate of opinion, it seems clear that one is not simply imposing modern readings on to the view or presenting merely one's private understanding of it. The view as interpreted in these contexts has a credibility apart, as it were, from either of these orientations. Moreover, other people can judge that one has accomplished such an interpretation with some ease; clarity and consistency within a reconstructed framework is an intersubjective standard. There are very few alternative standards in these cases, one can argue; that is why the assumption of clarity remains operative and uncriticized in the writings of otherwise exceptionally critical commentators.

Of course, one must note that there is truth in these matters as in others, and it would be wrong to give the impression that historical reconstruction is a completely speculative enterprise with only abstract policy and the rules of logic to prevent arbitrariness and to judge accuracy. There is such a thing as historical evidence; recorded testimony, reports of activities, documents. The theory that some prima facie unclear views--e.g., Newton's--are in fact clear when the actual context in which they were held is recovered is testable: one can look for the traditions of Newton's time and error in these traditions. Likewise, the view that a certain context of traditions and problems which make a theory clear or consistent--e.g., the one proposed by Rattansi and McGuire

as Newton's world view--were in fact held by Newton at the necessary time and in the way they propose is a testable view. This is to say, such theories are certainly testable in principle. In the cases in which the historical record is ample, one may well be able to check reconstructions with some ease. When the historical record is not very complete or when it is controverted, the ability to check is inhibited and one must rely more powerfully on historiographic policy such as the injunction to avoid hindsight in order to achieve an accurate view.

However, even in cases in which the historical record is reasonably complete and uncontroversial, reconstruction will require decisions as to which part of the record one will take seriously and what qualifies as an acceptable explanation. Evidence which bears on the rationality of the thinker directly may be impossible to admit without destroying policy; and this is just the kind of evidence that the cultivation of confused or inconsistent views is. An historian may argue that it is preferable to attribute belief to Newton which one has no evidence he held than it is to accept a reconstruction which leaves his alchemical views terribly obscure and confused.

Another related point which brings out the dependence on policy is the point that it may be our evaluation of a thinker's more famous views which need to be changed to avoid a picture of him as an intellectual schizophrenic. That is, instead of assuming that the context which makes his obscure views clear is missing, one can determine that the context which shows the seemingly clear view to be less so is the one which has been lacking. There is a tendency to interpret the historical reconstructions of Burtt, Koyré and others in this way at

times. This alternative is resisted since although it is not ad hoc, it is associated with the view that the thinkers involved are being disparaged, or that their success in their scientific endeavor is being held either merely apparent or inexplicable. This is particularly the case when the cultivation of confused ideologies are at issue. If one had a theory which expressed the intuitive view that a thinker can be rational and indeed admirable in cultivating a confused metaphysical idea, this association could be broken. Until such a theory is available, however, it will be difficult to associate a thinker with confused views without seeming to disparage him.

More specifically, a philosophic theory of the rationality of cultivating confused ideas is needed to explain what kind of explanation one could give of a thinker's contribution if one wanted to present it as both confused and valuable at the same time. It is all very well to say that, e.g., Rattansi and McGuire need not present Newton as either clear or irrational, that there surely is an alternative. But, it is another thing to say what that alternative amounts to. Regardless of the historical information available or the historical information recounted as part of the reconstruction, an account may simply not be taken as an explanation if it includes evidence against the rationality of the thinker.

The remainder of the discussion of this section concerns these last considerations: that abstract theory plays an essential role in understanding and criticizing historiographic policies and historical treatments. It will be clear by now that the policy of seeing a theory in its own time is far from determinate. The conclusion is an intriguing one to reach since the policy is associated with the most sophisticated and critical historians; they seem more than any others,

to have a characteristic style. Perhaps the fact that these historians have presented such detailed, careful, and stylistic studies explains why few people have noticed that the policy describing their approach is vague and inadequate. Although one knows that the policy implies recovering contemporary information, there is little explicit elaboration beyond this point: e.g., concerning what sort of information should be recovered.

Partly, of course, the policy of reconstructing a view "in its own terms" or "without using hindsight" is defined negatively as simply not judging the theory according to modern ideas of what is true or false. One can surely add that the policy implies not judging a theory on modern ideas of which are clear or which ideas are important. In all these cases, one tries to replace the modern context with that contemporary to the idea or achievement.

However, what one should recover as the proper context in looking at the historical records is not clear; and it may cause confusion in this debate that the standard by which one selects the elements to create the contemporary context will be a modern standard. It will be a theory concerning "what makes an idea justified in its own time", "what makes a theory clear in its own time," "what makes a theory reasonable in its own time" or whatever the commentator thinks makes an idea a discovery or a contribution. These criteria are used to reconstruct the historical situation and they are timeless in themselves and reflect modern standards in fact.

To take this slowly, the directive, "to recover the contemporary context instead of employing modern judgement", can yield a number of conflicting historiographic policies. For, the criterion as to what is important in the con-

temporary context will be a modern criterion of the commentator. Because this is so, Bacon may be said to advocate "seeing a theory in its own time" when he advocates recovering the facts which were known prior to the theory and judging the theory in that context. The facts and the open or prejudiced states of mind of the various participants are the contemporary elements he thinks it is essential to note to appreciate the theory.

Conventionalists leap to the other extreme and conclude it is necessary to recover that in the contemporary situation which puts the thinker in agreement with other scientific colleagues and which adds to the predictive efficiency of established system. Immediate predecessors, their small improvements and underlying concern for simplicity are what, to them, needs to be recovered to see a theory in its own terms. For Burtt and Koyré recovering the context of the time means recovering the full meaning of the times and the cosmological picture behind the theories. The recovery must include the full appreciation of the metaphysical content of the terms and theories--that which goes beyond the content needed for prediction.

Other historians may think that seeing a theory in its own time requires the reconstruction of local material and social conditions which may have been aids or obstacles to the development of theory. To them, Koyré who recovers only the contemporary intellectual setting of a new idea or discovery is lumped with the a-historical traditional commentators on science who thought the only important context was that of the truth and the facts of the matter. The truth and the facts are universal and timeless, so historical construction is irrelevant. Koyré's contexts, ideas, intellectual difficulties, are also abstract and timeless

and thus may be considered a-historical. Thus, Koyré, who is most often associated with the historiographic policy of seeing theory in its own time and avoiding anachronistic standards, can be put outside that category on some interpretations of it.

Finally, we have seen that followers of Koyré such as Rattansi and McGuire have no articulated policy of what it is to see a theory in its own terms; they alternate between showing a thinker was genuinely concerned with particular views, that he was right to be concerned with them, and that he was reasonable to be concerned with them. It takes much more discussion than one might at first consider necessary to determine what critical historiographic policies amount to.

This leads to the question what more does one need to characterize a particular historiographic approach than the description of it as the recovery of the historical context. The answer to this question is tricky. One can say that one needs is a philosophic theory of scientific advance--what is a contribution and what is not. One can say that what one needs is a theory that explains in some non-ad hoc way which elements of the historical context are important. It is particularly important in the present case to inquire into such requirements in order to explain the weakness of even the superior positions.

The explanation Agassi provides for the superiority of the Burtt-Koyré view of the scientific revolution involves their reports of the criticism, failures to reach the truth, blind alleys, and controversy. Their historical pictures demand theoretically the categories "error" and "realistic speculation". The weakness of the conventionalist treatment in comparison is their hesitance

to recognize error as such, which leads to an arbitrary and indiscriminate revival of errors. First, their hesitance:

I propose that the great merit of the conventionalist method is rooted in its daring revival of old errors...

This is a merit because only by such a revival are discoveries seen to be non-ad hoc and not arbitrarily, out of the blue; they have a context. But, Agassi continues,

...and the serious shortcomings of this method are rooted in the fear of admitting that the revived errors are indeed errors. The conventionalist too is still under Bacon's spell; he too still cannot boldly admit the existence of errors of which mankind can be proud.⁸²

Burtt and Koyré consistently discuss such errors; but it must be noted that they do not articulate the theory which explains why mankind may be proud of certain errors. That is, they do not provide a theory which explains why errors--instead of being barriers and threats to the achievement of truth--are elemental aspects of the achievement of truth and represent bold and difficult efforts.

According to Agassi, Popper's theory is the only theory of science which goes any distance in providing the explanation of why errors are not barriers to the search for truth.⁸³ Whether true or not, one can in this way see Popper's philosophy of science as providing a clear statement of why and how the recovery of the contemporary context aids in truly understanding a theory or discovery of fact. That is, one recovers the theories held on a particular issue--the very explanatory but false theories--which implied that the event

discovered was impossible or which prevented one seeing the problem or difficulty leading to a new theory. The theories which are obstacles to a new development can not be understood either as prejudices or as mere instruments of calculation if they are to play the necessary role in historical explanation. In order to be part of an explanation of why a certain discovery was difficult, one must understand then to be good theories, not mere prejudices, and theories which it was reasonable to believe described the state of the world. This general theory captures a great deal of the excellence of the Burtt-Koyré studies, although not quite enough. The theories involved here are only scientific theories, according to Popper not metaphysical ones. But this point will be investigated below.

To summarize, some such general theory as Popper's is essential to a proper estimate of Koyré or Burtt's historical treatment since it explains why Koyré is not arbitrary or ad hoc in presenting the historical details he presents. Without it, details of Koyré's presentation could be dismissed as irrelevant or incidental or decorative. The attribution of error to thinkers was unsatisfactory when the only way to explain it was to attribute prejudice to those thinkers as well. It was unsatisfactory because the attributions of prejudice are so ad hoc (as well as false in fact). Conventionalist historians revive mistaken theories abundantly; but they not only revive them without claiming they were errors, they revive them indiscriminately. This procedure is as ad hoc as its predecessor.

If Koyré's view were merely that all errors are sources of enlightenment, it would be equally ad hoc. Koyré does not pick out any error; and one needs a general theory to describe why his choice is not arbitrary or ad hoc. This is

what is given by Popper's theory: in Agassi's presentation:

valueless errors are those which can easily be criticized by the person who holds them whereas valuable errors quite often can be criticized only after generations of combined effort by many people.⁸⁹

Moreover, Popper's theory gives the independent reason why the effort takes so long: the old theories are so explanatory and new theories take boldness and imagination.

The weakness in the sophisticated Burt-Koyré historiographic treatments and policies must also be explored by way of general theories. It is argued here that just as the conventionalist revives errors without calling them errors, the critical realist revives confusions and obscurities in metaphysics without calling clear attention to them as that. But, one can argue just as there are "errors of which mankind can be proud" likewise there are "confusions of which mankind can be proud" and unless one admits the category clearly one cannot discriminate. That is, one can argue that it shows great boldness, imagination and courage to put forward a (mistaken) theory in the face of an established explanatory theory, and that without such bold actions science would not progress. Likewise, it takes, once can argue, great boldness and imagination and courage to cultivate confused notions in the face of clear alternatives, yet that just as such actions are also - at times - necessary for progress.

However, Burt and Koyré's revival of errors must be shown to be selective and non-ad hoc in order to be valuable one must have a theory which explains which of the many confusions available are helpful to progress in

enlightenment and which are not. Again, one can parallel the characterization given to the Burtt-Koyré advance over Duhem. Just as those errors are valuable which are so explanatory and comprehensive as to take generations or centuries to refute, one can argue that those confusions are valuable which are so captivating and interesting as to take generations to clarify sufficiently to be tested. The general theory of the rationality of cultivating confused views would be this: if the choice before a thinker is the cultivation of confused ideas or dropping an interesting problem, we can congratulate a thinker for cultivating confusion; in these cases, one is reasonable to cultivate confusion. This is a general theory to describe which confusions are helpful and which not. In order to provide a basis for criticizing the Burtt-Koyré positions, it would have to be developed more clearly and at greater length.

APPENDIX A: H. A. Wolfson on Principles of Talmudic Text Study

H. A. Wolfson, a renowned medievalist describes the principles behind the medieval Talmudic study of texts in his book, Hasdi Crescas' Critique of Aristotle.⁸⁵ The work of Crescas, the principles Wolfson reports, and his own attitude toward both are very similar to those of scholars studying Renaissance metaphysics; thus, an analysis of his discussion shows the uncritical side of their policies quite vividly.

Wolfson first describes the work of Hasdi Crescas; it is a description which would apply to many Hermetic-Kaballist works with barely a change of word.

He [Crescas] not only re-echoes the ideas of his predecessors but he collates torn bits of their texts. The expository part of his work is a variegated texture into which is rewoven many different strands. Mosaic in structure, it is studded with garbled phrases and expressions torn out of their context and strung together in what would seem haphazard fashion.⁸⁶

Wolfson goes on to say that such a text is often hopelessly "unintelligible" or positively misleading. But, with luck and a peculiar brand of application, the scholar can properly rescue such texts. He can do it by adopting the approach which Crescas and other Talmudic (and Christian) scholars used in their own work.

The Talmudic approach begins with assumptions which make the assumption of clarity in the Burtt-Koyré position pale.

In this method the starting point is the principle that any text deemed worthy of study must be assumed to have been written

with such care and precision that every term, expression, generalization or exception is significant not so much for what it says as what it implies. The contents of the ideas as well as the diction and the phraseology in which they are clothed are to enter into the reasoning.⁸⁷

Not only does the scholar search for the predecessors of certain ideas and the origin to the various threads in the Mosaic tapestry of a particular work, he also must ask abstract questions. The lengths to which the Talmudic Scholar would consider necessary on their behalf of any studied text: frighteningly elaborate and dizzyingly openended.

To give just a few of the contortions a Talmudic Scholar would consider necessary on behalf of any studied text:

Confronted with a statement on any subject, the Talmudic student will proceed to ask himself a number of questions before he satisfies himself of having understood its full meaning. If the statement is not clear he will ask, "What does the author intend to say here?" If it is too obvious he will again ask, "It is too plain, why then expressly say it?"⁸⁸

One can already see the door to indefinite study opening; the scholar already has room to say the text requires more work on his part than one might think appropriate on modern standards. But, the efforts to fully appreciate the text hardly stops here:

If it is a statement of fact or of a concrete instance, he will then ask, "What underlying principles does it involve?" If it is a broad generalization, he will want to know exactly all the circumstances under which a certain statement is true and what qualifications are permissible. Statements apparently irrelevant to each other will be subtly analysed into their ultimate elements and shown to contain some underlying principle.⁸⁹

Given such extensive liberties in interpreting a text, any passage, no matter how incoherent, could be found to have layers of rich meaning. Given such extensive conditions necessary to a sufficient "understanding" of a text, any passage could be said to be in "need or further study".

The point is that although these requirements appear severe and stringent, they are so unbounded by any aim--beyond perhaps devotional hard work--or any standard, that they are instead, carte blanche. From apparent extreme deference to the text, one reaches arrogant disregard of it as an expression of an actual thinker's opinion on an actual problem. The disregard of the possibility of careless or superficial comment is only the beginning. The Talmudic scholar was also unbounded in his analyses by any developed historical sense or ability to date texts. Thus, the attributions of meaning and tracing of expressions and phrases to their source was liable to rest entirely on abstract connections which never could have been made by the--or any--actual thinker at the time.

Even Wolfson recognizes that no independent purpose or standard of successful performance was involved in this questioning of the text. By the nature of the enterprise, "no limitation is set on any subject; problems run into one another; they become intricate and interwoven".⁹⁰ However, Wolfson himself goes on to say that the interaction is enlightening and that the method he has just described is "scientific"; moreover, that it must be followed, as he follows it, in studying the thinkers like Crescas who used it.⁹¹

Thus, Wolfson does not see any of the uncritical nature of these principles which I have been trying to draw attention to. It has been part

of the purpose of the Appendix to present another historical method which appears objective and critically skeptical of prima facie judgements of historical works but which defeats its own purposes because it is too comprehensive and ungoverned. Of course, the scholars of the Renaissance eclectic traditions nowadays, not to mention Burtt and Koyré, use standards far more clearcut and ascetic in the attempts to reconstruct the context of ideas of any particular thinker. Yet, the similarity is enlightening; the assumption of clarity which can operate in their approach is similar to the uncritical directives of the scholastics: to distort not capture the historical reality of thought or text.

Appendix B: On Overtly and Covertly Confused Metaphysics

Once abandon the identification of confusion with incoherent nonsense and many varieties of confused metaphysical views may be distinguished. Some may be confused due to vagueness in terms, others due to irregular variations in meaning, unclear reference, inconsistent application, or levels of interpretation interwoven with no theory of the relation. One might think that in the present discussion a distinction between overtly and covertly confused metaphysics would be particularly valuable. Certainly, there is a difference between blatantly confused views such as the wildest astrological or mystical interpretations of the world--views which often do not even approach coherence or consistency but positively feature paradoxes, contradictions and strange allusions--and those cosmological or otherwise metaphysical views which are attempts, at any rate, to capture a coherent world picture and which seem amenable to clear, straightforward interpretation. Closer examination or study reveals confusions and difficulties but they are not overtly confused. One can argue that theories such as the Kaballistic, light mystic metaphysics of the Renaissance may fall into the first category; but, theories such as Aristotle's world view, Harvey's metaphysics of the body, and perhaps also Kepler's vision, are better understood in the second. They are not overtly but covertly confused; on some level, at least, they appear clear. Such a distinction may allow a more limited and reasonable claim for the usefulness of confused metaphysical views in science; that is, overtly confused views can be excluded; whereas views which are superficially amenable to clear interpretation or which appeared to their proponents to be

clear but were actually confused can be identified as a variety which is often helpful in scientific efforts.

Unfortunately, such distinctions are of extremely limited value in this discussion. In fact, their tendency is antagonistic to the major lessons to be gained here; namely, that the metaphysical views important in several historical episodes were--whether one calls them overtly or covertly confused--definitely confused; they were, that is, confused on the (several) criteria used by the commentators who report them as clear--including at times even those of the scientists who made use of them. Another such lesson is that confused views do not constitute the threat to rationality previously feared; therefore, we have less reason to hunt for distinctions which will outlaw one or the other sort of confusion. These arguments are in no way softened--but rather obscured--by the distinction between overtly and covertly confused metaphysics.

Moreover, traditional proponents of clarity will be found in every position (except one), associated with that distinction: namely, that both overtly and covertly confused metaphysics are inimical to progress and should be avoided, that overtly confused metaphysics is not useful and should be avoided, that covertly confused metaphysics is not useful and should be avoided. The point is that even the most liberal of these positions is untenable on the view proposed here; namely, that both overtly and covertly confused metaphysics have sometimes been helpful in science. It would not be difficult to argue that any understanding of "overt confusion" which did not include Kepler's and Copernicus'--perhaps even Harvey's--metaphysics would not include most of the wild positions usually considered confused--e.g., the Kaballistic body worship of the late

Renaissance. But this is the position with respect to the distinction which makes it irrelevant. Thus, the idea of science and of rationality protected by the assumption of clarity, cannot be saved by a simple and innocuous shift to the view that all scientists did (after all) avoid overtly confused views.

Interestingly, most of the metaphysical views discussed in the dissertation are, in the literature, defended not as covertly confused but as covertly clear. Commentators point out superficial confusions which they claim are merely apparent; clarity is hidden but it is there really. The important implication of both distinctions is the same; namely, on some level the view can be called clear and it is on this level that the view ^{was} believed and used. The response is the same; that attempts to elicit such a level distort the views and attempts to argue such a function fail. One can interpret Harvey's views to be clear and consistent or to be confused only in a hidden way, but then they are not the views he can be seen to have advocated; and, if one insists that it was the clear interpretation not the covertly confused interpretation which was useful, one must ignore some of Harvey's work, debates, difficulties and accomplishments.

Let me turn, however, directly to the question, which kind of confused views are positively related to scientific productivity and which kind are indifferent to or have negative effects on such productivity? As stated, this question is not part of the investigation undertaken here. That is, I do not attempt to describe any demarcation criteria for confused views; nor any defining characteristics for the kind that have helped scientists. The major force of the arguments here is critical; they are directed to the refutation of the assumption

of clarity not toward the development of a further positive theory on the usefulness and character of confused views. The argument that some views which do not meet even the most liberal of accepted standards of clarity nevertheless have played important roles in productive science is the most direct declaration on confusion that is necessary here. It is not necessary to describe features of these views or to which have general validity or to propose a general theory of what distinguishes them from views which will be both confused and not useful. I doubt, in fact, that any such general theory exists. If asked which kind of confusion I would expect to be indifferent to scientific productivity or harmful in science, I would answer that the arguments here suggest there is no general criterion and that all conclusions on this point traditionally held are false because they are too restrictive. Inconsistent, vague, confused, irrefutable theories have all had a role in some productive scientific research despite the assumptions and theories to the contrary.

However, I do describe 1) how some scientists have used seemingly confused views and the use of them has appeared helpful; and, most importantly, 2) how it is possible to claim that this use of confused metaphysics--confused on accepted standards--does not destroy the rationality of science. To take the second claim first, I argue that a confused metaphysics can be part of a rational science when it keeps important questions on the agenda for debate, focuses attention on specific scientific areas, or gives a unified picture to disparate scientific theories. The explanation of how it is possible to claim that a confused metaphysics is helpful in a rational science can be understood to incorporate defining characteristics of the "kind" of metaphysics which will be

helpful. But, this would be an unadvisable misreading in my views; there may well be other ways in which confused views can be helpful in science.

Likewise, I describe in some detail in the next chapter how Harvey used his metaphysics in a number of ways which suggest strongly it was confused and a part of his research and a helpful part of it. This can be considered an account of how a confused metaphysics will be helpful in science; indeed, I claim it to be such an account. But, it may be rejected as inadequate until it is joined to the above mentioned explanation that the science Harvey is doing is still a rational learning activity, not prescience or psudeo-science for which Harvey has had unearned credit. One can always describe the most intimate contact between a thinker's metaphysics and his work only to be told that this intimate contact does not (cannot) imply that the metaphysics was beneficially influential; and commentators have preferred this alternative--as awkward as it often is--whenever they have not been able to describe the metaphysics as clear. [The pattern is described further in Section 4.2 below.]

One suspects that the motivation behind efforts to find usable distinctions which would exclude some flagrantly confused views is the wish to eliminate from the scope of the claim the idea that important and clever thinkers embrace openly incoherent positions. It does seem ridiculous to put forward Newton, Harvey or even Kepler as obliviously babbling incoherencies and asserting inconsistencies; we know them to be not only capable of lucid, clear, systematic thought but to excel in it. The idea that they believed confused and inconsistent views without demur appears ridiculous in the face of that fact; perhaps, then, the views they embraced only seemed confused to us or were

confused on a deeper level.

To this argument, I will say that one need not assert that ordinarily coherent and intelligent people would just as likely babble incoherencies and obviously assert contradictions in order to claim that they were confused in important ways. The conflation of all confused views into the morass of inane babblings is just the association that needs to be broken. Thus, it is not such a radical declaration to claim that regularly rigorous thinkers were actually confused. In addition, one can argue that there is an unjustified belief that it takes a weird and ominous blindness to embrace a confused view; that an ordinary reason must be seriously crippled to contenance it. This is, one can argue, simply a false prejudice clearly traceable to the traditional belief that clarity and consistency are easy and natural. Whereas, in fact, the fact is that nearly all thinkers--no matter how rigorous--are confused and inconsistent in some important ways because it is extremely difficult to *avoid this*. Yet, embracing a confused view can be quite compatible with overall critical and rational thinking.

In sum, rather than a positive theory of confusion and its usefulness, the emphasis here is on explaining how a confused view can be considered to have influenced science without abandoning rationality. The evidence from historical sources that confused metaphysics indeed has benefited science becomes creditable when this is done. One can even say, then, that the conclusion that confused metaphysics has had benign influence on some scientific developments becomes the best explanation of the evidence. But a concern for a demarcation or positive criterion of which like of confused metaphysics can do

this is not particularly valuable in my view; the point of the discussion is that one can be less careful, more liberal, in such matters.

NOTES

N.B. All references appear fully once in each chapter; a list of abbreviations preceeds the bibliography.

1. Neither Burtt nor Koyré say what "conform" means here; Agassi discusses the point explicitly in his "Scientific Problems and Their Roots in Metaphysics", in The Critical Approach, ed. by Mario Bunge (New York: MacMillan, The Free Press, 1964); the discussion is taken up in Section 4.2b and 4.5 below.

I cannot quote Burtt or Koyré defending the assumption of clarity; nor can I quote them stating their views on the relation of science and metaphysics in so many words. The approximations they give to a statement of their positions are, in fact, inadequate to their merits as I point out in Chapter One when I quote Koyré calling himself a language analyst. I reconstruct their position and then criticize it for being inadequate. This is often taken to be unfair. But, I do defend my approach in the dissertation on the following lines: that if I had reconstructed their position in a way that I would then be able to declare it without flaw or *without* a weakness--I would not be taken to be out of bounds. If such a reconstruction is admissible, a reconstruction which can be criticized must also be admissible.

- 2 Burtt, Metaphysical Foundations of Modern Science, (London: Routledge and Kegan Paul, 1932).
- 3 Ibid., p. 20.
- 4 Ibid., p. 23.
- 5 Ibid., pp. 3-4.
- 6 See the text below and Ibid., pp. 59-60. Burtt's criticism of Cassirer offers another view of the new world view in his opinion; he criticizes Cassirer's work on the epistemological history of modern thought as deficient because it is not broad enough. Many different aspects of the metaphysical view of the modern world must be criticized together if one hopes to attain the proper distance from them; Ibid., p. 16, also p. 22 cited below in the text.
- 7 Ibid., p. 20.
- 8 Ibid., p. 21.

- 9 Ibid., p. 22.
- 10 Ibid., p. 3.
- 11 Ibid., p. 11.
- 12 Ibid., pp. 9-11, for Russell on Mephistopheles; for the inescapability of the modern view, see particularly, p. 10.
- 13 Ibid., p. 60, for Kepler's contribution to a higher standard: "valid mathematical hypotheses must be exactly verifiable in the observed world"; p. 70, for Galileo against the Pythagoreans; p. 74, for a review of the idea that all confusion in ideas comes from the senses and can be eliminated from science; p. 93, for passages against essentialism in the name of the new metaphysics.
- 14 Ibid., pp. 45-46, for Copernicus' mysticism; pp. 46-52, 58-59, for Kepler's wilder views--often in these pages called "superstitious".
- 15 To state this another way: Burtt often says that the metaphysics colored the concepts of science and forced out certain facts, while determining others would be stated in a certain fashion. Yet in the passage which follows in the text, he seems to hold that the "remarkable developments in the sciences of mathematics and astronomy" are independent of the metaphysics Kepler was forging. How can this be? A related question is that on the significance of the success of science for Kepler's effort; is it just chance that Kepler's "venture in revising...the traditional ideas of causality, hypothesis, reality, and knowledge" proved "pragmatically successful". In fact, accident or serendipity is not a rational explanation for this success--although an explanation is indeed called for. Why, since unjustified, was Kepler's vision so successful? Agassi, in "Science in Flux" poses explicitly the interesting question, "why are so many false theories so spectacularly confirmed?", as a serious question. "SF" in Science in Flux in (Boston Studies in the Philosophy of Science, D. Reidel, Boston, 1976), pp.
- 16 Burtt, p. 59.
- 17 Ibid., p. 60.
- 18 Ibid, p. 262, 264ff.
- 19 Ibid., p. 88, for Galileo; pp. 20-21, 224-227 for delicious discussion of the inescapability of metaphysics and the dangers of a positivistic belief in the possibility.

- 20 *Ibid.*, p. 21, 224ff.
- 21 *Ibid.*, p. 303.
- 22 Alexandre Koyré, Newtonian Studies, (Chicago: University of Chicago Press Phoenix Books, 1965) p. 5. See also, From the Closed World to the Infinite Universe, (Baltimore: Johns Hopkins University Press, 1968; orig. publ. 1957), pp. viii-ix, 3, 277 n. 3, for a similar series of comments on the continuity of the development of the views which make up the scientific revolution.
- 23 NS, p. 6.
- 24 *Ibid.*, p. 7.
- 25 *Ibid.*, p. 8.
- 26 *Ibid.*, p. 26.
- 27 See Frances Yates' study, Giordano Bruno and the Hermetic Tradition, (London: Routledge and Kegan Paul, 1964) pp. , for a discussion of Kepler's orientation toward more strange metaphysical traditions. Also, Gerald Holton's study discussed in Section 2.3d below, n. 68.
- 28 *Ibid.*, p. 6. Also, Metaphysics and Measurement, Essays in the Scientific Revolution, (London: Chapman and Hall, 1968), p. 19 cited in Ch. I above p.
- 29 The Astronomical Revolution, (Ithaca, New York: Cornell University Press, 1973; orig. publ. 1961), p. 9.
- 30 One must still say only "in part" does the metaphysics determine which facts are significant; for, most metaphysical outlines are incomplete; moreover, although they may determine which facts are significant, they do not dictate only confirmations but, as noted, refutations as well. In this respect, one must remember that Kepler surrendered a beloved metaphysical view--concerning circular motion and regular motion--because it couldn't be squared with the facts; that it had to be and could be squared with the facts if true was a metaphysical belief. It is this second metaphysical belief which Duhem saw as unproductive in science as discussed in Ch. I, Section 1.3c. For Kepler's dialectical relation to metaphysics see Holton's study discussed below and Koyré.

I also say "in part" the metaphysics determines which facts are significant because Koyré does not spell out details here; he does not say how or what extent the metaphysics makes facts significant--not even in illustra-

tion. For an articulated theory and discussion, one must turn to the philosophers, e.g., Agassi, "SPRM".

- 31 NS, pp. 11.12. AR, p. 10, for a similar description of the developments which makes them seem almost inevitable.
- 32 For the idea that early scientists had more to do than simply note refutations of "scientific" theory and replace the views refuted, see: Koyre, MM, p. 19. See also the vivid and related description on p. 3, and references to Meyerson in the notes on pp. 18 and 19 of the same volume; and the excellent, forceful passage cited in n. 60, Ch. I above from MM, p. 90.
- 33 NS, p. 14.
- 34 *Ibid.*, p. 17. I think it is very clear that regularly any view which is taken as powerful or grand is not only considered more or less true but more or less clear--as Mach reports Newton's theory of attraction was taken. This is one of my major criticisms of the work of historians in the Burtt-Koyre school--namely, that they do not come explicitly against this conflation. We do have the tendency not to call such views errors or confusions; we tend to restrain for reasons independent of the truth or clarity of the views in questions: the reasons depend instead on our views or rationality which we mistakenly connect with truth and clarity too strictly. This "nicety of expression"--concerning the status of grand and powerful theories--hides exactly the issue at hand.
- 35 *Ibid.*, p. 20
- 36 *Ibid.*, p. 20.
- 37 Études Galiléennes, (Paris: Haerman et Cie., 1935-37), p. 84 (7).
- 38 NS, p. 19; MM, pp. 3, 90; for difficulties such as creating a new mathematics which would capture only essential qualities, translating substance into mass, etc.
- 39 NS, pp. 10-12, for the strength of neo-platonism compared with atomism in these early days.
- 40 NS, p. 19; MM, pp. 3, 90.

- 41 In the article discussed in Section 2.3d below, Holton hints that Kepler's metaphysics in its very confusion was helpful; but he doesn't quite call the quality "confusion"; see n. 75 below and citation intent. Agassi discusses, with several others, the view that our idea of comprehension must be expanded to include articulate views which do not meet former criteria; see Appendix B to Chapter IV. For the purposes of the present argument, I do not have to claim that views are helpful because of their confusion; "notwithstanding" their confusion will be more accurate.
- 42 J. E. McGuire and P. Rattansi, "Newton and the Pipes of Pan", Notes and Records of the Royal Society of London, 1966, pp. 108-144.
- 43 Ibid., p. 108.
- 44 Ibid., p. 126.
- 45 Ibid., p. 138.
- 46 Ibid., pp. 124-125.
- 47 Ibid., p. 125.
- 48 Ibid., p. 125.
- 49 Ibid., p. 138.
- 50 Ibid., p. 136.
- 51 Ibid., p. 137.
- 52 Frances Yates, Giordano Bruno and the Hermetic Tradition. (London: Routledge and Kegan Paul, 1964); also her article, "The Hermetic Tradition in Renaissance Science", in Art, Science and History in the Renaissance, ed. by Charles Singleton, (Baltimore: John Hopkins University Press, 1967), pp. 255-274.
- 53 Kristeller has many works on Marcilio Ficino and Giovanni Pico della Mirandola which express his view of them as neo-platonist philosophers significantly outside the literary humanist tradition. See, for example his Renaissance Thought, (New York: Harper Torchbooks, 1961); the sequel, Part II: and the comprehensive review: "Giovanni Pico della Mirandola and His Sources", [Opera E l Pensiero di Giovanni Pico della Mirandola nella Storia dell'Umanesimo, (Florence, 1965), pp. 35-84.
- 54 Yates, GBHT, p. x.

- 55 D. P. Walker, Spiritual and Demonic Magic From Ficino to Campanella, (Notre Dame University Press, 1974).
- 56 The works of Gershom Scholem, Francois Secret, A. J. Festugiere are often cited; Wayne Schumaker and Allan Debus have also contributed works in this area; Max Jammer has some interesting comments in his surveys of the development of science.
- 57 In his article, "Was Copernicus a Hermeticist", in Minnesota Studies in the Philosophy of Science, Vol. 5, ed. by Rober Steurer, (Minneapolis: University of Minnesota Press, 1970), pp. 163-171, Edward Rosen attacks Yates' attributions strongly arguing that she bases them on offhand comments which on examination can be seen not to indicate any deep familiarity with the Hermetic literature. He does not discuss the broader aspects of her attempt to reconstruct the world view of such researchers however; and so, how much his exposure of her weak conjectures on Copernicus' acquaintance with the Hermetic sources affects that broader view, I cannot tell--not as much as Rosen would think, I surmise.
- 58 Yates, "HTRS", p. 269, for just some examples of suggestions that more study will clear away any intellectual inconsistencies. Also, after many rhetorical questions to the effect that the Hermetic background may explain several of Bacon's seemingly misconceived and inconsistent positions, Yates recommends deeper study of the Hermeticists involved. The approach she outlines is particularly uncritical, see "HTRS", pp. 270, 271, where she speaks of "detecting intimations" both ways.
- 59 I. B. Cohen, "Commemoration", Isis, Vol. 57, part 2, No. 188, Summer 1966.
60. Ibid., p. 161.
61. Ibid., p. 161.
- 62 Ibid., p. 161.
- 63 Ibid., p. 161
- 64 See Appendix A of this chapter: On text study, by H. A. Wolfson.
- 65 Cohen, p. 161.
- 66 Ibid., p. 161.
- 67 Ibid., p. 161.

- 68 Holton, Gerald, "Johannes Kepler's Universe: Its Physics and Metaphysics", in Thematic Origins of Scientific Thought, (Cambridge, Harvard University Press, 1973). [Originally published: American Journal of Physics, Volume XXIV, No. 5, May, 1956, pp. 340-351.]
- 69 Ibid., p. 69.
- 70 Ibid., pp. 85-86.
- 71 Ibid., p. 68.
- 72 Ibid., p. 70.
- 73 Ibid., p. 70.
- 74 Ibid., pp. 69-71.
- 75 Ibid., pp. 70-71.
- 76 Ibid., p. 70.
- 77 Ibid., pp. 86-87.
- 78 Ibid., p. 69.
- 79 One can note the response here that historians' judgements are all that can be used to decide when one stops assuming that a position really is clear. The idea that judgement is all that can be used to decide once one drops the assumption of clarity is a common one although not often articulated; historians like to be objective--at least more than this implies. If the question is, is any more explicitness or definite policy in such cases useful, I would argue definitely yes--for the usual reasons; namely, when intuitions are made explicit they can be examined and criticized. In the case at hand on the assumption of clarity, the point is that the historians' judgements are strongly colored by theories of rationality which push them always toward the one solution: do not consider confusion a serious possibility. When the historian's intuitions are given such alternatives--assume clarity or assume irrationality--his intuition cannot treat the choice fairly. And this is what is happening here.
- 80 See Appendix B to this chapter: "On Overtly and Covertly Confused Metaphysics."
- 81 Exactly the same argument applies to the theory that all error is due to prejudice--viz. that it is just too uninformative and ad hoc to be appealed to in an explanation. Agassi's discussion of this point in his, Towards an Historiography of Science, Beiheft 2, History and Theory

(The Hague: Mouton & Co., 1963), pp. 44-45, provides the model for the one in the text here. Agassi describes the various attempts to explain why Priestly stuck to phlogistonism when the alternative belief in oxygen, now considered correct, had been accepted by many thinkers. He reports Toulmin's concern that calling Priestly prejudiced makes his previous work inexplicable; Priestly, according to Agassi, felt the same about his predecessors: if they were wrong to believe in phlogiston, they must have been prejudiced; if they were prejudiced, their other work and achievements would be inexplicable. That conclusion was unsatisfactory because it was not informative, quite apart from the bad reflections on the character of men like Stahl which it included. But, because of this argument, Priestly concluded that the phlogiston theory must be true; Agassi concludes that the doctrine of prejudice--that all error is the result of prejudice--is false.

- 82 THS, p. 54
- 83 *Ibid.*, pp. 49, 53, 54.
- 84 *Ibid.*, p. 54.
- 85 H. A. Wolfson, Hasdi Crescas' Critique of Aristotle, (Philadelphia; Jewish Publication Society Press, 1929).
- 86 *Ibid.*, p. 24.
- 87 *Ibid.*, p. 25.
- 88 *Ibid.*, p. 26.
- 89 *Ibid.*, p. 26.
- 90 *Ibid.*, p. 27.
- 91 *Ibid.*, p. 27.

CHAPTER III. THE RELATION BETWEEN SCIENCE AND METAPHYSICS IN THE
WORK OF WILLIAM HARVEY

Chapter III. The Relation Between Science and Metaphysics in the Work of William Harvey

In this chapter, I review the literature on the work of the early scientist William Harvey. The review is the attempt to illustrate in detail how the assumption of clarity *lead to* distortions and misleading views of the scientific process--particularly by failing to capture the richness of the relation between metaphysics and science.

As to the actual relationship--my view--between Harvey's science and his metaphysics, here it is claimed that Harvey's metaphysics was a confused mixture of Aristotelian and Kaballistic opinions which functioned in his scientific thinking in a number of ways. It suggested theories, became part of the criterion for the judgement of adequate explanation, was overthrown in part. To summarize, some confused metaphysical ideas aided Harvey in his research and were clarified and refined through it. The confused views--for example, the microcosm-macrocosm theory of the body--led to some confused stands in his science. One can take Harvey's inconsistent and ambivalent stand on the importance of final cause as an example. But, this confused stand is part of his science--part of his contribution--not a regrettable sideline. (That such a role can be not merely described but described as rational is discussed in Chapter IV.)

Little of this rich interaction is noticed or acknowledged or appreciated by commentators, who regularly either malign, ignore, or apologize for Harvey's metaphysics. It is also unappreciated by writers who streamline the metaphysics before they present it in connection with Harvey's science; a misleadingly

smooth view of the science, the metaphysics, and the scientific process results.

The work of William Harvey (1578-1657) is discussed in many books and articles; his work is part of the Scientific Revolution of the seventeenth century and is considered classic in the annals of science. The theory of the circulation of the blood was a momentous discovery and its presentation by Harvey in De Motu Cordis et Sanguinis (1628) is commonly pronounced a paradigmatic example of a scientific work. Thus, Harvey's case is in the mainstream in the history of science and as a counterexample to the assumption of clarity adopted by Burtt and Koyré and other commentators, it will be illustrative and appropriate.

I want to emphasize my appreciation of some of the historical work done on Harvey--especially that by Walter Pagel--even though I use it in this dissertation only

as an extended illustration, as in fact one case that shows a more complex situation in the relations between metaphysics and science than has hitherto been fully recognized. Philosophers--if I am at all in touch with their representative feelings--often have ambivalent attitudes toward historical cases. Lessons from history often seem harsh and ~~unsensitized~~--insensitive to the possibility of reinterpretation. Every case can have a number of interpretations; historians and those who advocate the study of history often seem to think that philosophers speculate and argue only because they are too lazy or ill-equipped to examine what has happened.

Yet, there can be the opposite feeling about historical work; the real historian--who loves science and philosophy--can be a cooperative partner in philosophic investigation although he does not participate himself. He can be just such a partner because he wants very much to transmit the full richness of an event to his readers--to give it a substance that makes it an experience which is not underestimated and which therefore breaks through our parochial understanding. That historian says eagerly that there is a depth and richness to the figure one is examining--or to his life and achievement--which one has not seen and appreciated yet. To communicate a hitherto unsuspected subtlety in thought--e.g., that Harvey has a new and informative view of vital forces--is the invaluable service of the best historians.

This chapter will include a review of the Harvey literature with special attention to discussions or lack of them on the relation between science and metaphysics in his work; a discussion of the metaphysical background of Harvey's science in order to show its troublesome nature as eclectic

and confused; and finally, a critical review of the roles of metaphysical views in his work.

Like the history of science generally, Harvey scholarship has developed from less to more penetrating in the last half century. Commentators used to praise him as a model modern scientist who dropped superstitious beliefs of Aristotle and observed and experimented. He was a saint in character and the only trouble he had in his work involved obscurantists who created external difficulties. This picture of Harvey á la the traditional inductivist view of science fits him more poorly than it does many, since Harvey was a marked conservative in many way--in more ways, for example, than Galileo or Kepler. The breakdown of less historically-oriented views of Harvey has led to improvements of various types; it will be necessary to review these steps so as to see how much of his scientific work has been accounted for and to judge against that background the gaps which are left.

For the purposes of reviewing the status of Harvey literature, I have divided the literature into two major categories; traditional and historical . Traditional commentators for the most part uphold an idea that Harvey had a method of discovery involving unbiased, unspeculative examination of facts and that whatever he discovered with it has a place in the modern textbook. Many writers who survey the history approach this position although they may be more sensitive to historical influences than they are about Harvey's approach - the latter is misleadingly modern. The major treatments of Harvey himself are those by Gwenneth Witteridge, Louis Chauvois, Kenneth Franklin and Geoffrey Keynes. Some of these work purport to treat Harvey's

science, some to concentrate on his life; but both types fail to see his work in a critical or historical framework--indeed in any broad intellectual framework.

It should perhaps be noted that there are several specifiable places in which a treatment of Harvey will break down if the author of the study is not sensitive to the broad metaphysical issues surrounding his work or unwilling to consider such issues in some relation to Harvey's scientific activity. A list of these points appears at the beginning of Section 3.1.; the points will be discussed one by one in the several sections.

The major gaps left in traditional treatments of Harvey--gaps and distortions--have been uncovered in the main by the later, but more historically-oriented commentators--most particularly by Kenneth Keele and Walter Pagel. These commentators stand in the same relation to Harvey that Burtt and Koyré stand to the astronomers of the Scientific Revolution; they describe with vividness the metaphysical background of the thinkers with whom they deal. In this achievement, they go beyond the views of previous commentators on the subject. But, they often do not state or even observe the theoretical, philosophical importance of their own historical discoveries. Keele and Pagel give their readers enough material to see the influence of metaphysical views--even of confused metaphysical views--on Harvey; they avoid many of the unfortunate apologetics of previous studies. Neither of them appreciates the change in a view of science their works indicate; namely, that science as Harvey did it is not simply attentive care for factual empirical accuracy. This detracts from their force; omissions and

distortions prompted by the inappropriate ideal of science result in an inaccurately smooth picture of Harvey's science in their works as well.

Section 3.1. Survey of Harvey Scholarship--more naive traditional treatments.

In trying to come to an appreciation of the metaphysical components of the scientific work of someone like Harvey, it is necessary to show the inadequacies of the most common traditional view of science which has gotten colored treatments of his work. For as noted generally in other places, a view of science as inductive, as certain, as possessed of a straightforward and reliable method even if not leading to certainty, will ipso facto exclude a consideration of metaphysical views. The treatments of Harvey which incorporate such views of science are often awkward and apologetic when the aspects of his work which do not fit the picture are in question.

To summarize these embarrassing aspects: Harvey's second work, De Generatione--his book on the reproduction of animals--is not as successful as his De Motu and this causes embarrassment to many commentators; the De Motu itself is not inductively organized and includes discussions of metaphysical issues as part of its argument. Harvey consistently expresses appreciation of both Aristotle and Galen metaphysically. The untempered criticism of Harvey's opponents--concerning the invisible pores of the heart and other inadequacies of the rival established theory of Galen--are often made in such a way that they backfire on Harvey's theory: Harvey could not demonstrate the existence of capillaries (any continuous connection between arteries and veins); he could not explain why the blood traveled in a circle particularly because arguments by which he defended his theory made nutrition almost incomprehensible; the initiation of the heart beat was markedly mysterious; the change in color when blood went through the lungs

was not explained--in fact, Harvey denied that it had significance.

All of the above genuine scientific inadequacies in Harvey's work cause great embarrassment and occasion evasive and apologetic maneuvers in many of the traditionally-oriented discussions which will be reviewed below. The difficulties can only be allayed, in many cases, by a view of science broad enough to include a cultivation of metaphysics--including confused metaphysics--and a place for speculation, criticism, and debate; these aspects of scientific activity are ignored or disdained in many treatments of Harvey's achievement.

(a) Commentators on Harvey who are committed to an idea of science as a radical departure from unfounded speculation including a method which establishes reliable truths, can only do credit to Harvey's actual research in moments of "carelessness and inattention"--as Hume would say. Such a position forces commentators to stress the factual side of research even when they do not put forward a idea of inductive method with no preformed ideas as Harvey's policy. The view of science many of these traditional commentators hold is anti-speculative, even positively anti-theoretical, and thus of course it is anti-metaphysical in the extreme. Some of the studies of Harvey which must be put into this category are very recent, and respected as authoritative in their realm; the traditional, quite restricted view of science and of Harvey is not at all a dead relic of the past.

For example, Gwenneth Witteridge's study and Geoffrey Keynes biography are two of the most recent works on Harvey; they are both considered major contributions and have been favorably reviewed. Yet both

ignore or disdain any connection between Harvey's science and metaphysics. There are important historians of science such as Charles Gillispie and Richard Garrison who present a restricted and inaccurate view of Harvey's work in spite of the fact that they can not be called generally unsophisticated; the possible connection between metaphysics and science suffers particularly from lack of discussion even when conjectural theories and mistakes are allowed as part of science as a matter of course.

The weaknesses of Gwenneth Witteridge's treatment of Harvey specifically concern an undeveloped philosophy of inductive method she seems to hold; she seems to assume that unless Harvey conducted his experiments before he hypothesized that the blood "might travel as it were in a circle" she could not call him a scientist. The suggestion that Harvey held the idea of the circulation of the blood as an hypothesis before he did his experiments is a libel to his scientific character in her view. The sections of De Motu in which Harvey reports his motivations and hunches, the very metaphysical eighth chapter in which he glorifies circular motion, and his final arguments in defense of his theory on the basis of final cause and its compatibility with a proper metaphysical framework, must therefore cause great difficulty for Witteridge. She tries to dismiss the sections by saying they could "only have been written once Harvey's new hypothesis was not only postulated but proved experimentally".¹ Other commentators follow Witteridge in seeing these chapters of Harvey's famous book as separate, sometimes they see a different style in these passages and conjecture that they were perhaps added during Harvey's old age when he was completing

the "unsuccessful" and equally "metaphysical" later work, De Generatione. However, other later commentators have concluded that on the contrary, the ideas at the basis of De Generatione were formed at just the same time and as part of the same research which went into the theory of circulation even though the book incorporating it was published thirty years later.²

The radicalism and belief in specifiable method of discovery or proof which established his work in some sense makes many commentators distort not only Harvey's strong Aristotelian metaphysical leanings and his debt to Galen, but also his interactions with critics. There were, however, several important critical exchanges between Harvey and other interested thinkers on the body--Descartes, Riolo, Hoffman, Marchi. Witteridge does not openly consider the possibility that there is any intellectual, scientific value in these exchanges; but she is nevertheless forthcoming in the details of her reports. She describes as potent Descartes' important objection--repeated by other critics also--that Harvey's theory made the difference between venous and arterial blood inexplicable; she sees Caspar Hofmann's criticism of Harvey's failure to explain the purpose of circulation as at least partially understandable given Harvey's earlier commitments. These episodes will be described below. Many historians of science and medicine who survey the history and many specialist commentators on Harvey do not mention these criticisms or mention them only to denigrate their importance and/or intellectual value. Witteridge, a page after she has noticed how difficult it is to observe a pulsating heart, still criticizes Descartes' powerful arguments by saying he was "theorizing and not looking".³

To go on for a moment to a related point, the fact that Harvey was a vitalist and that his vitalism is very apparent in the background of much of his research and ideas is often completely ignored by surveying historians; they often in fact suggest strongly that Harvey was a mechanist--particularly when they discuss his theory of heart motion. Examples are Singer and Garrison who do not mention Harvey's vitalism at all; Butterfield speaks of the modernity which characterizes Harvey's work--a modernity due to its mechanistic arguments. He adds that Harvey had some "unsatisfactory speculative views common in his time such as a belief in vital spirits".⁴

The treatment of Harvey's vitalism is characteristic of the way commentators shy away from an acknowledgement that a possibly unacceptable metaphysical view--from the point of view of clarity and distinctness of claims--played a role in science; this is particularly the case with metaphysics such as vitalism which did not become established in science; writers do not find attributions of a mechanistic orientation touchy enough to avoid even though it is as metaphysical as vitalism. Mechanism, though, has acquired a reputation as a clear, direct, unmysterious position without occult potential; it has been accepted as "scientific" metaphysics. Harvey however was a vitalist not a mechanist; and, his vitalistic beliefs--although not clear and spelled out--directed as much of his research as mechanism led others.

For example, Harvey's vitalism led him to react against the materialist associations of Galen's view of the body; and, it lead him to advocate a closed system for the blood on the basis that the blood, being life, should

be preserved. As an example of the lack of direct clearness in his position, one can note that Harvey on the one hand thought the blood was life, the source of sensation and feeling; but also thought the rest of the body was "vital" even though it did not need to be "preserved" in the same way that the blood needed to be preserved. Also, there were, well known, other ways of dying besides loss of blood and Harvey's vitalistic credits for the blood included no clear orientation toward such cases. These are not remarkable or unique gaps in the direct content of a metaphysics; vitalistic positions--as many metaphysical positions generally--regularly include similar ones. But, those in Harvey's vitalism are not examined when the existence of his vitalism itself is ignored.

To explain how much this view of Harvey's vitalism as incidental or nonexistent is a distortion, it will be helpful to explore the most quoted passage from Harvey's work; it indicates not only important metaphysical^a associations but the weakness in his position exposed by the best criticism; yet, it is cited regularly as a-metaphysical and unqualifiedly modern:

Since calculations and visual demonstrations [other translations read "reasoning" and "ocular demonstrations"] have confirmed all my suppositions, to wit, that the blood is passed through...

Then follows a statement of the theory of circulation which ends as follows:

...in such amount and with so large a flux and reflux...that it cannot be supplied from the ingesta, and is also in much greater bulk than would suffice for nutrition.

Readers should note here the lack of emphasis on nutritional functions; it is

strengthened by what follows: a strong reference to the metaphysics which connects "life" to perpetual motion in a closed circle. This connection is rarely remarked upon.

I am obliged to conclude that in animals the blood is driven around in a circuit with an unceasing, circular sort of movement, that this is an activity or function of the heart which it carries out by virtue of its pulsation, and that in sum it constitutes the sole reason for that heart's pulsatile movement.⁵

The importance of the stress on ceaseless circular motion is this: the fact that the blood--which Harvey thought was the primary living substance--traveled in a closed circle and therefore was perpetually preserved as life should be is a metaphysical commitment. That he declares this perpetual movement to be the "sole reason" or end of the heart's pulse makes it even harder to incorporate an adequate theory of nutrition into this view.

The stress exemplifies a central confusion in his views; that between fulfilling a purpose in the sense of physiological function and fulfilling a purpose in the sense of contributing to a metaphysically appropriate result; Harvey's statement has behind it a deference to the view that an organ may do what it does solely to establish the appropriate hierarchy in the body.

His critics who saw some obscurity in the disassociated heart activity and pulse (Descartes), the nutritive activity of the body abandoned (Riolan, Hofmann and others), and nature riding on a useless merry-go-round for the sake of Harvey's metaphysics (Hofmann), had some reason to complain.

Unless one sees the extent to which metaphysical views were part of Harvey's science, the debates on these issues can be scarcely understood as sensible

exchanges; neglecting such concerns makes this famous passage above much less informative than it truly is.

As noted, the idea that the blood is life and should--~~metaphysically~~--be preserved in *perpetuity in circular motion* points up Harvey's vitalism strongly. Ignoring his vitalism as such, then, also produces distortion of the rational progress and meaning of his scientific work. For example, Charles Gillispie has concentrated special attention on Harvey's work among the episodes in the history of science which he covers in his respected, Edge of Objectivity; yet his allusions to the issues surrounding these vitalistic references are apologetic or they miss the point. Gillispie concentrates nearly all his discussion on Harvey's (supposed) mechanism; this mechanism was so significantly different from a standard mechanist view, however, that Gillispie's rendition is troubled. Harvey's heart and blood worship is expressed in the lyrical enthusiasm with which he points out the metaphysical propriety of this scientific theories and in his willingness to leave the heart beat and nutrition unexplained. Gillispie dismisses the vitalistic enthusiasm as mere allusions, as "no more than the reverence which any honest biologist who has truly studied the classics must feel...". He goes on to add: "...no vital spirit, no principle of nourishment intrudes into the analysis".⁷

This last claim however, should be seen now to be false. Gillispie misses the point that Harvey's vitalistic attitudes toward the heart and blood were in fact attitudes related to a contentful metaphysics not just reverential expressions. Moreover, his views in this area did indeed intrude on the analysis, but in the opposite way to that Gillispie suggests; namely,

they created a mystery about nourishment that needed to be explained.

Harvey's reticence on the point of nourishment is not a sign that metaphysics of a vitalist cast did not intrude into his science, but, quite the contrary, that it was an active agent.

Gillispie, ~~and many~~ other commentators who take similar attitudes approach a good point in trying to keep a distance from Harvey's vitalism, but they do so in an inefficient and misleading way. They want to make clear the true and important point that Harvey's notions of vital force and metaphysical propriety were not as intellectual unsatisfactory as some previous appeals of the sort. In the case above, for example, Harvey does indeed not talk about "vital spirits" since he does not like "spirits". But he does speak of the blood itself as divine and everlasting and "vital"; thus, Gillispie's attempt to recruit this aversion to spirits into the cause for mechanism is a hopeless distortion.

In general, the evasions and neglect of Harvey's vitalist position distorts Harvey's relation to metaphysics and thereby distorts the nature of his scientific enterprise more than commentators realize. The sidestepping they see as innocuous actually is a major obstacle to seeing how his arguments were superior to those of less careful predecessors--when they were; and the confusions in his metaphysics are left as unexamined as the metaphysics itself.

A last note on the most unsatisfactory accounts--from the point of view of the relations between confused metaphysics and science--must concern the undiscovered connections between arteries and viens. Most readers will know

that Harvey could not discover the capillaries; that he postulated the existence of similar but not identical connections (anastomoses). Great awkwardness or distortion often occurs in reports of this. First, commentators tend to ridicule or to express astonishment at Galenists who insist on the passage of blood through pores in the thick muscular septu^m which divides the right and left halves of the heart. This was an important but false claim on which Galen's theory depended. When writers ridicule or decry the anti-observational position argued by Galenists, they find themselves in an awkward position in trying to transmit the information that Harvey had to make similar anti-observational hypotheses on behalf of the undetected capillaries needed in his own theory. Moreover, few reports of this matter include the fact that there are deep pits in the septum which look very much like holes, nor that according to Galen the blood was supposed to percolate through these holes after being heated sufficiently and that this is a phenomenon that could not easily be observed at any time and certainly not in dead or vivisectioned hearts. Nor do many commentators on the issue report that Harvey continued looking for connections for twenty years without success but held up his theory as accurate nonetheless.

What is important for our purposes is that all the above distortions arise in aid of the view that being scientific means committing oneself to a straightforward, more or less automatic, observational method or straightforward criterion of acceptability; no theoretical intellectual battles or difficulties--only technical ones--arise for the true researcher on this view. He observes and proves without trouble except from critics who do not do these things.

The urge to ridicule Galenists who believed in pores and to avoid noticing that Harvey's work was equally distant from observation goes hand in hand with a disdain of theoretical arguments and problems, and the latter, for Harvey, brings in metaphysical problems and views. One can only realize how important Harvey's metaphysical orientations were when one acknowledges that his observations did not do all the scientific work for him.

(b) Science as proof not discovery.

When the interpretation of Harvey as a radical inductivist breaks down, commentators will often try to rescue him as a modern scientist by stressing that he proved his views. Harvey it is said, clearly had working hypotheses, even speculations; he clearly did use Galen and Aristotle, both their theories and their methodological policies; so, in method of discovery, he was no radical inductivist. But, one can distinguish him from other speculators because he knew how to prove his theories; a sure scientific character belongs to his work thereby, and a character which distinguishes it clearly from anything metaphysical. So the argument goes at any rate. Of course, it is extremely difficult to spell out any criterion on which Harvey's theory is proven--since it is not; and thus distortions and awkward maneuvers characterize these attempts to demarcate Harvey's scientific activities from his metaphysical endeavors.

To elaborate on these attempts: Sir Henry Cohen's summary of Harvey's contribution is taken to be avant-garde among historians and is repeated by many--including Walter Pagel, who tries to acknowledge Harvey's metaphysics.

we place Harvey amongst the greatest physicians of all times not because he discovered that the blood circulates but because he demonstrated beyond all reasonable doubt that the blood circulates.⁸

Page1 gives another beautiful example of the emphasis on proof in a citation from Sir Henry Dale:

Harvey's greatest achievement, [is] not that he made a discovery of such profound and permanent significance, but that he created and displayed for all time the method by which such discovery may be attained and made secure.⁹

This attack makes one able to mention Harvey's predecessors in ideas and Harvey's metaphysical leanings without demur as part of discovery, and at the same time as "superficial" or unimportant aspects of the situation which do not touch the essence of Harvey's science. The latter lies in his demonstrations. The demonstrations which accomplish this are often associated with Harvey's use of calculations, discussed below. Yet, even without that explicit thrust one can see that the emphasis is one way to avoid Harvey's metaphysics. The stress is on the permanence of either the proof or the theory. Commentators do not have reason to remark on the weakness of the theory. The avoidance of the broader metaphysical framework of problems and interests makes it impossible to distinguish Harvey from his predecessors: who were not so successful except by reference to stricter proofs than Harvey did in fact manage. Let me go over this slowly.

When commentators try to specify the nature of the "demonstrations" which make Harvey's speculations scientific, the confusion here becomes more apparent. One of the central claims made on Harvey's behalf and important

in this debate is that he conducted a "limited investigation" as Gillispie puts it. He narrowed down his investigation moving away from grandiose (metaphysical) speculations and concentrated on what he could prove; that is, the implication when it is not the explicit claim. Witteridge expresses similar views when she insists on Harvey's "determination never to go beyond the facts" into questions that were obscure or into philosophies. W. P. D. Wightman, who purports to give a history of science that includes the connections it has with broader cultural traditions, still links Harvey's success as a scientist to the fact that he was "true to his ideal in not going beyond the facts". He grudgingly acknowledges that Harvey has not "exorcized" Aristotle from his mind but explains that he was successful nevertheless because he applied Aristotle to "the cultivation of a narrow plot of ground" and with a "caution and economy taught by two thousand years of error".¹¹

Such expressions in praise of caution, limited scope, and hypotheses which are carefully restricted to a testable (if not provable) area, clearly suggest the corollary that metaphysical concerns are not linked to science. As explanation of Harvey's approach in themselves, however, they are seriously deficient. For one thing, Harvey did not stick to the facts in any straightforward sense. For example, two pages before Wightman congratulates Harvey for his steadfastness in this respect, he praises him for his contributions to the very wide question, what is life? Witteridge, who is more consistent here, must completely deny Harvey's contributions to such discussions and to the wider questions of the overall function of circulation. As noted earlier, the existence of the capillaries is a bit of boldness in conjecture for which

Harvey is congratulated with little attempt to correlate this to other congratulations on "restraint" and caution.

This last remark indicates an important problem with the theory that a scientific spirit is to be equated with mildness of speculation or narrowness of problem. Many commentators--like Butterfield--when they are not praising Harvey for restraint praise him for boldness in, for example, risking controversy in publishing his book or for declaring the existence of capillaries despite lack of empirical findings. More importantly, there is a serious discrepancy between the remarks on admirable limitation above and the congratulations Harvey receives--much more deservedly and appropriately in my view--for his "breadth of vision" and sense of the problem as a whole, as Singer mentions, features this broad sense, and Butterfield also claims explicitly that Harvey's great merit lay not in experimental innovation but in his "power of seeing the whole subject in a new framework and restating the issue in a way which made the problem manageable".¹²

This point is extremely important since Harvey's broad sense of the problem and the importance of individual facts is, of course, directly related to his metaphysical interest and sensitivity, although few commentators beyond Pagel and J. Curtis¹³ seem to make the connection. Others are regularly forced toward contradictions. Thus, each of the authors above also sees--on other pages--that a careful attention to detail could not summarize Harvey's merit, ... Singer notes that Harvey went beyond Fabricius in seeing not simply the fact but the purpose of the valves in the viens--a "beyond the facts" concern for which Harvey is widely praised. Singer adds

also that Harvey excelled through relating the workings of organs to the body as a whole. His own summary of the facts reported in the exposition of the theory in De Motu includes careful notes that most of them were known before Harvey; for example, the process of dying from blood loss. This would seem to show that Harvey was superior in seeing these facts as part of an argument--and an argument against a whole physiological framework. Yet, it is his restraint and caution, not this broader vision and presumption, which gets the credit as scientific.¹⁴

Butterfield makes the same odd summary in the fact of his own comments. He emphasizes more than most, that Harvey "grasped the whole in a mighty synthesis"--the whole problem and the significance of various developments.¹⁵ Yet, like Singer, Butterfield does not make a connection here with any metaphysical interests. He reports the fact that disputes between Aristotle and Galen on the function of the heart were the beginning of the whole area of research but does not mention that this dispute involved two metaphysical systems. The function of the dispute in isolating problems and focusing issues could hardly be understood unless the full content of the systems as metaphysical is acknowledged. Likewise, Singer is careful to note that Harvey's fruitful concern with the way an organ fit into the body as a whole "not Galen's concern with Design but a modern concern for machine".¹⁶ As Harvey was a vitalist this is a misleading simplification of his departure from Galen. A better alternative would be that his concern was Aristotelian--a concern for Aristotle's sense of hierarchy and teleology--as opposed to Galen's overview. But, of course, this can not come out apart from metaphysical

interests. Many other examples of the lack of appreciation of Harvey's eye for a problem, his concentration on broad issues, and his boldness in tackling many problems from many angles can be given; such emphases distort the character of Harvey's scientific work as well as covering its connections with metaphysical interests, problems, and theories.

(c) Biographies as Opposed to Studies to Harvey's Science--Keynes.

Some commentary on scientists passes by critical review because it proposes to deal with a scientist's life rather than to be a specialist examination of his work. However, in this study of the relation between metaphysics and science, such a pardon can not be granted without seriously prejudicing the result. For, comments on a scientist's life include reports on his motivation and attitude toward science as a rational enterprise. These attitudes reveal metaphysical commitments in themselves and reflect metaphysical beliefs concerning the aims and potentials of science. Moreover, inadequate or limited treatments of these matters on the part of commentators often illustrates their own metaphysical commitments as well as an unwillingness to be explicit on the metaphysical commitments of the scientists they are studying. The "authoritative" biography of Harvey by Geoffrey Keynes¹⁷ illustrates these points.

Keynes tries at all times to present Harvey as a thoroughly admirable character and his behavior as completely defensible in all his scientific and personal encounters. Doing this makes him stress Harvey's metaphysically-oriented attitudes toward science but he does so without recognizing them as

such; Keynes is oblivious to the metaphysical base and accepts it uncritically. He presents Harvey as a devoted searcher after truth following a well established tradition in history of science;¹⁸ it is argued here that the orientation he describes is part of late Renaissance light mysticism--popularized as Kaballism, the Jewish mysticism. Keynes, although a student of mysticism himself, does not connect the traditions.

To elaborate, one of the strongest concomitants of Kaballistic--as all mystic--world views is the view that enlightenment depends on purity of soul, on commitment to the good without regard to one's personal glory. Conversely, there is a theory of the mystic potential of the body in Kaballistic theory in the form of the microcosm-macrocosm theory of the body--that all important aspects of the body have a parallel and significant connection in the world. Therefore, only purity of soul is required for success. It follows that all mistakes and failures indicate selfish and impure motives. Many commentators report such purity of soul among scientists as a matter of course. Robert Merton had indicated its puritan associations and Joseph Agassi has remarked on the strength of the doctrines of prejudice and of error in Bacon; Agassi notes that it is of Kaballistic origin.¹⁹

Such a theory leads of course to a very hostile attitude toward critics who, on this philosophy, can not criticize one's work without thereby criticizing one's character. Conversely, anyone who believes this theory feels obliged to point a successful scientist as a man whose soul is pure and who never makes a mistake. Keynes fits this description; he clearly thinks Harvey had to prove the theory of circulation without qualification, doubt,

or revision; he congratulates Harvey several times for doing just that and for insisting on the accomplishment.²⁰

Keynes is further led toward maligning Harvey's critics even when they have excellent criticisms. His acceptance of the standard metaphysics of science leads him to share and praise Harvey's expressed contempt for controversy. Keynes is ready to see Harvey as nasty and self-occupied; he introduces comments to that effect by repeating with apparent approval that controversy is a bit low--particularly when one has such terrible critics as Harvey had:

Harvey was on principle averse from indulgence in controversy and had complete confidence in the truth of his doctrine derived by induction from experiments, a brilliant innovation of method in 1638, when Galen's use of experiment had been forgotten.²¹

In the paragraph immediately above this, however, Harvey is "certainly irritated" by criticism; nevertheless, according to Keynes "Harvey perhaps found most of his opponents and their futile arguments as tedious as we sometimes do, and he took no steps to defend himself in print".²² Of course, Harvey did answer his critics; Riolan in not one but two published responses of some length, Hofmann in letters and repeated offers of demonstrations in public. Moreover, the criticisms occupied his own thoughts chronically according to other commentators; they were potent on Harvey's own criteria--not to mention ours. But, leaving this deeper issue aside, here it is clear that Keynes can not easily reconcile Harvey's supposedly admirable awareness of the irrefutability of his theory with these responses.

What is more interesting for the present purpose is that Keynes'

difficulty with Harvey's responses to his critics reflects Harvey's own ambivalence; Harvey is pettish and somewhat nasty--slow to acknowledge a good point and quick to accuse an opponent of personal interest and bias. All this reaction is made understandable if one recognizes the element of Kabalistic moralism in the atmosphere; the recognition makes Harvey's behavior less personal --good or bad--as well as more interesting and explicable. But Keynes is as blind to Harvey's "rudeness" as he is to the critics' merits; and, all of the written controversy between Harvey and critics is presented as extra-scientific by him: science is the demonstrations in De Motu.

Were Keynes more sensitive to Harvey's broader metaphysical orientation, he would see not only that his hostility to criticism had a metaphysical base connected with his idea of science, he would also note that the criticism made by Hofmann and Riola had a clear rational. Another point which would become more straightforwardly discussed is Harvey's curiously dissatisfied view of his life as a scientist. That the search for truth in science is related to the search for religious enlightenment in the Middle Ages is now an uncontroversial admission. It indicates a divided, confused metaphysical commitment is not so often noted. Yet, Harvey's optimism and willingness to search for true connections in the body is metaphysically based; his confused metaphysics; it gave contradictory impressions on the accessibility of reality and the status of searches for the truth.²³ That the search for truth is not To develop this point on the contradictory trends built into Harvey's attitude toward science, one can note the Kabalistic and the medieval mystic orientation (described by e.g., Wolfson) reveals the mystic is ambivalent as

to whether he studies in the hope of full God-like enlightenment or simply as a devotional exercise without pretensions to serious (rather than minor technical) enlightenment. Harvey may be quoted to express both views in a contradictory spirit, each of which reflects a side of his metaphysical coin. And, his sad resignation toward his life as a scientist (in spite of the fact that he was successful and recognized to be successful in his lifetime) reflects an attitude toward science different from that we are used to today. It is an expectation of peace and great change closer to the metaphysical environment of the Medieval Kabalist mystic. Yet, neither Keynes nor Cha^{vois}, or Witteridge--although they all devote effort to Harvey's personality--discuss his attitude toward science unless it is to ignore or apologize for his expressions of weariness and regret; however, they report sentimentally and with approval his expressions of saintly devotion.²⁴

A last word appropriate to treatments of Harvey's life is that a hesitation to discuss Harvey's attitude toward science as a lifework reflects a certain philosophy of science hostile to any explicit metaphysical connections as well as to those that reflect a contradictory metaphysics. That is, according to traditional philosophy of science, the attitude appropriate to a scientist is the disinterested love of truth as such--truth, in the abstract, particularly as opposed to any contentful metaphysics which would be a bias or prejudice. A commentator who gets too explicit on Harvey's attitude toward science risks revealing that Harvey's interests in it were indeed less abstract than the undistinguished "search for truth"; they were such motives as, e.g., the wish to defend and resurrect Aristotelian metaphysics toward

the body—in particular views on the position and functioning of the heart in the overall scheme.

Section 3.2. Survey of Harvey Scholarship--more sophisticated historical treatments.

Many of the criticisms of the more traditional treatments of Harvey's science are derived from the works of Kenneth Keele, Walter Pagel, and from the development of a generally more critical attitude in history and philosophy of science due to Burt, Koyré, and Popper. I am not going to review the development of this critical tradition in the history of medicine as opposed to the general history of science; this history would center on the studies of Karl Sudhoff in Germany and the school of historians he educated as well as the better works of scholars such as Osei Tempkin at Johns Hopkins as much as forty years ago. But, it is necessary to review a representative sampling of Harvey commentaries of a more tolerant sort on Harvey in order to see how much an appreciation of metaphysics is approached.

More realistic "historical" treatments are only developed in their philosophies of science to a degree; the recognition of the temporal status of even the most acceptable scientific theories is marginal, for example. However, most of the later historical treatments do incorporate a recognition of the need to understand the context of contemporary problems and obstacles and a recognition of the fallibility of scientists; these aspects go hand in hand. There is also a willingness to examine sympathetically the differences between Harvey's actual views and a modern interpretation of them; for this an understanding of Harvey's predecessors and the strong points in their work is necessary, as well as the actual detail of the problems in previous theories. Also, of course, one needs very often to understand the broader intellectual climate of the age in order to fully

interpret a theory as it was advocated by those who believed it--even the scientist Harvey speaking of his science. A notable example of this is the fact that "circulation" was traditionally understood to refer to the purifying cycle of evaporation and distillation of a liquid and some suppose precursors of Harvey postulated a "circulation" of this order. Understanding that Harvey knew the tradition and yet meant to communicate a mechanical circulation and a metaphysical one--not a circulation of semi-chemical nature--makes his position far more clear and interesting than it would be otherwise.

It is clear that the studies sensitive to wider intellectual problem situations of scientists are far more likely than others to encompass the metaphysical views influential in the science that they treat. And, both Pagel and Keele, and the philosophers treated below, report more of the connections I believe are significant than commentators mentioned previously. It is another matter, however, to take the connections between metaphysics and science as just that, and further, as significant; it is also another matter to articulate and discuss the significance. Moreover, none of the commentators below go far enough in seeing that the metaphysics influential in science may well be a renegade metaphysics--outside the established world view and functioning in the development of criticism. Nor do any of the commentators acknowledge the confused nature of the useful metaphysical views Harvey employed in his scientific thinking; even the possibility that they were of such a nature often implicitly barred in their descriptions.

(a) Critical Historians on Contemporary Setting

Kenneth Keele and Walter Pagel are far and away the best commentators on William Harvey's science from the point of view of disclosing a more accurate view of the relation between science and metaphysics. On many other counts--particularly in being critical of Harvey's work, ready to see its historical character including its weaknesses, they are superior. I will review their merits but it will still be necessary to conclude that their sensitive studies still do not yet capture the full reality and significance of the interaction between science and metaphysics in Harvey.

To begin with an extremely important point, Keele and Pagel acknowledge the scientific justice of the questions put by Harvey's critics--particularly those who ask the metaphysically potent question, why does the blood circulate? At this significant point in Harvey's scientific behavior and the history of the development of his achievement, Keele goes so far as to say Harvey is evasive.²⁵ This is far removed from the diffidence on these points of other commentators.

Keele also comments more directly than any commentator on the puzzle created by the contrast between Harvey's personal intellectual conservatism--as evidenced in his love of Aristotle and of stable Royalty--and his scientific groundbreaking. That this caused Harvey some considerable psychological discomfort is clear; as noted, in other studies his love of truth is taken as a matter of course and all hesitations he expresses are ignored or explained as justified by persecutory tendencies in others.²⁶

Observing Harvey's own conservatism and taking the conflict as a point to investigate opens the door to another place in which metaphysics has a role in Harvey's science; for, it seems clearly likely that Harvey gained a great deal of essential encouragement from the fact that his theory, if true, would conform to Aristotelian metaphysical framework. This is a kind of motivation, inspiration, which is undervalued because of the strong traditional philosophy that new ideas do not take anything but observation or application of automatic method; a view of science as needing intersubjective interaction and a lot of plain application in the sense of trial and error has a place for this kind of encouragement, but a more traditional view does not. Without the idea that he had the support of Aristotelian metaphysical backing, Harvey--as conservative and timid as he was--may not have had the temerity to pursue his dogged research.

Keele does not hesitate to note Harvey's doubts and suspicions. Compared to Keynes' insistence of Harvey's proof of the theory of circulation, Keele says,

Since it was not demonstrable, Harvey's circulation remained a theory for which he never ceased to look for support, nor did he cease to examine the criticisms which inevitably appeared.²⁷

This further acknowledgement of the scientific justice and appropriateness of criticism puts Keele's appreciation of Harvey's achievements in a superior class. He approaches a breakthrough when he speaks of the "characteristic metaphysical imagery"²⁸ which Harvey uses in presenting his theory.

Another point of superiority in his treatment comes when Keele reports Harvey's sarcasm in the discussion of "arterial spirits". This is a point at which commentators often show their obliviousness^{of} or their hostility to metaphysical

incursions into science. Keele reports it as sarcasm instead of as righteous, rightminded judgement on Harvey's part. And, Keele goes on to show that Harvey's own attitude was not "modern" despite his attempts at naturalizing the spirits.

...he opens (the discussion) with the sarcastic comment that 'persons of limited intelligence, when they are at a loss to assign a cause for anything, very commonly reply that it is done by the spirits'. His own conclusion, in keeping with contemporary thought, was that spirits are to be regarded 'in the same way as the spirit of wine in wine, and the spirit of aque vitae in brandy'. Just as it is the same blood in the arteries as in the veins, so are the spirits of the blood the same, though in the arteries 'it may be admitted to be more spiritous'.²⁹

Here Keele does not conceal Harvey's "contemporary", which is to say his metaphysical and not modern, theories about the blood which are connected with his vitalism.

However, Keele does not discuss the metaphysical aspects of Harvey's science at any length and often not as metaphysical; e.g., he does not discuss the evasions Harvey goes through on the purpose of circulation nor his lyrical notes on circular symbolism although he is exceptional in mentioning both prominently.³⁰ But, no definite view of how Harvey's metaphysics connects with his science is developed by Keele's historical picture despite the fact that it is far more sensible and unapologetic than previous treatments. This is mainly due to Keele's undeveloped philosophic position on science, to which I must now turn.

One must note that Keele is eager as many to credit Harvey with some sort

of scientific method opposed to philosophic thinking. His comments on Harvey's contemplative nature--meaning a contemplation of nature indicates this. That is, he congratulates Harvey for not speculating; his contemplation of nature was supposed to be significantly different from philosophic speculation. Keele also says it was wise of Harvey to avoid putting the word "circulation" in the title of his book since the fact of circulation was an "inductive inference rather than a demonstrable fact".³¹ Keele is one of the commentators who want to credit Harvey with proving the theory of circulation rather than discovering or articulating it; as he already noted that it was not demonstrable, this causes him several awkward moments.

For example, Keele gives considerable attention to Harvey's religious attitudes; the reason is not clear until one notices that Keele is trying to explain without saying so that Harvey's vitalism--which approached pantheism--was not obscurantist but was, "neither ancient nor modern...[but]...the essential core of religion possessed by many a scientist in any century".³² Thus, the passage is more apologetic than it might superficially appear; Keele is defending Harvey in the face of his vitalism, to his great credit, his quotes are representative enough section of the passage to show his defense is only partially possible.

Due to his anxiousness to show there was a scientific method of some sort to associate with Harvey, Keele can not go farther than "imagery" in putting forward the metaphysical concomitants of Harvey's thought. His section on "Harvey's Scientific Lineage" includes Plato's metaphysics but not qua metaphysics--only to show Harvey as "mathematical". This proof can not actually

be given, as I discuss in the next subsection, but if it could be the connection with Plato should appear as a connection with "metaphysics". The section also includes Aristotle's methodological recommendations but not his metaphysics, and Hippocrates' criticism of those who think "it is not possible for anyone to know medicine who does not know what man is..." which Keele quite surprisingly presents as a theory of induction which Harvey followed.³³ The strain to present this lineage as methodology shows the hesitations in connecting the science and metaphysics.

As far as the influence of Galen and of Aristotle on Harvey's scientific thought is concerned, Keele always deemphasizes their importance--particularly the importance of any tendency toward explanation in terms of final cause. The important point is that final cause explanation in Harvey's science shows not only its metaphysical connections but some important confusions in metaphysics. But, Galen is considered in Keele a regressive force in medicine because he combined philosophy and science: Harvey is credited with being unique in that "claim to be an ancient philosopher is as strong as that of being a modern scientist".³⁴ But this combination--although it acknowledges Harvey's traditionalism--has nothing to do with Harvey's scientific success. The latter Keele attributes to "the chance of time and genius" not to philosophy, especially not to any philosophy connected with final cause or metaphysics. Thus, in one of the only apologetic passages of the book Keele reports:

Harvey took the Galenic material that came down to him: he could not very well do otherwise since it comprised almost all the medicine of his day. But he flatly rejected the whole pattern of Galenic thought. Galen's whole attitude, as reflected

in such works as De Usu Partium was to demonstrate deductively the perfection of organs for their purpose, this purpose having been determined by divine plan according to Galen's specifications. This type of reasoning was never adopted by Harvey.³⁵

Keele reconciles this last statement with the passages in De Motu and De Generatione which contain Harvey's vivid arguments on the perfection of the organs and their function for their purposes by calling such passages expressions of Harvey's religious or political views.³⁶

Keele has a point in claiming that "Harvey refers to God only as a prime efficient, e.g., as a first active cause working throughout natural processes, and not as an agent of supernatural power".³⁷ But his suggestion on this basis, that for Harvey metaphysical arguments and the functions of his scientific views in them were not important, is not true. In a similar way, Keele surpasses his predecessors in mentioning the light mystic, neo-platonic tradition of Ficino and Pico but equals them in dismissing it as an appeal "to symbolism rather than observation". Thus, Keele forgets that he has argued previously that Harvey was concerned more with mathematical arguments than ocular demonstrations. The dismissal also ignores Harvey's use of microcosm-macrocosm theory which might easily be called symbolic in a way similar to the neo-platonic use.³⁸

Walter Pagel goes farther than anyone in discussing Harvey's metaphysical ideas qua speculations--which means qua metaphysics. The title of his book, William Harvey's Biological Ideas, is meant to indicate the importance of the theoretical background in Harvey's science. Pagel pushes Keele's merits farther and adds several others. He discusses the importance of circular

symbolism and hierarchical ideas in Harvey at great length; he includes a history of the microcosm-macrocosm theory back through Bruno; he calls Harvey the "life-long thinker about the purpose of circulation" and claims that he came to the theory of circulation by a consideration of Fludd's speculations on the metaphysical nature of the world and by pondering the "final cause" of the valves in the veins. Harvey's commitment to Aristotle's metaphysics and his vitalism are unqualifiedly admitted; his lyricism on the nature and preeminence of the blood is recorded and discussed.

Finally, Pagel explores rather than apologizes for Harvey's concern for metaphysical properties of the perfection of the organism, circular motion, hierarchical order in nature. I do not think he draws strong enough conclusions on the basis of his own discussions and that this leads him to weaken some descriptions, e.g., ^{he} suggests Harvey really proved the theory of circulation and that his method was clearly modern and distinct from a philosophic method when he did that. But, it is impossible to adequately summarize the discussion Pagel gives of these various points; the list is merely that; it shows that Pagel tries to credit Harvey's metaphysical views as part of his science, at most relevant points. Most of the criticism of earlier commentators would have been impossible without his work.

Pagel's approach to the metaphysics of Harvey and his earnest concern to rescue it from disdain and oblivion is the approach of an interested realist and true historian, not of a philosopher of science. Pagel wants the actual, rich character of Harvey's problems and ideas appreciated, not limited by some parochial abstract theory of what should have been his outlook and concern.

Pagel sees his mission as the recovery of an historical person who had been split and lost due to the uncritical attempt to present Harvey as a modern scientist.³⁹ This led to the split between Harvey the philosopher, the metaphysician, even of Harvey the researcher who used Aristotle and admired Galen from the allegedly different and more modern discoverer of the theory of circulation. Pagel--as will be noted below--is ambivalent as to whether or not there is an unfortunate split in theory of science here; but there certainly is an unfortunate and distorting historical split involved in his view; the thoughts and activities of the two Harvey's are, he shows, inextricable intermeshed; any theory which denies it is not describing Harvey as *he existed*.

The above summary of Pagel's work as the rescue of an historical picture of Harvey the actual researcher is not strong enough in my view; although I say this, it is Pagel's own description. Pagel's historical work, like Koyré's, does not merely reconnect images which theory of science separates--as theory always must do. Rather his work refutes and/or severely (irrevocably in my view) weakens certain ideas on science and important corollaries of them. That Harvey was not a genuine Aristotelian, that he was an inductivist methodologically, that he cared only for mathematical proof and not metaphysical proprieties in his theory, that his vitalism was shallow and unimportant--all these are examples. I am going to leave these points aside since they have arisen in discussions of other commentators and turn immediately to the deficiencies of Pagel's work from the point of an adequate view of a relation between metaphysics and science.

Pagel's failure, like that of Keele, Burt, and Koyré, is in not

appreciating the extent to which his findings force a change in the usual views of science; he tries to combine his work with an idea of science as a stable, reliable system of ideas; it leads him to streamline the metaphysics he recovers in Harvey's thoughts, to cover up and underestimate the difficulty and confusion of the views like final cause which helped Harvey, and generally to vitiate his own findings in spite of himself. As in the case of Keele, this failing is linked to a belief that we know what is the scientific method and can pick out which of Harvey's arguments and actions were part of it, and to the further belief that the method leads to secure results of more than historical interest.

As one specific example of how this streamlining occurs, the more convinced Pagel becomes that the metaphysics of various Renaissance scientists in an integral part of their scientific work, the more determinedly he defends and presents the metaphysics as coherent and unproblematic. This is part of the reason he stresses the Aristotelian leanings--as opposed to the stronger neo-platonic notions--of such thinkers as Harvey, Paracelsus and Marchi, (an opponent of Harvey's).⁴⁰ For example, Pagel's work on Harvey's metaphysics convinces him that the commitment was vital to his thinking and arguments; yet, he closes his article, "The Philosophy of Circles" with a characteristically unfortunate and not very sensible denial of the significance of his arguments.

Harvey's death occurred at a time which saw the emergence of iatrochemistry and iatrophysics; Harvey, the Aristotelian, died in 1657, while Harvey, the discoverer and physiologist, was to remain immortal.

However, the symbolism of the circle lives on and found resonance even in sober observers and experimentalists of the iatrophysical school. Indeed, the conviction that everything moves "in a circle" led to interesting and scientifically

correct observations and experiments concerning the movements of bodily fluids other than blood.⁴¹

This passage is an extremely disconcerting mixture, downplaying the role of metaphysics and listing facts which deny the understatement at the same time. Similarly, when Pagel has finished the historical sketch of the metaphysics of the circle behind Harvey's thoughts--which he takes through Bruno's idea of the "circular motion of the heart, blood, and semen", he plunges very bravely into the question: "What, then, is its significance, if any, in this history of Harvey's discovery?"

Harvey's discovery and much of his observational work in embryology can be seen as the scientific complement and proof of these contemporary ideas...To Bruno, they are philosophical and cosmological truth. The supremacy of the heart and blood which follows from the supremacy of the circle, i.e. ...So they are to Harvey--with the difference that he had something to give in addition. This addition remains the foundation of modern scientific biology, whereas the speculative background lost all but historical interest.⁴²

Pagel can not say very precisely what the addition actually amounts to which is both truly Harvey's thought and also of "more than historical" interest.

Pagel seems to think that the addition is the reliability, the something extra in security of science. That even scientific views are only of historical interest in the sense that they are no more permanent than Harvey's belief in the microcosm-macrosocm theory--in fact less permanent and more historical or transient--is an idea that would allow Pagel to conclude his work without misleading retrenchment.⁴³ But he does not explore the possibility of inter-

preting science in that way. As it is, I conclude that Pagel does not appreciate the richness of his own findings on the relation between metaphysics and science.

To conclude, one might think that, in my view, there is a conspiracy to find undeserved praise for Harvey. I do think that there is a non accidental--rather than a conscious--phenomenon to be noted here; the uncritical reports and tendency to hero-worship are the result of certain theoretical positions which affect historians and other commentators in spite of their conscious purposes. I would not say the praise is undeserved as much as uncritical, misdirected and misleading. Why praise Harvey for disdaining controversy, for example, when his debates with his opponents were important in the development of the field? This conspiracy is not one of negligent , lazy, prejudiced, or biased minds but is the result of powerful theories--theories of rationality particularly.

Now let me address the excellent point that there must be something important on Harvey's side that invites the defense of his work. There is no doubt about this. Harvey is far and away superior--in clarity, in fruitfulness, in interest, in the substantive character of his work--to nearly all of his predecessors including those who have entered standard histories of science--say, Vesalius, as a powerful example. There is no doubt that it is in trying to say why--in just what way--he is so superior, that many commentators make the reports for which I criticize them. I empathize with their effort to say why he is so good; but they do a much poorer job of it than comparable scholars in other fields because historiographic principles in history of science are infamously inferior--as Agassi as discussed--due to the background problems such

as I am investigating. I mean to be emphasizing Harvey's important "right-mindedness" in objecting to the uncritical way it is reported.

(b) Mathematics vs. Metaphysics in Sophisticated Historical Treatments

Many commentators who find the the presentation of Harvey as an inductive, modern, radical, positivist falls apart in their hands look for other bases from which to present Harvey as essentially distinct from non-scientific, unsuccessful thinkers. Often the second line of defense against any association of Harvey's science with philosophical or metaphysical ideas is Harvey's mathematical or quantitative methods: Harvey's use of quantification and his decisive quantitative calculations made his research fruitful and convincing; his commitment to these factors in his thought rather than to any metaphysical proprieties are what made his work valuable and successful. This view can replace more standard argument concerning the source of the idea of discovery and it can replace any argument that the theory is "based" solidly on empirical evidence of a pure sort in ways Galen's was not; alternatively, it can be a way to explain how Harvey's view is more empirically justified. In all these combinations, Harvey's stress on quantitative experiments which show facts about the heart and movement of the blood is taken as decisive and of a different character than any philosophic-cum-metaphysical consideration. This makes commentators who want to credit Harvey's metaphysical side a bit confused, as Pagel's treatment shows, despite the fact that it is superior.

Pagel opens his chapter on Harvey's scientific approach--what makes Harvey a scientist--as follows:

The truly scientific element and hallmark of Harvey's discovery is usually found in the calculus which he applied. Harvey's quantitative reasoning is said to form the dividing line between him and

and his predecessors. His discovery--so it is interpreted--pivots around this point.⁴⁴

Pagel is expressing some hesitation; but it is difficult for readers to find out what it is since Pagel wants to agree with the conclusions he reports, by and large. Pagel wants to agree that Harvey's arguments of a quantitative nature are decisive but in my view he does not know if he could be talking about "discovery" as he does in the passage above, or about "proof". That is, he must ask, is Harvey's use of quantification important to the discovery or to the proof? Pagel prefers discovery here because he knows that in the "proof" of the theory something^h approaching final cause explanations were also considered important by Harvey. Also, Pagel knows that to take the quantitative approach as demarcation between Harvey and others is not possible since others used it before Harvey did without similar results. Also, as noted below--the quantification was loose;^{and}_A certainly not what was considered "mathematical" by thinkers in Galileo's tradition. These are points missing from other discussions of Harvey's calculations and quantification, which vitiate them.

However, the stance in favor of quantitative methods as the hallmark of Harvey's science deserves a closer examination because it is so important as an alternative to taking seriously his metaphysical arguments. The full argument about quantitative methods centers on the powerful experimental series Harvey designed to show that the amount of blood passing through the heart into the aorta was more than could possibly be produced by the "ingesta" in the time allowed according to Galen's theory. The amount of blood passing through the heart in a matter of minutes was, in fact, more than that circulated in the

entire body of the live animal. The calculations which led to this result included estimates of the average number of heart beats, the average size of the left ventricle, and such quantitative estimates. The argument, of course, requires other experimental results showing that, e.g., the blood could not return into the heart chamber because of the arrangement of valves. The quantitative measures have to be connected with the whole range of arguments that show the only way for the blood to return to the heart is through the veins. Otherwise, it would not be a serious breakaway from Galen's view that the blood ebbed and flowed toward the extremities to be used nutritionally there while being replenished by the liver working on the food eaten. But, although the entire series of experiments is necessary to give the full force of the argument, the particular one as to the amount of blood passing through the heart was considered by Harvey, his critics, his friends and foes, as a particularly powerful and decisive point.⁴⁵

Commentators who want to keep metaphysics separate from science can claim that the quantitative argument which these experiments involve are straightforwardly empirical and entirely independent of any particular metaphysical commitments. One can contrast a commitment to this type of quantitative calculation to a commitment to the argument that a theory fulfills, conforms to, or corresponds to a particular type of metaphysical world view. One can say that although Harvey thought a great deal of arguments of the latter sort which defended his theory, he made his stand on the former, calculative claim. This fact, it may be claimed, limits the claims which may be made on behalf of a relation between metaphysics and science as important to Harvey's

scientific work; if the major experiments and criteria he used are unrelated to metaphysical commitment, these latter may be encouragement or motivation but are not part of his "science".

Such arguments are very powerful although rarely if ever stated: The defense of Harvey's method is vigorous; but *its* function as a sort of demarcation against philosophy does not often arise. I have presented a strong version of the arguments--one which does not make claims of proof or claims that the motivational aspects of the work are unimportant only that they are independent. These merits are lacking in most of the presentations by Harvey scholars; the lack makes their treatments unhelpful in this debate.

First, then, one must note that commentators often combine their claims on behalf of Harvey's calculations and quantification with the implication that Harvey was very careful and precise; the care and precision and mathematics is what is supposed to have made the significant difference between him and his predecessors. However, as Pagel has discussed, "mathematics" as such hardly comes in to Harvey's arguments and his calculations are inaccurate--very inaccurate on modern standards--although not in a direction which would weaken his argument. He underestimated the amount of blood in the body and the amount passing through the heart.⁴⁶ This was no accident but a function of his reasoning that even a rough estimate was good enough for his purposes.

Pagel reports that when this inaccuracy was noted among commentators, they often shifted their praise from Harvey's precision and mathematization to his "sure reasoning, based as it is on correct interpretations of anatomical, physiological and experimental data".⁴⁷

Pagel thinks the change in emphasis is a mistake; that the experiment and calculations are of decisive importance in spite of the fact that they are not accurate.

Pagel, however, can not carry out his argument in favor of the quantitative base as a "significant" unique hallmark of Harvey's science; the ambivalence and objective difficulty he has in this attempt brings up precisely the relevant point in the debate. First, Pagel and Keele both note Harvey's concern for ~~the~~ purpose of circulation and for the broad framework of problems and theories into which the theory of circulation fit; both emphasize that this was important to his research.⁴⁸ They find it difficult then to maintain that his "scientific" character lies only in his use of quantitative argument.

What is important to note is that Pagel (and Keele) is ambivalent about the debate for good reason: he does not want to say that Harvey's "precision" and calculations are more important than his ideas or arguments following Aristotelian methods; and, the emphasis on calculation is often used to denigrate the latter. More specifically, the emphasis leaves his own results about "purpose" on uneasy grounds. Were, one could ask, considerations of purpose important for Harvey's research, thinking, discovery, proof, or not? Trying to find a scientific hallmark for Harvey's science in his quantitative measures reduces all this to the sidelines. One must add that Pagel himself notices--as a last argument against quantification as a hallmark--that Harvey was hardly the first to use such methods even on physiological problems.⁴⁹

Thus, Pagel gets muddled in trying to defend the view ~~that~~ that Harvey's quantitative arguments make him a modern; he ends with the weak position that

although Harvey was not the first to use quantification, he was the first whose use of it led to a great discovery--that this--astoundingly--is what marks him off from his predecessors.⁵⁰ The argument, thus, is completely circular and unsatisfactory.

(c) Philosophers of Science on Harvey--Passmore and Michalos.

Many comments previously have indicated that a lack of philosophic sophistication limits even the better historical studies of Harvey considerably; here I want to review some philosophers who comment specifically on Harvey's science. These commentaries are also inadequate to give an account of the relation between metaphysics and science in Harvey's work; and to put them forward as representative in their weakness in this area. Not many philosophers have commented on Harvey directly; two recent and representative discussions by philosophers of science are those by John Passmore and by Alex Michalos.

Passmore's article, "William Harvey and the Philosophy of Science",⁵¹ is extremely promising; not only is the title propitious but Passmore tries to outline one of the most interesting--to my mind--problems about Harvey's scientific behavior; viz. his sometime stubbornness, approaching positiv^{istic}_A obscurantism, in the face of particularly important criticism. To explain, Harvey outlined the path the blood takes in proceeding through the body, but his theory left many questions open--including questions of the purpose of circulation, the cause of heart action, the possibility of nutrition. When challenged by critics because of the gaps in these areas, Harvey often was silent, sometimes responded in writing, and sometimes appealed to the "brute facts" in these responses. He

argued unhelpfully and uncharacteristically that he describes only what is there: "why" or "how" is not his business. What to say about this appeal to the "brute facts" is a serious and interesting question; raising it is a real credit to Passmore. He makes the difficulty more philosophically potent by raising it in the context of Descartes' insistence on a further, deeper, "explanation" via a mechanism, rather than a label which invites the postulation of faculties (such as the pumping tendency in the heart).⁵²

However, although Passmore clearly brings up Harvey's science in a context of philosophic and metaphysical views, his discussion does not advance our understanding of the importance of an interaction. First, Passmore conducts the entire discussion without using the word "metaphysics" in Harvey's context. Although he recognizes the metaphysical--he calls it "philosophical"--background to Descartes' criteria of explanation, he does not acknowledge any such background to Harvey's. Harvey, he says, insisted that the facts spoke for themselves and thereby introduced a truly "biological" mode of explanation.⁵³

As it happens, of course, Harvey was ambivalent and to a great extent thought more about deeper explanation--beyond the brute facts--of the purpose of circulation than many thinkers before or after; Passmore ignores this side of Harvey for most part. Concomitantly, he fails to distinguish Descartes' call for an "explanation"--some theory deeper than a mere description or labeling--from his call for a "mechanical" explanation. We--and Harvey--could acknowledge the first requirement but resist the second. This makes the argument as Passmore relates it less interesting than it would be otherwise.

Indeed, one of the most serious deficiencies of Passmore's treatment is his

complete neglect of Harvey's vitalism;^{53a} this produces distortion of the metaphysical elements in the debate and also confusion in Passmore's own claims. That is, Harvey's work is philosophically important in the first five pages of his article because it confirms mechanism, whereas in the next five pages it is important because Harvey resists mechanical explanations in favor of empirical results. Thus, Passmore's discussion of even this interesting aspect of Harvey's science--and of his divergence from Descartes--has serious limitations; without a full account of Harvey's vitalism and his own metaphysical commitment to purpose and function as part of explanation, the disagreement can not be spelled out in its interesting form.

Alex Michalos has ten pages on William Harvey in his book, The Popper-Carnap Controversy, which presents a new set of criteria for judging explanation.⁵⁴ The case review is meant to show how Harvey can be interpreted as applying Michalos' criteria--of Cost-Benefit Dominance--in his research on the theory of circulation. Michalos' philosophy of explanation seems unsatisfactory in general ways: like many theorists who talk of judging theories or evaluating them on certain criteria, he does not say for what purpose they are judged; therefore his efforts to describe criteria seem feeble and fruitless. I would not discuss his work but for its special treatment of Harvey and but for the fact that this special treatment is typical in that it purports to establish criteria of the value of the theory in terms completely internal to science. Michalos' theory is that Harvey judged his theory better than its opponent Galen's on four counts of "explanatory power, analogies to other theories, simplicity, and internal consistency". The best part of Michalos' presentation is his discussion of

Harvey's critical review of Galen on internal consistency as he calls it.

Michalos describes well how a literal examination of Galen's claims showed them to be extremely unclear or even unsensible--calling, for example, for the passage of air and blood in opposite directions in the same vessel with no explanation of how this could be done.⁵⁵

However, this aspect of Harvey's critique can only loosely be called a critique of the internal consistency of Galen's view. This points to the major weakness of Michalos' treatment--the arbitrary nature of his categorization. For example, he lists Harvey's failure to show by ocular demonstration that there were connections between the veins and arteries under "simplicity" not under questions of "explanatory power". In the latter category, he makes no mention of the fact that Galen's theory had a prominent place for nutritional function and for heart action whereas Harvey's theory made them both rather mysterious. Michalos dismisses the possibility that the absence of any demonstration between the veins and arteries was serious. He claims that Harvey's hypothesis of such a connection "did not create more problems than it solves"⁵⁶ as did Galen's similar hypothesis of pores in the heart septum. This view can not be taken seriously without a discussion of nutrition.

The most important of Michalos' categories for our purposes is "analogies with accepted theories in other domains". Under this head, Michalos mentions Harvey's concern with parallels between the circulation of the blood and the cycle of seasons, of fermentation, and other "circular movements". It is the shortest of all the sections in Michalos' treatment; he does not call these other theories "metaphysical" or at all indicate that they were non-scientific; also,

of course, they were not the "accepted theories" in their domain but outlawed metaphysics which Harvey was trying to reestablish. Michalos talks of relations and analogies with "accepted theories" so his listing of Harvey's metaphysics here is misleading on several counts.

Also, Michalos markedly avoids any indication of the more muddled interpretation of these "external theories". He does not refer to those passages in which Harvey extols the parallel between the situation of the heart and that of the sovereign. Nor does he point to any connection between establishing a parallel with these other theories and judgements about explanatory power. That is, Michalos does not discuss how the decision procedure works if the different categories yield different conclusions --for example, of one theory has more explanatory power but another has a closer analogy to theories in other disciplines. There is no clear way to judge. He indicates that this is not a problem with Harvey's theory and only discusses it after the Harvey section.⁵⁷

That Michalos fails to recognize metaphysical theories as such--and to see the problems which valuing a metaphysical framework may lead to--makes his account of Harvey unhelpful; his underestimation of Harvey's opponent Galen and of the weakness of Harvey's theory on explanatory power is also debilitating. Unfortunately, the philosophic references to historical cases such as Harvey's commonly suffer in similar ways from an uninformed appreciation of the problem as it was seen at the time--as shown, for example, by Pagel's work.

Section 3.3. The State of Harvey's Metaphysics.

Harvey was no philosopher. Although he makes more metaphysical comments than most historians report, he still does not elaborate his world view to any great degree. Nevertheless, his metaphysical position has been pieced together by some important writers, such as Pagel. Some of these commentators, again such as Pagel, have described some of the confusions in Harvey's view although they rarely describe them as confusions. Thus, the state of Harvey's metaphysics is not entirely obscure. Moreover, there was a powerful metaphysical climate in Harvey's day which has been extensively studied independently of Harvey's personal views. It would be extremely difficult to understand his personal metaphysical position without a familiarity with the background metaphysics of the time.

The late-Renaissance background has been intensively reexamined lately; recent research--by Yates, Scholem, Rattansi, Pagel--has emphasized the (hitherto largely ignored) aspects of the metaphysical climate which were more eclectic, intellectually obscure, and unusual on modern standards. The work has shifted the overall cast of the metaphysical picture of the day toward a less clear image. Therefore, reconstructions of Harvey's personal point of view may be shifted in the same way given his actual statement.⁵⁸ From all of this, the idea of Harvey's metaphysics as confused and a deliniation of the important confusions in it may be culled without great difficulty.

(a) The general question of confusion.

There are several interrelated general considerations which arise in

considering the question of the confused or unconfused state of Harvey's metaphysics. First, it is necessary to address the issue of how an intellectual system or view qualifies as a confused one. Secondly, one must consider how much metaphysical confusion needs to be established to make the claim about Harvey's science and science in general which is made in the general discussion; the claim, that is, that one must allow the possibility that the metaphysics influential in science was confused and contradictory. Thirdly, one must separate confusion from unfruitfulness. Lastly, one must consider how to argue that any author is confused--both how the confusion would be manifest and how to ascribe it to him without arbitrariness.

First, then, it is necessary to consider how a view or system qualifies as confused in the present discussion. No technically refined or profound view of "confused" ideas is required or postulated here. A confused metaphysical system is taken to be one which gives contrary or extremely indefinite and disjointed ideas of the world--either concerning the nature of things which exist or entire world pictures or cosmologies.

The Hermetic-Kaballistic world view, for example, gives contrary indications about the existence of evil and both indications play an important function in the system. The microcosm-macrocosm theory of the body says that the body is arranged in parallel with the world but no indications of how close and reliable a connection exists is forthcoming, and indications that the connection is strong operate in the philosophy regularly with indications that the connection is loose. Statements of each of the contrary positions are usually not forthcoming--only suggestions, hints, or con-

clusions which depend on them; the vagueness allows the contrary views to interplay without direct contradiction.⁵⁹ These very vague indications which verge on contradiction.

Another way in which a metaphysics may be confused is simply that it contains views which may be interpreted as cosmological, ethical, ontological, religious at the same time; the microcosm-macrocosm theory of Kabbala is such a view. One can say that it indicates the ethical cum religious view that the body should be used as an instrument to worship God through study; it can be taken as an objective metaphysical statement concerning the arrangement or ontology of the world. The various meanings melded and alternated. This also makes a metaphysics confusing. This particular view has been accepted as part of Harvey's world view.

How much metaphysical confusion one must claim as part of the general argument on the importance of considering a relation between science and confused metaphysics is a more complex issue. First the argument here includes the claim that Harvey's metaphysics was confused in some important parts. Thus, there is necessarily a claim that some late-Renaissance metaphysical views were confused. Work done by the historians mentioned above make this claim fairly unremarkable. Yet, the general argument does not require that all or even most late-Renaissance metaphysics was confused. It is also unnecessary to argue that confused views are preferable to clear ones.

One can add that the major criticism of the assumption of clarity, that all the metaphysics influential in science is clear, need not involve

any historical claims. The importance of seeing that the assumption is an uncritical one is independent of the history to a great degree. If there had been no incidents of a confused metaphysics influencing a scientists work, the criticism that the assumption is made uncritically and implicitly could still be made and still be important. One could still argue that any philosophical theory on the relation between science and metaphysics leave room for the possibility that the metaphysics involved is a confused metaphysics; alternatively that it provides better reasons than have yet been provided for excluding the possibility.

To go on to the third point, the crux of the argument here is that Harvey's metaphysics was both important and confused. It is essential therefore not to understand the word "confused" with its derivatives and declensions in any pejorative sense which would implicitly *separate it from a view which* was helpful. Also, in saying that the confused world view was helpful, one is not saying merely that the clear parts--or acceptable parts--which can be filtered out of the entire position--were helpful. Rather one is making the claim--which I do here--that the helpful part of the metaphysics can not be filtered into these components; or, that if it can, the "unacceptable" parts can be seen to be as important as the more recognized parts. Harvey's belief that a closed circle was an appropriate symbol for everlasting life is an example. This position in its turn is not to be understood as the view that a confused metaphysics was helpful because it was confused or that confused views are preferable to unconfused ones.⁶⁰ The claim is only that the metaphysics was in fact both helpful and confused at the same time.

Forthly, it is not easy to argue that any author is confused. The question may be said to break into two separable issues. First, how is a confusion manifest in an author's work? And, secondly, how can one definitely attribute it to him?

Both these questions are significant; neither has been discussed in the literature to my knowledge. That is puzzling since commentators have on occasion blamed writers for their own confusions; this makes it clear that there is a problem to be discussed; recognizing and properly attributing confusion is not a straightforward task.

In many cases, a mistaken attribution of reviewers' confusion to the reviewed author is easy to notice. For example, when a writer introduces a new concept, the reader may confuse it with the one it replaces. This is not to say that authors do not sometimes do the same. Harvey, for example, in my view regularly confuses the concept of function in physiology with the older concept of purposeful function. A more well known but unstudied example is that of Harvey's contemporary Galileo, who, in a famous passage reverts from his own mechanics to Aristotelian mechanics. Of course, Galileo does not shift into that framework securely, but only for a moment; this must be so otherwise one would not be talking about a confusion but a reconversion. The reverse of the example is also quite common; thinkers will capture for a moment an idea that is only developed fully later. If the later theory is taken as their context, they will seem confused. Historians who read Newton into Galileo can not but find him confused and confusing. Historians who read mechanism of the late 1600's into Harvey at the begin-

ning of the 1600's could not help finding him confused and difficult to report.

One could credit a reviewer with complete clarity on the thoughts of the thinker he is reviewing. (No one is ever in such a position.) This would enable one to leave aside the question of how much the reviewer is confused and reading his own confusion into the thoughts of the thinker at hand. Given these favorable circumstances, the task of indicating the confusions in a thinker's world view is more straightforward. Yet, even so it is not clear what needs to be done. It will not do, for example, to show that Harvey had momentary confusions like Galileo's. For if his metaphysics was confused, his difficulties would have to be more chronic. If the difficulties in a writer's thought can be seen to be deep, the ascription of them to a metaphysical difficulty is less likely to be unfair. In the summary of Harvey's metaphysics and its confusions below, the chronic nature of the difficulties he has is a strong feature.

What follows still could ~~be~~ mistakenly in my view--be taken to be a confused description of a metaphysics; I present it as a precise literal description of a confused metaphysics--with, I emphasize, admiration.

(b) Harvey's metaphysics and its concomitant confusions.

Harvey's metaphysics was in the main a variant of Aristotle's. Harvey understood the world to be a hierarchy. The laws of nature are such that they reflect this hierarchy, keeping all things in appropriate relations to one another. In fact, one could say that the establishment and maintenance

of this hierarchy is a reason why things behave as they do. One principle associated with the hierarchical arrangement is that important features of the system must occupy central places in it and govern the subordinate parts. Another principle of the metaphysics is that the circle is the most perfect form, related as it is to eternity and unchanging quality. This form suits and preserves motion. All essential things have existed indeed in perpetuity. Living things are dynamic and develop to reach their perfection in the hierarchy. The metaphysics included the generalized microcosm-macrocosm theory as a central feature; this theory is the view that the greater and the lesser worlds have intimate and powerful connections; thus principles and processes which apply to one have some association with the other. In all these processes and by these principles and connection, nature operates always in the simplest and most efficient way.

So much for a brief description of what is presented here as a confused metaphysics; parts, of course, are omitted and parts interpolated. But, let me go on to list the more important confusions it incorporates.⁶¹

In this metaphysics, there are confusions on the nature of the vital substance: should it be self-moving or not? Does it gain its special character from its source--or from its position in the hierarchy when fully developed; e.g., is it important and enlightening to know that the blood is the first thing in the fetus to develop sufficiently to move; or is the blood's vital quality attested and defined by its effect through the body; Harvey says both.⁶²

A related confusion--and one of the most central--concerns the move-

ment of the heart itself. The confusion concerns whether or not the movement of the heart is sufficiently explained by its function of moving the blood through the body or if some different, causal, explanation is required. Since Harvey made a special point of insisting on the heart's active role in contracting to send the blood on its route--as opposed to a passive expanding caused by the blood filling it--he made this difficulty of how the heart moves prominent.

More to the point here, Harvey gives chronically different judgements on whether the question is worth looking into or not; his metaphysics says both that explanation in terms of cause is necessary and sufficient and that explanation in terms of purpose has that status.⁶³ Moreover, one can add that "purpose" in Harvey's thinking regularly shifts between physiological function and proper Aristotelian final cause. When Pagel emphasizes that Harvey's concern over the "final cause" explanation of the valves in the veins led to his discovery of circulation, he means Harvey's curiosity about "purpose" in the sense of "function". Harvey meant sometimes that *and* sometimes a kind of metaphysical legitimacy.⁶⁴ But when Harvey emphasizes the explanation of the heart motion or of the circulation itself as "preserving life perpetually", one can not be sure if this means function or final cause. Both senses play important and continuing roles in his thinking. There are many changes that can be rung on this central confusion. When Harvey speaks of the blood developing in the fetus, he argues that it is clearly the source of life since it is the first thing to move--before the heart. This indicates that living things can be self-moving; yet, the most famous parts of

his De Motu concern his careful demonstration that the heart positively pushes the blood around and that this accounts completely for the pulse and the bloods movement and explains why the heart moves.

Harvey's vitalism meshed with the belief that the blood must travel in a circle--since that was the only perpetual motion, which thereby was preserving. But, the metaphysics on this point did not--as critics pointed out--mesh well with one standard interpretation of the view that nature does things simply and most efficiently. Why, said the critics (following Harvey's own metaphysics), is the blood perpetually moving--what could be the function of such endless motion of the blood around and around? In the old Galenic system, the blood is used up in the limbs for nutriment; Harvey's theory *can* not stress ^{these} features, in fact, nearly leaves no room for them.⁶⁵

The microcosm-macrocosm theory had several different meanings in Harvey's time and Harvey can be seen to alternate between various interpretations. The theory could mean that the lower world--beneath the moon--had parallels to the upper celestial world. It could mean that the human body was the microcosm which was organized like the larger earthly world; it had cycles and processes which were similar. It is important to note that the parallels could reach to the celestial world in many cases but are limited to the sublunar corrupted world in others. The microcosm-macrocosm theory could also be Kaballistically interpreted to mean not that the body was a small world but that the world was a large body; this interpretation has more in common than some others with vitalistic orientations and animistic beliefs. Harvey acknowledges views associated with several of these interpretations although they are incompatible when elaborated in any

unconfused manner. For example, the Aristotelian microcosm-macrocosm theory was associated with the view that the lower world including the body and its senses were corrupt and that no continuity of natural process bridged the gap between them and the perfect celestial realm. The Kaballistic microcosm-macrocosm theory of the world as a larger body had the opposite connotation that through the body one could have reliable information and power over the world and that the body could be perfected. The microcosm-macrocosm theory puts man on the same level as God and the perfect heaven.⁶⁶ In Harvey's comments, not only can one find allusions which suggest each of the above positions but it seems clear that the idea of relating the microcosm and the macrocosm was in part taken by him as a theory and in part as an analogy or symbol. Partly, that is, it was an actual statement concerning the arrangement of the world; in part, it was a poetic description which gave one the proper appreciation of the excellence of arrangements but did not describe them.⁶⁷

In the category of metaphysically related views,--on the possibility of knowledge, on the aim of science (research into natural philosophy), the relation of science and personal character--Harvey expresses characteristic ambivalences of his day. He alternately claimed the power and reliability of the human senses, alluding to the great enlightenment and progress that could be expected from research and then claimed that the senses were frail and liable to error. His emphasis on unadorned factual reports--the fact of circulation as opposed to its explanation in terms of purpose,--means alternately a hopeful view of knowledge and a resigned one. Occasionally, he

spoke of his science as a devotional duty rather than the expression of any hope of important knowledge. Harvey chronically praises freedom of thought and the importance of expressing views as false (Curtis) while he is curt with his critics and views their arguments as ad hominem cavils. ⁶⁸

These odd, ethical-cum-methodological views are often not taken as part of a metaphysics; yet they were assuredly part of the neo-platonic-cum-Kaballistic light mysticism which flourished in the late Renaissance; since many of Harvey's objective metaphysical views are part of this tradition, it is reasonable to take these fringe views as part of his metaphysics as well.

The metaphysical confusions which have more direct association with methodological issues are easiest to canvass for confusions since Harvey speaks explicitly on such points quite often. They have all appeared already; but since they are important, they can be summarized here. Harvey is confused on final cause as an explanatory category--whether it can be taken to be equivalent to purpose, to physiological function, to fitting a fact or theory into a broader framework. Likewise, Harvey did not know how to use his vitalism in explanation if at all. He emphasizes the mechanical--in the sense of positive active--actions of the heart; but the explanation is not mechanical or causal in fact; the heart pumps in order to move the blood. Later, the blood is primary since it is the first to exist and to move before the heart; so it causes or initiates the heart beat. The mixup and concern to connect motion with priorities in metaphysical, helpful, but also quite confused and difficult.

Finally, whether science demanded ocular demonstration, reasoning

based on efficient cause, facts, or interpretations according to metaphysical views: on all these points Harvey finds himself in a metaphysical jungle; the points of confusion have strong associations in the mixture of traditional Aristotelianism with more extravagant neo-platonic Renaissance theories which were part of his background and deeply established in his thought.

FOOTNOTES

NB. Full references appear only once in each chapter; a list of abbreviations precedes the Bibliography.

1. Witteridge, William Harvey and the Circulation of the Blood, (London: Macdonald, 1971), p. 114. See also her preface in which she claims on one page that she puts forward no theory of science but only tells the story as it happens and on the next that clearly Harvey's theory arose from the facts. Also, p. 196 in the same work for antagonism to working hypotheses. Witteridge complained that Walter Pagel distorted Harvey's science by his emphasis on Harvey's ideas in her review of Pagel's book in the Journal for the History of Science, Vol. IV, 1968-69, pp. 68-69. In that review she quotes Harvey saying that it was by an examination of the valves that the theory arose; Pagel has responded, by showing that an examination of the Latin text indicates that a phrase has been left out by the translator--a phrase to the effect that Harvey undertook the series of experiments "after having the idea that the blood might travel as it were in a circle"; see Pagel, "William Harvey Revisted", History of Science, Vols. VIII IX, 1970, pp. 3-5.
2. Kenneth Franklin, Introduction, in William Harvey, Circulation of the Blood, and other writings, Everyman's Library, (London: J. M. Dant and Sons, 1963), p. ix. Franklin notes that there is evidence that Harvey's research on the generation of animals was underway when he studied with Fabricius--that is, at just the time when the theory of circulation was occupying his mind, not later.

It will be almost impossible to elaborate sufficiently in the text on the ambivalent and confused reactions to De Generatione. I can say here that commentators often want to congratulate Harvey on the methodological principles he puts forward at some length in that book while apologizing that it contains no lasting contribution made with their help. The apology often includes the note that good microscopes were not available; but just as often, and occasionally in the same work, the commentator notes that Harvey showed himself a careful and accurate observer of details in the work even though no new theoretical advances came out of it. So, one does not know if he was inhibited by technical barriers to observation or not. See Louis Chauvois' embarrassingly congratulatory treatment of this work in, William Harvey: His Life and Times: His Discoveries: His Methods. (London: Hutchinson Medical Publications, 1957). And notes to treatments by Keynes below; also the flatly anti-metaphysical and contradictory attempt to deal with De Generatione by W. P. D. Wightman, Growth of Scientific Ideas, (New Haven: Yale University Press, 1953). pp. 354-355. Wightman's book is less traditional than some as its title indicates.

- 3 Witteridge, WHCB, p. 157. Although they are harsh, I think these criticisms of Witteridge's study are literally true exactly as written and her reports of Harvey's intellectual activities are just as inadequate as I say and suggest they are. She is only a scholar, not a thinker; unfortunately she is righteously proud of that fact; she says so in so many words in the preface to her book; and her disdain of her colleagues--e.g. Pagel--reflects it. For example, she "reports" many incidents (e.g. the timetable of the writing of DeMotu) on which she and Pagel directly conflict as fact; she never reports Pagel's differing opinion; she only refers to the "sources". That, of course, is quite irrelevant since Pagel quotes the same sources and the question is clearly one of interpretation and argument about interpretation. She is an inductivist--an uncritical inductivist--who thinks reporting the facts is enough so she insists on reporting Harvey reporting the facts; his debates with his colleagues are extrascientific just as her conflict with Pagel is, I suppose, part of the flotsam and jetsam of research.

In his unusually quite reasonable work, The Origins of Modern Science, 1300-1800, (New York: Free Press 1965), Herbert Butterfield does not even mention the existence of Harvey's critics nor does he use the word "metaphysics" in his very favorable chapter on Harvey. This is significant because Butterfield does not ignore the broad framework of Harvey's ideas but discusses it extensively. Arturo Castiglione, one of the best general commentators in history of medicine, only reports the existence of the critics with no indication that they were of any use in the progress of the field. The point is that one cannot put forward a reasonable view of the value of their criticism without due attention to Harvey's metaphysical arguments and rationale. Castiglione, A History of Medicine, ed. and trans. by E. B. Krumbhaar, (New York: Jason Aaronson, Inc., 1969, originally published, 1927), pp. 518-520. But, see notes below for more favorable comments on Castiglione's tolerance for metaphysics. Gillispie joins Keynes in seeing Harvey as fully justified in snubbing critics: "Occasionally Harvey betrays the authentic intolerance of science for stupid error", The Edge of Objectivity, p. 70. And, D'Arcy Power sees a great difference between Harvey's replies to his critics and the original work on circulation and clearly little value in the former; William Harvey, (London: L. Fisher Unwin, 1897), p. 224.

- 4 Butterfield, p. 65. He implies that none of these beliefs has anything to do with the De Motu which he has just described. D'Arcy Power does nearly the same thing in describing first Harvey's theory of circulation and then saying, "yet, he was often hampered by the theories of the ancient philosophic schools of medicine", (p. 186). One such theory is that blood is life and should be preserved. Wightman, GSI, p. 344, where he ridicules Aristotelianism as victimizing Harvey after just describing how the theories helped Harvey's interpretations.
- 5 Harvey, De Motu, in the Franklin edition cited in note 3 above, p. 87.
- 6 The metaphysical views associated with the blood as life and with life as preserved by perpetual circular motion are some of the most interesting and subterranean--easy to miss--involved in Harvey's work, D. P. Walker describes the significance of the revival of Pythagorean ideas of reincarnation as part of this metaphysics of cycles which was extremely popular in Harvey's day: "Eternity and the Afterlife", Journal of the Warburg and Courtauld Institutes, Vol. XXVII, pp. 241-250. Also extensively discussed in Walter Pagel's excellent study, William Harvey's Biological Ideas, (Basel, New York, S. Karger, 1967).
- 7 Gillispie, p. 60.
- 8 Sir Henry Dale, "Harvey and the Scientific Method", British Medical Journal, Vol. II. 1950, p. 1406; cited in Pagel, WHBI, p. 25).

Curiously, there are completely opposing opinions on the question of demonstration or method of discovery. For example, compare with Dale's view the following passages from Gillispie and Wightman on what makes Harvey's work significant:

"Harvey was the first to use observation and experiment to find out something fundamental instead of to demonstrate it." (EO, p. 68)

And Wightman seems to tend to this opinion of Harvey's merit as well"

"We are not concerned...so much with the actual circulation of the blood, ...as with the method by which it was arrived at." (GSI, p. 340)

Thus, there is clearly a great deal of confusion in the literature on the question. Whereas, G. K. Plochman, in "William Harvey and His Methods", Studies in the Renaissance, Vol. X, 1963, p. 192, mentions Harvey's "great demonstration" which "is the result of the application of certain principles, logical and metaphysical"; but Plochman considers that "paradoxically, these principles very early found their completest and most subtle formulations in the works of Aristotle himself." (p. 193). Plochman thinks the discovery was easy and could have been done by anyone looking through a microscope (p. 193-194) and that Harvey's achievement was methodological; whether it was beyond Aristotle's method declared complete above, or not, is unclear.

- 9 Sir Henry Dale, "One Some Epochs of Medical Research", The Harvian Oration, (London, Royal Society of Medicine, 1935), p. 6. The article is widely quoted. One can add many hopeful reference to a "proof" by D'Arcy Power, pp. 206, 208, 221. Even Castiglione who gives credit of merit to De Generatione although nothing in it was proven, says confusingly, that:

"Harvey's greatest merit, which makes him one of the greatest figures in the history of science is that of having proved the various steps of the problem by a series of inductively arranged physical demonstrations." (p. 518)

Castiglione then quotes Garrison saying (mistakenly) that Harvey's presentation is the first "mathematical proof and exact calculation used in biology"; (p. 518). For more on mathematics as important here see Section 3.2.(b) below.

- 10 For Harvey's devotion to Aristotle as a superficial accretion given the fact that his proofs are what is important, see Henry Sigerist, "William Harvey's Thought" Archive fur Kulturgeschick, Vol. XIX, 1929, pp. 158-168. Pagel criticizes this view in his WHBI, pp. 24-25. Sigerist, however, joins some bold thinkers in noticing that Harvey dismissed some factual evidence and even claims this was part of Harvey's greatness; see his On the History of Medicine, (New York, M.D. Publications, 1960), p. 189.
- 11 Gillispie, EO, p. 73; Witteridge, WHCB, pp. 168, 234-235. She indicates that no debate is reasonably part of science, and one should never go into questions which can not be resolved more or less immediately. She goes so far as to congratulate Harvey when he (uncharacteristically) ignores the question of the purpose of circulation and the cause of the heart beat. It is important to notice that not only can Harvey be shown to have wanted to answer these questions but that his consideration of them had physiological import closely supported by their metaphysical significance; thus, when Harvey ignored them he not only deserted his more characteristic metaphysical orientation but physiological fruitfulness. This is a major point in this review.
Wightman, GSI, pp. 342, 340 respectively. Also see Charles Singer's extensive and repeated congratulation of Harvey for his "restraint" in a Short History of Anatomy and Physiology from the Greeks to Harvey, (New York: Dover, 1957), p. 175.
- 12 Singer, p. 175; Butterfield, p. 64.
- 13 J. G. Curtis, Harvey's Views on the Use of the Circulation of the Blood, (New York: Columbia University Press, 1915). An extremely important and overlooked work which discusses Harvey's broad intellectual framework; see "Physicians vs. Philosophers" for a discussion of the fact that if Harvey's major contributions were an empirical method of proof, he would not have quarreled so stiffly with the Galenists of his time who were experimentalists as well.
- 14 Singer, pp. 175, 177-178. Also, Singer prefaces his work, then immediately forgets the comments, by saying that Harvey changed the history of biology by "thinking physiologically", (p. viii) which has little to do with restraint and proof.
- 15 Butterfield, p. 57. See also pp. 61-62. "What is very remarkable in him is the comprehensive and systematic character of his investigations as a whole."
- 16 Singer, p. 175.

- 17 Geoffrey Keynes, The Life of William Harvey, (Oxford: The Clarendon Press, 1966), I elaborate this view in my, "William Harvey and the Life of Science", Akten Des II Internationalen Leibniz-Kongresses, 1972, B and I, (Weisbaden: Franz Steiner Verlag, 1973), pp. 279-291.
- 18
- 19 Merton can not be said to distinguish these theories sharply especially since he rather agrees with the doctrine of error: that all error is prejudice. His famous study, Science Technology and Society in Seventeenth Century England, (New York: Harper & Row, 1970) involves only the claim that the scientific spirit and the puritan spirit are nearly equivalent; he does not notice that they are both offshoots of earlier Kaballistic trends nor that they are false. Agassi has commented on these points in his Towards an Historiography of Science, (The Hague: Mouton and Co., or Wellesley University Press, 1964); also his "Unity and Diversity in Science", BSPS, Vol. IV, pp. 463-522; see pp. 488, 513.
- 20 Keynes, pp. 185-186, 332.
- 21 Keynes, p. 332; For the willingness to see Harvey as self-occupied and somewhat nasty, see the citation from Sir Charles Scarborough which Keynes is ready to put into Harvey's mouth. The view of science expressed here is saturated with typical Medievalism (Kaballism in this case):

"It is not weighty enough [the business of answering critics] for me to trouble the Republic of Letters again on my own behalf. I shall be neither Founder nor Advocate of a new doctrine which can be disputed either way. Perish my thoughts if they are empty and my experiments if they are wrong--if I have observed them less than accurately, I am, anyway, satisfied with my diligence. I am not a disturber of human affairs; but if I am wrong (for I am human) let my writings lie neglected. If I am right, sometimes, in the end, the human race will not disdain the truth."

Taken from the Harvian Oration, 1662, cited in Keynes, p. 322

- 22 Keynes, p. 322.
- 23 Lindholm, pp. 284-288.
- 24 Several earlier commentators recognize that Harvey's character was not the most sublime, but they do not know what to make of it, if anything. See, for example, D'Arcy Power who speaks of Harvey's sourness and weariness, p. 185; and p. 146, where he quotes Ent's view of Harvey's weariness of spirit associated with these responses. None of this is remarked on by Keynes nor by any of the surveying historians I have noted; they praise Harvey's patience universally. Kenneth Keele, cited below, is the only specialist commentator on Harvey who sees this reaction as an expression of ambivalence and as out of place.
- 25 Kenneth Keele, William Harvey--The Man, the Physician, and the Scientist, (London: Thomas Nelson and Sons, 1965), p. 149.
- 26 Ibid., p. 99.
- 27 Ibid., p. 144.
- 28 Ibid., p. 55.
- 29 Ibid., p. 249
- 30 Ibid., p. 250. Keele mentioning prominently Harvey's evasions on circulation and lyrical notes on symbolism.
- 31 Ibid., p. 28.
- 32 Ibid., p. 56. In this place, Keele also discusses Harvey's view that one can see Jove in all things--an expression of his vitalism.
- 33 Ibid., p. 33.
- 34 Ibid., p. 216.
- 35 Ibid., p. 216.
- 36 Ibid., pp. 55-57.
- 37 Ibid., p. 217.
- 38 Ibid., pp. 191-196.

- 39 Lynn Lindholm, "Has Harvey's World View Been Rescued from the 'Lumber Room of the History of Human Error'", and essay review of Pagel's WHBI, Proc. of the XIII Intl. Congress in the History of Medicine, (London: 1972) pp. 1099-1104. I summarize Pagel's merits and defects vis a vis metaphysics and confused metaphysics there.
- 40 Walter Pagel and Pyrali Rattansi, "Vesalius and Paracelsus", Medical History, 1964, pp. 309-328.; and, "Harvey meets the Hippocrates of Prague", Med. Hist. Vol. VIII, 1964, pp. 78-84.
- 41 Pagel, "The Philosophy of Circles", Journal of the History of Medicine, XII, 1957, pp. 140-157. Keynes quotes the sentence cited on the death of Harvey the Aristotelian with a comment that Pagel is a "keen observer of Harvey's philosophy"--meaning not of his science (p. 430). This is the only notice he takes of Pagel's work--Witteridge does not mention it at all. The work refutes most of the conclusions each present on Harvey.
- 42 Pagel, WHBI, pp. 109-110.
- 43 Lindholm, p. 43.
- 44 Pagel, WHBI, p. 73.
- 45 Ibid., pp. 75, 77.
- 46 Ibid., p. 73.
- 47 Ibid., p. 73.
- 48 As do other noted in Section 3.1.(b); this was an important point of Harvey's scientific merit. Pagel has several articles on the importance of purpose and of circular symbolism to Harvey: "William Harvey and the Purpose of Circulation", ISIS, XLII, 1951, pp. 22-38. and his "William Harvey Revisited", In WHBI he discusses the importance of purpose repeatedly and Keele mentions it also.
- 49 Pagel, WHBI, p. 78, Lindholm. p. 443.
- 50 Pagel, p. 80.
- 51 Passmore, "William Harvey and the Philosophy of Science", Australasian Journal for the Philosophy of Science, Vol. 36, No. 2. August, 1958.
- 52 Ibid., pp. 88, 91, 94.

53 *Ibid.*, pp. 91-93.

54 Alex Michalos, The Popper-Carnap Controversy. (The Hague: Martinus Nijhoff, 1971, pp. 104-112.

One can ask, was Harvey really a vitalist? After all, Descartes believed in spirits too and wasn't a vitalist. Descartes' position is not clear to me; spirits which operated mechanically wouldn't be "spirits" in any traditional sense. Regardless of all that, Harvey had no physics which demanded action by contact; he in fact leaned toward thinking the heart--or the blood--should be entirely self-moving since it was "vital"; the essence of living things, according to him, was that they were self-moving and therefore were not moved solely by mechanical action; they were prime movers in a sense. Harvey talked about the heart as a pump--pushing the blood along--of course; and this is why he is called by some commentators a mechanist. But, when he is discussing this, he also says that there is no explanation of why or how the heart moves; it is the source of movement (and of heat) as the sun is for the solar system.

Commentators try to say Harvey declared the motion of the heart "unexplained" rather than "inexplicable"; but this is not at all the force of his comment - the meaning in context is plainly that a different, non-mechanical principle of action is involved. Later in his research when he discovered that the blood was the first thing formed in the fetus--before the heart--and that it moved (pulsed) before the heart was formed, he called it the source and essence of life because it was the first self-moving thing. (He was mistaken, by the way; the drop of pulsing liquid he observed in a chick embryo was encased in a pericardial sac and therefore was the predecessor of the heart; but he could not see that.) His vitalism consists among other things in the view that life is characterized by being self-moving, not mechanically moving. The "inexplicable" above means inexplicable by mechanical means.

55 *Ibid.*, p. 106, for the criteria: pp. 110-111, for Harvey's merits.

56 *Ibid.*, p. 108.

57 *Ibid.*, pp. 107-108, 112.

58 In any claim about Harvey's own metaphysics, the state of the governing metaphysical climate would be a central factor; yet, arguments about the state of that climate are far too comprehensive to go into. Moreover, they happen to involve a range of expertise in historical sources of classical and of non-European varieties which make even professional

Renaissance scholars quail.

Thus, of necessity, some results of the scholars mentioned in the text are used but not defended as claims in the thesis in spite of the fact that they are the subject of some controversy among experts. For example, the Kaballistic-neo-platonic tradition are prime sources of potent but strange and easily confused ideas; it must be accepted that they were an active part of the atmosphere. I sketchily survey Yates and the several authors below to show that this is not an outlandish claim in the context at hand even though it cannot be argued here.

I use the body of literature on the neo-platonic, Kaballistic, light mystic side of late Renaissance metaphysics as a background which makes the claim that Harvey's metaphysics was confused more reasonable than it otherwise would be and to help reconstruct the metaphysics itself. One need not accept *any* more general conclusion than the fact that such influences were far more potent than scholars had realized until recently.

More specifically, as a background to Harvey Gershom Scholem's work is used to reconstruct the Kaballistic blend of moral and cosmological and epistemological views which are associated with the microcosm-macrocosm theory. His major works concern that Kaballa only, not science nor the late Renaissance; the central one in this context is, On the Kaballa and its Symbolism, (London: Routledge and Kegan, Paul, 1965).

Yates describes such positions, in a corrupted and elaborated Renaissance form entered the broader European tradition via Pico, Dee, Copernicus, Fludd, Bruno. Thus, she describes how some thinkers associated with the scientific tradition were affected by these more obscure traditions. And, here arguments can be seen to be applicable to Harvey by association at least; that is, he knew some of the same people she discusses directly, believed many of the same objective views, etc.

Her central works in this area are Giordano Bruno and the Hermetic Tradition, (London, Routledge and Kegan Paul, 1964). Also, see her review of Charles Webster's book in the New York Review Vol. XXIII, No. 9, pp. 27-29, in which she argues that there is a strong suggestion of a "current of Cabalistic mysticism in both the German and English [Puritan-scientific] movements.

Several other scholars have now noted the similarity between Renaissance scientific philosophies and attitudes and Kaballistic ones. Agassi comments in this regard are referred to in n. 18 above, he records the con-

nection between the moral stance of Kaballa--with its confusions--and that of the Baconian theory of evidence, of the necessity for accidental discovery and the theory of error; esp. THS, Section 16 and notes to that section.

Max Jammer also notes on association between Kaballistic philosophy and science in the Concept of Space (Cambridge: Harvard University Press, 1958.) He claims that the concept of space adopted by Locke and Newton can be traced back through the mystic views of thinkers like Campanella, Paracelsus, and Agrippa, (p. 320). The development of the modern idea is presented as a purification of the tradition; Jammer noticeably distinguishes the Kaballistic philosophy--a mixture of theology, ethics, cosmology, epistemology (p. 25) of the kind mentioned in the text below--from "metaphysics" (p. 39) which the modern theory of absolute space exemplifies. Jammer is hesitant to claim any actual Kaballistic influence on any particular thinker but says it is clear that "certain general ideas of Kaballistic origin have been absorbed into the intellectual climate of that period." (p. 48).

But, ^{use} Harvey did ^{use} light-mystic metaphysics explicitly; and there are also pro-platonic elements in his views. He makes light-mystical statements in De Motu in Chapter VIII as well as at the end of the book in wrapping it up; and in De Generatione in discussing the blood.

One needs also to note that Harvey's metaphysical picture was not as confused as that of several of his predecessors--e.g., Michael Servetus, e.g. was an out and out mystic who also believed in circulation although he did not spell out the theory experimentally. And, the Kaballistically oriented metaphysics of the late Renaissance was much more muddled and involved in its usual expressions than anything Harvey can be credited with even in reconstruction. The point is that there was nothing better around, thus Harvey's own views reflect some of the same confusions of the available metaphysics. If he didn't accept all the confusion it was because he had to work at it--not because the metaphysics was clear in the first place.

- 59 In the preface to OKS, Scholem labels the Kaballa an oral tradition; many scholars who have attempted to describe the views it comprises or their influence have had trouble dealing with the vagueness that introduces. Yet, it cannot be dispelled without dispelling some element of what made the tradition influential; the alternate changes that can be rung on the microcosm-macrocosm theory of the body were very attractive.
- 60 The explanation that a confused view need not be helpful in its confusion is taken up again in Chapter IV; the ambivalence of meaning mentioned in the last note need not be the source of the importance of the view v even though it is a characteristic of it.

- 61 There are different kinds of confusion: one may confuse Tom's opinion with Dick's opinion; and one may confuse Tom's concepts with Dick's concepts. Each of these confusions leads to inconsistencies; but, not all inconsistencies are confusions. Moreover, the inconsistencies are not logical inconsistencies; e.g., when one has the hypothesis that Tim is the same person as Dick added to a text confusing Tom and Dick, the confusion vanishes. Therefore, it is of the essence when discussing the confusion of an author concerning Tom and Dick to indicate that he at time speaks of them as distinct and at times as identical.

That is to say, if an author confuses Tom and Dick the confusion must stand regardless of the question, are Tom and Dick distinct or identical?

The same should hold when applicable, to Tom's and Dick's concepts and/or opinions.

I use these points in the deliniation of Harvey's metaphysical confusions and the related difficulties which indicate them.

- 62 William Harvey, *The Works of William Harvey*, M. D. trans. by Robert Willis, (London, Sydenham Society, 1947), p. 510. The discussion on this point occurs mainly in the late work, *De Generatione*; see the discussion in Pagel. pp 220-224; 264-265; 332.
- 63 Harvey, *WWH*, the introduction to *De Generatione* contains Harvey's epistemological and methodological discussions; some also occur in *De Motu*. See *WWH*, p. 154ff. Also, the excellent review by Pagel on purpose and final cause even though he does not see difficulties here and interprets final cause as function; *WHBI*, pp. 25, 34ff. The exchanges between Harvey and Riolan on the purpose of circulation discussed earlier in this chapter point up the significant questions here. *WWH*, p. 137; Pagel, *WHBI*, p. 209.
- 64 Pagel, *WHBI*, p. 209, n. 1; also p. 210. Harvey, *WWH*, pp. 113-115; *De Motu*, Section XIII and letter exchanges with Hofmann in the Franklin edition

I certainly do not think that Harvey's uncertainties concerning teleological, purposive, explanation has been replaced by present day certainties. What I do claim is that his uncertainties involve the metaphysics of his time; that the ambivalence between final cause and purpose which he struggled with was in large part a struggle with a confused metaphysics. His work was in part a clarification of that metaphysics; in part a struggle to find the truth within that metaphysics, in part a criticism and rejection of the metaphysics he inherited. None of this can be discussed by commentators who either ignore, defined, or clarify his metaphysics before discussing its function in his work. That is the distinct

claim I want to make; and, I think it is clear enough to have considerable interest.

- 65 Harvey stresses the unity of the blood to the extent that the change in color of the blood going to and coming from the lungs was hard for him to explain; in fact, he denied its significance. See Pagel, WHBI, p. 131; he gives the references there to Curtis' important discussion of the gap. For Harvey on the efficiency of nature, see his exchange with Hofmann and Riolan, and Pagel, WHBI, p. 51ff.
- 66 For the Kaballistic interpretation of the world as the body of God, see Scholem, OKS; Yates points out the importance of the microcosm-macrocosm theory in this interpretation as a source of optimism on the power of man. I have discussed Harvey's ambivalent attitudes toward the pretensions of research and the aims of science in my "WHLS".
- 67 Pagel, WHBI, pp. 89ff.

CHAPTER IV. PHILOSOPHIC ISSUES BEHIND THE ASSUMPTION OF CLARITY

Chapter IV: Philosophic Issues Behind the Assumption of Clarity

The assumption of clarity--which concerns the character of the metaphysics important in the development of science--may appear to affect only historical descriptions. Historical accounts, however, often support particular philosophies of science even if only by not clearly conflicting with their claims. The Burt-Koyré accounts leave room for the assumption of clarity and thereby support many traditional views of science; when the assumption of clarity is explicitly challenged, a relation between metaphysics and science becomes much more clearly a difficult problem for a philosophy of science. It is not easy to explain how a confused metaphysics can be influential in a rational science. The possibility highlights the conjectural status of scientific views to a great degree, and at the same time challenges the adequacy of defining scientific accounts in terms of (deductive) explanation. In short, many theories of science which seem to accommodate a connection between metaphysics and science as described by Burt and Koyré can not do so when the assumption of clarity is not allowed to operate in the background. Most theories of science which address themselves at all to the problems of the relation between science and metaphysics depend on the assumption of clarity or the connection they propose is not a rational one: they surrender rationality with the assumption of clarity.

After a review of this situation, a combination of Popper's general philosophy of science corrected and supplemented by Agassi and Bartley is put forward as a framework for one satisfactory explanation of the role of metaphysics in a rational science. In this view, metaphysical theories are

said to be important in the rational criticism of scientific views. Researchers often select scientific problems which relate to metaphysical debates; they often try to develop scientific theories which can be interpreted according to one metaphysical view rather than another. This problem selecting is essential in itself for rational development of science in that it is the alternative to a division of intellectual labor which would inhibit criticism. The framework of content which the metaphysical issue provides also is essential for the rationality of the process; it provides a (tentative) selection device for criticism. Moreover, the scientific theories developed as interpretations of particular metaphysical views can be tested thereby providing indirect tests and challenges to the metaphysics.

Thus, science is part of the rational debate on metaphysics as well as metaphysics an important part of the rational development is preserved in spite of the influence of metaphysical views because the metaphysics provides only an incomplete framework for any science and because the metaphysical views which compete with each other and with established frameworks can be developed and used as described. Thus, although extremely difficult to refute, metaphysical views do not dictate scientific views but contribute to a critical discussion with them on scientific issues.

Confused metaphysical views and systems can fulfill all the above functions, although less efficiently and in ways more dangerous to rationality. Yet, guidelines and problem situations can be described which make their use critical and goal-oriented; thereby they can also be part of the rational development of science. This is particularly the case when

their rational cultivation allows important problems to remain on the agenda of science.

Section 4.1. A Bias in the Philosophy of Science Against a Recognition
of Confused Views--the Long Tradition.

Since the beginning of science in the Renaissance, clarity has been assumed to be a characteristic of all acceptable views; there are many reasons for this not the least of which is the further assumption that the truth on any matter will be clear and simple. Also, obscurity and occultism were connected with lack of clarity. When a clear position on any matter was thought to be easily attained, commentators had little reason to note that confusion did not necessarily imply obscurantism. To defend the view that a consideration of confused metaphysics has a rational role in science, these distinctions must be noted.

(a) Classic Modern Philosophers on Clarity and Confusion in Science
Renaissance thinkers in the neo-platonic tradition valued simplicity; Pico, for example, held as a matter of faith the view that truth was unified and easy to understand, if not easy to achieve. Pico's view is not part of a coherent philosophy on such matters; he was an eclectic and grasped for enlightenment from any direction on the theory that all views have some truth and would harmonize.¹ The belief in the unity of truth in itself is somewhat mystical and encouraged wild analogies and strained connections.

Simplicity and comprehensibility do not subsume clarity although it is easy to fuse them looking back. The mystical overtones to Pico's theory of truth were not securely combined with a standard of clarity; a simple truth could be understood easily as a shining revelation despite the fact that it

was not what one would call clear.

Galileo recognized that simplicity and comprehensibility could not be combined with mystic syntheses of views.² His decisive stance against obscurantism, against the intellectual dishonesty of implicit changes of meanings in arguments, and against vague and promisory claims which prevent criticism of theory,³ is well known. It can be interpreted^r as elaboration of a more strict and precise standard of clarity (although other definitions of clarity are clearly possible). And, Galileo's Archimedian-Platonic faith that the language of science was mathematics brings with it a commitment to clarity which was a departure in the tradition.

To go into this more deeply, Galileo understood the truths of nature--mathematically-set truth--to be far more clear than those of common sense although the latter are more intuitive. For him, that is, mathematics did subsume clarity although both previous and successive thinkers disagreed.⁴ Clarity can be read here to be absence of confusion of meaning or reference rather than "obvious to common sense"^{or,} as in Pico, "relevatory".

One can see the strength of Galileo's connection of mathematization and clarity of views when one remembers that simplicity and mathematization involved the exact formulation and precise application of truths to nature in his view. That is to say, that theories (and truths) were mathematical meant that they referred to exact quantities and precise entities. That nature was mathematical meant that theories about such quantities and entities should apply exactly to the state of affairs.⁵

Galileo did not claim, as Descartes later did, that one could intuit

the truth by clear thinking alone. He emphasized dialogue--including criticism and revision of views--as an essential part of the search for truth. But, one does not know if the dialogue could begin with confused ideas or if the only competitors were clear theories. In the latter case, the search for truth could not be said to have begun until they were provided as hypotheses. A thinker did, after all, have to know mathematics so as to formulate clear theories in the proper terms before he qualified as a philosopher of nature for Galileo. Thus, the search for truth might include the clarification of unclear ideas in Galileo's philosophy, but this alternative is certainly not emphasized. Galileo's tolerance for debate does make it conceivable.

In any review of the demand for clarity in science, Descartes must have pride of place.⁶ One can argue that he demanded clarity for two reasons. First, he held as a fundamental belief the view that all men could achieve and recognize the truth; thus, it must be something that is readily comprehensible and evident. To be readily comprehensible and evident a theory must be clear. Secondly, he shared with many early thinkers on science--particularly Bacon--the view that on argument entered the scientific domain, progress would end. Clarity was necessary in order to communicate the unquestionable truth which would forestall argument; thus it was essential to the existence and progress, and thus of course the rationality, of science.

Furthermore, Descartes method for achieving truth leaves no place for clarifying confused views; one just wiped them out with all prejudices and

unsure opinions through the method of doubt. In the development of science, one began afresh, considering only clear and evident theories.⁷ Descartes leaves no room for any kind of debate or discussion over intellectual difficulties--except of course as extra and anti-scientific activity. This is a characteristic his view shares with all theories which involve a sure method. To point out that he has no place in his view for the intellectual problem of clarifying a confused view is almost misleading since he has no room for the much less controversial activities such as developing a hypothesis, refining a theory, discovering and correcting mistakes. None of this is part of science for Descartes; therefore, that cultivating metaphysics, and confused metaphysics at that, is no part of it for him goes almost without saying.

Not being an apriorist nor a lover of mathematics, one might expect Bacon to make greater openings in his empiricism for views that were not clear and precise. But, Bacon also saw no need to deal with confused ideas because his method for achieving truth was radical, direct, and involved no difficulties. One cleared one's mind of prejudice and observed nature humbly. Any ideas which were not based on fact, much less confused metaphysical ones which could not be truly competing hypotheses, were outlawed. (In exceptional cases, they were considered pre-scientific instruments as considered below.) Like Descartes, Bacon thought error threatened the life of science since it opened the door to dispute; confusion is more culpable in this respect as indicated by Bacon's famous remark, "and yet... truth will come sooner from error than from confusion".⁸ From a philosopher who considers simple error irrational and who is not willing to countenance

it in science, this is a strong condemnation of confusion.

Newton's science brought a significant departure from the tradition on clarity in philosophy of science in that it forced a redefinition of obscurantism. In Galileo's and Descartes' view, clarity is related to the mechanical philosophy and to a rejection of obscurantism. Newton's theory of gravity was a force that acted at a distance and instantaneously was not mechanical. That is enough to make the theory incomprehensible on Descartes' strict standards of clarity. Indeed, on lesser standards, the theory had weaknesses; it was not an explanation which could involve essences. This was proven by the fact that the force varied. For this reason Galileo also would have spurned it as a theory of reality.

However, the theory of gravitation was not ad hoc and was highly explanatory through its generality; it therefore became acceptable. The idea that clarity and thus intellectual honesty came only out of theories which referred to simple mechanical motion and primitive geometrical properties was thereby surrendered and replaced. How the standard of clarity was understood thereafter is not so easy to say. But, it is important to note that the lesson of Newtonianism was absorbed into the practise of theorizing sooner than it was noted in the philosophy of that theorizing.⁹

Although Newton does stretch the limits of what one will accept as a clear scientific theory; he does make room for the cultivation of more "confusing" ideas as possibly scientific. However, given the background of Baconianism with its reliance on verification, there is no question--serious question--of debating and refining these confused views; they are accepted

as is. The definition of clarity has simply changed; the point is that confusion as a problem in science has yet to enter the picture.¹⁰

From Boyle onward, important thinkers of an empiricist orientation have allowed that working hypotheses are essentials in scientific research. William Whewell is perhaps the most notable. Given that they allow these hypotheses, the possibility is opened that they allow other activities as a rational part of the scientific enterprise--e.g., not only the cultivation of possibly false theories (hypotheses) but the clarification of possibly clear views as well. Yet, it seems a clear historical fact that the first of these possibilities has been more explicitly endorsed than the second. Working hypotheses are regularly assumed to be clear--simply not proven.¹¹

Thus, the idea that any theory worth pursuing must be clear can be seen in the background of science until the modern era which begins with Duhem.

(b) Post-Duhemian Philosophers on Clarity and Confusion in Science.

Duhem's views on clarity are not easy to summarize. He was of course an advocate of simplicity with its almost automatic corollary of clarity. Yet, because he surrendered realism and disliked the idea of rejecting theories, he made a point of arguing that theories develop by small refinements. These refinements could be made quite regardless of the availability of a coherent interpretation of the underlying reality. And, they therefore have the character of ad hoc hypotheses in some cases. His legacy vis a vis clarity is therefore obscure.¹²

Nevertheless, it is important for the argument defended here to notice that with Duhem and the crisis in physics a new stage in the approach toward a more favorable consideration of confused views is reached. This is so simply because Duhem realized that there was no sure method for producing truth because there was no sure demarcation between science and metaphysics if both were interpreted as realistic explanations of the world. Both these points--the absence of guaranteed method, the epistemological equivalence of metaphysics and science are extremely important in this debate.

The acknowledgement that there is no method (of discovery or proof) which insures truth requires that other qualities of theories are considered and valued besides those previously (and mistakenly) considered sufficient conditions for the truth of those theories. A plurality of valued qualities of theory, of possible aims for science, of possible rational methodologies given those aims, may be put on the agenda for rational discussion.

Duhem's own philosophy does not provide an advance in the direction of liberal methodology since although he allowed many previously outlawed strategems, he surrendered the search for realistic explanation and forbade speculations on that line. He combined a restrictive methodology--which has little tolerance for confused metaphysics of the type discussed here--with a surrender of realism and rationality. But this criticism does open these doors. Thus, several more modern commentators on science, being post Duhemian (and post-Einsteinian), discuss science with more consciousness that a guaranteed method is unavailable. They approach a toleration of confusion because they must value theories--and discuss them rationally--for other qualities besides

truth if they hope to value and discuss them at all.

The views on confusion of a selected few of these post-Duhemian commentators enter the discussion below. Perceptive readers will notice that two important schools of philosophy are virtually bypassed in this survey: positivist philosophers of the twentieth century and Whiteheadians. The gap which this appears to create in the presentation of the contemporary problem situation and my own new position is, however, more apparent than real. The more influential doctrines of these two schools--on clarity and on the relations between science and metaphysics--occur in the later more sophisticated commentators who will be discussed. These important legacies will be described briefly.

Many twentieth century philosophers followed both Mach and to a lesser extent Duhem in their stark suspicion of any unverified term or theory; neopositivists and some logical empiricist philosophers take Mach's position that no unverified theory or unobservable entity can be allowed as part of the body of science. In fact, twentieth century philosophy of science has been marked by the conflation of the unverifiability of a position with its incomprehensibility, confusion, lack of clarity, and nonsensical character; the two categories were equivalent for the purposes of science. When the weakness of this Machian interpretation were noted--specifically that all theory, as well as all confused, unverified, unclear, and nonsensical theory, is excluded from science on the verifiability criterion--the trend has been to adopt a Duhemian conventional stance. Any theory not directly connected with observation but necessary nevertheless is a heuristic device, an empty categorization mechanism.¹⁴

Only the most liberal of these theorists--liberal toward the proper status of scientific theories--can even enter the debate on a role for metaphysics in science. That is why I have taken Popper's philosophy to represent the entire tradition; his is the most liberal although he shares the positivist concern that all theories rationally considered part of scientific thinking be empirical in some way.

Whitehead, on the other hand, is a philosopher one would immediately consider in discussing relations between philosophy and science, for reasons opposite to those above: he emphasized a connection and tried to supply science with a new metaphysics. In this respect, Whitehead represents a twentieth century continuation of an earlier tradition of attitudes toward science--a tradition that was separate and even hostile to the empirical, positivist trend. This alternate tradition can be traced back to the Naturphilosophen of the previous century; it is characterized by the full realization that science projects and incorporates a world view; moreover, many thinkers of this tradition--for example, Henri Bergson--considered it possible to replace the accepted scientific metaphysics with an alternative. Some did and some did not think that the methods and rationality of science were also suspect.

Whitehead follows in this metaphysically-oriented tradition of philosophers of science; his new metaphysics was one he considered more compatible with the idea of human knowledge. He claimed that the old bifurcation between nature and knowledge--between sensing and knowing--was artificial. He tried to replace this sharp-edged view of reality with a view of reality as a blended stream, a process. In this he cannot only be taken to be promoting the idea

that metaphysics is not something alien to science, but also the idea that one must take a more favorable view of what would usually be considered "confused" metaphysics--metaphysics which "confuses" categories held distinct on more accepted theories.

Yet, Whitehead's views will not be included in the debate below for two main reasons. First, his interest in the metaphysics of science is not fully equivalent to an interest in a role for metaphysics in science; that is, the metaphysics he connects with science is "the scientific world view", the vision of the world produced by science after the fact. There is only one such view; and Whitehead, it can be argued, does not consider metaphysics important as part of the development of science nor does he consider metaphysics as conjectural speculation.¹⁵ Secondly, the more interesting elements in his philosophy appears in the views of Steven Toulmin without the same deficiencies, and these will be reviewed at some length.

Thus, the review of views on the clarity and confusion in science jumps from Duhem to Karl Popper. Popper is noted for thinking Duhem's policies on ad hoc qualifications and additions, changes of meaning, and other strategies for avoiding criticism and "refutation" are obscurantist;¹⁶ thus, he indicates that he considers them to surrender clarity as well as realism. Popper's position on clarity and confusion is extremely important not only because of those arguments but because he links clarity with realism and both with rationality and humanitarianism.

To spell this out more clearly, Popper's philosophy is liberal with regard to mistakes and speculations. Both are essential and positively valuable elements

in science in his view. However, Popper's sine qua non is refutability. And, although a bold speculation may be refutable, the same may not be said with confidence about a confused view. Such a view may well explain nothing in any clear sense of hypothetico-deductive explanation. Or, one may have a great deal of difficulty telling what it may explain. In this way, a confused theory is even more likely than a straightforwardly metaphysical view (irrefutable) to lead to pseudo-scientific claims of verification. For this reason, Popper's methodology does not have a place for them.¹⁷

One can in fact go farther and claim that in Popper's philosophy, the cultivation of confused views is positively forbidden to the rational researcher: it is irrational, not simply non-rational or extra-scientific. Popper's discussion of rationality and the function of clarity in the Open Society makes this conclusion very compelling. Clarity is required for communication itself and thus for the possibility of refutation and improvement. And, without communication and the hope of coming closer to the truth through critical debate, humankind will have to fall back on irrational procedures which flirt with violence. The value of the individual rests on this rational hope.

To expand, one of Popper's most important remarks about clarity unfortunately comes in the midst of his anti-metaphysical and harsh ridicule of both Kant and Hegel. The general point however, is a powerful one, and an obvious complement to Popper's theory that the rational and humanitarian traditions are linked.

"...the standard of clarity is perhaps the most important of [traditional standards in a language]; and it is a cultural inheritance which should be carefully guarded. Language is one of the most important institutions of social life, and its clarity is a condition of its functioning as a means of rational communication."¹⁸

And again, later in the same volume, Popper is discussing the moral aspects of the use of language and of rational communication:

"The adoption of rationalism implies, moreover, that there is a common medium of communication, a common language of reason; it establishes something like a moral obligation toward that language, the obligation to keep up its standards of clarity and to use it in such a way that it can retain its function as the vehicle of argument. That is, to use it plainly; to use it as an instrument of rational communication"¹⁹

This is hardly an attitude generous toward the possibility that cultivating confused metaphysics may be helpful and fruitful in science--or otherwise. When combined with the other rules of rational discourse--especially, to cultivate the most highly testable theory if forced to choose--it would outlaw metaphysics of most types.

Moreover, once it is realized, as Popper explains, that error is not to be feared or avoided but observed and valued, confusion is less excusable than ever. For most of the confusion in views--equivocal meanings, vague references, shifts of emphasis, results from adopting the objective of verification--from trying to be right. Popper explains that it is not acceptable to try to be right; one should try to imaginatively solve one's problems in as clear a manner as possible and then try to refute one's solutions. One need not be confused for one can be mistaken instead; mistakes are straightforward and easy--avoiding them in the effort to protect one's views from criticism is what

leads to confusion,²⁰ Thus, Popper's view of rationality is a powerful argument relating confusion to trifling with the inhuman for no good reason; both metaphysics and confusion are divorced from any rational status in his view of science.

Thomas Kuhn, and Paul Feyerabend are critics of Popper's overall views and their criticism may be thought to bear on the question of the role of a possibly confused metaphysics in science. Kuhn has a place in a discussion on the role of confused toward revision of theories and terms in the face of criticism. Kuhn's well-known view is that science is not what it is--a fruitful progressive enterprise--because scientists stress criticism and accept refutations as Popper describes; rather, an essential part of this enterprise is the rescuing, adjusting, propagandizing for theories of the accepted paradigm by way of "conventionalist stratagems" so maligned by Popper in the name of critical rationalism. The developing paradigm may become complex and contradictory and ad hoc at times; this is an essential preliminary to a crisis situation in a developing science and is not to be repressed. In as much as he defends such views, Kuhn may be taken to be approaching a toleration of confused ideas in science.²¹

Also, Kuhn's stress on the inadequacy of logical arguments to justify one's view of any particular development in science--refutation or expansion of a view reinforces the conclusion that he would tolerate confusion in science. Logic and fact must be supplemented by less clear cut psychological factors which induce change; persuasion and gestalt switch, shared and felt ideologies of a community. These vague and unspecified methodological

factors are also candidates for unclear views which function in science.

However, Kuhn is in fact, not definite enough on the state of the views he is willing to countenance in science for one to judge if he leaves a place for confused metaphysical views. The accepted paradigm becomes complicated and contradictory; but, one doesn't know if it becomes confused in some other more intuitive manner. And, the confused world view which is being developed as a competitor would get very little support if researchers shared Kuhn's philosophy.²³

Another vagueness depreciates Kuhn's seeming appreciation of confused ideas in science: he suggests that toleration of views which do not meet the traditional standards necessitates secrecy or implicit persuasion rather than explicit acknowledgments. Wisdom has noted that this gives many otherwise innocuous suggestions an air of irrationality. One would have to ask how much this secrecy was necessary to Kuhn's philosophy before taking his view of the role of confused metaphysics seriously; for, accepting confused views as if they were clear would be unacceptable.²⁴

Because Paul Feyerabend has made a reputation extolling chaos in ideas and anarchy in methodology, one might conclude he has an account of the role of confused metaphysics in science; however, a review of his position shows this not to be the case. It is true that Feyerabend sees the inadequacy of a methodology which requires that one always value and develop the most highly explanatory theories. Indeed, his criticism of this position are valuable.²⁵

Feyerabend--correctly in my view-- argues that many theories and many methodological practises which are frowned upon traditionally are of value in

research. Indeed, he claims one must defend "absurd, unrealistic, refuted views," and "each [methodological] demand...can be overthrown, or replaced by its opposite". Moreover, "the only principle we can trust under all circumstances is that anything goes",²⁶ according to Feyerabend.

Thus, from finding inadequacy in the accepted views on the value of theory and method, Feyerabend concludes both that confused views must be defended and by any means. No standards or rules can be sensibly formulated or rationally discussed. In sort, he argues for irrationalism. Although Feyerabend would claim that confused views--like any others,--may be valuable in science, he would not be able to discuss that role.²⁷ Moreover, Feyerabend sees the purpose of the "absurd and falsified" theories he defends as emotional and propagandistic not as intellectual.²⁸

In this respect, one must note that Feyerabend, like Kuhn, regularly suggests that his arguments against traditional methodological views show that departures from them must be practised surreptitiously. For example, he would like his readers to believe that the arguments of Galileo were effective only because they were dishonest.

Finally it was only through sleights of hand such as these [not mentioning Copernicanism in "Trattato della Sfera" when he already believed it] that progress could be made at this particular time.²⁹ (his emphasis)

In the same way, Feyerabend insists on interpreting commentators who advocate realism and intellectual honesty--such as Koyré and Popper--to be arguing that the search for truth must be rigidly controlled and dry; he takes this portrayal

of exciting episodes in this history of science--Galileo's spiritedness and Kepler's rhapsodies--to show they were not searching for the truth and were dishonest. Yet, the important alternative conclusion is that the search for truth as captured in these philosophies need not be narrowly moralistic and dry. Feyerabend again uses this sleight of hand to "prove" that cheating is required in science and that realism is not sensible; the conclusion he can support is that the rules of the game are different and more liberal than we thought formerly.³⁰

Thus, these last two commentators who approach a toleration of confused metaphysics in science are inadequate because their philosophies are not sufficiently rationalist or realist; they suggest too strongly that previously unacceptable views can only be accepted surreptitiously, only by admitting that no explicit methodological rules--as opposed to emotional devices or stratagems--apply to the enterprise of science. This is not a view which represents the behavior of Galileo, Kepler, Newton, and Harvey as portrayed in the best histories; at least, other positions can explain their critical behavior and their concern for realism more adequately and they will be explored below. Meanwhile, the only philosophy discussed so far which is liberal as well as realistic and rational--Popper's--has no room for confusion.

Section 4.2. Vagueness on the Relation Between Metaphysics and Science

Many commentators who have made their names by stressing a relation between science and metaphysics--such as Burt and Koyré--are vague when it comes to spelling out just what that relation is. Classic views on this matter have been reviewed in several places including the earlier chapters in this work.³¹ This section concentrates on the more modern, sophisticated, philosophical--as opposed to historical--views on a relation between metaphysics and science; the vague and equivocal--or plainly inadequate--nature of the connections posited will hopefully be revealed.

(a) Whewell, Burt-Koyré, Kuhn, versus Agassi.

Whewell, though a sophisticated empiricist, was a Kantian in outlook and, according to Agassi's report, "considered his own Newtonian-Kantian metaphysics a priori valid, namely demonstrable independently of empirical evidence."³² Here some metaphysics still has a demonstrable character and thus is scientific; Whewell has exception in empiricist circles for recognizing this framework to be beyond empirical support. In other cases, however, he had to explain how dealing with metaphysics could be reconciled with an empirical stance. He explained it in part by claiming that severe criteria for testing could eliminate distortions from false views even if they were accepted prior to the facts; he redefined and strengthened the notion of a severe and independent test because he fully appreciated the need for apriori orientations.³³

Thus, beyond the apriori and demonstrable metaphysics which made up

the Newtonian world view, Whewell was able to acknowledge inspirational metaphysics. Agassi puts his position very clearly. Although Whewell does not make metaphysics a part of science--since science was verified knowledge, he does acknowledge the need for inspiration to achieve that knowledge and thus allows a rational place for metaphysics in research; i.e., in the development of theory.

Since, according to Whewell, science begins by the invention of explanatory hypotheses, he was all for every possible source of inspiration. And, he viewed all reasonable metaphysics as such a possible source. He gave a striking example for this. Kepler had developed his scientific hypotheses, Whewell maintained, in an attempt to carry out Plato's metaphysical program as outlined in his *Timeus*. This idea of Whewell's was so revolutionary that this great philosopher is now almost entirely forgotten because Mill and his followers condemned him as an intuitionist.³⁴

The revolutionary character of Whewell's idea lies in the fact that admitting the cultivation of metaphysics as a legitimate part of scientific research--even if only in development of theory--makes it rational. When true theories can be produced by a simple direct method, being rational may well be acquainted with proposing only verified theories and believing only such theories. The method is not so clear in Whewell's view (although true theories may still be reached securely); therefore, being rational is a more wide open affair. It may include not only proposing hypotheses which may be false but using metaphysical theories to suggest them.

Yet, it should be noted that the actual connection between science and metaphysics is not definite. To say the connection is that between an

inspirational source and the inspiration is not particularly enlightening when one claims that this connection is inarticulable and/or irrational in itself. The metaphysics is still kept at a distance from the science--as inspiration not as validation or foundation--and the inspirational connection is not explicated.³⁵

Also, the unexplained inspirational connection is not adequate when one is deciding whether a commitment to metaphysically inspired theory is to be encouraged, say, above a commitment to an explanatory, verified, or testable theory which has not such inspiration. The view of metaphysics as inspirational suggests that the relations of meanings and content between the scientific view and metaphysical view are incidental and not valued. This is a distortion which Burt and Koyré among others have tried to expose. If one can deduce the preferred metaphysically inspired view from an apriori valid metaphysics--à la Whewell--all is well; otherwise, the preference seems a prejudice and irrational.

Whewell would have decided against the metaphysically inspired theory straightaway; unless the view that is inspired metaphysically was Newtonian and therefore had the other necessary characteristics to be a competitor scientifically. Most of the commentators who follow Whewell in allowing metaphysics as inspiration (including Popper) would either agree with him or avoid the question. They agree that inspiration function of metaphysics is the only one it has played qua metaphysics³⁶ (or the only one which it is rational to consider).

Conventionalists like Duhem who were seriously concerned with the autonomy of science knew better than Whewell; they knew that metaphysical views could not be disassociated from a realistic science by any sharp criterion

even though Duhem, at least, argued that there was no entailment.³⁷ The result is equivocation which has been discussed above. Conventionalists are so liberal as to inspiration that they seem liberal toward metaphysics; in as much as inspiration is irrational, it can come from anywhere even metaphysics. But, conventionalists do prefer that the source of inspiration be as definitively irrational or non-rational as possible; coffee cups and "genius" are preferred to intellectual views since the role played by the latter as speculation might be articulated to the extent that it could be rationally discussed and advocated. Thus, besides tempting actual belief which is disastrous, metaphysical theories as sources of inspiration are often seen as too articulate and amenable to possible rational connection with the theories they inspire to be admitted comfortably to science as sources of inspiration.³⁸

This conventionalist position points again to the fact that it is the rational connection between metaphysics and science which is problematic. The inspirational connection is too distant and too uninformative to be interesting. It is also simply not strong enough to capture the role metaphysics seems to play in science; whereas, more informative and bold positions such as those suggested in Burtt and Koyré immediately seem inadequate because the nature of the connections can not be spelled out.

To review, Burtt and Koyré present views which seem to be left in the amorphous states; they claim that the metaphysics informs the science, gives it language, major content, and critical standards, but is not deductively connected to it; the science is closely related and yet separate. It guides the science but does not become part of it; and yet, the influence is rational--part

of the essential scientific enterprise. The latter suggestion does not come by way of the developed theory on the rationality of science but by an insistence on the usefulness and even essential character of the role that metaphysical influence has in the development of science. Because their historical treatments present this novel view of metaphysical role, their vagueness on the nature of the role is particularly unfortunate; it tempts one to consider the status and rationality of the science in question unaffected. Yet, to push that interpretation vitiates the entire force of their studies.

One can try less vague interpretations of the Burtt-Koyré historical treatments. Agassi describes the Burtt-Koyré positions as the claim that the metaphysics provides a guide to scientific development and that the science which results "conforms" to that metaphysics. He spells out what "conform" means in the following way. First, he distinguishes the conforming relation from either "clandestine" or "confirming" relation.³⁹ It is particularly important that the conforming instance does not confirm the metaphysics. Rather, the conforming instance consists in a phenomena which is interpreted according to a metaphysical view. In his own words, the conforming instance.

served two purposes: one presentational and one to show that ...[the] doctrine is not as fantastic as it sounds...[the instance] illustrated the potentiality of his metaphysics and thus constitutes a challenge to construct instances of that metaphysics which are satisfactory explanations of all known physical phenomena.⁴⁰

Agassi has specified here a non-formal relationship between theoretically interpreted instances can relate to a metaphysical view. And, it is worth noting that the relation is one defined with particular attention to conflicts between

accepted and renegade world views. He holds that there is no formal entailment involved between the participant scientific and metaphysical views.

Generally a metaphysical doctrine neither entails nor follows from any of its conforming instances. Nor does it follow from the set of all its conforming instances unless it may be assumed that the set is exhaustive.⁴¹

Agassi names the latter circumstance the ideal case which, as far as he knows, has not been realized in history.

Kuhn's views on the function of metaphysics in science are among the most widely discussed; metaphysical world views are parts of paradigms--general frameworks--which govern scientific work. On the question of the precise nature of the connection between metaphysics and science, however, it is not easy to report his view. Kuhn does not see any distinction between metaphysics and science; metaphysical theories are the essential views that lie behind scientific interpretations and meld with them. This seems clear enough; it suggests that apriorist view of logical entailment--a suggestion which is reinforced by Kuhn's regular suggestion that no argument may be leveled at a metaphysics from an independent position and therefore all fruitful work presupposes the metaphysics. Yet, it is not clear that Kuhn holds explicitly the view that all theories which influence scientific views are entailed by them or entail them. His emphasis on psychological or propagandistic aspects of scientific belief and behavior, for example, lie outside this realm.⁴²

As an alternative to either a logical relation between science and metaphysics or a psychological relation, one can consider a heuristic or methodo-

logical, instrumental relation. Some of the best parts of Kuhn's treatment may be found under this heading in my view. Metaphysical views or frameworks set the stage for problems, allow criticism and define solutions; these views are very similar to those proposed by Agassi and developed below. Yet, here again Kuhn is vague on the point of whether participants in science have an rational control or defense for this function; and he fluctuates between the view that the metaphysics involved is an empty convention used heuristically or it is a substantive dogma.⁴³ The former is not powerful enough; the latter is too powerful.

(b) More on the Refutability of Metaphysics.

The discussion on the rational role of metaphysics in science clearly involves the question of the refutability of metaphysical views. This is particularly the case since the view put forward below is that scientists often choose to develop theories which can be seen as tests of certain metaphysical views. Metaphysical views have been defined, however, as realistic speculations about the world which are untestable. How then can this proposed test or criticism take place? This question requires a discussion of the importance of the epistemological status of science and of metaphysics as opposed to other characteristics such views may have and on which a rational relationship might be used--e.g., content.

Many commentators have assumed that the rational function of any element in science - fact or theory or metaphysic - depends directly upon its privileged epistemological status. Further, it is often assumed that unless the

epistemological status of two elements differs, no critical relation can obtain between them, at least not in science. There this view is denied. But, let me explain this more carefully.

First, it is necessary to point out that speculations about the world are taken to be metaphysical in this discussion because they are untestable or irrefutable in fact, not in principle. Thales' "all is water" could become a scientific view if it were embedded in a sufficiently developed framework to make it susceptible to empirical test. No claim is being made here that something in the formulation of metaphysical theories themselves makes them necessarily untestable or irrefutable, although some may have such built in elements.

Duhem's view that no theory is testable in isolation but only in a developed theoretical system⁴⁴ leads to the conclusion that there are no strictly crucial experiments in science in the sense hoped for by traditional realists: the logical legitimacy of redirecting a refutation by modus tollens to any part of the theoretical system may be appealed to to make any particular scientific view irrefutable in practise.⁴⁵ If one has defined metaphysical views as irrefutable, one may conclude that it is difficult to distinguish between metaphysics and theoretical science at all. This is particularly the case since in content they deal with the same questions.

They are identical views here that seem to be inversions. Duhem, and others, argue that no theoretical position is strictly refutable--both scientific and (so called) metaphysical views can be protected from attack by conflict with observation report or other empirical evidence. Popper (with strong quali-

fications), Bartley, and Agassi see all theoretical positions, scientific and metaphysical, as refutable in practise under certain conditions. (One can leave out self-contradictory views here.) The conditions are the development of premises which connect the view to empirical data and which are accepted intersubjectively. Both schools observe the same facts about science. The argument is redirected to aims and to whether the decisions involved in setting up the conditions can be made rationally.

However, the debate is complicated by the fact that Popper's most well-known position on metaphysics is not the one referred to immediately above--that metaphysical views such as atomism may be made testable. Rather he is noted for the position that undifferentiated singular statements which make existence claims are inherently irrefutable, and for labelling them therefore "metaphysical." Popper does note that given a theoretical system and some "reasonable" conventional agreements, such existence claims can be considered refuted. Nevertheless, like the positivists, Popper implied that some undeniable difference in epistemological status separated science from other disciplines.

Bartley is one of the commentators who has noted that Popper uses "empirical" with corresponding senses of "refutable" and "irrefutable" in two ways; one is helpful in his characterization of science, says Bartley, and one is not. The first sense of refutable is the sense in which a universal statement which denies existence is refutable. Such a statement is refutable by a singular statement of existence without additional framework, beyond logic; just as pure existential statements are irrefutable in themselves. Although Popper emphasizes this difference and it is in some context important⁴⁷, he tends to

assume it carries more weight than it does. He is particularly mistaken, according to Bartley, in considering it more fundamental to his philosophy than the other view of "refutable" or "empirical" namely, the view that a theory is refutable (or "held as empirical") when it is not saved from empirical criticism by conventionalist stratagems of varying sorts.⁴⁸

The point is that the fact that one can logically choose not to consider a theory or basic statement refutable except in a framework (and even then only given certain agreements) does not, in itself, make such theories metaphysical. One can admit this and claim both theory and basic statement are conjectural but testable in principle.⁴⁹ Popper's stress on singular existential statements as irrefutable and universals which deny existence as refutable in themselves is misleading exactly because it suggests the opposite: that none of the theoretical components are really testable. But the logical situation can legitimately be taken to reinforce the conjectural status of the theory; the practise of diverting empirical criticism may make the theory unexplanatory, a dogma, or a convention. But in none of these cases need one declare it metaphysical in the sense of irrefutable in itself.

The serious misunderstandings in this debate are compounded by the tradition that requires a special status--a more secure epistemological status--between a view that is used to criticize and its target.⁵⁰ The former - it is claimed - must have a more secure base in physical reality. In fact, positivists demand that it have a definitely secure base in reality; all statements which do not have it are more or less equivalent in epistemological status--at zero. Combine this with the view that meaning depends on epistemological status; and

finds all statements which are not epistemologically secure are equivalent in content--they are empty or nonsensical. Thus, of course, one such view can not be used to criticize another.

In tune with Popper's misleading views,⁵¹ Kuhn often gives the impression that any statement which logic and epistemological status does not force one to renounce in the face of criticism must be rescued by stratagems; also, that any view which does not have an epistemological advantage--being more secure--can not stand in a critical (or rational critical) relation to another view. Thus, when Popper or Agassi speak of metaphysics which can become testable, Kuhn wonders why they insist on the difference between the disciplines--implying that if there is no epistemological distinction in principle (in logic) then there is no possibility of rational critical debate.⁵¹ Wartofsky, in his summary of the positions of these commentators, is similarly preoccupied with the lack of secure distinction between metaphysics and science when Popper speaks of metaphysical views which become testable and Agassi, as he says, speaks of "refutable metaphysics". Why, he asks, does Agassi maintain a distinction between the two after this point?⁵²

Wartofsky, like Kuhn, misses one central point; that Agassi is interested in the critical relation between metaphysics and science; this relation may be based on conflict of meaning (content) not related to epistemological status. Agassi is not interested in a relationship of confirmation or entailment and he does not base this critical relationship--as Popper does at times--on claiming a privileged epistemological status for either partner in the debate.

Section 4.3. Metaphysics, Explanation and Rationality--Hanson and Toulmin.

Although traditionally theories on the relation between metaphysics and science have been taken to be independent of or complementary to theories of explanation, here they are seen as competitors. They are taken as competitors in as much as both bear on the more general question of the rationality of science. This position is similar to views developed by Stephen Toulmin and Norwood Russell Hanson: they stress the importance of function and change of theories or a context of discovery as opposed to the logical structure of a body theory as the locus of rationality in science. Yet, although this is the case, the view defended in this dissertation comes closer to Popperian theory of science than it does to those developed by Hanson, Toulmin, or other historically minded commentators.⁵³

First, then, although theory of explanation is often held to be independent of any views concerning the development of science, one can still force a conflict between theories of the two types in the following ways. Theory of explanation may be taken as important in as much as a commitment to produce such explanation is connected to progress in understanding the world. That is, theoreticians who extol explanations and who emphasize a characterization of science via the logical structure of a body of knowledge usually imply that producing such systems with such relations is what makes science valuable.⁵⁴ The rationality of the enterprise is tied to the commitment to a certain type of explanation. The theory of rationality of science is taken to be the theory of what makes science a progressive, fruitful, and valuable discipline; fruitful and

valuable are taken to refer to the greater true understanding of the world.⁵⁵

Both Hanson and Toulmin see their ideas on the development of science as competitors with theories stressing explanation in terms of logical structure in explicating the rationality of science. In this connection, it should perhaps be emphasized that the "development of science" can be understood to mean several different things. Besides referring to historical series of events, it can mean solely the development of ideas, theories, conjectures; or, one can indicate a concern for research as a whole not simply the development of theory alone. The historically oriented theorists, Hanson, (Kuhn), and Toulmin,--drift between these concerns but for the most part Hanson emphasizes development of theory whereas Toulmin is concerned with research as a whole.

Hanson, then, attacks the traditional theory of science by saying that there is a logic of discovery. Although a logic of the process of getting a new idea may be unanalysable, says Hanson, one can still distinguish between the reasons for suggesting an hypothesis and the reasons for accepting an hypothesis once proposed.⁵⁶ The reasons for the latter are the only ones admitted to bear on the rationality of science by theorists of explanation such as Braithwaite, Feigl, and Popper, says Hanson; these theorists conflate the reasons for suggesting an hypothesis with the psychology of discovery; yet, clearly the two are not the same.⁵⁷

Moreover, the considerations which lead one to propose an hypothesis may be quite important in the articulable methodology of science.

The context of discovery should be recognized to as having a logical credentials of its own and should not be relegated to a

kind of "puzzling out in reverse" of what will end up as the finished research report.⁵⁸

This indicates that what is valued in the process of discovery will not be the same as what is valued in the process of testing or validating.

However, despite what Hanson says in defense of reasons for putting forward a theory, he is not consistent as to whether or not these reasons differ from those which are important in accepting the theory. In his discussion of Pierce, e.g., he notes that many theorists of "retroduction" as he calls the reasoning which leads to the discovery of a new theory see the "recognize that their formal criteria for success in argument are precisely the same."⁵⁹ Only the sequence in time differs not logical elements or relations. But that the logic is the same does not mean that the elements which do differ are not unimportant⁶⁰ and have no rationality of their own:

Appeals to analogy, symmetry, simplicity, and elegance have a rational function within a Discoverer's attack on the unknown.⁶¹

That one can add so easily "cultivation of metaphysics" to this list is what makes Hanson helpful here. These less formal elements in the scientific process should not be dismissed as "poetic" or as part of the discoverer's "genetic constitution and upbringing".⁶²

However, it is not clear what Hanson would say if the rationality of the informal elements named above should conflict with the rationality for testing or accepting a theory logically; what would Hanson suggest? He seems to argue that this will not happen: he is simply expanding the scope of the previous

idea of rationality in science not changing it; or he is only changing it in as much as he expands it by attributing rationality to the process of discovery.

Also Hanson reneges from the point of view of a position favorable to metaphysics because he acknowledges the superior force of an inductive validation in any contest with criteria that govern the development of theory. The latter are not sufficient in themselves for validating a hypothesis whereas the confirmation by a factual base is sufficient in itself.⁶³

The position developed here is a denial of this last point; and it can be argued that Hanson is mistaken. Many other aspects of science are needed to support the factual confirmation; it is not sufficient in itself to authorize the hypothesis by any means.⁶⁴ Thus, in sum, Hanson's view does not explore sufficiently the idea of a conflict in the rationality revealed in discovering hypotheses and the rationality revealed in testing them; when he does, he sides too quickly with the traditional views on these matters.

Stephen Toulmin's departure from the traditional idea of rationality as centered in the logical structure of theories is far more extensive than Hanson's. Toulmin argues that the idea that achieving or maintaining certain logical relations "as the only measure of rational acceptability"⁶⁵ is a remnant of the days when the theorist hoped to found the rationality of science in a set of authoritative scientific theories. This must be surrendered in place of a theory of rationality which emphasizes change and progress, the maintenance of impartial, enhanced communication and investigation.⁶⁶ These views correspond to Popper's liberal theory of rationality as critical Socratic debate and are welcome in an attempt to accommodate a rational role for metaphysics in science.

Toulmin, on the other hand, argues that change is authorized by "higher order function" which have nothing whatever to do with such formal considerations as the logical conflict between theory and fact.⁶⁷ To explain, instead of concerning oneself with logical characteristics of theory, Toulmin suggests that researchers concern themselves with the "adequacy of concepts."⁶⁸ Toulmin assumes that the idea of what is a powerful concept is quite unproblematic in Toulmin's mind. Since an appreciation of the possible role of confused metaphysics views is in question here, a philosophy which stresses the adequacy of concepts and the context which makes the framework interesting and compelling rather than the context which validates them or makes them highly explanatory might seem promising.⁶⁹

However, the judgment of what is a powerful concept is hardly less problematic than the judgment of what is a true proposition; in fact, the two would have to be related directly if Toulmin wants to connect rationality with increasing one's understanding of the world as he seems to desire.⁷⁰

In this regard, Toulmin somewhat unaccountably disregards or misunderstands the stress that Popper puts on change of ideas and rational function of logical considerations and specifically his concern for rational debate as the reason for defending them. Toulmin sees Popper's demarcation criterion as apriori and arbitrary⁷¹ rather than a standard developed with reference to a function--namely that of continued progress in science through empirical criticism. Toulmin misses all this and concentrates instead on Popper's mistaken emphasis on the idea that formal considerations authorize the acceptance of theories and that achieving theories with formal characteristics (highly explan-

atory) is the only aim of science.⁷²

For Toulmin the higher function of formal considerations of any criteria is not criticism, which he scarcely mentions and in the possibility of which he may not quite believe.⁷³ In his view the function of techniques in which rationality lies is in the development of theory alone--in the "improvement of the theoretical inventory of science".⁷⁴ This improvement, as noted, is made by way of improving concepts, not--as Popper suggests, by criticizing and arguing about theories. But, as noted, the techniques to improving concepts are not sufficiently problematic in Toulmin's view for him to spell them out, nor is the adequacy of power of concepts a direct connection to any criteria of evaluating theories as true or false with reference to the facts. This lack makes it very unclear how being rational might lead to greater understanding of the world.⁷⁵

This last inadequacy connects with what must, in my view, be the major lack in Toulmin's overall theory of science; his view of rationality as inarticulate^{ate} or relative. To explain, Toulmin sees rationality as not only disassociated from establishing an authoritative set of scientific theories (with which Popper would agree) but also disassociated from any set of rational procedures for progress:

Men demonstrate their rationality, not by ordering their concepts and beliefs in tidy formal structures, but by their preparedness to respond to novel situations with open minds--acknowledging the shortcomings of their formal procedures and moving beyond them.⁷⁶
[my emphasis]

Popper talks only of acknowledging former theories and moving beyond them but

he is not amenable to the idea of moving beyond (his own) rational rules of method. Thus, in emphasizing that the rules of rationality themselves change, Toulmin is going an important step beyond Popper.

The idea that the theory of rationality may itself change and develop is important and fascinating; such a change is a necessary by product of an accurate account of the role of metaphysics in science. The problems here are, of course, can this higher level change in rationality be made rationally itself, and how do such changes affect the rationality of the entire enterprise? Toulmin does not answer satisfactorily. It is not even clear that Toulmin can escape the fixed and necessary principles which he dislikes. He tries to replace them with

a sufficiently full and detailed understanding of the features that make alternatives strategies and procedural innovation "adaptive" to particular problem situations.⁷⁷

Unfortunately, this understanding comes, one would think, by way of an articulated theory concerning the features of these strategies and it would expressing that ^{which} makes them adaptive. There is no difference between such a theory and, say, Popper's principles, that falsifiability makes a theory more amenable to improvement through criticism. Likewise, any set of "procedures by which men change from one set of concepts to another"⁷⁸ will be the "abstract" theories which Toulmin criticises in his more logically-oriented opponents.

Toulmin is right, however, that such principles of rationality need not be proposed as necessary in the sense of "dogmas". They need not be "put forward

as "unchanging principles mandatory on all rational thinkers"⁷⁹ if this means that one can not develop them or that they have not changed in history. But they will necessarily--one would think--state what is rational in a universal way although tentatively.

Toulmin however, does not come to the view of theories of rationality as tentative; in the effort to avoid "historico-cultural invariance of all rational procedures"⁸⁰ he drifts instead to an idea that the principles of rationality are inarticulable or that they change in no generally characterizable way. In fact, he comes to a position laterly popularized by Imre Lakatos, that what is rational is something which can only be determined at the moment or even after the fact.⁸¹ For example, the features of procedures mentioned above which make procedures adaptive to certain problem situations can only be understood "in the light of experience as we go along."⁸² To come to new evaluations of the truth or of explanatory power of theories in a system as time goes by need not disturb the progress of research, but to have the criteria of rational procedure inarticulate and judged only post-hoc changes the idea of rationality in a way that makes it useless in this debate. The general cases in which cultivating confused metaphysical views in science is rational can be explicitly characterized; otherwise, the vagueness remarked on above will remain and the chances in rationality required cannot be noted.

Section 4.4. Popper on the Rationality of Science and of Metaphysics

The realistic and critical orientation of Popper's theory of science makes it significant in a discussion of relations between science and metaphysics; his theory opens the way for a rationality which emphasizes rules of search and rejection more than an authoritative status for the end product. Therefore, the pressure to prevent incursions from possibly unsatisfactory elements--like (confused) metaphysical views is-- theoretically--eased. Also, Popper's views on metaphysical theories reinterpreted as regulative devices of a methodological character, is interesting and novel. It is novel in as much as he combines it with a realist's view of science and it is one way to make the functions of "metaphysics" in science rational.

However, Popper can not be said to have a favorable attitude toward metaphysics; only when separated from his own overly strict presentation does his theory reveal its potential in the respects mentioned above. Even in its best formulation, his theory of science can not accommodate fully the role of metaphysics in science developed by Burtt and Koyré; as Koyré's remarks on the inadequacy of a view of science as "merely testing and replacing false theories" shows this. Still, Popper's view--when taken in its pure form--comes the closest of any theory reviewed here to an accurate picture of science; and it provides the basis of the theory of interaction between metaphysics and science presented in the next section.

Popper is the heir to the Whewell tradition of liberalized views on the rationality of science which are necessary to a favorable consideration of meta-

physics. Like Whewll, Popper considers the role of speculation--bold conjecture--essential in science. Unlike Whewell, Popper considers the end result of scientific endeavor to be conjectural also. Thus, he must distinguish between the working hypothesis, other speculation, and the theory produced after considerable research by some other criterion besides the change from an insecure to a secure/epistemological state - a lower to higher epistemological status.

One might think that such a requirement would make the 'scientific' status of theory and metaphysical view closer. However, as is well known, Popper has his own criterion for distinguishing scientific speculation from unscientific conjecture--namely, refutability; moreover, he suggests that one judge between one theory and another in science on the basis of greater explanatory power. These two criteria contribute to a position which is rather unsympathetic to an interaction between metaphysics and science.

Thus, although Popper considers metaphysical views helpful to science as sources of inspiration for the bold conjecture which are the working tools of science, that is as far as metaphysics may penetrate. Other sorts of metaphysical influence are by implication irrational in his view: that is, the basic theory which is beyond test, the influence on content which is beyond direct empirical criticism, anything (in short) which smacks of countenancing irrefutability, can not be part of rational scientific procedure in Popper's view. Also, metaphysical views per se show up badly in Popper's theory of science not simply because they are irrefutable, but also because they are vague and general and therefore often not highly explanatory.⁸³

However, one might think that Popper would encourage the kind of

consideration of metaphysical theories which is most at issue here; namely, their refinement or development into testable theories. Also, since Popper is concerned more than most commentators with the idea that science begins with problems, the view of researchers struggling with an undeveloped, unexplanatory metaphysics which still is the only available answer to a question--defended below--ought to be appealing to him. However, the refinement of metaphysical theories into testable views is simply not emphasized in Popper's work; at least, such intellectual endeavor still remains outside and beyond the rational scientific operations of science and this is what is important here.⁸⁴ Thus, Popper must say that this kind of endeavor is irrational or a-rational--not governed by any particular rules, objective and intersubjective, as are the methodological principles which make scientific thinking rational and progressive. Rationality only starts with the testable conjecture which addresses itself to a problem.

Even this latter part of Popper's rationality criterion--namely, that a theory must address itself to a problem--is unsteadily projected as part of his philosophy. The requirement does not play an essential role in the rationality of the endeavor: a scientist can be rational on Popper's view if he simply proposes a highly explanatory theory and tests it with the idea of refuting it--whether or not he has attended to the problem the theory solves. Thus, the rational role that cultivating metaphysics might play when there are no testable solutions to offer to problems is not brought solidly into the scope of rational thinking by Popper. This emphasis must be credited to Agassi, as explained below.

Popper might well argue that he dismisses metaphysical theories as solutions to problems which have as yet no testable solutions because he sees the cultiva-

tion of metaphysics in these situations as very easily developing into pseudo-scientific behavior. This is an important objection since the major question at issue is whether even a confused metaphysics can play a rational role in science. As Agassi explains enlighteningly, Popper is somewhat more hostile to metaphysics than Whewll; Popper lumped together metaphysics, superstition, and pseudo-scientific theories;⁸⁵ all are untestable and unexplanatory and therefore unacceptable. The cultivation of metaphysical views as solutions to empirical problems may lead to claims about the explanatory nature of the theory when it has not actually been tested, in vague conditions for example. This is metaphysics becoming pseudo-science. Thus, Popper finds himself more comfortable with a rationality which makes all intellectual maneuvers which do not amount to the actual testing of theories by experience, extra-rational. Given Burtt and Koyré then, he is faced with a view that some of the most characteristic and seemingly helpful activities of scientists are extra-rational.

To see if Popper can account for such activities another way, one must note that he tries to interpret metaphysical principles as methodological rules instead of as untestable theory. All general metaphysical laws such as the law of causality or the metaphysically-stated law of conservation are transformed into criteria of adequate or fruitful explanation by Popper. The law of causality becomes the injunction always to look for a previous event to account for any observed effect. So translated, the principles can be judged objectively on the grounds of their utility without making science conventional; and, they do not occur as untestable theories of content within science, thus making science dogmatic.⁸⁶

This exercise in reinterpretation of metaphysics into methodological principle may be seen in a number of ways. First, as an imaginative maneuver which allows rational discussion on principles which other commentators take to be beyond the reach of rational criticism. One may consider the maneuver, however, a way to import content into scientific theories without laying that content out explicitly as theory. Some may think it preferable to keep laws such as the law of causality metaphysical and contentful simply to be straightforward about what their science assumes.

However, the point is that the translation of metaphysics into methodology does indicate a very decided aversion to allowing any untestable view into the scientific areas of science and the corresponding belief that testability is all that is needed in science. If Popper considered seriously the option of working on a metaphysical view to make it testable, if he seriously thought this could be rational thinking, he would not be so rigorously thorough at eliminating metaphysics from science. As noted above, Popper is extremely mistrustful of any such program of developing metaphysics in science and he does not see the need for it. As Agassi notices, Popper "seems to fear that dogmatism may creep into science through the back door". He therefore insists on two problematic and unnecessary principles as part of his philosophy of science: 1) that one apply the criterion of highest degree of testability in ordering and valuing theories, and 2) that one accept as true all observation reports made repeatedly.

In an attempt to block all attempts at dogmatism, Popper has isolated the open-minded from the possibly dogmatic and the dogmatic. I do not like this even as a limiting case. Why should we always be able to discern the possible apologist or dogmatist from the critically openminded?⁸⁷

These principles will be criticized below; but here they are mentioned to show that Popper cherishes an overly developed sense of the need to guard rationality.

The situation with respect to Popper's view on an interaction between science and metaphysics, then, is this. Popper accepts metaphysical views. But, this inspirational function is outside rationality and outside scientific research proper. On the other hand, his methodology emphasizes the conjectural status of theory and the critical basis of rationality. This puts the speculations of science and of metaphysics on the same level epistemologically, and Popper claims both are valuable in as much as they are put forward as realistic explanations of the world.

Another point which must be emphasized is simply that because Popper gives up the idea that the theories of science can ever be distinguished from other theories securely by their truth, he must necessarily deal with and discuss other aspects of theories. Their refutability and explanatory power are just two such characteristics; that theories can be criticized, can give some understanding of the world, can solve problems previously unanswered by any conjecture, can unite a series of explanations into an overview--these are all aspects of theories which have been largely neglected and which a theory emphasizing the conjectural nature of science may feature.

These latter ideas are particularly important in a consideration of the role of metaphysics in science; indeed, a "relation to a metaphysical world view" is another aspect of a theory which may be valued. Popper himself does not develop them but the scope for such a development is basic to his theory of science. Because Popper's theory puts the realism and rationality of science in

criticism, one can evaluate these new merits of scientific theory without claiming any authority via true description for them. The separation between the other positive aspects of theory and the realism and rationality of science makes his theory particularly helpful in this debate.

Section 4.5. Agassi's and Bartley on Science and Metaphysics--corrections
and additions of Popper's theory of science.

Popper's theory of science makes criticism a necessity for progress in science; he admits himself that criticism takes imagination and hard work. Feyerabend and Bartley points out that it may take more than that; it may take the development of alternative theory, even an alternative metaphysics. A complex and highly confirmed scientific view may not be easily criticized from within the framework which includes it. Thus, alternative metaphysical systems have a valuable place in rational scientific development in any era.⁸⁸ When Popper suggests that a preference to work with only highly testable theories and to try to refute them to prefer them to other less testable theories, is the only rational behavior for a scientist who cares about the truth, he is verging on contradiction. This position may well be a positively uncritical approach to the established theory.

One of Agassi's major criticism of Popper⁸⁹ is similar to the one above. Popper's theory of science, according to Agassi, does not require the great emphasis on high explanatory power in theories which he insists upon. Popper argues that this characteristic subsumes all valuable qualities about a theory--simplicity, generality, interest,--and that therefore one does not rationally estimate theories on any other basis. There are many points here that bear on the role of metaphysics in science; but a basic one is the necessity of a connection between high explanatory power and rationality, Agassi--among other others⁹⁰--denies the conflation as he explains at some length. Not only does

explanatory power not always to with refutability but "simplicity depends not only on explanatory power but paucity of parameters, and high explanatory power is not the sole characteristic of a satisfactory explanation." One needs independent tests; nor does abstractness go together with universality. Given the fact that explanation is not so narrow:

The result is pluralism: we may admire one theory for its boldness, another for its explanatory power, another for its elegance and yet another, I suggest for the light it throws on some topical metaphysical issues.⁹¹

An association with broader metaphysical questions is here put forward as one possible merit of a theory and one element in the decisions to cultivate, research or "accept" it. The characteristic is not antagonistic to achieving highly explanatory theory nor is such a merit useful merely as a preliminary to explanation. The association with a metaphysics, as the other characteristics names, is put forward as equivalent in value with high explanatory power in the relevant situation. To defend this plurality of values opens the door to a role for metaphysical views for another reason; it is a claim that views which are not highly explanatory may be nevertheless valuable and interesting.⁹²

Agassi argues further that Popper's theory of rationality needs augmentation of a kind which reinforces the need for toleration of less explanatory views such as metaphysical principles. The augmentation required is a more specific criterion of criticism. To explain, Popper's theory of rationality provides no answer to the question which criticism is worth listening to. One could argue that in the same spirit of distrust which leads him to bar cultivation of all but

explanatory theories, Popper resists developing a theory of criticism which indicates some criticism may be ignored. Such a theory is necessary; for, it is a simple fact that not all criticism can be attended to. The need for selection criteria also suggests to a role for metaphysics.

Yet a critical attitude is but a necessary condition. While it is true that unrepeatable facts are useless, too many repeatable ones are left unstudied. Footprints in the sand are as repeatable as one could wish, yet science says precious little about them. In my view the ignored phenomena are those which ~~our~~ metaphysical frameworks are too poor to interpret (in the sense discussed below). They are too problematic. The same applies to theories, like elasticity theory, which are too difficult to incorporate within the existing metaphysical frameworks, and hence are not scientifically interesting.⁹³

Moreover, to try to attend to such phenomena or theory would lead to a dissipation of energy which would spare the theories in question any severe test instead of encouraging it, as Popper hopes.

Thus, unless criteria of criticism are developed, Socratic debate may well not be critical at all. The missing element in it, says Agassi, is a reasonably defined aim to the critical debate:

... since in order to be effective, criticism must be selective and discriminating; and since in order to be properly selective and discriminating, one needs some criteria for selectivity and for discrimination...the modification suggested here is rationality is not any critical debate, but only that which is oriented toward a specific goal as well as might be reasonably expected.⁹⁴

The Talmudic Scholar, although he debated constantly, made no statement of aim but left the aim as understood; this understood aim clearly alternated between the wish to express respect to God and to the Talmud by devotionally

absorbing oneself in it, the wish to understand the meaning of the Talmud in all fullness, and several others. (The aim not included at all was the wish to criticize and refute the major theories of the Talmud. The framework could not be touched.) The criticism, then, did not tend to be a well enough defined aim to be judged fruitful or not--and shifting aims would prevent any rational account of progress. To get conditions for rational development, one needs to add to the idea of critical debate the idea of a reasonably well defined aim.⁹⁵

The importance of this last point is worth emphasizing. The aim of "truth" is not a sufficiently well-defined aim; a clear and still intellectual alternative is the instantiation or refutation of a particular metaphysical world view by scientific theory. The rationality of Harvey's case is well captured in this description. In developing his theory he was not searching for the truth in the abstract, he was trying to refute a corrupted and unsatisfactory metaphysics of Galen and replace it with the neo-Aristotelian metaphysics he preferred. Likewise, Galileo and Kepler were not simply trying to find the truth; their enterprise is far more accurately described as the attempt to refute and replace an unsatisfactory metaphysics.

This is not a view which conflicts with the idea of science as the search for truth--as Koyré's studies should illustrate, it shows more definitively what the search amounts to. The view does conflict with ideas of the search which refer only to the scientific theories themselves and to tests of them; *rather than to problems and metaphysical views as well.*

Just as the need for criterion of criticism is essential to a Popper-like theory of rationality of science, an explanation of the selection of problems

is also. It is another inadequately developed part of his theory of the rationality of science which--when developed--reveals a rational role for the cultivation of metaphysics:

Popper's theory is that rationality and progress depend on criticism. As Agassi has pointed out, this means that it is essential to the rationality and progress of science that researchers do not each pick separate problems.⁹⁶ There must be a concentration of effort on particular problems so that there may be criticism; as criticism is not easy to develop, a fruitful science needs quite a lot of concentration of research if it is to flourish.

Agassi argues that one important factor affecting this concentration of work is the fact that some scientific problems have a more direct bearing on metaphysical interests and conflicts than others. Scientists often choose to work on particular scientific problems and try to develop particular scientific theories because those problems and theories will shed light on the metaphysical issues of the day.⁹⁷

The argument above--that rationality needs a reasonably well defined aim for criticism--makes this interpretation of problem selection compelling, since scientists need a higher level selection device to proceed with the critical examination of an issue. Not only does the interest in metaphysics help to select and isolate issues, it provides the rationally necessary well defined problems that makes possible an intersubjective criterion of criticism.

Agassi also develops the idea that a metaphysics can be used to interpret theory. First,

An interpretation may develop into a scientific theory...and the new scientific theory may be difficult to interpret from a competing metaphysical viewpoint.⁹⁸

Although metaphysical views are not as criticizeable as scientific ones, something on the order of a crucial experiment of a body of theory or fact is possible if the theory is interpreted from one of two metaphysical positions and the interpretation is refuted.

The metaphysics behind the defeated scientific theory loses its interpretive power and is then abandoned.⁹⁹

To be clearer about the relation between the metaphysics and the science:

Metaphysical ideas belong to scientific research as crucially important regulative ideas; and scientific physics belongs to the rational debate concerning metaphysical ideas. ...relevance to metaphysics contributes to the uncontested high status [of many of the greatest modern experiments] and yet, I contend, the metaphysical theories related to these experiments were not parts of science.¹⁰⁰

They were as explained elsewhere not deductively related but as "conforming instances."¹⁰¹

Agassi's view is not a theory of metaphysics as a guideline to scientific research. He speaks of metaphysical views as providing a scientific research program, but, the metaphysics does not describe a smooth linear development of science from metaphysics even when he speaks of metaphysics as research program for future science. Rather, the theory concerns a critical interaction between science and metaphysics.

The standard approach is both to denounce the metaphysics of scientists and to commend parts of it as divination of future scientific developments. Burt's alternative is to view metaphysics as the possible [my emphasis] foundation of future science; to view it as often conflicting with existing scientific theories and as incentives to alterations which would remove the conflict. In my view, then, the interaction between physics and metaphysics is by way of metaphysics prescribing programmes [contending programmes] for future scientific development.¹⁰²

The science which is developed may refute metaphysical views--even those it is developed to instantiate. The metaphysics therefore develops in a critical way, as the science does.

To summarize then, when Popper's view is suitably interpreted and corrected, it accommodates metaphysics in various rational roles. First, Popper's theory of science must be stripped of its needlessly powerful commitment to highly explanatory theories as the exhaustive and inclusive criteria of the value of theories and the rational authority of basic statements. Feyerabend and Bartley encourage this purification by noting that Popper's own views may require the cultivation of metaphysical views to enhance criticism. Agassi adds that Popper's conflation of all values in a theory with explanatory power is mistaken and makes him ignore the role of metaphysical issues as both interpretive theories which may be indirectly tested and make other theories testable but also as factors which rationally encourage the coordination of research. All of these functions enhance criticism and thus can have a place in Popper's theory of science. When it is seen also that rational criticism requires a reasonably well-defined aim as well as the critical review of theory, the

potential legitimate role of metaphysics is enhanced even further.

Section 4.6. An Augmented Popper-Agassi View on the Relations between
Science and Metaphysics--the possibility of confused metaphysics.

Agassi's view of the relation between science and metaphysics as a critical dialectical relation in which metaphysical theories are used to interpret theories which are then made testable,¹⁰ help in the selection of problems and views to work on, and¹⁰ help to provide a standard for criticism, is a very good approximation to an adequate account of the rational role of metaphysical views in science. His theory fully captures the historical pictures which Koyré presents, for example, including their emphases on conflict and criticism. However, Agassi's theory does not make any reference to the possibility that the metaphysical views referred to might be confused and inconsistent, perhaps even incoherent. His theory does not exclude that possibility; he simply does not discuss it.

Although Agassi is a defender of metaphysics in its helpful and important connections with science, he emphasizes the useful and valuable parts of the positivist critique of metaphysics. To give his own summary,

In addition to being an overzealous [but necessary and valuable] criticism of irrationalist metaphysics, positivism has also served the rationalist metaphysician. Metaphysics can easily degenerate into pseudo-science by providing a framework for ad hoc explanations instead of scientific ones. The Baconian-positivist attack on metaphysics as ad hoc or pseudo-scientific helped the good metaphysician by putting him on his guard against irrational practises.¹⁰³

Thus, Agassi does not think that a consideration of metaphysical views is necessarily valuable in science. He goes on to say,

It is not my purpose here to disprove positivism but I feel I have to stress that in this essay I am speaking of good metaphysics after having acknowledged the partial justice of the positivists attack on it.¹⁰⁴

The reference to the partial justice of the criticism of metaphysics is important; the critique was that metaphysics provided a framework for ad hoc explanations and pseudo-scientific claims of theories which seem to have been tested but can not be. Whewell and Popper, as Agassi later discusses, were particularly sensitive to such uses and thus stressed the need to outlaw ad hoc qualifications and to insist on severe testing — they allowed metaphysics as inspirational source in science. All these criticisms are indeed justified and valuable. For the issue at hand, it is necessary to ask if they indicate that confused or contradictory metaphysical views must still be excluded from scientific research even though Agassi has rescued "good metaphysics" with his corrections to Popper?

In the spirit of Whewell's philosophy of science, one can argue that understanding the need to avoid ad hoc character of adjustments^{or} misleading tests and using certain other rules, will eliminate the irrationalism one might fear from a cultivation of confused, possibly "bad", metaphysics in science. The certain other rules have been discussed in the previous section; namely, the rational requirement to have the reasonably well defined aim for criticism and problem selection. Given an elaborated theory of when and how a confused metaphysics may be valuable in leading to a greater understanding of the world, one can allow it a place in science just as Whewell allowed a place for metaphysics as inspiration once it was understood that severe testing of claims

was the standard by which one could prevent most regressive developments.

One problem is, of course, the risk of wasting time with confused metaphysics in the hope of achieving something worthwhile; such efforts may be much more easily a source of stagnation and frustration if not actual pseudoscience than efforts involving the standard metaphysics which Popper feared. But, as argued, the risk is reasonable in certain conditions given the articulated standards of such activity outlined in the previous section. And thus, the Whewellian trust is the more appropriate stance.

One might reinforce the conclusion that a cultivation of confused metaphysical ideas in science would not be a source of frustration and stagnation by recalling the research aim that put a confused metaphysics in the running--to keep an interesting problem on the agenda of science. That is, when a problem is important one but the only means of handling it is by way of a confused metaphysics, the cultivation of such a metaphysics may be worth the risk.¹⁰⁵

In Harvey's case, for example, the problem of the organization of the body was an important one and he did not find the standard, more straightforward metaphysics satisfactory. The choice was to struggle with a confused metaphysics or work without a metaphysics at all. Yet, much of his research depends on a metaphysical framework--to provide interest, to give a broad view of the body, to suggest the direction in which theory could be developed, to make possible the development of new standards by which to judge the theory. It is hard to think of Harvey's work without this metaphysics; yet, in his age, a confused metaphysics was the only one available.

Moreover, Harvey worked in an intellectual surrounding in which stagnation and misdirection on account of an immersion in a confused intellectual system was far more likely than it is now. Not only have our standards of explanation, argument and clarity improved enormously in quality and simple articulation, but the stock of good metaphysical sources has grown tremendously as well. If Harvey could make valuable use of a confused metaphysics in his *situation*, it seems reasonable not to fear irrationality and stagnation from cultivating confused metaphysical views in the name of science now. The same summary may be given of Kepler's case, discussed in the section on Holton's article (2.3d).

One might also add that the power of a confused metaphysics is another intersubjective and non-arbitrary criterion by which a confused metaphysics could be considered fruitful and worth placing on the agenda of science. The confused metaphysical views of the late Renaissance were particularly enticing because they addressed themselves to great major questions such as the theory of man, possibility of knowledge, and the nature of the world. To take a chance with such theories--new, promising and addressed to urgent issues--also seems worthwhile. The thesis one comes to then is this: A confused or inconsistent metaphysics may contribute beneficially to scientific development when it is introduced for reasons specific enough to be critically debated; such reasons may be to provide an intellectual framework which keeps important questions on the agenda of science which otherwise could not be kept there; to focus attention on specific debate; to provide a unified framework (even though inadequate) for disparate areas; to give a framework in which to develop criticism of established scientific views. In all these cases, a metaphysics may be essential

to continued rational discussion in science--to the search for truth by rational means--and a confused metaphysics may be the only one available; it can, despite its confusions, perform these functions.

To elaborate, metaphysical issues can operate to select scientific problems as important and interesting. This function is essential to avoid division of labor which would prevent criticism. Metaphysical theories may contribute to the interpretation of views which may then be developed to be testable. These views may then be considered to illustrate the explanatory power of a metaphysical world view or to criticize an opposing world view. The criticism or the instantiation of metaphysics may provide the "reasonably well-defined aim" which makes criticism of a scientific endeavor rational. Some important problems--particularly of unification--could not remain part of the rational debate of science but for the metaphysical framework which forces them to the fore. Also, the metaphysical principles can play a role as regulative devices as a special case of the previous view: giving part of a theory of criticism.

A metaphysics can be said to progress--or perhaps more accurately--be said to be part of the progress of knowledge--if it becomes more clear, if it becomes more testable, as just some examples. The point is that none of these developments are excluded here by any means; in fact, the aim is to make them explicitly part of the rational development of our understanding of the world. If a metaphysics begins by being inconsistent and is made consistent without losing its relevance to the important problems it had answered, one can call this progress in a metaphysics. Since metaphysical views are theories and

theories about the world, one can talk of their progress in the same way one speaks of progress in science. The criteria are open to debate of course; but as they are open and traditional, one can be justified in using them to define progress in the development of a metaphysics.

As to the recurring question, is confusion to clarity progress, I like this question exceedingly because it is a question instead of an implicit assumption. The answer is this: under most conditions but not necessarily all. One point made in the dissertation is that cultivating confused views as opposed to clear ones may be advisable if that keeps important and/or intriguing problems under discussion which would otherwise fall from the agenda. For example, when Harvey prefers the clear position that the blood in fact circulates, nevermind why, to the uneasy and unclear view that one should explain why the blood circulates either in terms of purpose or metaphysical proprieties or some such, he is being less progressive than when he makes the opposite choice; his choice cannot be defended as the only rational alternative. Likewise, if Kepler had deserted his aim to give a unified picture of the world in order to avoid plunging into the complexities and obscurities of a strange world view that merged physical force with mathematical harmony, I would not like to be forced to congratulate him for making the only rational choice. In both these cases such a summary would be particularly unhappy, since it does seem clear that the struggles with a confused metaphysics cannot be separated from the valuable and indispensable achievements of the researcher. We find it unsatisfactory to say thinkers are irrational to do what is--not accidentally it seems--fruitful and essential to the growth of science.

Again the question, what is the relation of clarity to truth, is marvellous and relevant. I do not know the answer. What I can say is that it is unclear to nearly everyone. I can add that on the usual correspondence theory of truth, a statement would have to be clear enough to be understood in order to be said to correspond to the facts. If the situation to which it was supposed to correspond was vague or indistinct, the statement supposedly corresponding to the situation could be vague and indistinct and yet true by the definition. This is different from saying the statement is confused.

I want to add that in discussing the potential rationality of putting forward^{and} working with confused metaphysical ideas, we are in any case one step removed from getting the truth. That is, one might want to list conditions for truth which a theory must fulfill--one might say it should be clear, it should be testable, it should be confirmed on every test, it should be confirmed on every severe test, it should be general enough to have severe tests, etc. All these can be considered necessary conditions for the truth of a theory. But, in talking about the rationality or irrationality of cultivating confused metaphysical views in science, one is discussing what to do in order to get close to a theory which fulfills these conditions. On the way, one can have unclear theories even if one thinks that only clear theories can be true; the questions are distinct.

The point made below is that traditionally not only is clarity taken as a condition of truth but clarity of views is taken as a condition of the rationality of dealing with them. The Bacon epithet, that truth is more likely to come

from error than from confusion, is a strong expression of that tradition. From Bacon to Popper, this tradition is held. I understand it was broken by Niels Bohr who acknowledged that confusion regularly and indispensably invades the search for knowledge and that the community of thinkers must continually fight to keep the sea of it at bay, but they cannot hope to have a clean slate ever. I say in the dissertation that the assumption that clarity is easy to come by and that therefore to deal with is a mark of intellectual irresponsibility is false; this allows me to say cultivating confused ideas can be rational. But, I do not have to argue for or against the idea that any true theory must be either clear or confused in order to say that. As I say earlier, I do think all true theories must be clear enough to be understood; but this is hardly controversial, nor very substantial.

There will be limitations on the helpfulness of a confused metaphysics. Some are obvious and traditional--such as the liability in misunderstanding and misdirected or easily rejected criticism--and have been discussed. However, these limitations have been recognized throughout the development of this view of the role of metaphysics in science. And, the role claimed for metaphysics is itself limited in such a way that the special limitations on confused views are not overreached. That is, although the metaphysics may be a program for future science, it is not taken as an authoritative guide--laying out the science; it is seen as a critical dialectical partner to the developing science. Likewise, although one can aim to develop a confused metaphysical view so that it may first be made clear and then testable and then perhaps function importantly in a scientific-cum-metaphysical debate; success in this endeavor is not a condition

for claiming the procedure is rational. Besides, a confused metaphysics may be beneficial in science by becoming clear, but it may be helpful in other ways even when confused. This view of relations between metaphysics and science does not equate success in the advance of science with proper method. Tentative safeguards against seemingly unreasonable risks in methods replace any guarantee for success.

A serious consideration of this view of relations between metaphysics and science would then change one's idea of the development of science significantly. Formerly irrelevant facts in the development of a theory or discipline may be seen as quite central--e.g., Harvey's concern with the microcosm-macrocosm theory. Moreover, it can be explained why these previously considered irrelevant facts so regularly seem to be important to researchers. One need not simply note the fact but explain how their activities are part of a rational procedure. Their difficulties and problems in the area of clarifying and dealing with confused metaphysical views can also be examined and the importance of their struggles evaluated; this will be possible since at least one theory of the importance of metaphysical views to rational progress in science will have to be articulated.

Two words of caution on the view defended here needs to be recorded. First, one need not claim that metaphysics must be part of any rational development in science. The inspiration, selection of problem, a well-defined aim, criteria for criticism, all do seem to be necessary to rationality of science. But, they may be provided without appeal to metaphysical views. Such views are, however, extremely helpful and compelling in many cases. And when they

do perform these functions, they are functioning as part of a rational process.

Secondly, the view put forward here is certainly not so well developed that it can claim to have completely solved the problem of how to explain the role of metaphysics in science. It is not developed in detail but in outline only. The solution it involves affects so many branches of theory of science that the consequences in each would have to be examined carefully. Just one example of a troublesome gap in theory is the suspicion that first making a theory clear, then trying to make it testable, then trying to refute it, may be too vague and indefinite a mission for a researcher to claim the effort is rational. The conditions under which the aim is or is not too vague must be examined more closely; a more detailed review of cases such as Harvey's could illustrate the possibilities of difficulty more clearly than I have been able to do here. Also, other theories of the interaction between metaphysics and science are certainly possible, theories which explain in other ways the recalcitrant incidents and relationships which I have tried to describe and to explain in the pages above.

APPENDIX A: Against Feyerabend's Relativism on Rationality.

The certain methodological views may be improved does not mean no rules may be rationally discussed. This is a conclusion *which* distinguish^{es} the position I favor in the text from that of many commentators. It can be seen to apply to Feyerabend's views on relativism and anarchy as well, as the discussion below of "Problems of Empiricism, Part II" (see Bibliography) illustrates.

Feyerabend is famous for the following view:

each [methodological] demand...can be overthrown, or replaced by its opposite...the only principle we can trust under all circumstances is that anything goes. (277)

Feyerabend's position differs from that argued in the text in that he assumes these alterations in method must be arbitrary and random. He jumps to an irrationalist conclusion from the observation that rules of method--and thus views of rationality--may themselves be tentative and mistaken. That is, he presents the idea that each rule of method can be overthrown;

or replaced by its opposite as the result of an examination of concrete cases (this applies even to such "fundamental" demands as the demand for consistency, for falsification, for agreement with observational results, for maximum content in given conditions, and so on). (277-278)

But, he claims, there are no intersubjective articulable conditions under which the rule would be overthrown; no higher aim by which one would argue that the change of policy was advisable in specificable circumstances. Feyerabend does not exactly argue that such higher standards are impossible; he merely claims

they are because the future is not predictable. This argument finds a place in Lakatos and Toulmin as well:

Nor is it possible to say in advance under what circumstances a rule may be suspended and how one will behave then. The future cannot be predicted (after all, we do learn, we do have new ideas). (278)

Feyerabend--and Lakatos and Toulmin--mean of course that the future and our behavior can not be predicted authoritatively; conditions under which we could change a rule can not be prescribed with certainty. Why they can not be prescribed tentatively and with reference to aims that allow the rules to be open to intersubjective, rational, criticism, is not clear to me. His own recommendations--to surrender the methodological principle to work with the most explanatory hypothesis when the creation of an alternative seems to require development of other views--seems a perfect example of changing a rule in prescribed conditions in accord with a higher aim. The aim in this case is to maintain a critical examination.

Thus, even on Feyerabend's own terms his conclusions do not seem warranted. Above, he says one can not say beforehand when one will need to change methodological principles, yet a page previously he has noted that Lakatos' work shows him that historical cases can be enlightening by being "truly philosophical, being guided by abstract principles and giving rise to them in turn". (277) The principles he is speaking about are theories concerning when it is advisable to give up a particular methodological policy and when not. (See Toulmin's similarly vain hope that one can avoid a-historical

principles concerning when to expand our concepts.

Thus, Feyerabend's analysis supports the kind of rational view of methodology more than it does his own anarchism.

APPENDIX B: Note on Comprehension as Deductive Explanation

The problem of interpreting "understanding" as something less than deductive explanation is a recurring one in this study. One can describe various connections between metaphysics and science--more or less close; one *also* can describe various rules and criteria of argument. However, if the connection with increasing our understanding of the world is not maintained, the rationality of the contribution of metaphysics to science will not appear.

That is, rationality must, it often seems, be tied to an increase in understanding or knowledge--or at least the reasonable hope of such an increase. Usually, such an increase in understanding is understood to consist in explaining phenomena through deductive entailment. However, if metaphysical views contributed to our understanding of the world through being higher level principles that explained phenomena (or other laws) deductively, they would simply be part of science. Metaphysics must contribute to the search for understanding in some other way if it does at all.

Commentators who have argued that metaphysics has a rational role in science have understood the requirement to connect the contribution metaphysical views make in science to an increase in understanding; they have, correspondingly, criticized the idea that understanding (in science or elsewhere) consists only in deductive explanation. Some, they argue, we must make sense of understanding in other ways; for, they argue, it seems clear that comprehension of the world is a broader thing, aided by many more factors, than explanation through deductive entailment.

To give a selective review of such awareness, one might start with one of the more inviting of Feyerabend's suggestions; namely, that his conclusions as to the shaky basis of traditional explanation leads him to believe that understanding in science is not so very different than understanding in poetry or literature; words and arguments have a more symbolic and loose connection with an objective reality than traditional views suppose.

Both Shapere and Hanson clearly argue that other factors beyond those important for deductive explanation directly comprise the rationality of scientific research; but they are not clear as to whether they therefore think understanding itself must not be equated with deductive explanation or they take the other alternative of separating rational procedure from any hope of an increase in understanding the world.

Toulmin certainly is aware that his views involve criticism of the traditional understanding of "understanding". One can cite him explaining that the concept of explanation has changed and is changing--and that indeed it must change. He also regularly points out that coherence is hardly the same as predictive power or even the same as explanatory power. And he tries to outline an idea of explanation in terms of expanding and more powerful concepts rather than strict deductive entailment; the former is a more loose and intuitive notion. It is not clear that he tries to formulate any more elaborate alternative idea of comprehension or understanding than this however. One, that is, that can be understood to be different than explanation by deductive entailment (even if sketchy). He seems to combine attempts to broaden the notion of rationality in science so that it includes more than understanding the world--

but also prediction, practical use, etc. He does, though, say that the creative intellectual value of science is still the highest of the many values it rationally fulfills. Where Toulmin comes out more definitely is hard to say.

Since he accepts fully Popper's link between rationality and learning about the world, Agassi is quite aware that he must link metaphysics to enlightenment in some non-deductive manner if he is to argue that metaphysics qua metaphysics has a rational role in science. Another way to state the problem is to ask what are the aims of science and more specifically can they be articulated in terms other than the production of theories which deductively explain and yet not depart from realism? For if they can not, then one can show that metaphysics plays a role in science only in as much as it is part of such explanations. The alternative Agassi sketches appears in his "Scientific Problems and Their Roots in Metaphysics" cited many times in the text:

The aim of science is to comprehend the world rationally, as we all agree...But this is too vague...As to comprehension, Popper views it as deductive explanation, and he has suggested that explanatory power goes with refutability. I deny that explanation is the only method of comprehension...the attempt to co-ordinate our various explanation within one metaphysical framework is not explanation, yet it is, in some weaker sense, an attempt at comprehension. (199)

Agassi also denies that explanatory power is dependent on refutability and gives examples of theories, metaphysical mainly, which are explanatory to a certain degree and yet not testable.

The point is that this is an attempt at a form of comprehension as "valid"--to use an unsatisfactory word for lack of another--as that gained by

traditional explanation. Agassi supplements this expanded view of realism--and therefore of rationality--in his "Sensationalism" where he argues the pluralist position that there may be many pictures of the world in physics, biology, poetry, psychology and each of them may be realistic in as much as they are critical. (24)

Popper's theory of science itself indicates a new idea of understanding if it is taken in its pure form that one "learns by mistakes": that is, that seeing that one's very explanatory theory simply is not good enough (in some way or other) to capture the reality is what learning amounts to; not the comprehension of a positive theory and its deductive consequences. This aspect of Popper's view of the matter is seen most vividly in Agassi's and Bartley's elaborations where it is clear that understanding is equated with a knowledge of many theories and their limitations and interactions rather than any authoritative statement or fact at all.

NOTES

- 1 This belief in the unit and harmony of truth from all traditions despite outward differences is known in the Renaissance tradition as "syncretism". It is developed at length in Pico's On Being and the One and in his Heptaplus, although one can find it expressed rather than discussed in the famous Oration. These works are published in one volume with an introduction by Paul Miller, (New York: Bobbs-Merrill, 1965). P. O. Kristeller's many essays on Pico may be consulted for the peculiarly neo-platonic connections of the urge toward simplicity and comprehensibility, but Kristeller does not as far as I know discuss the precise methodological import of these new standards.
- 2 Galileo scorns the more mystical side of Pythagorean philosophy in the beginning of the Dialogue Concerning the Two Chief World Systems, trans. by Stillman Drake, (Berkeley: University of California Press, 1962), pp. 10-11.
- 3 One of the more notable places in which Galileo becomes methodological in his attack against the scholastic, Grassi, in the Assayer, published in the collection, Discoveries and Opinions of Galileo, trans. and ed. by Stillman Drake, (New York: Doubleday and Co., 1957), pp. 249-250, 268-269, 273.
- 4 Aristotle and the mystics of the Middle Ages would not see clarity and simple mathematical formulations as equivalent. The Aristotelians connected clarity to common sense; the mystic to abstract mathematical analogies but not mathematical statements of actual views. Berkeley and Burtt also would not see mathematical formulations as synonymous with clear formulations. But, it is to be noted that among these only the Aristotelians might disagree that science, if it is to be valuable, must deal with clear views--by whatever criterion clear views are distinguished.
- 5 Galileo's position must be defended against the criticism that he introduced vagueness because mathematical truths could only apply approximately to actual events. Dudley Shapere notes quite enlighteningly that Galileo was not a platonist with respect to that belief; in fact, that a notable part of his philosophy was his disagreement with Plato on the exact application of mathematics to the world as it stands; Galileo insisted this world was mathematical (geometric) and thus the mathematical theories would describe this world precisely and exactly. E.g., DOG, pp. 237-238. Also, Dudley Shapere, Galileo, (Chicago: University of Chicago, Phoenix Book, 1974), pp. 134-136.

Shapere notes ambiguities in Galileo's position also (p. 137). One must empathize with Galileo's difficulties for it is not easy to be clear on this

relation between mathematization and the standard of clarity in science. Although it seems obvious that there is far less occasion to be confused about the content of claims and meaning of terms in a mathematical theory than in a qualitative view, one cannot overlook the fact that the former often includes unobservable entities and ideal cases.

However, Galileo's preferred type of theory comes out better, it seems, since the fact that perfectly round balls roll down perfectly smooth incline planes matters less in this discussion than that they do so at mathematically precise and simple rates and are interfered with by unideal initial conditions in the same (i.e., mathematically regular) way. Also, one can deflect the criticism concerning unobservable ideal entities by arguing--as both Galileo and Descartes certainly did--that their mathematically characterized entities were actually more amenable to clear description than more traditional things. This is so because the attributes of those entities are simple, and strictly limited and defined. The point is that clarity in the sense of barriers to shifts of meaning and reference was made more important than clarity in the sense of immediate intuitive comprehension--for reasons of intellectual honesty, as alluded to in the note above.

- 6 René Descartes, The Philosophic Works of Descartes, Vol. I, trans. by H. Idane and Ross, (Cambridge: Cambridge University Press, 1970).
- 7 The undeviating and task-like automatic nature of these methods which leave no room for mistakes or working out of difficulties is discussed by Popper in his classic introduction, "On the Sources of Knowledge and of Ignorance", in Conjectures and Refutations, (New York: Basic Books, 1962); Stephen Toulmin also gives a brief and helpful historical review of approaches to the rationality of science which includes a description of Descartes' commitment to clarity in his Human Understanding, Vol. I, (Princeton: Princeton University Press, 1972), pp. 4-5.
- 8 Frances Bacon, The New Organon, Library of Liberal Arts, (New York: Bobbs-Merrill, 1960), Book II, Aphorism XX, p. 156. The strong methodological prohibitions against cultivating such views--having anything to do with them qua scientist--is the point to be noted here, not simply that the desired result is of a type different from "confused views".
- 9 It is not important to decide who was the first to notice that Newton's science changed the necessary conditions which were required of scientific theories from the demand from clarity in terms of mechanical explanation to clarity in terms of non-adhocness and independent test. The debate was vigorous at the time as Koyré reports at length; see his Newtonian Studies, (Chicago: University of Chicago Press, Phoenix Books, 1965).

The change in standard would not have been taken seriously philosophically until the idea of a certain (and unproblematic) method for discovering and/or establishing truth was surrendered. For, given a certain unproblematic method for discovering (identifying or establishing) truth, one attends only to those characteristics of a theory which are sufficient conditions of its truth; no lesser characteristics--such as being a good answer to a question--need be considered. One just goes directly for the truth. Add to this that the sufficient condition for truth was never the confusion of a view but its clarity, and one sees that it is still impossible to consider confused views seriously--as part of the rational methodology of science. See immediately below in the text.

- 10 The method still makes it unnecessary to tackle "confusion" rather than to avoid it. As Descartes' philosophy includes the methodological prohibition against arguing about possibly false views, Newton's philosophy prohibits arguing about possibly confused views. They do not agree on what a clear view is, but each says only clear views are part of science--as a body of theory or as a method.
- 11 Agassi's discussion of Whewell's attitude on metaphysics, which will be reviewed at length in Sec. 4.2 below, includes the note that Whewell was willing to consider even wild ideas as inspirations but also that he only allowed "reasonable" metaphysics this role in science. Joseph Agassi, "Scientific Problems and Their Roots in Metaphysics", in Critical Approach to Science and Philosophy, ed. by Marion Bunge (London: Free Press, Collier MacMillan Co. 1964), p. 195.
- 12 Pierre Duhem, Aim and Structure of Physical Theory, (New York: Atheneum, 1974; also Princeton University Press, 1954), Ch. V., Secs. 2 and 3. There Duhem describes the qualifications and accretions which attach to a developing theory--theory as classification. In the first section of this chapter however, Duhem reports in a complaining manner the conflicting interpretations of given fact and given theory that can result when theoretical interpretations are taken as explanation rather than as heuristic devices; this kind of confusion of claims--and mysterious, realistic references--he is against. His idea of clarity though is a clarity of abstract and formally interpreted mathematics not of coherent interpretation.
- 13 The second of these points, on the epistemological equivalence of science and metaphysics--has less to do with views on clarity and confusion than the former; and, its discussion will be postponed; it is an essential part of the debate however--especially between Kuhn, Feyerabend and Popper.

- 14 This summary is not controversial nor deep; it can be found in several commentators; see for example, Popper's account in CR, Chapter 3, "Three Views of Human Knowledge, pp. 97-120; also, preface to Logic of Scientific Discovery (New York: Harper and Row, 1959), pp. 15-23. In his Historical Introduction to the Philosophy of Science, (London: Oxford University Press, 1972), pp. 184-188; John Losee describes the progress from the verifiability criterion to the program of determining or creating an empirical language from which terms without empirical significance could be eliminated. This is the program Popper criticizes in the preface to LSD; and Losee too, records that many followers of Vienna Circle philosophers recognized in the end that only theoretical systems could be called verifiable, not even isolated theories no matter how restricted the language in which they were formulated. This debate may have started due to a desire to exclude metaphysics from science; but, as it is, it can be settled in favor of the most liberal interpretation of "theoretical terms"--as Popper settled it in fact--without opening the question of the role of metaphysics in science explicitly. See also, Section 1.2, n. 8 above. Ernest Nagel, in the Structure of Science, (New York: Harcourt, Brace and World, Inc., 1961) also describes the Machian program and its translation into "conventionalism", pp. 120-123, footnote on 123 particularly; although he is quite ready to see the difficulties in this research program, he does not mention "metaphysics" nor show any indication that he thinks the difficulties here open a debate on the role of metaphysical views, such as those Burtt and Koyre discuss, in scientific explanations.

- 15 I pretend no expertise nor even elaborate acquaintance with Whitehead's or with Bergson's philosophy; but the stress on intuition and the stream of reality, are, I understand, uncontroversial. See Filmer S. C. Northrop, "Whitehead's Philosophy of Science", in The Philosophy of Alfred North Whitehead, ed. by Paul A. Schilpp, Library of Living Philosophers, (Tudor Publ. Co., 1941 and 1951), pp. 169, 185, 205.

The same summary may be given--and in my view is accurate--of Errol Harris' discussion of relations between science and philosophy; in the main he concerns himself with "scientific metaphysics" in a classical way à la Descartes; he simply disagrees as to what is the metaphysics justified by science and promotes a Whiteheadian process philosophy. Harris, Foundations of Metaphysics in Science, (New York: Humanities Press, 1965).

- 16 CR, Ch. 3. Agassi has referred to the uncritical dismissal of unclear views as the positivistic vice of Popperians--practised, e.g., against the Hegelians. His efforts to describe what would be a critical practise in this area are reviewed in Sec. 4.5. below and form the basis for the view defended in this dissertation. Of course, as noted below, Feyer-

aband also makes this criticism that Popper and his defenders use methodological rules--including those against the cultivation of confused ideas as part of science--uncritically in ways *which* deny opponents legitimacy and which if followed would inhibit creativity and the development of new ideas; his views are not followed here however because he claims they have necessarily irrationalist consequences.

- 17 It may seem unfair to say that Popper prohibits the cultivation of views which are not highly explanatory since he is one of the more liberal methodological commentators and would certainly see that theories must be developed; they do not spring ready made to scientists minds. Developing a bold theory--beginning from less bold or even from confused views--would certainly be within the scope of his views. [See the wonderfully liberal note on the confused calculus, CR, p. 70^{and p. 706b}.] However, as explained below, Popper's concern that science not be stagnant and that one have reason to prefer one theory over a competitor leads him to emphasize high explanatory power over any of these other considerations. See Marx Wartofsky, "Metaphysics as Heuristic for Science", in Boston Studies in the Philosophy of Science, Vol. III, in memory of Norwood Russell Hanson, ed. by R. S. Cohen and Marx Wartofsky, (Dordrecht-Holland and Boston: D. Reidel Publ. Co., 1967), p. 143. Wartofsky points out that Popper's falsification criterion vitiates even his most liberal efforts to credit metaphysical views. Similar point in Agassi, "Science in Flux", in the same volume (BSPS III) as Wartofsky, p. 314.
- 18 Popper, The Open Society and its Enemies, Vol. II., Hegel and Marx. (London: Routledge and Kegan Paul, 1966). n. 30 to Ch. 13, pp. 307-308.
- 19 Ibid., p. 239.
- 20 See his critique of Hegel, of Aristotle, in Chs. 11 and 12 of the Open Society, Vol. II. Also, Sec. XIV of the Introduction and Ch. 3 of CR, as just one discussion of his view that one can cultivate mistakes not evasions in reference to science alone.
- 21 Thomas Kuhn, "Logic of Discovery or Psychology of Research", in Criticism and the Growth of Knowledge, ed. by I. Lakatos and A. Musgrave, (Cambridge: Cambridge University Press, 1970), pp. 5, 9, 13, 16-19. In these places, Kuhn describes the adjustments and shifts of meaning which allow progress in his view. Also, see his Structures of Scientific Revolutions, (Chicago: University of Chicago Press, Phoenix Books, 1962), pp. 77ff, particularly. These passages do not contain explicit discussion of confused views nor of metaphysical views but of theories being considered and modified outside traditional methodological rules.

Is clarity a good thing - both in methodology and in theories? I can answer the questions here directly. I do think both clarity in theories and clarity in methodology is good; I also think that both are reasonable aims but not necessary conditions for good research. (Also in as much as clarity implies being explicit, I hold it is a good thing.) But the major push in the dissertation is not at all a defense of clarity; rather it is a defense of ideas that are not clear. The defense of them in the sense that they are held to be useful, (also unavoidable regularly), and consideration of them as part of research is rational under some conditions.

I would follow Popper and Agassi in arguing that explicitness is good because it allows criticism and examination. The matter is considered a few places in Chapter IV. Some commentators--Kuhn more than Polanyi--intimate that methodological theorizing invites or demands rigidity, but this need not be so--see the Appendix A on Feyerabend at the end of this chapter.

- 22 In some places, these factors are not so vague and unspecificable, rather they are articulated but are dogmas which come with the paradigm; no criticism of them seems possible. For the methodological elements and functions of paradigms, see Kuhn, "LDPR", pp. 15-19 but cf. his "Reply to my Critics", also CGK, in Sec. 5 for rejoinders. Also, SSR, pp. 41ff, and Ch. X. One is not discussing precisely confused metaphysics here as metaphysics and metaphysics which is not intellectually respectable on some traditional standards; that Kuhn hold paradigm shifts to be shifts of metaphysical commitment--and therefore shifts of intellectually unrespectable views--is what puts him on the agenda in this argument; but his views on metaphysics as such rather than confused metaphysics as a special case are discussed below in the text.
- 23 See n. 22 above. Kuhn opens the door to confused metaphysics via irrefutable metaphysics supported by psychological commitment; but he does not discuss the former explicitly. Nor does he have a consistent theory of how any of these traditionally unacceptable practices work; see Wartofsky, "MHS", p. 149. In his "RC", Kuhn defends himself from the charge of irrationalism saying partly that he doesn't understand the charge and he relates his procedures to fruitful research and therefore is as concerned with rationality as anyone, pp. 234-235, 264, and Sec. 2. For the intolerance to new ideas--confused or not--SSR, p. 24. Kuhn disdains some of Feyerabend's attempts to interpret his philosophy as an invitation to wild views in his "RC", pp. 234-235.
- 24 J. O. Wisdom, "On the Nature of Normal Science", in The Philosophy of Karl Popper, Vol. II., ed. by Paul A. Schilpp, The Library of Living Philosophers, (LaSalle, Illinois: Open Court, 1974), p. 829. This article is particularly enlightening on the issues of how much irrationality is necessitated by Kuhn's view.

- 25 Paul K. Feyerabend, "Problems of Empiricism", in Beyond the Edge of Certainty, ed. by R. Colodny, (New York: Prentice Hall, 1965),
- 26 Feyerabend, "Problems of Empiricism, Part II", in The Nature and Function of Scientific Theories, ed. by R. Colodny, (University of Pittsburgh Press, 1970), pp. 277-278.
- 27 Ibid., pp. 277-278; on Feyerabend for the arguments which distinguish his position from that argued in the dissertation: Feyerabend, like a few others *who* are liberal toward metaphysics, becomes irrational concerning science as an enterprise. I must qualify *this* conclusion with a note on Feyerabend's intriguing and charming views on the value of viewing scientific theory as poetry and dream; these more charming and responsible experimental suggestions which occur in some of his papers seem worth considering. See his "Knowledge Without Foundations" lectures, Oberlin College Lecture Series; also, "On the Improvement of the Sciences and the Arts and the Possible Identity of the Two", in BSPS, Vol. III, pp. 387-415. Wartofsky develops a similar view when he argues that metaphysics is enlightening because it resembles the primitive form of understanding, the "story"; "MHS", pp. 156ff. His development is not so clear, but again intriguing from the point of view of this discussion. This position is not mentioned in Appendix B on alternative forms of comprehension but concerns the same point.
- 28 "PE--11", p. 323.
- 29 Ibid., p. 329.
- 30 Ibid., n. 35 and 165 particularly; and p. 323., where Feyerabend suggests that because Galileo's view incorporates a new metaphysics he must insinuate it rather than argue it.
- 31 Wartofsky, "MSH", pp. 131-138. This historical sketch includes the origins of positivism, its Marxist-materialist developments, and pragmatist views on metaphysics. Also, Agassi, "SPRM", pp. 192-196; Popper, CR, Ch. 8, for Kant's position particularly. Also, for a more traditional approach to classical views on science and philosophy, Gerd and Buchdahl, MPS, and Sec. 1.3b, 1.3c above.
- 32 For good reasons; see the discussion of comprehension as something other than deductive explanation below, Appendix B; Toulmin and Hanson take the difficulties in basing a rational relationship on a logical relationship to indicate that rationality is simply not a logical relationship: See Sec. 4.3.
- 33 Agassi, "SPRM", pp. 194-195.

- 34 *Ibid.*, p. 195.
- 35 Peter Caws, "The Structure of Discovery", Science, Vol. 166, Dec. 1969. pp. 1375ff. Also the discussion of Toulmin and Hanson below.
- 36 Popper's additional attempt to reincorporate metaphysical positions into science as methodological imperatives is discussed in the section concerning him, below.
- 37 See Duhem's extremely interesting--and it seems to me largely⁶ ignored--discussion on the fact that no metaphysical theory gives detailed enough instruction to develop a scientific view and conversely that all scientific theories need and incorporate views independent from the metaphysics by which they are largely interpreted: AS, Ch. 1, Sec. 5.

Agassi notes the same fact about metaphysical views in his discussion on the inadequacies of metaphysical frameworks as research programs; "SPRM", p. 205. Agassi makes the interesting comment that although *pro*-metaphysicians as well as anti-metaphysicians often claim that any scientific view a theorist will develop will conform to his metaphysics, this bold claim on behalf of the power of metaphysical frameworks is often false. The independent scientific imagination often stretches out the capabilities of the metaphysics. These comments by Duhem and by Agassi, are not, it seems to me, fully enough appreciated by Kuhn and Feyerabend when the latter discuss the inescapability of metaphysical commitments.

- 38 Duhem was positively antagonistic to any connection, even an inspirational one, between metaphysics and science; but Michael Polanyi and Kuhn are less afraid of realistic metaphysics because, it may be argued, they are less shy of dogmatism; they freely admit metaphysics as inspiration. Wartofsky mentions Kuhn's debt to Polanyi for the idea of metaphysics as "heuristic vision"; "MHS", p. 149 and n. 29, in which Wartofsky quotes a discussion by Polanyi in Scientific Change, ed. by Alistair Crombie, (New York, 1963), p. 375.
- 39 "Clandestine" instances are instances taken to reveal, by hints, important truths--e.g., the disappearance of a plane in the Bermuda Triangle may seem as a clandestine instance of the operation of a non-physical destructive force. Agassi, "SPRM", pp. 203-204.
- 40 *Ibid.*, p. 204.
- 41 *Ibid.*, p. 205.

- 42 Kuhn insists that metaphysical positions and scientific stances are "ideologies" which suggests that they may not be consistent or clear structures even internally; see, "LDPR", pp. 15, 16 and n. 1 to p. 9, where Kuhn notes that a theoretical system is not deductively organized--a point he makes regularly. How far he would push this and still be able to maintain its binding character--in setting puzzles and requiring assent--is not at all clear.
- 43 For examples of perfectly rational methodological functions of metaphysics as background knowledge--perhaps of an inadequate nature--see "LDPR", pp. 7, 9, where the similarity between severe testing and accepting problems dictated by the paradigm is mentioned. And, in many instances Kuhn describes the functioning of the paradigm as pinpointing trouble spots and making intersubjective criticism possible, as Agassi and Bartley argue metaphysical frameworks do as well. Also, see Wartofsky, "MHS". For the idea that the participants have no control--they are governed by the elite and the elite by psychology--one can consult many chapters in his books. The paradigm is used heuristically; but if it were not necessary to "believe" it, Kuhn would not stress unanimity to the point of blackballing those who do not express belief. See "LDPR", p. 21; SSR, p. 5.
- 44 Agassi, "Sensationalism", Mind, n.s., 75, 1966. pp. 1-24. And several other articles detailed below; Agassi is a declared skeptic and accepts the strong version of Duhem's argument stated in the text. W. W. Bartley, "Theories of Demarcation Between Science and Metaphysics", in Problems in the Philosophy of Science, ed. by I. Lakatos and A. Musgrave, (North Holland Publ. Co., 1968). Bartley's position is not so clear; he denies justification and skepticism at the same time and is thereby hard to place. However, his objection to the need for any "commitment" is clear enough; See his Retreat to Commitment, (New York: Alfred A. Knopf, 1962), particularly Ch. IV.
- 45 Discussions concerning the implications of Duhem's position abound as the literature on the Duhem-Quine argument shows. One distinction concerning the argument should be outlined here however; namely, the view that if a statement is derived from a theoretical system, and then is found to conflict with observation report, the theory from which it has been derived need not be considered refuted because ad hoc or auxiliary hypotheses may be added to deflect or remove the criticism. This view is somewhat confused in itself since although an ad hoc hypothesis which weakens one of the premises may deflect criticism, no auxiliary position which might be discovered--and discovered to be false--can be expected to do this. For, the statement which has been found to conflict with observation report has been derived from the premises already present; one of them must be false if the logical connections and the conflicting observation reports are accepted. Altering the theoretical system in the way described is a far different procedure from choosing to direct a refutation at one premise of

the original statement of a theory rather than another.

Also, see Bartley, RC, pp. 1165-171 for a discussion of his objections to Quine's changes in Logic which are not considered here.

- 46 Popper, "On the Status of Science and of Metaphysics", CR, Ch. 8. For an example of how this view is now seen to be odd, see William C. Kneale, "The Demarcation of Science", in PKP, pp. 206-207.
- 47 Popper, The Logic of Scientific Discovery, (New York: Basic Books, 1959), Secs. 6 and 15, where Popper links the asymmetry very strongly to the advisability and special point of falsification but not to its possibility. The theory becomes important when the confusion in n. 45 are exploited, as they often seem to be by Kuhn; see Wisdom, "NNS", p. 828.

See also, Bartley, "Rationality vs. The Theory of Rationality", in CA, pp. 24-29.
- 48 Bartley, "TSM", pp. 42-43. However, Bartley confuses this issue, in my view, by suggesting that these liabilities--which he calls "criticism reducing stratagems" are also built into theories.
- 49 Bartley, "RTR", pp. 26-28.
- 50 Agassi, "The Novelty of Popper's Philosophy", Intern. Phil. Quart., Vol. III, No. 3, Sept. 1968, p. 461; also his, "SF", pp. 310-311, where he quotes Popper, CR, Ch. 10, Sec. 5. Wisdom, "NNS", pp. 827-828 passim. Bartley, "RTR", p. 29.
- 51 Kuhn, Discussion, Ithaca, Proc. IX Intl. Cong. Hist. Sci., 1964, p. 248.
- 52 Wattofsky, "MHS", p. 145; also below where the lack of clarity in Agassi's view on why science and metaphysics are not identified is discussed. His "empirical scientific" outlooks are important.
- 53 This is somewhat puzzling because Popper is usually considered one of the theorists who stress logical relations--although Kuhn, for one, recognizes him as one of the commentators interested in historical considerations, in the "practise" of science rather than the abstract and sometimes sterile formalizations of structure. Kuhn, "LDPR", p. 1.
- 54 Feigl, Braithwaite, and Popper argue that explanation via deductive relation is the sine qua non; they are often unclear as to how valuable other aspects of scientific activity are in terms of rationality but often their remarks about explanation only make sense if they are interpreted as restrictive.

- 55 Peter Caws' comment on Harre's book (Synthese, 25, 1972, pp. 248-253), also is related to the function of science in giving us better understanding. Of course, this is a factor of rationality--the connection with learning about the world--which may not be considered necessary by many theorists; but it is accepted here as part of the definition of rationality without prejudice to the argument that rationality in this sense may be impossible to achieve or that another view of rationality could be articulated and defended. See Appendix B below for a brief discussion of some ambivalence concerning the connection between rationality and learning. It should be noted also that Popper's view that rationality is linked to learning about the world is one of the more favored parts of his theory of science; see Agassi, "NPP", p. 460.
56. Hanson, "The Idea of a Logic of Discovery", What I Do Not Believe and Other Essays, ed. by Stephen Toulmin and Harry Woolf, (Dordrecht-Holland: D. Reidel Publ. Co., Humanities Press, 1971), Ch. XVII, pp. 288-302. One can add, following Popper's or Bartley's theory of rationality, that Hanson could claim that such principles are far more articulable and intersubjectively debatable than they ^{were} ought to be and so contenders as part of rational procedure. Also, Hanson, Patterns of Discovery, (Cambridge: Cambridge University Press, 1972), pp. 85-87. See Hanson's discussion in the Ithaca volume, , p. 240, where he argues that such principles are not "subjective" and contrasts his view with R. S. Cohen's and Toulmin's.
- 57 Hanson, "ILD", pp. 289-290.
- 58 *Ibid.*, pp. 299-300; also pp. 293-294. Dudley Shapere also discusses "reasons in favor of a search for such a theory", and he says the fact they were "not logically conclusive did not make them any the less reasons--and good ones, relative to the science of the time--nor did it make action in accordance with them any the less rational"; Shapere, "Discovery, Rationality, and Progress in Science", PSA, 1972, BSPS, Vol. XX, ed. by Kenneth Schaffner and R. S. Cohen, (Dordrecht-Holland: D. Reidel Publishing Co., 1974), p. 409.
- 59 *Ibid.*, pp. 293-294. The Ithaca discussion also shows this for here Hanson argues that he is spelling out the "logic" of discovery as "inductive" discovery and suggests that Keynes gives the clue as to how this may be done. pp. 240-242.
- 60 "ILD", p. 300.
- 61 *Ibid.*, p. 300.

- 62 *Ibid.*, p. 300. Toulmin agrees that the reasons for proposing certain views deserves more credit than simply that they are "highly explanatory"; Toulmin, "Scientific Strategies and Historical Change", in Philosophical Foundations of Science, BSPS, Vol. XI, ed. by R. T. Seeger and R. S. Cohen, 1974, pp. 410, 412-413.
- 63 Hanson, "Is There a Logic of Discovery", in Readings in the Philosophy of Science, ed. by Baruch A. Brady, (New Jersey: Prentice-Hall, 1970), p. 632.
- 64 *Ibid.*, p. 632. This is a curious conclusion for a commentator as adventurous as Hanson. For it is clear that a theory must be an answer to a problem, be based in a tradition, and meet the criticism of previous answers to be "validated". Toulmin, Kuhn, Lakatos--as well as the Popper, Agassi, and Bartley--all stress this point; many quote Collingwood and Pierce as sources of the criteria. It seems strange that Hanson underestimates them.
- 65 Toulmin, HU, p. 478.
- 66 *Ibid.*, p. 43.
- 67 *Ibid.*, p. 480.
- 68 *Ibid.*, p.
- 69 Toulmin, "Rationality and Scientific Discovery", PSA, 1972, BSPS, Vol. XX., pp. 394-395, (Par. 2.2); Also p. 391, (Par. 1.22). This article is an "abstract" of HU.
- 70 *Ibid.*, p. 391. The real point in both considerations of adequacy of concept and true explanation is to give more role or credit to the usefulness of incomplete or inadequate concepts or explanations. Toulmin is sometimes sensitive to this other times not. Arthur Collins notes the need to have room-or recognition--for inadequate or incomplete explanation in his article, "Explanation and Causality", Mind, Vol. LXXV., n.s., No. 300, Oct., 1966.
- 71 Toulmin, HU, pp. 480, 484.
- 72 In Agassi's interpretation of Popper's view, one is not forced to accept any view because it is explanatory; the emphasis is on criticism and rejection; "NPP", pp. 461-462. Popper doesn't agree and his stringency, according to Toulmin (and I agree), invites "Feyerabend's irrational anarchistic response"; HU, p. 481.

- 73 Toulmin, HU, pp. 280, 480, for higher functions.
- 74 Ibid., pp. 480ff. and Foresight and Understanding, (An Inquiry into the Aims of Science), (New York: Harper Torchbooks, 1961), p. 38. There, Toulmin also speaks only of expanding science through "intellectual creation" not through selection and criticism. There are clear similarities between this and Feyerabend's wish to let a thousand flowers bloom.
- 75 Without a connection between advocated scientific procedures and increased understanding, a role for confused metaphysics may be generated but turns out to be empty.
- 76 Toulmin, HU, p. viii.
- 77 Ibid., p. 484.
- 78 Ibid., p. 478; see also criticism of Feyerabend in Sec. 4.1 above and Appendix A below for a criticism of his relativism on just this point.
- 79 Ibid., p. 51.
- 80 Ibid., p. 45 (1); There is an interesting note in HU on the need to vary procedures given historical circumstances: pp. 21, 34.
- 81 Lakatos, "History of Science and Its Rational Reconstructions" in PSA 1970, BSPS Vol. VIII, (In memory of Rudolph Carnap), 1971, pp. 92-136. See HU for Toulmin's own criticism of Lakatos on this point: p. 483, and n.1 to 482.
- 82 Toulmin, HU, p. 485; "RSD", p. 406.
- 83 Popper, LSD, pp. 81-87, 131.
- 84 As noted, Popper does represent the development of the calculus from a confused to a coherent view as bold--and as an incident which is an argument for tolerance; but, the calculus is not science nor metaphysics but mathematics. Still his attitude is hopeful.

"...many people talk nonsense; and it is conceivable that it should become one's task (an unpleasant one) to unmask somebody's nonsense, for it may be dangerous nonsense. But I believe that some people have said things which were not very good sense, and certainly not very good grammar, but which were all the same highly interesting and exciting, and perhaps more worth listening to than the good sense of others. I may mention the differential and integral calculus which, especially in its early forms, was no doubt completely

paradoxical and nonsensical by Wittgenstein's (and others) standards; which became, however, reasonably well founded as the result of some hundred years of great mathematical efforts; but whose foundations even at this very moment are still in need, and in the process, of clarification. ...had there been a Wittgenstein to use his weapons against the pioneers of the calculus, and had he succeeded in eliminating their nonsense where their contemporary critics (such as Berkeley who was fundamentally right) failed, he would have strangled one of the most fascinating and philosophically important developments in the history of thought." (p. 70).

Popper reports the service of atomism over 2000 years but in a Chapter, "The Nature of Philosophic Problems and Their Roots in Science" which is designed to argue that 1) there are real philosophic problems not simply linguistic puzzles; and 2) that these problems are real in as much as they become scientific. (p. 82). When Popper argues against Mach's rejection of atomism, he does not argue against the rejection of atomism qua metaphysics but atomism qua scientific conjecture. (p. 100). See Agassi's similar summary of this chapter, "SPRM", p. 200.

- 85 Agassi, "SPRM", pp. 196-198.
- 86 Popper, LSD, pp. 60-62, 252-254.
- 87 Agassi, "SP", p. 314.
- 88 Feyerabend mixes the helpfulness or even necessity of an alternative theory or metaphysics with the "impossibility" of criticism outside the framework; he implies that the latter indicates the impossibility of rational criticism whereas the former indicates how it may be done. Bartley emphasizes the function of metaphysics--established and alternative--as aids to criticism and notes that Feyerabend's point shows metaphysics is important in science and needed constantly for that reason; the point is anti-Kuhnian: "TDSM", p. 52(1).
- 89 "SPRM" and "SF", and "The Confusion Between Physics and Metaphysics in the Standard Histories of Science", in Ithaca.
- 90 I note ~~this~~ because Agassi says high explanatory power is unacceptable and unnecessary from the point of view of Popper's own theory--of science or of rationality as critical debate--his criticism differs from that of the many others, by Toulmin, Hanson, Shapere, of this idea. Toulmin, "RSD", p. 407--for the multiple aims of science.
- 91 "SPRM", p. 200 and see Appendix B; and "SF", p. 318.

- 92 "SF", p. 318.
- 93 "SF", p. 202. Wartofsky argues that metaphysical frameworks restrict conditions of reference so that criticism becomes responsible; "MHS", p. 155. Toulmin HU, p. 210, against fear of corrupting rationality in this way.
- 94 "SF", p. 318.
- 95 "SF", p. 318, for Talmudic fallacies
- 96 "SPRM", p. 189.
- 97 *Ibid.*, p. 189.
- 98 *Ibid.*, p. 191.
- 99 *Ibid.*, p. 191. Harris also reports the anti-metaphysical tradition; he notes that Feyerabend is inconsistent in seeing metaphysics as both vague and irrefutable but helpful--and says there is no need for the positivist ban; Errol E. Harris, "Methods and Explanation in Metaphysics", in The Problems of Scientific Realism, ed. by Edward A. Mackinnon, (New York: Appleton-Century, Crofts, 1972), p. 160.
- For the most part Harris argues that metaphysics is theory of a broader scope than the usual scientific view; but he does add occasionally that metaphysical views contribute to criticism of concepts and their applicability, so that metaphysics is "revisionary"; p. 172. This makes him similar to Agassi in seeing metaphysics obliged to take science into account to be rational and being an "interpretation of interpretation", p. 173. To Harris, however, it is empirical and verifiable in the same way as science.
- 100 "SPRM", p. 193.
- 101 See n. 39 above and text there for Agassi on "conforming instances".
- 102 "The Confusion Between Physics and Metaphysics in The Standard Histories of Science", in Ithaca; p. ; also "Sensationalism", p. 24: Science is the attempt to explain the existing picture of the world, but this attempt is not based on the adoption of this picture [contra Burtt and Kuhn], rather it leads to changes of the picture." And notes on the inadequacy of metaphysical frameworks, "SPRM", p. 206. Harris, p. 172.
- 103 "SPRM", pp. 192-193. Wartofsky, in his pro-metaphysical paper, mentions "bad" metaphysics without mentioning confused metaphysics specifically; but he characterizes bad metaphysics as vague, muddled, or inconsistent

characterization of entities, and this is good enough; Wartofsky does not comment on any special problems this metaphysics might lead to., "MHS", p. 154.

104 "SPRM", p. 193; also "CPM", p. 232.

105 Dudley Shapere makes the same point; i.e., that when a problem is important, it may be rational to attempt to handle it even, as he says, though science may not be ready: "RSD", p. 414. Also Toulmin is particularly sympathetic toward decisions made in the research moment; see "SSHC", pp. 412-413.

LIST OF ABBREVIATIONS

Abbreviations have been formed from the sequence of capital letters of the main words in the title. Those designating books are underlined; those designating articles are enclosed in parentheses.

<u>AR</u>	<u>Astronomical Revolution</u> (Koyré)
<u>ASPT</u>	<u>Aim and Structure of Physical Theory</u> (Duhem)
<u>BJHS</u>	<u>British Journal for the History of Science</u>
<u>BJPS</u>	<u>British Journal for the Philosophy of Science</u>
<u>BSPS</u>	<u>Boston Studies in the Philosophy of Science</u>
<u>CA</u>	<u>The Critical Approach to Science and Philosophy</u> (Bunge, ed.)
<u>CB</u>	<u>The Circulation of the Blood</u> (Harvey)
<u>CGK</u>	<u>Criticism and the Growth of Knowledge</u> (Lakatos, Musgrave, eds.)
"CPM"	"The Confusion Between Physics and Metaphysics..." (Agassi)
<u>CR</u>	<u>Conjectures and Refutations</u> (Popper)
<u>DG</u>	<u>De Generatione</u> (Harvey)
<u>DMC</u>	<u>De Motu Cordis</u> (Harvey)
<u>DOG</u>	<u>Discoveries and Opinions of Galileo</u> (Drake, ed.)
<u>EG</u>	<u>Études Galiléennes</u> (Koyré)
<u>EO</u>	<u>The Edge of Objectivity</u> (Gillispie)
<u>FCW</u>	<u>From a Closed World to an Infinite Universe</u> (Koyre)
<u>FMS</u>	<u>Foundations of Metaphysics in Science</u> (Harris)
<u>FU</u>	<u>Foresight and Understanding</u> (Toulmin)
<u>GBHT</u>	<u>Giordano Bruno and the Hermetic Tradition</u> (Yates)

<u>GSI</u>	<u>The Growth of Scientific Ideas</u> (Wightman)
<u>HCCA</u>	<u>Hasdī Crescas' Critique of Aristotle</u> (Wolfson)
"HHWB"	"Has Harvey's Worldview Been Rescued..."(Lindholm)
"HS"	"History of Science" (Kuhn)
"HTRS"	"The Hermetic Tradition in Renaissance Science" (Yates)
<u>HU</u>	<u>Human Understanding</u> (Toulmin)
<u>IESS</u>	<u>International Encyclopedia of the Social Sciences</u>
"ILD"	"The Idea of a Logic of Discovery" (Hanson)
"LDPR"	"Logic of Discovery or Psychology of Research" (Kuhn)
<u>LDS</u>	<u>Logic of Scientific Discovery</u> (Popper)
<u>LWH</u>	<u>The Life of William Harvey</u> (Keynes)
"MHS"	"Metaphysics as a Heuristic for Science" (Wartofsky)
<u>MM</u>	<u>Metaphysics and Measurement</u> (Koyré)
<u>MPS</u>	<u>Metaphysics and the Philosophy of Science</u> (Buchdahl)
<u>MSPS</u>	<u>Minnesota Studies in the Philosophy of Science</u>
"NNS"	"The Nature of Normal Science" (Wisdon)
"NPP"-1	"The Novelty of Popper's Philosophy" (Agassi)
"NPP"-2	Newton and the Pipes of Pan" (McGuire and Rattansi)
<u>NRRS</u>	<u>Notes and Records of the Royal Society</u>
<u>NS</u>	<u>Newtonian Studies</u> (Koyré)
<u>OKS</u>	<u>On the Kabbala and Its Symbolism</u> (Scholem)
<u>OMS</u>	<u>Origins of Modern Science</u> (Butterfield)
"ONNS"	"On the Nature of Normal Science" (Wisdon)

<u>OSE</u>	<u>The Open Society and Its Enemies</u> (Popper)
"OSK1"	"On the Sources of Knowledge and of Ignorance" (Popper)
"OSSM"	"On the Status of Science and of Metaphysics" (Popper)
"PC"	"The Philosophy of Circles" (Pagel)
<u>PD</u>	<u>Patterns of Discovery</u>
"PE"	"Problems of Empiricism" (Feyerabend)
"PE--II"	"Problems of Empiricism--Part II" (Feyerabend)
<u>PKP</u>	<u>Philosophy of Karl Popper</u> (Schilpp, ed.)
<u>PPS</u>	<u>Problems in the Philosophy of Science</u> (Lakatos, Musgrave, eds.)
"RC"	"Reply to my Critics" (Kuhn)
<u>RC</u>	<u>Retreat to Commitment</u> (Bartley)
"RTR"	"Rationality vs. the Theory of Rationality" (Bartley)
"RSD"	"The Rationality of Scientific Discovery" (Toulmin)
<u>SC</u>	<u>Scientific Change</u> (Crombie, ed.)
"SD"	"The Structure of Discovery" (Caws)
"SF"	"Science in Flux" (Agassi)
<u>SF</u>	<u>Science in Flux</u> (Agassi)
<u>SHAP</u>	<u>A Short History of Anatomy and Physiology</u> (Singer)
"SPRM"	"Scientific Problems and Their Roots in Metaphysics" (Agassi)
"SSHC"	"Scientific Strategies and Historical Change" (Toulmin)
<u>SSR</u>	<u>Structure of Scientific Revolutions</u> (Kuhn)
"TDSM"	"Theories of Demarcation between Science and Metaphysics" (Bartley)
<u>THS</u>	<u>Towards an Historiography of Science</u> (Agassi)

<u>TSP</u>	<u>To Save the Phenomena</u> (Duhem)
"UD"	"Unity and Diversity" (Agassi)
"VP"	"Vesalius and Paracelsus" (Pagel and Rattansi)
<u>WH</u>	<u>William Harvey</u> (D'Arcy Power)
<u>WHBI</u>	<u>William Harvey's Biological Ideas</u> (Pagel)
<u>WHCB</u>	<u>William Harvey and the Circulation of the Blood</u> (Witteridge)
<u>WHE</u>	<u>William Harvey Englishmen</u> (Franklin)
"WHLS"	"William Harvey and the Life of Science" (Lindholm)
<u>WHLT</u>	<u>William Harvey, His Life and Times</u> (Chauvois)
"WHPS"	"William Harvey and the Philosophy of Science" (Passmore)
"WHR"	"William Harvey Revisited" (Pagel)
"WWH"	"Works of William Harvey, M.D." (Willis-Harvey)

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