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SCIENCE AND METAPHYSICS

An explication of the role of metaphysical principles in scientific inquiry.

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DECLARATION

This thesis, except where acknowledgment is made in the text, is my own work. No part of the thesis has been submitted for any other degree.

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This thesis is largely a study of Collingwood's theory of metaphysics: not a scholarly study concerned with the careful exposition and explication of Collingwood's theory, but a critical study aimed at determining how much of Collingwood's theory is sound, or nearly sound, as an account of the relation of metaphysics to science.

The main theses of Collingwood's theory - which I expound in chapter two - are these:

First, metaphysical principles are not eliminable from science, as the positivists claimed, but enter science as presuppositions of the questions which scientists ask and strive to answer.

Secondly, the metaphysical presuppositions of science are not constant, but change in the course of scientific development, though not as a result of scientific or philosophic criticism of previously accepted presuppositions.

Thirdly, the presuppositions of science cannot be ascribed a truth value, are not propositions, and cannot be justified either empirically or a priori.

Fourthly, the study of metaphysics is possible only as a historical science, concerned with the discovery of the presuppositions...
operative in any scientific epoch, and with the description of the processes by which they change.

In the third and fourth chapters I try to determine in what sense it could be claimed that scientific inquiry has metaphysical 'presuppositions'. An explication of the notion of 'presupposition' leads me to the conclusion that Collingwood was mistaken in supposing that metaphysical principles are logically presupposed by scientific questions. Something is logically presupposed by a question, I suggest, only if it is logically implied by every admissible answer to that question. But Collingwood held, as I do, that metaphysical principles are not logically implied by scientific theories. I suggest, however, that metaphysical principles might be found a useful, if not indispensable, role in science as generators and co-ordinators of scientific questions—i.e., that they might function as regulative principles in science.

In order to evaluate this last suggestion I turn, in chapters five and six, to the problem of demarcation, seeking to develop a means of distinguishing between scientific statements and metaphysical statements. The criterion I defend is a modified version of Ayer's definition of verifiability. Having obtained a demarcation criterion I go on to consider, in chapter seven, whether
metaphysical statements can be sufficiently relevant to science to serve fruitfully as regulative principles. I suggest that they can, and offer an account of how it is possible for a metaphysical principle to 'generate' scientific questions. But my account leads to the rejection of Collingwood's claim that metaphysical principles are not true or false.

In the final chapter I take up the problem of the critical appraisal of metaphysical principles, and argue, as against Collingwood, that there can, to a limited extent, be both a priori and empirical arguments about the acceptability of competing metaphysical principles.
Note on References.

The following abbreviations have been used in footnotes:

A. R.G. Collingwood, An Autobiography
E.M. " " An Essay on Metaphysics
I.H. " " The Idea of History
D. Alan Donagan, The Later Philosophy of
R.G. Collingwood

References to these works are given in the form E.M. p. 10

References to philosophical journals employ the following abbreviations.

A.P.Q. American Philosophical Quarterly
A.J.P. Australasian Journal of Philosophy
B.J.P.S. British Journal for the Philosophy of Science
P.Q. Philosophical Quarterly
P.R. Philosophical Review
For some time now logical empiricism has been a dominant movement in the philosophy of science. It is distinguished by two demands which it makes of any adequate philosophy of science. The first is that the structure of theories and their functioning in science should be analysed within the conceptual framework of modern formal logic. The second demand is that the terms and sentences of science should have their meaning explicable, by means of formal logic, from a base of observation language. Both these demands have led to a concentration of the attention of philosophers on the hypothetico-deductive theory of explanation and confirmation, and so they have lead to a concentration on the conception of theories as empirically interpreted formal systems. But this, it is often suggested, means that philosophers of science tend to give their attention to finished physical systems, like planetary mechanics or classical thermodynamics, rather than to 'unsettled dynamic research sciences', like microphysics. And since science is essentially a growing changing organism, these philosophers, it is said, have given us an unrealistic and distorted picture of science.
The charge against the logical empiricists has been put in just these terms by N.R. Hanson in his *Patterns of Discovery*. Similar discontent is expressed by Stephen Toulmin, for example in *Foresight and Understanding*, and by Thomas Kuhn in *The Structure of Scientific Revolutions*. In their work one may distinguish the emergence of a new tradition in the philosophy of science, one in which the analysis of historical case studies takes the place occupied by formal logic in logical empiricism. It would not be inappropriate to describe them as 'historicist' philosophers of science.

In part the objection of historicist philosophers is to the severely logistic approach of H.-D. Theorists. Scientific theories are treated as interpreted formal systems. But it often suggested that a scientific theory admits deductive ordering only once it has reached a finished and stable condition. Theories in a growing science - themselves changing, open-ended and largely programmatic - cannot be ordered with deductive rigour without the loss of their fertility. A certain fluidity is essential to them.\(^1\) In part too the objection is to the prescriptivism of logistic theories. Some

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prescriptive component of course is inevitable in the logical analysis of science; what interests the philosopher is good scientific practice, and it is his task to lay down standards for the logical appraisal of scientific work. But philosophy cannot be wholly prescriptive, it has to do justice to actualy scientific practice: 'the critical questions which a philosopher brings to science need to be co-ordinated with factual studies of history'.\footnote{Stephen Toulmin, \textit{Foresight and Understanding}, p. 16.}

And here it is suggested that logistic theories fall down. They do not analyse the structure of actual scientific theories but set up ideal types, which actual theories may satisfy only after 'logical reconstruction'. What is lost in the reconstruction, and left unexamined by R-D. theorists, are the dynamic properties of live theories.

One thing that is widely agreed upon among the critics of logical empiricism is that philosophers should devote some attention to the 'context of discovery', that they should examine the process of theory formation as well as the use and testing of theories once formed. Logical empiricists have usually held that this is not a fruitful field for philosophical inquiry. The study of theory formation, it is said, must either be concerned with the psychology of creative imagination or seek to propound a logic of discovery,
i.e. a logical method for discovering new theories. The latter, they claim, is unattainable - 'there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process' - and the former inquiry lies outside the proper domain of philosophy. But this view has come increasingly into doubt. Some, like Hanson, claim that a logic of discovery, or 'abduction', is attainable, at least as an explication of forms of reasoning that may lead a scientist to suppose that successful theories for certain problematic phenomena will be of one sort rather than another. More frequently, however, the view is that something less than a logic of discovery should be sought; that there should be a critical scrutiny of patterns of plausible reasoning in science, of the use of analogy and of the role of regulative principles in inquiries leading to theory. It is not expected that we should find a single and coherent system of rules for theory formation, but a diversity of guiding principles and preferences, not always the same set adhered to by each theoretician. The absence and unattainability of a rigorous system of abductive logic does not

mean that logical scrutiny is out of place in the study of theory formation, but it does mean that an historical rather than formal approach is appropriate. What is needed is the critical study of cases.

Associated with this interest in theory formation has gone an interest in the study of scientific movements. The work of scientists is often done under the influence of a general movement, such as mechanism or behaviourism, which guides research in certain directions, helps to set the problems that should receive attention, and restricts the range of theories that may be considered plausible. The constitution of such movements, the manner in which they influence scientific research, and the conditions under which they change, are topics that have received little attention among logistic philosophers of science. But a few historians, and philosophers with an eye to the history of science, have recently been investigating these topics. Thomas Kuhn and Stephen Toulmin are the most conspicuous among them. I suppose it might be claimed that the study of intellectual movements is properly the business of the historian, and offers little scope for philosophical activity. But I think this response would be mistaken. The historical investigation of intellectual movements has already produced conceptual innovations which challenge the attention of philosophers.
For one thing the concepts that have been introduced in the attempt to understand intellectual movements, concepts like Kuhn's 'paradigms' and Toulmin's 'ideals of natural order', are lamentably vague and loose in use. The conceptual tools of trade of the new historiography of science urgently want critical discussion. And secondly, historians like Kuhn have claimed that the discoveries that have been made in the study of scientific movements are going to produce 'a decisive transformation in the image of science by which we are now possessed', one that will upset many of the cherished doctrines of logical empiricism.¹

One thing that has been brought into prominence by these inquiries is the role of dogmatic and metaphysical elements in the growth of science. Kuhn has laid stress on the function of dogma in science: emphasizing the importance of prior commitments, and the theory-saving activities of normal scientific research, rather than the critical, theory-testing attitude demanded by most philosophers of science.² The role of metaphysics is not stressed by him, but metaphysical elements seem to be involved in the constitution of the paradigms which in Kuhn's view give coherence to a scientific movement or tradition.³ In the case of Toulmin

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³ cf. The Structure of Scientific Revolutions, p. 123.
the role of metaphysics is more prominent. His 'ideals of natural order' come very close to being what philosophers of an earlier generation would have called 'metaphysical presuppositions' - as Toulmin himself remarks.\(^1\) They are not empirically testable, not at least in any straightforward sense.\(^2\) And they do not have an explanatory function in science, but 'govern and direct' the construction of theories, both through the identification of 'phenomena' needing explanation, and in determining the explanatory adequacy of suggested theories. They serve, that is, as regulative ideals for science rather than as theories propounded within science.

That metaphysical principles may have a regulative role in scientific inquiry has often been admitted by empiricist philosophers. But this has always been thought a peripheral and dispensable function. In the work of philosophers like Toulmin, however, metaphysical principles are allowed to penetrate far more deeply into the fabric of science. Such principles have, on his view, a fundamental and indispensable place in scientific explanation.

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Science progresses not by recognizing the truth of new observations alone, but by making sense of them. To this task of interpretation we bring principles of regularity, conceptions of natural order, paradigms, ideals, or what-you-will: intellectual patterns which define the range of things we can accept (in Copernicus' phrase) as 'sufficiently absolute and pleasing to the mind'. An explanation, to be acceptable, must demonstrate that the happenings under investigation are special cases or complex combinations of our fundamental intelligible types.1

An important consequence of all this is that, on Toulmin's view, one cannot account for the acceptability of one theory rather than another just in terms of its greater deductive power and successful test record. To be acceptable a theory 'must be not only "consistent with the numerical records"; [it] must also be acceptable - for the time being, at any rate - as "absolute" and "pleasing to the mind."'2

This shift towards relativism is a characteristic trend in the historicist movement. One finds it also in the work of Kuhn.

1. op.cit. p. 81. 2. op.cit. p. 115.
The distinction between theories and the facts which they must explain and by which they may be tested, is obscured in Kuhn's work by an insistence on the theory loading of observational facts. The facts, according to Kuhn, are not determinable independently of the theories by which we hope to account for them. Any large scale change in theory must bring a change not only in the interpretation of empirical facts, but in the constitution of the facts themselves. Thus theories which radically differ from one another - which differ in paradigm orientation - are always to a degree 'incommensurable'. One cannot have a crucial experiment between two such theories; for the facts will have to be expressed in the language of one theory or the other, and in either case categories will be employed which may have no place in the rival theory. Accordingly, in Kuhn's view, the overthrow of a theory does not come about by simple empirical refutation, but through a process which may be likened to a 'conversion experience.'

All the characteristics of the historicist movement to which I have drawn attention are to be found already well developed in

1. The Structure of Scientific Revolutions, p. 111 ff.
2. op.cit. p. 150.
the work of R.G. Collingwood. His theory of absolute presuppositions was perhaps the first explicit statement of an historicist position in the philosophy of science, and it is in many ways still the best representative of the movement. This is not to say that Collingwood's theory had any considerable influence in the development of the movement. As far as I can tell, although references to Collingwood are common enough in historicist writers, it is not because Collingwood's theory has contributed much to the development of their ideas, but because their studies have independently lead them to a position which very closely resembles that earlier articulated by Collingwood. For this reason an examination of the logical foundations of Collingwood's theory - which is to be my task in this thesis - should also be relevant to the general critique of historicist theories.

Collingwood first announced the theory of absolute presuppositions in his Autobiography, written in 1938, and expounded it more fully in the Essay on Metaphysics, which he began towards the end of the same year and completed early in 1939. He did not, however, find the opportunity to work out his theory at all carefully. His health was unstable. Both the Autobiography and the Essay were written during periods of convalescence, the latter while at sea. Both were written too quickly, and as a result neither the logical
articulation of Collingwood's theory, nor the historical illustrations which he offered, are at all adequate. But these deficiencies are not a serious impediment from my point of view. They mean that orthodox scholarship is unlikely to be profitable. The dominant concern has to be that of exploring the theory as the type and exemplar of historicist theories of science. What we must ask is whether the theory of absolute presuppositions can be coherently worked out, not whether Collingwood succeeded in doing so; and whether there is ground here for a 'transformation in the image of science by which we are now possessed.'
Collingwood's Essay on Metaphysics might well have been sub-titled 'A Prolegomena to any Future Metaphysics that will be able to present itself as a Science'. For, like Kant, he believed that the chronic failure of metaphysics to achieve any agreed progress, and the powerful anti-metaphysical polemics of the positivists, together made it necessary for any honest metaphysician to stand back and look critically at the nature of his inquiries. Once again it had to be asked 'whether such a thing as metaphysics is possible at all?'. In the Essay on Metaphysics Collingwood proposed an answer to this question: a theory to explain what metaphysics is, why it is necessary to the well-being and advancement of knowledge, and how it is to be pursued.\(^1\)

The Essay on Metaphysics, then, is not a book of metaphysics but a book about metaphysics. It does not undertake original metaphysical inquiries, except incidentally, and always very sketchily, to illustrate points about the nature of metaphysics. The theory it advances is a logical and epistemological theory.

Collingwood had come to agree with the positivists that metaphysics is a pseudo-science if it is taken to be either an

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\(^1\) ReM., Preface.
ontological inquiry, concerned with the properties of 'being in
general', or a transcendental inquiry, which seeks to know what
lies beyond the limits of experience. But he hoped to show that
both these inquiries rest on a mistake about the nature of meta­
physics, and that once the mistake has been uncovered it becomes
possible to develop a reformed metaphysics, and to pursue it as a
genuinely scientific discipline. The clue to the reform of meta­
physics, he believed, could be obtained from a careful study of
the history of science.

The positivists had maintained that natural science is
metaphysically neutral — that although individual scientists have
often held metaphysical beliefs, such beliefs have no proper place
in scientific thought, and ought everywhere to be excised. But,
according to Collingwood, this doctrine neglects the history of
science. For, as a matter of fact, metaphysical beliefs have
played a very important part in the development of science.
Scientists have always worked with certain basic metaphysical
commitments, such as a belief in determinism, which they have not
acquired as a result of empirical research, and which they do not
regard as hypotheses to be tested in the course of research. Such
commitments are the presuppositions of their scientific practice:
they set the problems scientists strive to answer, and influence
the selection of theories. Collingwood called these commitments
'absolute presuppositions'.
An example, Collingwood suggested, may be found in pathology. It is presupposed in pathology that every disease has a cause; the pathologist's problem is usually to trace the causes of particular diseases. But the causal principle itself is not a proposition established within pathology, nor a hypothesis which it is the business of the pathologist to verify. It is a presupposition without which pathology could not get going. A pathologist, if asked to justify his presupposition, might reply

that is a thing we take for granted in my job. We don't question it. We don't try to verify it. It isn't a thing anybody has discovered, like microbes or the circulation of the blood. It is a thing we just take for granted.

All science, according to Collingwood, has presuppositions of this sort, though not the same presuppositions in every science. However scientists themselves are not normally aware of their absolute presuppositions, and indeed nowadays they are disposed to deny that they have any. Yet skilful questioning of individual scientists, or close analysis of the inquiries in which they engage, can always draw out the presuppositions of their discipline.

The existence of absolute presuppositions is something that a positivist theory cannot accommodate. According to Collingwood there are two ways in which a positivist might try to handle absolute presuppositions once his attention has been drawn to them.

1. E.M. p. 31.
On the one hand he might maintain that absolute presuppositions should, despite general practice, be regarded as empirical hypotheses 'advanced upon credit and awaiting verification'. But this will not do, for according to Collingwood, absolute presuppositions are always unverifiable, and so must, by the positivists' criteria, be regarded as metaphysical. On the other hand a positivist might maintain that, despite their historical importance, absolute presuppositions are pre-scientific survivals which should be rigorously excised from science; and can be excised without the loss of anything of value. But according to Collingwood, such ruthlessness fails to take account of the fundamental role which absolute presuppositions have always played in the conduct of science. An absolute presupposition, Collingwood suggests, is like a catalyst. Although it is not itself a scientific proposition, and should not show up in a developed scientific theory, yet it is indispensable to the process of thought which leads to theory. Absolute presuppositions are ineliminably involved in that questioning of nature by which knowledge is extorted. So Collingwood declares

The doctrine of the logical positivists, that metaphysical propositions are nonsensical, will involve the bankruptcy of all thinking in which any use is made of absolute presuppositions; that is to say, the bankruptcy of all science. Any attack on metaphysics is an attack on the foundations of science, any attack on the foundations of science is an attack on science itself.
However the positivist is not the only one who finds that the history of science does not fit the pattern prescribed by his theory. The transcendental metaphysician is also likely to be embarrassed. Some metaphysicians, most notably Kant, have recognized the importance of the metaphysical presuppositions of science, and have made it their business to uncover them. But such philosophers have usually cherished the hope that if it could be shown that a certain metaphysical supposition is presupposed by science, this would count as a justification for that supposition. However, according to Collingwood, such a view would be tenable only if there were some metaphysical principles which are necessarily presupposed in any science whatever. And according to Collingwood the history of science shows that this is not the case. The absolute presuppositions of science change from time to time. When they do the structure of science undergoes a revolution - new questions are asked, different kinds of theory are entertained - one way of doing science is replaced by another. What the metaphysician takes to be the necessary presuppositions of all science are only presuppositions of science in his own generation. Or, what is more likely since philosophers are rarely in the vanguard of science, they are the presuppositions of a previous generation, already abandoned by those actively engaged in science. Perhaps, Collingwood concedes, it was in Kant's time possible to believe that science had at last found
the right presuppositions, the presuppositions which alone make scientific progress possible. But the subsequent history of science has made that view implausible. The presuppositions of classical physics, which Kant laboured to uncover, have now been abandoned in at least the more highly developed sciences. Contemporary physics works with a different set of presuppositions. And if history is any guide these too will be abandoned in the future.

These historical reflections provide the foundation for Collingwood's theory of metaphysics. If metaphysics is taken as the systematic study of absolute presuppositions, then the question 'Is metaphysics possible at all?' can be answered affirmatively. Clearly there is room for a descriptive and historical study of absolute presuppositions. Such a study would seek to discover what absolute presuppositions were at work in any period of scientific history; it would analyse the relation between the developing structure of science and its metaphysical foundations; and it would describe the processes by which absolute presuppositions come to be accepted and abandoned. Its methods would be those of the intellectual historian.

However one might suppose that there is another more important task to be performed by the metaphysician, namely the task of metaphysical criticism. The metaphysician's business, on this view, is that of 'assisting the progress of science by
showing which presuppositions, and therefore which schools of scientific thought, are justified in the light of metaphysical criticism and which are not.¹ For example, according to Collingwood it was an absolute presupposition of Newtonian physics that some events have causes and others not; while in post-Kantian physics it was presupposed that all events have causes; and in modern physics it is presupposed that no events have causes.² Now these presuppositions cannot all be true. So if Collingwood is right and they are each presupposed by some school of physics, then one might suppose that the metaphysician's task should be to determine which of the competing presuppositions is true — 'an important duty, because when we know which of these three propositions is true we shall know which of these three schools of physicists is on the right lines'.³

Yet according to Collingwood the metaphysician who sees this as his task must either lapse into dogmatism or face disappointment. For he is asking a question which cannot be answered. There is no way of establishing the truth of any one of these presuppositions. They are none of them true or false. They are not propositions at all.

It is a mistake therefore to fancy that by investigating the truth of their absolute presuppositions a metaphysician could show that one school of science was fundamentally right and another fundamentally wrong. To inquire into the truth of a presupposition is to assume that it is not an absolute presupposition. Such a phrase as 'inquiry into the truth of an absolute presupposition' is nonsense.

And this, Collingwood suggested, is the mistake upon which most previous metaphysics has rested. Supposing that absolute presuppositions are propositions, metaphysicians have been led to ask pseudo-questions, like 'Is it true that all events have causes?' or 'Can the principle of the uniformity of nature be justified?' - and inevitably their inquiries have led nowhere.

If that which is presupposed in an absolute presupposition is not a proposition, what is it? Collingwood used the word 'supposition'; but this is not illuminating. 'Supposition' will do as a term to apply to an item which has the form of a proposition but is not true or false, and I shall employ the word in this way. But it does not make it clear what sort of thing is involved. A plausible suggestion is that metaphysical principles, if they cannot be taken as propositions, should be construed as a sort of rule: as regulative principles or research directives. W.B. Gallie has recommended that Collingwood's theory should be interpreted along these lines. And,

1. E.M. p. 52.
2. E.M. p. 54.
3. Philosophy and the Historical Understanding, Chapter 10.
of course, such an account of metaphysical principles has often been defended in recent philosophy.\(^1\) I shall explore this suggestion in later chapters.

From the thesis that absolute presuppositions are not true-or-false Collingwood drew the conclusion that any criticism of metaphysical principles is impossible. The metaphysician can only describe the presuppositions operative in any period of scientific history, he cannot evaluate them. His statements must always be subject to a 'rubric' - namely 'In such and such a phase of scientific thought it is (or was) absolutely presupposed that ...' - which puts the metaphysical principle into oratio obliqua and excludes critical judgments.\(^2\) And the scientist himself, even when he has become aware of his absolute presuppositions, still can only presuppose them. Self-criticism is likewise impossible.\(^3\)

We must not now question, in the hope either of justifying them or of condemning them, the presuppositions which in that earlier stage of our life we were content to accept. The fact that we have learned what our absolute presuppositions are does not imply that our attitude towards them either should or can cease to be one of sheer presupposal.

This last claim of Collingwood's has aroused the most severe criticism of his theory. We shall have to examine some of these criticisms shortly. But here we may notice two points which tend to weaken their force.

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1. For example by L.J. Russell, 'Science and Philosophy' \(\text{P.A.S.}\), 1924; and by Stephen Körner, \(\text{Conceptual Thinking}\).
2. \(\text{E.M.\ p. 55}\).
3. \(\text{E.M.\ pp. 173-4}\).
The first point is that the conclusion that absolute presuppositions cannot be criticized, either by the working scientist or by the philosopher, does not follow from the thesis that absolute presuppositions are not true-or-false. There surely can be critical argument about suppositions which are not propositional - about imperatives for example - though of course then the argument cannot concern the truth of the supposition, but rather its propriety or perhaps its utility. Indeed one way in which one might seek to defend the thesis that metaphysical principles are not propositions but a species of rule would be to show that the problem of criticism - how there can be rational argument about metaphysical principles - is no longer intractable once metaphysical principles are regarded as rules. Considerations which would be irrelevant if considered as reasons for believing a principle to be true, might yet be good reasons for accepting it as a procedural rule. So even if we come to agree that Collingwood was wrong in supposing that metaphysical principles are not subject to rational criticism, this would not be to admit a fundamentally damaging criticism of Collingwood's theory. The remainder of the theory is independent of that particular thesis, and the theory would perhaps be stronger for its omission.

The second point is that Collingwood himself introduced a number of qualifications on the claim that absolute presuppositions are not criticizable. The first qualification arises out of
Collingwood's assertion that absolute presuppositions normally go in sets, which he called 'constellations'. Research is normally carried on within the framework of a set of several metaphysical principles. The constituents of such a set have, according to Collingwood, to be 'consupponible'; that is, they must be logically consistent with one another.\(^1\) So at least the possibility of a logical criticism of constellations of absolute presuppositions has to be recognized. One might be able to show that the presuppositions of a certain school were incompatible.\(^2\) The second qualification is more important but less clear.

According to Collingwood the presuppositions of science at any time will usually be subject to 'strains' of greater or less intensity, which, if unresolved, may cause changes in the metaphysical sub-structure of science. One task of the metaphysical historian, according to Collingwood, should be to identify the strains present in the period which interests him, using them to explain the shifts that have occurred in the constellation of absolute presuppositions.\(^2\) Now the notion of 'strains' in metaphysical system was not at all worked out by Collingwood; I shall try to develop the notion in the last chapter of this thesis. But *prima facie* there seems no reason to deny that a working scientist, or his philosophical critic, might recognize

\(^{1}\) E.M. p. 66.  
\(^{2}\) E.M. p. 74
the presence of such strains and strive to remove them. And this would seem to leave some room for critical discussion of absolute presuppositions.

What Collingwood wanted to deny was that metaphysical principles could be tested empirically, or proved deductively, or justified in Kantian style by a transcendental argument. Metaphysical principles, he wished to maintain, are neither empirical nor a priori. But this need not mean that they cannot be critically appraised. And there is in Collingwood's theory the undeveloped germ of a theory of metaphysical criticism.

I have, in the preceding pages, been concerned to outline Collingwood's theory of absolute presuppositions. Of course my account leaves a great deal that is vague and ill-developed. The detailed articulation of the theory, and argument in support of it, have been left until later chapters of the thesis. In particular I have left the notion of an absolute presupposition without adequate explication. What is the relation of presupposition said to be involved here? Does Collingwood claim that science in some sense logically presupposes metaphysical principles? Or are the absolute presuppositions of science 'presuppositions' only in a psychological sense: doctrines taken
for granted by the scientists of a period, which may influence their work in various ways, without being logically presupposed by it? And what is it that makes a presupposition absolute? Until these questions are answered Collingwood's theory remains indeterminate, hardly a theory at all. Yet what Collingwood had to say on these matters is obscure and at times confused or inconsistent. Indeed Alan Donagan, the most thoroughgoing of Collingwood's critics, has maintained that Collingwood's theory is 'too riddled with confusions and contradictions to stay afloat; and no considerable part of the wreckage can be salvaged.'¹ I think Donagan has exaggerated the extent of Collingwood's confusion, and I shall try to show this in a later chapter.² In any case I think it is possible to salvage a substantial part of his doctrine and to develop it into a coherent and interesting theory of metaphysics. That is to be my task in this thesis.

In the remainder of this chapter I want to consider some of the most serious criticisms of Collingwood's theory, criticisms which if sound would justify Donagan's claim that the theory is beyond salvage.

It has been suggested, first by T.M. Knox³ and more recently by Donagan⁴, that Collingwood's position is fundamentally

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¹. The Later Philosophy of R.G. Collingwood, p. 279.  
². Chapter Four.  
inconsistent. He wanted to regard scientific inquiry as the systematic pursuit of truth, a pursuit which sometimes at least is successful.

Natural science is not a tissue of fancies or fabrications, mythology or tautology, but a search for truth, and a search that does not go unrewarded.

Yet his theory of absolute presuppositions seems to go against this attitude. According to Collingwood's theory, all science rests on absolute presuppositions, and these presuppositions cannot be described either as true or false. But the claim that scientific theories have such presuppositions would seem to be incompatible with the view that scientific theories may be true or false. Either scientific theories are not true-or-false or anything presupposed by them must also be true-or-false.

Collingwood himself apparently came to think that his theory of metaphysics was incompatible with the belief that science yields true knowledge. In notes for his projected Principles of History he remarked

Natural science .... starts from certain presuppositions and thinks out their consequences, and since these presuppositions are neither true nor false, thinking these together with their consequences is neither knowledge nor error.

But Collingwood did not draw this sceptical conclusion in the

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2. Quoted by Knox, op. cit., p. xiii.
Essay on Metaphysics, or indeed in any other published work. In the Essay he still maintained that science does provide true knowledge, and railed against those philosophers whose work tended to obscure the fact of scientific progress.\(^1\) According to Knox and Donagan, however, his position here is inconsistent. If the theory of absolute presuppositions is sound, Collingwood's enthusiasm for modern science can only be regarded as dogmatism, a 'blind unquestioning acceptance' of the presupposition of contemporary science.

Donagan has summed up the criticism thus:\(^2\)

Having renounced the backing of reason for the absolute presuppositions of modern science, he could only champion them as a man of faith. 'The only attitude towards them that can enable us to enjoy what they have to give us', he declared, '... is an attitude of unquestioning acceptance. We must accept them and hold firmly to them; we must insist on presupposing them in all our thinking without asking why they should be thus accepted!' (E.M., p. 173)

Such a coincidence of scepticism and dogmatism is disastrous. After denying the truth of falsity of absolute presuppositions, Collingwood could only persevere in his adherence to modern European science by resorting to dogmatism. By a cruel trick of history, he had condemned that way out of his difficulties before he became aware that he would be obliged to take it himself. In the first Part of the Essay on Metaphysics, having distinguished what Newtonian, Kantian, and Einsteinian physics each absolutely presupposed about causation, he had remarked that an unreformed metaphysician would try to find out which is true, in order

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\(^1\) E.M., p. 342.  
\(^2\) D., pp. 278-9, the references are part of the quote.
to know which of these three schools of physicists was on the right lines (E.M. p. 51). And although in the third Part of that same Essay he was to go on to announce his own 'unquestioning acceptance' of the Einsteinian school (cf. E.M. pp. 173, 333), in the first Part he had contemptuously dismissed as 'an irresponsible and dogmatic person' any philosopher who would 'pronounce loudly and confidently in favour of one alternative' (E.M. p. 51).

Collingwood's position was not as blatantly inconsistent as Donagan suggests. What Collingwood had denied in Part One of the Essay was the possibility of anticipating the outcome of scientific research by investigating the truth of the presuppositions of competing schools of science. Of course, in some cases it may be possible to show that the presuppositions accepted by some scientific school are internally inconsistent, and then one might say that those who conform to these presuppositions are proceeding along wrong lines. Collingwood thought something like this could be shown of classical determinism. But, according to Collingwood, we can never hope to be in a position to say the presuppositions of some particular school are the right ones and all others wrong. Metaphysics cannot be made a substitute for science.

The charge of irresponsibility and dogmatism was directed by Collingwood against those philosophers who would pronounce a priori in favour of one set of absolute presuppositions. Their dogmatism lay in wanting to dictate the course that science
should follow. But Collingwood did not himself fall into that trap. He seems to have held that Einstein's theory was an advance on that of Newton. At any rate, he berated those philosophers - e.g. Wisdom and Ayer - who continued to uphold metaphysical principles, like that of determinism, which he thought had been superseded as presuppositions in science. Such philosophers, he claimed, pretend 'to prove that the advances actually being made in science are not advances at all.'\(^1\) But he nowhere proclaimed his unquestioning acceptance of the presuppositions of modern physics. Donagan's references do not sustain this charge.\(^2\)

Still it may plausibly be argued that Collingwood's attitude towards modern physics is incompatible with the theory of absolute presuppositions. In the Epilogue to the Essay on Metaphysics he appears to hold that a set of presuppositions may be 'superseded' - not just that they may come to be abandoned by working

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2. Donagan cites E.M. p. 173 & p. 333. E.M. p. 173 is the passage where Collingwood speaks of 'unquestioning acceptance' as the only appropriate attitude to take up towards one's absolute presuppositions. There is no mention here of Einsteinian physics. E.M. p. 333 mentions Collingwood's view that 'during what I will call the Kantian period, roughly speaking from Kant to Einstein, the fabric of natural science, spectacular though its progress was, rested on an insecure foundation.' He goes on from there to show that the presuppositions of the Kantian period were not 'consupposable'. There is nothing here that is incompatible with the doctrine of Part One of the Essay; and there is no avowal of 'unquestioning acceptance' of the Einsteinian school.
scientists, but that the advance of science may itself force their rejection. Thus he seems to have thought that the success of Einsteinian physics over the classical theories meant that those who continue to uphold the Kantian metaphysical principles are 'reactionaries' - 'trying with a clumsy hand to put back the clock of scientific progress.' But how can this be reconciled with the thesis of earlier chapters that the absolute presuppositions of science cannot be overthrown by the verdict of experience? It seems that in recognizing the reality of scientific progress Collingwood has opened the door to the empirical appraisal of competing sets of absolute presuppositions. The most the theory of absolute presuppositions can admit is progress within the framework determined by a single set of presuppositions, not progress through metaphysical change.

Collingwood appears then to be forked. If all science rests on absolute presuppositions it seems he must either deny that one theory can be described as an advance on another when the two theories have incompatible presuppositions, or he must allow that absolute presuppositions may be overthrown in the course of scientific development. The first alternative would involve him in what he himself regarded as obscurantism; the

second would require the rejection of an important part of the
theory of absolute presuppositions.

Whether Collingwood is indeed caught by this fork depends
on how the relation between science and its presuppositions is
understood. It depends, that is, on the nature of the presupposition
relation. Collingwood's critics seem to have thought that the
relation of presupposition is to be analysed as an entailment
holding between particular scientific theories and metaphysical
principles. On this view theories are related to their pre-
suppositions as consequences to postulates in a deductive system.
Collingwood himself described the relation in these terms in the
passage Knox quoted from his unpublished notes:

    Natural science ... starts from certain presuppositions
    and thinks out their consequences.

Moreover there is a passage in the Essay on Metaphysics which
requires that the relation of presupposition be analysed in this
way. At E.M. p. 67 Collingwood remarks that the constituents
of a constellation of presuppositions must be logically independ-
ent of one another: 'For if any one of these constituents
logically necessitated any other, the first would be a pre-
supposition of the second, and therefore the second would not be

1. I.H. p. xiii.
an absolute presupposition'. This makes 'A presupposes B' equivalent to 'B necessitates A', i.e. 'B entails A'; and identifies absolute presuppositions with the postulates of a deductive system.

Now certainly if this is how the relation of presupposition is to be understood then our fork can be imposed. If the postulates of scientific theories are not true-or-false then the laws which are consequences of those postulates are not true-or-false either. If the consequences of absolute presuppositions, i.e. putative scientific laws, are capable of empirical refutation, then the postulates from which they derive are likewise falsifiable, by modus tollens.

However the entailment analysis of presupposition is incompatible with the theory of absolute presuppositions as Collingwood developed it in the early chapters of the Essay on Metaphysics.¹ For in the Essay on Metaphysics not scientific theories but the questions they purport to answer, are said to have absolute presuppositions.² Now a question must always admit alternative possible answers; it cannot determine what its own correct answer will be. (Rhetorical questions are not

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¹ See especially Chapters IV, 'On Presupposing', and V, 'The Science of Absolute Presuppositions'.
² B.M. p. 25.
proper questions but a form of statement.) So if it is scientific questions that have absolute presuppositions, their presuppositions cannot be related to the theories which purport to answer them as postulates are related to theorems. Collingwood's remarks on the role of absolute presuppositions in science accord with this point. He does not claim that scientific theories follow deductively from absolute presuppositions. An absolute presupposition, according to Collingwood,

is ... fundamentally important to the science that makes it, because it determines the entire structure of that science by determining the questions that arise in it, and therefore determining the possible answers.

Absolute presuppositions, that is to say, restrict the range of theories that can be entertained at any time, because they determine what problems theories have to solve. But this is compatible with there being indefinitely many theories which all satisfy the absolute presuppositions of a science, but which are incompatible with one another. The absolute presuppositions of science might determine what questions scientists will ask without determining what the correct answers to those questions must be.

This makes it much less clear whether the arguments of Knox and Donagan can be made binding. If the relationship

1. R.M. p. 52, my italics.
between scientific theories and absolute presuppositions is logically more loose than the relation of deducibility, then it does not obviously follow from the claim that scientific questions have non-empirical, non-propositional presuppositions that the answers to those questions must equally be non-empirical and non-propositional. Granted that Collingwood's attitude towards absolute presuppositions is both sceptical and dogmatic, it remains to be demonstrated that his whole attitude towards science must be infected by scepticism and dogmatism. There is a possibility that the theory of absolute presuppositions might be reconciled with an attitude of critical empiricism towards the laws and theories of science.
Two features of Collingwood's theory of absolute presuppositions distinguish it from most other presupposition theories of metaphysics. The first is the thesis that presupposition analysis does not provide a method of proof for metaphysics. According to Collingwood, though the demonstration that science has metaphysical presuppositions undermines the positivist programme for the elimination of metaphysics—and so in a sense vindicates metaphysics—it does not provide a way of justifying specific metaphysical doctrines. For, he held, to have shown that certain metaphysical suppositions are presupposed in the science of a given epoch is not to have shown that those presuppositions are necessary: that science could not have developed with different presuppositions, or that the presuppositions of science could not change in the course of scientific development. Presupposition analysis can sustain only a descriptive and historical study of metaphysics, not a critical science.

The second distinctive feature of Collingwood's theory is the thesis that it is scientific inquiry, not scientific theory, that has metaphysical presuppositions. On this view, the metaphysical presuppositions of a science cannot be discovered by the analysis of the theories propounded in that science. They emerge
only from a study of the questions those theories purport to answer; only that is when science is studied historically as a progressing inquiry.

The second of these two theses could perhaps be made to explain why the metaphysical background of science has more often been noticed by historians than by philosophers of science. Indeed a positivist might claim that this reflects a legitimate division of interests. For, he might argue, if Collingwood is right then the role of metaphysical principles in science will strike us as important only if we are interested in the context of discovery; that is, only if our concern is to describe and explain the way in which scientific theories come into being. But this is the historian's domain. The philosopher's concern lies in the context of justification; he is not properly concerned with how theories are discovered, but with questions about the analysis and method of confirming theories once they have been discovered. His task is one of 'rational reconstruction'. The positivist thesis of the eliminability of metaphysics then has to be understood as the thesis that metaphysical principles can always be eliminated from science in reconstruction. And the historical importance of metaphysical doctrines in science need not conflict with this thesis.

But this is too narrow a view. If it is the case that metaphysical doctrines have had a continuing influence in the development of science, then there is a genuine philosophical
question to ask; namely, How is it possible for metaphysical doctrines to have such an influence? In what way could a metaphysical principle be presupposed in scientific inquiry?

It is of course possible to answer these questions in a way that is in line with the view that the influence of metaphysical doctrines in science is a matter of historical interest only. The metaphysical 'presuppositions of science', a positivist could argue, are presuppositions only in a subjective sense. Strictly they are not presuppositions of science but presuppositions of certain scientists: doctrines taken for granted by certain scientists which affect their work in various ways without being logically presupposed by it. For example, a scientist might seek theories capable of precise mathematical formulation because he believes that the book of nature is 'written by the hand of god in the language of mathematics'. He might, that is, adopt a methodological rule because he accepts a metaphysical principle which apparently supports it. But the rule does not logically presuppose the metaphysical doctrine, and someone might consistently adopt the rule for other purely methodological reasons. In any case, the theories which a scientist propounds in conformity with such a rule must stand on their own feet, without presupposing as a condition of their truth, or of their truth-or-falsity, any non-empirical doctrine.
The important point to notice here is that there is a distinction between what a scientist presupposes, in the sense of 'takes for granted', and what his theories and rules of method logically presuppose. This distinction makes it possible for a positivist to argue that while scientists have usually in fact operated with certain metaphysical assumptions, which have caused them to ask certain sorts of question, the scientific inquiry itself has, or need have, no non-empirical presuppositions.

There is a good deal in the Essay on Metaphysics to support this position. For although Collingwood presented the theory of absolute presuppositions as, in the first place, a logical theory, his discussion is infected throughout by psychologism. The relation between scientific questions and their presuppositions is claimed to be a logical relation: one discovers the presuppositions of an inquiry by purely logical analysis. Yet what is uncovered by such analysis, according to Collingwood, are thoughts in the mind of the scientist: 'presupposing is a thing people do in their minds', though often they are unaware of doing it. And the process by which these thoughts are supposed to be uncovered at times seems more akin to psychoanalysis than to logical analysis.

Moreover, even in developing his 'logical' theory of presupposition Collingwood allowed himself to import notions which are quite alien to logical theory. He speaks, for example, of the 'logical efficacy' of a presupposition, defining it as 'the fact that a presupposition causes a certain question to arise.' It is possible to interpret this in a purely logical way, but there is a very strong suggestion that Collingwood has confused the logical and subjective senses of presupposition, and thinks that to have discovered the logical presupposition of a question is also to have discovered the psychological cause of its being asked. And this suggestion seems to be confirmed by Collingwood's practice in the later illustrative chapters of the *Essay on Metaphysics*. For his samples of metaphysical inquiry proceed for the most part by disclosing the fundamental beliefs about the nature of reality which were 'taken for granted' by scientists in a given epoch and which partly determined the questions they set themselves to answer. It is by no means clear that such fundamental beliefs can be identified with the absolute logical presuppositions of the early theoretical chapters. Rather it seems likely that Collingwood has allowed himself to slide from the logical to the subjective sense of 'presupposition'.

1. *E.M.*, p. 27 my italics.
Our task in this chapter will then be to inquire whether a
genuinely logical theory of presupposition can be separated from
the psychological elements in Collingwood's discussion, and whether
such a theory can sustain the sort of metaphysical inquiry Colling-
wood projected.¹

Collingwood and Strawson on Presupposition.

There are, as I have already pointed out, at least two
senses of 'presupposition' which should be distinguished from one
another: a subjective sense, where a presupposition is what some-
body takes for granted in some discussion or inquiry, and a logical
sense, where what is presupposed is in some sense logically
implied by the questions someone asks or the statements he makes.

The subjective sense of presupposition poses no special
analytical problem. In this sense, to say that a certain supposition
is presupposed by a question is just to say that the person who
asks that question asks it in part because he assumes or takes for
granted that supposition. The question arises for him out of his
presupposition. Thus one might explain the question 'why were you
working so late last night?' by pointing out that it was asked in

¹ A similar inquiry has recently been undertaken by J.F. Post,
'A Defence of Collingwood's Theory of Presupposition', Inquiry
1965.
the belief that you were not in the habit of working late; or in
the belief that last night was your wedding anniversary, and one
would not normally work late on such an occasion. Here there is
no logical connection between the question and what is presupposed
in asking it. But one might still be unable to see the point of
the question, or fail to understand why it was asked, until one
discovers what was presupposed in asking it. So the subjective
presuppositions of a question would be of interest to someone who
wished to explain how the question came to be asked.

In the case of logical presupposition, however, the connection
between the question and that which it presupposes is logical.
Thus to use the same example, if someone asks 'Why were you working
so late last night?' but denies that he thinks you were working
late, we would want to say that he is guilty of a kind of incon-
sistency. The task of this and the following section will be to
analyse the nature of the logical connection involved here; or,
what comes to the same thing, to analyse the nature of the incon-
sistency involved in both asking the question and denying its
presupposition.

As I have already remarked, it often seems that what Colling-
wood called 'absolute presuppositions' are presuppositions of
scientific inquiry only in the subjective sense of the word. But
he certainly claimed that a stronger logical connection was involved
in the relation between science and metaphysics.¹ For he wanted to say that the questions asked by scientists at any time logically could not be asked, logically do not arise, except on certain absolute presuppositions. In this sense of the word 'presuppose', to say that a question presupposes a certain proposition is to say that acceptance of that proposition is a necessary condition for the question to arise, that unless that proposition is accepted the question is logically absurd.²

Collingwood's paradigm of a logical presupposition is given by the question 'Have you stopped beating your wife?'. He writes³

There is no verbal impossibility in the way of asking a man whom you suppose to be an indulgent husband whether he has stopped beating his wife. But there is a logical impossibility; for that question arises from the presupposition that he has been in the habit of beating her. If he is not supposed to have been in that habit, the question whether he has stopped 'does not arise'.

The problem here is to give an account of what it is for a question to 'arise' or 'not arise'. Clearly the idea is not that of a question's in fact being asked or not asked - for Collingwood allows that a question may get asked even although it 'logically does not arise', and he would presumably allow that questions which 'logically arise' oftentimes do not get asked. What we must investigate is the nature of the absurdity involved in asking a

question which 'does not arise'. Once this is clear we may define both the notion of presupposition, and that of a question's arising or not arising, in terms of it.

According to Collingwood the absurdity is logical. Yet it cannot be that a straightforward self-contradiction is involved, for a question does not assert anything. One might, perhaps, say that he who asks a question is committed to whatever is presupposed by his question, so that to ask a question while rejecting the proposition it presupposes is logically inconsistent. But it would be circular to refer to such inconsistency in an explication of the term 'presupposition'.

There is an obvious analogy between Collingwood's remarks about the logical presuppositions of certain questions and the latter-day attempts of P.F. Strawson to analyse presupposition as a form of logical relation, distinct from that of entailment.¹

The analogy is clearest, perhaps, in the following passage, where Strawson introduces the notion of presupposition by way of a paradigm.

Suppose someone says 'All John's children are asleep'. Obviously he will not normally, or properly, say this, unless he believes that John has children (who are asleep). But suppose he is mistaken. Suppose John has no children. Then is it true or false that all John's

¹. Introduction to Logical Theory pp. 173-5 & 213; cf. 'A Reply to Mr. Sellars', Philosophical Review 1954.
². op.cit. pp. 173-4.
children are asleep? Either answer would seem to be misleading. But we are not compelled to give either answer. We can, and normally should, say that, since John has no children, the question does not arise.

This analogy suggests that Strawson's analysis might be applied to the cases Collingwood was considering. On Strawson's account, to say that a statement S presupposes another statement S' is to say 'that the truth of S' is a precondition of the truth-or-falsity of S'.

On the face of it, there are two obstacles in the way of offering this analysis as an explication of the term 'presupposition' as employed by Collingwood. First, Collingwood denied that the truth of its presupposition is a precondition for any question to arise. The presupposition, he claimed, need not be true, or even thought true, for the question to arise, it has only to be 'supposed'; one can suppose something one believes false in order to see what questions then arise, and how they might be answered. However this point does not directly contradict Strawson's analysis. It indicates that the connection between that which presupposes and that which is presupposed is a logical connection, a connection which holds whatever the truth value of the proposition presupposed. But it does not follow from this, as Collingwood supposed it did, that the relation of presupposition should not be analysed in terms

which involve the notion of the truth of the proposition presupposed. Secondly, on Strawson's account the relation of presupposition holds between statements, whereas for Collingwood what has a presupposition is always a question. However Strawson claimed that his analysis applies as well to the presuppositions of questions as to the presuppositions of statements. If I understand him, his view was that to say that a question arises is to say that the possible answers to that question are either true-or-false. The question 'Are all John's children asleep?' can be said to arise only if the statement 'All John's children are asleep' has a truth value.¹ Neither of the two obstacles, therefore, should prevent us from giving a Strawsonian analysis of Collingwood's logical sense of presupposition.

Nevertheless Strawson's analysis should be rejected, or at least extensively reconstructed, for as it stands the account is incoherent. By now this is widely recognized, so I shall not argue the point in detail, but will be content with a brief and somewhat dogmatic criticism. A more thorough argument may be found in Grahame Kerlich's article 'Presupposition and Entailment'.²

Strawson stated his position as follows:¹

If a statement S presupposes a statement S' in the sense that the truth of S is a precondition of the truth-or-falsity of S, then of course there will be a kind of logical absurdity in conjoining S with the denial of S'. But we must distinguish this kind of logical absurdity from straightforward self-contradiction. It is self-contradictory to conjoin S with the denial of S' if S' is a necessary condition of the truth, simply, of S. It is a different kind of logical absurdity to conjoin S with the denial of S' if S' is a necessary condition of the truth or falsity of S. The relation between S and S' in the first case is that S entails S'. We need a different name for the relation between S and S' in the second case; let us say, as above, that S presupposes S'.

Now if by 'statement' we mean 'that which is true or false' - and this is how Strawson first introduced the term² - then the above account is incoherent. For if S' is false, then, under Strawson's definition, S is neither true nor false, i.e. S is not a statement. Thus if S presupposes S', and S' is false, then S does not exist, and a fortiori S does not presuppose S'.

There are two courses which one might take in trying to remove this paradox. First one might try to redefine the term 'statement' so that it would still make sense to speak of the statement S, and so to speak of its presupposing S', where because S' is false S is neither true nor false. Alternatively one might try to remove the paradox by redefining the term 'presuppose'.

¹. Introduction to Logical Theory, p. 175.
². Op.cit. p. 4; I do not myself want to use the term 'statement' in quite this way. On this point cf. my remarks at the beginning of the next section.
If the first course is taken the meaning of the term 'statement' will have to be altered to admit statements which are not true or false. And it seems that Strawson would have been content to do this, for he wrote:

One must distinguish between what can be said about the sentence, and what can be said about the statements made, on different occasions, by the use of the sentence. It is about statements only that the question of truth or falsity can arise; and about these it can sometimes fail to arise.

Where the question of truth or falsity does not arise Strawson says that the statement lacks a truth value, or is neither true nor false. And if this trichotomy is combined with the view that logical relations hold between statements, then it would seem to follow that Strawson is committed to the development of a three-valued logic. Presupposition, it then turns out, is a relation in a logic of three values.

I do not think, however, that an account along these lines can be made to work. For one thing, even if we grant that a three-valued logic, with neither-true-nor-false as the third value, is admissible, it remains extremely doubtful whether presupposition will be distinct from entailment in such a system. Rather it seems that once the law of the excluded middle has been given up, as it must in any three value system, then presupposition will be

definable in terms of entailment thus: S presupposes S' if both
S and ¬S entail S'; or S presupposes S' if ¬S' entails
¬(S v ¬S) — where '¬' is read as 'it is false that'. This sort
of point has been adequately discussed by Nerlich, and I shall not
pursue it further.

A more fundamental objection to the three value gambit is
that the trichotomy of true, false and neither-true-nor-false
statements is as much a 'bogus trichotomy' as the trichotomy of
true, false and meaningless statements. It seems clear that the
sort of thing about which Strawson wanted to say that we can ask
whether it 'has a truth-value', or whether 'the question of truth
or falsity arises', is always the use of a certain sentence in a
certain context. To ask whether the question of truth or falsity
arises is then, for Strawson, to ask whether the use of a certain
sentence in a certain context makes an assertion which is either
true or false. The question whether something is true, on the
other hand, is a question that can be asked about that which is
asserted by the use of a sentence in a certain context, and also
about that which is presupposed or entailed by the assertion made
by the use of a certain sentence in a certain context.¹ So we
could say, as Strawson does, that the question whether something
is true and the question whether something has a truth value are

both questions which arise only about the one sort of thing, statements, only if we are prepared to say that the use of a sentence to make a statement, and the statement made by the use of the sentence are identical; and then only if we are prepared further to say that whatever is entailed or presupposed by a statement is also a use of a sentence in a certain context. This does not seem to be a viable position.1

If we reject the idea of a third value for statements, then we must take the second of the courses mentioned earlier, and seek to redefine the relation of presupposition so as to make it compatible with the thesis that, for example, the sentence 'All John's children are asleep' may be used to make a statement only if John in fact has children. The most simple way of doing this, it seems to me, is the following: to define presupposition as the relation which holds between the statement S and the statement S' where the truth of S' is a necessary condition for the use of the sentence 'S' to make a statement.2 Here, of course, we cannot say that the clause the truth of S' is a necessary condition for the use of the sentence 'S' to make a statement is definitionally equivalent to

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2. This way of defining presupposition fits in well with Strawson's remarks in the essay 'On Referring' on the distinction between a sentence and the use of a sentence - Essays in Conceptual Analysis, Ed. A. Flew, pp. 34-5.
the statement \( S \) presupposes \( S' \). For it might be true that the truth of \( S' \) is a necessary condition for the use of \( S' \) to make a statement without there being a corresponding relation of presupposition. This would be the case where, because \( S' \) is false, no statement is made by the use of \( S' \). Thus it is a peculiarity of the relation of presupposition as defined above that if the relation holds then necessarily the statement presupposed is true. This might well make us loath to call presupposition a logical relation. Though what we could say is that wherever the relation of presupposition holds there underlies it an ordinary logical relation; for, on the above account, the relation of presupposition will hold between the statements \( S \) and \( S' \) only if there is a simple entailment holding between the metalinguistic statement that the use of \( S' \) makes a statement and the statement \( S' \). \(^1\) And this relation will hold whatever the truth value of the related statements.

There are cases where something like the relation I have specified does seem to obtain. For example, it seems plausible to say that the sentence 'He is asleep' can be used to make a true or false statement only if it is used in a context which provides a definite referent for the pronoun 'he': where that condition is not met no determinate statement is made by the use of the sentence

\(^1\) I owe this suggestion to Prof. R. Bradley, op. cit.
'He is asleep'. If so, then the meta-linguistic statement that a certain use of the sentence 'He is asleep' makes a true or false statement entails that that use of the sentence occurs in a context sufficient to identify a referent for the pronoun 'he'. Similarly it would seem that the sentence 'John is asleep' could be used to make a definite statement only in a context sufficient to determine who it is that is referred to by the name 'John'. So again it seems plausible to say that the metalinguistic statement that a statement is made by the use of the sentence 'John is asleep' entails that the sentence is used in a context sufficient to identify the referent of the name 'John'. And if we adopt the revised definition of presupposition, Strawson's view comes to be that the sentence 'All John's children are asleep' can be used to make a statement only if it is used in a context sufficient to identify a referent not only for the name 'John', but also for the descriptive expression 'John's children'. But the following considerations are apt to cast doubt on this suggestion.

It is I think in general true that one who uses a sentence like 'He is asleep' or 'John is asleep' in a context which is not sufficient to identify a referent for its subject, fails to say anything true or false. And this it seems to me is because in such a case we have partly to rely on contextual considerations in order to ascertain what it is that is being asserted, that is, in order
to identify what state of affairs is alleged to obtain. We must not only know that the subject has a referent, but also who the referent is, before we can know what is being asserted by one who uses such a sentence. If neither the sentence alone nor the sentence-in-context adequately specifies what state of affairs is asserted then no determinate statement has been made, i.e. nothing sufficiently determinate has been said for the ascription of truth or falsity. But in the case of 'All John's children are asleep', provided the name 'John' has an identifiable referent, no further specification is needed of the state of affairs in question. Moreover, even where the existential presupposition is known to be satisfied, where it is known that John has children, no further specification of the state of affairs is gained, the statement is not rendered more determinate by this knowledge. If then we are to say that no statement is made by the use of the sentence 'All John's children are asleep' where John has no children, this cannot be because what is asserted is insufficiently determinate for the ascription of truth or falsity. Indeed here we should be in the curious position of being able to specify exactly what statement would have been made had the presupposition been satisfied. And if we can specify what state of affairs is alleged to obtain by one who uses the sentence 'All John's children are asleep' even when John has no children then it seems strained to say that no statement has been made by the use of the sentence or that what has been asserted lacks a truth value.
Strawson claimed that the view that no true or false statement is made by the use of the sentence 'All John's children are asleep' when John has no children conforms to the tendency of ordinary discourse. For, he claimed, it would normally be considered a 'linguistic outrage' to say that the fact that John has no children is a sufficient reason for asserting that it is false that all John's children are asleep. We would rather say that since John has no children the question whether it is true or false that all John's children are asleep 'does not arise'. I think this is true. But even if it is granted that a logical system should do justice to the 'logic of ordinary language', it would I think be rash to conclude from the above considerations that no true or false statement is made by the use of the sentence 'All John's children are asleep' where John has no children. For it is not obvious that the statement 'The question whether John's children are asleep does not arise' is equivalent to 'It is neither true nor false that John's children are asleep'. An alternative interpretation seems possible, namely that although the statement that all John's children are asleep has a truth value whatever the truth value of the statement that John has children, yet it is pointless and perhaps

1. Introduction to Logical Theory, pp. 173-4.
2. But notice that the tendency in ordinary discourse is not uniform. As Strawson himself recognized, 'in some cases and circumstances it may be quite natural and correct to assign a
also linguistically outrageous to ask whether it is true that all
John's children are asleep unless it is taken for granted that
John has children.

The only independent reason Strawson gives for saying that it
is false to say that all John's children are asleep is false when
John has no children, is that to say this would be misleading,
since the claim that all John's children are asleep is false would
normally be taken as equivalent to some of John's children are not
asleep.\footnote{\textit{Op.cit.} pp. 173-4.} But a misleading assertion, unlike one that is self-
contradictory, may nevertheless be true. The misleadingness of
saying that it is false that all John's children are asleep when
John has no children may constitute good reason for not saying it,
and perhaps may explain the reluctance in ordinary discourse to
ascribe truth or falsity to it, but this alone does not suffice to
make it false to say that all John's children are asleep. The
misleadingness arises because people are inclined to make an
inference from the statement that it is false that John's children

\textquote{Reply to Mr. Sellars', \textit{P.R.}, 1954, p. 225.} For example, if, trying to impress you with the significance of
my work, I asserted that the Adelaide University Press has under-
taken to publish my thesis, you would I am sure not hesitate to
say that my statement is false, on the ground that there is no
such press. One can agree with Strawson that cases like this are
exceptional. But if we allow that in such a case it is \textbf{correct}
to ascribe a truth value, then it seems we have admitted a
counter-example which refutes the Strawsonian theory.
are asleep to the statement that John has children. But provided
the inference is not logical, provided, that is, that no such entail-
ment holds, the falsity of the statement inferred does not transmit
back to the statement from which it was inferred.

Towards an analysis of presupposition.

I want now to sketch an alternative approach to the problem
of presupposition - one that will do justice to the facts that led
Strawson to propose his account, yet one that does not involve
either the complexities of a three-valued logic or the supposition
that the truth of its existential presupposition is a necessary
condition of the truth or falsity of a statement like 'All John's
children are asleep'. In developing my account I shall assume
the adequacy of the lower predicate calculus as an instrument for
analysing the truth conditions - but not the whole meaning - of
ordinary language statements. This is a contentious assumption,
but much less contentious once Strawson's doctrine has been put
aside.

I shall need to employ a distinction between statements and
propositions. I shall use the term proposition, in what I think
is its traditional sense, to denote that which is either true or
false: which may be believed by different people at different times,
even though perhaps they express their belief in different language; which may at one time be asserted categorically, at another be made the antecedent of a conditional, or the subject-matter of a question; which may be explicitly stated by the use of a sentence or only implied by something which somebody says or believes. And I shall use the term 'statement' to refer to any specific verbal articulation of a proposition - making identity of sentence type a necessary condition for identity of statement. Thus on my usage we may say that John and Peter assert the same proposition John by using the words 'I am tired', Peter by using the words 'He is weary' in a context that identifies John as the referent of the pronoun. But we cannot say that they make the same statement. Nor could we say that they made the same statement if only they used the same words, for if they both said 'I am tired' they would have used the same words to make different statements, since they express different propositions by them.

(I shall, however, later want to extend the sense of 'statement' to allow that what is expressed by a statement may, in some cases, not be a proposition but, for example, a rule. The conditions for identity of statement will then be identity of sentence type and identity of that, whether proposition or rule, which is articulated by the use of the sentence. This will allow me to raise the question whether what is expressed by a metaphysical statement is a proposition or something else.)
Here briefly is the approach I shall adopt. A question like 'Have you stopped beating your wife?' apparently admits only the two answers, 'Yes, I have stopped beating her,' and 'No, I have not yet stopped beating her.' Both admissible answers entail that the person has been in the habit of beating his wife. If that proposition is false then neither of the answers admitted by the question can be true. There is, moreover, a kind of absurdity involved in both asking a question and rejecting the proposition entailed by every admissible answer to it. The absurdity may be brought out if we think of the question as being equivalent to the disjunction of its admissible answers: the questioner, as it were, asserts the disjunction and requests the respondent to detach one disjunct. Now in the case we are considering the disjunction entails that you have been in the habit of beating your wife, and it would be logically inconsistent both to assert this disjunction and to deny the proposition it entails. From this point I hope to develop my account of presupposition. I want to say that a question fails to arise if the proposition entailed by the disjunction of its admissible answers is false. And I want to say that a question presupposes a certain proposition if the disjunction of the admissible answers to the question entails that proposition. But in order to make this account work, and to explicate the logical
status of the notions of presupposition and failure to arise, I
must first give an account of what it is for a statement to be an
admissible answer to a certain question.

To be a possible answer to a certain question a statement
must be relevant to it. In the case of yes-or-no questions the
relevance required is easily explicated. Such a question can always
be reformulated as a statement hooked on to a request for assent
or denial. For example, the question 'Is it raining?' can be
reformulated as 'Is it true or false that it is raining?'. I shall
refer to the statement obtained by reformulating a question in this
way as 'the statement introduced in the question'. To be an answer
to the question, then, a statement must assert or contradict the
proposition expressed by the statement introduced in the question.

Now the statement introduced in the question 'Have you
stopped beating your wife?' can be denied in either of two ways: it
may be denied either by the assertion that you are still in the
habit of beating your wife or by the assertion that you have never
beaten her. Strictly, then, the negative answer to such a question
is indefinite. It contradicts the statement introduced in the
question without definitely stating what is the case. Yet, as
Strawson showed, we would not normally take the negative answer as
being indefinite. If someone answered 'No' to the question 'Have
you stopped beating your wife?' we would normally understand him
as saying that he was still in the habit of beating his wife: and if he wished to deny this he would not simply answer 'No' to our question, he would rather say that the question 'does not arise', or words to that effect. Accordingly let us distinguish between the strict contradictory and the conventional negation of a statement, using the latter term to refer to the definite statement which would normally be taken to be asserted by someone who asserts the negation of a statement whose strict logical contradictory is indefinite. We may then say that the admissible answers to a yes-or-no question are the statement introduced by the question and its strict contradictory in case that is a definite statement or otherwise its conventional negation.

The admissible answers to a question which is not of the yes-or-no variety - for example, the question 'What did you call your new baby?' - cannot be obtained in the same way. For such a question cannot be reformulated as a statement hooked on to a request for assent or denial. Such a question, however, can always be reformulated as a statement blank hooked on to a request for completion. For example, the question 'What did you call your new baby?' may be reformulated '"I called the new baby ...." - please complete'. And the admissible answers to such a question will then include any statement obtained by completing the blank, and also perhaps the conventional negation of any such statement.
If my approach is sound then the crucial problem for the analysis of presupposition is to elucidate the distinction between the strict contradictory and the conventional negation of a statement like 'All John's children are asleep'. What is the logical significance of the tendency in ordinary discourse to take as the negation of such a statement a statement which strictly - assuming an existential analysis of 'All John's children are asleep' - stands only in contrary relation to it? The view I want to support, which was first proposed by Wilfred Sellars, is that the tendency in ordinary discourse reflects a convention governing the order in which questions should be raised and answered. Logically it could be false that all John's children are asleep because John has no children; but in a well-ordered discussion that possibility would be excluded. The convention governing the order in which questions should be raised and answered would prevent one from asking whether John's children are asleep unless it were already taken as settled that John has children.

Underlying the convention which I am postulating in ordinary discourse are certain logical relations which make it apt to speak

1. One has first to decide whether to construe a universal statement in categorical or hypothetical form before the question whether it has existential presuppositions can be settled. 'All John's children are asleep' is clearly not hypothetical.
2. 'Presupposing', P.R. 1954.
of the logical priority of one question to another. Consider again the question 'Are all John's children asleep?'. The statement introduced by this question, namely

(1) All John's children are asleep.

will, I think, be true only if it is the case both that John has children and that no child of John's is not asleep. Then, assuming that a statement is false if it is not true, 'All John's children are asleep' will be false if either it is true that

(2) John has no children

or that

(3) Some of John's children are not asleep.

That is, (1) can be true only if (2) and (3) are both false: (2) and (3) each stand in contrary relation to (1) and their disjunction forms the contradictory of (1). (This, of course, disturbs the traditional doctrine of opposition, but that seems to be unavoidable.) Further (2) and (3) stand in contrary relation to each other, and the disjunction of (2) with (1) constitutes the contradictory of (3). Thus (2) is a constituent of the contradictory both of (1) and (3) though neither (1) nor (3) is a constituent of the contradictory of (2). These relations are brought out if the above three statements are represented in the formalism of the predicate calculus. We get
(1) \(\neg(\exists x)(Jx \cdot \neg Ax) \cdot (\exists x)Jx\)
(2) \(\neg(\exists x)Jx\)
(3) \((\exists x)(Jx \cdot \neg Ax)\)

Because these relations hold one cannot settle the question whether
John's children are asleep without settling the question whether
John has children. But one can settle the question whether John
has children so as to leave it an open question whether John's
children are asleep. In this respect the question whether John
has children is logically prior to the question whether John's
children are asleep.

Well then, the convention which I suggest is implicit in the
tendency in ordinary discourse to take (3) as the negation of (1)
is the convention that one does not raise a question unless any
question that is logically prior to it is taken as settled, and
settled in such a way that the subsequent question still remains
open. Given conformity to this convention, (3) can be taken as
the negation of (1), for to assert (2) is to raise a logically
prior question and to give it an answer that precludes the question
whether John has children from arising. Given such a convention
it would be outrageously misleading simply to deny that all John's
children are asleep if one's ground for doing so is that John has
no children.

Usually there are linguistic indicators to show that dis-
course is ordered in this sort of way. Thus the use of the definite
article in making a statement like 'The leader of the Commonwealth Mission to Rhodesia was ill-chosen' is in part to indicate antecedent propositions which the current assertion assumes. It makes a reference to the context of knowledge, separating off the point now being asserted, the point at issue, from other points which are not at issue though logically they are involved in the statement being made. And the choice of grammatical subject in the statement 'All John's children are asleep' serves a similar function. It separates a prior question taken as settled from the question for which the given statement provides an answer. In both these cases - as also in cases like 'John has stopped beating his wife' and 'Bill knows his driving is dangerous' - the choice of sentence to express the proposition involved would be inappropriate unless a part of its content were taken as settled.

It might be objected that what I have called a convention is misdescribed as such, since it is an obvious heuristic maxim whose adoption facilitates the unambiguous progression of discussion. To call it a 'convention' wrongly suggests that one might as well do without it, or anyway replace it by a different but equally suitable convention. But by using the term 'convention' I do not mean to deny the heuristic value of the rule, nor do I want to deny its peculiar logical appropriateness. My point in using this term
is simply to contrast such a rule with a rule of logic: to stress that it is not a necessary rule to be obeyed on pain of self-contradiction. Were it absent discourse would be more complex or less definite than it now is; and were the convention generally accepted but on some occasion flouted, the result would not be a self-contradiction, but only an ambiguity resulting in confusion and misunderstanding. For this reason heuristic conventions have to be distinguished from formal logical laws, and the study of them to be distinguished from formal logic. One might use the term 'informal logic' or, as I would prefer, 'heuristic' to mark the distinction. And similarly those notions which involve heuristic conventions, as on my account does the logical sense of presupposition, should be distinguished from strictly logical notions, like entailment. Perhaps it would be better to speak of 'heuristic presupposition' rather than of 'logical presupposition'.

Having presuppositions is, then, on my account not a logical property of statements abstractly considered as propositions, as Strawson apparently thought: it is a property of statements considered as episodes in a well-ordered discussion. To say that a statement $S$ presupposes $S'$ is to say that in a well-ordered discussion the question whether $S$ is true would arise only if the question whether $S'$ is true is already taken as settled. And this I think
is what is sound in Collingwood's suggestion that it is questions, or statements answering questions, that have presuppositions.

To summarize my view, what I began by calling 'logical presupposition' and want now to call 'heuristic presupposition', does indeed involve a logical relation, though heuristic presupposition is not just a logical relation, and the relation involved is not a peculiar logical relation but ordinary entailment. What there is over and above the entailment is a convention whose effect is to take as the negation of the statement which is said to have a presupposition a statement which is such that its disjunction with the statement negated entails the proposition presupposed. In a sense, what is presupposed is not part of what is asserted by someone who makes the statement which presupposes it. But this, on my view, is not because what is presupposed is not entailed by the statement made, but because a heuristic convention prevents one from making the statement until the truth of its presupposition is no longer at issue.

Metaphysical 'Presuppositions'

There were I think two things about the logical sense of presupposition which made it seem promising for the job of explicating the relationship between science and metaphysics.
First, it was evident that there is a sort of inconsistency involved in both asking a question, or making a statement, and denying the presupposition upon which it logically depends. So if it could be shown that scientific discourse always has metaphysical presuppositions, the ground would be well prepared for the charge that the positivists, who uphold science while denigrating metaphysics, are behaving inconsistently. It is clear that Collingwood hoped to rebut the positivist programme in this way. Secondly, it seemed that presupposition is a logical relation distinct from entailment, so that it might be possible to maintain that science has metaphysical presuppositions without maintaining that scientific theories always entail metaphysical suppositions. And in that case it could be granted not only that metaphysical suppositions are always eliminable from scientific theory, but also that they should always be eliminated, without at the same time admitting that metaphysical suppositions should or could be altogether eliminated from scientific discourse. Metaphysical suppositions, it could now be argued, are involved in scientific discourse as presuppositions of the questions being asked, not as elements in the theories which purport to answer them.

2. Collingwood does not explicitly assert that presupposition is distinct from entailment, but I think this is implicit in his claim that in neglecting to study presuppositions logicians have neglected a 'kind of connection between thoughts', E.M. p. 23.
If my account is sound, however, the notion of presupposition cannot fulfil the hopes held for it. For if I am right the sense of presupposition which makes it inconsistent to ask a question while denying what it presupposes is to be analysed partly in terms of the entailments of any admissible answer to that question. Hence scientific questions could be said to presuppose metaphysical suppositions, in that sense of the word which involves a logical commitment to that which is presupposed, only if whatever statements might be admitted as answering such questions entail metaphysical suppositions. So it turns out that the claim that scientific questions always logically presuppose metaphysical suppositions entails the thesis that scientific theories always have metaphysical entailments. And Collingwood believed this thesis false.¹

Moreover, if it were the case that scientific questions have metaphysical presuppositions - in that sense of presupposition in which it is inconsistent to ask the question and deny the presupposition - then it could not be maintained that a supposition which enters scientific discourse as a presupposition does not as such function as a proposition, i.e. as something true or false. For, on my account, if a certain supposition is presupposed by a question, its truth is entailed by the truth of any admissible

¹ E.M. pp.40-46.
answer to that question. Thus a supposition which is involved in science as a presupposition is as such involved in science as a proposition: and its truth or falsity cannot then be a matter of indifference to the scientist. The presupposition does not merely give rise to the question, its truth is a necessary condition of the truth of every admissible answer to that question.

Finally, if the sense in which scientific questions are supposed to have presuppositions is the sense in which a question logically cannot arise unless the presupposition is made, then the thesis that scientific questions ultimately rest on metaphysical presuppositions is a thesis that cannot be sustained. Collingwood apparently thought the thesis could be proved by a simple regress argument.¹ Every question has a presupposition; if the presupposition is relative it may be questioned, and this question in turn will have a presupposition which if relative may be questioned, and so on until an absolute presupposition, i.e. a presupposition which cannot be questioned, is reached. There are two things wrong with this argument. First, if the premise of the argument were granted it would follow that someone who systematically tried to question every presupposition being made in some inquiry would somewhere have to stop before he had completed his programme.

But it would not follow that there was some one presupposition at
which the questioning programme would always have to stop, i.e. an
unquestionable presupposition. Secondly, the premise of the argu-
ment, that every question has a presupposition, would seem to be
false. For a question can have a presupposition, of the sort we
are interested in, only if the statement introduced by that question
exhibits a logical structure such that the negation of that
statement is indefinite (disjunctive). Not every statement has
such a structure, and hence not every question has a presupposition.
For example the question 'Are all John's children asleep?' presupposes
that John has children, and this presupposition may be questioned
without introducing any further presupposition. If it is not the
case that every question has a presupposition then it is possible
for the systematic questioning of presuppositions to end with a
presupposition that can be questioned without further presupposition.

I conclude that the logical concept of presupposition - what
I have called 'heuristic' presupposition - cannot be used to explicate
the relation between science and metaphysics. It may be that meta-
physical principles are sometimes taken for granted in scientific

1. It might be claimed that there is a further presupposition in-
volved, namely that 'John' has a referent. But this is not a
presupposition of the statement that John has children, rather
it is a semantic condition which has to be satisfied for the
use of the sentence 'John has children' to make a statement.
A similar situation arises with the use of expressions like
'this' and 'you'. If the semantic condition is not satisfied
we cannot say what it is that is being asserted, whereas in the
case of presuppositions we can say what is being asserted, what
is claimed to be true, even if the presupposition is not true.
discourse, and influence the conduct of research, but there seems no reason to suppose that such principles are logically presupposed by science. There may still be some point, though, in speaking of 'absolute' or 'metaphysical presuppositions' of science. For suppose it could be shown that there are principles which have an indispensable, or at least a valuable function in scientific discourse; yet which are such that they could not be tested — confirmed or refuted — by any scientific procedure. One might appropriately call them Absolute presuppositions. Collingwood thought there were such principles. He mistakenly tried to explicate their role in terms of the logical notion of presupposition. But he had a good deal else to say about the function of absolute presuppositions in science, some of which is more promising.
What is an 'Absolute Presupposition'?

Collingwood distinguished between absolute and relative presuppositions. He defined a relative presupposition as 'one which stands relatively to one question as its presupposition and relatively to another question as its answer'.¹ And he defined an absolute presupposition as 'one which stands relatively to all questions to which it is related as a presupposition never as an answer'.²

Collingwood's definitions make it clear that he thought that the same relation of presupposition is involved in the two sorts of case, i.e. that the difference between an absolute and a relative presupposition is not a difference between two ways of being presupposed. For whether a presupposition is absolute, under the above definition, is determined solely by whether that which is presupposed stands in other relations to questions than that of being presupposed. If it is only presupposed by questions then the presupposition is called 'absolute'; if that which is presupposed by one question stands as answer to another question then the presupposition is called 'relative'. But it is not made clear whether the questions mentioned here are actual questions.

¹ E.M. p. 29. ² E.M. p. 31.
questions in fact raised by someone at some time, or possible questions. So it is unclear whether for something to be an absolute presupposition it must not in fact be asserted in answer to any question actually raised, or whether it must satisfy the stronger condition, that it should not even be a possible answer to some possible question.

It might seem that there is no real ambiguity here. For given any presupposition S, it is always possible to construct a question having that supposition as its answer. Thus the stronger condition mentioned above seems impossible to satisfy; and from this it would follow that it is only in the weaker (contingent) sense of the word 'absolute' that there could be any absolute presuppositions.

One way of arguing for this view is the following. Take any question Q whose presupposition is S - where S can be any supposition whatever - now ask a second question about the first, namely 'Does the question Q properly arise?' To this second question the affirmative answer would have to be 'Yes, since S', where S is asserted. Here, under Collingwood's definition, S is only a relative presupposition of Q. It follows that for any presupposition whatever, that presupposition can be absolute only
in the sense that what is presupposed has not been asserted in answer to any question actually asked - either because the relevant question has not in fact been asked or because the person making the presupposition has not recognized it and so cannot correctly answer a question of the second sort.

The contingent interpretation of the claim that there are absolute presuppositions apparently fits much of what Collingwood says about absolute presuppositions. In particular it would account for the importance he attached to the claim that absolute presuppositions are in general held unconsciously. For if absolute presuppositions exist only because the relevant questions have not been asked, or if asked not answered, one could perhaps best explain their existence by pointing out that scientific questions often do have presuppositions of which those who ask them are unaware. Indeed, if the term 'absolute' is interpreted in the contingent sense, then it is hard to see how anything could be an absolute presupposition unless it were not consciously recognized as such. For to discover what is presupposed by a

1. cf. E.M., p. 48 & my earlier exposition of Collingwood's theory.
certain question one must ask a question of the form 'On what supposition does Q arise?'; and if, having succeeded in answering this question, one yet persists in asking the original question, then surely one is in effect asserting the statement it presupposes. It begins to look as though there is a logical barrier to becoming aware of one's absolute presuppositions. Anything of which one is aware is, by virtue of that fact, not an absolute presupposition. One can discover only what one's absolute presuppositions were.¹

However Collingwood did not draw these conclusions. Although he held that people are not normally aware of their absolute presuppositions, he still wanted to allow that it is possible to become aware of one's absolute presuppositions, without either abandoning them or changing their status. He wrote²

The fact that we have learned what our absolute presuppositions are does not imply that our attitude towards them either should or can cease to be one of sheer presupposal.

This passage, it seems to me, is quite incompatible with the contingent interpretation of the notion of an absolute presupposition. And it is not an isolated passage.³ The fact is that Collingwood rejected the contingent interpretation; indeed, rejected it quite

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³ cf. E.M. p. 45.
explicitly. In the Chapter 'On Presupposing' he wrote

That a certain conclusion follows from certain premises
is not disproved by the fact that someone who states
the premises fails to see that the conclusion follows.
Similarly that certain presuppositions are questionable
is not disproved by the fact that someone who makes
them fails to see that they are questionable.

And later he wrote

In such cases the analysis has not been pushed home with
sufficient firmness to settle the question whether the
things are being taken relatively for granted or ab-
solutely for granted: whether they are suppositions
whose verification is being deferred to a more appropriate
occasion or procrastinated out of idleness or faintness
of heart, or suppositions which in principle neither admit
nor require verification.

Here it is clear that Collingwood is working with a more stringent
test for absolute presuppositions than that which would be
appropriate if the question whether a presupposition is absolute
or relative were to be settled solely in terms of the questions
actually asked. To establish that a presupposition is absolute
it does not suffice just to show that someone has not asked whether
the presupposition is true; what we must show is that the
presupposition is not questionable: where to question a presupposition
is to inquire whether it is true, to ask for it to be justified or
or verified.

To question a presupposition is to demand that it should be 'verified'; that is, to demand that a question should be asked to which the affirmative answer would be that presupposition itself, now in the form of a proposition ... Hence to speak of verifying a presupposition involves supposing that it is a relative presupposition.

So it would seem that for Collingwood, a presupposition is questionable if and only if it is verifiable; if it is unverifiable it is an absolute presupposition.¹ Verifiability thus provides the criterion by which relative presuppositions may be distinguished from absolute presuppositions.

Adoption of this more restrictive test for relative presuppositions avoids the argument, presented above, to the conclusion that absolute presuppositions are 'absolute' only in a contingent sense. It also provides a counter to a similar argument employed by Donagan.² Donagan's argument runs as follows:

A question can be constructed out of any absolute presupposition simply by prefixing the words 'Is it true that ... ?' to it; and doing so will convert the original absolute presupposition into an affirmative answer to the new question, and thereby into a relative presupposition. The fact that it is open to anybody who is 'ticklish in his absolute presuppositions' to refuse to ask the question, and so to leave his presupposition intact, is beside the point, which is that he could construct and ask the question if he chose.

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¹ cf. E.M. p. 42.
² D. p. 76.
Collingwood's counter to this argument would surely be that although one might always phrase such a question about the truth of a presupposition, it is a futile and indeed an improper question to ask if the presupposition is not questionable, i.e. if it is not such that its truth can be verified or tested empirically. The question 'Is S true?' presupposes that S can be verified, and if that presupposition is false then the question does not properly arise.1

Donagan used the argument which I have quoted to prove that an absolute presupposition can always be converted into a relative presupposition. To make the conversion, he claimed, we have only to ask whether what is presupposed is true. He then went on to argue that Collingwood's use of the verifiability criterion for distinguishing relative from absolute presuppositions is inconsistent with the convertibility thesis. For if every and only unverifiable presuppositions are absolute, no absolute presupposition can be converted into a relative presupposition - asking whether it is true does not alter the empirical status of a statement. And since, according to Donagan, the convertibility thesis is a

necessary consequence of Collingwood's definition of 'absolute presupposition'; it follows that the verifiability criterion should be abandoned. But, it seems to me, to abandon the claim that absolute presuppositions are unverifiable would drain most of the interest from the doctrine of absolute presuppositions. Verifiability was taken by Collingwood to be the mark of an empirical statement - a statement is to be counted empirical if and only if observations are 'relevant to the determination of its truth or falsehood' - so the claim that absolute presuppositions are unverifiable is essential to the thesis that metaphysics is the study of absolute presuppositions. Moreover, if the convertibility thesis is accepted then two other important theses of Collingwood's theory have to be rejected. First, the thesis that absolute presuppositions are not true-or-false propositions will have to go, for certainly something could not become true-or-false just because someone asks if it is true. And secondly, the claim that absolute presuppositions are ineliminable from science has to be rejected, for absolute presuppositions on this view may

2. This rather vague characterization of verifiability is drawn from Ayer's *Language Truth and Logic* (p. 38 of 2nd edition), but it is one of which Collingwood approved, though he expressed doubts about the adequacy of Ayer's more rigorous explication of the kind of relevance required for verifiability (B.M. p.65). I shall take up this problem in Chapters 5 and 6.
always be eliminated through conversion into relative presuppositions. It is not surprising, then, that Collingwood did not accept the convertibility thesis. And, it seems to me, he could consistently reject that thesis just because he employed verifiability as the criterion for relative presupposition.

Verifiability and Truth

However the central position which the notion of verifiability occupied in Collingwood's theory of absolute presuppositions tends to be obscured by the manner in which the theory is presented in Part One of the Essay on Metaphysics. For there Collingwood appears to deduce the unverifiability of absolute presuppositions as a corollary of the theorem that absolute presuppositions are not true or false: 1

... the distinction between truth and falsehood does not apply to absolute presuppositions, that distinction being peculiar to propositions. Hence any question involving the presupposition that an absolute presupposition is a proposition, such as the questions 'Is it true?' 'What evidence is there for it?' 'How can it be demonstrated?' 'What right have we to presuppose it if it can't?', is a nonsense question.

And the theorem that absolute presuppositions are not propositions is apparently supposed to be deducible from the thesis that every proposition answers some question, given the definition of an absolute presupposition as that which is presupposed by some questions but answers none.¹

**Absolute presuppositions are not propositions**

This is because they are never answers to questions whereas a proposition is that which is stated, and whatever is stated is stated in answer to a question.

It would appear then that Collingwood meant the thesis of the unverifiability of absolute presuppositions to be taken merely as one consequence of the theory of absolute presuppositions, whereas on my interpretation it is far more fundamental to the theory.

But Collingwood's argument, if it is meant as an argument, is blatantly inadequate to support the conclusions he hoped to draw. For if the unverifiability of absolute presuppositions is to be made a conclusion of the argument, then we cannot take their unverifiability as a criterion for identifying absolute presuppositions, or as a ground for asserting that certain presuppositions are unquestionable. And likewise if the thesis that absolute

¹. H.M. p. 32.
presuppositions are not true-or-false is to be demonstrated by the argument, then we cannot take their being neither true nor false as a reason for asserting that certain presuppositions are unquestionable. The only means then remaining by which absolute presuppositions might be identified would seem to be to identify them as those presuppositions which in fact have not been questioned by someone at a certain time. Whether something is an absolute presupposition would then, as Donagan maintained, be contingent upon the questions in fact asked by people in any given period. Yet if this were so then we could not hope to use the claim that absolute presuppositions are not answers to questions as a premise to the conclusion that absolute presuppositions are neither true nor false. For that would make the question whether a presupposition is true-or-false dependent upon whether someone asks if it is true: which is both untenable and explicitly denied by Collingwood.1 Rather from the fact that some presuppositions which might have been questioned have in fact not been questioned we should have to conclude that it is false that every true-or-false proposition has in fact been questioned.

The major premise of Collingwood's argument is plausible at all only if it is interpreted as claiming not that every proposition is in fact questioned at some time, but that every proposition is questionable. And if its major premise is interpreted in this way then Collingwood's argument can be got going only if the minor premise is taken as postulating that certain presuppositions are not questionable. What makes them not questionable? It can only be either that they are unverifiable or that they are not true-or-false.

But if, as I have suggested already, the unverifiability of a presupposition is taken as the criterion of its absoluteness and as the ground for the claim that it cannot be questioned, then a more powerful argument is obtained for the theorem that absolute presuppositions are not true-or-false. Collingwood's argument now becomes:

Every true-or-false proposition is verifiable.

Absolute presuppositions are unverifiable.

Therefore absolute presuppositions are not true-or-false.

Collingwood himself adopted this argument later in the Essay on Metaphysics.¹ Here the major premise of the argument is a fairly

straightforward statement of the verifiability principle, though a more moderate variant of the principle than that adopted by the early positivists. Verifiability, in this context, is made a condition of truth-or-falsity, but not a condition of significance or of scientific relevance. It remains an open question whether an unverifiable supposition might nevertheless have a useful function in scientific discourse.

How could the major premise of the revised argument - i.e. the verifiability principle - be supported?

Collingwood did not tackle this problem. Because he was in substantial agreement with the positivists on this point, he saw his task as that of showing how a supposition might yet have a useful role in science even although it is unverifiable and not true-or-false. The verifiability principle itself he adopted naively, without either carefully formulating it or trying to elucidate its logical status. But we shall have to do more than this.

I do not think it is possible to find a priori arguments for the truth of the verifiability principle. And it is clear that we shall not be able to find inductive support for the principle: for this would be possible only if we had an independent test by which it could be determined whether specific suppositions are
true-or-false, and no such test is available. By the nature of the case, then, the best we can hope for are plausible reasons for accepting the verifiability principle, either as a postulate or as a conceptual stipulation, to be evaluated in the end by an appeal to the coherence and fruitfulness of the system built upon it. And I think plausible reasons for accepting the verification principle may be discovered in Collingwood's argument.

It is commonly maintained that a non-analytic and unverifiable supposition can have no legitimate place in scientific discourse. It cannot be employed, for example, in a scientific explanation, for the idea of testability is written into the concept of a scientific explanation. Nor can it be employed in support or refutation of a scientific theory, for an unverifiable statement no more supports a theory than its contradictory refutes it: the two, being equally bare logical possibilities, cancel out. Such a statement might - i.e. it is not logically impossible that it should - be true; but whether it is true or false is a matter of no significance to the scientist. From the point of view of science we can proceed as though unverifiable statements are neither true nor false.¹

¹. Newton, Correspondence, ed. Edleston, pp. 154-5.
Whatever is not deduced from phenomena is called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy.

Here we have a restricted positivist position: it does not question the meaningfulness of even the factual significance of unverifiable statements, it merely excludes them from the domain of scientific discussion, counts them irrelevant to any scientific problem.

Collingwood would have rejected even such a restricted form of positivism. Although an unverifiable statement cannot legitimately be propounded in answer to a scientific question, yet, he held, such a statement might nevertheless have another function in scientific discourse. It might function not as a proposition—something that might be asserted in answer to a question—but as a supposition, something which 'gives rise' to questions. The causal principle might be taken as a case of this sort: it could be argued that the statement 'Every event has a cause' is such that no empirical inquiry could either verify or falsify it, yet the causal principle may 'give rise to' questions, like 'What is the cause of lung cancer?', to which the procedures of empirical science are relevant. Here, though the question whether the principle is true is not a scientific question, the questions which arise from that principle are genuinely scientific. In this way, by functioning as a question generator, an unverifiable supposition might gain
entry to scientific discourse.¹

What the restricted positivist position brings out is that unverifiable statements cannot legitimately function in scientific discourse as factual assertions, or as explanatory hypotheses. Our counter-example does not refute this, but suggests that unverifiable statements might have a different function in science, namely that of 'giving rise' to scientific questions. From the point of view of science the question of the truth or falsity of such statements remains a question of no significance: it does not affect their 'logical efficacy', i.e., their power of giving rise to questions, and in this alone consists their relevance to scientific discourse. Here then we have plausible ground for a legislative decision. The stipulative move is to declare that if a statement S cannot function in science as a true-or-false proposition — if we could never be in a position to claim to know either S or not-S — and if S can function in scientific discourse in another way, if it can generate scientific questions for example, then S shall not be considered to state a true-or-false proposition, instead it shall be taken as nothing but a question generator.

We must turn now to examine Collingwood’s remarks on the role of absolute presuppositions in scientific discourse.

¹. cf. E.N., pp. 32 & 147; I shall return to the elucidation and criticism of this point at a later stage in the thesis.
The Role of Absolute Presuppositions

Collingwood developed his theory of metaphysics in opposition to an account of scientific knowledge which he ascribed to radical empiricists like John Stewart Mill and the early logical positivists. According to this account scientific knowledge is inductive. It begins with observation and experiment and proceeds to the extrapolation of laws. Where metaphysical principles intrude their effect is just to impede the growth of scientific knowledge. The scientist and the philosopher both should be vigilant against such intrusions.

According to Collingwood this doctrine is seriously deficient as an account of scientific knowledge. It describes science in a primitive pre-theoretical phase, in which scientists are concerned exclusively with the discovery of empirical regularities and with the forecasting of future occurrences. But it quite overlooks the theoretical concerns of modern science; that is, it overlooks the scientist's interest in the explanation or understanding of natural occurrences. For what distinguishes modern science from sheer speculation is not a total exclusion of speculation, but the way speculation is disciplined in science by the demand for
testability. The modern scientist has learned to criticize his
speculations. Or, as Collingwood put it, he has learned the art
of 'putting nature to the question', and of 'extorting answers
from her'.

Scientific thinking is unlike the activity either of a
spider, which spins its web out of its own bowels, or of
an ant, which collects and stores every bit of food it
comes across. Scientists are neither mere theorists
nor mere fact collectors. Rather, they are like the
bee, which gathers food from flowers, but transforms it
in its belly: they seek facts, but only in order to
answer questions; and by their questions, they
transmute them into scientific knowledge.

The important point is that the theories of modern science, theories
like those of Newton or Einstein, are such that they could not have
been extrapolated from the manifold of experience. They not only
describe the course of nature, they explain it as well. They do
not merely extrapolate from experience, but also strive to penetrate
beneath it to the underlying structure of reality. An adequate
theory of scientific inquiry should be able to account for this.

Collingwood's methodological remarks stress the conjectural
classic of science. The scientist approaches nature with certain
problems in mind. He proceeds by framing hypotheses, possible

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2. D. p. 151, summarizing views expressed by Collingwood in the
New Leviathan, p. 248.
solutions to his problems, and then devising experiments by which these hypotheses may be tested. Instead of waiting for experience to reveal regularities to him, he actively strives to impose regularities upon it, to interpret experience in terms of theories which he has himself invented. Collingwood usually made this point by saying that science is the systematic questioning of nature. 'Questioning', he remarked in *Speculum Mentis*, 'is the cutting edge of knowledge.' The same point was repeated in most of his later books.

The science of nature begins when man begins 'putting nature to the question' ... , extorting from her an answer to the questions he chose to ask, instead of contenting himself with noting down whatever she elected to reveal.

Two conditions have to be satisfied by any scientific questioning of nature. First, the scientist's questions must be informed both by the knowledge already acquired (in answer to previous questions) and by his speculations about the manner in which everything fits together. A scientific question should

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not be haphazard or random, but one 'which "arises", a question which for logical reasons has to be asked.\(^1\) Secondly the scientist's speculations must be open to empirical testing; they should lead to the formulation of specific hypotheses capable of confirmation or falsification by observation and experiment. Unless this condition is satisfied the scientist's questions are purely speculative, incapable of being answered by empirical research.

An every-day example which Collingwood offered illustrates both these points.\(^2\) Suppose my car won't start and I want to know why. Knowing something about cars I can suggest possible causes; perhaps the carburettor is flooded, perhaps one of the spark plugs is not functioning. If the carburettor were flooded there would be a noticeable smell of petrol. I can check on that. If a spark plug is not functioning I can - or could have in a pre-war car\(^3\) - remove each plug and watch whether it sparks when the crank handle is turned. If no spark is observed from a particular plug I have solved my problem; if all the plugs function I must try yet another hypothesis. Collingwood distinguished two kinds question here, which both have their counterpart in scientific

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inquiry. First there are the general questions which trouble the scientist, questions like 'Why won't my car go?' One may ask such a question without being able to suggest a possible answer. A great deal of imagination, ingenuity and knowledge may be needed before even plausible possible solutions can be developed. Secondly there are specific questions, like 'Is there a strong smell of petrol', which arise once possible solutions to the general problem are proposed. These are the questions which the scientist puts to nature. Alternative possible answers are already specified by the question; one has just to decide between the alternatives by experiment. The difficult task in science is to pass from questions of the first sort to questions of the second; that is, to pass from general perhaps rather vague problems to specific empirically testable hypotheses.

Now do absolute presuppositions fit into this scheme?

According to Donagan, whose exposition of Collingwood's account of scientific inquiry is very fair so far, there is no place for absolute presuppositions at all. Indeed he thinks the doctrine of absolute presuppositions is incompatible with the critical empiricism of Collingwood's remarks on the logic of inquiry.

His logic of inquiry analyses the phases of the scientific search for knowledge. But if the theory of absolute presuppositions is sound that search should be both unnecessary—since scientific theories should then be deducible from the absolute presuppositions currently accepted in science—and futile—since absolute presuppositions, and presumably anything deducible from them, are supposed to be neither confirmable nor falsifiable, not possible items of knowledge at all. However this objection assumes that absolute presuppositions are related to scientific theories as postulates to theorems in a deductive system; and I have already argued against this interpretation of Collingwood's theory.¹

The role of absolute presuppositions is better construed as a regulative one. On this account—which W.D. Gallie advocates²—absolute presuppositions function as general directives for an ongoing research programme. They do not provide answers to the scientist's questions, but control and co-ordinate the questions he asks, and so condition the emerging structure of scientific theory.³

¹ In Chapter 2.
² Philosophy and the Historical Understanding, Chapter 10, esp. pp. 219 ff.
³ cf. E.M. p. 52.
If this is to be their role then absolute presuppositions can readily be found a place in Collingwood's account of scientific inquiry. Our car example will bring this out. Suppose when my car breaks down I merely shrug and leave it; and when someone asks why I don't try to find out what is wrong with the car, I reply 'There's nothing wrong, it just won't go.' My response would certainly be thought rather odd. Cars - and toasters and typewriters and all other contraptions - do not just not go; if they fail to function it is because something has gone wrong. This belief, which for most of us is very nearly unshakeable, gets us looking for the cause of a breakdown, and keeps us looking even when our search is unsuccessful. We may, of course, in time give up looking for the cause; but if we do we will probably say that it's a lack of time or knowledge or persistence on our part that makes us give up, we won't say that we've found that there is not something wrong at all. Here the belief that there must be something wrong if the car won't go functions as, on the regulative account, absolute presuppositions function in science.

Now a scientific example: Max Black\(^1\) cites the case of Claude Bernard, an eminent French physiologist of the last century.

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According to Bernard the principle of determinism is 'the absolute principle of science'. Black comments:

I am inclined to think that Bernard's determinism, as he used it, can be shown to have been an active instrument of research and criticism. His practice shows his faith in ultimate rationality lighting the path to original discovery. When the physicians of his day invoked the tact and intuition of the medical practitioner, Bernard counters with a stubborn search for causal sequences; when other theorists hid their ignorance of biological laws by appealing to some mysterious 'vitality' or vital force, he condemns them for irrationality and continues to experiment.

Examples of this sort are easy enough to find. There is no doubt that metaphysical principles do sometimes function regulatively in scientific inquiry, and that their influence on the conduct of research may be profound. If the task of metaphysics is just, in Gallie's words, 'to give us a followable and acceptable account of how certain regulative principles ... arose, came to dominance and perhaps declined during a certain period of intellectual history', then metaphysics has a secure place in the economy of learning.

Yet rather more than this is needed to sustain Collingwood's theory of metaphysics. For Collingwood sought not only to vindicate

1. op.cit. p. 18. 2. op.cit. p. 224.
metaphysics as the historical study of absolute presuppositions, he also wanted to vindicate having metaphysical commitments in science. He hoped to show that absolute presuppositions have a proper and integral place in scientific inquiry. And to show this something more is required than a demonstration that metaphysical principles may have a regulative role in research. It has also to be shown that their acceptance tends to promote the growth of science. Whether this can be shown depends, in part, on what account is given of where and how absolute presuppositions exercise their regulative function.

According to Gallie, 'metaphysical statements commend certain lines or styles of research and discourage others.' In a similar vein Max Black describes metaphysical principles, like the principle of determinism, as 'leading ideas' which guide and co-ordinate theoretical inquiries. But, even if we overlook the vagueness of such talk, it is difficult to see how anything as general as a metaphysical doctrine could be much use in guiding research. To give a positive lead in theory formation something much more specific than determinism is required, something more like an incipient theory. Perhaps, like religious faith, metaphysical doctrines are

1. op.cit. p. 221.  
3. op.cit. p. 19.
more a source of strength and determination than a positive help in the scientist's work. But in that case Collingwood's claim that the elimination of metaphysical commitments would undermine the foundations of science is grossly exaggerated.

The role of metaphysical directives begins to look more significant if their relation to science is seen as a normative one. Stephen Körner, for example, has suggested that metaphysical statements 'define ... standards of the intelligibility or explanatory power of physical formalisms.' This makes their role similar to that often ascribed to the principle of simplicity. A scientist may appeal to a metaphysical principle in choosing between two physical formalisms, both of which imply the same empirical laws of nature. Or, appealing to such a principle, he may express dissatisfaction with an empirically adequate formalism, thinking that it fails to make the phenomena sufficiently intelligible. Thus, to take one conspicuous example, Einstein objects to the quantum theory because of 'its attitude towards what appears to be the programmatic aim of all physics: the complete description of any individual real situation (as it supposedly exists irrespective of any act of observation or substantiation)."

Körner's account may be more adequate than any we have so far encountered as an explication of the influence of metaphysical principles in science, but still, it seems to me, it fails to vindicate their having such an influence. Körner tries to achieve this with the aid of a distinction between a physical formalism - i.e., an empirically interpreted deductive system - and a physical explanation. On his view a physical formalism may be adequate for the deduction of the laws for some domain, and survive experimental testing, yet not provide an adequate or intelligible explanation of those laws. But this distinction is of dubious value. There seems to be no other reason for making it than the fact that scientists occasionally criticize a theory on what look like metaphysical grounds. It does not seem to be the case that metaphysical principles do a job which is created by the distinction between a physical formalism and an adequate explanatory theory, but rather that the distinction is created by the influence of metaphysical commitments in science. And if this is so then the distinction between a physical formalism and a physical explanation does not show the regulative role of metaphysical principles in science to be other than a needless and dogmatic contamination.
Collingwood speaks of absolute presuppositions 'giving rise' to questions or 'causing questions to arise'\(^1\) - absolute presuppositions, on this view, regulate science through the generation of problems to be investigated.\(^2\) A scientist shows his adherence to a certain absolute presupposition not by what he asserts but by the sorts of question he is disposed to ask.

According to Gallie, 'no sense can be given to the idea of a presupposition as a sufficient condition or cause of the putting of a question.'\(^3\)

The most that could be expected ... is that certain presuppositions not only permit but have the effect of encouraging questions of a certain kind ... they guide us along certain lines of thought, they prompt us perhaps to put certain questions, but they cannot cause us to think or to ask anything. Or to speak more exactly, metaphysical propositions or statements of absolute presuppositions, do not entail any specific scientific consequences.\(^4\)

I think, however, that it may be possible to make something more of the notion of 'giving rise' to a question. I think, that is, that we may find something like an entailment relation holding between a supposition and a question, a relation such that if one makes the supposition then the question arises, becomes relevant,

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1. e.g. E.H. p. 27.
3. op. cit. p. 218.
4. op. cit. p. 219.
or pertinent. For example, if in the course of an archaeological investigation one found an artifact of some sort, and if one supposed that all artifacts have a use or function, then a question would arise as to the use to which the present artifact had been put. The supposition that all artifacts have a use gives rise to, or is a logically sufficient reason for asking, a question about the use of the present artifact. Of course that question might not ever actually be asked - just as one might fail to recognize the logical consequences of a statement one had made - but it would be a pertinent question to ask, and, if the supposition were sound, it would have a true answer. (I shall try to analyse the notion of giving rise to a question later, in Chapter Seven.)

**Giving rise to a question**, it should be noticed, is not the converse of being presupposed by a question, though I think Collingwood mistakenly thought that it was.1 A logically sufficient condition for asking a question need not also be logically necessary. For example, the question about the use of a certain artifact might equally arise on the assumption that all artifacts of this sort have a use, or that all artifacts found in this place have a use. Metaphysical principles might, then, give rise to scientific questions without being logically presupposed by them. If they do then they

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will not be logically indispensable to science, for the questions they generate might arise independently. But this need not diminish their value. The generality and open texture which are characteristic of metaphysical principles may make them especially fruitful as generators and co-ordinators of scientific questions; and give them an important place in the 'situational logic' of science: i.e., in the appraisal of the present achievement and future tasks of the scientific enterprise. At least this is a possibility worth investigating.
I turn now to consider the question whether a supposition which is unverifiable, or, more generally, non-empirical, could function in scientific discourse as something which gives rise to scientific questions. One problem which obviously arises here is to show how a supposition could be sufficiently relevant to science to generate empirical questions without itself being empirical. But in order to discuss this question we shall first have to look into two other problems. The first is the problem of demarcation, how to distinguish non-empirical statements from all other statements; the second is the problem of explaining what it is for a supposition to give rise to a question. The first of these problems will delay us for some time. But the delay is worthwhile because the notion of a non-empirical supposition occupies a position of fundamental importance in Collingwood's theory of metaphysics.

We already have a first rough characterization of empirical statements: a statement is said to be empirical only if it is such that observations are relevant to the determination of its truth or falsity. The term 'verifiability' is used to denote the relation which holds between an empirical statement and the relevant
observation statements. Our task is to specify more precisely the nature of this relation. Any attempt to specify the kind of relation which should hold between an empirical statement and the relevant observation statements must be partly stipulative. One cannot deduce the nature of the relation from the definition of 'empirical', for we want to specify that relation in order to construct a definition of 'empirical'. Nor can we expect to reach a specification of the kind of relation involved merely by describing standard cases of the normal use of the word 'empirical', for it is not to be expected that its use prior to such a specification will be either clear-cut or consistent. Some regimentation is unavoidable. Accordingly it is important to formulate criteria by which to judge the adequacy of any proposed explication of 'empirical', and these criteria should be selected in the light of the issues we hope to be able to clarify by the use of the refined concept of an empirical statement. I propose to adopt the following two adequacy conditions:

1. Any adequate explication of 'empirical' should exclude some statements from the class of empirical statements.
2. Any adequate explication of 'empirical' should be such that it does not exclude from the class of empirical statements any statement of a kind which has an indis-
pensable place in scientific explanations, and whose truth is a condition of its use in a valid explanation.

Thus, for example, any explication of 'empirical' which excludes statements of natural law from the class of empirical statements should be judged inadequate; and any explication which includes analytic statements or such obviously non-empirical statements as 'God exists' or 'Monads have no windows', should also be judged inadequate. The crucial question for the theory of metaphysics is the question whether an explication of 'empirical' which is adequate by these criteria will exclude from the class of empirical statements certain statements which are capable of 'giving rise to' scientific questions, i.e., statements which might function in scientific discourse as presuppositions.

I shall try to arrive at an adequate explication of the notion of an empirical statement by undertaking a criticism of two earlier attempts to find criteria for empirical statements, namely Ayer's verifiability criterion and Popper's falsifiability criterion. I shall consider Popper's criterion first.

Popper began, he tells us, in 1919 with doubts about the scientific status of the theories of Marx, Freud and Adler. 'What is wrong', he asked himself, 'with Marxism, psycho-analysis and individual psychology? Why are they so different from physical
theories, from Newton's theory, and especially from the theory of relativity?\textsuperscript{1} These theories appear to possess the credentials of science - they apparently have wide explanatory power and are extensively confirmed by experience - yet to Popper they seemed pseudo-scientific, more like astrology than astronomy. The trouble, he came to believe, was that these theories 'explain' too much and are 'confirmed' too easily: whatever happens, or might happen, in the field of human behaviour could be interpreted in accordance with the theory of Marx, or of Freud, or of Adler. In physical theory, however, the situation is quite otherwise. A theory like Einstein's yields precise predictions which not only admit spectacular confirmation but also expose the theory to the possibility of decisive refutation.\textsuperscript{2}

Take one typical instance - Einstein's prediction, just then confirmed by the findings of Eddington's expedition. Einstein's gravitational theory had led to the result that light must be attracted by heavy bodies (such as the sun), precisely as material bodies were attracted. As a consequence it could be calculated that light from a distant fixed star whose apparent position was close to the sun would reach the earth from such a direction that the star would seem to be slightly shifted away from the sun; or, in other words, that stars close to the sun would look as if they had moved a little away from the sun, and from one another. This is a thing which cannot normally be observed since such stars are rendered invisible in daytime by the

\begin{footnotesize}
1. Conjectures and Refutations p. 34.  
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sun's overwhelming brightness; but during an eclipse it is possible to take photographs of them. If the same constellation is photographed at night one can measure the distances on the two photographs, and check the predicted effect.

Now the impressive thing about this case is the risk involved in a prediction of this kind. If observation shows that the predicted effect is definitely absent, then the theory is simply refuted. The theory is incompatible with certain possible results of observation — in fact with results which everybody before Einstein would have expected. This is quite different from the situation I have previously described, when it turned out that the theories in question were compatible with the most divergent human behaviour, so that it was practically impossible to describe any human behaviour that might not be claimed to be a verification of these theories.

Taking this situation as his paradigm, then, Popper proposed that a theory should be regarded as empirical only if it runs the risk of being over-thrown by observations on the actual course of events in nature—'not the verifiability but the falsifiability of a system is to be taken as a criterion of demarcation.'

Popper's approach to the problem of demarcation — his concentration upon the critical, conjecture testing, character of science rather than on its supposed inductive method — has I believe been vindicated by its fruitfulness both for the study of scientific history and in the philosophy of science. But although I think his general approach is sound, I want to argue that the

definition of falsifiability which Popper proposed in the Logic
of Scientific Discovery is too stringent to provide an adequate
criterion of demarcation. Popper formulated his criterion thus:

A theory is to be called "empirical" or "falsifiable" if it divides the class of all possible basic statements
unambiguously into the following two non-empty subclasses.
First the class of all those basic statements with which
it is inconsistent (or which it rules out, or prohibits): we call this the class of the potential falsifiers of the
theory: and secondly the class of those basic statements
which it does not contradict (or which it "permits").

More briefly, a statement or theory is said to be empirical if, and
to the extent that, it is incompatible with certain possible basic
statements.

In order to evaluate this proposal it will be necessary to
look briefly at what Popper says about basic or observation-
statements.

Basic statements, according to Popper, are singular existential
statements, asserting the occurrence, in a certain individual
region of space and time, of an objective observable, physical state
of affairs. Examples are 'There is a raven in the space time region
k', and 'There is a pointer at rest at the place k'. Such state-
ments furnish the observational basis for empirical knowledge.
Being objective, basic statements cannot be justified, or con-

1. L.S.D. p.86.
Exclusively verified, by subjective perceptual experience: but then no statement which could be justified in this way would be relevant to physical science. Further, basic statements are not 'basic' in the sense of being known for certain or incorrigible when asserted in appropriate circumstances. Nor finally are they basic in the sense of being non-interpretative descriptions of brute fact. Our observation reports are often framed in the light and language of theories, they are always interpretative, open to dispute and capable of being tested by further observation. The acceptance of a basic statement must therefore, according to Popper, involve an element of 'free decision'; though, fortunately for the progress of science, it is possible to achieve an almost complete consensus among those skilled in the appropriate decision procedures - for example, among those trained in the use of telescopes.

Popper's remarks about basic statements treat the notion of an observable occurrence as primitive. In this respect his account is incomplete, and stands in need of a theory of perceptual knowledge. What Popper has done is to describe the features which observation-statements must have if they are to serve as basic statements in scientific discourse - i.e. if they are to be used

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1. Popper's account of basic statements is developed in L.S.D., Ch. V, esp., §28. I shall throughout this discussion take the terms 'basic statement' and 'observation-statement' as synonyms.
in determining the empirical status of theories, and for the 
corroboration of hypotheses. In order to do more than this, in 
Popper's opinion, we should have to move from philosophy into the 
domain of psychology. I think that Popper was wrong here, that the 
theory of perception does fall within the province of philosophy; 
but I think his decision to ignore the problems involved in the 
theory of perception was sound. For Popper's purposes, and also 
for mine, it is only necessary to ask what features observation-
statements must have to be adequate for the purposes of science. 
And seen from this point of view, I think, Popper's position is 
in most respects sound.

One formal condition which Popper imposed upon basic state-
ments seems over stringent, namely the requirement that 'a basic 
statement must have a logical form such that its negation cannot 
be a basic statement in its turn.' This requirement is certainly 
counter-intuitive; yet it is entailed by the conjunction of two 
other conditions which seem intuitively plausible, and which 
Popper took to be incontrovertible. These are

(a) 'From a universal statement without initial conditions 
no basic statement can be deduced', and

(b) 'A universal statement and a basic statement can contradict each other'.

These two conditions, if taken along with

(c) The negation of a basic statement is itself a basic statement,

form an inconsistent triad. Thus, for any basic statement \( O \), condition (b) will be satisfied only if there is a universal statement from which not-\( O \) is immediately deducible, and hence either not-\( O \) is not a basic statement or there are some basic statements which are deducible from laws alone.¹

Condition (b) does indeed seem incontrovertible. The question then is whether we should reject (a) or (c), or perhaps modify both. This question admits a straightforward test. For if (a) is to be rejected then any putative observation-statement derivable from a universal statement – i.e. the negation of any observation-statement which is incompatible with that universal statement – must be such that it in turn is incompatible with some other universal statement. And conversely, if (c) is to be rejected then the negation of an observation-statement must be such that it is not incompatible with any universal statement. Thus in denying that the negation of an observation statement is itself an observation-statement Popper is compelled to assert that there is a formal difference which prevents the negation of an observation-statement from functioning as a potential falsifier.

According to Popper conditions (a) and (b) but not (c) are satisfied by singular existential statements like 'There is a raven in the space time region k'. Such statements, according to Popper, are not immediately deducible from universal statements, yet they are incompatible with some universal statements. Moreover their negations, singular negative existential statements, are (the only singular statements) immediately deducible from universal statements, and they have a form such that they cannot in turn function as potential falsifiers for universal statements.\(^1\) Accordingly Popper stipulates that 'basic statements have the form of singular existential statements.'\(^2\)

Now it is clear that Popper held that the formal difference which precludes a singular negative existential statement from functioning as a potential falsifier is a difference over and above the presence of the negation sign — a difference comparable with the formal difference between a universal statement and its existential negation — and it seems that what Popper had in mind is that singular negative existentials take a conditional or disjunctive form which precludes their being used as test statements. Thus (in a gloss added in the English translation) he wrote:

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'It will be seen at once that the singular statements which can be deduced from purely universal statements cannot be basic statements. I have in mind statements of the form: "if there is a swan at the place k, then there is a white swan at the place k". (Or, "At k, there is either no swan or a white swan.") We see now at once why these 'instantial statements' (as they may be called) are not basic statements. The reason is that these instantial statements cannot play the role of test statements (or of potential falsifiers) which is precisely the role which basic statements are supposed to play. If we were to accept instantial statements as test statements, we should obtain for any theory (and thus for "All swans are white" and for "All swans are black") an overwhelming number of verifications - indeed, an infinite number, once we accept as a fact that the overwhelming part of the world is empty of swans.'

1. L.S.D. p. 101*1, cf. C.R. Addenda p. 386-7. It would seem, from the passage quoted, that Popper has shifted to the stronger demand that an instantial statement should count as basic only if it could constitute a possible test statement for the very statement from which it was deduced. Presumably what is required is that a test of the instantial statement should provide a test of the parent universal, i.e. that the negation of the instantial should falsify the universal statement. But if this is what Popper requires then notice first, that Popper's new requirement apparently presupposes that the negation of an observation-statement must itself be an observation-statement; and secondly, that the new requirement is in fact satisfied by instantial statements, since their negations are singular existential statements of the kind already admitted as potential falsifiers. The allusion to the Ravens Paradox is, in my view, strictly a red herring.
But this will not do. Not every singular existential statement has a disjunctive negation - 'There is a raven at k', for example, has the simple negation 'There is no raven at k' - nor do all singular statements deducible from universals take conditional form - 'There is no raven at k' is immediately deducible from 'There are no ravens.' So if singular negative existential statements can never function as potential falsifiers, the presence of the negation sign alone must be sufficient to debar them.¹ Now I take it that the negation sign is not sufficient to prevent a singular negative existential statement from being observational; for if we can decide by observation that there is a raven at k we must also be able to decide by observation that there is not. Were this not so there would be no significant difference between 'pure' and singular existential statements.² So if a singular negative existential cannot function as a potential falsifier, this can only be because there is no universal statement with which it is incompatible.

¹. In the Addenda to C.R. (p. 386) Popper concedes that 'in some simple cases of basic statements their negations may be acceptable as basic statements'. Perhaps cases like that above would count as simple basic statements. Popper however does not remark upon the consequence of his admission that if basic statements sometimes have negations which are basic, then basic statements are sometimes immediately deducible from universal statements. Nor does he say whether in those cases where a basic statement has a basic negation the negative basic statement is incompatible with some universal statements. Instead he proceeds as though his minor admission leaves undisturbed the position defended in L.S.D.

². I shall explicate the distinction shortly.
But 'There is no raven at k' is incompatible with the universal statement 'There is no space time region which is free of ravens'.

Admittedly universal statements like this are somewhat odd, for since they make an assertion about every space time region they will in general be overwhelmingly falsified. One might reasonably claim, therefore, that if singular negative existential statements can only be incompatible with universal statements of this sort, then their value as potential falsifiers is negligible. Since no one would ever assert the statements with which they are incompatible, one might as well exclude them by stipulation from the class of basic statements. However, their use as potential falsifiers is not confined within these limits, for a singular negative existential statement may pair with a basic statement to form a potential falsifier for a more interesting universal statement. (Or, what comes to the same thing, a singular negative existential statement may be a potential falsifier for the conjunction of a universal statement with an instatntial statement.) Thus 'There is no black raven at k' in conjunction with 'There is a black bird at k' is a potential falsifier for the universal statement 'All black birds are ravens', (and it alone is a potential falsifier for the conjunction of the latter two statements.) Were we to adopt the above stipulation we should have to say in cases like this that the conjunction of two statements may be basic even
although one conjunct is not a basic statement. And we should then have to specify the conditions under which the conjunction of two statements may be basic when one conjunct is not.

I conclude that there are no grounds for the assertion that the negation of an observation-statement cannot itself be an observation statement. There are, moreover, cogent logical reasons for maintaining that the negation of an observation-statement - and any other contingent truth functional compound of observation-statements - must in turn be an observation-statement. An impressive, and to my mind conclusive, battery of arguments for this point has been marshalled by David Stove in a paper which unfortunately has not yet been published. I shall not re-state those arguments here, but shall be content to point out one theoretical consequence of retaining (c).

If the negation of an observation statement is always itself an observation-statement, then there is a close formal analogy between Popper's falsifiability criterion of demarcation and Ayer's verifiability criterion. Indeed Popper's definition of falsifiability is now seen to be formally equivalent to Ayer's definition of

2. "Are Observation-statements Deducible from Laws alone?"
of direct verifiability. For under Ayer's definition, a universal statement is directly verifiable only if it is such that in conjunction with an instantiai observation-statement it entails a further observation-statement; and if (c) is retained, then necessarily the conjunction of the instantiai observation-statement with the negation of the derived observation-statement will itself be an observation-statement, and one immediately incompatible with the directly verifiable universal statement. Thus, as J.W.N. Watkins has stressed, 'Popper's requirement that a universal hypothesis h is empirical if and only if it is contradicted by some basic statement is equivalent to the requirement that h is empirical if and only if, in conjunction with subsidiary premises consisting of nothing but basic statements it entails further basic statement.'

Ayer, however, does not restrict the class of empirical statements to the class of directly verifiable or falsifiable statements.

1. op.cit. p. 295; Watkins does not draw attention to the presupposition of his argument that the negation of an observation-statement is itself an observation-statement. It should be remarked that Popper himself argues for the equivalence of falsifiability and direct verifiability; and attempts to reconcile this with his denial of (c) by asserting that the conjunction of an observation-statement with a non-observation-statement may sometimes be equivalent to an observation-statement. (L.S.D. p. 85#1 & p. 102). However it seems that Popper can only defend the formal equivalence of falsifiability and direct verifiability if he is prepared to argue that the conjunction of the negation of an observation-statement with any other observation-statement on the same space time region must necessarily be equivalent to an observation-statement; and he must assert this while denying that if the conjunction of two statements is an observation-statement then each conjunct is an observation-statement. Popper does not attempt to defend this position, and I do not see how it could be defended.
but admits as empirical also statements which are indirectly verifiable or falsifiable. That is, Ayer admits as empirical any statement which is such that in conjunction with other empirical statements it entails further observation statements. We should focus our attention, therefore, on the questions (a) whether Popper's strict falsifiability criterion is too stringent to provide an adequate demarcation between science and metaphysics; and, if it is, then (b) whether an extended falsifiability criterion can successfully be constructed along the lines pursued by Ayer.

Popper's criterion of demarcation puts two important classes of statement on the non-empirical side of the boundary. The first is the class of pure existential statements, i.e. those statements which assert the existence of something in a wholly unspecified region of space and time. Such statements are unfalsifiable - for 'we cannot search the whole world in order to establish that something does not exist, has never existed, and will never exist' - they are not incompatible with any possible result of observation, and so are non-empirical under Popper's criterion, even though they may happen to be conclusively verified by observation. The

1. I shall discuss Ayer's criterion in section 3 of this chapter.
2. *L.S.D.* p. 70
second class of statements which Popper's criterion renders non-empirical is the class of pure all-and-some statements i.e. those statements which have both unrestricted universal and existential quantification, for example 'Every substance has a solvent'. Such statements are unverifiable, because they make an unrestricted universal assertion; and they are unfalsifiable, because they include a pure existential component. Accordingly, under Popper's criterion, they must be counted as metaphysical, even although they are instantially confirmable. Hereafter I shall use the expression 'pure statement' as a generic term with pure existentials and pure all-and-some statements as its species.

Now both pure existential statements and pure all-and-some statements undeniably have a place in scientific discourse, so it may seem that Popper's proposal conflicts with the needs of science. For one thing, a pure statement may be entailed by a scientific theory; as, for example, the kinetic theory entails the existence of molecules. But this point would not worry Popper, for he might well say that an empirical theory may have non-empirical consequences: a fact which illuminates the connection between science and our changing world view. A more serious point is that a scientific theory may include as a postulate a pure existential or pure existential statements.

1. For example in the sense of 'confirmation' defined by Hempel.
all-and-some statement. Newton's second law of motion, for example, is a pure all-and-some statement, as also is the neutrino hypothesis in micro-physics. If Popper's criterion excludes such statements from the empirical class then it must run foul of our second adequacy condition.

One might try to meet this objection by arguing that since a pure statement cannot figure in a scientific explanation except in the context of a theoretical system, the demands of our second adequacy condition are satisfied if only the system as a whole is empirical. A system of statements may be admitted as empirical under Popper's criterion though its constituents, considered in isolation, are not. J.W.N. Watkins adopts this position and attributes it to Popper. Here, however, we must be careful not to obscure the important distinction between those cases where a statement, which in isolation is non-empirical, plays an essential part in an empirical system, and those cases on the other hand where a metaphysical statements gets caught up in a scientific theory as an idle passenger. We want, for example, to be able to distinguish the way in which the second law of motion figures in Newton's Theory from the way in which the doctrine of absolute space figures in it - without in either case saying that the inclusion of

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2. op.cit. p. 300.
the non-empirical element has deprived the system of empirical status. This distinction turns, according to Popper, on whether the inclusion of a certain statement in a theoretical system increases the empirical content of the system, i.e., renders it more falsifiable, or leaves its content unchanged, and perhaps even reduced. ¹

The second law clearly belongs to the former class, while the doctrine of absolute space, in Popper's view, belongs to the latter, and may be deleted without loss of content. One important function of deductive systematization in scientific theory is to permit the discovery and deletion of otiose metaphysical elements.²

Where a pure statement contributes to the empirical content of a system, Popper does not count it as metaphysical, but appears to ascribe conditional empirical status to it. He writes

'An isolated existential statement is never falsifiable; but if taken in a context with other statements, an existential statement may in some cases add to the empirical content of the whole context: it may enrich the theory to which it belongs, and may add to its degree of falsifiability or testability. In this case, the theoretical system including the existential statement in question is to be described as scientific rather than metaphysical.'³

We might speak here of contextually empirical statements.

The recognition of contextually empirical statements considerably mitigates the severity of Popper's criterion. Pure statements are not after all to be considered sheer metaphysics, but have a status analogous to that of theoretical statements: it is only if they are isolated from an empirical context that they have to be regarded as metaphysical. The separation between Popper's criterion and Ayer's also becomes less sharp once contextually empirical statements are recognized. The main difference now appears to be that whereas Ayer allows unequivocal empirical status to statements which are not directly verifiable, Popper will only allow them to be conditionally or contextually empirical.

If we examine more carefully the class of contextually empirical statements we may distinguish a number of importantly differing sorts of case.

First, it may happen that a particular statement, which in isolation is non-empirical, becomes specifically testable when conjoined with other empirical statements. Consider, for example, the statement 'There is a planet outside the orbit of Uranus', which can be treated as a pure existential statement, since it is practically unfalsifiable. One might happen to discover Neptune while star-gazing, but failure to discover it, however careful the search, would not falsify the statement that there is such a planet.
However if this statement is taken in the context of Newtonian theory it may become testable; for we can measure the perturbation in the orbit of Uranus, and so predict with some precision the location of the farther planet. A similar example is given by Popper;¹ and cases like this are very common. Other examples are statements about the chemical composition of the fixed stars, or about the rate of expansion of the universe, which are testable only with the aid of other scientific statements, here the laws of spectroscopy. Indeed I think it is true that most scientific hypotheses, even those which would not normally be regarded as theoretical, are falsifiable only against a background of assumed empirical laws or theories.

Second, there are cases where a general statement, though not falsifiable in its own right, may add to the empirical content of a theory already established as empirical and confirmed by observation. This situation may arise where additional postulates are added to an established theory which increase its explanatory power, or render it applicable to a new domain of phenomena.² Examples abound in the history of quantum theory. Or it may happen where the deductive systematization of a theory allows us to distinguish those theorems of the theory which are deducible with

¹. L.S.D. p. 69.  ². L.S.D. pp. 76-7 & C.R. p. 239.
the aid of a certain postulate from those which are independent of that postulate. Thus it may become possible, as Popper himself insists against Duhem, to get an intra-theoretical falsification, i.e., to isolate the source of a negative test result within a theory system, even though the statement so falsified is not directly or immediately incompatible with any observation-statement. Indeed Popper maintains that the major function of deductive systematization in science is to increase the penetration of experimental tests, so that the falsification of a certain prediction need not crudely be taken as refuting the whole theory, but may be focused upon a specific segment of the theory.\(^1\)

Third, there are cases where the inclusion of a certain statement in a theoretical system determines the empirical content of the system, but not in such a way as to render that statement specifically falsifiable. Thus it might be the case that the statement in question is involved in every deduction made within the system, so that its deletion would entirely deprive the theory of empirical content. The second law of motion, I gather, stands in this relation to Newtonian theory. In such a case, a falsifying observation would overthrow the system as a whole, though it could not be said 'of any one statement of the system that it is, or is

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1. L.S.D., p. 72.
not, specifically upset by the falsification. Yet even here, if it is conjectured that a specific component in a theory is responsible for its experimental refutation, that conjecture may be tested through the construction of alternative theories which differ from the original only in respect of the one postulate.

No doubt other discriminations are possible, and those that I have made need more careful exposition. But I think these remarks suffice to show that statements within a theoretical system may often be individually testable, though in the strict sense of 'falsifiability' defined by Popper only the system as a whole is 'falsifiable'. Moreover the possibility of intra-theoretical falsification is of great importance for the elucidation of the empirical character of science. Popper himself insists upon it - in my view correctly - as a pivotal point in the defence of piecemeal empiricism against the holism of Duhem and Quine. According to the Duhem-Quine thesis, scientific statements are not individually testable, but face the tribunal of sense experience as members of a corporate body. A negative test result may show that something is wrong within the system, but it cannot determine where the revision should be made. That question always remains a matter for decision. Any particular

statement may be preserved come what may, provided drastic enough adjustments are made elsewhere in the system. Against this view Popper argues that we are often able to locate the source of a negative test result within a system of statements, though logically every statement in the system is involved in the refutation. For our background knowledge, or the results of further tests, may focus the refutation upon some individual statement within the system. In such cases the specific refutation of a theory is not logically water-tight, or beyond subsequent revision. But this is a characteristic of all science; even the claim that the total system has been falsified is revisable, for one may come to question the observation-statements that were taken as falsifiers for it. The absence of absolute certainty does not warrant a shift toward conventionalism.

The argument against holism is I think sound. But one thing which emerges from it is the inadequacy of Popper's definition of 'falsifiability'. Were he to stick to his definition Popper would be forced to support a holist position. In arguing against holism he has, so it seems, tacitly adopted a wider notion of

1. W.V.O. Quine 'From a Logical Point of View' p. 43.
falsifiability, one which admits the indirect falsification of theories. The class of potential falsifiers for a theoretical statement is now allowed to include observation-statements which are incompatible with statements derivable from the conjunction of that statement with other empirical statements, provided that the potential falsifiers so obtained are not also potential falsifiers for the auxiliary statements alone. At this point Popper's criterion has become formally indistinguishable from Ayer's.

Ayer's criterion, however, has long been paralysed by devastating formal criticisms. Indeed Watkins has come to assert that 'any falsifiability criterion which allows empirical statements other than basic statements to figure among the subsidiary premises employed when a statement is being examined for empirical status will allow any statement whatever to be "empirical".' We must now examine these arguments.

Ayer formulated his criterion in the Introduction to the second edition of Language Truth and Logic as follows:

"(I) I propose to say that a statement is directly verifiable if (i) it is either itself an observation-statement, or (ii) is such that in conjunction with one or more observation-statements it entails at least one observation-statement which is not deducible from these other premises alone; (II) and I propose to say that a statement is indirectly verifiable if it satisfies the following conditions: (i) first that in conjunction with certain other premises it entails one or more directly verifiable statements which are not deducible from these other premises alone; and (ii) secondly that these other premises do not include any statement that is not either analytic, or directly verifiable, or capable of being independently established as indirectly verifiable."

For brevity sake I shall say that a statement which satisfies the conditions for direct verifiability entails an observation-statement; and that a statement which satisfies the conditions for indirect verifiability gives rise to a directly verifiable

1. Language Truth and Logic p. 13; both the numbering and the italics are mine. Ayer's original formulation (p. 39) read a statement is empirical if and only if 'some experiential propositions can be deduced from it in conjunction with certain other premises without being deducible from those other premises alone.'

2. Notice that if it is allowed that the negation of an observation-statement, and the conjunction or disjunction of two or more observation-statements, are themselves observation-statements, then every directly verifiable statement will immediately entail an observation-statement.
statement. Ayer's criterion is then that a statement is empirical if and only if it either entails an observation-statement or gives rise to a statement which entails an observation-statement. Ayer, of course, went on to propose than any statement should count as literally meaningful if and only if it is empirical. But I consider this proposal to be mistaken.

The idea underlying Ayer's definition is that verifiability is - except in the case of observation-statements - a deductive relation running from the verificandum to the verificans. It is important to notice that, on this account, to say that a statement which is not an observation-statement is 'verifiable' is to say that it is falsifiable or testable by observation-statements. For the formal relation holding between the verificans and the verificandum does not, on Ayer's account, admit the transmission of truth from the verificans to the verificandum, but only the transmission of falsity. Perhaps it is misleading to speak of 'verifiability' at all here; 'testability' would be a preferable term. However I think that having pointed this out it will be safe to use the term 'verifiable' wherever Ayer does.

Now it has been argued - for example by Church, Scheffler

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1. This view has recently been defended by J.W.N. Watkins (B.J.P.S. p. 300); but I think it was first advanced by J.A. Passmore, in his 'Logical Positivism', A.J.P. 1943 pp. 87-8.
and Watkins\(^1\) - that Ayer's definition of verifiability is too liberal, indeed that it admits any statement whatever to the class of empirical statements, and is thus inadequate for the demarcation between science and metaphysics. Church himself urged the modest opinion 'that any satisfactory solution of the difficulty will demand systematic use of the logistic method';\(^2\) but recent writers have drawn a more radical conclusion. Most have abandoned Ayer's approach, assuming that arguments like that of Church must generate a series of ad hoc amendments which would render the criterion both inelegant and over-stringent.\(^3\) I shall argue that this conclusion is mistaken.\(^4\)

(a) We should first examine an argument originally deployed by Hempel, and since used by Scheffler and others.\(^5\) Watkins has

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2. op.cit., p. 53.
3. Scheffler and Watkins reach this conclusion, as also do M. Hesse, Forces and Fields p. 12, and A. Pap, An Introduction to the Philosophy of Science, p. 28.
named it the **tacking paradox**.\(^1\) Take any verifiable statement \(T\), for example 'All cats are carnivorous', and conjoin it with any statement whatever \(S\), then the resulting compound statement, \(T \land S\), will be verifiable since it entails whatever is entailed by the verifiable statement \(T\). In this way it may be shown that under Ayer's criterion any statement whatever may be an element in a verifiable molecular statement. Prima facie this seems undesirable.

This consequence might be prevented by laying down restrictive conditions for molecular statements; for example by requiring that a molecular statement should count as empirical only if its every element made some difference to the observational consequences of the compound. Such a condition is suggested by Brown and Watling. However the tacking paradox alone does not establish the necessity for it. Initially there is no more reason for concluding that a molecular statement is metaphysical because it includes one metaphysical element than there is for concluding that it is empirical because it includes one empirical element. We are free to choose whatever alternative our criterion of verifiability commits us to. But, of course, if we do assign empirical status to any compound statement having observational consequences, we shall not be able to conclude that every element in the compound is empirical.

\(^1\) Watkins, *op.cit.* pp. 304-5.
From the fact, for example, that Newtonian mechanics is a very powerful empirical system it does not follow that it contains only empirical statements - indeed it has often been argued that Newton's system includes otiose metaphysical elements. It seems then that the tacking paradox is not a serious objection to Ayer's approach to the problem of demarcation.\footnote{Watkins makes this point, and so does Popper, \textit{L.S.D.} p. 85.}

But it is important to notice that the tacking paradox is not harmless if Ayer's criterion is taken as a test of significance: and this is how it was taken both by Ayer and his early critics. Under Ayer's criterion the conjunction of a verifiable statement with a non-empirical statement must be counted as meaningful; yet most empiricists would want to maintain that a molecular statement cannot be meaningful unless all its constituents are meaningful. The reason for maintaining this principle is that logical connectives like conjunction and disjunction are, by definition, applicable only where the elements they connect are meaningful, true-or-false statements. The tacking paradox is, then, only one aspect of the general logical incoherence which undermines Ayer's criterion of significance.
Finally it should be remarked that even when Ayer's criterion is taken merely as a principle of demarcation for empirical statements, the tacking paradox still has a sting in it. For it provides the logical foundation for the embarrassing arguments of Church and Scheffler, to which I now turn.

(b) Let us now examine an argument advanced by Israel Scheffler. Consider the statement

(1) \((S \cdot O_1) \vee O_2\)

where \(O_1\) and \(O_2\) are observation-statements and \(S\) is any statement whatever. Now (1) is directly verifiable since when conjoined with \(O_2\) it yields \(O_1\); and (1) is entailed by the conjunction of \(S\) and \(O_1\), so that \(S\) is indirectly verifiable under Ayer's criterion.

It may seem that Scheffler's argument violates Ayer's condition (1*ii and 11*1) that the verificans should not be derivable from the auxiliary premises alone. For here \(S\) is established as indirectly verifiable by deriving from it in conjunction with \(O_1\) a statement, (1), which in turn is established as verifiable only by the derivation of the original auxiliary premise, \(O_1\). Yet, to be strict, Ayer's criterion does not prevent this manoeuvre, since it requires only that the directly verifiable consequence of \(S\) and

o₁, namely (1), should not be derivable from O₁ alone: the verificans for (1) is not in turn subject to that same restriction.

The trouble lies in Ayer's extension of the class of possible verifiers for an indirectly verifiable statement to include any directly verifiable statement to which the verificandum gives rise. The advantage gained by this extension, it seems, is one of convenience only. For instance, it allows us to say that a theoretical statement is empirical if it gives rise to an instantial law statement. But no real extension of the class of empirical statements is gained in this way, since an observation-statement will always be derivable from such theoretical statements if the auxiliary premises are strengthened by the addition of instantial premises. The liberalizing factor in Ayer's definition is not the recursive extension of the class of possible verifiers, but rather the extension of the class of possible auxiliary statements. Moreover, if the class of possible verifiers is restricted to the class of observation-statements - as I propose - then cases like Scheffler's, where the verificandum figures only vacuously in the derivation of an observation-statement, will be excluded. The

1. I mean by 'real extension', an extension of the class of empirical statements to include a new class of statements, without thereby including every statement whatever.
amendment needed to block Scheffler's argument thus involves no less of elegance, and is no more stringent than the original criterion was intended to be.

(c) Church's argument is more difficult to handle. Consider the statement

\[(2) \ (\neg o_1 \cdot o_2) \lor (o_3 \cdot \neg S)\]

where \(o_1\), \(o_2\) and \(o_3\) are three mutually independent observation-statements. Now (2) is directly verifiable, since when conjoined with \(o_1\) it entails \(o_3\); and \(S\) and (2) together entail \(o_2\). Therefore (under Ayer's definition) \(S\) is indirectly verifiable—unless it happens that \((\neg o_1 \cdot o_2) \lor (o_3 \cdot \neg S)\) alone entails \(o_2\), in which case \(\neg S\) and \(o_3\) together entail \(o_2\), so that \(\neg S\) is directly verifiable.\(^1\)

Church's argument, of course, can be met by a 'monster-barring' amendment to Ayer's definition. The most obvious move is to strengthen the independent verifiability condition, (II ii), to require that a compound statement should count as verifiable only if each component is either directly, or independently indirectly verifiable.\(^2\) Indeed it is arguable that no amendment is needed: for if the term 'include' in Ayer's formulation of the independent verifiability condition is read as 'include intra se',

(rather than as 'include inter se'), then \((-O_1 \cdot O_2) \land (O_3 \cdot S)\) will
not be an admissable auxiliary premise. However, whether it is
an amendment or merely an articulation of Ayer's definition which
thus blocks Church's argument, the important point to notice is
that to defend Ayer in this way is to increase the stringency of
his independent verifiability condition. And that condition seems
already too stringent. For the independent verifiability require-
ment for auxiliary statements would lead us to class as non-empirical
a large class of statements, whose place in scientific explanations
is undisputed: namely those theoretical statements which, like
Newton's second law of motion, are testable only in the context of
a theoretical system which is such that none of its constituents
are testable independently of the statement in question. Yet if
this condition were abandoned then not only would Church's objection
be relevant, but also the more simple argument, that any statement
S may be shown to be indirectly verifiable by conjoining it with
S \rightarrow O to yield the observation-statement O, would also become effective.
It was this argument, originally proposed by Isiah Berlin, that
led to the introduction of the independent verifiability requirement

1. cf. P. Nidditch, Mind 1961
for auxiliary statements. The dilemma, then, is that Ayer's independent verifiability condition saves his definition from excessive liberality only by making it too stringent.

A possible way out of the difficulty would be to maintain that an individual theoretical statement cannot be assigned empirical status, though the theoretical system to which it belongs may, if taken as a compound theoretical statement, be assigned empirical status in accordance with Ayer's definition. This solution is a plausible one since in any case it is usually somewhat artificial to break a theory up into constituent statements. But notice that this solution could be adopted only if the independent verifiability condition were not strengthened to prevent a compound statement from being counted as empirical where it contains components which are not independently verifiable. Accordingly a new way of blocking Church's argument would have to be found. A possible alternative has been suggested by Brown and Watling.¹ They recommend the addition of the following clause to Ayer's requirements both for direct and indirect verifiability: "A molecular statement is verifiable if and only if it contains only components whose deletion leaves a statement which entails verifiable statements not

¹. Analysis 1951. A similar line is taken by Routley (op.cit.), but his proposal is too complex to summarize here.
entailed by the original statement, or does not entail verifiable
statements entailed by the original statement.'

I have no radical objection to this suggestion, though
I think that the problems of reductive analysis which it generates
would prove to be an encumbrance. In any case my own solution
to the problem cuts more deeply. I shall abandon the independent
verifiability condition as a general requirement for auxiliary
premises, and I shall seek a single way of meeting both Church's
argument and the earlier argument proposed by Berlin, without
introducing any real increase in the stringency of the criteria.
One consequence of my procedure is that the general conditions for
verifiability developed accordingly will not exclude individual
theoretical statements from the empirical class.

(d) Let us begin by considering a paradigm example. The
statement 'All metals expand when heated' may be shown to be
verifiable (under Ayer's criteria) by conjoining it with the
instantial premise 'This bar of iron was heated at t' and deriving
the observational consequence 'This bar of iron expanded at t'.
More formally, we can say that a statement of the form

(O) (x)(Fx \rightarrow Gx)

is verifiable if we can find a conditional statement of the form

(O+1) [(x)(Fx \rightarrow Gx) \cdot Fa] \rightarrow Ga

where Ga stands for an observation-statement. I shall refer to
such a conditional as a verification schema; and I shall refer to its contraposition as a falsification schema. Thus

\[(0\cdot2) \quad \vdash \neg Ga \supset (\neg Fa \lor (\exists x)(Fx \land \neg Gx))]\]

is a falsification schema for \((0)\). \((0\cdot2)\) provides a logical framework for the testing of \((0)\), since \((0)\) will be false if \((\neg Ga \land Fa)\) is true.

I shall seek a solution to the problems raised by the arguments of Berlin and Church by investigating the necessary logical properties of any adequate verification schema. Two adequacy conditions can immediately be specified. Firstly, a verification schema is adequate only if it is a tautologous conditional; and secondly, a verification schema is adequate only if the corresponding falsification schema is adequate. Both conditions necessarily follow from the requirement that the verificandum in conjunction with the auxiliary premises should entail the verificans. My aim is to eliminate verification schemata like those corresponding to the arguments of Berlin and Church by specifying a further necessary condition for the adequacy of falsification schemata.

The condition I propose is this: a falsification schema is adequate only if its consequent is not reducible to an equivalent statement in which the negation of the verificandum occurs only as one element in a disjunction one of whose alternants is the negation of the verificans. The justification for this
condition is simply that any falsification schema which violates it cannot, without contradiction, provide a framework for the testing of the verificandum. Thus, to take the simplest case, if the proposed falsification schema were equivalent to

$$\neg O \supset (\neg S \lor \neg O)$$

then we could conclude \(\neg S\) only if both \(O\) and \(\neg O\) were asserted.

I shall refer to this condition as the 'requirement of non-vacuous falsifiability'. And I shall say that a statement is 'vacuously falsifiable' if the only falsification schemata that can be found for it violate the requirement of non-vacuous falsifiability.

Now both Berlin's argument and Church's violate the requirement of non-vacuous falsifiability. For the verification schema corresponding to Berlin's argument, namely

$$(3\cdot1) [S \supset (S \supset 0)] \supset 0$$

has as its contrapositive the expression

$$(3\cdot2) \neg 0 \supset [\neg S \lor (S \supset 0)]$$

which is equivalent (by distribution) to the statement

$$(3\cdot3) \neg 0 \supset [(\neg S \lor S) \supset (\neg S \lor 0)]$$

which in turn (since \(p \equiv [p \supset (q \lor q)]\) is a tautology) reduces to

$$(3\cdot4) \neg 0 \supset (S \lor \neg 0).$$

Similarly, the verification schema corresponding to Church's argument, namely
\[(2 \cdot 1) \quad [((\sim O_1 \cdot O_2) \vee (O_3 \sim S)) \cdot S] \supset O_2\]

has the contrapositive

\[(2 \cdot 2) \quad \sim O_2 \supset [(O_1 \sim O_2) \cdot (\sim O_3 \sim S) \vee \sim S]\]

which is equivalent to

\[(2 \cdot 3) \quad \sim O_2 \supset [(O_1 \sim O_2 \sim S) \cdot (\sim O_3 \sim S \sim S)\]

and so reduces to

\[(2 \cdot 4) \quad \sim O_2 \supset (O_1 \sim O_2 \sim S).\]

In either case the antecedent occurs in the disjunctive consequent as one alternant - thereby rendering the schema vacuous.\(^2\)

I propose then that we replace Ayer's independent verifiability condition by the requirement of non-vacuous falsifiability. There are two considerations which might be taken as suggesting that the requirement of non-vacuous falsifiability is too restrictive. Firstly, it might be argued that if the verification schema

\[S \cdot (S \supset O) \supset O\]

is to be rejected then we should not be able to prove that a pure existential statement, like 'Mermaids exist', is empirical by conjoining it with a subjunctive conditional like 1.

In effect what I have done in these cases is to reduce the consequent to normal conjunctive form.

2. A similar result may be obtained for other arguments that might be used to show that any statement whatever is verifiable on Ayer's account. Thus the verification schema

\[[(O_1 \sim O_2 \sim S) \cdot S] \supset [O_1 \sim O_2]\]

can be shown to violate the requirement of non-vacuous falsifiability, as also may the argument proposed by Watkins in 'When are Statements Empirical'.

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'If there were any mermaids they would inhabit the Mediterranean'. Here however, it may be disputed whether subjunctive conditionals are true-or-false statements. The most plausible account of the matter, I believe, is that which analyses subjunctive conditionals as condensed arguments. On such an account the use of a subjunctive conditional as an auxiliary premise in a verification schema would always be elliptical; and if the premises were fully specified - if, for example, the assumed law statements were made explicit - then the schema would not be eliminated by the requirement of non-vacuous falsifiability. In any case, it is I think certain that a subjunctive conditional cannot be analysed as a simple statement of material implication, and if it cannot the above objection will dissolve. Secondly, if every verification schema of the form 
\[ S \cdot (S \supset O) \supset O \] were to be rejected then, it may be argued, we should not be able to establish the verifiability of a theoretical statement T by taking it in conjunction with a correspondence rule T ⊢ O. However my stipulation does not prevent the use, as auxiliary premises, of correspondence rules which correlate a theoretical statement with an instantial law statement (e.g., T ⊢ L, where L is an instantial law), or which correlate a theoretical term with an

1. cf. e.g., J.L. Mackie 'Counterfactuals & Causal Laws', in R.J. Butler, Analytical Philosophy, 1st series.
observation term (e.g. \((x)(\theta \land \phi \land x)\), where \(\theta\) is a theoretical term and \(\phi\) a term in an observation language): and these I think are the only cases that need worry us. It seems then that the requirement of non-vacuous falsifiability does not produce any real increase in the stringency of the conditions for empirical statements.

I have been trying to specify formal conditions whose adoption would insulate the testability criterion of demarcation against formal proofs of inadequacy such as that proposed by Church. Two requirements have been proposed: the first is that the class of potential verifiers or falsifiers for a given statement or theory should be confined to the class of basic or observation-statements; the second is that any argument which is used to establish the empirical status of a given statement or theory should also permit its non-vacuous falsification. These conditions are formal in that their adoption does not involve any real increase in the stringency of the testability criterion: they block a formal loophole without preventing the admission of any statement that would otherwise have got through. It may, however, readily be shown that these formal conditions are not sufficient to ensure the adequacy of the testability criterion.
Let M be any indisputably non-empirical predicate, say 'spiritual being', and let P be any predicate belonging to the observation language, say 'language user', provided that M and P can without absurdity be ascribed to the same individual; and let i designate an identifiable individual. Then, I presume, the statement

$$(5) \quad \forall x (Mx \rightarrow Px)$$

will be indisputably non-empirical. Yet one might argue for its empirical status by conjoining (5) with the non-verifiable instantaous statement Mi to deduce the observation-statement Pi. Further the negation of Pi will entail

$$(5\cdot1) \quad \neg Pi \lor \exists x (Mx \rightarrow \neg Px)$$

in which the contradictory of the verificandum occurs non-vacuously. The statement (5) would, therefore, have to count as empirical if the requirements so far specified were taken as sufficient. Evidently our criterion is still too lax. Moreover it is clear that the difficulty cannot be overcome by the introduction of a further condition designed to prohibit verification schemata of the form

$$[(x)(fx \equiv gx) \cdot fa] \supset ga$$

for we shall certainly want to admit that there are some valid verification schemata of this form, most notably in cases where the
verificandum is directly verifiable. It seems, then, that the further condition which we need will have to impose a restriction either upon the verificandum or upon the auxiliary premises.

At this point it is tempting to follow Ayer in introducing an independent verifiability condition for auxiliary premises. We might, that is to say, require that the auxiliary premises employed in determining the empirical status of any non-directly verifiable statement should include only statements which are either observational, or directly verifiable, or independently established as indirectly verifiable. Moreover this condition seems desirable since were it not satisfied it would not be possible to specifically test the truth of the statement whose empirical status is in question. Unfortunately however, as I have already remarked, scientific statements may sometimes not be individually testable, but may nevertheless be essential components in a system which, as a whole, is testable. In order to meet this difficulty one might further liberalize Ayer's definition by allowing that a statement which is not directly testable may count as empirical if either it is indirectly verifiable - i.e. if it is such that in conjunction with auxiliary statements which satisfy the independent verifiability condition, it non-vacuously entails an observation-
statement — or if it is contextually verifiable\(^1\) — i.e., if it is an essential component of a theoretical system which as a whole is directly or indirectly verifiable.

It is difficult to evaluate this suggestion owing to the vagueness of the notion of a theoretical system. The worry I have is that manifestly metaphysical statements might find admittance to the class of empirical statements as elements in an artificially contrived theoretical system.\(^2\) One might hope to exclude this possibility by formulating logical requirements for any system that is to count as a genuinely explanatory theory. But it seems to me that if only formal requirements are stipulated then the notion of contextual verifiability will still be too liberal. For if we are free to postulate as explananda for a theory any logically possible set of phenomena — if, that is, we can postulate events and regularities to be explained which radically differ from those that in fact obtain — then it is doubtful whether there is any statement which might not have a place in a possible genuinely explanatory theory.\(^3\) What makes, for example, theological statements

\(^{1}\) This is of course a narrower notion than that of 'contextually empirical statements' introduced in the previous section.


\(^{3}\) This point was suggested to me by David Bostok of the A.N.U.
non-empirical is not, it seems to me, the logical character of the statements themselves, but their assertion against the background of existing scientific knowledge. Such statements have not been fitted into a potentially satisfactory theory - and it seems unlikely that they ever will be, the world being what it is - but had the train of events been radically different, the postulate of a divine creator and controller of affairs might have had potential explanatory value.

Similar considerations apply in the case of indirect verifiability. The reasons for regarding a statement as indirectly rather than directly verifiable will, I think, normally be empirical. We consider statements about the chemical composition or temperature of the fixed stars to be only indirectly verifiable because our background astronomical knowledge shows it to be empirically impossible to perform the normal direct tests. But if this is so, then we should make it a condition of indirect verifiability that indirect tests should be empirically possible. That is, we should require that the auxiliary premises employed in any argument for the indirect verifiability of a statement should not merely be testable but should in fact belong to the store of scientific knowledge. Otherwise there would only be a bare logical possibility of testing the verificandum.
These considerations tend to show that our assessment of the empirical character of a statement should, at least in many cases, be in part determined by the background of existing empirical knowledge. And the proposal which I now offer takes up this point.

Let us say that a statement $S$ is an integral component of a theory $T$ if the deletion of $S$ from the theory would result in a loss both in the explanatory power and empirical content of $T$. And let us say that $T$ is a potentially satisfactory theory for a certain class of phenomena if $T$ provides a unified and relatively simple explanatory framework for those phenomena and further admits the deduction of new directly or indirectly testable consequences.\(^1\)

I propose, then, the following definition of contextual verifiability: a statement $S$ is contextually verifiable or testable if it is an integral component of a potentially satisfactory theory for a certain class of actual phenomena. Finally I propose the following definition of the term empirical statement:

A statement or theory may be said to be empirical if and only if it is non-vacuously falsifiable either directly - where the statement itself is either an observation-

\(^1\) These definitions are based upon certain remarks of Popper's in "Truth, Rationality and the Growth of Knowledge", C.R. pp. 238-244.
statement or is such that in conjunction with instantial observation-statements it entails a further observation statement - or indirectly - where in conjunction with auxiliary premises which include only observation-statements and statements which have been independently confirmed, either directly or indirectly, it entails an observation-statement which is not entailed by the auxiliary premises alone - or contextually - where it is an integral member of a potentially satisfactory theory for a certain class of actual phenomena.

Two features of this definition should be remarked upon. The first is its vagueness. I have given no explication of the notions of the unity and simplicity of a theory relative to the class of its explananda; and, furthermore, I have not attempted to specify the degree to which a theory must possess these properties in order to count as potentially satisfactory. Indeed I do not think it possible to give rigorous criteria for potential satisfactoriness. But this has the consequence that the definition of contextual verifiability which I offer, and hence my definition of empirical, is in this respect vague and will leave a band of cases whose status is indeterminate. My definition of empirical, therefore, cannot provide a criterion for an unambiguous demarcation of empirical statements.
The second feature to be remarked upon is that, on this account, being empirical is a relativistic property of statements. Both indirect and contextual testability have been made dependent upon the accepted background knowledge. Thus a statement is to count as indirectly verifiable only if it can be conjoined with statements drawn from the store of background knowledge to yield new observational consequences. And since the background knowledge is constantly changing, not only being added to but being revised and reinterpreted, our appraisal of the indirect verifiability of a statement may also change from time to time. A statement may come to be indirectly verifiable when new laws are added to the background store, and it may cease to be indirectly verifiable if those laws have later to be rejected. The situation is similar for contextual verifiability. My account of what it is for a theory to be potentially satisfactory requires a distinction between the explananda for a theory and its further empirical consequences. This distinction, I think, can only be made with reference to the prevailing problem situation in science. The explananda for a theory, that is to say, will comprise the facts recognized at a certain time as standing in need of explanation by a theory of the sort in question: the regularities that have been discovered in that domain, and the anomalies that have upset previous theories. So the appraisal of the contextual verifiability of a
statement also will change with changes in the scientific background knowledge. On my account, then, the question of the empirical status of a statement becomes a relative matter, and is to some extent itself an empirical and historical question. We must sometimes qualify the claim that a statement is empirical with the phrase 'empirical in such a context and against such and such background knowledge'.

It may seem that these two features, particularly the latter, are defects of the account I have offered. Yet, as far as I can see, neither the vagueness nor the relativity of the notion of being empirical can be eliminated. In any case, I hope it will be found that the concept I have defined is a useful analytic tool for the understanding of science.
It remains for us to consider whether there are any metaphysical statements: i.e. whether there are statements which are neither empirical (judged by the criteria I have articulated) nor logically true and which are potentially relevant to scientific discourse. What we have to consider is whether there are any candidates for the status of absolute presuppositions.

One way in which metaphysical statements might be obtained is through the degeneration of a scientific theory.

Faced with the falsification of one consequence of a theory, a scientist might conclude that the theory had been refuted. But more often he will endeavour to save the theory, or at least some considerable part of it, by making adjustments among the auxiliary hypotheses required for the deduction of the falsified consequence. Frequently this is the correct response: frequently it is not the theory but some lacuna in the background information which results in the falsified consequence: and frequently the attempt to save a theory yields important new discoveries - the discovery of Neptune and the discovery of the neutrino are two well-known cases.
The danger here is that one may slip into making ad hoc hypotheses, that one may come to make adjustments to a theory which do no more than insulate the theory against one kind of empirical test, with a consequent loss of empirical content. And were this process carried far enough a theory might be wholly deprived of empirical status, the class of its empirical consequences might come to be identical with the class of its explananda.

At times an ad hoc hypothesis may seem preferable to the abandoning of an otherwise successful theory, particularly where the collapse of the theory would have widespread disruptive consequences. Moreover the question whether an hypothesis is ad hoc may not be at all clear cut. The neutrino hypothesis, for example, was advanced in order to save the law of the conservation of energy from an apparent falsification in the phenomena of beta decay. But the postulated particles had properties - e.g. nil charge and nil mass - which made them well-nigh unobservable. Admittedly the hypothesis could be used to explain other anomalies in beta decay, yet for a long time it could not be made to yield testable empirical predictions. Yet the conservation law was so deeply embedded in physical theory that scientists were prepared to tolerate a hypothesis whose only apparent virtue was that it enabled the
fabric of existing theory to be preserved. It would not be altogether wild to say here that the conservation law had become a presupposition of physical theory, a supposition which could be abandoned only at the cost of a major scientific revolution, and which scientists consequently strove to preserve even at the cost of ad hoc adjustments elsewhere in the system.

Alan Donagan has maintained that what Collingwood took to be the absolute presuppositions of science in any period are doctrines which bear this sort of relation to the scientific theories of the period. They are non-empirical only in the sense that for a time scientists tend to make adjustments elsewhere rather than admit any observation as falsifying them.¹

Collingwood plainly took it for granted that 'the verdict of experience' cannot condemn any assertion that can be reconciled with the observed facts by some hypothesis or other ... His point was that if you interpret your experience in the light of some presupposition, even one we now know to be absurd, e.g. that the earth is flat; then provided that the presupposition is logically possible, you can always invent some hypothesis or other which will square it with what you observe.

And this interpretation tends to be supported both by the examples Collingwood gives, e.g. conservation laws and Newton's laws of motion,² and by the metaphors Collingwood employs to describe

¹ D. p. 31. ² E.M. p. 265-6 and 270.
absolute presuppositions: they are, for example, described as "the yard-stick by which 'experience' is judged", and are said to be unsatisfiable in consequence. But, Bonagm argues, one can regard such 'principles' as non-empirical only if one holds an inadequate theory of refutation, a theory which pays no attention either to the possibility of crucial experiments between competing theories or to the role of the principle of theoretical economy. Yet when a natural scientist claims that 'the verdict of experience' favours Newton's physics against Aristotle', he does not mean that no hypothesis can be imagined which would reconcile Aristotle's physics with the facts on which all competent observers agree. Rather, he means that the only hypotheses that would do so would be ad hoc; that is to say, they would have nothing to recommend them except that they bring about that reconciliation. The verdict of experience, as natural scientists think of it, is always given according to a principle of economy or simplicity, by which ad hoc hypotheses are required to be as few as possible. The possibility of crucial tests between competing theories, with their results judged in the light of the principle of economy, provides an effective check on the empirical degeneration of scientific theories. Scientists may be reluctant to abandon an effective theory, and may tend to bolster it with ad hoc props.

2. B. pp. 81-2.
but the emergence of alternative theories will usually prevent the thoroughgoing entrenchment of a theory as an absolute presupposition of science.

But the empirical degeneration of a theory may take a different course.

We have already seen that the postulates of a theoretical system will often be such that a refutation of the theory will not in strictness of logic constitute a specific refutation of any individual postulate. Only the conjunction of the set of postulates, correspondence rules and auxiliary assumptions which together yield a testable consequence is refuted by the falsification of that consequence. But any specific member of the set - or any sub-set of these the conjunction to which is yet too weak to yield testable consequences - may in principle be reconciled with the results of observation, whatever they may be, provided 'we make drastic enough adjustments elsewhere in the system'. Such adjustments need not be *ad hoc*. Even where it is recognized, in the light of the economy principle, that a certain theory has been refuted, it yet remains possible that some of the postulates of the theory may be saved by being conjoined with new auxiliary premises to form a new theory. Of course, if *ad hoc* devices are proscribed then we cannot say that a new theory - one which shares the
fundamental postulates of the refuted theory yet is capable of surviving critical confrontation with an alternative theory—can always be developed given only sufficient ingenuity on the part of the theorist. Only the logical possibility of such a theory is assured. In practice it may prove impossible to devise one, and presumably it would were the alternative theory true. One might nevertheless hold out in support of some part of a refuted theory in pious hope that it may yet prove possible to incorporate it in a new and more powerful theory. In such a case one would not be adhering to an articulated theory, but, as it were, only to a promissory note for a theory yet to be discovered. Thus though the refutation of a theory may not conclusively refute every, or any specific, postulate of that theory, it does at least, pending the development of a new theory which incorporates some of them, deprive those postulates of empirical status. Only the metaphysical ghost of theory survives.

It may be objected that theoretical statements cannot be abstracted in this way from a theoretical system and still retain a significant role in scientific discourse. But this does not appear to be so. Although such statements cannot any longer serve as premises in scientific explanations, they may continue
to have a regulative function in inquiry, as guide-lines for theory
construction. The postulate of uniform circular motion, for example,
continued to influence astronomy as a regulative ideal, long after
the collapse of the Aristotelian and Ptolemaic systems. Many
thinkers indeed were prepared to entertain the Copernican system
precisely because it seemed to offer a way of retaining the
ancient ideal of circular motion. Secondly it may be objected
that where the refutation of a certain theory has been achieved
through a crucial experiment which strongly confirms an alternative
theory, then an extended principle of economy may be employed to
prevent adherence to those postulates of the refuted theory which
conflict with the surviving theory, until such time as it too
has been refuted. But to achieve this end the principle of
economy would have to be so extended that it would prevent in-
dependent theoretical speculation as long as an existing theory
remains unrefuted. And if crucial experiments have an important
place in the testing of theories, such a 'bird-in-hand' principle
of economy would tend to insulate theories against refutation.

But are there any instrinsically metaphysical statements -
statements which are capable of playing some part in scientific
discourse, yet which are such that they could neither be refuted
by any finite set of observations nor taken up as postulates in any empirical theory?

It has recently been suggested by J.W.N. Watkins¹ that there are two important classes of statements, some of whose members have been influential in the development of science, which are metaphysical just in virtue of their logical form. These are the classes of pure existential and pure all-and-some statements. I have already mentioned this doctrine in discussing Popper's demarcation principle. But it will be useful to take up the matter again, for, although I think the doctrine is indefensible in the form in which Watkins presented it, it seems to me that there is, nevertheless, something of importance here for our inquiry.

Following Popper, Watkins maintains that pure existential statements, like 'Mermaids exist', qualify as intrinsically metaphysical. For although there are possible observation statements which would verify such a statement - e.g. 'There was a mermaid at Surfers' Paradise in the summer of 1960' - no finite set of observation statements could possibly falsify it. For only an exhaustive search of the entire universe could establish the

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falsity of such purely existential statements. There are, of course, existential statements which are not unfalsifiable: 'There was a mermaid at Surfers' Paradise in the summer of 1960' is such a case. But these are impure, or 'circumscribed', existential statements, having written into them a reference to a specific and in principle searchable space-time region. Moreover, although pure existential statements are unempirical because unfalsifiable, they may still have some relevance for scientific discourse. A pure existential statement may contradict a scientific theory; for example, the contradictory of the second law of thermo-dynamics may be expressed in purely existential form as 'There exists a perpetual motion machine'. A pure existential statement may be entailed by a scientific theory. And belief in a pure existential statement, e.g. the belief (or hope) that there is some cure for Leukaemia, might be the inspiration of a scientific research programme.

All-and-some statements are statements which have both universal and existential quantification. An example is

For every metal there exists an acid in which that metal will dissolve .... S
Other examples are 'Everybody has a mother' and 'Every event has a cause.' Expressed symbolically $S$ has the form\(^1\)

$$(x)(\exists y)[Fx \supset (Gy \cdot Rxy)] \ldots S'$$

Now like any statement of law, an all-and-some statement is instantially confirmable but not conclusively verifiable. An all-and-some statement is instantially confirmed whenever for a given instance of its universally quantified term an instance is found for the existentially quantified term such that the specified relation obtains.\(^2\) Thus $S$ is instantially confirmed whenever a particular metal is observed to dissolve in some acid. But whereas an ordinary law statement is also falsifiable - since in conjunction with instantial premises it entails a specific observational hypothesis whose falsity entails the falsity of the law - an all-and-some statement, taken in conjunction with instantial premises, entails an existential statement, which may be pure. Thus $S$ taken in

1. Watkins gives its form as $(x)(\exists y)Rxy$, which more clearly brings out the interesting features of all-and-some statements, but is less accurate than the form given above.

2. I use the phrase 'instance of the universally quantified term' thus:
   
   $a$ is an instance of the universally quantified term in $S'$ if $a$ is an instance of the universally quantified variable $x$ and has the property $F$; i.e. if $a$ is an $x$ which is $F$.

   And I use the phrase 'instance of the existentially quantified term' thus:

   $b$ is an instance of the existentially quantified term in $S'$ if $b$ is an instance of the existentially quantified variable $y$ and has the property $G$; i.e. if $b$ is a $y$ which is $G$.

   This seems to me to be better than to speak, as Watkins does, of 'instances of the universally (or existentially) quantified variable'.
conjunction with 'This is a piece of gold and gold is a metal' entails 'There is an acid in which this metal will dissolve'. To falsify a pure all-and-some statement, then we should have to falsify a pure existential statement, and since the latter are unfalsifiable, pure all-and-some statements are also unfalsifiable. Pure all-and-some statements, then, are instantially confirmable but neither verifiable nor falsifiable.

As with existential statements, all-and-some statements may be circumscribed, either explicitly or implicitly, by a restriction on the field of possible instances for the existentially quantified term. Thus 'For every metal there is some acid stored in the National Physics Laboratory in which that metal will dissolve' is not unempirical, since the existential statements to which it gives rise are not pure. But it is important to notice that a restriction on the spatial region within which instances of the existentially quantified term are to be found, is not a sufficient condition for the falsifiability of all-and-some statements. For consider the statement 'Every criminal leaves a clue'. Here, although in any particular case the physical region might be fairly precisely delimited, it is not clear how clues are to be identified, so that even where the region has been exhaustively
searched it is still possible to say that the clue was there though the searcher failed to recognize it. What is needed is a restriction upon the logical space that has to be searched, i.e. a way of limiting the class of specific existential hypotheses which must be tested.

Now, under my explication of 'empirical', pure all-and-some statements are not necessarily non-empirical. For statements having this form may be, and very commonly are, employed in the context of a theoretical system. This point has often been made by Popper's critics. Newton's second law of motion may provide an example. Ernest Nagel has suggested that what the second axiom can be taken to assert is that there are determinants for changes in momenta which can be formulated in a relatively simple manner and can be specified in terms of the spatial configurations and certain physical properties of bodies. Accordingly, if the class of functions to which \( F \) is restricted is designated by \( 'K' \), then instead of stating the axiom in the form that gives it the appearance of a definitional equivalence (namely, 'The force \( F \) is equal to the product of the mass and the acceleration'), it is more clear and less misleading to formulate it on the present interpretation of the axiom as 'For every change in the momentum of a body, there is a force \( F \) such that \( F \) is a member of \( K \), and \( F = ma \).

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2. The Structure of Science p. 190
* The restrictions are given in the preceding sentence, and do not suffice to 'circumscribe' the all-and-some statement,
Watkins apparently would contend here that a metaphysical statement may be an integral component of a theoretical system without either conferring metaphysical status upon the system or acquiring empirical status from the system. He writes

Being a component of an empirical theory does not, however, entail that the component is itself empirical—for instance, the mathematics of a physical theory is not empirical. Indeed, a number of philosophers from Duhen to Quine have held that all the components of a testable system are individually unempirical because 'our statements about the external world face the tribunal of sense experience not individually but only as a corporate body.

But it seems to me over stringent to regard as non-empirical a postulate whose inclusion in a theoretical system increases the empirical context of the system—particularly as we want to be able to distinguish genuinely theoretical statements from idle metaphysical passangers in a theory, like the doctrine of absolute space. Accordingly I have allowed derivative empirical status to such statements.

At any rate, in 'Between Analytic and Empirical', Watkins allowed that a statement which is not directly falsifiable may count as empirical provided that in conjunction with other statements whose empirical status may be independently determined,

it entails observation statements not entailed by the auxiliary statements alone - i.e. he allowed as empirical statements which are indirectly testable. But this opens up the possibility of indirectly testing an all-and-some statement. For even where the statement is not 'circumscribed' by a restriction on the field of possible instances for its existentially quantified term, the possibility remains that such a restriction might be introduced through auxiliary statements which satisfy Watkins requirements for indirect testability. Watkins himself provides an example in one of his more recent essays. ¹ Consider the statement

_All solid substances are liquefiable . . . T_

which is an unrestricted all-and-some statement. Now 'suppose that there exists a standard kind of apparatus and procedure for liquefying solid substances by electrolysis, known as "the usual electrolytic method".' Then the statement

_If a solid substance is liquefiable at all, then it is always liquefiable by the usual electrolytic method . . . R_

is clearly directly falsifiable. And the conjunction of R and T entails the circumscribed all-and-some statement,

All solid substances are liquefiable by the usual electrolytic method .... T'

which, of course, is also directly falsifiable. The unrestricted all-and-some statement T thus comes to be indirectly falsifiable. It would seem then that pure all-and-some statements are not non-empirical just in virtue of their logical form. They will be unfalsifiable where the class of possible instances for their existentially quantified term is in fact infinite, but having unrestricted all-and-some form does not alone ensure this. Watkins has reacted to this sort of counter-example by further restricting the criteria for empirical statements, so that now only directly falsifiable statements, i.e. statements which entail observation statements given only observation statements as instanial premises, count as empirical for him. But this seems to me to be absurdly restrictive. Rather what has to be given up is the attempt to characterize empirical statements solely in terms of their logical relations to possible observation statements. It may be, and I think is, the case that statements having all-and-some form are very commonly non-empirical; but their being non-empirical is determined as much by features of our background

1. cf. 'When are Statements Empirical', B.J.P.S. 1959-60.
knowledge, i.e. by the absence of putative knowledge sufficient to circumscribe the class of possible instances of the existentially quantified term, as by their logical form.

The class of pure all-and-some statements, however, includes an important sub-class of statements whose logical features differ in an interesting way from those of simple all-and-some statements, like 'Every metal has a solvent', and which may much more plausibly be regarded as intrinsically metaphysical. The distinctive features of such statements were not noticed by Watkins, or at any rate not discussed by him. I shall refer to this sub-class as the class of multiply general all-and-some statements. Their distinguishing feature is that they contain at least a further quantifier beyond the universal and existential quantifiers found in simple all-and-some statements, though this additional generality is commonly apparent only after an analysis of the terms in which the statement is formulated.

The general causal maxim is a typical example. Watkins assimilates this statement to simple all-and-some form, but to do so is to overlook the further universal claim implicit in the notion of causal connection. For to assert that some event is the cause of some other event is, in part, to assert that there
is a lawlike connection between occurrences of the one sort and occurrences of the other. If we express this explicitly in the notation of the predicate calculus then the general causal maxim turns out to be multiply general. Indeed in order to express the general causal maxim in the notation of the predicate calculus we shall have to quantify over both events and their properties. The formula will be something like this:

\[(\forall \mathbf{G})(\exists \mathbf{x})(\mathbf{P}(\mathbf{x}) \land (\exists \mathbf{y})\mathbf{G}(\mathbf{y}) \land \mathbf{F}(\mathbf{y}) \land \mathbf{P}(\mathbf{z}))\]

where 'w', 'x', 'y' and 'z' are variables which range over events, and 'F' and 'G' are variables which range over properties of events, and 'P' stands for the relation 'precedes'. Others of the examples of all-and-some statements cited by Watkins in 'Confirmable and Influential Metaphysics' will be found on analysis to be multiply general.

The additional source of generality in multiply general all-and-some statements has two important consequences. First, the general claim built into any causal statement means that the general causal maxim is not confirmable by observation statements in the way that simple all-and-some statements are. We do not find instances of causation in our experience in the same way that we

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1. I have used ordinary brackets here to indicate the scope of quantifiers and dot brackets for punctuation within the scope of quantifiers.
find instances of metals dissolving in acid, for to find an instance of causation we must succeed in subsuming a particular observable event under a causal law. Thus the existential statements to which a multiply general all-and-some statement gives rise are neither verifiable nor falsifiable by any finite set of observation statements. Secondly, the generality implicit in the notion of causal connection means that any statement which contradicts the general causal maxim will itself be a multiply general statement. 'Every metal has a solvent' is contradicted by the falsifiable hypothesis 'Gold is insoluble'; but the contradictory of an existential statement derived from the causal maxim in conjunction with instantial premises, e.g. 'There is a cause of cancer', is itself a multiply general statement, namely 'There is nothing such that whenever it occurs cancer occurs', which is not directly falsifiable by any finite set of observation statements. Thus neither the existential statement derived from a multiply general statement, nor its contradictory is directly falsifiable by any finite set of observation statements.

It might seem that here at last we have found a class of statements whose logical form is such that every member of the class is non-empirical. Yet this does not seem to be so. There remains the possibility that a multiply general statement may function as a postulate in a scientific theory, contributing to
the empirical content of the whole. And in such a case we should have to say that the multiply general statement is contextually empirical. Once again Newton's second axiom is a case in point, for to find a force function of the sort specified by the axiom we must articulate a further natural law, e.g. the inverse square law of gravitation, which conjoined with the axiom yields observational predictions. But the causal maxim does not seem to be a case of this sort, nor do most of the other metaphysical principles cited by Watkins in 'Confirmable and Influential Metaphysics'. In these cases the multiply general principle does not, in conjunction with the laws involved in its instantiation, yield empirical consequences not independently yielded by its instential laws. These principles are instantiated only in the sense that empirical laws may be discovered which entail, for a certain class of phenomena, that the multiply general principle holds - i.e. laws may be discovered which entail whatever is entailed for a certain domain of phenomena by the multiply general principle. Thus an empirical law may provide instantiations of the causal maxim, cases of causal connection, without at the same time conferring empirical status upon the general causal maxim. In such cases I propose to say that an empirical law satisfies a multiply general statement.
We have then to distinguish two ways in which a multiply general all-and-some statement may be related to empirical statements. It may be that a multiply general statement, taken in conjunction with other scientific laws, gives rise to observation-statements not independently entailed by those other laws: in which case the multiply general statement is contextually empirical. Or it may be that a multiply general statement is satisfied by an empirical law whose empirical consequences are all derivable independently of the multiply general statement. A statement which is only related to empirical statements in the latter way may well be described as 'metaphysical'. But it does not seem possible to determine a priori whether any given statement belongs to this class.

The importance of this discussion of all-and-some statements for my purposes is that it enables me to outline an account of how a doctrine which is unempirical may nevertheless give rise to questions which have empirical statements as their possible answers.

We have seen that it is logically possible that all-and-some statements should be indirectly or contextually empirical - that is, they may have observational consequences when conjoined with other empirical statements. But they may also be related to
possible observations in another way, for an observation statement may entail a statement which is given rise to by an all-and-some statement without itself being given rise to by the all-and-some statement. Thus 'Every metal dissolves in some acid' gives rise to 'This metal dissolves in some acid', which in turn is entailed by 'This metal dissolves in sulphuric acid'; and this last statement, I think, we may take as an observation statement. The statements to which a pure all-and-some statement gives rise are, of course, pure existential statements; and such statements are, in an important respect, indeterminate. One can always ask a question like 'Which acid will dissolve this particular metal?' - to which 'This metal dissolves in sulphuric acid' is a possible answer - and such a question will here be a particularly compelling one, since the pragmatic value of the information that there exists an acid in which this metal will dissolve is negligible unless we can determine which acid will do the job. What we learn from an isolated all-and-some statement in any particular case is something that requires a further specification if it is to be of any value to us. If we cannot supply the specification, and if we nevertheless accept the all-and-some doctrine, then we must recognize a gap in our knowledge. And the gap will be all the more blatant where a multiply general all-and-some statement is involved.
This suggests that we might try to explicate Collingwood's notion of the 'logical efficacy' of a statement, i.e. its capacity for 'giving rise to' questions, along the following lines. Consider any simple all-and-some statement, S, then we might say that S gives rise to the question Q if S, in conjunction with instansial premises for its universally quantified term, entails an unrestricted (or pure) existential statement, E, such that Q may be obtained from E by replacing the existential quantifier by a question forming operator. To explain: suppose S is a statement which we could represent by the formula

\[(x)(\exists y)[Fx \supset (Gy \cdot Rxy)]\]

Then S entails for any a which is F, the existential statement

\[(\exists y)(Gy \cdot Ray)\]

from which we may obtain the question Q

\[(?y)(Gy \cdot Ray)\]

by replacing the existential quantifier in E by a question forming operator, (?y). (?y) is here introduced as an operator attachable to a propositional function whose effect is to form a question from the function. (?y) may be read as 'which is a y such that ...?'. Now provided that E is a pure existential statement, E will not entail an answer to Q, or even a finite disjunctive set of possible
answers; but any possible answer to Q, except the answer 
\(~(\exists y)(\exists y^*\exists y^*^*\forall y\forall y^*\forall y^*^*\exists x\exists x^*\exists x^*^*\exists x^*^*\exists x^*^*^*\exists x^*^*^*)\)\) which is ruled out by S, will entail E. In the case of multiply general all-and-some statements, the account will be analogous, though rather more complex. But we need not go into the complexities here. The important point to notice is that both simple and multiply general all-and-some statements may give rise to questions in this sort of way, and the questions thus generated may have answers which are empirical, even where the all-and-some statements do not themselves stand in the sort of relation to empirical statements which warrants their being called empirical. This situation will arise where an all-and-some statement can either be instantially confirmed or satisfied by empirical statements without being empirically testable.

My suggestion, then, is that a considerable part of Collingwood's theory of metaphysics might be salvaged if we take the absolute presuppositions of science in any period to be those non-empirical all-and-some statements which are capable of giving rise to empirical questions, and which are in fact accepted by scientists working in the period. Absolute presuppositions, on this account, will be presuppositions of science only in the subjective sense; and whether science in a given period has absolute presuppositions will be in large part a contingent and historical question. But whether a given presupposition is absolute
will not merely be a factual question, since for a presupposition to count as absolute it must be both non-empirical and capable of giving rise to empirical questions: and these are essentially logical matters.

But if this account is accepted as a plausible reconstruction of the logical foundations of Collingwood's doctrine of absolute presuppositions, then the thesis that absolute presuppositions are not true-or-false will have to be abandoned.

I suggested (in Section Two of this Part) that there were two reasons for maintaining that absolute presuppositions are not factually significant. The first is that the content of an absolute presupposition is not an empirical proposition, and so cannot be propounded as an answer to a scientific question or as a hypothesis whose truth may later by tested. The second is that absolute presuppositions are supposed to have a regulative role in scientific discourse as question generators - a scientist's metaphysical presuppositions help to determine the sort of questions he sets himself to answer. For these reasons it was suggested that absolute presuppositions might best be construed not as propositions but just as regulative principles; i.e. it was suggested that questions about the truth-or-falsity of metaphysical principles should be ruled out, and replaced by questions about
their fruitfulness as question generators. The account which I have outlined in this section would save both the claim that absolute presuppositions are non-empirical and the claim that they may give rise to empirical questions; and so it might seem that the thesis that absolute presuppositions are not true-or-false could also be sustained. Indeed it has sometimes been maintained that all-and-some statements, our candidate absolute presuppositions, are vacuous, or lacking in factual significance. Warnock, for example, has maintained that the general causal maxim is vacuous, and Hampshire has further maintained that every 'multiply general sentence' either is not used to make a genuine statement or is 'inexplicit and to be replaced by a sentence showing explicitly what definite assertion if any is actually intended.' But I think there are good grounds for rejecting this thesis.

In the first place, the account which I have given of the logical efficacy of a supposition presupposes that what has this property is a proposition, i.e. something true or false. For on my account the logical efficacy of a supposition is to be explicated in terms of its entailments. And as one cannot suppose that a

1. G.J. Warnock, 'Every Event has a Cause', Logic and Language 11.
supposition has entailments without supposing that it may have a
truth value, so one cannot suppose that a supposition is capable of
giving rise to a question without also supposing that it may have
a truth value. Moreover it seems to me that this consequence
could be avoided only if the claim that logical efficacy is a
logical property of suppositions were given up.

Secondly, it has to be noticed that all-and-some statements
stand in logical relations to other manifestly factual statements,
which compel us to accept their truth-or-falsity.¹ Thus an all-and-
some statement may, in conjunction with instantial premises, entail
a pure existential statement; and such a statement, while not
directly falsifiable, may chance to be verified. But if it could
be verified, a pure existential statement must be true-or-false;
and, were it false, its falsity would be transmitted back to the
all-and-some statement from which it was derived.² Again, an
all-and-some statement is falsifiable if it is circumscribed, so
a circumscribed all-and-some statement must be regarded as true-
or-false; but were such a statement true we could infer the truth
of a corresponding uncircumscribed all-and-some statement. For
example, the falsifiable statement 'There are acids in the National

¹. This point has been made by Watkins, 'Between Analytic and
². This point, of course, applies only to simple i.e. instantially
confirmable, all-and-some statements. Multiply general all-
and-some statements do not give rise to verifiable existential
statements.
Physics Laboratory capable of dissolving any metal whatever, if true, would entail the truth of the pure all-and-some statement 'Every metal dissolves in some acid'. Finally, an all-and-some statement may be contradicted by an empirical hypothesis: 'Every metal has a solvent', for example, is contradicted by the falsifiable empirical statement, 'Gold is insoluble'. But if there are empirical hypotheses incompatible with an all-and-some statement, and if those hypothesis are true-or-false, it follows that all-and-some statements must be regarded as true-or-false. For these reasons I think it must be accepted that all-and-some statements are true-or-false; and if we take all-and-some statements as candidate absolute presuppositions, then we must also admit that absolute presuppositions may be true-or-false.

A further important point emerges from these considerations, namely that even where all-and-some statements are not strictly empirical, yet one may have good empirical reasons for accepting or rejecting them. For if an all-and-some statement may be incompatible with an empirical law or theory, then whatever confirms the law or theory must also count against any all-and-some statement incompatible with it. In order to falsify a pure all-and-some statement we should have to falsify a pure existential statement, and this may not be possible. But the contradictory of a pure
existential statement may be entailed by an empirical theory, and in so far as that theory is corroborated by observation, to that extent the existential statement is controverted. The position becomes clearer once we take account of the difference between simple all-and-some statements and the kind of principle which Collingwood took to be absolute presuppositions, for example the general causal principle, which tend to be multiply general. Such principles are neither directly confirmable nor falsifiable by observation. But they may be satisfied by empirical theories, as when a theory is constructed which conforms to the general causal principle, and equally they may be controverted by the construction of theories which are incompatible with them. This suggests that we might regard statements like the causal principle as second order theories: hypotheses about the kinds of theory that may successfully be developed in some area of scientific inquiry. Such theories may be supported or not supported by the developing structure of scientific theory.

1. cf. J.A. Passmore: 'Popper's account of Scientific Method', Philosophy 1960, p. 238; and also G. Schlesinger and R. Brown, 'Falsification without exploration', P.Q., 1961. Schlesinger and Brown give the following example: 'If someone told us "There exists a gas which does not expand when heated at constant pressure" we should controvert him by pointing out that the Kinetic Theory teaches that increase in heat implies increase in the momentum of molecules, and hence, either the pressure or the volume of the gas must increase upon heating'.

2. A view similar to this has been defended by C.H. Whitely, 'Metaphysics and Science', P.Q. 1959.
The representation of metaphysical theories as second order hypotheses which may or may not be supported by the progress of science is an attractive one. It suggests an account of the manner in which metaphysical beliefs change in the course of scientific development, an account which does not make such change wholly arbitrary or irrational, as Collingwood's theory oftentimes seems to do. But there are two objections which Collingwood might have made to this view which need to be taken account of. The first is that this view obscures the regulative role of metaphysical principles, that is, obscures their function as presuppositions. The heuristic value of metaphysical principles, it may be held, depends on their being accepted unconditionally; their adequacy is not to be judged in the light of scientific theories, but theories are judged by them. Where existing theories fail to satisfy an accepted metaphysical principle, the scientist remains dissatisfied, and is impelled to search yet deeper. The second objection is to taking scientific theories as confirming or disconfirming instances of metaphysical principles. For one thing theories are always tentative, conjectural, and liable to change; if one admits a theory as support for a metaphysical principle one has to recognize that the support may later collapse; and
if one takes a theory as undermining a metaphysical doctrine, again one must allow that later developments may reinstate it. Metaphysical doctrines have a way of surviving the scientific theories which support or confute them at different times.
8. METAPHYSICAL CRITICISM

1. It has often been alleged that Collingwood's theory of metaphysics is incompatible with his proclaimed allegiance to science; or, at least, that his theory of metaphysics reduces his acceptance of 'the modern scientific frame of mind' to a dogmatic attitude of commitment. Thus Knox contemptuously remarked, 'his doctrine of absolute presuppositions, with its religious and theological background, has affinities with Kiekegaard and even Karl Barth.'

It is, moreover, possible to make out a strong prima facie case in support of this charge. For Collingwood did argue that, although the whole structure of science depends upon its absolute presuppositions, and although the presuppositions of science have a history of change and development, yet there is no rational way of justifying the choice of one presupposition rather than another. Moreover, he claimed, we cannot even speak of 'choice' here; for absolute presuppositions come to be accepted or abandoned 'by a process of unconscious thought', and normally they 'do their work in darkness'. The metaphysical historian can uncover the absolute

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presuppositions of a scientific epoch, and he can explain how those presuppositions came into currency and why their influence subsequently declined. But his explanation is a giving of causes, not of reasons. Finally, Collingwood did himself draw the conclusion that the only appropriate attitude towards our presuppositions, even once we have succeeded in discovering what they are, 'is an attitude of unquestioning acceptance'.

We must accept them and hold firmly to them, we must insist on presupposing them in all our thinking without asking why they should be thus accepted... We must not now question, in the hope either of justifying them or of condemning them, the presuppositions which in that earlier stage of our life we were content to accept. The fact that we have learned what our absolute presuppositions are does not imply that our attitude towards them should or can cease to be one of sheer presupposal.

So, it would seem, on Collingwood's own admission, that the theory of absolute presuppositions is fundamentally irrational and obscurantist. It may, however, still be asked whether the theory of absolute presuppositions, as I have so far explicated it, does logically compel us to take up an irrationalist position, or whether the theory leaves room for the rational criticism of absolute presuppositions, and for a logical analysis of metaphysical change.

I have been able to find two reasons and a motive which Collingwood apparently had for maintaining an irrationalist position concerning the justifiability of absolute presuppositions. Firstly he seems to have thought that the irrationalist conclusion could be established by the following infinite regress argument.¹ If every argument (justification, or giving of reasons) rests upon certain presuppositions, and if these presuppositions can in turn be justified only by a further argument, with a further set of presuppositions, and so on, then: either every argument generates an infinite regress of arguments; or the drive for justificatory arguments must somewhere be brought to a stop by an absolute presupposition, or fundamental postulate of discourse, which must be either accepted or rejected entirely without argument. I have elsewhere maintained that this argument cannot be used to support Collingwood's theory. For it does not establish that every argument ultimately rests upon presuppositions which are such that they logically could not be questioned. It shows only that the quest for reasons must always come to a stop before it reaches its logical limit — though wherever it stops, should the need arise, it may yet be pushed back even further.

Collingwood's second argument is no more satisfactory.
If absolute presuppositions are neither true nor false, he argued, then the concepts of proof and justification are inapplicable to them, for to demand that an absolute presupposition should be justified is to demand that some reason be given for assuming its truth.1 Collingwood had reached the thesis that absolute presuppositions are not true or false statements by arguing that absolute presuppositions are neither logically true nor capable of empirical verification or falsification. We have found reason to reject his conclusion, but even if it were accepted that absolute presuppositions are not true-or-false propositions, it would not follow that they are immune to criticism. If they were not propositions but rules of some sort, there might yet be critical discussion of their worth as rules. If their function in science is a regulative one, if they serve to generate questions for research, or help to guide and co-ordinate scientific inquiry, then it may be possible to raise questions about their fruitfulness or fecundity in that role. Collingwood's arguments do not rule this out.

1. cf. E.M. p. 47.
Collingwood's motive for denying that absolute presuppositions could be critically appraised was, I think, a desire to prevent any a priori criticism of scientific theories by an extra-scientific investigation of the absolute presuppositions which they satisfy. If absolute presuppositions cannot be appraised by empirical procedures then, he was inclined to believe, the autonomy of science can be preserved only if absolute presuppositions cannot be appraised at all.\footnote{cf. B.M. pp. 51-3.} I think his motive is sound, but I shall argue that it does not compel us to draw an irrationalist conclusion. For from the fact that the truth or falsity of absolute presuppositions cannot be tested by empirical procedures, it does not follow that their adequacy as directives cannot be subjected to critical appraisal. And in any case, if a thorough-going irrationalism is accepted for absolute presuppositions then Collingwood's account of science runs into trouble at another point. The autonomy of science will have been saved, but only at the cost of a relativistic account of scientific progress. For if there cannot be a critical appraisal of absolute presuppositions then we may not say that one set of presuppositions is preferable to another, nor, where one set of presuppositions has
superseded another, may we speak of progress. Thus, if Collingwood's irrationalism is accepted, we shall have to deny that the presuppositions of contemporary physics are an advance upon those of classical physics, or that they in turn are an advance upon the presuppositions of Aristotelian physics. All of these, we shall have to say, are equally legitimate (or equally irrational) alternative systems. And if we say this we shall at least be very tempted to say that the structures built upon these alternative foundations - i.e. Contemporary physics, Classical physics, Aristotelian physics, or pre-scientific mythology - are also equally legitimate alternatives.

At times Collingwood came close to accepting this view; but in the end his good sense was triumphant, and rather than accept a relativistic view of scientific progress, he loudly condemned that 'obscurantist philosophy which by sophistical arguments pretends to prove that the advances which are actually being made are in fact no advances.' In doing so he came to recognize that there is some sense in which absolute presuppositions may be said to have been 'superseded' in the course of scientific development.

2. The difficulty is considerably mitigated once allowance is made for Collingwood's tendency towards a polemical overstatement of the irrationalist strand in his theory.¹ For in fact he was able to recognize two ways in which a critical discussion of absolute presuppositions might proceed. His thesis was merely that neither line of argument could lead to a theoretical justification of any absolute presupposition. We cannot, by investigating the truth or validity of its metaphysical presuppositions, determine whether a scientific programme is 'fundamentally right', or whether those scientists who adhere to it are working 'on the right lines'.² But neither are we reduced to blind 'unquestioning acceptance'; for we can call into question the adequacy of an absolute presupposition.³

In the first place there may be room for the logical criticism of absolute presuppositions. For Collingwood laid

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¹. He was influenced here, I think, by two considerations. Firstly he was concerned to block any rationalistic programme for the a priori appraisal of scientific movements; and secondly, he was interested in preparing the ground for the thesis that metaphysics is a purely historical inquiry. My development of the critical possibilities implicit in his theory hardly affects the first concern, and I think allows the preservation of a modest historicism in the theory of metaphysics.


³. The concept of the adequacy of a presupposition is intended to gain its sense from the discussion in this section.
down a logical condition which should be satisfied by any absolute presupposition or constellation of presuppositions, namely that it should be composed of elements which are logically consupponible.\(^1\) Certainly a part of what he had in mind is the condition that any constellation of presuppositions, or metaphysical system, should be composed of individual presuppositions which are logically compatible with one another. We should not, for instance, combine in a single metaphysical system both the principle of universal causation and the idea that biological evolution is teleologically directed.\(^2\) This does not, of course, mean that in fact the presuppositions which are accepted in any scientific epoch are always consupponible; indeed it frequently happens that they are not, that the metaphysical beliefs in accordance with which science is pursued in a given epoch are, as a system, incoherent. The point is that a metaphysical system whose elements are mutually incompatible is logically incapable of being satisfied by any consistent unified theory.

The logical incompatibility of these two suppositions does not prove that they were not concurrently made; it only proves that, if they were concurrently made, the structure of the constellation that included them both was subject to severe strain, and that the entire fabric of the science based upon them was in a dangerously unstable condition.

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1. E.M. p. 66.  
2. The example is not Collingwood's.  
In such a case the metaphysician’s task should not merely be the historical task of uncovering the metaphysical presuppositions of a given epoch. He should also seek to disclose whatever inconsistencies and conceptual confusions may be embedded in those presuppositions; and he should demonstrate how the logical inadequacy of its metaphysical presuppositions gives rise to 'strains' in the superstructure of science. Here, that is to say, the metaphysician has a critical, as well as descriptive, task to perform.

Collingwood himself undertook such an investigation of the concept of causality and of the associated Kantian presupposition of determinism. He tried to show that the theoretical concept of causality - as distinct from the practicalist or 'handle' conception of causality - is radically incoherent. The theoretical concept, as formulated by Hume and Kant, comprises two elements: (1) the idea of necessary connection and (2) the idea of the temporal priority of causes. The idea of necessary connection he claimed is anthropomorphic in origin, for it derived

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1. Defined by Collingwood as follows: 'A cause is an event or state of things which it is in our power to produce or prevent, and by producing or preventing which we can produce or prevent that whose cause it is said to be'. His example is turning on a switch in order to light. E.M., pp. 296-7.

2. His argument was somewhat confused; I have endeavoured to straighten it out, and my presentation is to that extent inaccurate. Collingwood's argument, by the way, was strongly influenced by Russell's essay 'On the Notion of Cause'.

from the animistic notion of compulsion. However this part of its meaning can be purged, leaving us with the notion of strict conformity to law. The idea of necessary connection thus becomes the idea of necessary and sufficient conditions for the occurrence of an event. The other element, the idea of the \textit{temporal} priority of causes, according to Collingwood, is derived from the practicalist conception of causation, and is anthropocentric. It presupposes a world divided up into \textit{discrete} events. This second element, when combined with the first, gives us the following definition of causality: For any event $E$, if $E$ is caused, then there exists some other event (or cluster of events) $C$ which is distinct from $E$ and temporally prior to it, and which is such that the occurrence of $C$ is a necessary and sufficient condition for the occurrence of $E$. Now according to Collingwood it is logically impossible that any set of occurrences prior to $E$ should be necessary and sufficient for the occurrence of $E$. If $C$ and $E$ are distinct events then the law connecting them must take the form; if $C$ occurs then \textit{ceteris paribus} $E$ must occur; there must always, that is to say, be additional \textit{conditiones sine quibus non} for the operation of a cause.
Thus Collingwood concluded,

'the two suppositions which together constitute Kant's definition of the term 'cause' are not consupponible: or at any rate, not consupponible except under pressure which must produce a somewhat violent strain in the resulting structure.'

Such a strain, Collingwood believed, existed in Nineteenth Century physics. Even although outstanding progress was made in this period, it was unstable and liable to break down as long as the Kantian causal principle was presupposed. For the requirement of strict conformity to law could be satisfied only if the idea of a series of discrete events were replaced by the idea of the continuity of natural processes, and the notion of causality abandoned. This, Collingwood naively suggested, is what has happened in contemporary physics.

We need not worry about the accuracy of Collingwood's remarks on the history of the concept of causality in physics; all that concerns us here is the theory of metaphysical change which underlies those remarks and which they illustrate. Collingwood, it seems, holds that a logical inconsistency in the metaphysical presuppositions of science need not immediately be communicated to the theories which are formulated accordingly. Indeed he held

2. E.M. p. 327.
that genuine progress may occur in a science even though it is directed by presuppositions which are logically incompatible.

But, according to Collingwood, where the presuppositions of science are logically non-consupponible then there develops a strain in the structure of science; and such a strain must in the long run produce a breakdown of scientific progress. In such a case progress can be renewed only if there occurs a change in the metaphysical presuppositions of science, by which the incompatibility is removed.

Collingwood hoped that this point could be developed to give a general account of the dynamics of metaphysical change.

'Why ... do such changes happen? Briefly, because the absolute presuppositions of any given society, at any given phase of its history, form a structure which is subject to 'strains' of greater or less intensity, which are 'taken up' in various ways, but never annihilated. If the strains are too great, the structure collapses and is replaced by another, which will be a modification of the old with the destructive strain removed; a modification not consciously devised but created by a process of unconscious thought.'

Unfortunately he did not expand upon these remarks, and did not anywhere explain how the metaphors of structure and strain should be interpreted. He did not, for example, make it clear whether he was speaking of the structure of scientific theories,

or of the structure of an on-going scientific activity; indeed he did not even make it clear whether the structure of science is to be distinguished somehow from its content. Similarly he did not make it clear whether the 'strains' he spoke of were to be identified as logical defects in the scientific theories which are formulated in accordance with incompatible presuppositions, or whether they could only be identified as conflicting tendencies in scientific movements, which might never give rise to inconsistencies in theory, but show up only as irresolvable dilemmas confronting the theorist. These last alternatives are not exclusive. Indeed if they are combined we gain a more plausible account of metaphysical change. So interpreted the position is roughly as follows. If the presuppositions of a scientific movement are not consupponible then the programme of the movement cannot be consistently carried through. The incompatibility of the presuppositions may give rise to theoretical inconsistencies - and this, Collingwood thought, has happened in classical physics as a result of its use of the concept of causality - or it may give rise merely to theoretical dilemmas - conflicting ideal structures for scientific theory. In either case continuing scientific

progress is possible only if there occurs a change at the level of absolute presuppositions.

Collingwood contended that such changes normally occur 'by a process of unconscious thought'. People do not analyse out the situation and consciously decide upon a reformed metaphysic, indeed they are usually not even aware what absolute presuppositions they are making. It is only in the retrospective view of history that it can be seen where a shift has occurred in the presuppositions of science. Perhaps in point of fact this is so, though I suspect that in times of acute intellectual crisis there often does occur an explicit questioning of presuppositions. At any rate there is no logical foundation for the doctrine of unconscious metaphysical change. It is not that the process of metaphysical change can only be given a psychological or historical and causal analysis; for the preceding argument has provided a logical foundation for the rational criticism of absolute presuppositions. If Collingwood's general account of the dynamics of metaphysical change is sound, then it must be possible for a scientist or philosopher to search

1. E.M. p. 48; Collingwood in this passage slides from the claim that 'people are not ordinarily aware of their absolute presuppositions' or of changes in them, to the implicit claim that they are never aware of them, as the ground for the claim that the changes that occur cannot be a matter of choice.
out the contemporary presuppositions of science, to recognize that
they are incompatible and therefore inadequate for the continuing
progress of science, and consequently to recommend that they be
changed in certain ways. Collingwood himself sometimes admits
this possibility.¹ Logical analysis, it is true, cannot show
which self-consistent presupposition should be chosen, or which
will make possible a further growth of knowledge, but it can show
that and where a choice has to be made.

However the notion of logical strains in a scientific
movement cannot provide a general analysis of the conditions in
which metaphysical change may be necessary. For the concept of
logical non-consupponibility upon which it depends is applicable
only where the component presuppositions of a metaphysical system
are logically incompatible. If no incompatibility exists, then,
although the system is surely still subject to change, there is
no possibility of explaining such changes in terms of structural
strains. We must either allow that here changes occur by processes
which are irrational, i.e. not capable of logical analysis, or we
must extend our account to provide an analysis of changes in
systems of logically consupponible presuppositions.

¹ cf. E.M. pp. 203 & 206.
Collingwood tried to avoid this problem by suggesting that a metaphysical system would be completely free from strain only if its components were so related that the rejection of any one would entail the rejection of all the others.¹ Such a system is unattainable; and therefore, he claimed, the notion of structural strains can always be employed in the explanation of metaphysical changes. However this argument would be plausible only if the notion of strains were cut free from the concept of logical non-consupponibility. For, as Collingwood himself insisted², it is a sufficient condition of the consupponibility of two presuppositions that they be logically independent of one another - they need not be deductively related to be consupponible.

3. A second and less restricted mode of argument about absolute presuppositions opens up if we distinguish between the theoretical justification or validation of metaphysical principles and their pragmatic justification or vindication. Collingwood did not hold that there is no way of vindicating the choice of one presupposition rather than another, though he certainly did not stress this possibility. His theory committed him only to the

assertion that pragmatic arguments cannot be used to establish the truth of a particular metaphysical principle. Thus he wrote,

The only sense in which [an absolute presupposition] can be justified by research is the pragmatic sense. You can say, and rightly, 'See what noble results have come from its being accepted for the last three hundred years! One must surely admit that it works; and that is sufficient justification.' Perhaps. It depends on what you want. If all you want is to congratulate yourself on having the kind of science that you have, you may do so. If you want to congratulate yourself on having the best of all possible kinds of science, that is not so easy; for nobody knows what all the possible kinds would be like.

The qualifications with which this passage closes are, I take it, meant as a warning against treating pragmatic arguments as grounds for believing in the truth of a specific metaphysical principle. It may be shown that the adoption of a certain absolute presupposition has proved fruitful for the growth of science, but this does not mean that it will continue to be so in the future. The same presupposition may later come to impede the growth of knowledge.

Here then we have a further avenue for the critical discussion of absolute presuppositions. One might exhibit the historical consequences of the adoption of a certain presupposition, or compare the consequences of different presuppositions, with respect to their

fruitfulness for the development of science. There might, that is, be a retrospective criticism of absolute presuppositions; and this might well illuminate our understanding of past phases in the history of science. Furthermore one might dispute whether the adoption of a certain presupposition would have desirable consequences. One might, for example, argue that a certain presupposition - e.g. any doctrine of essences or absolutes - is obscurantist, and that its adoption would stifle rather than promote scientific questioning. Collingwood himself makes this sort of point at times.¹

Once the possibility of a pragmatic justification or refutation of absolute presuppositions is admitted it becomes possible to recognize a second way in which the constituents of a metaphysical system might be non-consupponible. For it might happen that two presuppositions which are logically compatible prove, in the development of science, to be divergent. This could happen where two presuppositions were such that (a) it is possible to conceive theories which would satisfy both presuppositions, and (b) it is possible to conceive theories which would satisfy one presupposition and not the other. Now if in such a case it

¹ cf. E.M. p. 192.
happened that all attempts to construct an empirically adequate theory in conformity with both presuppositions proved fruitless, especially where a long accepted theory which did satisfy them both had been refuted, then one might conclude that the two presuppositions were contingently non-consupponible, and that progress could be made only if one or the other presupposition were abandoned.

As an example consider the following pair of (candidate) absolute presuppositions:

(i) The world is such that natural processes can be explained by laws expressed in mathematical functions.

(ii) The world is a deterministic system, i.e. its laws contain no irreducibly statistical components.

The first of these principles belongs, according to Collingwood, to the metaphysical core of modern science, the second is a special principle associated with classical physics. It is clear that the two principles are logically consupponible. For the second satisfies the first - that is, any theory which satisfies (ii) also satisfies (i) - and both are satisfied by Newtonian theory. But it is also possible to conceive theories which satisfy (i) yet are incompatible with (ii), and, it seems, the quantum theory is such a case. Here it seems we have a case where a theory which paradigmatically satisfies both presuppositions
has been refuted and superseded by a theory which is incompatible with one of them. Yet the refutation of Newtonian theory does not entail the falsity of either of the associated presuppositions, nor does the current success of the quantum theory entail the falsity of that supposition which is incompatible with it. It remains possible to adhere to the deterministic principle and to search for a new and deeper theory which both satisfies it, and explains the quantum theory (as an approximation): to search, for example, for a deterministic sub-quantum mechanical theory.\(^1\) The dispute between the determinist and one who accepts quantum theory along with its indeterministic presupposition, then becomes a dispute not about the relative merits of the quantum and classical theories, or a dispute about the interpretation of quantum theory, but a dispute about the likely path of future scientific progress, or about the proper direction for future theoretical inquiries.

As far as I can see no conclusive arguments can be brought against the deterministic programme; yet it does seem possible to marshal arguments of a plausible or pragmatic kind against it — to argue that so far all endeavours in the deterministic direction

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1. A programme of this sort has recently been recommended by, e.g., D. Bohm in 'A Proposed Explanation of Quantum Theory in Terms of Hidden Variables at a Sub-Quantum Level', Observation & Interpretation, ed. S. Körner; and also in Causality and Chance in Modern Physics (1957).
have been unfruitful, or that the renewed momentum of scientific progress, which was made possible by the rejection of determinism in quantum theory, does not yet show any sign of slackening. ¹

Although the notion of contingent non-consupponibility was not developed by Collingwood himself, it does nevertheless provide a useful concept for the explication and development of his theory. In particular it makes it possible to extend his account of metaphysical change to cover changes in a system of presuppositions whose components are logically consupponible. For the logical independence to two presuppositions does not guarantee that they will not prove, in the development of science, to be divergent. Indeed a metaphysical system would be guaranteed against contingent non-consupponibility only if its components were so related that to reject any one while accepting one or all of the other components of the system would involve one in self-contradiction. Such a tight deductive system - in which not even the postulates are logically independent - is of course unattainable. Yet any looser system is liable to develop pragmatic strains.

¹, See for example the Discussion following Bohm's paper, op.cit. pp. 46-51.
Furthermore there are many ways in which an account of metaphysical change in terms of the concept of contingent non-consupponibility accords better with the general features of Collingwood's theory than would an account solely in terms of the logical concept of non-consupponibility. This comes out when we notice the respects in which contingent non-consupponibility differs from its stronger yet narrower logical sibling.

In the first place, contingent non-consupponibility is emergent. We cannot tell by the purely logical analysis of a constellation of presuppositions whether its components are consupponible in this sense. Rather we must wait upon the development of science to see whether and where such pragmatic incompatibilities break out. The same set of presuppositions may be fruitful at one stage in the development of science— as, for example, the special Kantian principles proved fruitful for the development of classical physics— yet later— as the classical system breaks down— develop pragmatic incompatibilities and become an obstruction to the further progress of science. There cannot, therefore, unless the presuppositions are logically non-consupponible, be an a priori appraisal of the potential fruitfulness of a metaphysical system:
It is a mistake, therefore, to fancy that by investigating the truth of their absolute presuppositions a metaphysician could show that one school or science was fundamentally right and another fundamentally wrong.

In the second place, to say that two presuppositions are contingently non-consupponible is to make a practical judgment upon the expediency of a theoretical programme, in the light of its fruitlessness hitherto. But contingent non-consupponibility does not entail that there is no empirically adequate theory which satisfies both presuppositions; nor does it entail that such a theory could not be contrived given sufficient pig-headed persistence. That possibility, like the possibility of winning the lottery, always remains open.

In the third place, contingent non-consupponibility is a matter of degree. The intensity of the strain involved in continued adherence to two contingently non-consupponible presuppositions will depend upon such matters as the frequency and variety of the anomalies which have been encountered, their theoretical centrality (e.g. whether they lead to the refutation of an accepted and perhaps paradigmatic theory, or can be explained

1. E.M. p. 53.
by an available theory which fails to satisfy one of the accepted metaphysical directives), and the amount of effort that has gone into the search for metaphysically satisfactory explanations for the anomalous phenomena. In this context, it seems to me, Collingwood's talk of structural 'strains' in science is particularly apt.

'The ambition of "deductive" metaphysics is to present a constellation of absolute presuppositions as a strainless structure like a body of propositions in mathematics ... A reformed metaphysics will conceive any given constellation of absolute presuppositions as having in its structure not the simplicity and calm that characterizes the subject-matter of mathematics but the intricacy and restlessness that characterize the subject-matter, say, of legal or constitutional history.'

Finally something has to be said about the logical status of the account of metaphysical change that I have been developing. Collingwood, as I have several times remarked, took his theory to be concerned with the dynamics of metaphysical change, i.e. with the analysis of the processes which in fact always operate, though normally subconsciously, whenever metaphysical changes occur.

1. E.M. pp. 76-7: the text mistakenly has the term 'propositions' here.
I however have been arguing that his theory provides, or rather that it can be so developed as to provide, an account of the logic of metaphysical change, i.e., an account of the sorts of consideration that are relevant in an explicit critical appraisal of absolute presuppositions. The theory has thus undergone a change in logical status. Whereas it was presented as a descriptive non-critical analysis, it has become, in my hands, a matter of prescription. This does not mean that it could not often provide a descriptively correct account of the actual process of metaphysical change; nor does it mean that it could not be applied where the change has occurred 'by a process of unconscious thought'. But it does mean that I should not be embarrassed if, as I would expect, it turned out that many of the metaphysical changes that have occurred in the history of science cannot be explained in terms of the theory. It suffices if the theory allows for a (retrospective) criticism of those changes.
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