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JOHN LOCKE AND THE SCIENTIFIC REVOLUTION

A study of the Essay Concerning Human Understanding
in relation to Seventeenth Century Science

by

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"Medicine has for long possessed the qualities necessary to make a science. These are original observations and a known method according to which valuable discoveries have been made over a long period of time. By such a method, too, the rest of science will be discovered if anyone who is clever enough is versed in the observations of the past and makes these the starting point of his researches. If anyone should reject these and, casting them aside, endeavour to proceed by a new method, and then assert that he has made a discovery, he has been and is being deceived. A discovery cannot be made thus, and the reason why such a thing is impossible I shall endeavour to show by expounding the true nature of the science. My exposition will demonstrate clearly the impossibility of making discoveries by any other method but the orthodox one."

(The Medical Works of Hippocrates. 5th century B.C.)

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Abstract

This work aims to set Locke's Essay Concerning Human Understanding in the context of the scientific revolution of the seventeenth century. It does this by showing that the problem of the extent of human understanding to which Locke tried to find an answer was an issue which itself arose out of developments in the sciences in the seventeenth century, and that Locke's answer was itself indebted in a variety of ways to the work of the scientists. Locke's Essay, therefore, cannot be appreciated fully unless its background is also understood.

There is, therefore, a dual aspect to the programme: the scientific background, and the Essay itself. For this reason the thesis is divided into two parts. Part One traces the course of a series of scientific and epistemological issues in the scientific revolution, and shows how scientists answered the problems, sometimes rightly, sometimes wrongly, but, nearly always within the confines of natural philosophy. Part Two is concerned with the intimate link between Locke's answers and the problems faced and the method practised by the scientists. It emerges that not only is much of the content of Locke's answer indebted to the scientific revolution, but also that much of the form of his argument borrows from the new science. The two parts of the work are preceded by an Introduction.

Part One has seven chapters. Each chapter is concerned with a particular period or a key figure in the scientific revolution. A common theme is the outlook of the various scientists to the possibility of science achieving knowledge, as opposed to either belief, or its function being only to 'save the appearances', to be, that is, merely a useful

calculating device. Other aspects of the thought of the scientists are also considered, but the strong central theme is whether or not they believed that man could obtain knowledge.

Chapter One is about a group I call the Neoplatonists. These include Copernicus, Kepler, and the Englishman John Dee. With Copernicus and Kepler I have concentrated particularly on their attitude towards the truth of the heliocentric system. I have used this example to draw out their attitude towards the possibility of achieving truth in the science of astronomy, and I have contrasted their attitude with that of Oslander. Dee's general attitude to science and knowledge, as revealed in his Preface to the first English edition of Euclid's Elements, is shown to have important likenesses with the outlook of Copernicus and Kepler.

In Chapter Two I consider the general scientific outlook of Galileo, beginning with his acceptance of the heliocentric theory, and relating his views on astronomy to his larger conception of the nature of science and the possibility of achieving truth in the sciences. Galileo, like the earlier Neoplatonists, emerges as a man who saw the natural sciences as achieving a degree of certainty which is in fact unjustified; it is an attitude which stands in marked contrast to most pre-Renaissance thinkers.

Chapter III looks at the nature and limits of science as conceived by Descartes. The paradigm of mathematical certainty which Descartes propounded is assessed, and the limitations of the programme which Descartes mapped out are identified. The chapter also includes a section on Rohault, a Cartesian who did not follow Descartes in all his

recommendations, and who was influential in science in the latter part of the century.

In Chapter IV we return to the work of Francis Bacon whose influence over the development of the English conception of the right method to be pursued by scientists was central. Bacon laid down the general empirical approach to nature which was to become the dominant procedure of the scientists of the Royal Society; but Bacon never set the method in a general epistemological context.

The general approach of Englishmen in the period from Bacon to Newton towards the proper extent and nature of the scientific enterprise is examined in Chapter V. Generally, there were two rather different approaches. Some men, like Sir Kenelm Digby and Robert Hooke, expected certainty from science in the true Baconian fashion. Others, like Joseph Glanvill, had no such high expectations. The position, therefore, remained uncertain as to the possible extent of human knowledge.

In Chapter VI we look at the approach to this problem of one of the most important thinkers of seventeenth century England, Robert Boyle. Boyle, we discover, was not so optimistic in outlook as the true Baconians, for he did not expect science to give absolute certainty. Indeed he saw good reason why it could never do so. But he did believe that science could and did achieve some form of knowledge about the physical world.

Similar attitudes towards the scope of science were exhibited by Isaac Newton whose views on the possibility of knowledge are considered in Chapter VII. Newton did not expect science to be able to go much

beyond experience, but he did hold that there was a correct method to be followed in scientific procedure which could and did lead to knowledge. This knowledge might have to be revised in the light of experience, but it should not be rejected out of hand simply because that possibility existed.

In the second part of this work I have attempted to show how John Locke's Essay was a very real attempt to answer the question which had arisen largely as a result of the new science, namely: What was the extent and the limits of human knowledge? We see also how right Locke was to characterize himself as an underlabourer of the new science, for not only was he concerned to clarify issues which scientists had raised about the nature and limits of knowledge, but he also took over many of their presuppositions. Further, Locke's method of approach to his problem is one which itself borrows much from the method of contemporary science.

In Chapter VIII we look at Locke's own scientific background to discover the extent of Locke's acquaintance with the new science. Locke emerges as a man who was well established in at least two important branches of contemporary science, medicine and chemistry, and familiar with most others. He was, therefore, eminently well suited to try to relate science to epistemological issues.

In Chapter IX we turn to the first Book of Locke's Essay in which we find that Locke's argument is one that takes a great deal from the method of contemporary science. The theory of innate ideas is treated by Locke as an hypothesis which is to be rejected as not being substantiated by the empirical data.

Locke's positive thesis, that the mind is furnished with ideas which are all drawn from experience, is considered in Chapter X. Locke's arguments to show that certain of our ideas are the result of experiences are found to be lacking. Thus we find that Locke fails to give an adequate account of such central notions to the scientific revolution as those either of causation or of material object. But the effect of Locke's programme is to emphasise the importance of the contingent and the empirical for man's knowledge.

Chapter XI examines Locke's treatment of the notion of a material object. I consider the reasons why he is led to postulate the existence of material substance, his arguments to show that we cannot know the essences of physical objects, and the implications which these points have for the possibility of achieving knowledge in the sciences.

Finally, in Chapter XII, we turn to Locke's positive answers to the question of the limit and extent of human knowledge. We find that although Locke's definition of knowledge is excessively narrow, the general points which he has to make about the possible extent of knowledge are substantial, and substantially correct. Furthermore, they reflect very accurately the views expressed, usually only with regard to science, by Locke's contemporaries such as Boyle and Newton. Fundamentally, Locke's message was that whilst man should not have expectations about the degree of certainty which science could give which went beyond the contingent and the empirical, it was, nevertheless, right to have such expectations: there was indeed a middle course between the two evils of scepticism and dogmatism.

Preface

My earliest opinion of Locke's Essay was formed in my first year as an undergraduate and it was, I believe, a typical reaction. I found this great classic of British Empiricism wordy and dull. There is some justification for this. The Essay is a long book, often repetitive, and not given to much humour. But what I did not then appreciate was the pivotal position which the Essay holds in the development of the modern European mind. More particularly, I had little understanding of how much our minds have been shaped by the achievements in the natural sciences, and how close the connection was between the development of those sciences and the epistemology advocated by Locke.

In this thesis I have attempted to explain how Locke's Essay is related to the rise of modern science, and how it fits in to the intellectual landscape which existed around it. More than any other great work in British philosophy the Essay is a product of the intellectual issues of its time, and for this reason alone to be appreciated, it must be seen in the light of those issues.

I do not pretend that this work is a comprehensive treatment of the connection between seventeenth century thought and Locke's Essay. Indeed, it is not even a comprehensive treatment of the connections between the Essay and seventeenth century science. I do, however, hope that it begins to make such a treatment by showing that the more that the Essay is probed, the more strongly do the resemblances emerge between its contents and the issues which exercised some of the greatest

scientists of the period. I have particularly concentrated on one question, though not to the exclusion of all others. That question is: How does Locke's answer to the question 'What is the scope of human knowledge?' compare with the answers given by scientists during the course of the scientific revolution? The answer to that question is enlightening for an appreciation both of Locke's philosophy and of the scientific revolution.

It will be fairly obvious, I believe, that I have been influenced in my thinking on this subject by a variety of historians of science and philosophers. Without in any sense being able to claim to emulate him, the historian of science who has most influenced me is the late Alexandre Koyré. My whole sense of philosophy has been much influenced by my mentor Gilbert Ryle.

For this work in particular I am very grateful to Professor A. G. N. Flew who has at all times been a hardworking and helpful supervisor, and to Mr. Patrick Day who has offered many helpful comments on the script. I would also like to thank colleagues in the department for conversations on a variety of aspects of the work, and my pupils who have criticised my ideas. I would also like to thank Mr. Alan Hall, Mr. Eric Hill, and Mr. Richard Wallace, of the Department of Classics in Keele University, for their aid in matters of classical scholarship. My thanks are due too to the staff of Keele University library for their many efforts on my behalf, and for similar efforts and assistance from the staff of the Bodleian Library. Finally may I thank Mrs. Rita Lee for her excellent typing, and my wife Jo-Ann for her help and constant support.

INTRODUCTION

Introduction

1. John Locke is the father of British Empiricism, and the Essay Concerning Human Understanding stands at the threshold of the Enlightenment. Both the British Empirical tradition and the Enlightenment are rightly recognised as central features of modern intellectual history; Locke's importance is guaranteed. But Locke is generally viewed with the hindsight supplied by his successors, most especially in the light of criticisms levelled at him or towards him by Berkeley and Hume. Locke is seen as an instigator, whose initial assumptions were largely taken over by the later classical empiricists, but whose logic was often faulty. It is thought that if Locke had been more rigorous his position would have been far closer than it was to that of Hume.

There is some truth in this view of Locke. It is the one most commonly advocated in the teaching of undergraduates at Commonwealth and American universities. It is the one, partially as a result of this, that is held in those countries by most academic philosophers. But this approach to Locke leaves too much unanswered. It fails to take sufficient account of Locke's stated purpose in writing the Essay. "Everyone must not hope to be a Boyle or a Sydenham;" wrote Locke in the Epistle to the Reader, " and in an age that produces such masters as the great Huygenius and the incomparable Mr. Newton, with some others of that strain, it is ambition enough to be employed as an underlabourer in clearing the ground a little, and removing some of the rubbish that lies in the way of knowledge."¹ Locke was wholly

committed to the achievements of the new science. He was not a critic of those achievements. And the presuppositions upon which his work rested were largely those which were held in common by him and the master builders whom he so much admired. The Essay, therefore, cannot be understood unless the scientific context in which it developed is also understood. My aim is to show the way in which the Essay arose in response to a very real need which the scientific revolution generated and how it was itself indebted to that revolution.

My argument is this: Locke's Essay is the last great work of the Seventeenth Century Scientific Revolution: it summed up the basic presuppositions of that revolution, and stated the basic lessons which had been learned.²

The Essay is not generally viewed in this light. This is, I believe, a function of the division of academic learning which has occurred since Locke wrote; but these artificial compartments should not be allowed to distort our vision of events. When Locke wrote, science and philosophy were not so clearly separable. Even though Locke himself is most responsible for that division, we must look back before it to appreciate the significance of Locke's work.

Frederick Copleston in his History of Philosophy, points out that "Locke was really the first philosopher to devote his main work to an inquiry into human understanding, its scope and its limits. And we can say that the prominent place occupied in modern philosophy by the theory of knowledge is in large measure due to him."³

A basic question arises: why did this issue raise itself as an important one when it did? Why was it that the problem of the scope of human intellect became so crucial in the middle of the seventeenth century? The answer is that the scientific revolution generated a whole series of problems and confusions at an epistemological level, a series which has its beginnings with the publication in 1543 of Copernicus's De Revolutionibus, and which persisted right through until Newton's Principia in 1687, and indeed beyond. Only against this background is it possible to see how important Locke's Essay was. For it was truly the philosophical counterpart of Newton's great work, itself not only a monumental statement of Newton's discoveries, but of a method in contemporary science which emerged from a long series of attempts to identify the scope and method of the natural philosopher. I am not suggesting that Locke offered adequate solutions to all the questions raised by the new science. Many of Locke's answers are still hotly contested, others are simply wrong. But Locke left the issues in a form which made further progress possible, and made it possible within a framework which took full account of the contemporary method and achievement of the scientists. In order, therefore, to understand Locke's work we must see it in the context of the issues which confronted the creators of the new science, and the answers which they proffered.

2. The problem of the scope of human understanding was crucial in the middle of the seventeenth century. When in the autumn of 1517

Luther nailed the ninety-five theses to the door of the castle church at Wittenberg religious authority suffered a blow, and religious teaching received a challenge, which was by no means resolved a hundred years later. Similarly, Copernicus's De Revolutionibus which appeared in the spring of 1543, in commending a stationary sun, produced a movement throughout the intellectual world of gigantic proportions.

Although the eddies emanating from these two sources washed one against the other, it is only those that flowed from the pen of Copernicus that directly concern us. Copernicus offered to the world a new cosmology, and although the world was slow to accept it, the consequence of this offering was radically to call in question the grounding of all contemporary views of the nature of the universe. Copernicus himself was pretty sure of his ground. But his argument was esoteric, imbedded in mathematics and Neoplatonic theory. It is not surprising, therefore that the full impact of the new cosmology was not felt for some decades. It was not until 1632, with the publication of Galileo's Dialogue Concerning the Two Chief World Systems, that the arguments for the Copernican system were spelled out in a form which laymen could appreciate.

The men of science of the seventeenth century were not unnaturally convinced that the methods they used to wrest Nature's secrets from her were adequate to the task. They were anti-sceptics: to have been otherwise would have nullified their efforts. Indeed, as I shall

show, they were often too optimistic.

A powerful sceptical tradition was opposed to the scientists on this score. The sceptics drew their inspiration largely from the works of Sextus Empiricus and they were a force to be reckoned with throughout the seventeenth century. ⁴ The scepticism of this period took many forms and pervaded large, probably most, areas of intellectual enquiry. It was used both to attack natural religion, and also to advocate fideism (for example by Montaigne and Charron). It supplied a basis for rejecting science, ⁵ and the possibility of obtaining knowledge through any form of empirical investigation. Science, in part, actually reinforced the scepticism of the seventeenth century. There was, for example, the feeling of complete uncertainty in the first half of the century generated by the competing accounts of the celestial orbs: in Donne's oft-quoted lines:-

"New Philosophy calls all in doubt
... 'Tis all in pieces, all coherence gone;
All just supply, and all relation."

The new scientists were themselves clearly opposed to scepticism. It is not therefore surprising to find a classic confrontation between these two traditions in the middle of the seventeenth century. That confrontation was Descartes' Discourse on the Method of rightly directing one's Reason and of Seeking Truth in the Sciences. Descartes, beginning from a position of complete scepticism, developed a system whereby it was possible to achieve certain knowledge - or so he maintained. The sceptics' weapons were turned upon themselves, and

for a period scepticism took a very secondary place to the achievements of the sciences.

As the new scientific tradition gathered momentum the ground separating the sceptics from the scientists became clearer. The problem of the nature and limits of scientific knowledge was drop by drop distilled from purely theological and metaphysical matters. Almost always the scientists overstated their case. They claimed more certainty for their results than their methods could support. But gradually there emerged a compromise position. Especially, and characteristically, was this true in England. It was Boyle, Newton, and most important for our purposes, John Locke, who advocated this middle way.

All of these later thinkers took the sceptic's point that matters of fact about the world might - logically might - be other than they are. But they upheld the scientist's position that natural philosophy, properly practised, could lead to knowledge of the world as it is. Their position was summed up by David Hume, who wrote of Newton :-

"In Newton this island may boast of having produced the greatest and rarest genius that ever rose for the ornament and instruction of the species. Cautious in admitting no principles but such as were founded on experiment; however new or unusual ..."

And further Hume saw in Newton a man who had come to terms with the limits of scientific enquiry:-

"While Newton seemed to draw off the veil from some of the mysteries of nature, he shewed at the same time the imperfections of the mechanical philosophy; and thereby restored her ultimate secrets to that obscurity in which they ever did and ever will remain. 6

When in 1690 Locke published the Essay the issues generated by

the new science had become fairly clear. But answers to the problems had not been clearly expounded. It was in large part Locke's task to do this.

3. The problem of the scope of science was one which had been answered, sometimes explicitly, often implicitly, by the scientists of the revolution themselves. It is with their solutions that Part One of this work is concerned. The solutions were varied, but there was a fairly definite development towards the position with regard to the scope of science which was to be advocated by Locke in the Essay.

Later medieval science cannot easily be separated from religious and metaphysical attitudes towards the physical world. As A. C. Crombie has written:

"medieval natural philosophers were primarily interested less in the concrete problems of the world of experience than in the kind of knowledge natural science was, how it fitted into the general structure of their metaphysics, and, if it extended so far, how it bore on theology." 7

By the beginning of the seventeenth century, however, scientists were achieving successes with the concrete problems of the physical world on a scale never before realised. Once again the question arose: what kind of knowledge were the new discoveries? how indeed could the new science be fitted into the metaphysical and theological preconceptions of the age? The irony is in fact that the success of science itself generated problems. Was the scientist's method a way to reach knowledge? Was there indeed such a thing as "the

scientist's method"? And anyway, what exactly was or were the method or methods employed by scientists?

The problem of method became crucial. The competing accounts were many and varied. In astronomy Copernicus appeared to offer one dogmatic solution, echoed by Kepler, and even by Galileo, whilst Osiander, (the author of the Preface to Copernicus's great work) and the sceptics remained cautious. Descartes and Bacon offered methods which in their different ways each claimed would lead to certain knowledge.

In contrast, though not always markedly, other scientists like Hobbes and Boyle expected less certainly from their method, were less eager to offer a general panacea for dispelling all ignorance. Newton and Locke supported this approach to the problem, and it was Locke especially who produced the arguments to justify this caution. Locke put it thus:-

"I think not only that it becomes the modesty of philosophy not to pronounce majestically where we want that evidence that can produce knowledge, but it is of use to us to discern how far our knowledge does reach." 8

The Essay, then, arose in response to a very real problem which the new science generated, and it emerged, too, as a natural development from a tradition which grew during the century.

4. That the Essay is in large part a product of the scientific revolution is not, nor could be, in question. Not only was Locke a personal friend of many of the leading scientific figures such as Boyle, Sydenham, and Newton, he was himself a Fellow of the Royal

Society, and had considerable knowledge and wide interest in virtually all branches of contemporary scientific enquiry. It is not therefore surprising that science was a great influence in formulating Locke's philosophical ideas.⁹

The nature of this influence was threefold. First, Locke took over from contemporary science some of its main presuppositions. These included a belief in the existence of matter, and a belief that matter had two sorts of properties, the primary qualities of "solidity, extension, figure, motion or rest, and number",¹⁰ and the secondary qualities such as "colours, sounds, tastes etc."¹¹ Second, the existence of science, and the success of scientists forced Locke to consider the nature of scientific reasoning, and subject it to careful scrutiny. Third, inseparable from any natural science is empirical observation. The importance attached to observation by the scientists of the period fluctuated. But by the time that Locke wrote, certainly in England, it was generally reckoned the most fundamental aspect of scientific work. Locke took over this empirical approach and turned it into an epistemological axiom. Whether his work was always consistent with this axiom, and indeed whether it is possible to be entirely consistent with it, is another matter.

5. Despite these obvious influences, the actual extent of Locke's debt to the scientific revolution, and the way in which Locke's Essay arose out of issues grounded in contemporary science has never been satisfactorily explored. The only published work devoted specifically to an examination of the impact of science on Locke is

F. H. Anderson's The Influence of Contemporary Science on Locke's Method and Results.¹² But Anderson's work is much narrower in

scope than the present one. The most illuminating account of Locke's historical debt has been supplied by J. Gibson in his Locke's Theory of Knowledge and its Historical Relations.¹³ But there is there no

sustained specific concern with the topic of this work. The same is true of the excellent critique of the Essay by R. I. Aaron.¹⁴

Further, none of these works attempt to show in an historical context how it was that the problem of the extent of human understanding arose out of the developments in the science during the century. The aims of this work, I would suggest, have not been attempted at any length before. This plus the intrinsic interest of the topic surely make it well worthy of consideration.

6. We should make some preliminary distinctions. Philosophers may be classified in a variety of ways. One broad classification is that between Rationalists and Empiricists. Rationalists believe that it is possible to obtain factual knowledge of the world a priori. Empiricists believe that all knowledge is ultimately derived from sense experience.

Another classification is in terms of the number of basic substances to which a philosopher subscribes, and the nature of those substances. A substance, for these purposes, may be understood as ultimate stuff out of which the totality of creation is composed.

Those who hold that there is only one basic stuff which is matter are called materialists. Those who claim that there is only one substance, but it is mind and not matter are called idealists. Others see no chance of either of the former views being correct and maintain that there are at least two basic substances, mind and matter. These, not inappropriately, are labelled dualists.¹⁵

It is a sad comment on intellectual freedom, or, more important, the lack of it, that until comparatively recently it has been difficult to advocate materialism. Materialism, it has been maintained, leads naturally - even inevitably - to atheism, and atheism is wicked. In the seventeenth century the two most important materialist philosophers, Hobbes in England, and Spinoza in Holland, were hounded for their intellectual views simply because they were held to lead to atheism.¹⁶ It is not surprising, therefore, that the theory of knowledge which emerged as an accompaniment to the scientific revolution was not founded on materialism, even though the predominant model used to explain the nature was almost entirely mechanical. Cartesian and Lockian dualism allowed both for the mechanical universe and the religious preconceptions of the age.

But clearly there were reasons, other than sociological, for the acceptability of dualism in the seventeenth century. Descartes, especially, presented important arguments in favour of a dualist theory, arguments which if not conclusive would have to be met if dualism was to be replaced by either of its alternatives. It is, I believe, a significant fact that the rejection of the Aristotelian cosmology, which sharply distinguished between the terrestrial and

superlunary spheres of the universe, was followed by the acceptance of a cosmology or metaphysic involving an equally sharp dualism, that between mind and matter. The Prime Mover, in Aristotle's system, sanctified by Aquinas, was otherworldly, and physically unapproachable. Similarly Descartes's God was not part of the physical world, but was an infinite mental substance. It is important too, that Descartes' rationalist system would certainly collapse without the two substance dogma: Locke's empiricist position was much more flexible.

7. I do not intend to assess the merits of the three views, but some of their implications are worth noting. First: the issue itself is not an empirical one. All experience is compatible with all three views. The issues are preeminently metaphysical, and to choose one rather than another is to make a choice between one metaphysic and another. Second: although this is true, empirical evidence can be relevant. To give an example: if it were found to be possible to give an account of human behaviour in purely physical terms, which explained all the thoughts, actions, and experiences of all human beings, many would see this as highly important in choosing between a dualist and a materialist account of reality. Third: the acceptance of one view rather than another has implications some of which are important for our considerations.

The most important is that idealism leads inevitably to positivism in science. For, as G. E. Moore pointed out,¹⁷ it is fundamental to all idealist theories that esse is percipi for all objects other than minds. This view, first clearly stated in modern times by

Berkeley in the Principles of Human Knowledge, identifies the existence of objects which are not themselves minds, with their existence as objects of perception ontologically dependent on mind.¹⁸ Nothing, therefore, can be said to exist (except another mind) unless it is the object of some mind. Anything which is not observed does not exist. Postulated entities which are held to be 'in principle unobservable' are ruled out as possible candidates for reality. Connected with this view is the thesis that there are no real causal relationships in the so-called physical world. Mechanistic explanations are at best merely useful analogies, enabling the natural philosopher to make predictions, but which can in no sense be held to be truths about the world.¹⁹

8. The implications of these points are vital to an understanding of the connection between Locke's Essay and the scientific revolution. The optimism of the scientists who produced the scientific revolution is reflected in their deep commitment to the possibility of discovery by the new techniques they produced; discovery of the true reality which underlay the phenomena of appearances. They believed, in short, that science could and did reveal the true causes which actually exist in nature.

Far from holding any a priori objection to this programme, Locke in fact, subscribed to it. The problem for Locke was not whether science could give knowledge, but what were the limits to the scientist's method. The issue was not whether there were any ways in which knowledge could be reached, but which amongst the competing criteria employed by the scientists were and were not acceptable.

9. To appreciate this point, consider the main criteria which emerged during the scientific revolution. Five of these are easily identified. But there was also a sixth, less easy to label. The five basic ones we may call (a) empirical confirmation, (b) simplicity, (c) induction, (d) necessity, and (e) explanatory power. Probably no scientist of those times used any one of these to the exclusion of all of the others. It was largely a matter of emphasis rather than of total acceptance or rejection. Further, it is not always easy to separate these different criteria. How far, for example, can induction be separated from the criterion of simplicity? Despite these difficulties the labels will place discussion in a useful framework. The sixth criterion might be labelled the criterion from general cosmological considerations. In what sense I wish to use it will emerge when we consider some examples.

10. These examples, as used by sixteenth and seventeenth century scientists, are merely illustrative, and naturally further discussion of them occurs in the chapters which follow.

(a) Empirical confirmation.

Kepler postulated that the structure of the cosmos was based upon the regular Platonic solids. He made detailed observations and calculations and on their basis concluded that his theory was correct.²⁰

(b) Simplicity.

Copernicus held that the heliocentric system was superior to the Ptolemaic system because it was simpler. The criterion of simplicity was not merely a reason for preferring the heliocentric theory, it was a reason for believing it to be the true system.²¹

(c) Induction.

Newton's Rule IV, in his 'Rules of Reasoning in Philosophy' reads:-

"In experimental philosophy we are to lock upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypothesis that may be imagined, till such times as other phenomena occur by which they may be made more accurate or liable to exceptions." 22

Thus he held that as all observed bodies tend to move towards one another with a force proportional to their mass, and inversely proportional to their distance, we should assume that all bodies whatsoever tend to behave in this way.

(d) Necessity.

Descartes, in the Principles of Philosophy, held that "it is contrary to reason to say that there is a vacuum or space in which there is absolutely nothing." (Principle XVI).²³ That is, it is necessarily true that there are no empty spaces. This followed from Descartes' belief that matter and extension are identical, a view which in its turn he held to be necessarily true.

(e) Explanatory Power.

In his Elements of Philosophy, Hobbes recognised that his explanations of the behaviour of bodies might not be certainly true. However, he argued,

"seeing I have assumed no hypothesis, which is not both possible and easy to be comprehended; and seeing also that I have reasoned aright from those assumptions, I have withall sufficiently demonstrated that they may be the true causes; which is the end of physical contemplation." 24

Hobbes here suggested that all that can be expected from physics is plausible explanation, and that a criterion for accepting an explanation is its explanatory power.

(f) General cosmological considerations.

It is very likely that one of the reasons why Newton held that space was infinite was that he believed God was infinite. Thus in the General Scholium added to the second edition of the Principia he wrote:

"It is the dominion of a spiritual being which constitutes a God; a true, supreme, or imaginary dominion makes a true, supreme, or imaginary God. And from his true dominion it follows that the true God is a living, intelligent and powerful Being; and from his other perfections that he is supreme or most perfect. He is eternal and infinite, omnipotent and omniscient; that is his dominion reaches from eternity to eternity; his presence from infinity to infinity; he governs all things and knows all things that are or can be done." 25

Newton would have argued that to accept that space was finite would be to attempt to place a limitation on God, which is an impossibility.

How these various criteria should be deployed was a constant problem in the scientific revolution. In his epistemology Locke was to argue in favour of an austere empiricist position, and against the more fanciful conceptions of the Neoplatonists of the turn of the century, the dogmatics of Descartes, and the over-optimism of Bacon.

11. At this stage we should take note of some aspects of Locke's work. Already we have seen how Locke thought of himself as an under-labourer clearing the path to knowledge. Philosophy, he suggested, is in great part clarification. Knowledge of the world could only be obtained by experience, and by implication, was best gained by the method practised by scientists such as Boyle and Newton. Much of the failure to appreciate this, Locke suggested, is due to conceptual confusion. Thus he wrote:-

"Vague and insignificant forms of speech and abuse of language, have so long passed for mysteries of science To break in upon the sanctuary of vanity and ignorance will be, I suppose, some service to human understanding." 26

That philosophy is primarily concerned with conceptual clarification was a suggestion of the greatest importance. It has remained the dominant view amongst English philosophers ever since.

It would, however, be wrong to over-emphasise the modernity of Locke's approach. His commitment to conceptual analysis in the Essay was linked with a much less satisfactory aspect: his psychologism. Indeed, it was Locke's concern to write a 'natural history of the soul', to follow the method of Baconian science in philosophy, which led him into some of his worst errors.

Locke's view of the nature of philosophy was itself undoubtedly a product of his view of science. Science was substantially the only way in which knowledge of the world could be obtained. And even that was limited. The philosopher qua philosopher was not generally in a position to give answers to questions about the world. And, for the most part, the method of the scientist was not conceptual analysis but observation. And this was so because there was no a priori road to knowledge of nature. Indeed, as we know, Locke held that ultimately, in some sense, all knowledge was grounded in experience.

12. Locke's contention that all our knowledge was grounded in experience was hardly original. Gassendi, earlier in the century, had asserted that "Every idea which exists in the mind originates in the senses".²⁷ And, of course, the empiricist tradition went back to the classical Greeks and especially to the philosophy of

Epicurus, from whom Gassendi drew so much. Before Gassendi in modern times, Leonardo da Vinci had written that "All our knowledge has its origin in our perception", and that "all sciences are vain and full of errors that are not born of Experience, mother of all certainty."²⁸ Nor was Locke's empiricism of a particularly radical sort. He did not commit himself to the position that all a priori knowledge was impossible. But Locke rehabilitated the empiricist thesis at just the right moment and with much supporting argument. The Rationalism of Descartes, although much discussed, had no committed adherents in Restoration England, strongly influenced as it was by Bacon's programme of knowledge and the control of nature through experiment. Most of the scientists were Baconians, not Cartesians. What they required was a sustained justification of the method to which they felt temperamentally drawn. That support they received from Locke's Essay. Locke argued that the method of Bacon was bound to be correct for ultimately it was the only method open to man in his estate.

13. One of the most important of Locke's contributions to a clear understanding of the nature of science was undoubtedly his rejection of innate ideas. The consequence of this was to rule out of court the pretensions of a priori physics. It is a classic example of the way in which philosophical analysis can dispell vain fictions which have cluttered the path to knowledge. The rejection of innate ideas was entirely in keeping with the conception of knowledge and its limitations which the scientific revolution generated. It is significant that Locke's thesis rests - partially at least - on

arguments identical with those that Newton deployed against Cartesian physics.

14. Although Locke apologised for the frequent use of the word 'idea' in the Essay such an apology was insufficient. The use of the term in different senses²⁹ produces some of the greatest difficulties in assessing Locke's arguments.

Locke held that many of our ideas were the product of a causal chain running from physical objects to the mind. He was thus committed to the existence of a physical reality independent of the mind. Locke believed that this account of the origin of our ideas was substantiated on empirical grounds. But the basis on which the empirical evidence could be accepted was one which itself presupposed an external world.

Locke's commitment to the existence of matter, of material substance independent of our phenomenal experiences, was ultimately an axiom upon which much of his epistemology rested. In this commitment he was entirely at one with the scientists of the times.

Locke believed that empirical evidence established that amongst the causes of the order observable in nature were the unobservable structures out of which physical objects were composed. (Though what the nature of those structures was, was another matter.) The acceptance of this theory was fundamental to the whole of the mechanistic philosophy of the seventeenth century whether expressed as Epicurean atomism by Gassendi and Walter Charleton,³⁰ by Galileo in his distinction between the primary and secondary qualities of objects in the Assayer of 1623, or by Boyle in his account of the

corpuscular philosophy. Material substance, with its powers, was for the scientific revolution and for Locke, not simply an after-thought added to a more or less coherent theory. More nearly it was an axiom upon which the new attitude to nature was founded. Berkeley's rejection of Locke's material substance was not only the rejection of an epistemology but also the rejection of contemporary physics, as Berkeley well realised.³¹

15. Locke defined knowledge as 'nothing but the perception of the connexion and agreement, or disagreement and repugnancy of any of our ideas.'³² The definition was soon attacked by Leibniz³³ for its narrow conception, and the attack was in many respects justified. Locke's definition is, among other things, actually inconsistent, at least on most possible interpretations, with many of his own examples of knowledge. But in an important respect, one too often underplayed when considering the importance of Locke, his account of knowledge and probability in the fourth Book of the Essay came to terms with the developments in the physical sciences more completely than those of any of his contemporaries. Locke, typically, and fairly happily, advocated a course between scepticism and dogmatism with regard to the power of science to explain nature. His conception of knowledge and probability allowed for the important fact that scientific investigation can approach both more nearly to, and also sometimes actually arrive at, the truth about particular areas of the physical universe. What Locke does deny is that we can always know when or if we have arrived at some final truth. On this question Locke was right.

16. Locke shared with Newton the view that the most that could ever

be claimed for much of science was some degree of probability. Both were also committed to a mechanistic interpretation of the physical world. But, equally, they were sceptical about man being able to give a completely satisfactory account of those mechanisms. "The cause of gravity is what I do not pretend to know"³⁴ wrote Newton to Richard Bentley. And it seemed only "probable" to Newton, though no doubt highly probable, "that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles."³⁵ Similarly, Locke was cautious in his commitment to the atomic theory of matter as a known truth of the physical world. "The Corpuscular theory of matter," he wrote in On Education, should be construed as an hypothesis, rather than "a comprehensive scientific and satisfactory knowledge of the works of nature."³⁶

On the possibility of natural philosophy revealing knowledge Locke and Newton were in agreement. Both made experience the truth criterion for propositions about the physical world. Both recognised the dangers of generalisation, or of allowing theory to over-ride facts. Newton's famous remark in the Principia that "I frame no hypotheses"³⁷ echoed not only his own attitude, as expressed in a letter to Oldenburg over twenty years before,³⁸ but also that expressed by Locke when he wrote commending Sydenham and his scientific method that he had increased our knowledge of disease "by way of accurate practical observation." He did not, said Locke, "by speculative hypotheses, fill the world with useless, though pleasing visions."³⁹

Despite the important parallels between Locke and Newton, Newton had no great influence on Locke, at least through his published works, for the very good reason that they were not published until after Locke had formulated his own views. Certainly there was some influence, reflected in various changes and additions in succeeding editions of the Essay. But none was fundamental.⁴⁰ If Newton's writings had no great influence on Locke, then neither had those of Locke on Newton, though once again there are signs of minor influence.⁴¹ In general it would seem, therefore, that the attitudes of these two men emerged from a common background, a background that we shall explore.

17. In contrast, the influence of Robert Boyle on Locke was substantial. Boyle's writings not only presented Locke with many of the scientific assumptions that he absorbed into his epistemology, but they also contained remarks on such subjects as the limits of knowledge which re-appear, sometimes improved, in the Essay. Boyle was not, of course, the only contemporary scientist whose work influenced Locke. But his impact is the most discernable, and probably difficult to overestimate.

18. I do not intend to give an account of all aspects of the scientific revolution which led up to the production of Locke's Essay. Still less is this a history of that revolution. Rather I wish to show how certain problems presented themselves to the scientists, how they attempted to deal with them, and then to appraise their solutions. In the light of this it should be possible to see how Locke's work emerged as a very real product of that

revolution, as offering a series of answers to questions which had emerged during the course of the scientific revolution.

In order to appreciate the importance of Locke's work we must look in some detail at the methods employed by many of the key scientists. This will show how deeply ingrained were certain mistaken assumptions: how many false starts were made, in the attempts to identify the scope of scientific enquiry. After looking at these historical examples we can look at Locke's answers to the problems presented. Consequently, the second part of this work is not a comprehensive treatment of Locke's epistemology. Many of Locke's most important ideas are hardly touched upon. But it is an attempt to see Locke's work in a rather different perspective from that which it is normally viewed. I believe that this perspective goes some way to explaining why Locke holds such an important position in modern intellectual history.⁴²

Part I

THE SCIENTIFIC BACKGROUND

Synopsis of Part I

As the physical sciences developed in the seventeenth century there arose a very definite view both of what counted as scientific method, and of what counted as establishing something as a fact in natural philosophy. This view reached its culmination in the work of Newton. Newton's attitude to science, as expressed in succeeding editions of the Principia, in the Opticks, and in his private correspondence and papers, crystallized a tradition which emerged from the rejection of Aristotelian explanations in the physical sciences, by the dominant thinkers of the age in the preceding hundred years. Thus, whilst Newton's views on science are different from those of, say, Galileo, Kepler, Bacon, Descartes, and many English scientists who founded the Royal Society, there can be traced a pretty constant development towards the sorts of views that Newton put forward.

This development was not a simple process. Any brief account is for that reason alone likely to leave important questions unanswered. Part One of this work, therefore concentrates on illuminating only some key aspects of that development. In particular, I have concentrated almost exclusively on what it was that certain central scientists took to be the scope of scientific investigation, and the possible truth value, if any, of their work.

This task is justified on its own merits, quite apart from any light that it throws on Locke's work. There has been comparatively little done, either by historians of science, or philosophers, on the scope that various scientists saw their work as having, or an appraisal of their views on this. The work that perhaps comes closest

to covering the same area as I do myself is E. A. Burt's classic The Metaphysical Foundations of Modern Physical Science (London, 1924, second edition 1932). But Burt's work is more general in conception than the present one, besides there having been much important research carried out in the intervening period.

My thesis in Part One is this: The new science arose largely on the back of an epistemology which was drawn out of Neoplatonic conceptions both of nature and of knowledge. Gradually the limitations of these preconceptions were realized. This generated a vacuum as to what the possible achievements of science were. The vacuum was partially filled by the theories put forward, in their turn, by Francis Bacon and René Descartes. But neither of these men succeeded in answering satisfactorily the epistemological problems which the new science, because of its obvious achievements, raised. Bacon's method seemed to promise certainty by an empirical method, but failed to live up to that promise. Descartes' rationalism also promised certainty, but in practice only provided conjecture - conjecture of a sort that was found unacceptable to the inheritors of the Baconian tradition. At the time of the foundation of the Royal Society, therefore, there were competing accounts of the scope and nature of science, each reckoned, for different reasons, to be inadequate. Two scientists, especially, responded to this problem, Robert Boyle and Isaac Newton. And they responded in much the same way. Both maintained that the pursuit of absolute certainty in science was a vain enterprise. But the philosophical justification was not supplied by them. In Part Two we shall go on to see that it was in fact supplied by John Locke.

Chapter I looks at the Neoplatonic revival which took place in later Renaissance scientific thought. It will show that Neoplatonism was an important influence on scientific development. In particular it appeared to offer a justification for believing that science could be certain. We shall see that in itself Neoplatonism was not capable of providing this solution.

Chapter II will show that Galileo's conception of science was much influenced by Neoplatonic views. Galileo combined his commitment to the special role of mathematics in understanding the physical world, with an empiricist approach, and did so with obvious success. But, despite this success, various problems remained about the nature of scientific investigation and its possible scope. It will be shown that Galileo's conception of nature, and Neoplatonism generally, rested on a particular, and mistaken, view about the status of scientific laws.

In Chapter III we shall examine the Cartesian solution to the problem of certainty in scientific investigation. Descartes, by his method, should have concluded that all knowledge of the physical world could be absolutely certain. But in fact he was equivocal on this central topic. We shall find that Cartesian rationalism was not capable of achieving absolutely certain knowledge, and that its epistemology was incapable of solving the problems raised by the new science.

In Chapter IV Bacon's method in science will be explored. Although Bacon supplied a method for science, his system was not supported by an epistemological theory of any profundity. There still

remained, therefore, the need for such a theory, even though the Baconian method was considerably successful in practice.

Chapter V looks at the developments in England immediately after the work of Bacon and Descartes, and illustrates the wide variety of views about the nature and scope of science which could be found in England between 1640 and 1680.

In Chapters VI and VII we shall examine how Boyle and Newton came to terms with the problem of the limits of scientific knowledge.

Chapter I.

The Neoplatonists

Osiander's Preface.

Osiander's famous Preface to De Revolutionibus¹ is rightly reckoned one of the scandals of intellectual history. Whether or not Osiander had the author's permission to include it², his suggestion that the heliocentric theory was merely a dubious conjecture on the part of Copernicus was at odds with Copernicus's own commitment. For all that, and it is much, the Preface contains some important remarks concerning methodology in astronomy, methodology in science generally, and on the limits of scientific explanation. These represent an important and long standing school of thought, to become prominent in the seventeenth century.

Osiander's rejection of a realistic interpretation of the Copernican system may have been based partially on his fear that it would be considered contrary to scripture. But it was not his only reason, and it was probably not the main one. Two years prior to the publication of De Revolutionibus Osiander had written to Copernicus:-

"For my part I have also felt about hypotheses that they are not articles of faith but bases for computation, so that even if they are false, it does not matter, provided that they exactly represent the phenomena It would be a good thing if you could say something on this subject in your preface, for you would then placate the Aristotelians and the theologians whose contradiction you fear."³

To assume that the letter is sincere is reasonable. It was not that Osiander simply complied with what he took to be the wishes of his

church superiors. (In contrast to Copernicus, the church for Osiander was that of Luther not Rome.) Rather, he believed, as he was to state in the Preface, that there were no good grounds for asserting the truth of the Copernican system.

In the Preface Osiander propounded the objectives of astronomers:-

"it is the job of the astronomer to use painstaking and skilled observation in gathering together the history of the celestial movements, and then - since he cannot by any line of reasoning reach the true causes of these movements - to think up or construct whatever causes or hypotheses he pleases such that, by the assumption of these causes, those same movements can be calculated from the principles of geometry for the past and for the future too."4

And later:-

"For it is sufficiently clear that this art (astronomy) is absolutely and profoundly ignorant of the causes of the apparent irregular movements. And if it constructs and thinks up causes - and it has certainly thought up a good many - nevertheless it does not think them up in order to persuade anyone of their truth but only in order to provide a correct basis for calculation. But since for one and the same movement varying hypotheses are proposed from time to time, as eccentricity or epicycle for the sun, the astronomer much prefers to take one which is easier to grasp ... And as far as hypotheses go, let no one expect anything in the way of certainty from astronomy, since astronomy can offer us nothing certain, lest if anyone take as true that which has been constructed for another use, he go away from this discipline a bigger fool than when he came to it."5

Osiander was not a Platonist. But there are shades of Plato in some of these remarks. One of the questions set to students of the Academy was "What are the uniform and ordered movements by the assumption of which the phenomena in relation to the movement of the planets can be saved?"⁵ Like Plato, Osiander did not expect the true causes of the phenomena to be revealed. Unlike Plato,

Osiander believed in the importance of "painstaking and skilled observation."⁷

Osiander's emphasis on observation as a basis for astronomy is offset by his scepticism as to the possibility of ever knowing the true causes of the observed phenomena. What in this context did Osiander understand by "causes"? From his remarks it would seem that Osiander was referring to the actual paths of the sun and planets. Possible causes in this sense would be the various different paths the heavenly bodies could actually follow to produce the observed phenomena. Two possible causes, so understood, would be the Ptolemaic system, interpreted realistically, and the Copernican system, though these would only be two among many, indefinitely many, alternatives.

In one important respect Osiander was clearly right. There was no way to choose between the two systems with respect to the then observed phenomena. But he was clearly wrong to assume that there never could or would be any way to test between the two hypotheses or causes. Within seventy years Galileo was to make observations with his telescope which were directly relevant and important empirical evidence - though never knockdown proof - in favour of an heliocentric theory.

In the passages we have considered Osiander presents the following attitude towards astronomy, and, by implication, to science in general:-

- (a) Observation and measurement are the vital first step.
- (b) These should be improved upon as far as possible.
- (c) Various possible systems for explaining these observations should be propounded. A 'possible system' is any system compatible with all of the current empirical data.
- (d) There is no way to choose between the possible systems, since, ex hypothesis they are all compatible with all of the empirical data.
- (e) Given this, the astronomer ought to use the one that is "easiest to grasp".

Osiander's reluctance to interpret celestial theory realistically was one that stemmed from a long tradition. Plato was of a similar view. Ptolemy's position is not clear.⁸ By the time that we reach the later Middle Ages, the general position was that exemplified by Aquinas who wrote in the Summa Theologica:-

"Two kinds of argument are put forward to prove something. The first goes to the root of the matter and fully proves some principle, for instance in natural philosophy there is a conclusive argument to prove that celestial movements are of constant speed. The other kind does not prove a principle conclusively but shows that its acceptance fits in with the observed effects; for instance an astronomical argument about eccentric and epicyclic motions is put forward on the ground that by this hypothesis one can show how celestial movements appear as they do to observation. Such an argument is not fully conclusive, since an explanation might be possible even on another hypothesis." 9

Osiander emerges as representative of a long tradition in approaching explanations of phenomena, a tradition which was based on empirical criteria. If there was no direct confirmation of an

hypothesis by observation, then the hypothesis could only be put forward tentatively.

Such a position, as stated, is unexceptionable. Yet, in fact, it was one entirely at odds with the position of Copernicus himself. How was it, then, that Copernicus came to hold such a different view? The answer lies in the intimate connection between the attitude of Copernicus and the revival during the Renaissance of Platonic, or more correctly, Neoplatonic philosophies.¹⁰

Plato and the Neoplatonists

There were several strands of Plato's thought which gained wide acceptance during the Renaissance. Central to this influence was the so-called Platonic Academy of Florence, active in the second half of the fifteenth century, and its leading philosopher Marsilio Ficino.

The elements in Plato's thought which are important for us were the following:-

1. Plato held that knowledge was only obtainable by an intellectual process. Knowledge was the intellectual apprehension of the real, and the real was not presented directly by experience. It transcended experience.¹⁰

2. As a consequence of this, phenomenal appearances were not accepted as giving knowledge.

3. Knowledge could only be obtained by following a strict

intellectual education, which by gradation led to knowledge of the Form of the Good - which existed timelessly.¹²

4. The study of mathematics played a central role in this education, because the study of mathematics was the study of the eternally existent.¹³

5. By implication, it is possible to attribute to Plato the view that what is known is necessarily true. That is, that real knowledge is absolute necessity.¹⁴

These various strands of Plato's thought appeared with differing emphases in the Neoplatonic philosophers. Not all of them emphasised all of the strands, and some repudiated some of them. Despite this, it is fairly easy to recognise Platonic influences in many of the key figures of the Renaissance, some of whom are vital to our story.

Amongst the Neoplatonists there were both optimists and pessimists about the possibility of ever obtaining knowledge. The pessimistic view was perhaps most ably expressed by Nicolas Cusanus in his most important work Of Learned Ignorance completed in 1440.¹⁵ Cusanus argued that because there is an impossible gulf between the finite and the infinite "absolute truth is beyond our grasp."¹⁶ Man must learn to come to terms with his finite condition and not have pretensions beyond his station.

The attitude of Cusanus was not entirely sceptical. Like Plato he believed that we could approach knowledge by an intellectual route. He held further that in some degree knowledge was possible. Following Plato again, he thought knowledge was only obtainable by abstraction,

and that mathematics was central to the programme. He wrote:-

"Our knowledge of things is not acquired by completely disregarding their material conditions, without which no image of them could be formed; nor is it wholly subject to their possible variations; but the more we abstract from sensible conditions, the more certain and solid our knowledge is. Mathematics is an example of such abstract knowledge ... so that Boethius, the most learned of Romans, went so far as to say that knowledge of things divine was impossible without some knowledge of mathematics." 17

The emphasis which Cusanus placed on abstraction, and the lack of weight which he gave to purely phenomenal appearances are examples of an attitude which was to be of central importance in the development of astronomical and physical theory.

For all their Platonism, the views of Cusanus and the members of the Florentine Academy did not seriously conflict with Aristotelian conceptions of the world.¹⁸ Most of the Neoplatonist philosophers borrowed happily from Plato and Aristotle in forming their outlooks. Their ideas were wielded into a cosmology which nearly always remained fundamentally religious in attitude. Questions about the physical truth of a theory, like the heliocentric theory, were assessed in terms of such a religious attitude. This approach to nature was almost as characteristic of Copernicus as it was of other Neoplatonic thinkers.

2. Copernicus and the Heliocentric Hypothesis.

In contrast to Osiander, Copernicus was quite clearly committed to the truth of his system and thus reveals a very different attitude towards science. Copernicus, like Osiander, recognised that the orbital motion of the earth could not be detected by current observations. But he had other reasons for believing his system to be the true one.

He explained how it was that he came to propound his theory thus:-

"Yet the planetary theories of Ptolemy and most other astronomers although consistent with the numerical data, seemed likewise to present no small difficulty. For these theories were not adequate unless certain equants were also conceived; it then appeared that a planet moved with uniform velocity neither on its deferent nor about the centre of its epicycle. Hence a system of this sort seemed neither sufficiently absolute nor sufficiently pleasing to the mind.

Having become aware of these defects I often considered whether there could perhaps be found a more reasonable arrangement of circles, from which every apparent inequality would be derived and in which everything would move uniformly about its proper centre, as the rule of absolute motion requires the suggestion at length came to me how it could be solved with fewer and much simpler constructions than were formerly used, if some assumptions (which are called axioms) were granted me." 19

An important factor for preferring his system was that Copernicus thought it "more absolute and pleasing to the mind." But for Copernicus, unlike Osiander, such criteria were basis enough to assert its truth. In a note to his seventh axiom Copernicus wrote: "Accordingly let no one suppose that I have gratuitously asserted, with the Pythagoreans, the motion of the earth; strong proof will be found in my exposition of the circles." 20 Copernicus was committed to his system not simply to "save the appearances". For he claims "strong proof" in the more pleasing arrangement that his system offered. The universe must be orderly, it must follow "the rule of absolute motion". And for Copernicus this meant that it must be explicable in terms of circular, and only circular, paths for the planets, moving with constant velocities. If such a system could be produced it would be the true system of the world. And he, Copernicus, had produced that system.

The question arises: why did Copernicus accept the criteria which he did as sufficient to establish the truth of his theory? The answer, I believe, lies in Copernicus's commitment to certain Neoplatonic views. This attitude is exhibited in a variety of places in his writings, and I shall seek to show that Copernicus accepted the heliocentric hypothesis, not solely on the grounds of its greater simplicity, but also, as perhaps Osiander had rejected it, as a result of general cosmological considerations.

Rheticus, Copernicus's pupil, explained his master's allegiance to circular motion to account for the movements of the planets: "Following Plato and the Pythagoreans, the greatest mathematicians of that divine age, my teacher thought that in order to determine the cause of the phenomena circular motions must be ascribed to the spherical earth."²¹ Circular paths, the perfect form of motion, were the only acceptable ones for the heavenly bodies and also for the earth. And like the Pythagoreans, Copernicus accepted the role of the sun in the cosmological order. "In the centre of all rests the sun. For who would place this lamp of a very beautiful temple in another or better place than this where from it can illuminate everything at the same time?"²² Copernicus went on to praise the sun in terms not inappropriate for a Pythagorean sun-worshipper:- "not unhappily do some call it the lantern; others the mind and still others, the pilot of the world. Trismegistus calls it a "visible god"; Sophocles' Electra, "that which gazes upon all things." And so the sun, as if resting on a kingly throne, governs the family of stars which wheel around."²³

Is it possible to explain Copernicus's lavish attribution to the sun? One simple answer might be that Copernicus thought it necessary to use such extravagant language in order to make his heliocentric theory plausible. (But plausible to whom?) Alternatively, we might think that the words truly reflect Copernicus's own views. Further considerations will make this second option very plausible.

Rheticus supplies evidence that the position put forward by Copernicus in De Revolutionibus was honestly held by him:-

"My teacher is convinced that the rejected method of the sun's rule in the realm of nature must be revived. For his aware that in human affairs the emperor need not himself hurry from city to city in order to perform the duty imposed on him by God; and that the heart does not move to the head or feet or other parts of the body to sustain a living creature, but fulfils its function through other organs designed by God for that purpose." 24.

There seems no reason why Rheticus should lie about his master's convictions. Given that he did not, why was Copernicus so eager to assert the importance of the sun's role within his system? The clue to this perhaps is provided by Copernicus's reference in De Revolutionibus to Hermes Trismegistus.

Hermes Trismegistus was believed by Renaissance Neoplatonists, such as Ficino, to have been a real person, probably a contemporary of Moses, and an Egyptian magus, who was the author of the Hermetic writings. In fact the Hermetic writings were composed much later, in the second century A.D. But this was not discovered until 1614. The great influence of the Hermetic writings on Renaissance thinkers has been admirably recounted by Frances A. Yates in her Giordano Bruno and the Hermetic Tradition (London 1964). It is not my intention

to summarise her excellent account, but some points about it are of importance for us. The tradition of natural magic which formed a very large interest for many Renaissance Neoplatonists rested to a very large extent on the Hermetic writings. This tradition is clearly very closely connected with the origins of the scientific revolution. In the Hermetic writings the sun played a role quite different from the one it played in the Ptolemaic-Aristotelian system. It was regarded by Hermes as a second God: "The sun illumines the other stars not so much by the power of its light, as by its divinity and holiness, and you should hold him, O Asclepius, to be the second god, governing all things and spreading his light on all the living beings of the world, both those which have a soul and those which have not."²⁵ There is a fairly remarkable similarity between this passage and Copernicus's justification for the heliocentric theory quoted above. I suggest, therefore, that at least a part, and probably an important part, of the acceptance by Copernicus of the heliocentric theory was his attraction towards the Hermetic outlook, an outlook with which he would have had plenty of opportunity to have become familiar during his ten years stay in Bologna and Padua between 1495 and 1505, if at no other time. The Hermetic writings were at that time the centre of much interest, especially in Italy, because of the attention given them by Ficino and other Neoplatonists. Frances Yates has shown that they had a great influence on Bruno's acceptance of the heliocentric system. And there is some evidence to suggest they had at least a little influence on Copernicus.

Given that this view of Copernicus is plausible, then Bruno's verdict on him must be rejected. Bruno wrote of Copernicus:-

"To him we owe our liberation from several false prejudices of the commonly received philosophy, which I will not go so far as to call blindness. Yet he himself did not much transcend it; for being more a student of mathematics than of nature he was not able to penetrate deeply enough to remove the roots of false and misleading principles and, by disentangling all the difficulties in the way, to free both himself and others from the pursuit of empty enquires and turn their attention to things constant and certain." 26

Rather, it is probable that Copernicus did see along the road that Bruno took. Only Osiander's Preface and his own reticence prevented it appearing more explicit.

We can see that the acceptance of the heliocentric system by Copernicus is a good deal more complicated than at first sight it might appear. The commitment reflects some aspects of the Neoplatonic and Hermetic traditions so powerful in the Renaissance. Only on one level, and that a fairly superficial one, was Copernicus operating with what can be called scientific criteria for the justification of his theory. True, the Copernican theory did accord, pretty well, with the movements of the planets. Certainly Copernicus employed a criterion of simplicity. But even the latter rested on such metaphysical considerations as that circular paths are the most perfect form of motion.

Copernicus and Plato

In Copernicus there were many elements which were to become central to the achievement of the scientific revolution. Several of these had Platonic connections. Like Plato, Copernicus was committed to geometry and geometrical form. This was exemplified in the whole

of his system, and in particular in his commitment to perfect circular paths for the planets. But, unlike Plato, Copernicus saw geometry as a key for unlocking the actual structure of the physical world.

With Plato again, Copernicus rejected appearances as being reality. As he said: "The principle arguments by which the natural philosophers attempt to establish the immobility of the earth rest for the most part on the appearances; it is particularly such arguments that collapse here since I treat the earth's immobility as due to an appearance." ²⁷ Reality, Copernicus held with the Neoplatonists and the Hermetic magicians, lay at a level deeper than that of the phenomena. It lay at a level which could only be reached by mathematical abstraction.

These key concepts were central to the whole of the scientific revolution. They were carried forward to their logical limits in the century which followed Copernicus's death.

Kepler and the Copernican Theory.

More blatantly, but perhaps no more deeply, Kepler found his key to reality in Neoplatonic and Pythagorean ideas. In his Epitome of Copernican Astronomy (1618-21) Kepler offered his answer to the question "By what arguments do you affirm that the sun is situated at the centre of the world?". He replied:-

"The very ancient Pythagoreans and the Italian philosophers supply us with some of those arguments in Aristotle (On the Heavens, Book II, Chapter 13); and these arguments are drawn from the dignity of the sun and that of the place (i.e. the centre, or very near the centre of the universe) ²⁸ and from the sun's office of vivification and illumination in the world." ²⁹

It is most likely that Kepler's Italian philosophers included Ficino who had published an edition of the works of Hermes Trismagistus. (Hermes, as we have seen, argued for a special role for the sun within the universal framework, but not specifically for its central location.)

In the case of Kepler, unlike that of Copernicus, it is not even plausible to suggest that he might have been an admirer of Hermes. Kepler rejected the secret magical traditions of the Hermeticists, Paracelsians, and Rosicrucians quite explicitly in his controversy with the English Hermeticist Robert Fludd. In a letter written at the time Kepler wrote:-

"One sees that Fludd takes his chief pleasure in incomprehensible picture puzzles of the reality, whereas I go forth from there precisely to move into the bright light of knowledge the facts of nature which are veiled in darkness. The former is the subject of the chemist, followers of Hermes and Paracelsus, the latter, on the contrary, the task of the mathematician." 30

There were no overt Hermetic commitments in Kepler's work. But there were strong tendencies to accept some of the conclusions of the Hermetic tradition. Kepler's views both that the sun deserved its privileged position because it was in keeping with its dignity, and that its central location was important for its function in supplying the universe with light, were hardly modern criteria for a scientific theory. They certainly played an important part in Kepler's acceptance of the Copernican system.

There were other considerations. Kepler, like Copernicus, greatly admired elegance in scientific explanation. Thus he described how he came to advocate the heliocentric theory:-

"Already in Tübingen when I followed attentively the instruction of the famous Magister Michael Maestlin, I perceived how clumsy in many respects is the hitherto customary notion of the structure of the universe. Hence I was so delighted by Copernicus, whom my teacher very often mentioned in his lectures, that I not only repeatedly advocated his view in the disputations of the candidates, but also made a careful disputation about the thesis that the first motion (the revolution of the heavens of the fixed stars) results from the rotation of the earth. I already set to work also to ascribe to the earth on physical, or, if one prefers, metaphysical, grounds the motion of the sun, as Copernicus does on mathematical grounds." 31

Kepler believed that God would not, or could not, construct a clumsy edifice, and the tests for elegance were the criteria of the mathematician.

These criteria were most amazingly exploited by Kepler in the Mysterium Cosmographicum which he had published in 1596. In it, on a more exotic scale than the Copernican system developed, Kepler claimed to have established a bond between geometry and celestial harmony. He explained: "If for the sizes and the relations of the six heavenly paths ascertained by Copernicus, five figures possessing certain distinguishing characteristics could be discovered among the remaining infinitely many, then everything would be as desired." Kepler had discovered that the five regular solids of Euclidean geometry appeared to fit the empirical data. He therefore continued:-

"The earth is the measure for all other orbits. Circumscribe a twelve-sided regular solid (dodecahedron) about it; the sphere stretched around this will be that of Mars. Let the orbit of Mars be circumscribed by a four-sided solid (tetrahedron). The sphere which is described around this will be that of Saturn. Now place a twenty-sided figure (icosahedron) in the orbit of the earth. The sphere inscribed in this will be that of Mercury. There you have the basis for the number of the planets." 32

Kepler's explanation for the number of planets completely accepted a mathematical model. A true explanation in astronomy was, for him, the identification of geometrical form in nature.³³

Kepler's account illustrates that his assumptions about what it was to explain any phenomena were far from simple. For, having established that the universe was constructed according to his mathematical model, Kepler went on to claim that this explained why there were just six planets.

Lying behind Kepler's account were these sort of considerations. God, Kepler believed, had made the Universe according to a mathematical model. God, the Great Geometrer, was above all concerned to deploy this skill in his construction of the universe. When, therefore, the earthly geometrician discovered that geometrical pattern in the workings of nature he had uncovered the bare bones of the Creation. For God to have made more, or less, planets would have been inconsistent with his mathematical designs, and this by his very nature he was incapable of being. To discover geometrical form in nature, therefore, was for Kepler to discover the necessary eternal truths of God's creation. It was for these reasons that he could write:- "The reasoning of the Ancients is merely probable, but the demonstrations of Copernicus arising from his principles, bring necessity",³⁴ and as he remarked "Geometrical reasons are co-eternal with God".³⁵ There was, then, in Kepler, a commitment to a form of necessity in God's

creation of a very particular kind. It exemplifies a considerable confusion about the nature of necessity which was to run through a vast amount of seventeenth century science.

Ultimately, I believe, the confusion can be traced back to Plato, with his view that knowledge is knowledge of the forms which necessarily are as they are. It was a confusion exhibited by many of the Neoplatonists, but also by other important thinkers of the scientific revolution. It was exemplified by Cusanus in his obscure view that "The Maximum is Absolute Necessity",³⁶ the view that God, truth and necessity meet in one, a doctrine echoed often in Neoplatonic writings throughout the Renaissance.

The belief in the existence of necessity in nature, as G. de Santillana has argued,³⁷ was undoubtedly very important for the achievement of the scientific revolution. It was psychologically advantageous for the scientists to believe that they could know God's necessities. But to admit the historical importance is not to condone the muddled thinking which generated it. It was one of the conceptual points that needed to be cleared before a true understanding of science could emerge.

We have found that Kepler used three criteria in his assessment of scientific theory. First, throughout his astronomical work Kepler insisted as perhaps only Tycho Brahe had done before him, on accurate empirical information. Second, he drew into his work general cosmological considerations about God and his creation. Third, he employed a test of elegance which used mathematical criteria, and

which was justified largely in terms of the second criterion. As with Copernicus, Kepler maintained that it was through the intellectual discipline of mathematics that it was possible to pierce the veil of appearances and penetrate to the true form of nature, the eternal structure of the universe.

Neoplatonism in England: The Elizabethan Astronomers.

In later medieval England the earlier Platonism of Robert Grosseteste and Roger Bacon was never completely replaced by the Aristotelianism of Aquinas.³⁸ The first two English astronomers of any importance, Robert Recorde (1510 - 1558), and John Dee (1527-1608), traced their scientific ancestry back to Roger Bacon. Their libraries contained the works of even obscure fifteenth century scientists, and the reading lists which Recorde recommended to his students included many works by Platonically inclined English astronomers.³⁹ F. R. Johnson has written: "The conception of science which existed in the sixteenth century was fundamentally that of Plato, rather than that of scholastic Aristotelianism, and it is noteworthy that the sixteenth-century English humanists were, on the whole, students of Plato in preference to Aristotle."⁴⁰ Given this, we should not be surprised to find that the Elizabethan astronomers were inclined to the Copernican system, and for much the same reasons as Copernicus himself had held his theory to be true.

Robert Recorde was very attracted to the Copernican system, if not an actual believer in it.⁴¹ Thomas Digges (c. 1546 -), a pupil of John Dee, was a Copernican. He justified the heliocentric system in much the same way as Copernicus himself had done, as we

can see from the "Addition" which he added to his father's work, A General Prognostication for ever (1592). "In this fourme or frame may we beholde such a wonderful symmetry of motions and situations, as in no other can be proponed." He went on to argue that it was more appropriate for the earth, rather than the heavens to move because "immobility is so divine." Copernicus offered no better reasons himself.

It is the work of John Dee which best exemplifies the Neoplatonism of Elizabethan astronomers. Unlike Kepler, Dee combined his Neoplatonism with Hermeticism, and in Yates's phrase had "to the full the dignity, the sense of operational power, of the Renaissance Magus."⁴² But his attitude towards the possibility of knowledge of nature and its justification was very similar to Kepler's. Dee's views on these topics were most comprehensively expressed in the Preface he wrote to the first English edition of Euclid's geometry, published in 1570. (That Dee should write this Preface is in itself significant.) He showed, as clearly as Galileo was to do, that he believed the book of nature was written in the language of mathematics, and he shared with the Neoplatonists the doctrine that number was the key to reality. Dee, like Cusanus before him, approved of Boethius's remark that:-

"All things (which from the very first original and being of thinges, have been framed and made) do appeare to be Formed by reason of Numbers. For this was the principall example or pattern in the minde of the Creator." 43

Boethius is thus committed to the existence of numbers

independently of, and prior to, physical objects, but they were not identified with God. For Boethius, therefore, there were at least three kinds of real things: God, numbers, and physical objects. This triad was also accepted by Dee.

Dee explained how numbers could lead us to an understanding of the world unobtainable by any other means:-

"By Numbers propertie therefore, of us, by all possible meanes, (to the perfection of the Science) learned, we may both wind and draw ourselves into the inward and deep search and vew, of all creatures distinct vertues, natures, properties, and Formes: And also, farder arise, clime, ascend, and mount up (with speculative winges) in spirit, to behold in the Glas of Creation, the Forme of Formes, the Exemplar Number of all thinges Numerable: both visible and invisible: mortall and immortall, Corporeal, and Spirituall."

Dee saw the role of number in both knowledge of the Form of Forms and also in knowledge of the physical world. How did Dee think this was possible? He explained it in terms of the "triple diversite" of all things. Reality, he held, consisted of two levels, the supernatural, corresponding to Platonic ideas, and the natural, consisting of all the material things. Between these two levels Dee said there was a level of "thinges Mathematicall" which allowed one to move from the material to the forms of the supernatural. He wrote:-

"For these beyng (in a manner) middle betweene thinges supernaturall and naturall: are not so absolute and excellent as thinges supernaturall: Nor yet so base and grosse, as thinges naturall; But are thinges immaterial: and nevertheless, by materialll things hable somewhat to be signified. And though their particular Images, by Art, are aggregable and divisible: yet the generall Formes, notwithstanding, are constant, unchangeable, untransformable, and incorruptible ..."

We can illustrate the position which Dee advocated by an example taken from Galileo's physics. Galileo discovered that a body in free fall accelerates in proportion to the time of the fall. This can be expressed in a mathematical formulation as $d \propto at^2$, where 'd' is the distance, 'a' is a constant, and 't' is the time of fall. Dee would have understood this mathematically expressed regularity as the Form of naturally accelerated motion.

Dee believed that the Forms are to be discovered by empirical investigation. He offered no hope that they can be discovered in any a priori fashion from first principles. He did not anticipate the position of Descartes who was to hold that fundamental laws of physics could and should be established a priori.

The law $d \propto at^2$ is not a statement about any particular body. It is a timeless generalization. For Dee a mathematically expressed law represented that stepping stone from the single, transient occurrence, or number of single occurrences, to the timeless universal law, prescribed by God for objects in free fall. The timeless laws, Dee believed, were examples of the eternal.

Dee's use of the term 'Form' is significant. The Platonic Forms were immutable. It suggests that Dee, like Kepler, believed or presupposed that there was a form of necessity about the laws of nature which arose from their very essence. If this was one of Dee's presuppositions, then we would expect him to have believed that natural philosophy could reach not merely probable but certain conclusions. This position we indeed do find Dee advocating.

After arguing that mathematics was central to almost all areas of human enquiry, Dee discussed what he called Archemastrie "which name is not so new, as this Arte is rare." He explained what he meant by it:

"This Arte teacheth to brying to actuall experience sensible, all worthy conclusions by all the Artes Mathematical proposed, & by true Naturall Philosophie concluded: & both addeth to them a farder scope, in termes of the same Artes, and also by his propre Method, and in peculiar termes, procedeth, with helpe of the foresayed Artes, to the performance of complet Experiences, which of no particular Art, are hable (Formally) to be challenged."

By this method, then, Dee argued that we can reach certain, not probable, conclusions. Dee therefore felt entitled to call it a "science", that is demonstable knowledge, "rather than an Arte: for the excellency and Mastership it hath, over so many, and so mighty Artes and Sciences. And because it procedeth by Experiences, and searches forth the causes of conclusions, by Experiences: and also putteth the Conclusions Themselves in Experiences, it is named of some, Scientia Experimentalis. The Experimental Science."

Dee's understanding of a cause was the same as that of Osiander and Kepler: to find the cause was to find the eternal law which lay behind the phenomena under investigation. The only branch of knowledge which Dee currently saw as having reached that sort of development was Optics, a science which had from the Greeks onwards utilized geometry extremely successfully.

Conclusions.

The criteria which the Neoplatonists used in their acceptance of scientific theory arose from their general cosmological commitments.

Their metaphysics preceeded their physics, and their physics was often judged by their metaphysical criteria. This is not to say that these criteria were always employed. Often a simple observation would be sufficient to establish or reject some hypothesis. But when there was no such possible test they had recourse to general metaphysical considerations. These often took a very specific form. Causes, for example, were generally understood in terms of mathematically expressable regularities. But these regularities were not accepted as contingent regularities of the physical world. To identify a regularity was to identify an eternal form of nature. It was a form of nature which in some obscure sense could not have been otherwise. To succeed in such an identification, therefore, was both to penetrate into the essence of the universe and the soul of God. To find order in the Universe was to partake of the eternal. It was for this reason that the conclusions of the natural philosophers could be regarded as certainties. Such an attitude to the discoveries of science depended entirely on accepting the Neoplatonic doctrines. Without them there could be no justification for either the certainty or necessity of the conclusions.

Neoplatonism was to be rejected. But not as a result of a frontal assault on the philosophical arguments supporting its metaphysics, as it might have been. Rather its rejection arose out of a new-born confidence in the possibility of pursuing science without the need for it to rest upon such metaphysical foundations. The maturing child, science, felt no further need of such parental protection.

Although Plato had argued against the view that sense experience, or at least sensation, gave knowledge, he had held, in opposition to Protagoras, that objective knowledge was possible, albeit only for the enlightened few. The Neoplatonists we have considered took over this view. They were optimistic in outlook compared with the radical scepticism found in the sixteenth century.⁴⁴ This positive belief was undoubtedly a powerful and important force. Without it one wonders if the scientific revolution would have occurred at all. The Neoplatonists believed that real knowledge was obtainable by an intellectual process. Although observation was important, it was necessary to penetrate beneath or behind the phenomena of sensation to find the form of reality. This view, modified from the original Platonic conception had a vast influence on the development of the scientific revolution.

Chapter II

Galileo's Bridge

Introduction

Galileo's attitude to the physical world must be seen in the light of the Neoplatonic tradition which preceded it. Many of Galileo's presuppositions were taken straight from that tradition. Until this is recognised many of Galileo's attitudes must remain a mystery. Although Alexandre Koyré was right to maintain that "the Platonism of Galileo Galilei is indeed, quite different from that of the Florentine Academy"¹, this is no reason to reject the existence of common links between the two, or to assume that Galileo's physics is independent of the metaphysics of the earlier Neoplatonists. Quite certainly it is not. Galileo's Neoplatonism is reflected in his presumption of a cosmological order which was very similar indeed to that subscribed to by Copernicus, Kepler, and Dee; thinkers themselves influenced by Neoplatonic and Pythagorean theories.² This commitment has tended to be overlooked, or at best underplayed, by commentators on Galileo. Galileo's achievements were so great, and his method in many respects so modern, that it is fairly easy to pass over those aspects of his work which are not easily assimilated into what we now take to be scientific.

To suggest that many of Galileo's presumptions were taken from the earlier tradition is in no way to detract from Galileo's achievement. Galileo was deeply involved in a conceptual revolution, and conceptual revolutions of the magnitude of that with which Galileo

was concerned take generations to absorb. Few men contributed more than Galileo to its production and it is one mark of his genius that he prized physics free from its too close involvement with cosmological considerations.

2. The Acceptance of the Heliocentric Theory.

It was not until 1610, with the publication of his Siderius Nuncius,³ that Galileo committed himself in print to the heliocentric theory. With his telescope he had observed and identified the satellites of Jupiter, which in their orbits around the planet were, on the Copernican view, a model of the solar system. Galileo wrote of them: "Variously moving about most noble Jupiter as children of his own, they complete their orbits with marvellous velocity - at the same time executing with one harmonious accord mighty revolutions every dozen years about the centre of the universe; that is the sun."⁴ Stillman Drake suggests that this is not an unequivocal commitment to an heliocentric system.⁵ But there seems no reason to believe it is not. The statement certainly implies that the sun is stationary. It is impossible to see how this could be combined with any other position than a heliocentric one.

Galileo publicly acknowledged his commitment to Copernicus only after his telescopic observations. But he was a Copernican long before this. In 1597 Galileo wrote to Kepler: "I adopted the teachings of Copernicus many years ago, and his point of view enables me to explain many phenomena of nature which certainly remain inexplicable according to the more current hypotheses."⁶ Galileo, then,

was a Copernican in his twenties. Why then did the young mathematician accept the Copernican system as being true? The answer lies, I believe, in his commitment to the sort of Neoplatonism which we have found in earlier thinkers.

I shall explore this question largely with reference to Galileo's printed works. In them we shall find many clues about his general attitude towards the physical world. But a word of caution is appropriate. In Galileo's main work devoted to the merits of the Copernican system, the Dialogues Concerning the Two Chief World Systems,⁷ Galileo showed some reluctance to accept beyond question the truth of the Copernican system. "I have taken the Copernican side in the discourse," he wrote, "proceeding as with a pure mathematical hypothesis and striving by every artifice to represent it as superior to supposing the earth motionless - not indeed, absolutely, but as against the arguments of some professed Peripatetics."⁸

Galileo further recognised that the acceptance of the Copernican system was not, and could not be, a straightforward empirical matter, for "all experiments practicable upon the earth are insufficient measures for proving its mobility, since they are indifferently adaptable to an earth in motion or at rest."⁹ But as with Copernicus, this professed reluctance must be construed as a stance, and not Galileo's own belief. The internal argument of the Two Chief World Systems is that of a man firmly committed to the physical truth of the Copernican system.

3. The Method of Science.

To understand Galileo's commitment to the Copernican system we must appreciate his general conception of the physical world, and how it could be comprehended; central to this was Galileo's conception of the method of science. The method in science, Galileo wrote, is the method of Copernicus, who "stands always upon physical conclusions pertaining to the celestial motions, and deals with them by astronomical and geometrical demonstrations, founded primarily upon sense experiences and very exact observations."¹⁰

Galileo believed that the conclusions which could be arrived at by this method were something considerably more than Humean constant conjunctions. As Einstein noted of Galileo's method :-

"The antithesis Empiricism vs. Rationalism does not appear as a controversial point in Galileo's work Galileo, himself, makes considerable use of logical deduction. His endeavours are not so much directed at 'factual knowledge' as at 'comprehension'. But to comprehend is essentially to draw conclusions from an already accepted logical system." ¹¹

What then was the system which Galileo accepted and upon which he built? Fundamental to Galileo's account of the physical world was his commitment to the role of mathematics in nature. In his own most famous words:-

"Philosophy is written in this grand book, the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and read the letters in which it is composed. It is written in the language of mathematics, and its characters are triangles, circles, and other geometric figures without which it is humanly impossible to understand a single word of it; without these one wanders about in a dark labyrinth." ¹²

The key to comprehension is mathematics. But what is it to comprehend the universe? Galileo gave his own answer. It is to discover the regular mathematical forms which underlie the purely phenomenal data with which our sense supply us. To demonstrate something in science we have to show that a particular phenomenon exhibits some form of mathematical regularity. Galileo hints at this in reply to the criticisms of Sarsi.¹³ The issue arose when discussing the paths of comets. Galileo wrote:-

"lines are called regular when, having a fixed and definite description, they are susceptible of definition and of having their properties demonstrated. Thus the spiral is regular ... So is the ellipse. Irregular lines are those which have no determinacy whatever, but are indefinite and casual and hence undefinable; no property of such lines can be demonstrated and in a word nothing can be known about them. Hence to say, 'Such events take place thanks to an irregular path' is the same as to say, 'I do not know why they occur.'" 14

By contrast, to give an explanation in terms of regular paths would be to explain why comets move as they do.

In what sense, then, did Galileo understand it to be an explanation in science to show that a particular phenomenon exhibited a mathematical regularity? To understand Galileo's position it is important to see that it was not just a simple matter of fact that mathematics applies to the world. "The world" he said, "is of necessity most orderly, having its parts disposed in the highest and most perfect order among themselves."¹⁵ And perfect order for Galileo was the order of mathematics. There are more than shades of the attitude we have seen in Dee in the following remarks:-

"That the Pythagoreans held the science of numbers in high esteem, and that Plato himself admired the human understanding and believed it to partake of divinity simply because it understood the nature of numbers, I know very well; nor am I far from being of the same opinion." 16

Further, to reveal the mathematical foundations of the physical phenomena, Galileo believed, was to comprehend necessary and eternal scientific truths. Thus he criticised William Gilbert's method in science:-

"What I might have wished for in Gilbert would be a little more of the mathematician, and especially a thorough grounding in geometry, a discipline which would have rendered him less rash about accepting as rigorous proofs those reasons which he puts forward as verae causae for the correct conclusions he himself has observed. His reasons, candidly speaking, are not rigorous, and lack that force which must unquestionably be present in those adduced as necessary and eternal scientific conclusions." 17

Mathematics, then, was the key to reality, was the key to unlock the secrets of the eternal order in nature. And Galileo was convinced that not only were the heavenly phenomena amenable to mathematical treatment. According to Copernicus the earth was part of the perfect heavens. Mathematical order was as much a terrestrial as a celestial phenomenon. Galileo rejected the Neo-Aristotlean conception of an imperfect earth totally.

Connected with this, Galileo also rejected the view, held by many Aristotelians, that abstract mathematics could not apply to the concrete world. He expressed his attitude on both these topics in the Two Chief World Systems. Salviati, the spokesman for Galileo is in conversation with the Aristotelian Simplicio.

"Salviati: "Are you not saying that because of the imperfection of matter, a body which ought to be perfectly spherical and a plane which ought to be perfectly flat do not achieve concretely what one imagines of them in the abstract?"

Simplicio: That is what I say.

Salviati: Then whenever you apply a material sphere to a material plane in the concrete, you apply a sphere which is not perfect to a plane which is not perfect, and you say that these do not touch each other at one point. But I tell you that even in the abstract, an immaterial sphere which is not a perfect sphere can touch an immaterial plane which is not perfectly flat in not one point, but over a part of its surface, so that what happens in the concrete up to this point happens in the same way in the abstract. It would be novel indeed if computations and ratios made in abstract numbers should not thereafter correspond to concrete gold and silver coins and merchandise. Do you know what does happen Simplicio? Just as the computer who wants his calculations to deal with sugar, silk, and wool must discount the boxes, bales, and other packings, so the mathematical scientist, when he wants to recognise in the concrete the effects which he has proved in the abstract, must deduct the material hinderances, and if he is able to do so, I assure you that things are in no less agreement than arithmetical computations." 18

Galileo believed that there were exact mathematical descriptions of nature either already discovered, as by himself, or awaiting discovery.

Discrepancies between theoretical results and what is found in experimental practices were not for Galileo of any great concern.

Thus in The Discourse on Two New Sciences (1638) when Galileo is considering his conception of inertia and his law of falling bodies, he is not at all put out by the fact that his theoretical results are never exactly reproduced in practice. The conclusions in the abstract will be different in the concrete because of extraneous influences like air resistance.¹⁹ And thus the experimental

confirmation of a theory "shall be little short of a rigid demonstration."²⁰

The method in science, therefore, was this. First we must start with accurate empirical data. Then we try to find within the empirical data some mathematical relationships. The mathematical relationships would never exactly be reproduced by experiment because of the existence of experimental error. The mathematical relationships were examples of the necessary order which exists in nature.

4. Perfect Circular Paths.

A good example of Galileo's understanding of order in nature is his commitment to the circle as a basic explanatory concept. Given Galileo's commitment to a universe which is "of necessity most orderly", the circle had a very special place within that order. Circular motion, Galileo held, is the only motion which can be entirely uniform. As he expressed it: "this being the motion that makes the moving body continually leave and continually arrive at the end, it alone can be essentially uniform."²¹ From which he concluded:-

"only circular motion can naturally suit bodies which are integral parts of the universe as constituted in the best arrangement, and that the most which can be said for straight motion is that it is assigned by nature to its bodies (and their parts) whenever these are to be found outside their proper places, arranged badly, and are therefore in need of being restored to their natural state by the shortest path."²²

The justification which Copernicus offered for his assumption of circular motion is worth comparing with Galileo's.

Section Four of Book One of De Revolutionibus is headed:-

"The Movement of the Celestial bodies is Regular, Circular, and Everlasting - or else Compounded of Circular Movements."

Copernicus explained this:-

".... we will recall that the movement of the celestial bodies is circular. For the motion of a sphere is to turn in a circle; by this very act expressing its form, in the most simple body, where beginning and end cannot be discovered or distinguished from one another, while it moves through the same parts in itself." 23

Galileo was a Copernican not simply because he accepted the conclusions of Copernicus but also because he accepted many of the assumptions and arguments which Copernicus had used to support those conclusions.

It is a point of interest that, given the commitment to circular motion, a special case has to be made out for any object that does not revolve in circular paths. In the Aristotelian system it was the earth which did not revolve. Although at the centre of the universe it was not one of the perfect spheres. Copernicus had to justify a stationary sun, and, as we have seen, he did this partly on the grounds of its majesty, not its imperfection. The Copernican universe was more perfect than that of Aristotle. And perfection was exhibited, according to Copernicus and Galileo, not only in the celestial order, but also by the earth itself. This point, as we have already noted, Galileo generalized in his physics.

Galileo's commitment to the circle was a feature not only of his astronomy but also his mechanics. His concept of inertia was that an object moving on a great circle of the earth had neither a

tendency to proceed or recede from the earth's centre and was therefore indifferent as to motion or to rest. In the Two New Sciences Galileo explicitly linked his inertial principle with the paths of the planets. Having offered his proof for the parabolic path of a projectile (which uses his inertial principle) Sagredo says:-

"Allow me, please, to interrupt in order that I may point out the beautiful agreement between this thought of the Author and the views of Plato²⁴ concerning the origin of the various uniform speeds with which the heavenly bodies revolve. The latter chanced upon the idea that a body could not pass from rest to any given speed and maintain it uniformly except by passing through all the degrees of speed intermediate between the given speed and rest. Plato thought that God, after having created the heavenly bodies, assigned them the proper and uniform speeds with which they were forever to revolve; and that he made them start from rest and move over definite distances under a natural and rectilinear acceleration such as governs the motion of terrestrial bodies. He added that once these bodies had gained their proper and permanent speed, their rectilinear motion was converted into a circular one, the only motion capable of uniformity, a motion in which the body revolves without either receding from or approaching its desired goal. This conception is truly worthy of Plato; and it is to be all the more highly prized since its underlying principles remained hidden until discovered by our Author (i.e. Galileo) who removed from them the mask and poetical dress and set forth the idea in correct historical perspective." 25

Kepler had been forced to abandon perfect circular paths for the planets in the light of his discovery of the elliptical path of Mars. Circular paths were replaced first by Descartes' straight-line inertial principle and his system of vortices, and then by Newton's formulation of the law of inertia and the theory of universal gravitation.

Galileo, however, never abandoned the belief that matter has a natural tendency to follow circular paths. It was for him a belief grounded firmly in his understanding of perfect order, and in his

understanding that the universe was of necessity most orderly. The acceptance of circular motion was fundamental to his whole outlook. Without it it is difficult to see how he could have either accepted the Copernican system or generated his own conception of inertia. The extremely high place which Galileo accorded to uniform circular motion probably goes a long way to explaining why it was that he never acknowledged Kepler's discovery of the elliptical paths of Mars. Such a feature of the heavens would have implied that the universe was less perfect than it might have been; a conclusion that Galileo would have been loathe to accept.

Given Galileo's commitments we can now see why it was that Galileo accepted the Copernican system. It was for similar reasons to those for which Copernicus himself, Kepler, and other Neoplatonists accepted it. The heliocentric system provided a model which allowed the planets to have uniform circular paths. It was a system which could be interpreted both mathematically and realistically. In contrast, the Aristotelian and Ptolemaic systems could not: the Aristotelian system could be interpreted realistically, but not mathematically; the Ptolemaic system could be interpreted mathematically but not realistically. The Copernican system was the only one which Galileo saw as offering both intelligibility in terms of the mathematical forms, and truth.

Further, as Galileo's empirical researches continued, he found what appeared to be - and indeed was - supporting evidence for

the theory in his observations of the phases of Venus, and - more dubiously - in his discovery of the satellites of Jupiter. Combined with this were his successes in mechanics. These in many ways shared the same assumptions as his astronomical theory. They must have encouraged him to believe that his general conception of nature was correct.

Galileo's own favourite evidence for the truth of the Copernican theory seems to have been that it allowed him to account for the tides. He himself granted that his account was only a speculation; and his speculation was in fact entirely misconceived. Perhaps Einstein was right to suggest that "it was Galileo's longing for a mechanical proof of the motion of the earth which misled him."²⁶

Galileo's commitment to the view that the only theory worth considering was one that could be interpreted realistically is an important point about his approach to science. Galileo was determined to maintain such a realistic interpretation against the 'hypothetical' interpretation recommended by Cardinal Bellarmine.²⁷ If he had been concerned merely to 'save the appearances' there would have been much less point in his arguments from his observations with the telescope. Nor would there have been any dispute with the Church.

We have found in the heart of Galileo's work many common features with the Neoplatonic tradition. There is none of the Hermetic mystic in Galileo, and little overt sign of a religious interpretation of

nature. But he shared many common assumptions with the earlier thinkers. His commitment to mathematics as the key to nature, his allegiance to perfect circular paths, and his belief in the necessary order of nature, are remnants of the Neoplatonic attitude which preceded him. What Galileo understood by 'the necessary order of nature' we shall consider below.

Primary and Secondary Qualities.

Galileo never offered a fully-developed epistemology. But he did give an account of the properties of objects. This account is important as the first clear commitment in seventeenth century science to the distinction between primary and secondary qualities. Versions of this account remained dominant in science and in philosophy throughout the period. Galileo's arguments, therefore deserve attention.

In The Assayer (1623), his most important work on scientific method, Galileo argued that the properties of objects are of two kinds. Some properties of objects, he maintained, really are in those objects; other putative properties are not, they are merely the names of sensations in the observer. Galileo's words are worth quoting at length. He wrote:-

"Whenever I conceive any material or corporeal substance, I immediately feel the need to think of it as bounded, and as having this or that shape; as being large or small in relation to other things, and in some specific place at any given time; as being in motion or at rest; as touching or not touching some other body, and as being one in number, or few or many.

From these conditions I cannot separate such a substance by any stretch of my imagination. But that it must be white or red, bitter or sweet, noisy or silent, and of a sweet or foul odour, my mind does not feel compelled to bring in as necessary accompaniments. Without the senses as our guides, reason or imagination unaided would probably never arrive at qualities like these. Hence I think that tastes, odours, colours, and so on are no more than mere names so far as the object in which we place them is concerned, and that they reside only in the consciousness." 28

The argument of the passage begins by considering what are the necessary conditions for conceiving a material object. Galileo in fact lays down what he took to be the defining characteristics of material objects. And it is by reason (or imagination) that we establish what these are.

So far the argument is unexceptionable. But Galileo's next move must be rejected. The secondary qualities, Galileo maintains, are not necessary properties of a material object - in the sense that they are not defining properties - therefore, he concludes, they are not real properties of the object. We can see the fault in Galileo's argument if we consider another example: man may be defined as a rational animal. Therefore, on Galileo's argument, the contingent fact that some particular man has hair on his head is not a real fact about that man, but a product of the consciousness of the observer!

The point is, simply, that the defining characteristics of any object do not establish what characteristics any object does have, (though no doubt it rules out some); defining characteristics only establish what properties an object must have, in order to be an object of that sort. The properties which any object actually has

can only be established empirically.

Galileo did not rest his case for the distinction between primary and secondary qualities on the argument we have just considered only. If he had there would really be no case to answer. He offered further considerations which we shall turn to in a moment. But that Galileo did put forward the above argument is, I believe, significant. It suggests that Galileo believed that it was possible to establish what things are really by intellectual analysis. It shows, in short, that Galileo was no simple empiricist in his dealings with the world. But it is also significant, and indeed typical of Galileo, that he supports his intellectual analysis with some further considerations which are drawn directly from experience.

Galileo supported his distinction with some examples. A feather, he pointed out, can tickle us. But the tickle is not a property of the feather. Rather it is the name of a sensation which we experience. Without our experiencing the tickle, there would be no such thing as the tickle. "The titillation belongs entirely to us and not to the feather." 29

Galileo is quite correct when he says that 'tickle' is a word which is often used to denote a sensation. But to point out that some words are the names for sensations does not prove that other words are. In particular it does not prove that the words for secondary qualities are the names of sensations. Why, then did Galileo accept the distinction?

The answer, I believe, lies in what Galileo was willing to accept as an ultimate explanation of our experiences, and the properties of objects. Ultimate explanations, Galileo believed, would only be in terms of the primary qualities of objects. Reference to secondary qualities, Galileo believed, could always in principle be eliminated in favour of primary quality talk. To see this we can look again at the example of the feather. The sensation of tickling can be explained in terms of the structure of the feather, its size, shape, and motion of the parts of the feather (and, more complexly, in terms of human skin, nervous system and so forth). Ultimately, then, the sensation could be shown to be the experience we have when objects having only the primary properties are set in a particular motion. Galileo believed that if we had enough scientific knowledge we could always explain all secondary qualities as really being manifestations of primary qualities.

In fact Galileo gave us an example of just such a reduction. Heat, he maintained, is the name of a sensation which can be accounted for entirely in terms of primary qualities:-

"Having shown that many sensations which are supposed to be qualities residing in external objects have no real existence save in us, and outside ourselves are mere names, I now say that I am inclined to believe heat to be of this character. Those materials which produce heat in us and make us feel warmth, which are known by the general name of "fire", would then be a multitude of minute particles having certain shapes and moving with certain velocities. Meeting with our bodies, they penetrate by means of their extreme subtlety, and their touch as felt by us as they pass through out substance is the sensation we call heat." 30

Unsophisticated though it is, the general causal explanation which Galileo offers here is perfectly reasonable. On this model causal explanations for all sensations can be given. But it is important to see that it rests upon a certain ontology, and certain assumptions of what it is to explain a putative property of an object.

The ontology is simply that ultimately the physical world is made of matter which has the primary qualities. If one can account for any quality, such as colour, in terms of the primary qualities, then the property so accounted for is dismissed as a real property.

It follows on this sort of argument that if we can explain the primary qualities by some other qualities or quality, then they too will turn out not to be 'real'. Thus if we can explain solidity and size (mass) in terms of energy - as modern science does - it will follow that there are really no solid objects in the universe. Such a position has in this century often been argued for, by, for example, Sir Arthur Eddington in his The Nature of the Physical World (1928). But such a position as Susan Stebbing pointed out (Philosophy and the Physicist [1937]) leads to absurd consequences. If really there is nothing in the universe but energy, then there is nothing in the universe except energy. And really there are no books, people or arguments!

We can contrast Galileo's position with another which does not lead us to the same reductionist conclusions. Granted that science has established that there is a causal explanation of perception, it does not follow that one is thereby bound to distinguish between primary and secondary qualities. Rather we can hold that the

properties which an object has are simply the properties which it is observed to have (and no doubt other properties which are not observed). We can grant that all the properties so observed are observed by means of a causal mechanism. But this does not imply that what it is we see, hear, taste, smell or touch is a sensation 'in the mind'. We often, in fact nearly always, see etc. physical objects. And it is just by such observations that we establish what properties objects have. The scientist's job, on this view, is not to legislate about which properties are real and which are not. But he can find out which properties are causally dependent on other properties.

The issues raised by Galileo's distinction will be considered further when we turn to Locke's use of the distinction in his epistemology. But before we leave the topic there are some general points about the distinction which are worth noting. It was very natural for scientists like Galileo to give a preferred status to certain properties of physical objects. Some properties are easily amenable to quantitative treatment and some are not. Given the Neoplatonic belief that number is the key to reality it was very easy to identify only those properties as real which could be treated mathematically. Shapes, size, numbers, and slow or rapid movements are all such properties. Further, the Galilean theory was not a new one. Its reintroduction was in keeping with the high place accorded the early Greek atomists, such as Democritus, by seventeenth century

scientists. Thus the introduction of atomic theories of matter based upon the writings of Epicurus and Democritus tended also to encourage the acceptance of their epistemological views.

Galileo's Theory of Matter.

Reference to atomic theories of matter raises the question: did Galileo accept such a theory? The answer is that Galileo was certainly inclined to favour corpuscular explanations, but he was not an important advocate of the atomic theory.

As we have seen, he explained heat as the product of tiny particles of matter.³¹ In the Two New Sciences he suggested as an explanation for metal melting the corpuscular structure of fire, which was for Galileo one of the four elements. But he did put his theory forward in the most tentative manner; "a passing thought" rather than "absolute fact."³² Certainly it was an oversimplification for G. B. Stone to write that "Galileo was no atomist"³³. As early as 1590 in his paper 'On Motion' Galileo showed some sympathy for Democritean atomism;³⁴ and his whole explanation of the strength of materials in the Two New Sciences presupposed both the existence of a vacuum, which Galileo claimed is established on experimental grounds, and also the existence of "very smallest particles".³⁵

Galileo did not give his reasons in detail for being at least partial to atomism. But it is not unreasonable to suppose that they were similar to his reasons for accepting other theories in science. Thus, given the distinction between primary and secondary qualities, it follows that the basic constituents of matter must have the primary,

quantifiable, properties. Number becomes the key, not only to the structure of the solar system, but also to the stuff out of which the solar system is constructed.

Galileo and the Scope of Science.

Some of Galileo's most important contributions to the scientific revolution's success lay outside his particular discoveries. They are found in certain aspects of his method, his optimism in the scientific potential, and his claim that science is an autonomous discipline.

In his method Galileo continually emphasised the importance of the particular, in contrast with the general, as the starting point for investigation. It is the swing of a pendulum, an object in free fall, that he investigates so successfully. "We have decided to consider", he wrote, "the phenomena of bodies falling with an acceleration such as actually occurs in nature and to make this definition of accelerated motion exhibit the essential features of observed accelerated motions."³⁶

Although Galileo's method involved presumptions which went far beyond the particular, and we can sometimes doubt that he carried out all the experiments that he claimed, he never attempted to justify - except incidently - his method by appeal to metaphysical or cosmological principles. He could calculate when a beam would break, he could tell the gunners where to aim their cannon. His science could be judged by its achievements.

Science, for Galileo, explained why things happened. But there were limits to what could be expected. The scientist must set himself

limited, but possible, objectives. Entirely in keeping with this approach Salviati is made to say:-

"The present does not seem to be the proper time to investigate the cause of the acceleration of natural motion concerning which various opinions have been expressed by various philosophers all these fantasies, and others too, ought to be examined; but it is not really worthwhile. At present it is the purpose of our Author merely to investigate and to demonstrate some of the properties of accelerated motion (whatever the cause of this acceleration may be)." 37

In areas where we cannot establish our conclusions, Galileo maintained, it is better to reserve our judgement. But this does not rule out the possibility of conclusions not yet known one day becoming so. His discoveries relating to acceleration, Galileo said, open the door "to a new method fraught with numerous and wonderful results which in future years will command the attention of other minds."³⁸

Galileo's optimism, his belief in the autonomy of science, combined with a recognition of its limits, were all expressed by him in a letter to the Grand Duchess Christina of Tuscany. In 1615 Galileo wrote to her in defence of his belief in the Copernican system. He said:-

".... among physical propositions there are some with regard to which all human science and reason cannot supply more than a plausible opinion and a probable conjecture in place of a sure and demonstrated knowledge; for example, whether the stars are animate. Then there are other propositions of which we have (or may confidently expect) positive assurances through experiments, long observation, and rigorous demonstration; for example, whether or not the earth and the heavens move, and whether or not the heavens are spherical.

As to the first sort of propositions, I have no doubt that where human reasoning cannot reach - and consequently where we have no science but only opinion and faith - it is necessary in piety to comply absolutely with the strict sense of Scripture. But as to the other kind, I should think that first we are to make certain of the fact, which will reveal to us the true senses of the Bible, and these will most certainly be found to agree with the proved fact (even though at first the words sounded otherwise), for two truths can never contradict each other." 39

Galileo believed that science could arrive at truth, and that the scientist, not the theologian, was the best judge of when that truth had been discovered. Science, Galileo maintained, was not subservient to other disciplines. But science cannot supply the answer to every question. Human reason has its limitations.

Conclusions

I have argued that much of Galileo's thought can only be understood in terms of the Neoplatonic tradition of which he was undoubtedly a part. Central to Neoplatonism in science was the view that the cosmos was created by the Great Mathematician. The form of nature was the form of mathematics. To identify the mathematical form was to identify the causes of the phenomena. This conception of the relationship between mathematics and the physical world was not one that was justified empirically - although the successes of the scientists undoubtedly confirmed their views. Rather, it arose from the faith of scientists in the role of number and geometry in the universe.

The Neoplatonists believed that the laws of nature ordained by God must exhibit the most perfect form. The most perfect form must be

the most perfect form mathematically. Therefore God, by his nature, could not produce a universe other than one exhibiting the most perfect mathematical structure. Contingent alternatives to its actual structure could not exist on pain of denying God's perfection. As a consequence of this it was very natural for the Neoplatonists to believe that their discoveries, the laws of nature expressed in mathematical form, were not simple contingent facts about the world. They were eternally necessary statements about God's creation. This attitude is clearly one which rests upon much more general considerations than simply empirical evidence. Indeed, I have argued, that general cosmological considerations were fundamental to the whole of the Neoplatonic approach to nature.

We have seen that this tradition relied predominantly on cosmological considerations, simple empirical confirmation, and an appeal to order and simplicity. There was little attention given to purely inductive criteria to justify the acceptance of a particular law as being true. And the reason for this is not hard to understand. If you can establish that a law of nature, or form, must be as it is, there is no need to rely on inductive evidence to support it. Inductive criteria only become important when you recognise that there are no grounds for assenting to the necessity of a law on the basis of cosmological considerations. The problem of induction, as it is classically conceived, only becomes a problem when other criteria to justify a general conclusion about the behaviour of physical objects are found unacceptable. This is not to say that, for example, the

repetition of experiments was completely ruled out by the Neoplatonists. Galileo claimed that he repeated an experiment "a full hundred times".⁴⁰ But the role of experiment was not to show that the events always occurred. Rather, it was to show that an accurate result had been achieved.

The Neoplatonists were committed to the discovery of truth. They rejected completely the implied programme of Osiander that they were concerned only with 'saving the appearances'. The emphasis on discovery was handed on from Galileo to his successors. Though not always accepted, it was an important inheritance. The claim that truth can be reached by scientific means forces the question: how do you know when you have reached truth? It raises questions about the limits of human understanding.

Galileo's commitment to the distinction between primary and secondary qualities raised further epistemological problems for, implicitly at least, it raised the question of the existence of the external world. If we can only be aware of the secondary qualities, themselves only sensations, how are we to justify our belief in the existence of an external world at all? Implicit in Galileo's science was the choice between Cartesian dualism or Berkeley's idealism. Furthermore Galileo's distinction itself drew attention to the role of experience in acquiring knowledge.

All these aspects, and others, of the early days of the new science were to be the subject of further re-evaluation for the rest of the century. Galileo, more than anybody else, placed before the world examples of the success of the scientist's work, and exhibited its potential.

Chapter III

Cartesian Rationalism

Introduction.

Descartes' role in the scientific and philosophical revolutions of the seventeenth century was of the utmost importance. His work in mechanics, mathematics, and optics was outstanding. His metaphysics and epistemology were the foundation for Spinoza, Leibniz, and in some important respects, Locke. It was Descartes who established the mechanistic conception of nature in modern Europe. It was Descartes who supplied the basic categories in terms of which epistemological questions were pursued until the contemporary era.

Yet, somewhat paradoxically, many of his specific views were soon to be rejected. By the beginning of the eighteenth century it was not Cartesian rationalism, but Lockian empiricism which was most influential in European philosophy; it was not Cartesian, but Newtonian, mechanics which was accepted most widely. But it is some indication of Descartes' stature that it required two men of genius to overthrow and replace his theories of matter, motion, and knowledge.

Fundamental to the whole of Cartesian science and philosophy, as Descartes constantly emphasised, was his theory of method. Descartes had discovered "the method of rightly conducting the reason and seeking for truth in the sciences". If Cartesian science and Cartesian philosophy were to go, it was likely that it would be because of a rejection of its method. Indeed it was largely this that produced its downfall.¹ It is, therefore, largely with

Cartesian method, and its limitations, that this chapter is concerned.

The influence of Descartes on Newton and Locke was considerable.² Thus Newton, for example, was directly influenced in the conception, as well as the formulation, of his first law of motion by Descartes' Principles of Philosophy.³ Although Locke was hardly forward in acknowledging his debts to other thinkers in his published works, he did admit that it was reading Descartes which first gave him 'a relish in philosophical things'.⁴

Descartes, then, is important for understanding the connection between Locke's Essay and the scientific revolution. And this is not simply because of the general importance of his theories, but also because of his direct influence on Locke and his contemporaries. The exact extent of that influence is more problematic. In general, I believe, there has been a tendency to overestimate the Cartesian influence on Locke's epistemology.⁵

One of the most significant aspects of Descartes' system is that it was the first large-scale attempt since Aristotle to produce a unified account of knowledge: a metaphysics and epistemology which generated a physics. It is important, therefore, to see Descartes' work as a whole. Too often, especially in teaching him, Descartes is seen as either an epistemologist or as a scientist (though generally the former), with the consequence that the full power of his system is unfairly underplayed.

Despite its many merits we shall find that Cartesian theory did not live up to its pretensions. The method Descartes evolved was inadequate to the stated task. It was very much as a response to these inadequacies that Empiricism, as an alternative, came into its own.

In this chapter I shall make some reference to Descartes' younger contemporary and disciple, Rohault. Rohault represented most clearly the continuation of the Cartesian tradition in physics; but he introduced important modifications from Descartes' own position as to the possibilities of physical science and the status of scientific discovery. He was, too, one of the most influential of Cartesians in the period immediately preceding the publication of Locke's Essay.

Cartesian Method.

We have seen that Galileo believed in the possibility of obtaining knowledge in the natural sciences. He was also, in a modest degree, an epistemologist, as well as a mathematician and physicist. In all these interests Descartes followed him. But in one vital respect Descartes differed from Galileo. Galileo never attempted to present his account of the world as a deductive system. Indeed he often failed to expand fully his basic premises and relate them to the method he actually employed. Although Galileo was a mathematical physicist, he presented his work largely in the form of dialogues. Descartes, in contrast, presented most of his work in deductive form. He is the disciple of Euclid rather than Plato. Descartes himself explained the difference between the two of them

in a letter he wrote to Mersenne in 1638:-

"I will begin this letter with my observations on Galileo's book. 6 In general I find that he philosophises much better than the average, in that he abandons as far as he can the errors of the Schools, and tries to examine physical matters by the methods of mathematics. In this I entirely agree with him, and I believe that in no other way can truth be found. But it seems to me that he suffers greatly from continual digressions, and that he does not stop to explain all that is relevant to each issue, which shows that he has not examined them in order, and that without considering the first causes in nature, he has only looked for the reasons of various particular effects, and thus built without foundation." 7

And as an example:- "As to what Galileo has written on the balance and the lever, he explains very well what happens but not why it happens, as I have done by my Principles." 8

The quotation is significant on many counts. Descartes welcomed Galileo's discovery of regularity in nature. But to discover regularities was in itself, Descartes held, to explain nothing. What was required was some account of why those regularities do occur. And the answer which Descartes required was one founded on certainty, not conjecture; an answer which can be seen as necessarily so, not merely contingently likely. He was committed to the view that explanations in the physical sciences must be 'complete' in the same sense as a theorem in Euclidean geometry can be completely explained by reference to the rest of the system, and its derivation from the initial axioms.⁹

About the status of his axioms Descartes was quite clear: they were true. He explained the reason for his conviction in the Author's letter to the Principles of Philosophy:-

"After having made these matters very clear, I should have desired to set forth the reasons which serve to prove that the true principles by which we may arrive at the highest point of wisdom in which the sovereign good of the life of man consists, are those which are put forward in this book. And only two are requisite for that, the first that the principles must be very clear, and the second that from them we may deduce all other things; for there are but these two conditions that are essential to true principles." 10

What exactly did these two conditions mean for Descartes? And was he correct to see them as justifying the weight he placed upon them? To consider these two points we must understand what Descartes meant by "clear and distinct ideas", and his conception of deduction.

Clear and Distinct Ideas.

In the third Meditation Descartes explained why he accepted clear and distinct ideas as his criterion for truth. He wrote:-

"I am certain that I am a thing which thinks; but do I not likewise know what is requisite to render me certain of a truth? Certainly in this first knowledge there is nothing that assures me of its truth, excepting the clear and distinct perception of that which I state, which would not indeed suffice to assure me that what I say is true, if it could every happen that a thing which I conceived so clearly and distinctly could be false; and accordingly it seems to me that already I can establish as a general rule that all things which I perceive very clearly and very distinctly are true." 11

Vital to an understanding of Descartes' whole philosophy is the recognition of the fact that the criterion for truth which Descartes employed is a psychological, no a logical one. Although it is "the natural light of reason" which informs us of the truth of the propositions so accepted by Descartes, reason itself is not subject to logical, but psychological, criteria. No doubt Descartes' reluctance to employ logical criteria is in part explained by his

opposition to syllogistic inference, which Descartes held only made explicit what was already implicitly conceded in the premises. No doubt, also, it could be explained partially in terms of the peculiarity of his first discovered 'truth', the certainty of 'I think therefore I am', a proposition which is not itself logically necessary, but which must be true if it is ever asserted by anybody. But it is not here my intention to explore this question, though further exploration it certainly needs.¹²

Because Descartes used a psychological criterion for truth he was led into grave difficulties in his account of mathematics, and in his account of logically necessary propositions generally, as we shall see.

Descartes held that those propositions which are recognised as clear and distinct, are so recognised by an intellectual intuition which is itself impervious to further analysis. All propositions, whether necessary or contingent, which exhibit this clarity are beyond doubt, and are certainly true.

It can, and has been, argued that the recognition that a proposition is beyond doubt does not in itself establish that the proposition is certainly true, only that it cannot be doubted. It has further been argued that this was a point which Descartes recognised.¹³ But if he ever did, he certainly did not let it worry him very much: the French edition of the Principles of Philosophy, prepared just before his death, asserts in the passage which has been quoted above (p.60, top) that clarity is a sufficient condition for truth.

Descartes would have probably argued that clarity is sufficient for truth once we know that God exists. And certainly if it is true that God does exist, and also true that he would not deceive us if we use our intellectual faculties properly, and if to use our intellectual faculties properly is to accept only as true propositions that we clearly and distinctly perceive, then clarity and distinctness are sufficient grounds for asserting truth. But, notoriously, God's existence, etc., can only be known to be true if we already have a criterion of truth.¹⁴

Noting but not pursuing these difficulties at the heart of Descartes' system, we can notice further that knowledge is for Descartes something entirely within the intellectual consciousness. To apprehend a clear and distinct idea is to have knowledge, is to comprehend the truth. Nor is this apprehension confined to hypothetical knowledge. Descartes did not say "if I think, then I must exist", nor did he say "if God is an intelligible concept, then it must be substantiated". The first two premises of Descartes' system are unequivocally categorical and existential: a thinking substance exists, God exists.

Given the existence of self, given the existence of God, given the criterion of truth, Descartes saw himself ready to proceed. From then he went on to show that "there are bodies extended in length, breadth, and depth, which have diverse figures and move in diverse ways."¹⁵

The External World.

Given the existence of God, Descartes argued, it follows that our belief in the external world is justified. We are aware that there is something "extended in length, breadth and depth, and possessing all those properties which we clearly perceive to pertain to extended objects."¹⁶

There is an external world. And Descartes held that its properties are to be discovered not by observation, but by an intellectual process: by applying the criterion of clarity to our concept of a three-dimensional object. Like Galileo, Descartes held that it was the intellect that led us to a knowledge of the nature of matter.

When we apply Descartes' criterion we find, not surprisingly, that body has only those properties which characterise three-dimensional space. Not surprisingly, because if one attempts to have a clear and distinct idea of an object extended in length, breadth, and depth, and no other properties, which is what Descartes asks us to do, then it is not very extraordinary that these are all the properties that we assign, or can assign, to it. We could equally have had a clear and distinct idea of an object with the three-dimensional properties plus weight - or any other compatible property - and that too would have as equal a claim to be "the nature of body". Descartes' justification for assigning body the properties he does is completely circular. Why did he make such an error?

I believe that the explanation lies in Descartes' total commitment to the mathematisation of mechanics. It is an excellent illustration of the way in which Descartes' physics and metaphysics were interdependent. Unless matter and space could be identified, the possibility of subjecting all phenomena to geometrical analysis would stand in jeopardy. If matter had other than geometrical properties, then mechanics could not be reduced to mathematics.

Objection to the Cartesian identification of matter and space was made very forcefully by Descartes' contemporaries.

Descartes' answer to them is very revealing:

"Several excellent thinkers, say (your friends), believe they see clearly that Mathematical extension, which I point as the principle of my Physics, is nothing other than my own thought, and that it has, and can have, no other subsistence outside my mind, being only an abstraction that I make of physical body; and in consequence, that all my Physics can be only imaginary and feigned; as are all pure Mathematics; and that, in the real Physics of things that God has created, a real, solid and not imaginary matter is necessary. That is the objection of objections, and the abridged form of all the doctrine of these excellent minds here cited. All the things we can understand and conceive are only, according to them, imaginations and fictions of our mind, which have no subsistence: from which it follows that there is nothing that one can in any way understand, or conceive, or imagine, that can be admitted as true, that is to say that the door to reason is completely shut, and one must content oneself with being a Monkey, or a Parrot, and no longer a Man, in order to merit being ranked with these excellent minds. For if the things that can be conceived must be judged false for the simple reason that they can be conceived, what remains, if not that only those must be accepted as true that are not conceived, and from the basis of one's doctrine, imitating others without knowing why one imitates them, as Monkeys do, and in preferring only the words the sense of which is not understood, as Parrots do? But I have something to console myself with, because my Physics is here joined to pure Mathematics, which I particularly wish it to resemble." 17

The paragraph is a classic example of Rationalist dogmatism. The sophisticated nature of Descartes' rhetoric seems to suggest that the objections bit deep. Descartes presents no argument in refutation, except, significantly, the appeal to mathematical resemblance. Mathematics, Descartes believed, must be the key to physics, and to reject his account of matter is to reject that programme, a price which Descartes was quite unwilling to pay. The passage also hints at another aspect of Descartes' system. It suggests that Descartes assessed his system, not only - or even not at all - in terms of its truth, but for its explanatory power. This point we shall explore below.

The identification of matter with space supplied Descartes with the basis for his distinction between primary and secondary qualities, though Descartes never actually used that terminology. Once again, in justifying the distinction, Descartes' argument is unsound. Weight, hardness, and colour, Descartes said, are not part of body in its universal aspect. He argued:-

"For as regards hardness we do not know anything of it by sense, excepting that the positions of the hard bodies resist the motion of our hands when they come in contact with them; but if whenever we moved our hands in some direction, all the bodies in that part retreated with the same velocity as our hands approached them, we should never feel hardness; and yet we have no reason to believe that the bodies which recede in this way would on this account lose what makes them bodies. It follows from this that the nature of body does not consist in hardness." 18

All that Descartes' argument in fact establishes is that it is only a contingent fact that we have discovered hardness to be

a property of material objects. As it is a contingent fact, we might not have discovered it. But that we might not have ever attributed hardness to body is in itself no reason, one way or the other, for deciding that hardness is or is not a property of body.

The same, or similar, arguments, Descartes claimed, establish that "weight, colour, and all the other qualities of the kind that is perceived in corporeal matter may be taken from it".¹⁹ And all such arguments are equally fallacious. The basis of Descartes' system of the external world rests squarely on confusion.

Descartes' was totally committed to the view that "the simple natures" of all substances could come to be known by his intellectual method. "... there is always one principal property of substance" he wrote, "which constitutes its nature and essence and on which all the others depend."²⁰ Descartes was thus diametrically opposed to any view which held that essences were unknowable. Knowledge of the essences of mind, matter, and motion, were all Cartesian claims upon which his whole system of the external world was based. All of this knowledge, Descartes held, was gained by intellectual, not empirical, investigation.

Necessity and Contingency in Descartes' System.

As we have seen, the certainty of the intuited clear and distinct ideas of the Cartesian system did not depend upon their logical necessity. But this is only one aspect, the tip of the iceberg in fact, of Descartes' conception of the lack of connection between certainty and logical necessity. As Emile Brehier has

pointed out,²¹ Leibniz wrote in the Monadology (Section 46):-

"We must not, however, imagine, as some do, that because the eternal truths are dependent on God, they are therefore arbitrary and depend on His will, as Descartes, and after him M. Poiret, seem to have thought. This is true only of contingent truths, whose principle is fitness or the choice of the best; whereas necessary truths depend solely on His understanding, of which they are the internal object."

Leibniz was right to believe that Descartes held the eternal truths to have been created by God. In his reply to the Sixth Objection to the Meditations Descartes made the point very clearly.²² To suggest that the eternal truths existed from all eternity unchangeable, Descartes argued, would place a limitation on God's choice. But this is impossible. Rather, he held, it is because God wills the three angles of a triangle necessarily to equal two right angles that they do so necessarily equal two right angles. It was God's choice which made certain propositions necessary, not necessity which dictated God's choice.

This explains why it was possible for Descartes to imagine, in the Meditations, that an Evil Genius who could do anything, could be deceiving us about the truths of mathematics or the possibility of the existence of a mountain without a valley.²³ If, that is, the eternal truths were the product of a will, then they could be otherwise, and those that I take to be necessary truths, could in fact, given the Demon, be false.

Once the demon is vanquished we do not have to doubt that the eternal truths are or could be false. God's known existence guarantees their certainty. But the thesis that the eternal truths

have been created leaves Cartesian method with certain chronic problems. If all truths are equally dependent on God's will, then all have an equal status. There is no room within Descartes' system for a distinction between contingent and necessary truth, even though Descartes himself sometimes makes use of such a distinction.²⁴ The truths of mathematics and the truths of physics are entirely on a level; all contingent on the will of God; all necessary, given that God has so decided. Thus Descartes was able to write to Mersenne:-

"As to physics, I should consider I knew nothing about them were I only able to explain how things might be, and were unable to demonstrate that they could not be otherwise. For having reduced physics to the laws of mathematics, such demonstration is possible." 25

The effect then of Descartes' views on the creation of the eternal truths was to produce confusion about the distinction between the necessary and the contingent. It was a confusion which ran through much of Descartes' own work, and it was to re-appear in the work of later thinkers. But it is important to see that the confusion in Descartes had an entirely different basis from the similar confusion which we observed in Galileo and the Neoplatonists. Whereas the necessity which the latter attributed to nature arose from consideration of God's perfection, and his creation of the world according to the forms of Euclidean geometry, for Descartes the basis of the necessity which existed in nature arose directly from the total freedom of God, and his criterion of clear and

distinct ideas for truth, a criterion which itself confused and conflated the necessary and the contingent.

The Role of Experience in Descartes' System.

Descartes was never opposed to experiment in the physical sciences, and he expressed much admiration for the experimental methodology of Bacon. But he held that experiment in itself gave no knowledge. It is only from the relation between experiment and the whole system of the Principles of Philosophy is it possible to extract knowledge. As Descartes explained it:-

"The principles we have found are so vast and fruitful that there follow from them many more things than we see to be contained in this visible world; indeed many more things than lie even in the power of our mind to traverse in thought. Let us set before ourselves a short history of the principle phenomena of nature (the causes of which are to be investigated here); not however that we should use them as if for reason to prove anything; for our desire is to deduce the reasons of the effects from causes, not contrawise those of causes from effects; but only in order that, out of the innumerable effects which we judge can be produced from the same causes, we may determine our mind to be consideration of certain among them rather than others." 26

Observation is necessary to find out what has to be explained. But observation itself, Descartes maintained, can explain nothing. Explanation and knowledge only occur when the data is shown to be explicable by the Principles. As he explained to Mersenne:-

"You ask me if I hold what I have written on refraction to be a demonstration; I think it is, at least as far as it is possible to give one in this matter, without having previously demonstrated the principles of Physics by Metaphysics (which I hope one day to do) 27.

This letter, written in 1638, anticipated the putative success of the Principles of Philosophy published six years later.

The role of experience in Cartesian method was the direct antithesis of that to be proposed by Newton. The whole approach of the Newtonian method placed total weight on experiment and observation. General propositions, Newton was to argue, must be based only on observation.²⁸ Descartes, in contrast, relegated observations to an entirely subordinate role. Knowledge, he maintained, could only arise from knowledge of principles known with certainty by his criterion of truth. Explanation consisted in showing how particular phenomena were related to those general principles within the total system.

Science and Knowledge.

Descartes was justly proud of the explanatory power which his total system generated. Thus he wrote in the Discourse on the Method:-

"In subsequently passing over in my mind all the objects which have ever been presented to my senses, I can truly venture to say that I have not there observed anything which I could not easily explain by the principles which I have discovered." 29

But in what sense, if any, did the total system produce true explanations of the phenomena? On this question Descartes' position often appears to be strangely ambivalent. On the one hand he claimed that the method led step by step from one certainty to the next, implying that truth percolates through the whole system. On the other hand, Descartes often maintained that he had

made use of explanatory fictions, false hypotheses, to account for phenomena,³⁰ but that this was justified because of the explanatory power of the whole system.

It might be thought that Descartes' references to 'false hypotheses' is just a front; fear of the treatment meted out to Galileo, it might be suggested, encouraged Descartes to believe that discretion was the better part of valour; undoubtedly Descartes was much troubled by the theological implications of many of his views. But because the problem runs through parts of Descartes' work which have only the most distant connections with any theological questions, this explanation seems inadequate. The truth seems rather that Descartes changed his criterion for the acceptability of the truth of propositions. Descartes lowered his standards from the high ones of clear and distinct ideas, to the comparatively low one of explanatory power, with the question of knowledge and truth per se receding from the central position of being the major objective of the whole enterprise. Descartes, in much of his physics, is more concerned with being able to explain phenomena than with the question of whether or not the explanations are true.³¹

Descartes' attitude is illustrated by a section at the end of the Principles of Philosophy. We find Descartes describing "how we may arrive at knowledge of the figures, magnitudes, and motions of the insensible particles of bodies." Knowledge, not probable knowledge, can be obtained, Descartes argued; the method,

basically, is by analogy. The passage is rather long, but worth quoting in full:-

"But since I assign determinate figures, magnitudes and motions to the insensible particles of bodies, as if I had seen them, whereas I admit they do not fall under the senses, someone will perhaps demand how I have come to my knowledge of them. To this I reply that I first considered generally the most simple and best understood principles implanted in our understanding by nature, and examined the principal differences that could be found between the magnitudes, figures and situations of bodies insensible on account of their smallness alone, and what sensible effects could be produced by the various ways in which they impinge on one another. And finally, when I found like effects in the bodies perceived by our sense, I considered that they might have been produced from a similar concourse of such bodies especially as no other mode of explaining them could be suggested. And for this end the example of certain bodies made by art was of service to me, for I can see no difference between these and natural bodies, excepting that the effects of machines depend for the most part on the operation of certain instruments, which, since men necessarily make them, must always be large enough to be capable of being easily perceived by the senses. The effects of natural causes, on the other hand, almost always depend on certain organs minute enough to escape every sense. And it is certain that there are no rules in mechanics which do not hold good in physics, of which mechanics forms a part or species (so that all that is artificial is also natural); for it is not less natural for a clock, made of the requisite number of wheels, to indicate the hours, than for a tree which has sprung from this or that seed, to produce a particular fruit. Accordingly, just as those who apply themselves to the consideration of automata, when they know the use of a certain machine and see some of its parts, easily infer from these the manner in which others which they have not seen are made, so from considering the sensible effects and parts of natural bodies, I have endeavoured to discover the nature of the imperceptible causes and insensible parts contained in them." 32

Typical of much of Descartes' science, the initial claim to knowledge appears partially withdrawn in the last sentence.

The objections to this form of inference as a way to obtain certain knowledge were to be made in the seventeenth century by

many thinkers, including Glanvill, Boyle, Newton and Locke. The most important objection is that explanations by analogy cannot give us certain knowledge unless we can, independently, examine both that which is explained and the object with which it is compared. I take a simple example. I may wish to explain cricket to an American. Knowing his familiarity with baseball, and also myself knowing baseball, I explain cricket by analogy with baseball. As a result, the American comes to understand some of the more obvious points about cricket. But he cannot infer from some feature of baseball to a corresponding feature of cricket unless he can check that inference, for example by asking me. "Can you be out at cricket by being caught?" he asks. "Yes", I reply. In that respect the analogy holds. "Must the pitcher throw the ball full-toss to the batsman?" "No," I reply. Here the analogy breaks down. There is no way of establishing how far the analogy holds a priori. There is no other test than empirical observation.

In the example that Descartes has offered, between the mechanical workings of a clock and the hidden mechanisms of nature, ex hypothesi there is no possibility of anybody being able to observe both sets of mechanisms, and so there is no way of knowing, (as opposed to supposing) that there is such an analogy. Descartes was in fact aware of this important limitation. Thus he wrote:-

"But here it may be said that although I have shown how all natural things can be formed, we have no right to conclude on this account that they were produced by these causes. For just as there may be two clocks made by the same workman, which though they indicate the time equally

and are externally in all respects similar, yet in nowise resemble one another in the composition of their wheels, so doubtless there is an infinity of different ways in which all things that we see could be formed by the great Artificer." 33

Descartes explained that he would be fully satisfied if "the causes I have assigned are such that they correspond to all the phenomena manifested by nature without inquiring whether it is by their means or others that they are produced." 34

We have a moral certainty of the truth of his findings, Descartes maintained, because of the total coherence of the whole system:-

"But they who observe how many things regarding the magnet, fire, and the fabric of the whole world, are here deduced from a very small number of principles, although they considered that I had taken up these principles at random and without good grounds, they could yet acknowledge that it could hardly happen that so much could be coherent if they were false." 35

Yet of course they could be. Indeed some of them were false. Had not Descartes admitted as much himself in proposing to accept false hypotheses? We can see, then, when we turn to the detailed application of the physics, that Descartes used an entirely different criterion of acceptability from the one proposed in the Discourse on the Method. Descartes no longer claimed to establish the fáctúal cértainty of his conclusions by an appeal to their clarity and distinctness. Rather it is in terms, and almost solely in terms, of their explanatory power.

The penalty for this move is severe. Descartes cannot claim certain truth for his principles of the physical world. Theory has become heuristic. It was a penalty that Descartes was extremely reluctant to pay. He attempted to reject the charge against his science by once again asserting the general grounds for the acceptability of his philosophy:-

"as God is supremely good and cannot err, the faculty which He has given us of distinguishing truth from falsehood, cannot be fallacious as long as we use it aright, and distinctly perceive anything by it." 36

Descartes, in his discussions of our knowledge of the physical world, vacillated between, on the one hand, dogmatically asserting the truth of his conclusions because they appeared clear and distinct (to him), and on the other, acknowledging that even if his conclusions could not be accepted as certainly true, they at least explained a lot more than other people's theories. Only the latter claim was certainly justified.

Rohault's Scientific Method.

No such wavering was exhibited by Descartes' disciple Jacques Rohault. On the question of certainty in scientific knowledge Rohault was unequivocally committed to the conjectural nature of scientific discovery.

It is worth considering Rohault's comments on scientific method in general for a number of reasons. Among such considerations are that he was widely read in Cambridge,³⁷ Locke when in France read him,³⁸ and most important of all, Rohault's method shows clearly that Descartes' influence on him was not towards an a priori

Rationalist science. Rohault made full use of Descartes' method and discoveries, but he dropped all claims to certainty in his findings, indicating that Cartesian physics was accepted largely for its explanatory power, rather than its agreed truth. Given this, Descartes' importance as a scientist was bound to diminish if and when a more powerful system of physics could be discovered.³⁹ With the publication of Newton's Principia, for many, that moment arrived.

Rohault steered a course between dogmatic rationalism and uncontrolled and unordered experiment. In the Author's Preface to his Treatise on Physics (1671) he wrote:-

"A third defect I have found in the method of philosophers is that some of them are wholly for reasoning, and depend so much upon the strength of their arguments (especially if they are borrowed from the Ancients) that they judge it superfluous to make any experiments." 40

Others, he said, "think everything ought to be reduced to experiment, and that there should be no reasoning at all."

Both methods are equally faulty. The only correct method, Rohault recognised, was to combine reason and experiment. He went on to describe his version of the hypothetico-deductive method:-

"The third sort of Experiments are those which are made in Consequence of some Reasoning in order to discover whether it was just or not. As when after having considered the ordinary Effects of any particular Subject, and found a true Idea of the Nature of it, that is, of That in it which makes it capable of producing those Effects: we come to know by our Reasoning, that if what we believe concerning the Nature of it be true, it must necessarily be, that by disposing it after a certain Manner, a new Effect will be produced, which we did not before think of; and in order to see if this Reasoning holds good, we dispose the Subject in such a manner as we believe it ought to be disposed in Order to produce such an Effect." 41

The Cartesian Rohault, in fact, propounded just that scientific method utilized by Newton.⁴² Rohault was equally clear about the status of scientific enquiry:-

"If that which we fix upon, to explain the particular Nature of anything, do not account clearly and plainly for every Property of that Thing, or if it be evidently contradicted by any one Experiment; then we are to look upon our Conjecture as false; but if it perfectly agrees with all the Properties of the Thing, then we may esteem it well grounded, and it may pass for very probable." 43

Furthermore, Rohault accepted that many causes could produce the same effects:-

"Thus we must content ourselves for the most part, to find out how Things may be; without pretending to come to a certain Knowledge and Determination of what they really are; for there may possibly be different Causes capable of producing the same Effect which we have no means of explaining." 44

Thus we find that the 'Cartesian' Rohault held a position with regard to the certainty possible in the natural sciences entirely different from that put forward by Descartes in his metaphysics. Descartes had maintained that we can discover independently the causes of phenomena by the method of clear and distinct ideas, developed from the most simple to the most complex. No such possibility is held open by Rohault. We observe the effects, and can at best only offer tentative hypotheses as to their causes; hypotheses which we may have to abandon at a later stage, and which we ought to be willing to abandon in the light of empirical falsification.

It is remarkable that such an attitude should emerge from such a source at such a time. It suggests that the method of the Meditations was not in detail accepted by Descartes' scientific followers, that the victory of empiricism in opposition to the high priori road for

science was in large measure achieved within the framework of Cartesian physics itself. One is reminded that a Cartesian, Christian Huygens, was singled out by Locke as one of the masterbuilders of the age.

Rohault did not expect science to give certainty.⁴⁵ But he was no Osiander. He did not believe that truth was unobtainable in science, that discovery was not the goal. Rohault denied the sceptics charge that knowledge was impossible. If an explanation accounts for many different properties, Rohault argued, "we shall find it very difficult to believe that they can be explained in two different ways. In which case our conjecture is not only to be looked upon as highly probable but we have reason to believe it to be the Truth."⁴⁶ Thus Rohault claimed that explanatory power should itself be a criterion of truth.

In making this suggestion Rohault set the scene for a whole new conception of knowledge, a conception which was by no means immediately accepted. Put simply, what Rohault asked us to do is to be willing to predicate "is true" of propositions which it is agreed could be falsified by further experience. In so doing he was clearly breaking with the Cartesian ideal. For Descartes, truth and certainty meet in such a way that the possibility of doubting what is accepted as true, of conceiving its falsity, is strenuously excluded.

In opening this door, in rejecting the necessity of absolute certainty as a requisite for knowledge, Rohault cut himself away from his past, and performed a service for science and knowledge. However

it was not a move that was easily accepted. The spectre of absolute certainty as the criterion of truth was to haunt philosophy for a long time to come, even if the scientists were in practice to abandon it.

Rohault's position illustrates very clearly how the impact of the new science was producing a crisis in epistemology. He, a scientist, was calling for a redefinition of knowledge and truth. Yet no philosopher was at hand to supply that redefinition. What was required was a rethinking of the nature of knowledge, a statement of its possible forms, and a justification of the scientist's method. Although not all of these tasks were to be achieved immediately, an important start to their solution was already under way when the Traité de Physique was published. It was at this time that Locke was beginning the Essay.

Rohault differed from Descartes in another important respect. He did not claim for his physics the sort of comprehensiveness that Descartes accepted for his. Thus, he argued that just "because we cannot upon the spot explain by it [the conjecture] a property, which appears from some new experiment, or which we before did not think of" the probability of the conjecture is not lost, "For it is one thing to know certainly, that a conjecture is contrary to experience; and another thing not to see how it agrees to it, for although we do not all see the agreement, it does not from thence follow that it is repugnant." ⁴⁷ Implicitly at least, Rohault was committed to a piecemeal programme of discovery in science. Neither general principles

nor particular hypotheses can be accepted or rejected on a priori grounds. It is how far particular principles or hypotheses stand up to the acid test of experiment which is crucial.

In many ways the passages from Rohault which I have so far chosen to comment on show him in too modern a light. There were many aspects of Rohault's work, including his actual practise in science, which were much closer to Descartes than might be supposed from what we have so far considered. For example, he argued, in his discussion of light, for its instantaneous propagation in a very Cartesian fashion. But again, unlike Descartes, he recognised the conjectural nature of his thesis.⁴⁸

Again, when arguing for a distinction between primary and secondary qualities there are important Cartesian overtones, and Rohault's remarks on this topic are worth considering for they clearly express the mood in which the whole discussion of primary and secondary qualities was set in the second half of the seventeenth century. He argued that those who attribute heat as a real property of fire are guilty of an unsubstantiated inference, which is a result of their uncritical assent to the evidence of their senses. Thus he wrote:-

"They deceive themselves ... who, in order to prove that there is in the fire something, I know not what, like that heat which it excites in us, bid us go near it and try. Now, though we go near it a thousand times, nay, though we were scoured by it, all that this demonstrates is only what the fire does to us, and not what it is in itself. When we speak therefore of the Heat, or Cold, or Smells, or Sounds, or Light, or Colours, of bodies, to say that they are really things which are properly objects of our senses, is a great mistake. For he who says

this, must imagine that we come to the knowledge of them by bare sensation only, which is absolutely false." 49

The implication is that we have independent a priori reasons for drawing a distinction between the properties of objects, though Rohault also thought that standard cases of illusion were good empirical evidence for wishing to draw a distinction.

In many ways, then, we can see that Rohault remained a Cartesian. But, on the vital question of the possibility of the Cartesian programme being carried through in all its a priori glory, he was not. Many of his views about the nature and limitation of scientific method were to be taken further by thinkers who followed him, as we shall see.

Conclusions.

We have seen that Descartes' attempt to place knowledge on firm foundations failed on several counts. His epistemology and metaphysics relied too heavily on his criterion of clear and distinct ideas which itself generated confusion about the status of necessary and contingent propositions. Even Descartes himself was unsure how far his criterion could take him along the path to knowledge in the physical sciences. In fact it could take him very little way as Rohault recognised. There is not, nor can there be but one criterion for truth. There are at least two such criteria. They are that a proposition is true if either it is logically necessary, or, it describes a state of affairs which actually pertains, in which case the usual method of establishing this is by observation. In neither case is it simply a matter of psychological certainty.

But for all its faults, the Cartesian programme was certainly worth attempting. Descartes' commitment to the possibility of a unified system of knowledge, the view that all knowledge was one, with the implication that the same criterion could be used to test all cases of purported knowledge, forced men to consider how far such a programme was possible, and where it would break down. In short, the Cartesian programme was a stimulus. It encouraged the exploration of new paths, many of which were to lead not only to great achievements in the sciences, but also to a deeper understanding of knowledge and the limitations of science.

Descartes, too, had succeeded, with his sharp distinction between mind and matter, in affirming the belief that nature could be understood entirely by rational procedure. The physical world, created by God's intellect, was capable of being understood by man's intellect. After Descartes it seemed only a matter of time before all the secrets of the universe would be unlocked. Before him, such an aspiration was possible only when supported by a metaphysic of a very different order, such as we found in the work of the Neoplatonists.

Further, Descartes had changed once, and perhaps for all, the idea of what it was to understand the physical world. It was no longer necessary to seek mystic or moral order in its parts. What was required was to understand the cogs of a great machine. The teleology of Aristotelian science had been moved to a second order of abstraction.

This said, on qualification is in order. Descartes undoubtedly gave to mathematics a paradigmatic role of the highest order. The

attractions of this paradigm for a mathematical physicist are obvious. But the justification of the use of the paradigm is another matter. We have seen that the special place allocated to mathematics by the Neoplatonists was drawn from their conception of God as the great mathematician. It is perhaps not too far fetched to see some of that thinking in Descartes' work.

Chapter IV

Baconian Induction

Introduction.

Before we turn to consider the attitudes towards science and knowledge of Locke's scientific contemporaries we must look at the work of one of the greatest figures in the whole landscape of the scientific revolution, Francis Bacon.

Bacon's importance in either philosophy or science is a matter of considerable dispute. Some writers see it as difficult to over-estimate his contribution¹; for R. F. Jones "Sir Francis both expressed and moulded his age",² and Benjamin Farrington writes: "With Bacon we enter a new mental climate."³ Opposed to this high rating, it can be argued that Bacon was not a great original philosopher, and that he made no important discoveries in any of the natural sciences; because of these two rather major limitations he was of no great consequence, compared, say, to Galileo or Descartes.

The truth is that whilst Bacon was neither a philosopher of genius, nor a great working scientist, he did express a very new attitude to the possibility of obtaining knowledge of the physical world. Furthermore, some important parts of that attitude were to be accepted completely by the great scientists of the seventeenth century.

Before we turn to Bacon's contributions some explanation is in order as to why I have chosen to treat him after Descartes, when in fact all his great works were completed before any of Descartes'

were published. My reason is that in many respects already considered Descartes' work fits more naturally with the tradition in science which proceeded it than it does with the work of Bacon or the English scientists who followed him. From now on in our historical survey of attitudes to science we shall find that all the major figures drew more upon Bacon than any other single source; in short I did not wish to separate post-Baconian English science from its natural heritage.

Bacon's novel attitude towards the physical world anticipated Descartes' in important respects. His position can be summarised under three heads, the first two of which were also fundamental to the Cartesian programme:

1. Reject past teaching because it is either prejudiced, unsubstantiated, or both.
2. There is a method of investigation, open to man, which will lead to knowledge of nature.
3. This knowledge can be used for man's betterment.

In promising a programme which will lead to knowledge Bacon anticipated Descartes. But the programme he advocated was very different from Cartesian rationalism, for Bacon placed central emphasis on experiment, unfettered by preconceptions drawn from the mind. But in other respects his attitude had much in common not only with Descartes, but also with Galileo, for it was essentially a programme looking not to the past, but to the future. In this respect all three thinkers were remarkably different from the

tradition of Renaissance Humanism which largely saw itself as returning to the attitudes and achievements of the classical world. If Descartes gave mechanism to the modern world, then certainly Bacon gave optimism: a belief that man was not simply at the mercy of nature, but rather that man could master nature and make it his slave.

Bacon offered no detailed epistemological theory, and for this reason alone he cannot be considered the founder of the modern empirical tradition. But what he did was to make modern empiricism possible. If experience was, as Bacon maintained, the basis for knowledge of nature, why could it not be made the basis of knowledge in general? It was, indeed, the generalisation of the Baconian conception that we find in Locke's Essay at the end of the century.

Bacon's methodology for science can be, and has been, criticised on many counts. Science in general has not progressed according to the Baconian programme. Bacon never fully appreciated the importance of conjecture in science - or in fact ever appreciated the importance of theory at large. But his failings in this direction were in consequence more therapeutic than harmful. It encouraged concern with the particular, rather than the general, it curbed the flights of fancy which, as Bacon readily pointed out, had dominated man's concern with nature.

It has been held that Bacon's importance for the scientific revolution is not as great as is sometimes supposed because Bacon's conception of certainty was different from those of his successors.

Henry G. Van Leeuwen has argued that "though the problem he [Bacon] set out to resolve resembled that encountered by his successors, the solution he proposed was essentially different from theirs, and that consequently it was not Bacon's theory of knowledge which became the basis for empirical science and philosophy in seventeenth century England."⁵

This claim appears to me to rest on a false assumption. It was not the case that Bacon offered, in the relevant respects, any theory of knowledge at all. Bacon side-stepped the philosophical questions almost totally. In so doing he left the road open for such a theory to be propounded. It was just this vacuum that Locke's Essay was to fill.⁶ What Bacon did was to supply a practical method which, for all its faults, could be utilized to achieve scientific knowledge. How far the results could be construed as absolutely certain knowledge when subjected to philosophical scrutiny Bacon did not see fit to explore. In fact it was only when the method had been tried did it become obvious what the epistemological questions were which could be asked of it.

As with previous thinkers so far considered I shall not attempt to give a summary of all of Bacon's views in this chapter. Rather I shall try to point up those aspects of his thought which bear most directly on the questions of the limits of knowledge and the nature of the scientific enterprise.

Bacon, Theology and Science.

Bacon, even more explicitly than Galileo, divided human knowledge into autonomous sections. He believed that it was vital to progress in science that it be separated from questions of theology or morality. But for Bacon this did not mean that there were no common elements which appeared in all branches of knowledge, for the branches all had a common trunk. This Bacon called the Philosophia Prima which would consist of "all such axioms as are not peculiar to any of the particular sciences, but belong to several of them in common."⁷ Bacon offered as an example of such axioms: "if equals are added to unequals the result will be unequal", which he said held of both mathematics and distributive justice. It is not too inaccurate to think of Bacon's Philosophia Prima as covering roughly the subject of what we now call logic.

The three main branches of knowledge, Bacon believed, were of God, of Nature, and of Man. It is worth noticing that Bacon did not, in any important sense, regard these three branches as forming a hierarchy; just as with a tree one branch being above another does not, for that reason, make it a better branch, so knowledge of God, natural theology, is no better qua knowledge, than natural philosophy, or the philosophy of man. Given this, it was possible for Bacon to maintain that a man's religious beliefs were irrelevant to assessing his worth as a scientist. This is not to say that Bacon went so far as to maintain that an atheist could make a good scientist, rather all men, including scientists, must recognise their

mortal nature before God's majesty. But, as Marjorie Purver has shown⁸, Bacon was anxious that differences on religious grounds would not prevent Catholic and Protestant scientists working together.

Not only did religious belief not conflict with science, Bacon positively believed that it was of the utmost importance to separate science and theology. He wrote in the Novum Organum:-

"But the corruption of philosophy by superstition and an admixture of theology is far more widely spread and does the greatest harm, whether to entire systems or to their parts. For the human understanding is obnoxious to the influence of the imagination no less than to the influence of common notions. For the contentious and sophistical kind of philosophy ensnares the understanding; but this kind being fanciful and timid and half poetical, misleads it more by flattery. For there is in man an ambition of the understanding, no less than of the will, especially in high and lofty spirits."⁹

Theological preconceptions were likely to mislead the enquiring intellect, Bacon believed, as they had misled Pythagoras and Plato, and as they were currently misleading those who "attempt to found a system of natural philosophy on the first chapter of Genesis, on the book of Job, and other parts of the sacred writings, seeking for the dead among the living."¹⁰ This particular fault, Bacon held, is no less dangerous to theology as to natural philosophy, for it is likely to produce "a heretical religion". Natural philosophy, Bacon argued, has its own criteria for success, its own criteria for knowledge. To link it with other areas of human enquiry such as religion is likely to do harm to both.

Bacon and Gilbert.

One way in to an appreciation of Bacon's whole approach to nature is to look at his attitude to the Elizabethan scientist William Gilbert. It is often alleged that Bacon misunderstood Gilbert¹¹; it has even been suggested that Bacon did not read Gilbert's most influential work De Magnete.¹² But the truth is the other way around; it is Gilbert's supporters, and Bacon's detractors who have done the misunderstanding.

In the history of English science Gilbert provides the stepping-stone from the highly Neoplatonic doctrines of Dee to the anti-Platonic outlook of the Bacon of the Novum Organum. Although Gilbert was not a great mathematician (the one point, we will remember, on which Galileo faulted him)¹³ he was a great experimenter. Experiment, he believed, gave sure results. In the Preface to De Magnete (1600) he wrote:-

"in the discovery of secret things and in the investigation of hidden causes, stronger reasons are obtained from sure experiments and demonstrated arguments than from probable conjectures and the opinions of philosophical speculations of the common sort." 14

Gilbert, then, was committed to detailed examination of the particular before pretending to any general conclusions, an attitude one would expect to find in a doctor of medicine. Thus he argued that starting from detailed observations of the loadstone it would eventually be possible to learn truths about the earth, and indeed all the planets, though he did admit that in suggesting this he is allowing himself to "philosophize freely, as freely as in the past."

He compared his method to geometry in a way that is by now very familiar to us:-

"And even as geometry rises from certain slight and readily understood foundations to the highest and most difficult demonstrations, whereby the ingenuous mind ascends above the aether: so does our magnetic doctrine and science in due order first show forth certain facts of less rare occurrence; from these proceed facts of a more extraordinary kind; at length in a sort of series are revealed things most secret and privy in the earth, and the causes are recognised of things, that in the ignorance of those of old or through the heedlessness of the moderns, were unnoticed or disregarded." 15

Gilbert's suggestion that geometry allows us to ascend "above the aether" is reminiscent of Dee's threefold classification of things whereby mathematics allows us to reach eternal knowledge of the physical world. His description of the method allowing us to proceed from common facts to the causes of the most "secret and privy in the earth" looks forward to the Baconian aspirations of the New Instauration.

Surely, it can be argued, Gilbert's method and achievement was modern, was entirely in keeping with the new science that Bacon wished to see established. Why, then, did Bacon repeatedly single out Gilbert for adverse comment? Bacon very clearly gave the answer himself; it is surprising that so often his reply has been overlook. "The Empirical school of philosophy", Bacon wrote, "gives birth to dogmas more deformed and monstrous than the Sophistical or Rational school." 16 As examples of the Empirical school Bacon took alchemy and Gilbert's science. The reason Bacon held this school so dangerous was because it based its conclusions "in the

narrowness and darkness of a few experiments." ¹⁷ Constantly, Bacon attacked theories based upon a very limited range of observations: "For as Aristotle saith, that children at the first will call every woman mother, but afterward they come to distinguish according to truth, so experience, if it be in childhood will call every philosophy mother."¹⁸

For this reason Bacon was not willing to accept as true any of the current theories of the structure of the universe, including the Copernican system. Both the Ptolemaic and the Copernican system agreed with the phenomena, and therefore neither had the right to be accepted as true.¹⁹

Bacon did not deny that one of the theories may be true. What he did deny is that the possibility of a theory being true is an end to the matter. What he demanded in natural philosophy was certainty, not conjecture. The object was "to discover true causes and axioms"²⁰ not possible or merely probable ones. He expressed his position very clearly when discussing astronomy:-

"The truth is that, without meaning to throw away the benefit of former inventions, I am attempting a far greater work; for it is not merely calculations or predictions that I aim at, but philosophy: such a philosophy I mean as may inform the human understanding, not only of the motion of the heavenly bodies and the period of that motion, but likewise of their substance, various qualities, powers and influences, according to natural and certain reasons, free from the superstition and frivolity of traditions; and again such as may discover and explain in the motion itself, not what is accordant with the phenomena, but what is found in nature herself, and is actually and really true." 21

Now, it is entirely within this context that we must see Bacon's rejection of Gilbert's approach to nature. Gilbert, by his own admission allowed himself to "philosophize freely"; and this is patently obvious in the latter half of De Magnete. From the fact that the earth appears to be a large loadstone Gilbert concluded that the earth rotates, that the planets rotate, and indeed that the earth has a soul! Bacon rightly held that such speculations went far beyond the evidence. In Bacon's eyes, Gilbert had become fixated on the loadstone, and attempted to use its properties to explain far too much. It was just such "few experiments" leading to grand conclusions that Bacon deplored. People were too easily seduced into thinking that their own pet theories could explain all sorts of phenomena which had nothing to do with each other.

Bacon and Certainty.

As we have seen already, Bacon required that the new method which he professed should give conclusions that were both true and known to be true. His method of induction was not to be "the induction of which the logicians speak, which proceeds by simple enumeration."²² Such induction "is a puerile thing; concludes at hazard; is always liable to be upset by a contradictory instance; takes into account what is known and ordinary; and leads to no result."²³

To reach certainty in the conclusions, Bacon said, "what the sciences stand in need of is a form of induction which shall analyze

experience and take it to pieces, and by a due process of exclusion and rejection lead to an inevitable conclusion."²⁴ Bacon was thus totally and completely opposed to construing the function of science as one of 'saving the appearances'. He was totally opposed to the sort of attitude which we have found exhibited in Osiander's Preface to De Revolutionibus, and which we have seen was to be echoed in some respects in Rohault's approach to explanation in the natural sciences.

We wish to know, then, what Bacon's basis was for such certainty in the new philosophy. But here, unfortunately, Bacon is not very helpful. It is much easier to establish what sort of certainty it is not, than what sort of certainty it is, that Bacon expected his method to generate. Clearly Bacon was opposed to the psychological certainty which often accompanies simple induction, always open to "upset by a contradictory instance". Equally certain is it that Bacon was opposed to any form of certainty derived a priori from self-evident axioms. Bacon believed that knowledge of the world must begin always from experience, and experience was always of the particular, never of the general. Only gradually was it possible to arrive at the most general axioms, and these last of all. Thus he wrote:-

"There are and can be only two ways of searching into and discovering truth. The one flies from the senses and particulars to the most general axioms, and from these principles, the truth of which it takes for settled and immovable, proceeds to judgement and to the discovery of middle axioms. And this way is now in fashion. The other derives axioms from the senses and particulars, rising by

a gradual and unbroken ascent, so that it arrives at the most general axioms last of all. This is the true way but as yet untried." 25

And certainly there is no hint in Bacon that the certainty of the conclusions which can be reached by his method are because we recognise the conclusions to be logically necessary.

If we are to approach an understanding of what Bacon understood by certainty in the natural sciences it is important to realise that he did not expect "the most general axioms" to be reached over night. His whole programme for science was one which would occupy men for a very great time, and he never suggested that the road would be easy. We must start by examining particular phenomena, and in such cases Bacon believed that certainty could be reached by the method of exclusion; if one can find an exception to any particular general proposition about anything, then the proposition must be rejected. Thus in attempting to discover the form of heat, Bacon listed fourteen "exclusions" from it: light is not part of the form of heat, because the moon's rays are light but not hot; rarity is not part of the form of heat because air is for the most part cold, and so on. Then he says: "In the process of exclusion are laid the foundations of true induction, which however is not completed till it arrives at an affirmative."²⁶ But when he does arrive at an affirmative, he recognises that it can only be regarded as an hypothesis about the form of heat, which Bacon called "the Indulgence of the Understanding, or the Commencement of Interpretation or the First Vintage". Bacon then went on:-

.....

"Let us now therefore proceed to the first vintage concerning the form of heat.

"From a survey of instances, all and each, the nature of which heat is a particular case, appears to be motion."

The form of heat, then, Bacon suggested, is motion. He arrived at this conclusion by examining a wide sample of cases of heat, and excluding other suggestions for its form by discovering counter-examples by observation. But this is only a First Vintage. He did not claim any certainty for his conclusions at this stage; this would have to await further experiment and attempt at falsification. Unfortunately Bacon did not take us any further. He never explained how we could ever get beyond First Vintages in our enquiries into nature. Nor did Bacon ever explain what it was that was in motion. And clearly, on his own principles there were very good reasons why he should not, for the candidates are likely to be objects such as atoms or aetherial fluids which are not themselves directly observable, certainly in practice, and perhaps, also not even in principle. If this were so then some of Bacon's forms would indeed never be discovered by a direct empirical method. Others, however, such as the form of naturally accelerated motion, might be discovered empirically, as indeed they were by, amongst others, Galileo.

There were then, very important limitations to the Baconian method as it was presented. Bacon promised certainty in the conclusions of natural science, but he failed to explain what sort of certainty would be revealed or how knowledge in certain areas could be obtained. What then are we to make of the Baconian aspiration? The answer is, I believe, that Bacon set an ideal

towards which science could aspire. Scientific investigation can constantly move towards a higher degree of certainty, even if it never arrives at that total certainty that Bacon hoped for. In setting a target towards which scientists could always aim Bacon broke completely with the view that since science could never reach certain knowledge of the physical world, it was not, therefore, worth attempting to approach it.

It might be objected that to construe the Baconian conception of certainty as an unobtainable ideal is to misunderstand what it was that Bacon had in mind. It might be maintained that all that Bacon sought for in scientific knowledge was the sort of certainty which we attach to those discoveries in science which are called laws of nature; laws of nature, after all, are just those propositions about the physical world which are normally taken as unquestioningly true; that we are willing to call some of the propositions of natural science, statements of laws of nature, indicates that we accept them as being certainly true.

Bacon, no doubt, would have agreed with this. But the difficulty with this formulation of the issue is that it does conflate the important distinction between propositions which are accepted as being true, on the basis of evidence, and those propositions which in some unqualified way could not turn out to be false. It was just this distinction which needed to be made, but which Bacon failed to make, as indeed did Descartes and all the scientists we have so far considered.

Thus although Bacon was in a sense right to emphasise the importance of aiming at certainty in the results to be achieved by science, his very formulation of that objective bypassed the important question of the nature of the sort of certainty to be expected from the scientific programme. The issue was to remain open until the end of the century.

Bacon and Epistemology.

I have suggested already that Bacon failed to look closely at epistemological questions in his philosophical writings. We have just seen that on a central topic, that of supplying criteria for identifying certain truth, his answer is obscure and inadequate. In other areas, too, there are only hints at a theory of knowledge, not a cogently established position. Thus we find that whilst Bacon emphasised the central role of experience for gaining knowledge of the physical world, he never presented any sustained argument against a Rationalist programme for science other than to condemn a priori principles tout court. Nor is this very surprising. No great rationalist physics had as yet presented itself, as it was to do later in the century with the work of Descartes.

Not surprising though it is, it does mean that Bacon gave no great consideration to a variety of topics which were to emerge as central issues in the identification of the nature of scientific enquiry and its possible scope. Thus Bacon did not examine whether or not there were such entities as innate ideas in the human mind which might supply the foundation for our knowledge. Nor did Bacon

attempt to give any general account of the relation between phenomenal experience and the physical world, even though he did deny that the physical world and its properties can be inferred uncritically from sense experience.²⁷ Thus we find that whilst Bacon utilized a rather crude distinction between primary and secondary qualities in much of his discussions of scientific method, for example, in his discussion of heat, he never attempted to supply detailed arguments to justify such a distinction.

In general one can say that Bacon offered no well-substantiated answers to any of the traditional epistemological questions. What he did do was to propound a method for obtaining knowledge of nature which is predominantly - one could almost say totally - empirical in character. This approach was then put into practice by working scientists, and especially by those working scientists who were connected with the early days of the Royal Society. Once this method was in operation, it was possible to see what were the unanswered problems which were inherent in the programme.

Bacon and Mathematics.

It is often maintained that Bacon did not understand the importance of mathematics for the development of science. Certainly we find in his writings none of that veneration accorded to number by the Neoplatonists; nor is there in his works a firm commitment to the possibility of explaining nature in terms of geometry, as we find in Descartes or Galileo. But this is not surprising. To believe that number was the key to reality, or that the language of the universe was geometry, would be, on Bacon's own assumptions, an

unfounded belief, until proven by vast empirical testimony. The attitude of the Neoplatonists exemplified for Bacon one of the things wrong with contemporary natural philosophy, not what was right, even though in particular areas their researches were fruitful.

There was in fact a very real danger in Bacon's belief that it was possible to approach nature without any presumptions at all, for any and every science must have some ontology, and some basic presumptions from which it must begin. But, given the general danger, Bacon's lack of commitment to the role of mathematics in science in the strong Neoplatonic sense was itself an important counter to the powerful influence of the Neoplatonic trend. For one thing, not all science was - or is - amenable to quantified treatment; and for another, the lack of commitment in Bacon encouraged a critical attitude to the actual role of mathematics in the physical sciences. Mathematics was undoubtedly useful in science, Bacon agreed, but it did not supply the raison d'être of scientific investigation. It is worth noting that the introduction of the 'Corpuscular Philosophy' as the most important theory of matter in the seventeenth century was achieved largely by men such as Cassendi and Boyle who were not particularly inclined towards mathematics.²⁸ Further, Boyle rightly thought of his scientific method as one which owed much to Bacon.

If Bacon did not have that high regard for mathematics which many seventeenth century scientists had, he was by no means opposed

to it. Pure mathematics should be advanced, Bacon held, and pointed out that geometry had hardly advanced at all since Euclid.²⁹ 'Mixed', that is, applied, mathematics, he said, was essential in "Perspective, Music, Astronomy, Cosmography, Architecture, Machiery, and some others." Bacon also predicted the advance of mixed mathematics "for as Physic advances farther and farther every day and develops new axioms, it will require fresh assistance from Mathematics in many things, and so the parts of Mixed Mathematics will be more numerous."³⁰

Mathematics, then, Bacon held, was not to be neglected, let alone rejected; but as with his treatment of epistemological issues, Bacon did not attempt any sort of analysis of the nature of mathematics, he provided no sustained case for the downgrading of the role of mathematics against the contemporary Neoplatonic trends. The nature of mathematical truth and the certainty of mathematical propositions still stood in need of explanation.

Bacon and Reason.

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In emphasising, as one must, the empiricist outlook which Bacon fostered there is a natural inclination to assume that reason as such was not an important element in Bacon's method. But nothing could be further from the truth. There is a strong but misleading tendency, encouraged by anti-empirical philosophers, to construe the introduction of empiricism as an attempt to devalue reason in place of experience. But it is important to remember that reason

remained a central aspect of all empiricist philosophy until the demolition it received at the hands of Hume. Furthermore, even Hume's attack on reason as a key to knowledge was essentially a rejection of the view that reason, understood as pure deduction could supply any knowledge of the world. Reason, as understood as "that intellectual power or faculty which is ordinarily employed in adopting thought or action to some end; the guiding principle of the human mind in the process of thinking" (O.E.D.) was never rejected by the Empiricist philosophers - indeed it is hard to imagine how any philosopher could reject reason in this, its most usual, sense. But the Empiricists were concerned to give reason its proper place within the framework of human knowledge, and this was as true of Bacon as it was to be of Locke later in the century.

For Bacon, reason was that faculty of the mind which was essentially concerned or connected with philosophy. "History has reference to the Memory, poesy to the Imagination, and philosophy to the Reason"<sup>31</sup> he wrote. He explained its role thus:-

"Philosophy discards individuals; neither does it deal with the impressions immediately received from them, but with abstract notions derived from these impressions; in the composition and division whereof according to the law of nature and fact its business lies. And this is the office and work of Reason." 32

For Bacon, then, reason was essential to science. It was its function to ruminate upon the impressions of the sense, and then to classify them, to supply an order; and that order would be the discoveries of scientific investigation.



But, as with Galileo, and indeed Locke too, there were for Bacon some areas which reason could not hope to comprehend. For Bacon these were in the field of religion, and he gave a warning:-

"out of the contemplation of nature and elements of human knowledge to induce any conclusion of reason or even any strong persuasion concerning the mysteries of faith, yea, or to inspect and sift them too curiously and search out the manner of the mystery, is in my opinion not safe. "Give unto faith the things which are faith's "..... And therefore it were a vain error to attempt to adapt the heavenly mysteries of religion to our reason." 33

For Bacon, then, reason had its place, indeed a central role in his whole programme. But it also had its limitations. In natural philosophy, given that men applied themselves, there were no a priori limitations; but religion remained for Bacon almost entirely a matter of faith.<sup>34</sup>

#### Conclusions.

Bacon, undoubtedly, was largely responsible for what T. S. Eliot called "the dissociation of the sensibility" in English thought. After Bacon, poetry, religion, and natural philosophy tended to be viewed as autonomous areas of human endeavour with their own particular and peculiar contributions to make to the totality of human understanding. Poetry was linked to the imagination, and imagination was not only irrelevant to science, it was in fact held to be a hindrance, flights of fancy were the enemy of true philosophy.<sup>35</sup> Religion transcended reason, and therefore the canons of rationality were not appropriate to it. It was in natural philosophy especially that reason and knowledge linked arms.

In thus dividing the areas of human intellectual enquiry Bacon precipitated the rejection of the Neoplatonic attitude towards the physical world. For if one accepted the Baconian programme, there was no possibility of justifying one's conclusions in science by an appeal to God's eternal order. Science had to justify itself by criteria independent of theology; natural philosophy had to be secular. Whether science could justify its method without transcendental support was a problem which had still to be faced.

Although Bacon never supplied the argument to refute all a priori claims to knowledge of the physical world, he presented a programme which was entirely opposed to such possibilities. In Baconian terms the programme would be justified if it led to true knowledge of nature which could be used for the benefit of man. The programme needed testing, and it was so tested by the scientists who followed Bacon. In some ways the Baconian ideal was found to be lacking. But its main message, that knowledge of nature could only be obtained by observation and experiment tempered by reason, was in general accepted. There remained, however, the philosophical questions: how far could the method be justified?, did the method rest upon firm foundations? In short, did the Baconian programme generate certain knowledge?

Chapter V

English Science in the Post-Baconian period, 1640 - 1680

Introduction

In this chapter we shall consider some aspects of the development in science in England prior to the publication in 1687 of Newton's Principia and Locke's Essay in 1690. We shall be concerned, therefore, with some of the ideas of men who were either contemporaries of Locke or his immediate predecessors.

Once again, in no sense could this chapter be considered a comprehensive survey of the period. Undoubtedly for the richness of its talent it is one of the most important in the history of English science, and indeed of science generally. About it many volumes have already been written and many more await conception. What I shall be attempting to do is to show how certain methodological problems, arising both out of the Baconian method and the impact of Cartesian science, were constant issues lying at the very foundations of the scientific programme.

Because of the great growth of interest in the physical sciences around the middle of the seventeenth century it is not easy to select individuals for particular consideration. I have tended to draw on examples which illustrate the general theme of uncertainty about the scope and nature of science, rather than those scientists who either saw no problems of method, or raised none. But the bias so exhibited is, I think, justified, for many problems did arise, and even those who ignored them did so at some peril: their work was always in

danger of being undermined by those more conscious of the problems.

The publication of Bacon's works did not immediately herald the New Atlantis of scientific learning for which Bacon hoped. It took at least a generation for his real impact to be felt. Indeed, partly because of the civil war, the interest in science which grew rapidly from 1640 onwards did not manifest itself powerfully until the Restoration in 1660<sup>1</sup>. However, there were some important works published in the period to 1660, which emerged both as a result of the developing general interest, and also as an effect of the impact of Cartesian views on English thinkers.

Most of Bacon's views on science were not shared by his contemporaries, many were not even accepted by his successors. Thus Bacon's hope of achieving certainty in the sciences, shared for different reasons by Dee, was not always the view of the Elizabethan scientists. Sir Walter Raleigh, for example, in his History of the World (1614) maintained that reason was important in science, but he held that reason was powerless to understand essence, as distinguished from accidents.<sup>2</sup> This view was one that was sustained throughout the period that we are considering by many thinkers, and was given empirical and philosophical support by, amongst others, Mersenne in his La Verité des Sciences (1625), by Glanvill in The Vanity of Doematizing (1660), by Boyle in his Origins of Forms and Qualities (1666), and by Locke in the Essay.

If Bacon's views on the certainty possible in natural philosophy were not universally adopted, neither was his rejection of

Aristotelian views, or Greek philosophies generally. Bacon, for example, rejected Epicurean atomism as an unfounded hypothesis,<sup>3</sup> but many scientists were attracted to the theory, and a large number accepted it as true.<sup>4</sup> And many mid-century scientists quite clearly show a great admiration for Aristotle and for his theories. One such person was the intriguing Sir Kenelm Digby (1605 - 1665).

There was, then, no immediate wholesale adoption of the Baconian method or teachings. Even the most devoted of Baconians among the founder members of the Royal Society were, as we shall see, not all of one mind as to what the Baconian method implied. But equally, almost none in the mid-century escaped the influence of Bacon's thought.

Undoubtedly the most important scientist of the period was Robert Boyle. In this chapter we shall not consider him - he deserves one to himself.

#### Sir Kenelm Digby.

Digby managed to be a great admirer and advocate of Descartes, Galileo, and Bacon all at the same time, but his theories are largely a mixture of Cartesian and Aristotelian views. Aristotle, he said, was "the greatest logician and metaphysician and universal scholar peradventure that ever lived". But the admiration was mixed with caution: "Yet withall we are to consider that since his reign was but at the beginning of sciences, he could not choose but have some defects and shortnesses among his many great and admirable perfections."<sup>5</sup> Gone is

the great admiration for Plato, the dominance of Neoplatonic and Pythagorean views from English science; the general conception of nature found in the Neoplatonists never survived as an important force in science after the Cartesian and Baconian secularisation of nature. And Digby showed none of <sup>that</sup> reliance on the Ancients which characterised so much Renaissance thinking.

In his description of his method, Digby was influenced by Bacon, Galileo, and Descartes:-

"In delivering any Science the clearest and smoothest Method and most agreeable to nature is to begin with those things which are most common and obvious, and by dissection of them to descend by orderly degrees and Steppes (as they lye in the way) to the examination of the most particular and remote ones." 6

He was equally Baconian in his distrust of theory:-

"We must narrowly take heed lest, reflecting upon the notions we have in our mind, we afterwards pin those airy superstructures upon the material things themselves that begot them, or frame a new conception of the nature of any thing by the negotiations of our understanding, upon those impressions which itself makes in us; whereas we should acquiesce and be content with the natural and plain notion, which springs immediately and primarily from the thing itself: which when we do not, the more we seem to excel in subtilty, the further we go from reality and truth." 7

In practice, however, Digby often failed to adhere to his stated principles. Thus, he accepted Aristotle's view that motion in a vacuum is impossible, writing, "Aristotle has demonstrated that there can be no motion in a vacuum."<sup>8</sup> He also accepted Aristotle's four elements, and argued that light was identical with fire.<sup>9</sup>

With regard to hypotheses, Digby adopted a thoroughly Cartesian position. He explained many phenomena in terms of atoms, in a manner

clearly much influenced by Descartes, thus presupposing their existence to account for phenomena, and taking the satisfactory account as evidence for the atom's existence. Thus we find him explaining the fact that a loadstone lines up north and south as an empirical confirmation of his general theory about floods of atoms moving from the north pole to the equator.<sup>10</sup>

Digby, like Bacon and Descartes, believed that science would lead to a vast increase in man's knowledge, and he showed no signs scepticism about possible limits to the programme, or the certainty possible for scientific conclusions. It is not, therefore, surprising to find him firmly committed to the view that demonstration is possible in the natural sciences. Whoever shall be, Digby argued,

"as exact and orderly in treating of philosophy and theology, as mathematicians are in delivering their sciences, I assure myself that demonstrations might be made, and would proceed in them as currently, and the conclusions be as certain and full, as in mathematics themselves. But that is not all; these demonstrations would have the odds exceedingly of the other, and be to us inestimably more advantageous: for out of them spring much higher and nobler effects for mans use and life, than out of any mathematical ones." 11

Digby thus shows a faith that natural philosophy can be as certain as mathematics. But he did not offer any rationale of this position, and it is unclear what his grounds were. Indeed within Digby's system it seems little more than a pious hope. The hope was linked with a Baconian commitment to the utility of science.

Digby's exaltation of science over mathematics illustrates how unaffected he was by the Neoplatonic ideas of such figures as Dee. As with Bacon, Digby rated mathematics not as a key with which to

reach the ideal forms of nature, but rather as an aid within the body of science. Despite this, and somewhat paradoxically, it is the mathematical conception of demonstration which he believed it was possible to transfer to the natural sciences.

In Digby, then, we still find the commitment to the mathematical paradigm of certainty that we found in the Neoplatonists, but without the supporting metaphysics. There is no supporting theory to substantiate the hope of certainty in science, but the hope is still maintained. No doubt the optimism was sustained by the promise of the Baconian and Cartesian programmes. But, as we have seen, the promise of those methods was at best obscure, and at worst, ill-founded. This optimism was a marked feature of the period, but nobody was able to supply the supporting argument to justify it, a problem which troubled many thinkers of the time. It was an optimism which Locke's Essay did much to put in its true perspective.

Digby can hardly be rated a scientist of the first rank. But he is of interest not only for the points we have considered, but also because he was the first Englishman to be strongly influenced by Descartes, whom he knew personally.<sup>12</sup>

Thomas Hobbes.

Another person to be strongly influenced in his conception of scientific method by Descartes was Hobbes (1588 - 1679). Hobbes not only knew Descartes, whom he admired as a mathematician and scientist - if perhaps not so much as a philosopher - but he was also a close personal friend of Digby, despite the fact that the latter was a Catholic. (For Hobbes the Catholic church was anathema.)



As with Digby, Hobbes is another representative of that way of looking at science which derived from Bacon, Galileo, and Descartes. Generally, for him, explanations in the physical sciences were to be esteemed according to their explanatory power, and their utilitarian consequences. Thus, unlike Bacon, but like Descartes, whether the postulated causes of a given effect were the causes was only of secondary importance for Hobbes. Hobbes explained his position thus:-

"The Doctrine of Natural Causes hath not infallible and Principles. [Sic.] For there is no Effect which the Power of God cannot produce by many several ways.

"But seeing that all effects are produced by Motion, he that supposing some one or more motions can derive from them the necessity of that effect whose cause is required, has done all that can be expected from Natural Reason." 13

The most that we can expect from natural reason, Hobbes maintained, is to know that a certain effect "may be produced" in a certain way; and "may be produced" meant for Hobbes that the effect will occur if those causes are present, even if we do not know that they really are the causes, "which is as useful as if the causes themselves were known." The identification of causes was for Hobbes largely an empirical matter. He held, also, that we are only to be concerned with causes which will produce the particular effects. Hobbes saw no point in being sceptical about whether putative causes are really the causes of particular phenomena, so long as the putative causes are always sufficient to produce the particular effect. Hobbes did not concern himself with the fact that the putative causes cannot be guaranteed to produce the effect unless they are the real causes.

Hobbes's treatment of the possibility of a vacuum illustrates very clearly his method in science, and also contrasts him with both Bacon and Descartes. Both the latter argued, or asserted, the impossibility of a vacuum, Bacon for reasons that remain obscure<sup>14</sup>, and Descartes as a consequence of his belief that matter and space are identical.<sup>15</sup>

Descartes ruled out the possibility of a vacuum on a priori grounds. In contrast, Hobbes deployed both argument and experiment to prove that there is no empty space. The experiment he cites is "a common one, but I think unanswerable".<sup>16</sup> It is that water will not flow out of a hole in the bottom of a vessel unless there is a hole open at the top of the vessel.<sup>17</sup> Hobbes was, of course, unaware of the existence of air pressure.

In his Elements of Philosophy Hobbes argued that the phenomena of the heavens can be accounted for by various assumptions which he lists. But he was quite clear that they were only assumptions, and he concluded:-

"And though the causes I have here supposed be not the true causes of these phenomena, yet I have demonstrated that they are sufficient to produce them, according to what I at first propounded." 18

Hobbes, like Bacon, saw the function of physics and the sciences generally, as the control of nature for the better estate of man. But, unlike Bacon, he did not envisage the results of man's enquiries reaching the status of absolute certain knowledge. He held out no promise of reaching Bacon's forms. In this attitude he shared much with Osiander, and anticipated some of the approach of Rohault.

But, as with Digby, Hobbes made no detailed or profound analysis of the conditions for knowledge, or the general connection between physics and philosophy.

Bacon's influence on Hobbes was considerable. The emphasis on the possibility of the control of nature through physical enquiry was all-important, the epistemological considerations remained of minor interest. But, unlike Bacon, Hobbes had no fear of conjecture; hypotheses were for him of first rate importance. Hobbes saw the danger not in conjecture, but in assuming that one's conjectures were certainly true.

Sir Thomas Browne.

Given that no satisfactory epistemological account of natural philosophy was accepted by the mid-seventeenth century scientists (except for a very small group of Cartesians) there was always a danger that the Baconian programme would founder on the sceptical arguments deployed by the supporters of Sextus Empiricus. It is not therefore surprising to find various thinkers urging that men should not be led by sceptical arguments into rejecting out of hand plausible conjectures simply because they were not established beyond doubt. A good example of this attitude is to be found in the work of Sir Thomas Browne. In his Pseudodoxia Epidemica (1646) Browne argued that "as credulity is the cause of error, so incredulity oftentimes of not enjoying truth."<sup>19</sup> God, said Browne, "hath proposed the world into our knowledge, and thereby the notion of Himself."<sup>20</sup> Therefore:-

"if any affirm, the earth doth move, and will not believe with us, it standeth still; because he hath probable reason for it, and I no infallible sense, nor reason against it, I will not quarrel with his assertion. But if, like Zeno, he shall walk about, and yet deny there is any motion in Nature, surely that man was constituted for Anticera." 21

Browne was a man of common sense; plausible conjectures must be allowed their due, absurd ones must be rejected.

John Wilkins.

Similarly, John Wilkins in his Discovery of a New World (1638) argued that it is plausible to think that the moon may be inhabited, though he did not offer any "necessary proofs". Wilkins urged his readers not to dismiss an idea simply because it was unusual.

He showed a similar purpose in his A Discourse concerning a New Planet tending to that (it is probable) our Earth is one of the Planets (1640), a title which itself emphasised the importance of conjecture in our thinking about the physical world.

In his Mathematical Magic (1648) - subtitled of the wonders that may be performed by Mechanical Geometry - Wilkins, like many others, showed his commitment to the utilitarian ends of science without attempting to give any epistemological justification for pursuing it as an enterprise. Mechanics was seen by Wilkins as a discipline "whereby nature is .... quickened or advanced in her defects."<sup>22</sup> And Wilkins blamed Plato for not encouraging the application of mathematics to nature.<sup>23</sup> Under the impact of Bacon, the reverence shown to Plato had all but disappeared. The justification for mechanics was not in terms of a stepping stone to divine knowledge, but is seen

almost entirely in terms of works.

With regard to the possibility of reaching certainty in science, Wilkins held generally that certainty could be obtained in those areas of science which were capable of detailed quantitative treatment. In A Discourse Concerning a New Planet he wrote that the "demonstrations of astronomy are as infallible as truth itself",<sup>24</sup> and he believed that mechanics was a branch of mathematics,<sup>25</sup> his attitude being very similar to that of Descartes.

Like all his contemporaries Wilkins was eager to show that science was entirely compatible with religious belief. Like Galileo, Wilkins argued in Discovery of a New World that the Copernican theory was conformable both to reason and religion. Indeed the book is actually based on Campanella's Apologia pro Galileo.<sup>26</sup> And in perhaps his most important work, published posthumously in 1675, Of the Principles and Duties of Natural Religion, Wilkins argued that religion can be founded upon as sure a footing as reason can offer, and steered a middle course between scepticism and dogmatism.

Wilkin's attitudes are worth noting because he was one of the most important, if not the most important, person in the group from which the Royal Society took its foundation; as Aubrey described him, he was the "Principal Reviver of Experimental Philosophy at Oxford."<sup>27</sup> It is fairly certain, too, that his views are representative of the virtuosi of the period.

Henry Power.

Another person with similar views to Wilkins was Henry Power (1623 - 1668) who became a Fellow of the Royal Society in 1663. His Experimental Philosophy in Three Books (actually published in 1663, though dated 1664) propounds both "deductions, and probable hypotheses raised from them in Avouchment and Illustration of the now famous Atomical Hypothesis."

According to Power:-

"This is the age wherein (me-thinks) philosophy comes in with a spring-tide ..... These are the days that must lay a new foundation of a more magnificent philosophy, never to be overthrown: that will empirically and sensibly canvass the phenomena of Nature, deducing the causes of things from such originals in Nature, as we observe are possible by art, and the infallible demonstrations of mechanicks: and certainly this is the way and no other to build a true and permanent philosophy ..... And to speak yet more close to the point, I think it is no rehetorication [sic.] to say that all things are artificial; for Nature it self is nothing else but the art of God." 28

Power expected natural philosophy to yield certainty; but he was quite ready to admit that much of science was still conjecture. Like Wilkins, Power believed in the certainty of mechanics and astronomy, but he would not go so far in his support of atomism, even though he expected that in the future it would be conclusively established. Thus he wrote:-

"Lastly, many more hints might be taken from the former observations to make good the Atomical Hypothesis; which I am confident will receive from the microscope some further advantage and illustration, not only as to its first universal matter, Atoms; but also as to the necessary attributes or essential properties of them, as motion, figure, magnitude, order and disposition of them in several concretes of the World." 29

Observation, then, Power held, would lead to a knowledge of the structure of matter, and would probably lead not only to the vindication of the atomic hypothesis, but also to an understanding of the essence of atoms. Power believed that science, based on observation, would lead to knowledge of the Baconian forms.

Power's conjecture as to the possibility of obtaining knowledge of the essence of matter was a very common form of speculation in the period. Indeed, just such a possibility as Power propounded was to be one of the subjects that Locke considered in the Essay; and it is not even too far-fetched to conjecture that Locke had Power in mind when he turned his attention to this topic, for he certainly read Power's work.<sup>30</sup>

Robert Hooke.

Another person who shared Power's optimism about finding certainty in natural philosophy as Bacon had promised, but who did not yet see it as accomplished, was Robert Hooke (1635 - 1703). Undoubtedly Hooke was one of the very great scientists of the seventeenth century; and, as Curator of the Royal Society, he was excellently placed to be aware of the general attitudes of scientists in the early years of the Society's foundation.

Those writings of Hooke which were published during his lifetime tell us comparatively little about his aspirations for science. But in a collection of works published after his death there are many interesting remarks about the nature and scope of natural

philosophy.<sup>31</sup> Through them we can see how close Hooke was to the position we have seen Power to have held.

Hooke recognised that the then-present state of natural philosophy did not guarantee true knowledge, only conjecture. Nevertheless, he looked forward to the day when true knowledge would be established. Hooke (any more than Bacon) never appreciated the logical difficulties to such an accomplishment as long as mathematical knowledge remained the paradigm towards which science should move. The title of one of Hooke's work is indicative of his approach and is entirely Baconian: "A General Scheme or Idea of the Present State of Natural Philosophy and how its defects may be remedied by a methodical proceeding in the making Experiments and collecting observations. Whereby to compile a Natural History as the Solid Basis for the Superstructure of True Philosophy."<sup>32</sup>

Hooke stated that "the Business of Philosophy is to find out a perfect knowledge of the nature and properties of bodies, and of the Causes of Natural Productions." Bacon could not have put his objective more clearly himself. He would have agreed, too, with Hooke's justification of the pursuit of knowledge in science, which was to improve man's position in relation to his environment.

Natural philosophy, Hooke maintained, could be made absolutely certain on the model of mathematics. As he wrote:-

"The greatest and most accomplished Wits have not been able to bring forth any greater Effects than Probabilities: whereas I cannot doubt but that if this Art be well prosecuted and made us of .... even Physical and Natural Enquiries as well as Mathematical and Geometrical will be capable of Demonstration." 34



Hooke, then, believed that certain knowledge would be possible in science. We have already seen that Power and Wilkins held similar views.

In the light of this it is impossible to accept the general contention of Van Leeuwen.<sup>35</sup> Van Leeuwen argues that Baconian science was different from the science of the Royal Society because the former expected absolute certainty from his method, whereas, typically, the members of the Royal Society did not. Thus he writes:-

"for Bacon scientific knowledge is demonstrative and is absolutely certain, a theme in the sequel we shall find to be unacceptable to the leading members of the Royal Society. Further, since a mechanical method . . . . can be provided for the discovery of forms, no extraordinary genius is required of the scientist and certainty of the results is assured." 36

But, as the quotation from Hooke shows, the Curator of the Royal Society shared the Baconian aspiration. This is brought out even more clearly if we continue the passage from Hooke which goes on: "so that henceforward the business of invention will not be so much the effects of acute wit, as of serious and industrious prosecution."<sup>37</sup>

Van Leeuwen is wrong to maintain that in general there was a non-Baconian attitude among the Fellows of the Royal Society. Wilkins, Power, Hooke and Thomas Spratt, the Society's first historian, were all very Baconian in their ideal and in their practice. Rather, there were two schools of thought within the Royal Society about the possibility of demonstrative and certain knowledge in natural philosophy. One group, represented by Power, Wilkins and Hooke, subscribed to Bacon's view. Others were more

sceptical and included such men as Joseph Glanvill, Robert Boyle, and John Locke. These latter, as we shall see, expected less of the scientific enterprize, but were no less committed to its importance.

It is clear from the membership of the groups that there is no correlation between ability and commitment; Hooke and Boyle, two of England's greatest scientists were in different camps. But it was the more philosophically inclined members who tended to be sceptical of the Baconian programme.<sup>38</sup> It was, then, I have suggested, for philosophical reasons that some members of the Royal Society did not wholeheartedly support the Baconian programme. It is to one of these thinkers that we must now turn.

#### Joseph Glanvill.

Although Glanvill was not a great scientist, he was a staunch defender of the Royal Society and the New Philosophy.<sup>39</sup> He was much influenced by Henry More, and Cambridge Platonism generally, and Glanvill's writings show at least hints of a return to a much more authodox Platonism than that exhibited by the Neoplatonists of the Renaissance.

One of the main contentions of Plato's philosophy had been that man cannot obtain certain knowledge of the world of experience, and it is with this attitude of Plato's that the views of Glanvill on the possibility of knowledge through natural philosophy have much in common.<sup>40</sup>

Glanvill saw the limits to man's knowledge in theological terms. Like Wilkins, he saw man's ignorance as a product of the fall of Adam. Before the fall, no doubt, Glanvill believed, Adam's "knowledge of natural effects, might probably arise from his sensible perception of their causes."<sup>41</sup> But since then, man had had to have artificial aids to penetrate to the causes of natural phenomena, such as "Galileo's tube".

Despite his avowed scepticism, Glanvill did not hold that all knowledge was impossible. Indeed, his sceptical programme was designed to lead to knowledge; "my endeavour is to promote it" he wrote, "confidence in uncertainties, is the greatest enemy to what is certain, and were I a sceptick I'de plead for Dogmatizing: For the way to bring men to stick to nothing, is confidently to persuade them to swallow all things."<sup>42</sup> Glanvill, like Descartes whom he greatly admired, saw the possibility of establishing knowledge only after men had come to see the limitations of the specious, currently accepted, roads to truth.

For Glanvill, knowledge was equivalent to demonstration. But, importantly, Glanvill introduced a different criterion from Descartes' concept of clear and distinct idea for deciding when demonstration was successful. It was only successful for Glanvill when it established its conclusion to be necessary; "we hold no demonstration in the notion of the Dogmatist", he wrote, "but where the contrary is impossible: For necessary is that which cannot be

otherwise."<sup>43</sup> As a consequence of this, Glanvill argued that most of what we normally assume to be knowledge is not knowledge in fact, merely opinion.

Glanvill offered us no account of what it is for something to be necessary. But it seems fairly clear that he would employ a logical, rather than a psychological, criterion. That he offered no explicit criterion illustrates that the issue was still not appreciated by him: the problem remained both as to what constituted necessity and what contingency.

That Glanvill was not clear on the issue can be seen if we return to consider what he could have possibly understood when he assumed that Adam before the fall had a knowledge of causes which had since escaped man. Glanvill maintained that through his senses Adam had knowledge of causes. If we accept that to have knowledge is to know that some proposition is logically necessary, then we can easily show that such knowledge was impossible for even Adam to have, simply through his senses. Glanvill believed that a knowledge of causes can lead to knowledge of effects. Thus, if I know that the only cause of heat is atoms in motion, then it follows that if there is heat, then there are atoms in motion. Such knowledge, Glanvill would have argued, is the sort of knowledge for which scientists were searching. But how could we or Adam know that only atoms in motion produce heat? Even if Adam, through his superior senses, could see atoms, how could he know that he was witnessing the real cause of heat, rather than some intermediate step in a chain, or simply constant conjunction? Indeed, he could

only know this on either of the following suppositions (or similar ones); (a) God told Adam that he was witnessing the causes - hardly a normal understanding of empirical knowledge - or, a priori, he knew that (b) 'Atoms are the cause of Heat' is logically necessary. But (b) is false. There was, therefore, no way in which Adam could have the sort of knowledge which Glanvill claimed for him.

If Glanvill had been fully aware of his criterion for knowledge he would have realized that he was wrong to make such grandiose claims for the power of Adam. But Glanvill did recognise the limitations for ordinary mortals, and for this he certainly deserves credit. His position emerges most clearly in his remarks on causation which must be reckoned some of his most significant.

He wrote:-

"All knowledge of causes is deductive: for we know none by simple intuition: but through the mediation of its effects. Now we cannot conclude any thing to be the cause of another but from its continual accompanying it: for the causality itself is insensible. Thus we gather fire to be the cause of heat and the sun of daylight: because wherever the sun is, light attends it and e contra. But now to argue from a concomitancy to a causality, is not infallibly conclusive: Yea in this way lies notorious delusion. Is't not possible, and how know we the contrary, but that something, which always attends the grosser flame, may be the cause of heat? and may not it and its supposed cause, be only parallel effects?" 44

Out of context this passage might well be taken to establish Glanvill and not Hume as the first great analyst of the concept of causation. Undoubtedly Glanvill's remarks are extremely important, but for all their clarity, it would appear that Glanvill

himself did not appreciate their full import. Why he did not, I have suggested, is because Glanvill failed to analyse the concept of necessity with any great profundity. Despite this limitation, Glanvill was quite clear that all so-called causal knowledge must be conjectural, and his argument for his conclusions was entirely correct.

Glanvill's views on mathematical knowledge are also of considerable importance, and, like his views on causation, may even be original. Mathematics, Glanvill held, does not really give us knowledge, because it is essentially of our own production, and it is not necessarily connected with anything external to ourselves. Thus he said that "the knowledge we have of the Mathematicks, hath no reason to elate us; since by them we know but numbers, and figures, creatures of our own, and are yet ignorant of our Maker's."<sup>45</sup>

Glanvill appears to have held the view that the mathematical entities with which we are familiar are some sort of copies of eternal mathematical entities to which only God has access. But, if the numbers with which we operate are copies of ideas in the mind of God, Glanvill held that we have no way of knowing this; we therefore knew nothing for certain except our own ideas.

There are at least shades of some form of Platonism in Glanvill's scepticism here, which would not be out of keeping for a disciple of Henry More. But the passage also shows that Glanvill had no deep understanding of the nature of mathematical truth.

However, in suggesting that mathematics, or at least the mathematics with which we are familiar, has no known reference to anything independent of the mind, Glanvill implied that mathematical truth required no independent justification from the mind. In this sense he was anti-Platonic, for there was no appeal to the eternal forms to justify the propositions of mathematics which we actually use.

Glanvill's views on mathematics point towards the account of mathematics which was to be put forward by Locke. Like Glanvill, Locke was to argue that we had no need to transcend the human intellect to account for our understanding of mathematics. As with his conception of causation, Glanvill was opening the road to the view that much of what we take to be truths about reality, are in fact more a question of how it is that we, as human beings, construe reality.

With regard to scientific knowledge, Glanvill was exactly as one would expect, a conjecturalist in the tradition of Osiander, Hobbes, and Rohault. He maintained that Descartes's system was intended by its author only as an hypothesis,<sup>46</sup> and in general held that no further account can be expected from humanity "but how things possibly may have been made consonantly to sensible nature .... For to say the principles of nature must be such as our philosophy makes them, is to set bounds to omnipotence, and to confine infinite power and wisdom to our shallow models."<sup>47</sup>

Glanvill's use of the word 'model' is both interesting and instructive. It shows that Glanvill was committed to seeing the theories of men about nature as always open to revision in the light

of further empirical enquiry. It also shows an acknowledgment of the role of analogy in scientific explanation, a point which we shall see in the sequel, fully grasped by Locke.

Glanvill, then, was committed to a view of man which placed very severe limitations on his possible knowledge. Like Bacon, he went to very considerable lengths to outline the causes of our misunderstandings about nature. He wrote, for example, of "the impostures and deceits of our senses",<sup>48</sup> and the dangers of the imagination which "impresses a persuasion without evidence"<sup>49</sup> which "an ungrounded credulity cry'd up for faith."<sup>50</sup> Like Bacon, he believed that such limitations could be overcome; but unlike Bacon, Glanvill held that it was not possible for man to reach certain knowledge of nature.

Glanvill's commitment to the impossibility of knowledge of nature was tempered by his very definite criteria for what should count as rational belief. These were either simple empirical confirmation, or else the acceptance of an assumption, even though it could not be directly confirmed, because of its explanatory power. Thus he wrote:

"And now since a great part of our scientific treasure is most likely to be adulterate, though all bears the image and superscription of Verity; the only way to know what is sophisticate and what is not is to bring all to the Examen of the Touchstone." 51

And Glanvill recommended us to follow Descartes' method - always remembering that Glanvill believed that Descartes held his conclusions to be only hypotheses.



Hypotheses, Glanvill held, can be rationally believed, even though they are recognised as hypotheses. Thus he recommended us to believe that all sensitive perception is derived from motion;<sup>52</sup> "This I conceive to be an hypothesis well worthy of a rational belief"<sup>53</sup> he wrote. Perhaps significantly, Glanvill attributed this thesis to Descartes without any mention of Gassendi or any other thinkers.

Quite correctly, Glanvill saw that certain of Descartes' hypotheses were not open to empirical confirmation or falsification. Thus, he held that the choice between accepting and rejecting the Cartesian form of the primary and secondary quality distinction could not be settled by observation, "for if it would be as Descartes would have it; yet sense would constantly present it to us as now."<sup>54</sup> Here, the basis of choice was to be made in terms of the fruitfulness of the outcome in terms of the explanatory power so generated. Thus, on the choice between Descartes' wave theory of light and Gassendi's atomic theory Glanvill wrote:-

"Whether sensation be made by corporall emissions and material  $\xi \zeta \varsigma \omega \lambda \alpha$  or by motions impressed on the aetherial matter, and carried by the continuity thereof to the Common Senses I'll not revive into a dispute: The ingenuity of the latter hath already given it almost an absolute victory over its rival." 55

Glanvill's whole tone was empiricist, and he believed that rational belief should always be proportioned to the evidence in its favour. It is an important feature of many, perhaps all, of the English scientists strongly influenced by Descartes, that they nevertheless were of this frame of mind. The lessons which the English learnt from Descartes were generally not inclined to make them a priori

in their science.

To class Glanvill as an empiricist is reasonable, though many of his religious views would not settle happily in that camp. But even here Glanvill often argued for his belief in the Christian religion by means of empirical evidence. Thus, to our minds bizarrely, in his Sadducismus Triumphatus he maintained that witches were empirical evidence for the existence of the Devil, and that the existence of the Devil was evidence for the existence of God.

Though an empiricist, Glanvill shows few, if any, signs of denying the reality of postulated, but unobservable, entities, as Berkeley and Hume were to do; and though a sceptic in conventional terms, Glanvill did not draw any sceptical conclusions about the possibility of rational belief. What he did do was to challenge the complacency of those too eager to accept the aspirations of the full-blooded Baconian programme. His function was to temper the enthusiasm of those who were inclined to rest too smugly on the achievements of the new science, and this, at any time, is no bad thing.

#### The Cambridge Platonists.

Although not unconnected with the New Science, the Cambridge Platonist were primarily theologians. Directly, they contributed nothing of importance to science, though indirectly, they could be held to have had some influence.<sup>56</sup> The objective of the Cambridge Platonists was largely to supply a rational theology, and in their thinking they were greatly influenced by Descartes.

The most important of their number was Henry More. More agreed with Descartes on most questions, but he disagreed on the rather important one, whether spiritual substance was extended. Primarily for theological reasons, More argued that spirits were extended, but physical reasons played a large part in his case. More, perhaps better than anybody else, illustrates how closely connected were physics and theology for seventeenth century thinkers.

In his Immortality of the Soul (1659), More argued that not all physical effects are produced by mechanical causes. Thus he said that gravity, that is, the tendency of a body to fall towards the centre of the earth, could not be explained on mechanical grounds. Gravity, he argued, is "some power more than mechanical". On purely mechanical grounds, he said, physical objects ought to fly off and recede from the earth. More was not opposed to mechanistic explanations, but he believed that their limitations should be recognised. Ontologically, More held that spirits and gravity were in the same class.

More tried to steer a middle course between the rigid dualism of Descartes, and the materialism of Hobbes. In doing so, his concern was primarily with the nature of substance. On the limits and nature of knowledge he had comparatively little to say. Using the scholastic terminology of faculties, he maintained that truths are confined to (a) "Common Notions", which corresponded roughly to Bacon's 'First Philosophy', and included such truths as that the whole is greater than the parts; (b) "External Sense", which included besides knowledge by sensation, memory as well; and (c) "Deductions of

reason". He held that "whatever is clear to any one of these three faculties is to be held undoubtedly true, the others having nothing to evidence to the contrary."<sup>57</sup> But he did not give an account of the nature and limits of knowledge beyond this, and he did not consider the sorts of epistemological problems with which we have been concerned, arising out of the scientific developments.

The general attitude of the Cambridge Platonists to the New Science was well summed up by Ralph Cudworth in a letter to Robert Boyle written in 1684. Cudworth wrote:-

"The writers of hypotheses in natural philosophy will be confuting one another a long time, before the world will ever agree if it ever do. But your pieces of natural history are unconfutable, and will afford the best grounds to build hypotheses upon. You have much undone Sir Francis Bacon in your natural experiments; and you have not insinuated anything, as he is thought to have done, tending to irreligion, but the contrary." 58

The slightly patronising tone, the general impression of uninterest in natural science, the belief that theology was still Queen of the Sciences, are attitudes which I believe are all discernable in Cudworth's remarks.

As one would expect from their name, the Cambridge Platonists were committed, in one way or another, to a doctrine of innate ideas. But their views on these matters were not of central importance to scientific developments, except in so far as they encouraged a tendency to believe in the possibility of an a priori system of natural philosophy.<sup>59</sup> But such a tendency never developed strong roots in England, if it did anywhere else. The only great attempt towards

such a system was the work of Leibniz, and the influence of Leibniz in England, in either epistemological or scientific matters, was never very great; the controversy with Newton over the priority of the invention of the calculus was enough to see to that.

### Conclusions.

The central thesis of this chapter has been that the achievements of the scientists of the period was not accompanied by a solution of the epistemological problems which the new science generated. The Baconian expectation of certainty in science, whilst subscribed to by many, was not wholeheartedly supported, and nobody succeeded in providing a justification for the Baconian aspiration. Although Descartes' influence was vast, few, if any, of the English scientists adopted his epistemology, and they believed that his science supported a conjecturalist view of natural philosophy.

But, if these issues remained unresolved, the progress was considerable. Virtually all of the scientists of the period, with the possible exception of the Cambridge Platonists, came to accept a mechanistic view of nature: the key to natural philosophy was matter in motion. Thus Hobbes could write: "Galileus in our time .... was the first that opened the gate of natural philosophy universal which is the knowledge of the nature of motion."<sup>60</sup>

Descartes' sharp distinction between mind and matter was just another aspect, though an important one, of seeing all problems of

the physical world in terms of mechanical cause and effect. The causes of change were physical objects, whose exact nature had yet to be established, producing effects by virtue of motion. Already, too, the possibility of qualitative changes being accounted for in terms of quantitative changes of matter in motion was becoming accepted.

But, this granted, it is as well to remember that many were also opposed to such radical new thinking. We have only taken account of those who argued for the new science, but at least as many people were actively opposed to it as in its favour. The issue was by no means decided with the foundation of the Royal Society, though royal patronage helped the victory, and it is worth remembering that as late as 1703 there was a movement in Oxford to ban John Locke's Essay for undergraduates, because the new philosophy "was too much read".<sup>61</sup>

The science of the period was almost totally devoid of the Neoplatonism of the earlier phase of the scientific revolution, and indeed, as we have seen, there was a very definite compartmentalisation of knowledge developing under the influence of Bacon. This in itself meant that the scientific enterprise could not so easily be justified by an appeal to a religiously-based metaphysic; science needed a justification within its own terms. The dominant criterion, for lack of any other, was to point to the success of the scientists in accounting for various phenomena, and their ability to predict (in advance) the outcome of various events. But, as Glanvill had

shown, the foundations of such a justification were at least shaky, if not downright rotten. But in general the optimism of the age discouraged any profound disbelief in the rationality of the programme: observation and experiment, it was generally held, could and did bring knowledge.

Chapter VI

The Sceptical Chemist

Introduction.

No thinker better reflected the intellectual climate which accompanied the arrival of the new science in England than Robert Boyle (1627 - 1691). Boyle was not only the greatest English scientist of the early days of the Royal Society, and the one who argued most effectively for the 'Corpuscular Philosophy', but he also wrote widely on theological and philosophical topics. In his theology and his philosophy, Boyle showed how great was the impact of science on his wider thinking; if the science of the Neoplatonists of an earlier age was shaped by their theology and metaphysics, then it was, for Boyle, science which shaped his theology and metaphysics. Perhaps nothing is more indicative of the modern mind than this profound shift.

This is not to say that the change was total: Boyle never conceded that the method of science was the only road to knowledge. But Boyle's whole thinking was pervaded by an empirical outlook, in some respects even more comprehensive than Bacon's. Thus, Boyle argued that the Christian religion was substantiated largely by "the testimony of divine miracles", and the "great effects produced in the world by it" (i.e. the Christian religion). And Boyle accepted this as correct because these justifications "are bottomed upon matters of fact, and consequently are likely to be the most prevalent upon those that have a great veneration for experience,



and are duly disposed to frame such pious reflections, as it warrants and leads them to make."<sup>1</sup> For Descartes, God had justified accepting empirical evidence: for Boyle, empirical evidence justified accepting God. Such a basis for a belief in Christianity was to become very common in the later seventeenth century, and, incidentally, it is one reason, among several, why Hume's chapter 'On Miracles' in his first Inquiry was such an important critique of early eighteenth century religious attitudes.

Boyle, like Glanvill, offered no general panacea for the problem of certainty in science. He held that ill-founded optimism in the certainty of science was a danger to true progress; but, as we shall see, this in no way placed him in the camp of the dogmatic sceptics. His scepticism was of the sort which should accompany all good scientific work, a recognition that no matter how strong the evidence in favour of a particular hypothesis, it must remain open to the possibility of either revision or even rejection at some later stage.

Boyle, more than any other Englishman, was seen by his contemporaries as supplying the evidence to support the Corpuscular view of matter, against any Aristotelian account of matter's properties. He was particularly concerned to show that properties such as hot and cold were capable of being explained by mechanical principles, rather than that they should be accepted as irreducible properties of matter, and in favour of this view he gathered a great deal of experimental evidence. How conclusive this evidence

was is still a matter of dispute,<sup>2</sup> but it could not be dismissed easily. Certainly, in this respect we can say that John Locke worked in a world which Boyle had done much to make.

In his work to substantiate the atomic view of matter Boyle produced a great deal of evidence to support the distinction between the primary and secondary qualities of matter, and much of Locke's case for the distinction as he presented it in the Essay can only be understood against this background. Many other aspects of Boyle's work were also important to Locke's theory of knowledge: Locke's concept of method, his views on the possible extent of human inquiry, and classifications of the types of knowledge, all reflect aspects of positions for which Boyle argued. The importance of Boyle, therefore, is central not only to the development of English science, but also to English philosophy.

Boyle claimed that he was not much influenced by earlier thinkers. Thus he wrote in 1661 in regard to Descartes' works that he "purposely refrained, though not altogether from transiently consulting, about a few particulars, yet from seriously and orderly reading those excellent (though disagreeing) books, or so much as Sir Francis Bacon's Novum Organum, that I might be not prepossessed with any theory or principles, till I had spent some time in trying what things themselves would incline me to think."<sup>3</sup> But, clearly, much of Boyle's later thinking at least drew on the work of Galileo, Bacon, Gassendi, and Descartes. Indeed, Boyle went on to say that he was just "beginning now to allow myself to read those excellent

books," and acknowledged that "if I had read them before I began to write, I might have enriched the ensuing essays with divers truths, which they now want."

In this chapter we shall look at Boyle's conception of science, and his views on the possibility of knowledge. As I have already suggested, we shall find in his works both caution and optimism, the middle ground, between the certainties promised by Bacon and Descartes, and the scepticism of the Pyrrhonians.

#### The Limits of Reason.

An appropriate road in to Boyle's thinking on the limits of knowledge is by way of a paper by Richard S. Westfall. As Westfall is an influential historian of science it will be worth considering his argument in a little detail. His claims about Boyle's attitude towards the status of scientific theories are, I believe, fundamentally mistaken.

In his 'Unpublished Boyle Papers Relating to Scientific Method'<sup>4</sup>, Westfall argues that there is a contradiction in Boyle's attitude towards the status of his theories. On the one hand, Westfall claims, Boyle recognised that the Corpuscular Philosophy could only be construed as an hypothesis when he was discussing metaphysical questions; but, on the other hand, Boyle wished to claim absolute certainty for it when contrasting the new Corpuscular philosophy with Aristotelian physics. Westfall writes:-

"When he was denouncing the Aristotelian philosophy, Boyle had no doubt of the necessary truth of the mechanical philosophy, while he was never more sure of the limitations of human understanding than when he was defending religious truths above reason." 5

Contrary to this, I believe that to argue as vigorously as Boyle did for the Corpuscular Philosophy did not in itself commit him to the view that whatever truths it did embody were necessarily true in any sense, nor is it even to commit him beyond all possible recall to the mechanical philosophy. Rather, what we find in Boyle is the same sort of attitude that we found in Glanvill, that is, an acceptance of the mechanical philosophy as a rational belief, established as certainly as a theory which involved an appeal to entities which could not be observed might be established, without any suggestion that the theory might not, at a later stage, be replaced by a more adequate theory.

In support of his view, Westfall quotes from Boyle's Sceptical Chemist (1679):-

"I do not with the true Scepticks prepose doubts to persuade men that all things are doubtful and will ever remain so (at least) to human understandings; but I propose doubts not only with design, but with hope, of being at length freed from them by the attainment of undoubted truth; which I seek that I may find it ....."<sup>6</sup>

Westfall takes this as evidence that Boyle believed, with the Aristotelians, "that a science should be more than possible hypotheses, that it should be a system of truth demonstrated through necessary causes."<sup>7</sup> As further support, he refers to one of Boyle's criteria for an "Excellent Hypothesis", viz., that it is the only hypothesis that can explicate the phenomena, with the implicit assumption that there are such hypotheses. Let us, then, turn to these two pieces of supporting evidence for the Westfall interpretation, and

see if they really go any way to establishing Westfall's claim.

I shall take the second point first. Among Boyle's unpublished papers to which Westfall has drawn our attention,<sup>8</sup> there is one in which Boyle lists the "Requisites of a Good Hypothesis", and also "The Qualities and Conditions of an Excellent Hypothesis."<sup>9</sup>

The requisites of a good hypothesis, Boyle said, are these:-

1. That it be Intelligible.
2. That it contains nothing impossible or manifestly false.
3. That it suppose not anything that is either unintelligible, impossible or absurd.
4. That it be consistent with it self.
5. That it [be] fit and sufficient to explicate the phenomena, especially the chief.
6. That it be at least consistent with the rest of the phenomena it particularly relates to, and do not contradict any other known phenomena of nature, or manifest physical truth."

We can see from this, that, consciously or not, Boyle's position was anti-Cartesian: there is no room in Boyle's conception of science for 'false hypotheses', acceptable as long as they explicated the phenomena. Boyle was committed to the view that the object of scientific work was discovery of the truth, in so far as that was possible. In several respects his conditions for a 'Good Hypothesis' are like the conditions that we have seen Hobbes lay down for a scientific explanation. We can also see that Boyle was not a rigorous Baconian for he layed much more weight on the usefulness

of hypotheses than Bacon would have accepted.<sup>10</sup>

The qualities and conditions of an excellent hypothesis Boyle listed as follows:-

"1. That it be not precarious, but have sufficient grounds in the nature of things itself, or at least be well recommended by some auxiliary proofs.

2. That it be the simplest of all the good ones we are able to frame, at least containing nothing that is superfluous or impertinent.

3. That it be the only hypothesis that can explicate the phenomena, or at least that does explicate them so well.

4. That it enable a skilfull naturalist to foretell future phenomena, by their congruity or incongruity to it; and especially the events of such expts. as are aptly devised to examine it; as things that ought or ought not to be consequent to it."

Boyle presumably held that "an excellent hypothesis" had all the properties of a "good hypothesis" with these four extra ones.

In the list of conditions for "an excellent hypothesis", that of simplicity was not a feature of the Baconian tradition, though we will remember that it was a feature of the criteria employed by the Neoplatonists.

None of the criteria, or even all of them together, is offered as a guarantee of truth for the hypothesis; all that Boyle was offering were reasons for believing a hypothesis is likely to be true, and some reasons, he maintained, are better than others.

When Westfall quotes the third criterion of an excellent hypothesis as indicating that Boyle expected science to give absolute certainty, it is significant that he does not quote the whole of that condition, for the qualification in the second part of it must surely be taken as an indication that Boyle was far from sure that there could be an hypothesis which was the only hypothesis that could explain the phenomena. But, more important, to claim that a particular hypothesis is the only hypothesis that can explain certain phenomena does not necessarily imply that the hypothesis is itself necessarily true, that is, that the hypothesis could not, logically, be false. There is a very straightforward sense in which a particular hypothesis might be accepted as the only hypothesis to explain some phenomena without implying that the hypothesis must be, logically, necessarily true. Thus, if I know that a number of electric light bulbs are connected in series in a circuit, that the circuit is properly connected, and the ammeter shows a positive reading when it is used to test the circuit, then, if the bulbs do not light, I can conclude that the only possible hypothesis to explain the facts is that at least one of the bulbs is faulty. There is, then, a clear sense in which one may talk of there being only one hypothesis to explain the facts which does not imply any commitment to the necessary truth of the hypothesis. Westfall then, is, I believe, mistaken in citing Boyle's third condition as evidence for the view that Boyle ever held that it was possible to identify necessary causes of events.

We shall now return to Westfall's other argument drawn from the passage already quoted from the Sceptical Chemist. For this to count as evidence in favour of Westfall's interpretation of Boyle then Boyle's words "the attainment of undoubted truth" must be interpreted as meaning the attainment of necessary truth. But Westfall offers us no reason for such an interpretation of Boyle's words, and I shall argue that it is much more plausible to support another reading of the phrase.

First, let us note that the passage is very similar to one already quoted from Glanvill (Chapter V, page 141). Glanvill, it will be remembered, was sceptical because scepticism of uncertainties would be an aid, not an hindrance, to truth. Boyle, too, argued for the same role for scepticism in the path to knowledge; true scepticism, Boyle held, was the enemy of natural philosophy, but there was a healthy scepticism which all natural philosophers should show, namely a scepticism of unsubstantiated hypotheses, and it is of hypotheses that Boyle was talking in the Sceptical Chemist.

Next, it is important to realize that what counted as an undoubted truth in the physical sciences was, for Boyle, quite different from what counted as an undoubted truth in metaphysics. He was very clear about this; in his Considerations about the Reconcilableness of Reason and Religion (1675), he explained the three sorts of certainty that he thought there were:-

"There are among philosophers three distinct, whether kinds or degrees, of demonstration. For there is metaphysical demonstration, as we may call that, where the conclusion is manifestly built on those general metaphysical axioms that can never be other than true; such as nihil potest simul esse



& non esse; no entis nullae sunt proprietates reales, etc.  
There are also physical demonstrations, where the conclusion is evidently deduced from physical principles; such as are ex nihilo fit: Nulla substantia in nihilum redigitur, etc. which are not so absolutely certain as the former, because, if there be a God, he may (at least for ought we know) be able to create and annihilate substances; and yet are held unquestionable by the ancient naturalists, who still suppose them in their theories. And lastly there are moral demonstrations such as those, where the proof cogent in its kind or some concurrence of probabilities that it cannot be but allowed, supposing the truth of the most received rules of prudence and principles of practical philosophy." 11

Boyle did not expect natural philosophy to rise above physical demonstration, though, equally clearly, he did hope that it would rise as far as that. And physical demonstrations could never yield necessary truths in any tough sense of that expression, because, as Boyle pointed out, this would require a knowledge of God's intentions, and, even more, though Boyle did not point this out, a Leibnizian type of determinism governing God's choices, which would in fact remove God's freedom of choice. That is, the only way in which physical demonstration could become metaphysical demonstration would be if we knew that all the things which do occur, occur as a matter of logical necessity.

It by no means follows from this, however, that Boyle did not believe the Corpuscular Philosophy to be true; quite certainly he did. And, no doubt, he did so because he believed that it matched up to his criteria of "an excellent hypothesis". Similarly, he accepted the heliocentric theory, which he continued to describe as "the Copernican hypothesis". The reasons which he offered for its acceptance were that it avoided many difficulties of other

hypotheses , it "doth render as good an account as the other" (the Ptolemaic system), it was simpler, and it was supported by the discovery that Jupiter and Saturn are planets circling the sun.<sup>12</sup>

Consistently, Boyle maintained the conjectural status of natural philosophy, and not only, as Westfall implies, in his theological writings. In The Usefulness of Natural Philosophy (1663) Boyle pointed out the dangers of assuming that one's theory did embody the truth:-

"As confidently as many atomists, and other naturalists, presume to know the true and genuine causes of things they attempt to explicate; yet very often the utmost they can attain to, in their explications, is , that the explicated phenomena may be produced after such a manner, as they deliver, but not that they really are so." 13

What is more, even if we cannot find another hypothesis to explain all the phenomena it does not follow that there is no such explanation:-

"For supposing the argument to be conclusive, that either the supposed explication must be allowed, or men can give none at all that is intelligible; I see not what absurdity it were to admit of the consequence. For who has demonstrated to us, that men must be able to explicate all nature's phenomena ... And how will it be proved, that the omniscient God, or that admirable contriver Nature, can exhibit phenomena by no ways, but such as are explicable by the dim reason of man?" 14

Boyle's reference to "that admirable contriver Nature" was a dig at the Aristotelians, many of whom, like the doctors of Moliere's Le Malade Imaginaire, found answers to problems simply by rephrasing the problem and asserting categorically what had been originally interrogative, thus, Why is fire hot? Because it has a hot quality. A whole and very interesting work of Boyle's was devoted to showing

up the absurdity of such forms of explanation.<sup>15</sup>

Boyle, then, was committed to the view that science can arrive at two sorts of explanation. First there were those that could be taken as certainly true (but not necessarily true); second, there were those that were only probable. On this basis all scientific knowledge was hypothetical, and any physical demonstration might turn out either not to have the scope that it was originally thought to have, or even, on further investigation, to be totally false. Boyle understood by a physical demonstration nothing more than that there was substantial empirical evidence for a particular conclusion about the world. This conclusion might be either a statement expressing a regularity such as Archimedes principle - in Boyle's own words: - "That a solid body weighs less in water than in air, by the weight of water equal to the bulk of that body"<sup>16</sup>; or it might be the analysis of a particular property, say heat, in terms of motion.

Even when a particular conclusion was deducible from more general principles, the more general principles could not be accepted as true until and unless they were substantiated by empirical research. Boyle was thus completely out of sympathy with Cartesians who found total confirmation of the general principles solely in terms of their success in accounting for less fundamental ones. Boyle would have been the first to argue that the axioms of any system of physics were not necessarily true. But, given the axioms, established by the best possible experimental means, he believed that it was

possible to demonstrate the properties of various objects. Boyle's paradigm for such inquiries was Galileo's Two New Sciences.<sup>17</sup>

But Boyle never showed any tendency to construe Galileo's findings as anything other than contingent empirical truths.

Boyle was certainly committed to the view that it was possible to establish with at least moral certainty basic explanatory causes of phenomena, but, interestingly, he did not wish to call these Laws of Nature. Laws, Boyle held, were prescriptive, not descriptive, and prescriptions could not be applied to "inanimate bodies." He saw the important point that "The actions of inanimate bodies ..... are produced by powers, not by laws, though the agents, if intelligent, may regulate the exertions of their power by settled rules."<sup>18</sup> For Boyle, the ultimate rules were those ordained by God. And the object of natural philosophy was to discover the rules and the powers which exist in nature, by empirical means. The ultimate physical sources of those powers were thought by Boyle to be unknowable, though we could make plausible suggestions as to what they might be.

The 'rules', which Descartes had called laws, were primary the laws of motion, at this, pre-Newtonian, stage of mechanics, still only imperfectly understood. But, given matter, God could then supply it with any form of motion. The fact that He seemed to give it a motion which could be expressed by quantified 'rules' was an argument in favour of seeing God as the great clock-winder, of seeing the universe as a great machine.

Boyle recognised that our knowledge of the universe was very limited. There was, for example, no reason to believe that the laws of nature (Boyle did sometimes use the term) held for all parts of space. Thus Boyle wrote:-

"Now if we grant, with some modern philosophers, that God has made other worlds besides this of ours, it will be highly probable, that he has there displayed his manifold wisdom in productions very different from those wherein we have admired it .... In these other worlds we may suppose that the original fabric or that frame, into which the omniscient architect at first contrived the parts of their matter, was very different from the structure of our system; besides this, I say, we may conceive, that there may be a vast difference between the subsequent phenomena and productions observable in one of those systems, from what regularly happens in ours, though we should suppose no more, than that two or three laws of local motion, may be differing in those unknown worlds, from the laws that obtain in ours." 19

Such a view is only compatible with construing laws of nature as contingent truths about our world.

Entirely in keeping with this approach, Boyle expressly repudiated Descartes' attempt to deduce the laws of motion from metaphysics. Boyle said that he thought Descartes' laws of motion were generally accepted because Descartes was such a famous mathematician, rather than "any convictive evidence, that accompanies the rules themselves; since to men ..... some of them appear not to be befriended either by clear experience, or any cogent reason."<sup>20</sup> Further, Descartes' proof for his most useful law, viz. that the quantity of motion in the world remains constant "being drawn from the immutability of God, seems very metaphysical, and not very cogent to me, who fears, that the properties and extent of the divine

immutability are not so well known to us mortals, as to allow Cartesius to make it, in our present case, an argument a priori."<sup>21</sup> And although this cannot be taken as a total repudiation of a priori science, nowhere did Boyle ever attempt to deduce physical conclusions from metaphysical premises.

Generally, in contrast to Descartes, Boyle emphasised the limited scope of man's ability to comprehend the solution to all the problems which he can postulate. Thus he said that "we know not the manner of operating, whereby several bodies perform what we well know they bring to pass", for example "the cause of the cohesion of the parts of matter". Nor do we know "whereby the rational soul can exercise any power over the human body", or how the will and the understanding can act upon each other, or how the memory can operate.<sup>22</sup> But the limitations of man's ability did not mean that all knowledge was impossible, and Boyle attempted a rudimentary and rather confused classification of the sorts of knowledge that he believed there were. He held that there was both intuitive knowledge and demonstrative knowledge, and that it was possible for both of these sorts to be either empirical or a priori. Thus he wrote:-

"as the understanding is want to be looked upon as the eye of the mind, so there is analogy between them, that there are some things, that the eye may discern (and does judge of) organically, if I may so speak, that is, by the help of instruments: as when it judges of a line to be straight, by the application of a ruler to it ..... But there are other things, which the eye does perceive, (and judge of) immediately and by intuition, and without the help of organs or instruments; as when by the bare evidence of the perception, it know, that this colour is red, and that other blue ..... For thus there are

some things, that the intellect judges of in a kind of organical way, that is by rules, or hypotheses, such as are a great part of the theorems and conclusions in philosophy and divinity. But there are others which it knows without the help of these rules, more immediately, and as it were, intuitively, by evidence of perception; by which way we know many prime notions and essata or axioms metaphysical, etc. as, that contradictory propositions cannot both be true ..... And it is also upon this evidence of perception, that we receive with an undoubted assent many primitive ideas and notions, such as those of extended substance or body, divisibility, or local motion ....." 23

Boyle's conception of the role of intuition in knowledge is not very clear, and his list of examples is not very helpful. But it is significant that he thought that intuition had a place not only in formal inference, but also in empirical knowledge. Locke in his account of knowledge was to lay considerable weight on intuition, and it is of interest that before Locke's Essay was published, intuition already had a place in English thought on epistemological issues.

Boyle was probably influenced by Descartes in his thinking about intuitive knowledge, as undoubtedly he was in his use of the word 'idea', which, although not a frequent one in his scientific writings, occurs in his philosophical and theological works.<sup>24</sup> It illustrates that the Cartesian influence, which undoubtedly existed in Locke, could have come not only directly from Descartes' own works, but also distilled through the English empirical tradition.

The Corpuscular Philosophy and the distinction between Primary and Secondary Qualities.

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In many ways one of the most influential of Boyle's works was his Origin of Forms and Qualities (1666). In this, Boyle argued against the Aristotelian, or, more accurately, the Scholastic version of Aristotle's theory of the nature of physical properties.

Although the theory which he was attacking was very obscure, Boyle felt that in general outline it could be shown to be unsatisfactory, and that the Corpuscular theory was both easier to understand and was supported by a great deal of empirical evidence.

The scholastic theory which Boyle rejected may be crudely represented as follows: all the properties of objects are due to their innate qualities, and there is a one-to-one correlation between the perceptually-identifiable putative properties of an object and its actual properties; thus an object is blue because it has the property of blueness, or hard because it has the property of hardness, and so on. According to this theory, if it may be so called, when we say that an object is blue because it has the property of blueness, we must be understood to be asserting a causal statement, not, as it might be interpreted, a tautology. The implication of the theory is that we cannot analyse one property, say colour, in terms of some other properties which are not colour properties. But it was just such an analysis, Boyle was to argue, which was in order. Blueness, for example, Boyle would argue, should be analysed as a product of an interaction between light, object, and observer, rather than being thought of as an irreducible property of an object.

The Origin of Forms and Qualities is divided into two sections.

The first, Boyle called the theoretical part, and in it he outlined the arguments which he believed supported his case for qualities. In the second, the "historical part", he produced the experimental evidence for his account. Significantly, it is the experiments which Boyle believed clinched the case in his favour: "But in the second or historical part of it [the book] he [the reader] will be invited to grant that we have done that part of physicks we have been treating of some little service; since by the lovers of real learning it was very much wished that the doctrines of the new philosophy (as it is called) were backed by particular experiments; the want of which I have endeavoured to supply"²⁵

The theory of the origin of forms and qualities, therefore, for which Boyle argued, was a physical hypothesis, justified by the test of experiment, and not, in any straight-forward way, an epistemological theory to be judged entirely by a priori criteria. But, this granted, Boyle was quite willing to allow that his case rested upon certain assumptions. The most important of these he expressed right at the beginning of the historical part:-

"I agree with the generality of philosophers so far as to allow, that there is one catholick or universal matter common to all bodies, by which I mean a substance extended, divisible, and impenetrable." ²⁶

Boyle did not attempt to justify this assumption, he takes it to be the common ground between himself and his Aristotelian opponents.

Because Boyle included impenetrability as a fundamental property of matter, his assumption was at odds with the Cartesian conception of the physical world, a point which Boyle could hardly have failed to notice, and given the prevalence of Cartesian ideas, it is surprising that Boyle did not make any attempt to argue for his view.

The assumption which Boyle makes divides in fact into two; the first is that there is a universal matter common to all bodies, the second is that the nature or essence of bodies is extension, divisibility, and impenetrability. It could be argued that both these assumptions are justified by experience: all the objects that we are willing to call bodies do in fact have these properties, and anybody who did deny the general propositions would be flying in the face of overwhelming empirical evidence. No doubt, if he had been challenged, it would have been such a justification which Boyle would have offered, but it was not in fact until the work of Locke and Newton that these considerations were in fact offered.

Following from his first assumption Boyle had a second:-

"But because this matter being in its own nature but one, the diversity we see in bodies must necessarily arise from somewhat else than the matter they consist of. And since we see not how there could be any change in matter, if all its (actual or designable) parts were perpetually at rest among themselves, it will follow that to discriminate the catholick matter into variety of natural bodies, it must have motion in some or all its designable parts: and that motion must have various tendencies, that which is in this part of the matter tending one way, and that which is in that part tending another; as we plainly see in the universe or general mass of matter, there is really a great quantity of motion, and that variously determined, and that yet divers portions of matter are at rest."27

Motion, then, Boyle identified as the cause of the differences between various parts of matter. It was, Boyle admitted, only an hypothesis, but one for which he intended to offer considerable evidence; "Local motion" he said, "seems to be indeed the principal amongst second causes, and the grand agent of all that happens in nature"²⁸

Matter and motion, then, Boyle held, are the two "most grand and catholick principles of bodies", and all matter had the basic properties of magnitude or size, shape, and either motion or rest. These primary properties of matter were, Boyle held, to be distinguished from "those less simple qualities (as colours, tastes, and odours) that do belong to the body on their account."²⁹ The "less simple" qualities are less simple because they are a product of an interaction, as we have already seen, and are not inherent in objects in any, straight-forward, and, indeed, simple sense.

To substantiate the claim that secondary properties cannot be thought of as simple properties, Boyle offered many examples which he intended to illustrate the difficulties inherent in the Scholastic view. Thus he pointed out that modern goldsmiths use as a criterion for gold the fact that it will dissolve in aqua regis, but not in aqua fortis, "yet these attributes are not in the gold anything distinct from its perculiar texture, nor is the gold we have now of any other nature than it was in Pliny's time, when aqua fortis and aqua regis had not been found out And this example I have rather pitched upon, because it affords me an opportunity to

represent, that unless we admit the doctrine I have been proposing, we must admit, that a body must have an almost infinite number of new real entities accruing to it without the intervention of any physical change in the body itself."³⁰

Boyle's argument is that if we accept the Scholastic account, then we have to postulate a corresponding property, as a real entity of the object, for every property that an object manifests, and that this is surely implausible. Rather, Boyle held, it was much more reasonable to assume that the newly-discovered property was a product of the interaction of the parts of the body and, in this case, aqua regis or aqua fortis.

Boyle also pointed out that we should not be surprised that the great variety of properties which objects appear to have can be accounted for on the assumption of the theory he propounded, "For we must consider each body, not barely, as it is in itself, an entire and distinct portion of matter, but as it is a part of the universe, and consequently placed among a great number and variety of other bodies, upon which it may act, and by which it may be acted on, in many ways each of which men are wont to fancy as a distinct power or quality in the body"³¹

When Boyle distinguished between the primary and secondary qualities of size, shape, and rest or motion, and the secondary qualities of colour, taste, and smell, he did not make the mistake of denying that it is perfectly correct to talk of bodies having a colour or being coloured. He did not, that is, construe the

secondary qualities as being subjective - to use a far from satisfactory word. He explained the point like this:-

"But there I foresee a difficulty, which being perhaps the chiefest that we shall meet with against the corpuscular hypothesis, it will deserve to be, before we proceed any farther, taken notice of. And it is this, that whereas we explicate colours, odours, and the like sensible qualities by a relation to our senses, it seems evident that they have an absolute being irrelative to us: for snow (for instance) would be white, and a glowing coal would be hot, though there were no man or animal in the world. And it is plain that bodies do not only by their qualities work upon other, and those inanimate, bodies; as the coal will not only heat or burn a man's hand if he touch it, but would likewise heat wax (even so much as to melt it and make it flow) and thaw ice into water, although all the men and sensitive beings in the world were annihilated." 32

To this difficulty Boyle offered the correct answer. The secondary qualities, he said, must be construed as dispositional qualities of objects, not simply subjective properties. For Boyle, the secondary qualities were not, as they were for Galileo, the names for sensations only. They were the names of real properties, although they did not have to manifest themselves on all occasions. "Bodies", he wrote, "may be said in a very favourable sense to have those qualities we call sensible, though there were no animals in the world: for a body in that case may differ from those bodies which now are quite devoid of quality, in its having such a disposition of its constituent corpuscles, that in case it were duly applied to the sensory of an animal, it would produce such a sensible quality which a body of another texture would not

And so we say, that a lute is in tune whether it be exactly played

upon or no, if the strings be all so duly stretched as that it would appear to be in tune, if it were played upon."³³

It is important to note that Boyle's acceptance of the objectivity of secondary qualities is linked with a denial of any similarity between the sensations which an observer has and the quality in the object. That is, whilst Boyle accepted that it is correct to talk of snow being white, he denied that there is a property 'whiteness' in the snow which is anything over and above the arrangement of the parts of snow which cause it to reflect light in a particular way. Thus, for Boyle, to say that snow is white is a shorthand way of saying that the matter which is called snow has the dispositional property of reflecting, say, sunlight, in such a way as to cause an observer with normal vision to see white.

Much of the "speculative" first part of The Origin of Forms and Qualities is devoted to an attack on the Scholastic doctrine of substantial forms. Boyle expressed the difference between him and the Peripatetics like this:-

"the sum of the controversy betwixt us and the schools is this, whether or no the forms of natural things (the souls of men always excepted) be in generation educed, as they speak, out of the power of the matter, and whether these forms be true substantial entities, distinct from the other substantial principle of natural bodies, namely matter." 34

Boyle rejected this view of the relation between matter and form. Form, he argued, could not in any sense be separated from matter, and, indeed, there was no need to introduce any such concept as sub-

stantial form at all: "matter and the accidents of matter being sufficient to explicate as much of the phenomena of nature as we either do or are like to understand." ³⁵ His own view was that the form of a natural body was "a convention of the bigness, shape, motion (or rest) situation and contexture (together with the resulting qualities) of the small parts that compose the body, as is necessary to constitute and denominate such a particular body."³⁶

The form of a body was, then, for Boyle nothing but its properties, and since most of those properties were dispositional, and could therefore be explained by reference to the primary properties, the form of an object was its parts and their movements. He supported this interpretation by citing various experiments. These experiments established that one substance could be changed into another by chemical means. Boyle claimed that such a result would be impossible on the theory of substantial forms.

Connected with the substantial forms theory was the doctrine of 'real species'. Rather than there being any such real species, Boyle argued, "it was very much by a kind of tacit agreement that men had distinguished the species of bodies, and that those distinctions were more arbitrary than we are wont to be aware of".³⁷ Distinct species as commonly accepted, were often just those "that have had the luck to have distinct names found out for them."³⁸ This view of species was to be much more fully advocated by Locke.

In the 'historical part' of the work Boyle listed many experiments which he believed were incompatible with the Scholastic

account of forms and qualities, but which he believed could be easily accounted for on his own theory. Some of these were very simple, thus, by beating an egg white it is possible to change its properties, illustrating that mechanical action alone can in some cases produce a change of properties; others were much more complicated, drawing on Boyle's considerable alchemical interests. The total effect is to produce a very large body of evidence which could not be accounted for (or at least was not) by any current hypotheses other than the mechanistic one that Boyle advocated.

We have spent some little time in examining Boyle's views on matter, not only because they are of interest in themselves, but because an appreciation of them is essential for a correct understanding of Locke's epistemology. They represent, more clearly than the works of any other thinker, the context in which Locke was operating when he wrote the Essay. Boyle was the advocate of a theory about the nature of chemical and physical change which was accepted by Locke as being probably true, if not all of it was actually proven. The actual extent of Locke's debt we shall explore later.

Conclusions.

We have seen that Boyle was committed to an empiricist account of science in several particulars. Not only did he accept that theory should always be tested against experience, but he also believed that theory should take as its starting point assumptions

or hypotheses which were drawn from experience. In this he was totally opposed to the Cartesian tradition of science, and a follower of Bacon. But, unlike Bacon, Boyle was not committed to the view that science could arrive at necessary truths about the world - if, indeed, Bacon himself was ever so committed. Boyle believed that scientific certainty could never rise above physical demonstration, and the truths of science could never be known with that certainty found in metaphysics.

The contrast between Boyle and the Cartesians is perhaps nowhere better illustrated than in the conflict of approach to nature in letters exchanged between Henry Oldenburg, (representing Boyle), and Spinoza.³⁹ Constantly, in this correspondence, Boyle insisted that science must be grounded upon experimentally justified premises. Spinoza, on the other hand, following Descartes, was concerned to ground science upon metaphysical axioms, and to justify the conclusions of science by logical argument rather than empirical test.

If Bacon had freed science from an unhealthy awe of established authority, and had set it on the path towards an enlightened empiricism, then it was Boyle who showed, more than anybody before Newton, what the fruits of that empiricism could be. Most particularly, Boyle supplied the evidence and the argument for an empirically grounded account of change in physical objects. Although it was Galileo, Descartes, and, finally, Newton, who produced the physicist's understanding of motion, it was to a

very large extent Boyle's achievement to give an account of matter.

Although Boyle wrote many works which touched on epistemological problems, and although his whole approach to science presupposed a theory of knowledge, he never devoted a work exclusively to that issue. Boyle's epistemological theory has to be inferred from his theological and scientific writings. This is in itself significant. It was science and religion which forced into the open considerations of a general epistemological nature. But, as yet, no single work since Descartes' Discourse of Method had been concerned exclusively with epistemological problems. There was clearly a need for a general account of the nature of knowledge, which, whilst taking account of the achievements of science, would also place science within a wider epistemological framework. The time was ripe for the production of an essay concerning human understanding.

Chapter VII

Newton's Conception of Science

Introduction.

The zenith of the scientific revolution was reached in 1687 with the publication of Newton's Philosophiæ Naturalis Principia Mathematica. Newton's work was important not only because it accounted for a large number of phenomena, both on the earth and in the heavens, and enabled man to predict an indefinite number of physical events; it was also the vindication of one tradition over another in natural science. It was the triumph of the method of Bacon and Boyle over that of Descartes. What is more, the victory was achieved in an area in which the rationalist tradition was reckoned to be at its strongest, in the cultivation "of mathematics as far as it relates to philosophy".¹ The method which Boyle and others had recommended and followed in the pursuit of an understanding of matter, was generalized by Newton to cover both matter and motion, the two first principles of the physical world as conceived by Cartesian and post-Cartesian science.

Newton, of course, was lucky, as all great scientists are; much of the groundwork for his achievement had been accomplished by other men. Not only had Kepler, Galileo, and Descartes discovered mechanical laws which applied to particular parts of the universe, but Descartes especially, had shown the power of a mechanistic interpretation of nature. By the time that Newton turned to the

problems of the universe the conceptual victory had been won; the cosmos was already conceived as a vast machine. Mechanical explanations were accepted not only for macrocosmic phenomena, but also for the microcosmic; and since Harvey's discovery of the circulation of the blood, for the human body also.

We shall not consider Newton's achievements in detail. Once again, the main emphasis will be on method. As I have already suggested, we shall find that largely Newton continued the tradition of English science which had developed during the preceding half-century.

There was a negative aspect to Newton's achievement which it is important to note. Nothing that Newton produced helped to break the hold of two Cartesian doctrines on either English or Continental thought, at least in the immediate post-Newtonian period. These were Descartes' paradigms of knowledge, which were the propositions of Euclidean geometry; and Descartes' fundamental distinction between mind and body. Indeed, if anything, Newton's work consolidated these two aspects of Cartesian thought within the European tradition. So, if Newton's achievement in vindicating empiricism in science was a victory over Cartesian rationalism, it was not a victory over the Cartesian world-view.

This said, however, an important qualification is in order. Whilst it was certainly true that the philosophical paradigm for knowledge remained the propositions of mathematics, the practice of scientists was generally to accept as knowledge propositions

about the world which had no such comparable certainty. The success of science in explaining and predicting was to generate an intellectual schizophrenia in which the Cartesian paradigm played Hyde to the scientists' Dr. Jeckyll. Nowhere did this confusion emerge more clearly than in Locke's Essay.

Newton, certainty, and hypotheses.

On February 8th 1671/72 a paper was read before the Royal Society from Mr. Isaac Newton containing his new theory about light and colours.² The paper argued that white light was a mixture of rays of different colours which each had their own particular refractive index as against those who maintained that, for example, white light was itself a simple and not a compound. The paper's importance in the history of optics is central, but its immediate impact was to cause controversy, not helped by Newton's own response, which, sometimes at least, did less than justice to his critics.³

In the paper Newton claimed to show that his account of light was not an unsubstantiated hypothesis, a conjecture drawn from the skies, but a well established fact. He made his own position very clear in reply to one of his critics:-

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

the test of truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised which shall seem to overcome new difficulties. Hence it has been here thought necessary to lay aside all hypotheses, as foreign to the purpose, that the force of the objection should be abstractly considered, and receive a more full and general answer." 4

At this comparatively early stage in Newton's career we find him clear about what the method was for science. He was concerned to find certainty, and he was antagonistic to at least one sort of hypothesis because their employment will lead neither to certainty nor truth. But what did Newton understand by certainty, and what exactly were the sort or sorts of hypothesis to which he objected? Let us turn first to Newton's conception of certainty.

Newton explained his position with regard to the certainty of physical science, and optics in particular, in his famous letter to Oldenburg following the controversy over his papers on light and colours. There Newton wrote:-

".... I said indeed, that the science of colours was mathematical and as certain as any other part of optics; but who knows not that optics, and many other mathematical sciences, depend as well on mathematical demonstration? And the absolute certainty of a science cannot exceed the certainty of its principles. Now the evidence by which I asserted the propositions of colours is in the next words expressed to be from experiments, and so but physical, whence the propositions themselves can be esteemed no more than physical principles of a science. And if these principles be such that on them a mathematician may determine all the phenomena of colours that can be caused by refractions and that, by disputing or demonstrating after what manner and how much, those refractions do separate or mingle the rays in which several colours are originally inherent, I suppose the science of colours will be granted mathematical and as certain as any part of optics. And this may be done, I have good reason to believe, because ever since I became first acquainted with these principles I have, with constant success in the events, made use of them for this purpose." 5

To say that the optical discoveries he had made were certain, Newton argued, is to say that they had been comprehensively confirmed by experiment. White light, Newton maintained, had been shown experimentally to consist of rays differently refrangible, and to the same degree of refrangibility ever belonged the same colour. But Newton claimed no other certainty for this conclusion than physical certainty. It was, simply, a brute fact about white light. It was, for Newton, a contingent true empirical proposition; and physics was built upon such foundations.

We can see, then, that Newton at this stage did not expect anything grander from science than Boyle had claimed for his "physical demonstration". Certainty in science did not for Newton mean anything more than well-tested physical fact. Whether or not Newton was correct to see all of his propositions as physical facts, or whether they embodied hidden hypotheses, is, for the moment, beside the point.

The same outlook was exhibited by Newton in his later works. Thus in the Principia Newton explained again that the starting point for arriving at truths about the physical world was empirical observation. In the General Scholium, added to the second edition of 1713, Newton wrote: "In this philosophy [i.e. experimental philosophy] particular propositions are inferred from the phenomena and afterwards rendered general by induction";⁶ and he went on to list the basic facts about bodies which had been arrived at by this method, and which were the bedrock upon which the Principia was built. "Thus it was", he wrote, "that the impenetrability, the

mobility, and the impulsive forces, and the laws of motion and of gravitation, were discovered."⁷

Newton was fully aware that this method did not guarantee any sort of metaphysical certainty. The kind of certainty that was generated he illustrated in his 'Rules of Reasoning in Philosophy' which he placed at the beginning of Book III of the Principia. These rules are of fundamental importance to an understanding of Newton's position, and we shall return to them later.

The first rule reads:-

"We are to admit no more causes of material things than such as are both true and sufficient to explain their appearances." 8

Newton thus had two conditions which had to be satisfied for the identification of a cause. The first condition was truth, and what Newton meant by a 'true cause' was not simply one that might be true, but one that was known to be true; and to know that a cause was a true cause required having substantial empirical evidence for it. The second condition is a criterion of simplicity in explanations. "For nature", Newton wrote in justification, "is pleased with simplicity, and affects not the pomp of superfluous causes."

It might be thought that this second condition was nothing more than a pious hope on Newton's part. But I think that in fact it was more than this. He would, I think, have justified the condition of simplicity on inductive grounds. If pressed, he would have said that as a matter of fact the simplest explanation always has turned out to be the true explanation, and for this reason we are entitled to accept it as a working principle in science. Whether or not this

is true of scientific explanations, and if it is true, in what sense we are to understand the concept of simplicity is another question, though undoubtedly an important one in the philosophy of science.

There could be another explanation for Newton's commitment to simplicity. It is possible that Newton believed, for other than empirical reasons, that "Nature is pleased with simplicity". He might have believed that God would not have created anything other than the most simple system consistent with the phenomena. If this was Newton's reason for accepting the principle of simplicity, then it would appear that right at the heart of Newton's scientific method there was a theological element.

If Newton believed that the rule of simplicity could be justified inductively, then he would be wrong to think that the criterion of simplicity could justify induction, for clearly this would involve a circularity. Yet in the Rules of Reasoning this was exactly what Newton did, for the second Rule reads:-

"Therefore to the same natural effects we must, as far as possible, assign the same causes."⁹ (My emphasis.) What Newton should have done, I am suggesting, is to have justified the criterion of simplicity by reference to induction, and thus make the inductive principle the more fundamental. If Newton had done so, then there would have been no need to speculate as to whether or not there were any theological elements involved in the Rules of Reasoning.¹⁰

If Newton had followed this order rather than the one he did, then he would still have been faced with an unsupported first Rule, namely, the principle of induction, but this would have been consistent with his whole approach to nature in which inductive arguments, in practice, were much more dominant than appeals to a principle of simplicity. Thus, induction was fundamental to Newton's two further rules which read as follows:-

Rule III. "The qualities of bodies which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever." 11

Rule IV. "In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypothesis that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate or liable to exceptions." 12

Induction from particular phenomena was fundamental to the whole of Newton's scientific method. But Newton was well aware that induction did not guarantee truth. It is just this point that is brought out by Rule IV. If induction did guarantee truth then there would be no possibility of further phenomena occurring which would make induction "more accurate or liable to exceptions."

Newton, then, was committed to the total contingency of experimental science, and committed, also, to the corresponding lack of absolute certainty in the findings of the natural scientist. But he did want to draw a sharp distinction between empirical fact, whether generalized by induction or not, and speculative hypotheses.

It is to this second question of ours, what did Newton understand by hypotheses, that we must now turn.

Newton's use of the word 'hypothesis' has been the subject of much debate. Koyré has drawn our attention to the fact that the first edition of the Principia made much more frequent use of the term than subsequent editions,¹³ and he has suggested that Newton construed the term in two senses, a good, and a bad.¹⁴ Koyré was undoubtedly correct about this, and it will be worth our while to identify these two senses.

In its traditional sense a hypothesis was an assumption or a fundamental supposition of a theory. Any theory will have such hypotheses, and Newton's was no exception. But, from what we have already seen of Newton's views, only certain sorts of assumption were for him permissible, namely those based upon empirical observation in the way we have considered. Other forms were not permissible, and it was these other forms to which Newton took such strong exception. On some occasions Newton actually restricted the word 'hypothesis' to its use to apply to bad hypotheses. Thus he wrote to Roger Cotes in 1713 discussing the second edition of the Principia:-

"I had yours of Feb. 18th and the difficulty you mention which lies in these words, "since every attraction is mutual", is removed by considering that, as in geometry, the word 'hypothesis' is not taken in so large a sense as to include the axioms and postulates; so in experimental philosophy, it is not to be taken in so large a sense as to include the first principle or axioms, which I call the laws of motion. These principles are deduced from phenomena and made general by induction, which is the highest evidence that a proposition can have in this philosophy. And the word 'hypothesis' is here

used by me to signify only such a proposition as is not a phenomenon nor deduced from any phenomena, but assumed or supposed - without any experimental proof

"And for preventing exceptions against the use of the word 'hypothesis', I desire you to conclude the next paragraph in this manner: "For anything which is not deduced from phenomena ought to be called a hypothesis, and hypotheses of this kind, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy" 15

According to Newton, then, "Hypotheses of this kind" were bad hypotheses, and were not to be accepted in experimental philosophy.

But surely, it has been urged, Newton himself, especially in the Queries added to the second edition of the Opticks (1717), did in fact propose various hypotheses, about the nature of light, the cause of gravity, and other topics. How was it possible for a man to propose such hypotheses, and yet to eschew them, as Newton had done, not only in the Principia, but also in the first sentence of the Opticks itself, where he had written: "My design in this book is not to explain the properties of light by hypotheses, but to propose and prove them by reason and experiments." 16

The answer is, I believe, very simple. When Newton was concerned to produce what he held could be shown to be true, he eschewed hypotheses: the Principia, the Opticks, excluding the Queries, were such. The Queries were not; they could not yet be proved, and could not, therefore, be proposed as true. Newton explained his position at the end of the 31st Query:-

"As in mathematics, so in natural philosophy, the investigation of difficult things by the method of analysis, ought ever to precede the method of composition. The analysis consists in making experiments and observations, and in drawing

general conclusions from them by induction, and admitting of no objections against the conclusions, but such as are taken from experiments, or other certain truths. For hypotheses are not to be regarded in experimental philosophy. And although the arguing from experiments and observations by induction be no demonstration of general conclusions; yet it is the best way of arguing which the nature of things admits of, and may be looked upon as so much the stronger, by how much the induction is more general. And if no exception shall occur from phenomena, the conclusion may be pronounced generally. But if any time afterwards any exception shall occur from experiments, it may then begin to be pronounced with such exceptions as occur. By this way of analysis we may proceed from compounds to ingredients, and from motions to the forces producing them; and in general, from effects to their causes, and from particular causes to more general ones, till the argument end in the most general. This is the method of analysis: And the synthesis consists in assuming the causes discovered, and established as principles, and by them explaining the phenomena proceeding from them, and proving the explanations."17

Having thus stated his general position, Newton then went on to point out how he had followed this method in the Opticks. But in the third book, he explained, the book that contained the Queries, "I have only begun the analysis of what remains to be discovered about light and its effects upon the frame of nature, hinting several things about it, and leaving the hints to be examined and improved by the farther experiments and observations of such as are inquisitive"18 The third book of the Opticks, that is, largely contained suggestions which Newton hoped could be resolved by further experiment. The Queries were a series of hypotheses, in a third sense of that term; they were hypotheses which Newton hoped would suggest experiments to discover whether or not they were true. (Cf. Newton's remarks of 1672 quoted above, pages 183-4).

So considered, we can see that the method of analysis which Newton recommended was not to be carried out in an arbitrary way. It would consist in conducting experiments, some at least of which would be attempts to verify (or falsify - Newton was Baconian enough to recognise the significance of falsification in science) various conjectures such as those contained in the Queries.

There were, then, two senses of the word "Hypothesis" to which Newton did not object. These were, first, hypotheses understood as first principles which had the support of experiment to justify their use as first principles; second, hypotheses which suggested experiments which could lead to the discovery of facts, themselves supported by experiment and observation.

The type of hypotheses, as we have seen, that Newton objected to, were unsubstantiated conjectures, used to supply a theoretical explanation of the phenomena, which might be true, but, for all the scientists knew, might equally well be false. And, undoubtedly, Newton had especially in mind Descartes' Principles of Philosophy, in which Descartes appeared to be content to accept even false hypotheses within his system as long as their assumption led to a coherent explanation of the phenomena.¹⁹ For Newton, the goal of science was the discovery of truth, and although he recognised that the degree of certainty possible in the natural sciences was not equivalent to that obtainable in mathematics, he did believe that it could be, and indeed must be, higher than the sort of confirmation supplied by a theory or hypothesis simply because it succeeded in

accounting for the phenomena. Newton, that is, held that the expectation of the scientist could and should be higher than the expectation which Osiander or Hobbes had offered. He believed that the work of Descartes, in so far as it only succeeded in accounting for the phenomena, without empirical confirmation of its suggested laws and physical causes, was no better than work in progress, and he strongly objected to those who believed that such work in progress was the end of science, for he held that it was merely the beginning of the analysis, merely 'Queries', which had yet to be supplied with their justification.

Newton's opposition to the Cartesian approach to the physical world can be illustrated by considering the contrast between Newton's position and that of the moderate Cartesian, Rohault. The difference can be brought out by comparing Newton's Rules of Reasoning with one of Rohault's methodological principles. Rohault had written:-

"If that which we fix upon to explain the particular nature of any thing, do not account clearly and plainly for every property of that thing, or if it be evidently contradicted by any one experiment; then we are to look upon our conjecture as false; but if it perfectly agrees with all the properties of the thing, then we may esteem it well grounded, and it may pass for very probable." 20

Rohault, like Newton, clearly did not expect certainty, and placed great importance on the agreement between experiment and theory. But such superficial similarities in fact cover a gigantic difference of approach between the two men. For Rohault, the cause which we are entitled to assume to be probable need not be empirically

observable; any conjectured cause, as long as it was conformable to all the data, was allowable. In contrast, as we have seen, Newton demanded that causes themselves must be in principle identifiable, and must in fact be so identified, if we are to talk at all of our explanation being true. Newton would have agreed that we could not always so identify conjectured causes, but in so far as this was so, then it was a part of science to try to find ways in which those causes could be brought under the umbrella of observation, by, for example, the invention of such things as telescopes and microscopes.

It was this sort of point which lay at the centre of the controversy between Newton and Leibniz over the nature of gravity. Leibniz claimed that Newton's gravity was "a chimerical thing, a scholastic occult quality."²¹ To this Clarke, on behalf of Newton, replied:-

"That the sun attracts the earth, through the intermediate void space; that is, that the earth and sun gravitate towards each other, or tend (whatever be the cause of that tendency) towards each other, with a force which is in a direct proportion to their masses, or magnitudes and densities together, and in an inverse duplicate proportion of their distances; and that the space betwixt them is void, that is has nothing in it which sensibly resists the motion of bodies passing transversely through: all this, is nothing but a phenomena, [*sic.*] or actual matter of fact, found by experience. That this phenomena is not produced sans moyen, that is without some cause capable of producing such an effect; is undoubtedly true. Philosophers therefore may search after and discover that cause if they can; be it mechanical or not mechanical. But if they cannot discover the cause; is therefore the effect itself, the phenomenon, or the matter of fact discovered by experience (which is all that is meant by the words attraction and gravitation) ever the less true?" 22

Newton was prepared to allow, by his second Rule of Reasoning, that the cause of gravitation would be the same for all objects, that all objects tended to move towards each other according to the inverse square law, as objects near the earth could be observed to move towards the earth according to the law. But what caused them to do so he did not pretend to know, though he did venture to suggest to Boyle in a letter that the ether might be the cause,²³ a suggestion which Newton made again in the Queries.²⁴

No doubt Newton believed that truth and certainty in science were worth pursuing for their own sake, but he also had another reason. He believed that the perfection of natural philosophy by the method he propounded would lead to a greater knowledge of God, and thus to a greater awareness of our moral duty. Thus, he concluded the Opticks by saying that "so far as we can know by natural philosophy what is the first Cause, what power he has over us, and what benefits we receive from him, so far our duty towards him, as well as that towards one another, will appear to us by the Light of Nature."²⁵ Natural philosophy, as propounded by Newton, led both to a knowledge of God, and of man's moral responsibilities. Sciences required truth, therefore, not only for its own sake, but also because through it it was possible to arrive at theological and moral knowledge. And since science rested on experience, experience was the key not only to the world but also to God. As Newton wrote to Richard Bentley in 1692, "When I wrote my treatise about our system I had an eye upon such principles as might work

with considering men for the belief of a Deity, and nothing can rejoice me more than to find it useful for that purpose."²⁶ It was not possible, Newton believed, to account for the Universe except on the assumption of a God,²⁷ and it was the Argument from Design, or, more properly, the Argument to Design²⁸ to which Newton often appealed to justify his belief in God.²⁹

It is worth noting that Newton's position with regard to the justification of God's existence, and its relation to his science, is entirely contrary to that of Descartes. Descartes justified his science in terms of the existence of God, known a priori; Newton, like Boyle, justified his belief in God in terms of his science. Perhaps nothing better exemplifies the difference between the two men.

There is a further aspect to the link between Newton's science and his theology; it is that Newton's argument for the existence of God is in fact in conflict with what he maintained about hypotheses. If Newton were to be consistent, he could not infer or postulate a God to account for the order which he had discovered in the universe, because, ex hypothesi, this would be to account for the phenomena by appeal to an hypothesis not substantiated on independent grounds. As Newton said of gravity "..... the cause of gravity I do not pretend to know"³⁰ so he should have said about the cause of order in the universe.

On this basis, it can be seen that David Hume's attack on the Argument from Design in the first Inquiry was an attempt to

apply consistently Newton's own criteria for rational belief. It was the fate of Newton's own achievement that it was to be used to undermine the very belief that he thought his work substantiated.

Newton's Theory of Matter.

Newton was no exception to the general run of seventeenth century scientists in maintaining a distinction between the primary and secondary qualities of matter. But Newton's formulation of the distinction was in important respects different from those of earlier scientists. For Newton, the distinction was based entirely on empirical considerations, together with the ubiquitous principle of induction.

Newton expressed his version of the distinction in the third Rule of Reasoning of the second edition of the Principia as follows:-

"The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever." 31

In his justification and explanation of this Rule, Newton was totally and unequivocally empiricist. The qualities of bodies, he asserted, "are only known to us by experiments, and such [qualities] as are not liable to diminution can never be quite taken away."³² It is, therefore, only empirically known properties that can be attributed to objects; and all properties which are found in all bodies, can, by induction, be assumed to be the universal qualities of all bodies whatsoever.

But what exactly did Newton understand by his first condition? What are qualities "which admit neither intensification nor remission of degrees"? Newton explained what he meant when he was considering whether or not gravity was a universal quality of bodies. The gravity of an object, Newton explained, was not a quality in the relevant respect, because the gravity of bodies "is diminished as they recede from the earth". In other words, what an object weighs is entirely dependent on its location in respect to the earth, and, therefore, is not a quality which is immutable. The properties or qualities which Newton identified as the primary qualities were: extension, hardness, impenetrability, mobility, and inertia. All these properties, he claimed, are ones we know by experience that all physical objects have. By omission, we are left to conclude that colour, taste, and odour were not either universal, or they were considered by Newton to admit of intensification and remission of degrees.

There is one other property which Newton also considered for his list of qualities; but, significantly, he did not include it with them, and that was divisibility. Surely, it can be argued, divisibility is a quality which satisfies Newton's criteria. Why did he not include it?

After listing the properties that he took to be "the foundation of all philosophy", Newton continued:-

"Moreover, that the divided but contiguous particles of bodies may be separated from one another is a matter of observation; and in the particles that remain undivided, our minds are able to distinguish yet lesser parts, as is mathematically demonstrated. But whether the parts so distinguished and not yet divided may, by the powers of Nature, be actually divided and separated from one another we cannot certainly determine. Yet had we the proof of but one experiment that any undivided particle, in breaking a hard and solid body, suffered a division, we might by virtue of this rule [i.e. Rule III] conclude that the undivided as well as the divided particles may be divided and actually separated to infinity." 33

But surely, many experiments do establish that a particle can be so divided; often the collision between two objects has just the effect that Newton is looking for, and Newton could not have been unaware of this. Either, then, Newton meant by 'particle' something other than 'small body', or he was deliberately overlooking the empirical evidence.

The explanation of Newton's position seems probably to lie in the fact that Newton, on other grounds, strongly suspected that matter was not capable of infinite divisibility. It was this position which he expressed at the end of the Queries to the Opticks where he wrote:-

".... it seems probable to me, that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures, and with such other properties, and in such proportion to space, as most conduced to the end for which he formed them; and that these primitive particles being solids, are incomparably harder than any porous bodies compounded of them; even so very hard, as never to break in pieces; no ordinary power being able to divide what God himself made on in the first creation." 34

Therefore, Newton held, "that Nature may be lasting, the changes of corporeal things are to be placed only in the various separations

and new associations and motions of these permanent particles."³⁵

It would appear from this that Newton held divisibility not to be a primary quality of bodies because he was committed to an atomic view of matter which itself held that atoms were not capable of physical division. Further, at least a part of Newton's reasons for accepting such an atomic view of matter were connected with his conception of the original creation of the universe. This does not imply that Newton's conception of the creation was derived from his theological preconceptions alone. Undoubtedly they were drawn out as an extension of his physics and chemistry. But it is interesting that it is possible to trace this link between his physics and his theology in the fairly austere environment of the Rules of Reasoning.

The Limits of Newtonian Empiricism.

We have seen that Newton constantly asserted a link between empirical evidence and grounds for accepting an hypothesis as true. But it has been often pointed out that Newton's work contains several assumptions which either were not, or could not be, grounded on empirical evidence. It says much for Newton's influence that these assumptions were largely taken over by both science and western society generally for the next two hundred years. The metaphysics of Newton became the metaphysics of the world.

I do not intend to examine these assumptions in detail; to some of them we shall return in the second part of this work; but it will be worth listing them. Some of Newton's assumptions we can now see to be false; others cannot so easily be dismissed;

still others are indispensable for any conceptual scheme that is to do justice to our experience. Many, but not all, of Newton's assumptions are to be found in Locke's theory of knowledge.

Clearly, fundamental to Newton's physics was a belief in matter. Linked with this was a belief in the existence of motion. Both of these assumptions Newton took to be hard facts, beyond dispute, about the world. Despite this, both of these assumptions were immediately challenged, by Leibniz and Berkeley, who each presented powerful and persuasive arguments for their rejection.

Part, at least, of Berkeley's attack on Newton's conception of the world took as its starting point another assumption to be found in Newton's writings, especially in the Opticks.³⁶ It was that Newton took for granted a sharp distinction between mind and matter. Although it is unclear that Newton's views on the nature of mind were the same as Descartes' there is at least some evidence to show that they were probably very similar to those of Locke, who also made this sharp distinction.³⁷ In several passages in the Opticks Newton put forward a causal account of perception which implied such a distinction between mind and body.³⁸

Newton's account of perception was only one of the areas of science in which Newton employed causal explanations; clearly the whole of his physics assumed the viability of such explanations. But the concept of causation was one which had received remarkably little attention from either philosophers or physicists. Again it was Berkeley, and, more importantly, Hume, in the period following

Newton's acceptance, who turned their attention to this concept.³⁹

Fundamental, too, to Newton's whole method was the viability of inductive argument. As we have already seen, all of Newton's Rules of Reasoning were connected with, or employed inductive inference, with the possible exception of the first. But, perhaps wisely, Newton never attempted to offer a justification of this form of inference. All he did was to maintain that there was no other method open to the scientist by which he may proceed. He certainly never made the mistake of thinking that inductive inference either did, or could, approach the certainty of deductive reasoning. Once again, it was Hume who most clearly recognised the central importance of inductive inference, and subjected it to critical analysis. As an issue in the philosophy of science it has remained central ever since.

Another feature of the Principia which is generally reckoned an important part of Newton's metaphysics was the commitment to absolute space and time. Undoubtedly one of the reasons why Newton accepted these notions was theological in origin, but he also believed that there was empirical evidence to justify them. It was one of the topics which featured in his disputes with Leibniz, who, with Descartes,⁴⁰ - and, indeed, Locke as well,⁴¹ - wished to treat space and time entirely as relations.⁴²

All of these aspects of Newton's work remained issues arising both out of the mechanistic conception of nature and of Newton's

basic empiricism; indeed, they were to dominate the philosophy of the eighteenth century. Since many of them did not emerge as real problems connected with the new science until Newton's achievement had been digested, it is not surprising that Locke's Essay did not present answers to them all. It was Hume and Kant, the two great philosophers who owed most to Newton, who were to focus most attention on the issues. In this sense the Newtonian achievement is not central to the philosophy of Locke, who drew his problems and his solutions largely from an earlier period of the scientific revolution. It is for this reason that I have passed over the problems and achievements of the Newtonian synthesis of mathematics and the empirical tradition rather rapidly.

Conclusions.

Newton, following Bacon, attempted to set science entirely within the empirical tradition. Experimental philosophy, he maintained, was concerned with the discovery of truth. And truths about the physical world could only be known by experience. All claims to knowledge must, therefore, be judged by the criterion of observation. But such claims, he held, even when so supported, provided no guarantee that the conclusions must be true, for fundamental to science was generalization by the method of induction, and induction, he recognised, did not guarantee certainty. Science was concerned with the discovery of the contingent structure which God had placed upon the world, but there was no independent guarantee that the explanations which scientists produced were in

fact the truth about God's creation. Revision, in the light of further experience, might always be necessary, and certainly it always remained a possibility. Newton's achievement is a constant reminder that there are no short ways to obtaining knowledge.

Conclusions to Part I

The scientific revolution produced three quite distinct conceptions of the nature and scope of scientific enquiry. Each, was, in its turn, a powerful factor in the advancement of science. The first two of these views were, philosophically speaking, rationalist. It was the third only which attempted to be unequivocally empiricist in its outlook on nature.

The first of these outlooks was underpinned by a Neoplatonic cosmology and a Neoplatonic epistemology which assumed that there was an eternal and necessary order underlying the world of phenomenal appearances which could be penetrated by the application of mathematics to the empirical data. The Neoplatonists expected to discover, with the aid of mathematics and observation, the necessary order which God had imparted to the world. Mathematics was central to this programme because it supplied not only the criteria of truth for what counted as the discovery of that order, but also because the universal timeless truths of mathematics were the stepping-stones from the transient appearances of the world to the eternal truths of God. It was in such an atmosphere that the first important steps of the scientific revolution were taken.

The attitudes associated with the Neoplatonic revival of the later Renaissance outlasted the sixteenth century, and penetrated deeply into the seventeenth. Indeed Galileo was in many ways the

last great representative of the Neoplatonic outlook in science. But at the same time his genius looked forward towards the much more secular attitudes towards science which emerged during the seventeenth century.

The men most responsible for the secularization of science were Bacon and Descartes. God's creation, Descartes held, was divided eternally between mind and matter, and the phenomena of matter could be almost entirely accounted for on mechanistic principles; (not "entirely", for Descartes held that God intervenes in the Universe to preserve a constant amount of motion. Cf. Principles of Philosophy, Principle XXXVI). Man, he believed, could come to understand the causes of all change, and indeed he offered his system as the true account of the world, derived from a clear comprehension of the only possible principles which could govern the phenomena.

Cartesian science rested on the method of the Discourse, which began with the certainty of cogito ergo sum, and finished with the certainty of the nature of body, a certainty which was to percolate through the Principles of Philosophy. Bacon, in contrast, offered no initial certainties as paradigms for knowledge, but he did offer the expectation of reaching truth through diligent experiment. Whereas the discovery of truth was, for Descartes, an intellectual process, for Bacon it was an activity to be pursued on field course on in laboratory.

For all its ingenuity and elegance the Cartesian system, especially in England, was less than wholeheartedly accepted. For the English it was only a theory, to be admired as such, but not grasped as the truth about all. Meanwhile, it was held, the only way that one can in fact discover truths of the world was by the Baconian method. Central to the Cartesian programme was the belief that it is possible to discover essences by intuition: the essence of mind, of body, of motion. In contrast Bacon offered no such possibility; the essence of anything could only be discovered by investigation.

On these issues the English scientists of the mid-seventeenth century all sided with Bacon. Theory no doubt could be brilliant, and often useful, but it was no substitute for truth, and truth, if it could be obtained at all, could only be reached by means of observation. Thus Boyle and Newton rejected the pretensions of a priori science and with it the possibility of absolute certainty about physical matters.

The Cartesian programme, then, was rejected. But upon what was this rejection based? And if it was to be replaced by the approach to nature of Boyle and Newton, why should anybody place more confidence in the approach of the latter than in the rationalist's programme? If the inheritors of Bacon's method placed so much weight on experience, could they justify reliance on such a notoriously fickle support? Or, from the other side, was the inherent pessimism in the possibility of reaching absolutely certain knowledge of the world in the natural philosophy of Boyle and Newton really justified?

These questions, and others, arose naturally from an enquiry into the Newtonian conception of science. It was these and related ones that Locke attempted to answer in the Essay.

Part Two

Locke's Theory of Knowledge

Introduction to Part Two

After our investigation of the aspirations of the scientists of the Revolution, we are now in a position to turn to Locke's Essay to see how it was that the developments we have considered had an impact on Locke's thought. We shall find that Locke borrowed substantially from both the method and the discoveries of science in his enquiry into the limits of human knowledge, and that the total effect of his argument is a recommendation that the only substantial way to knowledge of the world is to follow the method employed by the later English scientists whom we have considered, such as Boyle and Newton.

I believe that much of Locke's argument cannot be appreciated either for its achievements or its limitations without an understanding of this background. It is, therefore, worth looking briefly at some of these connections now, before we turn to a more detailed examination of his work.

We have already seen that although the effect of Cartesian views about the programme for science did not encourage English scientists to accept an a priori road to knowledge of the physical world, there was a strong tendency, exemplified by such thinkers as Digby and Hooke, to believe that certainty in knowledge was possible in natural philosophy. This attitude, we have also seen, was encouraged by the Baconian programme for science, and extended back beyond Bacon to the

Neoplatonists, who had held, for very different reasons, that man could reach certain knowledge of God's creation. In several important respects Locke's Essay can be seen as a critique of that programme. To begin with, Locke argued that one possible justification of that aspiration was unfounded in his attack on the possibility of knowledge through innate ideas. Innate ideas, Locke argued, could not be the foundation for any of our knowledge, and a fortiori, could not be the basis upon which knowledge of nature could be based. Instead, he argued, we must turn to experience as the only possible foundation upon which knowledge could rest. But experience, he maintained, was severely limited in what it could substantiate. Experience could only lead to knowledge of particular truths, and even these were limited by the nature of our senses. Thus he argued that we could not know the nature of the structure of physical objects when our conjectures went beyond our experience; we could not be certain, for example, what the essence of gold is, though we could know what it is not. But this, for Locke, did not mean that the scientific enterprise was doomed to failure, because he held that the natural philosopher could aspire to probabilities. Probabilities, Locke held, were both rational and important; and the foundation of probability lay in induction by simple enumeration, and was therefore grounded in experience, even though it went beyond experience. Further, such probabilities were for Locke candidates for knowledge, even if we did not know, nor perhaps could ever know, whether or not they were true, in Locke's rationalistic and restricted sense of 'know'.

If we are to establish that Locke's Essay arose in large part from both the presuppositions and problems of contemporary science then we must show that Locke was in fact familiar with the achievements of the scientists and had a knowledge of their work. I have therefore devoted the first chapter of this second part to just that task. Second, we shall look at Locke's arguments for the rejection of the thesis of innate ideas, as being for him a necessary preliminary to his exposition of his positive account of the nature of the origin of our ideas and his thesis about the nature and limits of knowledge. We shall then go on to consider Locke's positive thesis, concentrating especially on those themes which have so far concerned us.

Chapter VIII

Locke's Knowledge of Contemporary Science and its Sources.

Introduction.

There is quite a lot of evidence that Locke was pretty well acquainted with contemporary science, and, indeed, with the development of science throughout the seventeenth century, not only within the Essay itself, but also in a variety of other works and in his private papers and letters. But Locke rarely referred to particular scientists, or to particular scientific discoveries; even more rarely did he refer to particular books. Generally, therefore, the extent of Locke's knowledge of the works of scientists has to be inferred. Sometimes the evidence for an inference is substantial; at other times it is slender; but there is sufficient to establish that Locke was well acquainted with most, if not all, areas of contemporary science, and had a knowledge of the classical scientific texts. In one area there can be no doubt about Locke's knowledge. In medicine, and in medicine alone, Locke can be reckoned to have been by contemporary standards a master. Locke has been described by distinguished medical historians as "an experienced and skilful physician of sound judgement and advanced views".¹ As physician to the first Earl of Shaftsbury he was truly a professional, and he collaborated with the great Thomas Sydenham in research, and possibly also in writing some of Sydenham's important medical works.²

Locke was elected a member of the Royal Society in 1668, and although the records show that he was not a frequent attender at

meetings he was undoubtedly a respected one. Soon after joining he started a smaller club which met at Exeter House to discuss scientific, theological, and philosophical questions.³ It was at such a meeting in 1671 that the Essay Concerning Human Understanding began its long gestation.

Locke could count many of the greatest scientists of the period among his friends; he was acquainted with many more. It was from within the great circle of English science at perhaps its most prolific period, that Locke came to grips with the fundamental problems of the extent of human knowledge.

Locke's Medical Knowledge.

The extent of Locke's medical knowledge has been thoroughly explored by Kenneth Dewhurst,⁴ so here I shall only outline Locke's accomplishments. His serious interest in medicine seems to have begun in 1664 when he attended the lectures of Thomas Willis. Willis was then a physician of nearly twenty years standing and Sedleian Professor of Natural Philosophy at Oxford. Locke's full notes on Willis's lectures on therapeutics are still preserved. At the same time he became interested in the physiology of respiration which was currently the subject of investigation by Willis, Richard Lower, Robert Boyle, and Robert Hooke, and Locke worked with these men who were all then at Oxford. Concurrently he started experiments in medical chemistry, conducted with another doctor, David Thomas.

In 1666, again with Thomas, Locke began medical practice, and as a result came to meet Lord Ashley, later to be the first Earl of

Shaftsbury. Locke made such a favourable impression that Ashley asked him, in 1667, to become his personal physician, a post which Locke gladly accepted. In London, with Ashley, Locke soon became closely associated with Thomas Sydenham with whom he not only did clinical work, but also cooperated with him in producing Sydenham's medical books. Although Locke had many other interests he continued with medical work, graduating Bachelor of Medicine on 6th February 1674/5, and throughout the rest of his life he maintained his interest, and indeed, to practise.

As one would expect, Locke's whole approach to medicine was one entirely within the empirical tradition. But more than this, it was one characterized by a very definite method in science, a method with which, by now, we shall be familiar. It was the method commended by Thomas Sydenham which had much in common with the approach of Boyle and Newton, and which had much in common with that of Bacon. As one writer has expressed it:-

"[Sydenham's] philosophy of science lies squarely within the tradition of British Empiricism, which began, to take a convenient figure, with Francis Bacon. For example, I think that a historian of this tradition could find in Sydenham's writings the employment of many of Bacon's methodological doctrines, such as: the development of a Natural History, the importance of studying abnormalities, the requirement that experiments supporting general statements must be repeatable and hence testable, the existence of a small number of "natural classes", the dangers of the Idols of the Tribe, and the celebrated inductive Tables." 6

Sydenham's conception of causation in medicine was expressed in a way which anticipated the approach of Newton as expressed in the first two Rules of Reasoning in Philosophy:-

"Nature, in the production of disease, is uniform and consistent; so much so, that for the same disease in different persons the symptoms are for the most part the same; and the selfsame phenomena that you will observe in the sickness of a Socrates you will observe in the sickness of a simpleton." 7

Sydenham, like Newton, and entirely within the Baconian tradition, eschewed speculative hypotheses in medicine. Locke described Sydenham's method in a letter to William Molyneux in 1692:-

"That which I always thought of Dr. Sydenham living, I find the world allows him now he is dead, and that he deserved all that you say of him. I hope the Age has many who will follow his Example, and by the way of accurate practical Observation, as he has so happily begun, enlarge the History of Diseases, and improve the Art of Physick; and not, by speculative Hypotheses, fill the World with useless, though pleasing Visions. Something of this kind permit me to promise my self one Day from your judicious Pen." 8

Sydenham eschewed hypotheses in a pre-Newtonian age, and Locke followed him.⁹ Locke's clearest statement of method in medicine followed in another letter to Molyneux a few months later. It is rather long, but as it shows that Locke, as a practising scientist, accepted completely the method of Boyle and Sydenham, and the method that was to be clearly expounded by Newton, in opposition to the Cartesian approach to nature, it is worth quoting in full. Locke wrote:-

"But I perfectly agree with you concerning general Theories, that they are for the most part but a sort of waking Dreams, with which, when men have warmed their own Heads, they pass into unquestionable Truths, and then the ignorant World must be set right by them; Though this be, as you rightly observe, beginning at the Wrong End, when Men lay the Foundation in their own Fancies. I wonder, that after the Pattern, Dr. Sydenham has set them of a better Way, Men should return

again to that Romance Way of Physick. But I see it is easier and more natural for Men to build Castles in the Air of their own, than to survey well those that are to be found standing. Nicely to observe the History of Diseases, in all their Changes and Circumstances, is a Work of Time, Accurateness, Attention and Judgement; and wherein, if Men through Prepossession or Oscitancy, mistake, they may be convinced of their Error by unerring Nature and Matter of Fact, which leaves less room for the Subtlety and Dispute of Words, which serves very much instead of Knowledge in the learned World, where methinks Wit and Invention has much the Preference to Truth. Upon such grounds as are the establish'd History of Diseases, Hypotheses might with less Danger be erected, which I think are so far useful, as they serve as an Art of Memory to direct the Physician in particular Cases, but not to be rely'd on as Foundations of Reasoning, or Verities to be contended for; they being, I think I may say of all of them, Suppositions taken up gratis and will so remain, till we can discover how the natural Functions of the Body are performed, and by what Alteration of the Humours or Defects in the Parts they are hinder'd or disorder'd. To which purpose, I fear the Galenists four Humours, or the Chymists Sal, Sulphur and Mercury, or the late prevailing invention of Acid and Alkali, or whatever hereafter shall be substituted to these with new Applause, will upon Examination be found to be but so many learned empty Sounds, with no precise, determinate Signification. What we know of the Works of Nature, especially in the Constitution of Health, and the Operations of our Bodies, is only by the sensible Effects, but not by any Certainty we can have of the Tools she uses, or the way she works by. So that there is nothing left for a Physician to do, but to observe well, and so by Analogy argue the like Cases, and thence make to himself Rules of Practice: And he that is in this way most sagacious, will, I imagine, make the best Physician, though, he should entertain distinct Hypotheses concerning distinct Species of Diseases, subservient to this End, that were inconsistent one with another, they being made use of in those Several Sorts of Diseases, but as distinct Arts of Memory in those Cases. And I the rather say this, that they might be rely'd on only as artificial Helps to a Physician, and not as Philosophical Truth to a Naturalist. But, Sir, I run too far, and must beg your Pardon for talking so freely on a Subject you understand so much better than I do. I hoped the Way of treating Diseases, which with so much Approbation Dr. Sydenham had introduced into the World would have beaten the other out, and turned men from Visions and Wrangling to Observation, and endeavouring after settled Practice in more Diseases, such as I think he has given us in some. If my zeal for saving Mens lives, and preserving

their Health (which is infinitely to be preferred to any Speculations ever so fine in Physick) has carried me too far, you will excuse it in one who wishes well to the Practice of Physick, though he meddles not with it." 10

The last sentence of this passage can only be attributed to characteristic Lockian modesty, for his involvement with matters medical outlasted 1692 by some considerable time.¹¹

Clearly, Locke's attitude to the possibility of knowledge in medical science, as revealed in this letter, is entirely within the Baconian tradition, as exemplified in the philosophy of science of Boyle and Newton. It is a statement, almost as forthright as any of Newton's, of the rejection of speculative hypotheses as a method in science; the view that science itself is concerned with the discovery of truth in nature; and, equally, there is an insistence on the fundamental role of observation to the programme of discovery. Locke held out no promise of certainty by the method; it was largely by analogical argument that diagnosis would be achieved, and such arguments, as Locke had pointed out in the Essay, are only probable.¹²

Dewhurst summarises Locke's position admirably:-

"Such was the extent of Locke's empiricism, that after a lifetime of study, practice, and experiment, he had come to discard all hypotheses in favour of careful clinical observations from which a diagnosis could be made by analogy. His treatment was always simple and safe. Here again he believed that judgement firmly tempered on the anvil of experience should replace book knowledge, as the physician's sure guide With his plain, historical approach to clinical problems, and his analogous reasoning from similar cases, Locke showed physicians that medicine could be placed upon a basis of probability. He was unable to do more than this" 13

What, historically, the effect of Locke's philosophy of medicine was is difficult to judge, but in so far as it was inseparable from Sydenham's, it is worth remembering that it is said of Hermann Boerhaave of Leyden (1668-1738), one of the greatest clinical teachers of all time, that when he lectured at Leyden, he always raised his hat on mentioning the name of Sydenham.¹⁴

Locke and Chemistry.

If Locke's medical knowledge can be rated very highly, then his knowledge of chemistry was certainly very good. A sound chemical background was required for producing medical preparations, and this Locke possessed.

On 23rd April 1663 Peter Stahl, a protégé of Boyle, and the first public lecturer in chemistry at Oxford, started a course of chemistry classes which Locke soon joined.¹⁵ According to Wood: "This John Locke was a man of turbulent spirit, clamorous and never contented. The club wrote and took notes from the mouth of their master who sat at the upper end of a table, but the said J. Locke scorn'd to do it; so that while every man besides of the club were writing, he would be prating and troublesome."¹⁶ But, as Dewhurst points out,¹⁷ by this time Locke already had more chemical knowledge than the other pupils, and Locke's notebooks show that he was in fact an excellent student.

Locke's knowledge and research in chemistry developed rapidly from this time; not only did he read a large number of books on the subject, he also conducted many experiments.¹⁸ Included in his

reading were the books of Boyle as they were published, on which he made extensive notes.¹⁹

As a result of such reading Locke undoubtedly became well versed in the atomic views of matter which were rapidly becoming dominant in chemical and physical theory. Not only was Boyle's Origins of Forms and Qualities one with which Locke was very familiar, but almost certainly he later read Francois Bernier's Abregé de la Philosophie de Gassendi, which presented the modified Epicurean atomism which Gassendi put forward as an account of matter and change.²⁰ Bernier became an acquaintance of Locke in Paris in 1678, soon after the publication of the former's work.²¹

Locke was not only knowledgeable about chemical theory, but also of chemical practice. Both Boyle and Newton reckoned him a colleague in chemistry, a point well illustrated by a letter from Newton to Locke written in 1692 which shows the cooperation between the three men in chemical experiments. Although the top of the letter is missing, the remainder is in the Lovelace collection of Locke manuscripts. In it Newton wrote:-

".... as I can. You have sent much more earth than I expected. For I desired only a specimen, having no inclination to prosecute ye process. For in good earnest I have no opinion of it. But since you have a mind to prosecute it I should be glad to assist you all I can, having a liberty of communication allowed me by Mr. B [oyle] in one case wch reaches to you if it be done under ye same conditions in which I stand obliged to Mr. B. For I presume you are already under the same obligations to him. But I fear I have lost the first and third part out of my pocket. I thank you for what you communicated to me out of your own notes about it." 22

In chemistry, Boyle, Newton, and Locke collaborated in their work, though, no doubt, Boyle was regarded as the higher authority by the other two.

Locke and Mathematics.

It is sometimes suggested, I believe unfairly, that Locke knew hardly any mathematics. The basis for this suggestion is Brewster's remark that "the celebrated Locke was incapable of understanding the Principia from his want of geometrical knowledge" and "inquired of Huygens if all the mathematical propositions in that work were true."²³ But such a remark, even if true, does not establish that Locke was poor at mathematics. He was hardly alone in not being able to appreciate all the mathematical proofs of the Principia, and we might remember that, according to Conduitt, Sir Edmund Halley went to see Newton because he could not do the necessary mathematics to establish that the paths of the planets would be ellipses on the supposition that gravity diminished as the square of the distance.²⁴ Newton was himself aware of the difficulties in following all of the proofs, as he noted in the Introduction to Book Three of the Principia. He did not recommend anybody to study all the propositions of the preceding books, he wrote, "for they abound with such as might cost too much time, even to readers of good mathematical learning."²⁵

The case, then, for Locke's lack of mathematical knowledge is not proven, and there is at least some evidence to show that he had at least a moderate understanding of the subject. To begin with,

as an undergraduate, Locke attended John Wallis's lectures on geometry. Wallis was then Savilian Professor of Geometry, and a man of considerable reputation, and his lectures, given twice a week, were attended by Locke for the last two years before his graduation in 1656. Wallis's lectures not only included Euclid, Appolonius, and Archimedes, but also arithmetic, mechanics, practical geometry, and the principles of music; possibly, they also included plane and spherical trigonometry.²⁶ Furthermore, Locke continued to attend Wallis's lectures for a year after his election to a Studentship at Christ Church, and he also obtained instruction in astronomy from Seth Ward, the Savilian Professor of Astronomy.²⁷

Even allowing that Locke was not greatly attracted to mathematics as a subject, he must have learnt a fair amount from these studies. Certainly his later notebooks reveal an interest in pursuing mathematics beyond the level that he achieved as a student and young don. Thus we find that much later, in 1693, Locke entered in his notes the opinions of his friend and patron, the Earl of Pembroke, on various mathematical works.²⁸ (The Earl was President of the Royal Society in 1690, and he helped Locke greatly in the publication of the Essay to whom it was dedicated.) That Locke bothered to make such entries in his private notes seems to indicate at least a moderate interest in mathematics, and a desire to increase his knowledge.

Undoubtedly Locke believed that a knowledge of mathematics was important. In Some Thoughts Concerning Education²⁹ Locke rated both arithmetic and geometry highly. Of arithmetic he wrote, "This is certain, a man cannot have too much of it, nor too perfectly ..."³⁰

And even a young gentleman going into business should have mastered the first six books of Euclid.³¹

On the evidence available, it would be absurd to pretend that Locke was a good mathematician. His major interests in science, such as medicine and chemistry, did not require a great mathematical background, and there are no mathematical papers in his manuscripts. But, equally, there is some evidence to show that Locke was familiar with much of the subject, and interested in its progress.

Locke and Physics.

Locke was familiar with contemporary physical discoveries, and was always interested in the work of scientists on mechanics and optics especially, but there is no reason to believe that he could have been described as a physicist in any sense. His familiarity with contemporary work is well testified by his authorship of his Elements of Natural Philosophy in which he very succinctly stated not only the fundamental laws of Newtonian mechanics, but also the empiricist philosophy of nature. As a summary of contemporary mechanics, both terrestrial and celestial, it can hardly be faulted, and it shows that Locke not only fully understood the principles of Newton's physics, but also shared his philosophy of science.

The first chapter, entitled 'Of Matter and Motion' defines matter as "an extended solid substance"³² Motion, Locke says, is so well known that it does not require definition. Following this is an explanation of inertia, which, whilst entirely taken from

Newton, does not actually mention him. Then Locke wrote: "It appears as far as human observation reaches, to be a settled law of Nature, that all bodies have a tendency, attraction, or gravitation towards one another."³³ Gravitation, Locke accepted, was founded in observation; his inclination was not at all towards the Leibnizian interpretation that gravity was an occult quality. As he expressed it: "Two bodies at a distance, will put one another into motion by the force of attraction; which is unexplicable by us, though made evident to us by experience, and so to be taken as a principle in natural philosophy."³⁴

The Elements of Natural Philosophy was clearly written after the publication of Newton's Principia. It was first published in 1720 in A Collection of Several Pieces of Mr. John Locke by P. des Maizeaux, under the direction of Anthony Collins. In the Dedication, des Maizeaux explained that Locke had dictated these Elements for "the use of a young Gentleman whose education he had very much at heart". Des Maizeaux rightly added that although only very brief, "in its kind, this piece is no way to be despised. We wanted such a work in English; and it would not have been an easy matter to find any such person, who could have comprehended so many things in so few words, and in so clear and distinct a manner."

That Locke was able to express so neatly the fundamentals of natural philosophy indicates a mind that had fully grasped the latest developments in the field; it is also worth noting that the pedagogical advantages of the work must have been appreciated, for

although the Elements was not published until 1750 (except in des Maizeaux's collection), it then became extremely popular. It was printed together with his Some Thoughts Concerning Reading and Study for a Gentleman, and went through five editions in fourteen years, testifying to its relevance late into the eighteenth century.

Another piece of evidence which contributes to our picture of Locke's knowledge of physics is a paper by Newton among Locke's manuscripts.³⁵ The paper, in the hand of Locke's amanuensis, Sylvanus Brownover, is a demonstration that the planets move in ellipses, but the proof is a different one from that offered by Newton in the Principia. It has been suggested that the paper was composed for Locke by Newton because Locke wished for a simpler demonstration than that found in the Principia. This conjecture, though just possible, has no documentary evidence to support it, and how the paper in Brownover's hand originated remains a mystery. But, whatever its origins, it seems unlikely that Locke would have wanted the copy at all, (for it is clearly a copy from some other document,³⁶) unless Locke expected to be able to follow the complicated mathematical argument which it contains. The **existence** of the document, that is, suggests that Locke expected to master Newton's mathematical physics, a hope to which somebody not familiar with both mathematics and physics would hardly have aspired.

Although Locke's practical scientific work was largely confined to medicine and chemistry, there is some evidence that Locke might

have conducted some physical experiments. In Locke's manuscripts there is a paper which examines the concept of density, and seems to imply that Locke carried out what might have been original experiments, of a comparatively simple sort, on the nature of density, and how, assuming a corpuscular theory of matter, it may be explained. Although the paper may have been copied from some other person's work, this is unlikely; Locke almost always gave the source of any quoted notes.³⁷

There is very little evidence of Locke's interest or accomplishments in other branches of physics. But his library did contain such volumes as Huygens' Traité de la Lumière and Galileo's works.³⁸ It is also worth noting that the library included 240 books on natural science (excluding medicine) and 69 of these were on physics, the largest single sub-group.³⁹ How far he was a master of their contents we can only guess.

Locke and other sciences.

Locke's interests were not limited to the four major sciences so far considered; his library contained volumes on natural history (61), zoology (29), botany (13), astronomy (11), and mineralogy (9).⁴⁰

Although there is little evidence to establish Locke's expertise in these various fields, he clearly had some knowledge, as equally clearly he did in almost all branches of intellectual enquiry. Indeed, although Locke is primarily remembered as an important philosopher, if he had written no philosophy at all he would surely have a place in history as an excellent example of an English virtuoso, for not only was he well acquainted with the various branches of science,

and an acknowledged master in at least one, he was, as well, a theologian, an economist, and political theorist of deserved repute.

The Scientific Influences on Locke.

Undoubtedly the greatest scientific influences on Locke were Robert Boyle and Thomas Sydenham, to whom, with Huygens and Newton, Locke paid special tribute at the beginning of the Essay. We have already considered Sydenham's direct influence; that of Boyle was at least as great, and stemmed from their acquaintanceship in Oxford, where they met in 1660.⁴¹ Although at this stage they were not close friends,⁴² in subsequent years their mutual regard grew, and they remained firm friends until Boyle's death in 1691. At that time Locke was attending to the publication of Boyle's A General History of the Air, and Locke, with Newton, were two of Boyle's literary executors. Locke followed with interest the publication of each new work of Boyle,⁴³ and in Locke's library catalogue for 1697 there is a list of sixty two works by Boyle, far the largest number of books by any single author.⁴⁴

Newton's influence on Locke was not great. Although Locke's views on space were probably altered as a result of reading the Principia,⁴⁵ Locke's general conception of knowledge and scientific method was developed from a tradition from which Newton himself drew. Locke and Newton first became acquainted in 1689, after Locke's return from Holland, where he had been since 1683. The Essay Concerning Human Understanding had by this time been completed for three years, and thus Newton's major work published in 1687, was

also not an influence on Locke's writings, though, as we have seen, succeeding editions of the Essay were marginally modified as a result of Locke's reading of Newton's work.⁴⁶ The attitude towards hypotheses in medicine, therefore, which we have seen Locke support, whilst being very much the attitude of Newton also, was not a product of the latter's influence. The full significance of this has not been always appreciated by commentators on Newton: Newton's rejection of hypotheses was one that was entirely consistent with the empiricist approach to nature exhibited by such men as Sydenham and Locke. Newton was merely the most famous spokesman for an attitude, rather than its originator.

Undoubtedly it would be a mistake to identify any particular person, or persons, as the scientific influence on Locke. He spent much time talking as an equal with the greatest English scientists of this greatest period in English scientific history.

Locke's scientific standing is well illustrated by his record in the Royal Society. Locke was elected a Fellow on 26th November, 1668, when he was thirty-six; a year later he was elected to its Council,⁴⁷ to which Boyle was not appointed until 1671,⁴⁸ and although Locke was not a frequent attender at meetings, he was re-elected in 1672.⁴⁹ Although this reflects primarily a recognition of Locke as a man of sense and general intelligence, his standing as a scientist would undoubtedly have been relevant. He was, as John Evelyn described him in his Diary, "an excellent learned gentleman";⁵⁰ his learning covered not only the classics but also

almost all areas of intellectual enquiry.

The general ethos in which Locke moved was expressed by Hooke in a paper on the business and design of the Royal Society, to which is appended the date 1663. The Royal Society, Hooke said, existed "to improve the knowledge of natural things, and all useful arts, manufactures, mechanic practices, engines and inventions by experiments" Hooke went on:-

"... in the meantime this Society will not own any hypothesis, system, or doctrine of the principles of natural philosophy, proposed or mentioned by any philosopher ancient or modern, nor the explication of any phenomena whose recourse must be had to original causes (as not being explicable by heat, cold, weight, figure, and the like, as effects produced thereby); nor dogmatically define, nor fix axioms of scientific things, but will question and canvas all opinions, adopting nor adhering to none, till by mature debate and clear arguments, chiefly such as are deduced from legitimate experiments, the truth of such experiments be demonstrated invincibly.

"And till there be a sufficient collection made of experiments, histories and observations, there are no debates to be held at the weekly meetings of the Society, concerning any hypothesis or principal of philosophy, nor any discourses made for explicating any phenomena, except by special appointment of the Society or allowance of the President ..." 51

The atmosphere of the Royal Society, deliberately created by its members, was one opposed to metaphysics and speculation, entirely opposed to unsubstantiated hypothetical explanation, entirely in favour of undogmatic investigation of nature. It was the attitude expressed by the young Professor Newton in correspondence arising out of his first paper to the Royal Society;⁵² it was entirely the attitude of Locke himself.

Of the influence on Locke of those who were not his contemporaries, the evidence is never very conclusive. Clearly the impact of Bacon

on all the early members of the Royal Society was great, but there is very little evidence of a direct Baconian influence on Locke. In his writings Locke rarely referred to Bacon, but neither did he to Boyle or Descartes, who were undoubtedly influential. Locke recommended Bacon once, but as an historian, not a natural philosopher, (in Some Thoughts Concerning Reading and Study for a Gentleman).

However, in one of Locke's notebooks, in which he quoted various authors' views on other writers, opinions about Bacon are quoted as often as those about anybody else.⁵³ Further, in Locke's library there were, not surprisingly, several of Bacon's works. His list of books for the year 1687 includes the Novum Organum, De Augmentis Scientiarum, Sermones Fideles, and Historia Vitae et Mortis.⁵⁴

But Locke no more was, nor saw himself as, a disciple of Bacon, than, say, Hume was, or thought of himself as, a disciple of Locke.

Much the same applies as regards Locke's relationship to such continental thinkers as Descartes and Gassendi. Although Locke admitted to Lady Masham that it was reading Descartes which first gave him "a relish in philosophical things",⁵⁵ the extent of Descartes' influence is a matter of speculation, though clearly large parts of the fourth book of the Essay derive, at least indirectly, from Descartes' metaphysics and epistemology, and Descartes is often the subject of attack in other parts of the Essay.⁵⁶

Of Gassendi's direct influence there is much less evidence. Even though Fox Bourne, in his biography of Locke suggested that it was Gassendi "to whom unquestionably Locke owed most,"⁵⁷ there is

no evidence that Locke saw himself in such a debt, and it is much more plausible to maintain that Gassendi's views became accepted by Locke only after they had received further support from the English atomists such as Boyle. Locke was hardly the man to accept the latest theories simply because they were new; indeed, in all things he was a conservative revolutionary. In many respects Locke's epistemology matches that of Gassendi, and no doubt Fox Bourne was right to see Gassendi as a precursor of many of Locke's views, but there is no evidence that Locke borrowed directly from him. It could be maintained that Locke could, and probably did, discuss Gassendi with Francois Bernier, the French populariser of Gassendi's philosophy, for Locke met Bernier in France in 1677. Yet Locke's journal entries on Bernier are exclusively about his travels.⁵⁸

Of other scientific influences on Locke there can be much interesting, but inconclusive, speculation. Locke read widely and assiduously; he enjoyed discussions on almost any intellectual matter, and being successively at the focus of the scientific circles of England, France, and Holland, he was both able to, and certainly did, absorb from a great variety of sources.

Conclusions.

Enough has been said to establish that Locke was clearly a man of science as well as a man of letters. In an age when such distinctions were barely formed, too rigid comparting of Locke's place within our present categories of learning would be inappropriate. Locke was a distinguished amateur in science; he was not a master

builder, and had no special axe to grind in defence of his own particular expertise. In such a position he was admirably placed to supply judicious assessment of the method of science, to examine how far man could expect to go along the road to knowledge, and where he would have to acknowledge his ignorance; the task, indeed, which faced the author of the Essay Concerning Human Understanding.

Chapter IX

The Rejection of Innate Ideas

Introduction.

In Part I we traced the development towards the conception of science which existed in England at the end of the seventeenth century from the beginnings of the scientific revolution. We saw how, especially in England, there was a gradual recognition of the limitations of science and the potential of man to understand nature. Coupled with this was the commitment to the view that the only way in which nature's secrets could be revealed was by careful observation and experiment. As a corollary of this attitude, it was held that theories or hypotheses about the mechanisms of nature, ungrounded in observation, could not, for that reason, be accepted as being true, even if, as they often did, they were capable of explaining the known facts. This general attitude found its most able expression in the work of Newton. But Newton confined himself to the specific problem of the limits and nature of science; he did not attempt to place his considerations in a more general epistemological framework. One important reason why Newton never had to attempt this task was because the programme was carried through by his friend John Locke.

The argument of Locke's Essay is, in essence, very simple. If we are to inquire into the extent and limits of human understanding, Locke said, then we must recognise that the concepts - I deliberately do not use the word 'idea' - with which the human mind can operate, are derived

from experience: there is no other source from which they may come. Our experiences are of varying sorts, but the vast majority of them are the product of our senses. We can, in fact, show how all our concepts are derived from experience if we make a careful analysis of them. Our concepts are used to stand for, or represent, things as they are, but the only way to establish whether or not our concepts in fact represent things as they are is by comparing them with our experiences, thus, if we wish to know whether our concept of gold really accurately represents gold as it is, then we must find out by experiment and observation whether or not gold has the properties which our concept of gold implies. The extent of human knowledge is demarcated by the extent to which we can match our concepts against experience; when we can so match, then we have knowledge, when the matching cannot take place, then we have something less than knowledge, we are up against the limits of human understanding. Where we do not as yet have a matching, but where there is no reason why the matching cannot take place, then there is room for scientific investigation and scientific discovery to bridge the gap in our knowledge. This, in crude and bold outline, is Locke's argument. The remainder of this work will examine some of the key points of that argument and show how the Essay succeeded, and how far it succeeded, in justifying the approach to nature which had emerged from the scientific tradition which we have already considered.

In this chapter we shall look at Locke's rejection of the doctrine of innate ideas, to establish exactly what thesis it was that Locke was

arguing for, and to take special note of the method whereby Locke hoped to justify his conclusion. Before we turn to Locke's argument, however, there are one or two points which I wish to make by way of a preliminary.

For those too eager to find a connection between the scientific revolution and the first Book of Locke's Essay it might appear that the rejection of innate ideas implied both a rejection of Cartesian epistemology and of Cartesian physics. The argument for such a view would be that since Descartes' epistemology, and therefore his physics, began from certain a priori truths, held to be true without recourse to experience, and Locke denied that there are any such truths, because he held that all knowledge has its origins in experience, it follows that the rejection of innate ideas entails the rejection of the Cartesian programme. But such a view will not stand critical examination, for Locke's rejection of innate ideas did not, in itself, rule out the possibility of all a priori knowledge, in the sense in which Descartes' system was based upon such knowledge. Locke rightly held that there are many propositions which we can know a priori to be true; but what he did deny, in a confused and confusing way, was that we could know the essence of things, as opposed to the essence of concepts, except by experience. We could only know what the essence of a thing was a priori, therefore, when the thing and the concept were identical. In all other cases we could not know a priori that our concept accurately represented the thing, and even a posteriori it was seldom easy, usually difficult, and often impossible, to discover.

But if the actual argument of Locke's first Book is not in general inconsistent with Descartes' programme, the tone of the argument is very different from anything which Descartes wrote. Further, what there is which is in conflict with Descartes' views is of great significance. Locke insisted, as against Descartes, and all subscribers to any innate ideas doctrine, that the criterion of knowledge, the criterion as to whether any person has a particular piece of knowledge, must itself be an empirical, not an a priori, criterion. The only test, Locke insisted, that enables us to attribute knowledge, is assent to the statement purported to be true. He denied the possibility of knowing a priori that anybody, including oneself, knows anything. The implications of this point, though generally unnoticed, are considerable, a point which, in the sequel, we shall explore.

Locke's Thesis.

The second chapter of the first Book of Locke's Essay begins with the following words:-

"It is an established opinion amongst some men that there are in the understanding, certain innate principles, some primary notions, *ΚΟΙΝΑΙ ΕΨΧΑΙ*, characters, as it were, stamped upon the mind of man, which the soul receives in its very first being and brings into the world with it." 1

It is this thesis that the first Book is concerned to reject. It is to be replaced by Locke's own positive thesis which he put forward at the beginning of the second Book of the Essay, where he argued that the source of all of our ideas is "Experience. In that all our knowledge is founded; and from that it ultimately derives itself."²

Although it was only comparatively late in the writing of the Essay that Locke decided that a long section should be concerned with showing the falsity of the innate ideas thesis, (the early drafts of the work give very little, but increasing, attention to the issue)³, it was certainly no strawman that Locke was attacking. It has been clearly shown by Yolton⁴ that there were many advocates of innate ideas theories of knowledge both before and after the publication of the Essay, and, no doubt for this reason, Locke felt that he had to devote a fairly long section to refuting them before turning to his own positive ideas. Those who advocated innate ideas were of varying sorts. Some, like Stillingfleet, later to be one of Locke's great antagonists, argued as crude a version as one could find with which to contrast Locke's own position. (In Stillingfleet's Origines Sacrae published in 1662). Others, like Descartes, put forward a much more subtle, if vaguer account.

Even if there had been no advocates of an innate ideas theory there is a clear sense in which it would have been necessary for Locke to argue that any innate ideas account of the origin of the "furnishings of the mind" was mistaken. For, given that ideas must enter the mind at some stage, they must either enter at the mind's creation, or at some later stage, and if Locke could show that it was not the case that they entered the mind at its creation, then he would be a long way to showing that his own positive thesis was correct.

Locke's rejection of innate ideas was not only seen by him as a stepping stone to establishing his own theory, however. He also

had a moral reason which went to the heart of his intellectual attitude. The belief in innate ideas, Locke held, had and did encourage men to accept unquestioningly principles which ought to be questioned. It encouraged an uncritical acceptance by the weak and an intolerable dominance by the strong where no such attitudes should prevail. He explained the point towards the end of the first book as follows:-

"When men have found some general propositions that could not be doubted of as soon as understood, it was, I know, a short and easy way to conclude them innate. And it was of no small advantage to those who affected to be masters and teachers, to make this the principle of principles, - that principles must not be questioned. For having once established the tenet, - that there are innate principles, it put their followers upon a necessity of receiving some doctrines as such; which was to take them off from the use of their own reason and judgement, and put them on believing and taking them upon trust without further examination: in which posture of blind credulity, they might be more easily governed by, and made useful to some sort of men, who had the skill and office to principle and guide them. Nor is it a small power it gives one man over another, to have the authority to be the dictator of principles, and teacher of unquestionable truths; and to make a man swallow that for an innate principle which may serve to his purpose who teacheth them." 5

The moral fervour should not go unnoticed. Locke understood as few, if any, had done before him, the connection between intellectual arrogance and moral bigotry. This is only one of the many connections between Locke's epistemology and his political theory.⁶

The argument of Book I of the Essay attempts to establish that the acceptance of the doctrine of innate ideas is, on three basic counts, unsound. First, the suggestion is implausible; second, it is unnecessary; third, to accept the doctrine in fact leads to contradictions. Let us, then, turn to Locke's arguments to rehearse and assess them.

Significantly, from the beginning of the Essay Locke treats the thesis of innate ideas as an hypothesis. Thus, against those who justify innate ideas from universal consent, he reasoned in the following way: this argument, Locke said, "has this misfortune in it, that if it were true in matter of fact, that there were certain truths wherein all mankind agreed, it would not prove them innate, if there can be any other way shown how men may come to that universal agreement, in the things they do consent in, which I presume may be done."⁷ The hypothesis of innate ideas, Locke argued, is put forward to account for universal consent. But even if we grant that there is universal consent (which Locke believed there was not) still this would not establish that innate ideas were the only hypothesis that could explain such agreement. The supposed agreement might be, indeed, could be, equally well explained by some other hypothesis. What that other hypothesis was we shall consider later; what is now to the point is that Locke's argument here is logically the same sort of argument that Newton, and Locke himself, employed against those scientists who attempted to base both science and truth upon hypotheses. The theory of innate ideas was, for Locke, an hypothesis in just that sense which Newton later identified in his letter to Roger Cotes in 1713 when he wrote: "the word 'hypothesis' is here used by me to signify only such a proposition as is not a phenomenon nor deduced from any phenomena, but assumed or supposed without any experimental proof."⁸ Innate ideas, Locke held, were postulated as the cause of the phenomenon of universal agreement, but the cause was not independently identified, and for all we know the cause might well be something else.

The theory of innate ideas, therefore, was treated by Locke as an hypothesis which exemplified just that fault to which Locke himself showed opposition in his medical writings, a point we noted in the last chapter. It was, too, just that sort of hypothesis which we discovered Newton was so hostile towards. The theory of innate ideas was an hypothesis which had no direct foundation in direct confirmation: it was just the sort of theory which Baconian science and its most able practitioners were most eager to reject. It was a postulated cause, not independently identifiable, just as for Bacon was the Copernican system, or, for Newton the vortices of Cartesian physics.

The phenomena for which the hypothesis of innate ideas was to account varied. They included the acceptance by men of such principles of demonstration as the Law of Identity, 'whatever is, is'; the belief that all men knew the same moral code; and the belief that really all men knew that there was a Deity. From his wide reading of travel books, Locke had no difficulty in showing that the supposed facts were not as the supporters of innate ideas would have them; the supposed universal phenomena were by no means universal. Children and idiots do not subscribe to the Laws of Thought, Locke said, they do not even understand them. As for innate moral principles: "The virtues whereby the Touopinambos believed they merited paradise, were revenge, and eating abundance of their enemies".⁹ Nor were they alone in not recognising any God, for there were "whole nations at the bay of Soldania, in Brazil, in Boranday, and in the Caribbee islands, etc., amongst whom there was

to be found no notion of a God , no religion."¹⁰

The facts, then, that required explanation were not those that the supporters of innate ideas imagined. If it was a mistake to invoke hypotheses and construe them as truths, then it was doubly mistaken to accept the hypothesis as true when the facts it was supposed to explain were not as the hypothesis required.

The claim that innate ideas exist, like all existential claims, cannot easily be disproved. (For this reason, if for no other, the burden of justification for existential claims ought always to lie with the propounder, rather than with the rejector, of any such claims.) But if it can be shown that no empirical observations substantiate the claim, and if it can be shown that any facts purportedly explained by the claim, can be explained without invoking the claim, on grounds of simplicity, the latter explanation ought to have preference over the former. Part of the argument, therefore, which Locke deployed against innate ideas was identical in method with the recommendations which were contained in Newton's first and second Rules of Reasoning in Philosophy in the second edition of the Principia, and, more relevantly, were also contained in slightly different form, in the first edition of 1687. There, at the beginning of the third book Newton listed various hypotheses which were the fundamental assumptions of the work. The first two of these were: "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances", and "therefore the causes of natural effects of the same kind are the same."

The phenomena of ideas in the human mind can be explained, Locke urged, by one cause, experience. And that cause could be known to be true by induction, because nobody wished to maintain, in the face of the empirical evidence, that all ideas were the product of innate ideas; ideas of colours, sounds, tastes, and so on were, paradigmatically, experiential.

One strand of Locke's argument against invoking innate ideas, then, was drawn entirely from the method of the new science. The principle that entities should not be multiplied beyond necessity, significantly known by the name of an earlier English empirical philosopher as Ockham's Razor, was employed by Locke both in his science, as we have seen in his medical method, and in his philosophy.

Ockham's Razor, by itself, as deployed by Locke, did not establish that there are no innate ideas. All it did was to throw back the burden of proof on those that wished to advocate their existence. But Locke argued further that the supposition of innate ideas itself leads to contradictions, a move, which if successful, renders the concept not only unnecessary but also absurd. Locke's argument was this: if innate ideas are to serve any useful function at all in explaining man's beliefs, then they must be reckoned universal to all men. Given that some men do not acknowledge that they have these ideas, as is well established empirically, it must be the case that they have them without being aware of them. But this, Locke claimed, is a contradiction. He made the point, often made later against Freudian psychoanalytic theory, that the concept of unconscious ideas is self-contradictory. Those who believe in

innate ideas, believe, Locke said, "that there are truths imprinted on the soul which it perceives or understands not".¹¹ But this, said Locke, is absurd, because "No proposition can be said to be in the mind which it never yet knew, which it was never yet conscious of."¹²

As long as it is granted that ideas can exist only as objects of conscious awareness, then Locke's argument is sound. It is worth noting that Locke's subscription to this view was itself thoroughly Cartesian, for Descartes had himself defined 'thinking' in terms of every and any mode of consciousness.¹³ But it might be held, against both Locke and Descartes, that there is no good reason to restrict the concept of an idea to something which is an object of conscious awareness.

Argument on this point could very easily ramify into a discussion of several fundamental questions in the philosophy of mind which would take us far from the consideration of Locke's theory so I shall restrict comment on this topic to one point. Even if we did allow that ideas could exist without the person who had the ideas being aware of them, we should still wish to draw a distinction between ideas that are consciously known about and those that are not. Further, we would need some criterion for attributing ideas of the second sort to individuals, including ourselves, which, ex hypothesis, could not be the criterion of introspection, which we can use in the case of conscious ideas. The only criterion which we could use would be that the person was capable of making conscious the idea which previously he only had unconsciously. But then to attribute innate ideas to an individual turns out to be nothing more than

establishing that the person has the capacity to have certain conscious ideas. But, this granted, it follows that to attribute innate unconscious ideas to individuals is only to attribute the capacity to have ideas to them. But if we are to do that, then it follows that all ideas are innate, and therefore the distinction between innate and non-innate ideas disappears, though we are left with the distinction between conscious and unconscious ideas, that is, the ideas we do have and those that we are capable of having.

It might be held that this is to oversimplify the issue. For surely, it might be held, the distinction between innate ideas and other ideas can be upheld on other grounds. Thus, innate ideas can be understood as ideas which a person can have even if he has no other experiences. On this conception of innate ideas, any truths known a priori would be examples of innate ideas. But to this line of argument Locke offered a reply. First, when considering the possibility of maxims, such as the Laws of Thought, being innate ideas, Locke said that if it is argued that they are innate because man recognises their truth when he comes to the age of reason, then this will hold for all such necessary truths, but this can hardly be allowed, for many necessary propositions are ones which most of us only learn with difficulty, for example the more obstruse propositions of Euclidean geometry. Thus he wrote:--

"If it be said, the understanding hath an implicit knowledge of these principles, but not an explicit, before this first hearing (as they must who say 'that they are in the understanding before they are known',) it will be hard to

conceive what is meant by a principle imprinted on the understanding implicitly, unless it be this, - that the mind is capable of understanding and assenting firmly to such propositions. And thus all mathematical demonstrations as well as first principles, must be received as native impressions on the mind; which I fear they will scarce allow them to be, who find it harder to demonstrate a proposition than assent to it when demonstrated." 14

Locke, of course, did not wish to deny that the way in which we come to know that a necessary proposition is true is different from the way in which we come to know that a contingent proposition is true. But what he did want to deny was that an account of how it is that we can know that a necessary truth is true requires the postulation of innate ideas. He argued that even in the case of maxims, the words had to be learnt, and if we construe the words as representing ideas, the ideas themselves we get "by degrees". This is especially true of general ideas such as are involved in the maxim that "it is impossible for the same thing to be and not to be", a proposition which Locke held involved ideas that could only be known after we have knowledge of particular ideas from experience.¹³

Locke's argument here is actually circular. He presupposes that no general ideas are innate, that they are all ultimately derived from particular ideas, which are themselves the product of particular experiences, in order to substantiate his claim that there are no innate ideas, itself required by him in order to establish that all ideas are derived from experience: But this charge cannot be made against the earlier arguments we have considered.

The total case against innate ideas which Locke presented includes not only the argument of Book I of the Essay, but also his

own positive account of the origin of ideas in Book II. But this acknowledged, the case that Locke amasses in Book I alone is formidable. The hypothesis, he argued, is unnecessary, and is, anyway, self-contradictory. But his argument depends upon assuming a certain view of ideas, namely, that ideas are always necessarily the objects of consciousness. It is worth noting that Descartes, for one, did not understand ideas in this sense. I wish, therefore, to digress slightly to consider Descartes' doctrine of innate ideas, in order to bring out this point, and also because, by contrast it will bring out an important aspect of Locke's argument.

The Cartesian Conception of Innate Ideas.

When Descartes, in The Meditations of 1641, first introduced the concept of innate ideas, he used it to help draw a distinction between three kinds of idea. He wrote:-

"But among these ideas, some appear to me to innate, some adventitious, and others to be formed [or invented] by myself; for as I have the power of understanding what is called a thing, or a truth, or a thought, it appears to me that I hold this power from no other source than my own nature." 16

Descartes admitted that he was uncertain of these distinctions, but in his Notes against a Programme, written a few years later, his position emerged more clearly. Descartes' opponent, Regius, had written: "The mind hath no need of innate ideas, or notions, or axioms, but of itself the faculty of thinking suffices for the accomplishment of its processes."¹⁷ To this Descartes replied:-

"he appears to dissent from me only in words, for when he says that the mind has no need of innate ideas, or notions, or axioms, and at the same time allows it the faculty of thinking (to be considered natural or innate) he makes an affirmation in effect identical with mine, but denies it in words. For I never wrote or concluded that the mind required innate ideas which were in some sort different from its faculty of thinking; but when I observed the existence in me of certain thoughts which proceeded, not from extraneous objects nor from the determination of my will, but solely from the faculty of thinking which is within me, then that I might distinguish the ideas or notions (which are the forms of these thoughts) from other thoughts adventitious or fictitious I termed the former 'innate'." 18

Innate ideas were, for Descartes, to be understood as ideas which a person can have, not necessarily does have, at any particular moment, which are causally independent of any sense experience. Thus, Descartes believed that knowledge of God was innate in this sense because it was possible for man to have an idea of God simply as a result of contemplating the concept of perfection, itself not derived from any ideas causally dependent on the five senses, but a product of the intellect's recognition of the limitations of the powers of the mind.¹⁹ Descartes' use of the term 'innate idea', therefore, was not intended, except in a negative sense, to indicate a causal explanation of the origin of some of our ideas. As he used the expression, innate ideas were simply ideas not derived from sense experience; what they were derived from he did not explain.

From this two differences emerge between what it was that Descartes supported and what Locke attacked with regard to innate ideas. First, Locke's objection was aimed at those theories which

proposed innate ideas as a causal explanation of some of our ideas, but, as we have seen, for Descartes innate ideas were not construed as causal agents, but simply understood as ideas derived a priori. Second, Descartes was committed to saying that men have an innate idea of, say, God, because he held that man can come to know that there is a God a priori. Thus Descartes was committed to using the term 'innate idea' of what was not necessarily an actuality of the mind, only a potentiality; whereas, as we have already seen, Locke denied that we can call an idea innate simply because by reason alone man is capable of recognising certain propositions to be true.

Given this, the question arises: what exactly were the implications of Locke's rejection of innate ideas for the possibility of a priori knowledge? To this question we shall now turn.

The Rejection of Innate Ideas, and the Possibility of A Priori Knowledge.

The full implications of the rejection of innate ideas for the possibility of a priori knowledge in Locke's work will only become evident after we have considered his account of knowledge in the last Book of the Essay. But some preliminary points can and should be made before we leave the topic of innate ideas.

It might be argued that if any a priori knowledge is possible, then there must be, in some sense, innate ideas. The argument to support this position might run like this: a priori knowledge is knowledge independent of externally derived experience (that this

phrase is undoubtedly itself obscure need not for the moment delay us). But any knowledge is knowledge of something. It follows that something must exist (in the mind), ex hypothesi not derived from experience, for there to be a priori knowledge of it. That which so exists, is what is meant by an innate idea. Therefore, if there is a priori knowledge, then there are innate ideas. I believe that on the basis of what we have already considered Descartes would have subscribed to some such argument, and certainly Leibniz did, for he held that the eternal truths "must be grounded in the existence of a necessary substance." That is, eternal truths must exist in the mind of God, for they must exist somewhere, and their existence is "anterior to the existence of contingent beings." Such truths, Leibniz held, were "graven in our souls".²⁰ It was this conception of innate ideas, and therefore this conception of a priori knowledge to which Locke was so opposed.

This conception of a priori knowledge is not, however, the only possible one. We can, and philosophers do, talk of knowing something a priori when we come to know a new truth, not by experience, but by deduction. Thus, by way of an example, I may know that Jones is a bachelor by experience. From this, without recourse to any further experience, I can infer that Jones is male. In so far as I obtained the knowledge that Jones is male by deduction, and not from experience, it was not empirically derived. I came to know it a priori.

In this sense of the possibility of a priori knowledge there is clearly no need to introduce any concept of innate ideas to explain how the a priori knowledge is possible. There is, then, a perfectly clear sense in which one can talk of a priori knowledge in which, although the knowledge is not unconnected with experience, it is not derived from experience directly. Whether the a priori knowledge so obtained is synthetic or analytic is not in question; the point is simply that the rejection of innate ideas is itself not inconsistent with the possibility of a priori knowledge. For this reason alone it is not, or should not be, surprising to find that Locke is committed to both. What would be surprising is if Locke were both to maintain that there were no innate ideas, and that it was possible to derive all knowledge independently of experience, but this absurdity is not one he approaches.

The sense in which I have indicated that a priori knowledge is not ruled out by the rejection of innate ideas can be identified by drawing a distinction between two questions which it is easy to conflate. These two questions are: (a) Where did I get the idea?, and, loosely, (b) How do I know that the idea or proposition is true?. Locke's answer to the first question is, obviously, 'by experience'. But it does not follow from this that all propositions must be verified by experience, if we are to know that they are true. The sense in which a priori knowledge is not ruled out is just that sense in which Kant allowed a priori knowledge. Kant's position on this has been clearly explained by Arthur Pap as follows:-

"Kant's explicit definition of a priori knowledge is a negative one: "Knowledge that is independent of experience and even of all sense impressions." (Critique of Pure Reason, second edition, intro. I). The kind of independence in question is not, of course, genetic, for Kant explicitly says that "undoubtedly all our knowledge beings with experience. " What he had in mind is that a judgement is a priori if the evidence on which it is accepted is not empirical." 21

This sense of a priori knowledge, Pap points out, rests squarely on the notion of necessity. Thus, for Kant, a priori knowledge was knowledge which was absolutely necessary, i.e. knowledge to which "no exception is admitted even as a possibility."²²

Granted, then, that Book I of the Essay does not have the effect of ruling out all a priori knowledge, it follows that Locke has either to find some other way of ruling it out completely, or accepting that perhaps ultimately all knowledge is a priori, or , thirdly accepting that some, but not all, knowledge is a priori. It was the latter path that Locke in fact followed. To do so he had to find a criterion for distinguishing between acceptable and unacceptable claims to a priori knowledge. And this, in a sense, is what quite a large part of the Essay is about.

Locke's Method.

Before we leave Locke's treatment of the topic of innate ideas, I wish to draw attention to certain features of Locke's argument which, I believe, have not been noticed by commentators and critics of the Essay. These features are important not only of the method of Book I, but also re-occur throughout the work.

We have seen that, like a natural philosopher, Locke approached the problem of innate ideas as if it were a scientific or para-

scientific causal hypothesis about the origin of some of our ideas. By adopting this approach Locke makes it plausible to think that it can be rejected if it does not match up to the requirements of such an hypothesis. Locke implicitly argued that by this test the hypothesis of innate ideas fails. The point can be brought out by comparing the hypothesis of innate ideas with the conditions which had to be met if an hypothesis were to be accepted by the men of English science of the later seventeenth century. More specifically, we can compare the theory of innate ideas with the conditions laid down explicitly by Boyle for a good scientific hypothesis, conditions which we considered when looking at Boyle's work in Chapter VI.

Boyle's first requirement for a good hypothesis was that it should not be "precarious, but have sufficient grounds in the nature of the thing itself ...". The hypothesis of innate ideas did not fulfil this condition, because, as Locke argued, there are no universal ideas common to all men which could be candidates for examples of innate ideas.

The hypothesis of innate ideas also failed to satisfy Boyle's second criterion, that of simplicity, for Locke believed that the phenomena of ideas could be explained by a simpler hypothesis, namely that all ideas are the product of experience. The theories of knowledge which appealed to innate ideas did not deny that some ideas are the product of experience, they therefore postulated two causes, where Locke was content with one, and therefore did not satisfy Boyle's second criterion, that of containing "nothing that is superfluous or impertinent."

By the same token, Locke argued that it was not "the only hypothesis that can explicate the phenomena"; Boyle's third condition. And, interestingly, Boyle's fourth condition was not met either, though Locke only half acknowledged this. The fourth condition was that the hypothesis "enable a skilfull naturalist to foretell future phenomena". That the theory of innate ideas had no predictive power can be easily shown. Since Locke demonstrated that to have an innate idea turned out to be nothing other than the capacity to have certain ideas, with no specific criterion for being able to test that capacity, to attribute innate ideas to an individual guaranteed nothing about his professed beliefs, his future behaviour, or, indeed, anything at all. Locke half recognised the point when he showed that there appear to be no actions which have not been thought good by some people or other, even though condemned by most Europeans; cannibalism, for example. But Locke never clearly made the point that the attribution of innate ideas had no predictive power.

Given that Locke's rejection of innate ideas is achieved by treating the theory as a scientific hypothesis, and showing that it did not match up to the criteria for an hypothesis that were accepted by the empirical scientists, cannot it be argued that Locke's rejection of the theory in order to substantiate the empirical method is circular? Did not Locke presuppose the method for which the Essay was intended as a vindication? Undoubtedly there is some substance in this charge. Locke had himself described his approach

to the issues considered in the Essay as "this historical, plain method"²³, but it is important to remember that Locke did not confine himself only to that method, for, as we have seen, he did not reject the theory of innate ideas only on the argument that it is an unsatisfactory hypothesis in the way we have just considered, for he also argued that the concept of an innate idea is an incoherent one. The theory which Locke attacked maintained that there were ideas in the mind of which the mind was not aware. Against this, Locke held that ideas must be conscious if they are to be said to exist at all. This is a logical point about the concept of an idea, and has nothing directly to do with whether or not innate ideas are acceptable as scientific hypotheses.

Given this, it appears that the crucial argument against innate ideas is whether or not they are a coherent notion. It is not my intention to pursue this topic at any length; the argument on this topic, although important, is more far-reaching than the scope of this work; but one argument is worth urging in favour of Locke's position. In the normal use of the word 'idea', it makes sense to ask when any particular idea was acquired. Acquisitions of ideas are thought to be datable, and their dating is tied to the time at which the holder of the idea became conscious of the idea. Expressions like 'Aristotle was the first man to have the idea of drawing up the rules of formal inference' seem both intelligible, decidable, and often true. But, if we were to accept the theory of innate ideas which Locke attacked, no such expressions could be

known to be true, for on the theory, all that we could say was that Aristotle was the first man who became conscious of the idea, not that he was the first to have it, because for all we know, somebody else might have had it before unconsciously. But what would be the advantage of so talking? The answer is 'None'. It would be a way of talking introduced just to preserve a theory, namely, the theory of innate ideas. Further, the theory itself would have no useful function; it would complicate our language without explaining anything. Indeed it would be worse than this, because whereas before to have an idea was linked with some definite criterion, namely consciousness, there would now be no criterion for having ideas: their status would slide into obscurity. There is, then, nothing to be gained, and much to lose, by accepting the theory, and this is sufficient reason for rejecting it.

Undoubtedly part of the difficulty in assessing Locke's contention that the concept of an innate idea is incoherent is that seventeenth century philosophy turned the word 'idea' into a technical term. It became, under the influence of Descartes and Locke, a specialist term of the epistemologist, whilst still retaining many of its non specialist implications. Technical terms can be used by their employers in any way they see fit, as long as their employment is consistent. But the same is not true of non-technical terms. They cannot be used in any way

a particular person thinks fit, in Humpty Dumpty fashion²⁴, because their meaning is not dependent on the fiat of an expert, but on the use which they normally have within the language of which they are a part. Undoubtedly, a great deal of confusion, both for the writer and the reader, of seventeenth century philosophy is the product of the use of the term 'idea' in both a technical and non-technical sense in the same work by the same writer.

The first Book of the Essay concludes with a series of remarks which I believe are significant, not only for the argument against innate ideas, but also for the whole of the Essay. Locke said that since some of the arguments used against the notion of innate principles "rise from common received opinions, I have been forced to take several things for granted; which is hardly avoidable to any one, whose task is to show the falsehood or improbability of any tenet."²⁵ But he went on to promise that the remainder of the Essay would not be so dependent on a multitude of unproven assumptions. He wrote:-

"But in the future part of this Discourse, designing to raise an edifice uniform and consistent with itself, as far as my own experience and observation will assist me, I hope to erect it on such a basis that I shall not need to shore it up with props and buttresses, leaning on borrowed or begged foundations: or at least, if mine prove a castle in the air, I will endeavour it shall be all of a piece and hang together." 26

Locke held that even if the premises of his argument were not all acceptable to other men, he still believed that the remainder of his argument would supply a coherent account of the extent of human knowledge.

But what of the premise of the remainder of the work? They, Locke said, are derived from experience. "All I shall say for the principles I proceed on is, that I can only appeal to men's unprejudiced experience and observation whether they be true or not;" Locke wrote, "and this is enough for a man who professes no more than to lay down candidly and freely his own conjectures, concerning a subject lying somewhat in the dark, without any other design than an unbiassed inquiry after truth."²⁷

We should not, of course, be surprised to find that Locke construed his axioms as experiential. (Though, unfortunately, he never attempted to set them down in any ordered way.) If all our ideas are derived from experience, then this must also be true of the ideas which are used to compose the axioms of any account of knowledge. But it interesting and important that Locke's stated position with regard to the axioms of his system or account of knowledge should be the same as those from which Newton also began in the Principia. As Newton claimed for the axioms of his physics that they were deduced from the phenomena, so Locke also appealed to "men's own unprejudiced experience and observation."²⁸

But if Locke's axioms are all experiential cannot the charge of circularity be made once again? If all the axioms are experiential, then surely they cannot be used to establish that all knowledge is ultimately the product of experience, for, surely, the conclusion is presupposed in the premises? I think that Locke might have

been tempted to reply to this charge along these lines: 'Very well, if you wish to press that point you may have it. But at least allow that my account is internally consistent. If it is only "a castle in the air, I will endeavour it shall be all of a piece and hang together"'. But if this would have been Locke's reply it would not in fact have done justice to his own case, for it is in fact considerably stronger than this, for two, rather different, but substantial reasons.

First, Locke's thesis that all our knowledge is ultimately derived from experience is a better hypothesis, judged by the criteria of the new science, than any of the alternatives, or rather, the alternative, for Locke believed that there was only one, namely, the theory of innate ideas. This, in itself, does not eliminate the charge of circularity, but it does make the circle larger, for it exemplifies that Locke's account of knowledge is consistent with the method of the new science. Further, Locke had argued that the alternative to his account had at least one internal contradiction. Given that the theory of innate ideas is unsatisfactory for this reason, and as long as Locke's own theory is not also inconsistent, and there are no other possible explanations, then Locke's account must be in general, if not always in particular, correct.

Conclusions.

A large part of the significance of Locke's argument in Book One lies in his adoption of the approach of a scientist to a problem in epistemology. It exemplifies, in a way characteristic of the new science, the natural philosopher's approach to an existential claim about an hypothetical entity. Perhaps for the first time in the history of philosophy, and certainly for the first time in a great work, the method of science as we have come to understand it, dominates a piece of philosophy. Before the seventeenth century it was characteristically philosophy which dominated science, and this is to a marked degree true even of such a modern scientist as Descartes. And, equally important, the transference of the method of science to philosophy is not found wanting. The hypothesis which Locke attacked could not withstand the onslaught of the new intellectual weapons.

But the effect of Locke's attack on innate ideas had wider repercussions. For if his rejection was justified it followed that a new account of the nature of the eternal truths or maxims was required. For the theory of innate ideas had supplied an answer to the question 'Why do we accept some propositions as eternally true?' According to the theory, we accepted them as true because God had imprinted them on our minds at our creation. But, given the efficacy of Locke's rebuttal, a new answer was called for. Although Locke never gave a general answer to the question, he did

make it possible for such a general answer to be given. One effect, therefore, of Locke's rejection of innate ideas was to clarify the issue of the status of propositions which are necessarily true, a problem which, as we have at various points seen, was an issue which had led to not a little confusion in the course of the scientific revolution.

Chapter X

The Basis in Experience

Introduction.

Locke believed that if he was to succeed in supplanting the theory of innate ideas by his experiential account of knowledge he must show that all the ideas that we do have are the product of experience. This is the task which he set himself in the second Book of the Essay. Not surprisingly, it is the longest of the four. The task is, in the Baconian fashion, to give a natural history of our ideas, to show that every one of them can be accounted for by reference only to experience and nothing else. The method gives a specious objectivity to Locke's approach which is not always justified, for several key concepts, such as those of causation, and substance, are claimed by Locke to be grounded in experience in at least some sense, when his argument in fact does little to substantiate the claims.

Book Two contains the meat of Locke's empiricism. As already hinted, it also contains several of his most notorious errors. The juxtaposition of sound, often brilliant, argument, with unjustified assumption, is one of the most frustrating aspects of Book Two. What is even more annoying is that the mistakes that Locke made often are irrelevant to the general trend of his argument. Thus Book Two takes for granted that the objects of all experience are ideas, a position which, as we shall see, Locke never justified, and which is largely irrelevant to his general account of the extent and limits of knowledge which we find in Book Four.

In one chapter it is quite impossible to treat all of the topics raised by Locke, and, indeed, some of his most interesting views, for example, his theory of personal identity, will not be considered. We shall concentrate on those aspects of the Book which most clearly illustrate the Essay's connections with the scientific revolution, and those which are most central to his positive account of knowledge in Book Four.

The Nature of Ideas.

In Chapter I of the Essay Locke had apologised "for the frequent use of the word idea It being that term which, I think, serves best to stand for whatsoever is the object of the understanding when a man thinks, I have used it to express whatever is meant by phantasm, notion, species, or whatever it is which the mind can be employed about in thinking; and I could not avoid frequently using it."¹ The existence of such entities, Locke believed, was substantiated empirically. "I presume it will be easily granted me, that there are such ideas in men's minds: everyone is conscious of them in himself, and men's words and actions will satisfy him that they are in others."² Granted their existence, the problem which Locke saw himself as solving was "how they come into the mind". Having, in Book One, vanquished the theory of innate ideas as a plausible answer to the problem, in Book Two Locke put forward his own positive account. All ideas, he argued, are the product of experience.

Before we turn to Locke's detailed account of the origin of ideas we should take note of various aspects of his initial position.

Ideas, Locke held, are whatever it is that a man is aware of when he thinks, they are the raw materials which the mind uses. The mind, Locke believed, was a kind of thing, an entity, analogous with a physical object, but made not of physical, but mental, substance. Locke explained the problem that he will solve in Book Two using this analogy: "Let us then suppose the mind to be, as we say, white paper, void of all characters, without any ideas:- How comes it to be furnished? Whence comes it by that vast store which the busy and boundless fancy of man has painted on it with an almost endless variety? Whence has it all the materials of reason and knowledge?"³ Locke took over from Descartes the view that the mind is a thing, separate from physical things, yet analogous to them, unquestioningly. Given the assumption, it is very natural to construe mental entities as properties of mind, just as it is natural to think of the type on the printed page as being a property of that page. And given that physical objects were subject to the laws of physics, and had a structure which could be analysed according to the corpuscular philosophy, it was fairly natural to suppose that the mind was also subject to para-physical laws, and had a para-physical structure which could be analysed by a similar process. Locke took only the most elementary steps towards supplying the specious laws of this para-physics, in a chapter called 'Of the Association of Ideas', which he added to the second Book of the fourth edition of the Essay. But Locke's work did attempt to give an account of the para-physical properties of mind, and he did this assuming a model which was the same, or very nearly the same, as the

model employed by those who advocated an atomic theory of matter. Whether Locke did this deliberately we do not know, though it seems unlikely, for to have done so would have entailed Locke approaching his "natural history of the soul", (to use Voltaire's phrase), with preconceptions, an attitude at odds with the plain historical method to which he aspired. In support of this, it is worth noting that a large part of the first chapter of Book Two reiterates Locke's objections to the Cartesian view that the souls always thinks, underlining how important Locke took it to be that mental events cannot sensibly be talked of except as conscious happenings, a point which he rightly saw as crucial to the establishment of his empiricist thesis. Once again he inveighed against Descartes and the Cartesians with the argument that the Cartesian view was merely an hypothesis of the unacceptable sort. Thus he wrote:-

"It is doubted whether I thought at all last night or no. The question being about a matter of fact, it is begging it to bring, as a proof for it, an hypothesis, which is the very thing in dispute: by which way one may prove anything, and it is but supposing that all watches, whilst the balance beats, think, and it is sufficiently proved, and past doubt, that my watch thought all last night. But he that would not deceive himself, ought to build his hypothesis on matter of fact, and make it out by sensible experience, and not presume on matter of fact because of his hypothesis, that is because he supposes it to be so; which way of proving amounts to this, that I must necessarily think all last night, because another supposes I always think, though I myself cannot perceive that I always do so." 4

A man so opposed to hypotheses was hardly likely consciously to approach his account of the mind with preconceptions about its nature.

At the end of the first chapter Locke draws an important distinction between two difference causes of our ideas. Some ideas, he said,

are caused by sensation, and others are caused by reflection. What Locke meant by an idea of sensation is any idea which is the product of a causal chain which has its origin in the motion of some part of the human body. On the basis of empirical evidence Locke believed that there are no ideas until man has ideas of sensation.

The second sort of cause for ideas that Locke offered was what he, significantly, called 'reflection'. He explained what he meant by ideas of reflection in the following words:-

"The mind receives the ideas [of sensation] from without, when it turns its view inward upon itself, and observes its own actions about those ideas it has, takes from thence other ideas, which are as capable to be the objects of contemplation as any of those it received from common things." 5

Two examples are thinking and willing.

Ideas of sensation and reflection are generally unavoidable, Locke pointed out, in the sense that we have no control over what colour we see when we look at a rose, or what sound we hear when we listen to a bird singing,⁶ any more than "a mirror can refuse, alter, or obliterate the images or ideas which the objects set before it do therein produce."⁷ The implication of this is that the origin of many ideas can be explained entirely mechanically. Locke certainly did not believe that man was a machine, but he did believe that quite a lot about his mental life could be explained by invoking mechanism. (Locke, incidentally, rejected the Cartesian view that animals are machines. He believed, on the basis of observation, that some of them could reason.⁸).

A more detailed examination of Locke's account of the causal chain involved in the production of ideas will follow later. For the moment let us just note that Locke evoked causal explanations of ideas entirely on an empirical basis in a way in which everybody is familiar. We are, indeed, so well aware of the causal origins of sensation that Locke's thesis is almost a commonplace. But this was one of its greatest strengths; nobody from Plato on (with the possible exception of Malebranche, to whom Locke himself offered a reply) had rejected the view that physical causal explanations were appropriate as an account of some of men's ideas. Locke's much more difficult task was to show how it could account for all of them.

If the causal account of ideas was a contemporary commonplace, substantiated by vast empirical evidence, the same cannot be said of Locke's second thesis about ideas, the division he propounded between simple (atomic) ideas, and complex (molecular) ones. The confusions in Locke's theory will not long delay us, for they have been well considered elsewhere.⁹ I shall just take one example to illustrate the difficulties that Locke is in with his distinction, and then we shall consider why it was that Locke wished to put forward such a theory.

Locke put forward at least three different criteria for simple ideas of sensation. Two of them were:-

(a) a simple idea is one which "in itself uncompounded, contains in it nothing but one uniform appearance, or conception in the mind, and is not distinguishable into different ideas",¹⁰

and, (b), a simple idea is one which cannot be invented or framed. It is one which we come by as passive recipients and which is entirely unconnected with the imagination or will.¹¹

As an example of a simple idea Locke offered "the whiteness of a lilly", and it is not difficult to see what it was that Locke meant by such an example. Assuming that the degree of whiteness is uniform, then there does seem to be a sense in which the colour is entirely simple. (Though, even here there are difficulties; are we not bound to notice both the colour and its boundary?) But if we accept Locke's example, it might be argued that such examples could be framed independently of experience, Locke's second criterion. For, as Hume pointed out, it certainly seems perfectly possible for somebody who is acquainted with all the colours that there are, except for one particular shade of blue, and who is presented with all the other shades of blue in a continuous gradation, except the shade with which he is not acquainted, to imagine the missing shade, even though he has never seen it.¹² So it might well be the case that Locke's two criteria could in fact come into conflict, and certainly it is only a contingent fact, if it is a fact, that they do not.

How damaging are these difficulties to Locke's account of ideas? The answer, I believe, is that they are only damaging because Locke believed that an empiricist account of knowledge must be given in terms of an atomic and molecular theory of ideas. But there is no reason to believe, a priori, why an empiricist account of knowledge should be given in these terms. There is no reason, even on empirical grounds, why one should believe that experience should be analysed in this sort

of way. For us to see that this is so it is necessary for us to do what we have already suggested, namely to examine the reasons why Locke wished to put forward this particular account of the genesis of ideas.

A powerful influence on Locke's account of simple and complex ideas was his recognition of the success of the corpuscular philosophy in explaining the properties of material objects, and I believe that it was this model which was of crucial importance for Locke's theory. The evidence for this view, though generally unnoticed, is actually to be found in the Essay. The crucial passage is the one which follows. It is significant that in it, when Locke explains the origin of simple ideas, he directly compares the workings of the mind with the physics of nature. He wrote:-

"But it is not in the power of the most exalted wit, or enlarged understanding, by any quickness or variety of thought, to invent or frame one new single idea in the mind, not taken in by the ways before mentioned: nor can any force of the understanding destroy those that are there. The dominion of man, in this little world of his own understanding being muchwhat the same as it is in the great world of visible things; wherein his power, however managed by act and skill, reaches no further than to compound and divide the materials that are made to his hand; but he can do nothing towards making the least particle of new matter, or destroying one atom of what is already in being. The same inability will every one find in himself, who shall go about to fashion in his understanding one single idea, not received in by his senses from external objects, or by reflection from the operations of his own mind about them." 13

The modern age of science has generally been taken as the overthrow of the medieval conception of the macrocosm and microcosm. But, undoubtedly, in Locke's words here we find an updated return to that analogy, albeit with a subtle twist. Man is to be understood

in terms of the universe; not the universe in terms of man. Simple ideas, Locke believed, correspond to the atoms of matter of which

Newton was to write in the Opticks:-

"it seems probable to me that God in the beginning formed matter in solid, massy, hard impenetrable, movable particles, of such size and figures, and with such other properties and in such proportion to space as most conduced to the end for which he formed them; and that these primitive particles being solids are incomparably harder than any porous bodies compounded of them, even so very hard as never to wear or break into pieces, no ordinary power being able to divide what God himself made one in the first creation." 14

Newton's words were, of course, written several years after Locke's.

But the same theory had been argued for by Boyle in the Skeptical

Chemist. We can illustrate Boyle's position with his own words as follows:-

"And to prevent mistakes, I must advertize you, that I now mean by elements as those chemists, that speak plainest, do by their principles, certain primitive and simple, or perfectly unmingled bodies, which not being made of any other bodies, or of one another, are the ingredients, of which all those called perfectly mixt bodies are immediately compounded, and into which they are ultimately resolved" 15

Boyle also subscribed to the view that the basic atoms of matter could not be created by man:-

".... it far exceeds the power of meerly natural agents to produce anew, so much as one atom of matter, which they can but modifie and alter, not create; which is so obvious a truth that almost all sects of philosophers have denied the power of producing matter to second causes." 16

In such views surely we can see the origin of Locke's account of ideas. Just as in the corpuscular theory it was reckoned possible to account for the great variety of entities by reference to the combination of a limited number of simple elements in various complexes, so Locke

held it was possible to account for the wide variety of ideas which man is capable of experiencing, by reference to the elements of simple ideas, which, like the atoms of the physical world, cannot be created or destroyed, but only combined in various ways.

There was one important difference between Locke's atomic account of experience and the corpuscular philosophy with which it had so much in common. Locke believed that it was better grounded in the facts than the corresponding physics because, unlike the atoms of matter, the atoms of experience were directly visible. Locke recognised that the atoms of Boyle's science were only inferred entities, we have "no ideas of the particular primary qualities of the minute parts".¹⁷ But the atoms of experience were everywhere presented to us. The implications of this, though Locke did not fully recognise them, are considerable. It follows that in theory at least, the science of mind can be better grounded in the known facts than the science of matter. It is not, therefore, surprising that the philosophers who followed Locke were more attracted to an Idealist account of the world. The world of ideas, on Locke's own theory, was much more knowable than any other.

I do not wish to be taken as suggesting that the only reason why Locke supported an atomic theory of ideas was because of his inclination towards an atomic theory of matter. Quite clearly, he believed that the account of experience which he offered was substantiated by the facts. But the facts of experience are in fact

themselves compatible with other than atomic theories of experience, and I would suggest that Locke might well have recognised this himself if he had not been so inclined to support an atomic theory of matter.¹⁸

Given the theory, Locke's task was to show that any idea can be shown to be composed of simple ideas. For many ideas Locke offered plausible explanations, but, notoriously, he was not universally successful. One such case, perhaps the most important, was the "obscure and relative idea of substance in general", which Locke could never convincingly claim was analysable into simple ideas, but which he was equally reluctant to abandon.

If the analysis of experience for which Locke argued owed much to contemporary physical theory, it also exhibits the first signs of what has become known as logical empiricism, a position itself logically - if not causally - independent of contemporary science. The position might be expressed in this way. When Locke talks of simple ideas, what he should really have said is that there are some terms which cannot be defined in terms of other words, but only ostensively. Other terms can be defined in terms of other words, but eventually the terms included in the definiens must themselves be capable of ostensive definition. All meaningful discourse, therefore, must use terms which are either themselves derived directly from experience, (by ostensive definition), or must be capable of being indirectly so derived. Any terms which cannot be so defined or derived are meaningless. Therefore, all meaningful discourse is ultimately dependent upon experience, and, a fortiori, so are all claims to

knowledge.

Locke, however, was prevented by his own account of ideas and language from ever presenting his theory in this sort of way, even if he had wanted to, which is itself rather doubtful. Locke's theory of language included the doctrine that words were simply names for ideas.¹⁹ It was for Locke, therefore, ideas which were basic, and it was upon his account of ideas that his theory of meaning was founded. In contrast, the logical empiricist's account of meaning does not presuppose a theory of experience, for it is from the theory of meaning that the empiricist thesis that all knowledge is derived from experience is itself derived. Thus, for the logical empiricist, the way in which I come by my experience is itself not a central issue, whereas for Locke it was entirely central.

The reason why this was a central issue for Locke hardly needs elaborating. It was because he was treating the mind or human understanding as if it were an object which could be examined and explored in the same sort of way as the objects of the physical world can be examined. The plain historical method led him to explore the phenomena of the mind, and he tried to understand its mechanics before he ever turned to the problem of how it was at all possible for the language which he used to be significant.

Locke's Causal Theory of Perception.

It is clear from Locke's writings that he considered the theory of perception which he advocated to be a well-established empirical

fact, in so far as it both could be an empirical fact, and was also actually known about. The phenomena are clear, Locke argued, but some parts of the detailed mechanism are obscure. The account can be stated, using the most prevalent example, that of sight, as follows. Light travels from a light source to an object; it is reflected from that object to the eye of an observer; it enters the eye and produces an image on the retina. From the back of the retina, motion is propagated along the nerves to the brain; in some (unexplained) way the brain causes an idea to occur to the mind which is our idea of the object. Although Locke never gave a very clear statement of the theory which he obviously accepted, he came close to it in the following:-

"The next thing to be considered is, how bodies produce ideas in us; and that is manifestly by impulse, the only way which we can conceive bodies operate in.

"If then external objects be not united to our minds when they produce ideas in it and yet we perceive these original qualities in such of them as singly fall under our senses, it is evident that some motion must be thence continued by our nerves or animal spirits, by some parts of our bodies, to the brains or the seat of sensation, there to produce in our minds the particular ideas we have of them. And since the extension, figure, number, and motion of bodies of an observable bigness, may be perceived at a distance by the sight, it is evident some singly imperceptible bodies must come from them to the eyes, and thereby convey to the brain some motion, which produces these ideas which we have of them in us." 20

Like everyone else in the seventeenth century, Locke accepted that action at a distance is impossible, and that motion can produce nothing but motion in the physical world. (Though clearly not in the mental world.) But whether these are contingent or necessary truths Locke does not say. As so often in his account he uses a

psychological, not a logical, word to indicate what he takes to be impossible, and fails to tell us whether the inconceivable is also the logically impossible, or is simply taken by him to indicate the limit of our psychological capacities. But Locke openly admitted that the transition from the physical motion to the production of the non-physical idea in the mind was something which he could not explain, an inability which does not appear to have worried him; the gap in the story was openly admitted and openly dismissed:-

"It is therefore the actual receiving of ideas from without that gives us notice of the existence of other things, and makes us know that something does exist at that time without us, which causes that idea in us; though perhaps we neither know nor consider how it does it. For it takes not from the certainty of our senses, and the ideas we receive by them, that we know not the manner wherein they are produced" 21

The phenomena, Locke claimed, establish that ideas, in what is called normal perception, are produced by outside objects; the fact that the mechanism which produces them is inexplicable, is irrelevant. It was the constant conjunction of ideas and objects, Locke held, which allowed us to assert the causal link, not our detailed knowledge of the actual mechanism which in fact is the causal chain. Indeed Locke's position about the causal connection which exists between object and idea was exactly the same as the position which Newton advocated with respect to gravitation. That objects do tend to move towards each other inversely as the square of their distance, and in relation to their mass, was Newton held, a phenomena well established empirically. But Newton did not pretend to have

established "the cause of this power". He would not frame (or feign) an hypothesis to account for this, because such an hypothesis would go beyond the evidence in an unacceptable way. "And to us", he wrote, "it is enough that gravity does really exist and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies and of our sea."²²

What I am here suggesting is that Locke's lack of concern for his incapacity to explain the connection between mind and matter is explained by his commitment to the same sort of empirical outlook that we find exhibited by Newton towards his inability to explain gravitation. But it is another matter whether Locke was right to see the problem in this way. For, whilst Newton could, and indeed did offer - albeit reluctantly - explanations for gravitation in terms of the ether, (an entity for which he believed there was some empirical evidence), it is not at all clear that any plausible hypothesis could be devised by Locke to account for an interaction between the two entirely disparate substances mind and body.

This difficulty for Locke, and for all dualist theories, can best be appreciated by considering the notion of interaction. We normally understand the concept of interaction in terms of physical objects, and the paradigm of such was the interaction between two physical objects as described by Cartesian - or later - Newtonian mechanics. The most simple example of such interaction is when one object in motion pushes another. But the mind, as Locke understood

it, was not the sort of thing of which it makes sense to say that it can be pushed, for nothing physical can be truly said of it at all. We, therefore, do not know what it is to talk of physical substance interacting with mental substance at all. It was not, therefore, just that Locke did not have an hypothesis to account for the mechanism of mind-body interaction, but that there seems to be no possible hypothesis which would do what is required.

This objection had already been made in the seventeenth century when Locke wrote the Essay, for Gassendi had made it in reply to Descartes' conception of the relation between mind and body. Concerning the possibility of bodily movement being initiated by the unextended soul he wrote:-

"How can there be effort directed towards anything, and motion on its part, without mutual contact of what moves and what is moved? How can there be contact apart from body, when (as is so clear to the natural light)

'Apart from body, nought touches or is touched'?" 23

To this sort of objection Locke, no doubt, would have considered he had a reply. It might be expressed like this : 'I grant our inability to understand the nature of this interaction, but it is obvious that it does occur from experience. Further, our position is no better with a whole range of phenomena with which we are constantly presented.' One such was the problem of the cohesion of the parts of material objects, which Locke used to illustrate that our understanding of matter was at least as scanty as our understanding of minds. Locke took as an example, the cohesion generated in water by freezing. Water he assumed to be atomic

in structure. The parts of water do not cohere when it is in a liquid state, but do when it is frozen, and of this he wrote:-

"He that could find the bonds that tie these heaps of loose little bodies together so firmly; he that could make known the cement that makes them stick so fast one to another, would discover a great and yet unknown secret; and yet when that was done, would he be far enough from making the extension of body (which is the cohesion of its solid parts) intelligible, till he could show wherein consisted the union, or consolidation of the parts of those bonds, or of that cement, or of the least particle of matter that exists. Whereby it appears that this primary and supposed obvious quality of body will be found when examined, to be as incomprehensible as anything belonging to our minds, and a solid extended substance as hard to be conceived as a thinking immaterial one, whatever difficulties some would raise against it." 24

Locke's point here is clear. In passing it is also worth noting that it was not merely academic. Galileo, for one, had offered a theory of the strength of materials which raised the difficulty which Locke here had identified.²⁵ Similar problems arose, Locke argued, when we looked in detail at how it was that one body communicated motion by impulse to another body, just as much as when we tried to understand how mind could excite motion by thought.

Of these Locke wrote:-

"For in the communication of motion by impulse, wherein as much motion is lost to one body as is got to the other, which is the ordinarist case, we can have no other conception, but of the passing of motion out of one body into another; which, I think, is as obscure and inconceivable as how our minds move or stop our bodies by thought, which we every moment find they do." 26

And, like Newton, Locke, in his second reply to Stillingfleet, showed that although the cause of gravitation was something he did not understand, nevertheless its existence was established empirically by Newton's work:-

"The gravitation of matter towards matter, by ways inconceivable to me, is not only a demonstration that God can, if he pleases, put into bodies powers and ways of operation above what can be derived from our idea of body, or can be explained by what we know of matter, but also an unquestionable and every where visible instance, that he has done so." 27

Locke went on to say that the next edition of the Essay would take account of Newton's discovery; which is further evidence that the Principia did not make an impact on Locke before the publication of the first edition of the Essay.

Locke's conception of Causation.

From the examples which we have just considered it is possible to see that Locke was committed to a certain theory of causation. In fact Locke was committed to an account of causation which comes very close to, though it is by no means so clearly stated, as that which was later to be advocated by Hume. Implicit in Locke's examples is the view that cause and effect are to be identified by means of the existence of constant conjunctions in experience. By experience we know, so Locke argued, that our idea of an object, and it being before us, (as in ordinary vision), are conjoined. We therefore have the right to assert that the object's being before us is a cause of our having this idea, even though we cannot understand the mechanism which brings this about. And just the same situation holds of attributing the cause of motion in one object to its being pushed by some other object, even though the actual transmission of motion by impulse is to us unintelligible.

Locke explained how we come by our idea of causation in Chapter XXVI of Book II of the Essay. His account begins, unhappily,

with a circular definition. But when he turned to particular examples, although even here his formulation is rather muddled, the picture which emerges is one which is generally consistent with his thesis that all our ideas are derived from experience.

He began his account as follows:-

"In the notice that our senses take of the constant vicissitudes of things, we cannot but observe that several particular, both qualities and substances, begin to exist; and that they receive this their existence from the due application and operation of some other being. From this observation we get our idea of cause and effect." 28

Already Locke is in some difficulty. Clearly we cannot derive our idea of cause and effect from noticing that some object ("other being") operates to produce the existence of some other object or quality, for to notice this is to notice that a cause and effect relation holds, and cannot, therefore, be itself an explanation of that idea. Similarly, Locke's definition of causation leads him into difficulty: "That which produces any simple or complex idea we denote by the general name, cause, and that which is produced, effect." As Fraser noted, Hume asked 'what does he mean by production?'²⁹ But, this aside, Locke's statement is true, even if unhelpful. He is much more helpful when he turned to examples:-

"Thus finding that in that substance which we call wax, fluidity, which is a simple idea that was not in it before, is constantly produced by the application of a certain degree of heat we call the simple idea of heat, in relation to fluidity in wax, the cause of it, and fluidity the effect." 30

For the moment, ignoring Locke's confusion in treating heat both as an idea and as a property of the wax, (a difficulty which he is led into by his distinction between primary and secondary qualities), we can see that his basic assertion is that we call heat the cause

of fluidity in wax because whenever heat is applied to wax the wax melts. Our knowledge of cause and effect, Locke here maintained, is entirely the product of experience, and he held that the experience consists of two elements, constant conjunction of cause and effect, and what Locke called 'power'. The origin of the constant conjunction in experience is clear enough. But what about power? Locke also believed that this could be explained empirically. His account of the origin of our idea of power was this:-

"The mind being every day informed, by the senses, of the alteration of those simple ideas it observes in things without; and taking notice how one comes to an end, and ceases to be, and another begins to exist which was not before; reflecting also on what passes within itself, and observing a constant change of its ideas, sometimes by the impression of outward objects on the senses, and sometimes by the determination of its own choice; and concluding from what it has so constantly observed to have been, that the like changes will for the future be made in the same things, by like agents, and by the like ways, - considers in one thing the possibility of having any of its simple ideas changed, and in another the possibility of making that change; and so comes by that idea which we call power." 31

There is a sense in which Locke's statement of how we come by the specific abstract idea of power is perfectly adequate, for he gives an explanation of how it is that we come by this idea as a result of experience. But, in another, and more important, sense, Locke's account is far from acceptable, because he slides from a statement of observations of successive ideas to the assertion that we notice that some of these changes seem to be caused by our choices. Thus he employs the concept he is explicating in his explanation. He therefore failed to explain at all why we ever, or

how we ever, make the move from succession to causality. Granted that Locke's account is for this reason far from satisfactory, it would be wrong to dismiss it completely, because it does have some important merits, as well as its fairly obvious inadequacies.

The first and most important merit of Locke's account is that by it Locke is not committed to the view that there is a logical connection between cause event and effect event. That this was in fact Locke's position can be seen if we take his account of causality in conjunction with his remarks on logical necessity, and many comments about the powers of God.

Although Locke did not use the expression 'logical necessity', he did talk of 'the necessary agreement between ideas', and also of contradiction. An example of the necessary agreement between ideas is exemplified by any tautology, or what Locke called 'trifling proposition', such as 'lead is a metal'. In Locke's language, the complex idea of lead contains within it the idea of metal.³² Knowledge, Locke held, was always a case of "the perception of the connexion of and agreement, or disagreement and repugnancy of any of our ideas".³³ And in this example there is a connexion between our idea of lead and our idea of metal. But in the case of most causal statements (though not all) there is no such agreement between our ideas. The attribution of a cause and effect relation is based upon observation of succession, and nothing else. Thus, to take one of Locke's examples, there is no connexion in our ideas between wood, fire, and ashes. We simply discover empirically "that the substance wood by the application of fire, is turned

into another substance, called ashes."³⁴

What emerges, therefore, from Locke's account of causation is that although he failed to give an adequate account of the origin of our idea of causation because his explanation presupposes our already having the idea of causation, he nevertheless did imply that causal connections can only be discovered through the observations of regularities.

Locke's conception of how we can identify causes must be seen in the light of his opposition to hypotheses, which we have already found in both his science and his philosophy. It was essential to that view of science that causes should not be (because they could not validly be) inferred from effects. It was central to Newton's view of science, as we have seen. It was the position which Glanvill had so clearly expressed in The Vanity of Dogmatizing. It lay at the core of the anti-Cartesian attitude in science which, on the question of method, was so prominent amongst the English scientists of the period.

But there was a further reason for Locke to reject a logical link of a sort that would make it possible to deduce causes from effects. The view that there was no such connection followed from a theological premise which the English scientists accepted. The premise was that God was free to make anything the cause of anything. It followed from this that what he had in fact made the cause of what was only decidable empirically. It was for exactly this reason that Locke, in the Essay, was willing to postulate the proposition

that matter might think, a position which he explained most clearly in his second reply to Stillingfleet.³⁵ If God had so decided, Locke argued, He could have created thinking matter, because there is no contradiction in the notion of thinking matter, and nobody, before Descartes, Locke said, ever thought that there was. If there is no contradiction, to deny that it is possible that matter might think is to place boundaries on God's omnipotence, and the only boundary, rightly, that Locke was willing to allow was contradiction. It is worth adding, in connection with this, that the advocacy of the unrestrained freedom of the Divine Will had a history in English empirical thought going back at least to Ockham, who had also advocated the theological premise which we find in Locke and the English scientists of the period.³⁶

Our investigation of Locke's concept of causation, presupposed by his theory of perception, reveals that although Locke's account is far from adequate, it was one which was consistent with the developments in the physical sciences currently taking place in England. Moreover, for all its haziness, the concept of causation which Locke explored and for which he offered his account was the concept which was actually employed by the scientists, and by Newton especially. Like Newton, Locke accepted the total contingency of the discoveries of the scientist, including the causal discoveries, and he was committed to that conception of causation which was itself to be both clarified and criticised by Hume, and later, Kant.

It was a conception of causation which made its impact under the influence of the new science and the new empirical philosophy.

Ideas and Objects.

According to Locke's causal theory of perception, ideas are produced in our minds as a result of a causal chain stretching from object to mind. This has given rise to what has become a traditional objection to Locke's account. If we are only presented with ideas in experience, and all our knowledge comes from experience, surely the only things which we can know to exist are ideas. We can never know that there are independent material objects of the sort that Locke believed are often the initial causes of our ideas. If this criticism of Locke is correct then clearly it is very damaging to his whole account of perception at least, if to nothing else.

Generally, philosophers have agreed that the criticism is justified. Locke assumed that there are material objects, yet on his own theory he cannot know that there is anything besides ideas. In the last analysis I think this criticism of Locke is correct; but the issue is more complicated than it is often taken to be. It will therefore be worth considering the argument in a little detail.

The first point to note is that no account of perception can escape from the fact that we normally construe perceptual situations as ones in which there are things perceived and an awareness of the perception. Thus, if I see a tree, then there is a tree, and I also

have a visual experience. Normally, that is, perceptual occurrences are taken to involve both objects independent of me and my having certain experiences. In his account of perception, Locke wished to do justice to this part of our normal understanding of perception. Thus, when Locke argued that what we are aware of in perception are ideas, he generally meant that when we perceive objects we have experiences. There is no reason to believe that Locke thought of these experiences as being of a particular sort, for example, images, but, equally, he thought images to be one sort of idea. The point is made by Locke in his chapter on perception:-

"this is certain, that whatever alterations are made in the body, if they reach not the mind; whatever impressions are made on the outward parts, if they are not taken notice of within there is no perception. Fire may burn our bodies with no other effect than it does a billet, unless the motion be continued to the brain, and there the sense of heat or idea of pain, be produced in the mind; wherein consists actual perception." 37

Locke justified his acceptance of this conception of perception entirely on empirical grounds. It was, for Locke, simply an empirical fact that the causal chain linking the mind and the world must be completed if experiences of the external world, including pain and other bodily experiences, are to occur. But the problem is whether or not we could ever be justified in believing that there are external objects at all, whether there are any other 'things' in the world besides ideas and minds.

To this question Locke answered that man was justified in a belief in an external world. He offered two arguments for his position, justifications which, if not absolutely conclusive, he

believed could be taken as satisfactory by all reasonable men. Locke's first argument is an appeal to general theological considerations about man's nature and his relationship to God and the world. The second is an appeal to experience as a justification for the belief in the existence of material objects. This second argument comes interestingly close to being an appeal to a paradigm case.

Locke's theological justification was nowhere spelt out by him very clearly, and he did not rely on it to establish his point. But it is an argument to be noted because it brings out certain of Locke's preconceptions. For the argument to be appreciated we must remember that Locke, like many of his contemporaries including his critic Berkeley,³⁸ believed in the Great Chain of Being. Locke expressed this commitment in the Essay thus:-

"When we consider the infinite power and wisdom of the Maker, we have reason to think that it is suitable to the magnificent harmony of the universe, and the great design and infinite goodness of the Architect, that the species of creatures should also, by gentle degrees, ascend upwards from us towards his infinite perfection, as we see they gradually descend from us downwards: which if it be probable, we have reason then to be persuaded that there are far more species of creatures above us than there are beneath; we being, in degrees of perfection, much more remote from the infinite being of God than we are from the lowest state of being, and that which approaches nearest to nothing." 39

Unlike Leibniz, and other, earlier, supporters of the theory, and in entire consistence with the approach to philosophy that I have been arguing that Locke held, he did not accept the Great Chain of Being as a necessary fact about God's creation, but merely as a probability based on empirical evidence: "That there should be more

species of intelligent creatures above us, than there are of sensible and material below us, is probable to me from hence: that in all the visible and corporeal world, we see no chasms or gaps."⁴⁰

Given our comparatively lowly position on the scale, it is not surprising that there are many things which we do not understand and cannot justify in any absolute sense. One such area where no absolute justification is possible is in our belief in the existence of objects external to us. No absolute justification is possible, but we have all the proof we require for our particular station in the whole of creation. The position is clearly expressed by Locke in Book Four of the Essay where Locke considered our knowledge of external objects. If, he said:-

"any one will be so sceptical as to distrust his senses, and to affirm that all we see and hear, feel and taste, think and do, during our whole being, is but the series and deluding appearances of a long dream, whereof there is no reality; and therefore will question the existence of all things, or our knowledge of anything: I must desire him to consider, that, if all be a dream, then he doth dream that he makes the question, and so it is not much matter that a waking man should answer him. But yet, if he pleases, he may dream that I make him this answer, That the certainty of things existing in rerum natura when we have the testimony of our senses for it is not only as great as our frame can attain to, but as our condition needs. For our faculties being suited not to the full extent of being, nor to a perfect, clear, comprehensive knowledge of things free from all doubt and scruple; but to the preservation of us, in whom they are; and accommodated to the use of life: they serve to our purpose well enough, if they will but give us certain notice of those things, which are convenient or inconvenient to us." 41

Granted that we have no absolute certainty, Locke argued, we also need no absolute certainty. The faculty of perception which various animals have, Locke elsewhere said, reflects the way in which God has created each animal with the sort of faculties most appropriate to its kind "so that the wisdom and goodness of the Maker plainly appear in all the parts of this stupendous fabric, and all the several degrees and ranks of creature in it."⁴²

Locke's theology, then, supplies him with an explanation why we can offer no absolute proof, through our senses, for the world around us. But the theology is itself only bare probability, and it cannot, for that reason, be taken as supplying any sort of justification either for the existence of the external world, or for the theological propositions themselves.

Locke's argument on the basis of experience for the existence of an external world is more interesting and more substantial than the one we have just considered. The two arguments were often run together by Locke, (especially in Book Four, Chapter XI,) but I shall now consider the experiential argument separately. Once again Locke does not take the argument to be absolutely conclusive, but a modification of it, not entirely lacking in textual support, is.

Locke argued, in effect, that there are situations in which it would be absurd to doubt that there are objects external to us. Locke did not construe this absurdity as logical absurdity, which he would have to do if on his terms, he was to establish conclusively

the existence of an external world. But he did come very close indeed to construing the absurdity as a logical one. In his reply to the sceptic who holds that knowledge of the existence of the external world is not possible Locke put forward the following counter-example:-

"he that sees a candle burning, and hath experimented the force of its flame by putting his finger in it, will little doubt that this is something existing without him, which does him harm, and puts him to great pain: which is assurance enough, when no man requires greater certainty to govern his actions by than what is as certain as his actions themselves." 43

Locke's argument here amounts to this: if it is held that in the situation described we cannot talk of the existence of a flame, then we ought to note that neither can we talk of human actions as events in the world, for our claim to knowledge of the existence of human actions has much the same presuppositions as our claim to knowledge of the existence of the flame.

Locke went on to say something considerably stronger, and, indeed, different from this, for he said that the existence of the flame was as certain as "our pleasure or pain". Undoubtedly he was wrong about this. We can doubt that a flame has an existence independent of our experience in a way in which we cannot doubt that what we are now experiencing is painful. But what I take Locke to be getting at is that there is something patently absurd in denying that a flame exists in such a situation. And the reason why it is absurd is because what we mean to imply when we say that a flame exists is that, among other things, fingers get burnt when

placed where the flame is taken to be. One function of existential claims about physical objects is to indicate certain actual and possible experiences. Furthermore, we understand the word 'flame' because, in such situations as Locke describes, we use the word 'flame' to identify the object before us. If such situations are not cases of flames existing, we are tempted to ask, what would be? The sceptic would indeed be pushed to specify what it was, over and above what is already given, that he would wish for before he would be sure that there was a flame, as opposed to some illusory flame, before him.

Locke's argument has at least some elements in common with what has become known as the Argument from the Paradigm Case.⁴⁴ Locke's argument is not in fact such an argument, because he did not claim formally to prove the existence of the candle by means of it. But there are other passages in the Essay which come even closer to such an argument; thus, we find Locke accounting for how it is that we come by our idea of solidity in the following way:-

"If any one asks me, What this solidity is, I send him to his senses to inform him. Let him put a flint or a football between his hands, and then endeavour to join them, and he will know." 45

What we mean by solidity, Locke was saying, is just that sort of thing which we come to know in such situations.

Although Locke did deploy such arguments, he recognised that they did not substantiate the existence of an external world against the absolute sceptic, because the arguments do not prove such an existence, rather they presuppose it. One cannot

substantiate the existence of a candle, simply by relating the experiences which one can have if one places one's fingers in a flame (of a candle) because what is at issue is whether these experiences do substantiate the claim or not. To this radical form of scepticism Locke provided no answer, any more than Hume was to do when he wrote with regard to the senses:

"tis in vain to ask, Whether there be body or not? That is a point which we must take for granted in all our reasonings."⁴⁶

The existence of bodies Locke did indeed take for granted in all his reasonings.

Locke's position, therefore, about the connection between experience and the existence of an external world is ultimately unsatisfactory. It might be expressed in this way: Locke accepted that any finite set of flame-like experiences did not prove that the flame existed independently of the experiences; or, formally, that any finite set of experiential statements does not entail a material-object statement. Nevertheless, Locke held that we are entitled, for the reasons which we have considered, to believe that there are independent objects on the basis of experience.

Locke's fundamental troubles here are undoubtedly a product of his terminology, itself at least partially a product of the preconceptions which he brings to his philosophy from contemporary physics. Having asserted that all that we are ever aware of are ideas, Locke believed that he had thus identified a sort of entity, a parapsychical thing, which is the mental counterpart of the physical world. Instead of describing perception as "having ideas"

Locke should have said that although it is necessarily true that whenever we are aware then we are having some experience, it does not follow from this that what it is that we are aware of is the idea or experience. What we see, hear, feel, taste, etc., on any particular occasion is not normally a mental entity, and is certainly not necessarily some mental entity. Normally we see, hear, feel, etc., such things as tables, pins, and people, although, undoubtedly, when we see such objects, it is also true that we do have experiences.

Interestingly, Locke on occasions seemed to be very close to being aware of these sorts of consideration. As A. D. Woozley has pointed out, in a very interesting introduction to the Essay⁴⁷, the evidence which is normally taken to establish that Locke was a holder of a representationalist theory of perception is not nearly so overwhelming as most commentators on Locke have taken it to be. The question, Woozley asks, is whether there is absolutely conclusive evidence that Locke did believe that ideas were things, were indeed the objects of perception.⁴⁸ As he points out, although Locke held some kind of representationalist theory, it is not clear what kind of representationalist Locke was in his theory of perception. Locke "talked of seeing tables, and of having ideas of tables, but never of seeing ideas of tables."⁴⁹ And it would appear from this that, sometimes at least, Locke held that the correct objects of normal perceptual verbs are words which refer not to mental entities but to objects in physical space, independent of us.

But, for all its ingenuity, Woozley's account of Locke's theory of perception cannot be accepted. For, although it is true that Locke often does refer to objects of the physical world as if we can and do perceive them directly, ultimately his whole account does presuppose that what we are directly aware of are mental entities, ideas which are modes of the mental substance, mind, and not modes of the physical substance, matter. Locke's ultimate commitment to this position emerges very clearly from his account of language. For Locke, words are the signs of ideas, not of things external to their user. Words, for Locke, are "signs of internal conceptions"⁵⁰ they are "marks for the ideas within his own mind".⁵¹ Locke held, in an uncompromising way, to what, under the influence of Wittgenstein's Philosophical Investigations, has come to be called a private language theory of meaning, in which the objects of reference for all of our words are not public, but logically private, entities. Indeed, Locke would have been inconsistent to have advocated any other account, given his view that the objects of our awareness are always mental entities, ideas or experiences which are things in our minds and never things in the world. If Locke, therefore, had held a direct realist's account of perception it would have been radically inconsistent with his theory of meaning, but no such inconsistency arises if we accept, as I have argued we would be right to accept, that he in fact held that what we are aware of in perception is some kind of mental entity, itself often produced by a causal

chain stemming from some external object.

For all the difficulties in Locke's account of perception, and there are many, we must not lose sight of its positive merits. Locke's account presents the problem of perception in terms of the science of his day and he fairly clearly sets out the main facts of the situation. He left the issues in a form which made them amenable to further scientific investigation by both physiologists and psychologists and the problems that he failed to solve have remained central issues to the present day.⁵²

Conclusions.

I have not attempted to look in detail at all the problems raised by Locke's positive account of how it is that we come by the ideas that we do have; what I have attempted to do is to select several key aspects of Locke's position and examine these. We have seen that Locke often failed to establish that central concepts such as 'causation', and 'physical object' can be accounted for satisfactorily by his particular brand of empiricism. But, despite these important limitations, he did follow through his programme in a way which threw important light on the issues involved. Constantly, also, we find in Locke a conscious rejection of the a priori road to knowledge, an insistence, almost a dogmatic insistence, that the "furniture of the mind" is contingent and empirically founded. Further, we have found that his thinking is permeated with preconceptions taken from the method of science which we have earlier found in the works of the contemporary English scientists: it is the rejection of hypotheses, in favour of the

historical plain method that he constantly pursued.

Chapter XI

Locke on Material Objects

Introduction.

In the last chapter we examined Locke's account of the origin of some of our ideas, and we looked at some aspects of his theory of how it is that we come to have knowledge of an external world. But there still remain many other points of interest and importance in Locke's discussions about our knowledge of material objects, and the genesis of that knowledge, to be considered. It is with these other topics that this chapter is concerned. Once again we shall find that Locke's thinking is permeated with ideas taken from contemporary science; once more, too, we shall find that Locke's position is much easier to refute on grounds of inconsistency than it is to reject on grounds of implausibility. We shall also discover that Locke's thinking in this area illustrates various important strands of his total outlook on science and knowledge.

Material Substance.

Locke was aware that we had an idea of matter. To be consistent with his empiricist programme he had to show that we come by this idea as a result of experience. We have already seen how Locke attempted to justify a belief in the external world by his theory of perception, but there were other important aspects to Locke's account besides the ones that we have considered, and this is especially true of his detailed discussion of material substance.

One distinction which Locke made in his discussions of material things is a useful and important one for his arguments and should be

noted at the outset. It is the distinction between the "obscure and relative idea of substance in general" and "the idea of particular sorts of substances"¹ such as "man, horse, sun, water, iron."² The distinction is important on several grounds, not least because unless it is clearly recognised, there is a danger of construing objections to Locke's account of "substance in general" as also counting against his remarks about particular substances or particular things.

We shall turn first to Locke's account of substance in general. It is possible to find in Locke's writings two different justifications for a commitment to material substance, one empirical and the other deductive. The empirical argument is inductive and entirely consistent with Locke's empiricist programme. It runs as follows: Locke said that when we are willing to say that a property is instantiated then generally we can find something to which we are willing to say the property belongs. But, when we press this, in the last analysis, we find that we are left only with properties and nothing which independently is identifiable as the thing which has the properties. At this juncture we invent a thing, substance, which is taken as that which has the property even though we cannot separately identify substance as such.

Thus Locke wrote:-

"If any one should be asked, what is the subject wherein colour or weight inheres, he would have nothing to say, but the solid extended parts; and if he were demanded, what is it that solidity and extension adhere in, he would not be in a much better case than the Indian who, saying that the world was supported by a great elephant, was asked what the elephant rested on, to which his answer was, a great tortoise; but being again pressed to know what gave support to the broad-backed tortoise, replied, something, he knew not what." 3

On this argument the existence of substance is only a supposition based on inductive evidence. The evidence is that when we normally attribute properties there is something which we can identify independently as the thing which has the properties, so that when we have properties but no independently identifiable object to which the properties can be attributed then it is reasonable to suppose that there is such an object even though we cannot see, or in any other way, identify it. Such an argument, however, would not reach "beyond bare probability".

But Locke also held that there were other, firmer, reasons for believing there to be material substance. He offered these most clearly in his Third Reply to Stillingfleet. There Locke argued that there were logical as well as empirical reasons for believing in substance. "It is", he said, "a repugnancy to our conception of things that modes should subsist by themselves because we cannot conceive how qualities should subsist by themselves."⁴ Locke held, then, that there was a necessary connection between a quality existing and there being something which has that quality. The logical point which Locke has made might be paraphrased in this way: it is always nonsense to use a predicate term without predicating it of something. Whenever, to use the symbols of the predicate calculus, one asserts 'f' then there must be some 'x' of which the 'f' is asserted. This is necessarily true because we always take the 'f' to be the predicate term of some subject-predicate proposition, by definition.

Given this, it follows that whenever one asks the question 'What is some particular property the property of?' there must be some

answer. But, given that we can only find out the answer by observation, for Locke offered no other way to establish the existence of material objects, it follows, Locke held, that eventually we will arrive at a position where we have predicate terms, but no observably identifiable subject. All that we shall be able to say is that there is something "we know not what" which has the properties. As Locke remarked:-

"He that is satisfied that Pendennis-castle, if it were not supported, would fall into the sea, must think of a support that sustains it: but whether the thing that it rests on be timber, or brick or stone, he has by his base idea of the necessity of some support that props it up, no clear and distinct idea at all" 5

Locke is undoubtedly right to maintain that there must be something which has the property simply because it is a flat tautology to say that properties or qualities are always properties, or qualities of something. But to allow this does not require him to invent something called material substance which is the thing which has the properties. We can see this if we consider an example: A stone has the property of weight, we can ask of the stone what is it which has this property. One answer is that it is the solid extended parts of the stone which have this property. And what are solidity and extension the properties of? Locke's answer is that they are properties of something called the substance of the stone. But Locke was not forced to give that answer, in fact he was mistaken to do so. What he could have said instead is that solidity and extension are not properties of anything other than the stone itself, we have no need to invoke anything else in order to find the appropriate, and correct, subject which has the

properties.

Why, then, did Locke insist that there must be some other thing, substance, which is the ultimate property-bearer? A further part of the answer is that Locke was pushed towards the introduction of substance by the scientific preconceptions which he brought from contemporary physics to his philosophy. Locke believed that material objects have a structure which contemporary science had not yet fully comprehended but which was probably best understood in terms of an atomic theory of matter. Locke, that is, was strongly inclined to believe that matter was atomic in structure. His inclination is to be found in many passages in the Essay of which the following is but one, though typical, example:-

".... gold or saffron has a power to produce in us the idea of yellow, and snow or milk, the idea of white, which we can only have by our sight, without examining the texture of the parts of those bodies, or the particular figures or motion of the particles which rebound from them, to cause in us that particular sensation: though, when we go beyond the bare ideas in our minds, and would inquire into their causes, we cannot conceive anything else to be in any sensible object whereby it produces different ideas in us but the different bulk, figure, number, texture, and motion of its insensible parts." 6

No doubt one reason why Locke was strongly inclined to such an atomic interpretation of matter was his general acceptance of Boyle's corpuscular philosophy. But Locke also realized that the evidence was not conclusive. He expressed his attitude clearly when he wrote Some Thoughts Concerning Education:-

"Though the world be full of systems of it, yet I cannot say, I know one which can be taught a young man as a science, wherein he may be sure to find truth and certainty, which is what all sciences give an expectation of I think the systems of natural philosophy, that have obtained in this part of the world, are to be read more to know the hypotheses, and to understand

the terms and ways of talking of the several sects, than with hopes thereby to gain a comprehensive scientific and satisfactory knowledge of the works of nature: only this may be said, that the modern Corpuscularians talk in most things, more intelligibly than the Peripateticks." 7

The corpuscular theory of matter was for Locke an hypothesis, and the one "which is thought to go furthest in an intelligible explication of those qualities of bodies".⁸ But it was only an hypothesis, not a proven fact. Despite this, Locke, on occasion, showed more than a tentative commitment to it, thus he could write:-

"If a great, nay, far the greatest part of the several ranks of bodies in the universe escape our notice by their remoteness, there are others that are no less concealed from us by their minuteness. These insensible corpuscles being the active parts of matter and the great instruments of nature on which depend not only all their secondary qualities but also most of their natural operations, our want of precise distinct ideas of their primary qualities keeps us in an incurable ignorance of what we desire to know about them." 9

And, as W. D. Joske has pointed out, some of Locke's remarks commit him a priori to a Lucretian atomism.¹⁰

Locke, then, was ambivalent in his attitude towards the corpuscular philosophy. He believed that there was a great deal of evidence for it, but he recognised that he had no conclusive reason for accepting it as being true for it was only an hypothesis, a supposition to account for the observed phenomena, and not an independently established empirical fact about matter. Nevertheless, the arguments of Boyle and the other supporters of atomism undoubtedly encouraged Locke in his commitment to material substance as the stuff out of which the atoms of matter (if there were such) were composed. And often in his work the hypothesis of atoms can be seen to over-ride Locke's philosophical caution, illustrating an important way in which

contemporary science helped to determine Locke's philosophy.

So far we have identified three separate reasons why Locke wished to introduce the concept of material substance. The first was empirically based, viz., in our experience there is always something which has the properties which we attribute to things, therefore, when we still have properties, but no independently identifiable subject for those properties, it is inductively justifiable to assume that in these cases also there is a subject. The second argument turns on the fact that it is logically necessary that qualities belong to some object, because predicate terms are necessarily always predicated of some subject term. Third, undoubtedly, if we consider that material objects have a structure, then we are committed to the existence of stuff out of which the structure is made; and given that we can have no direct experience of the parts, either because they are internal to the object or too small to see or in any way directly experience, we cannot have a "clear idea" of them, but only a "confused idea" of something "we know not what" to which the simple ideas of experience are causally related, just as, today, understanding of physical objects is related by the average layman to a "confused idea" of sub-atomic particles.

After his exploration of how it is that we come by our idea of substance in general, Locke turned to examine how it was that we come by "the ideas of particular sorts of substances".¹¹ Locke says that our idea of substance in general must precede our ideas of particular substances, a view which, on two counts, is inconsistent with his own stated position. Locke held that ideas are always first of the

particular and only later, after a process of abstraction, made general;¹² further, a substantial part of his explication of substance in general presupposes the existence of particular substances. But Locke is forced to conclude that the idea of substance in general is prior to that of substance in particular because the analysis of any particular substance must, of necessity, make reference to the notion of substance in general. Thus Locke held that our ideas of particular sorts of substances are a combination "of simple ideas, as are, by experience and observation of men's senses, taken notice to exist together; and are therefore supposed to flow from the particular internal constitution or unknown essence of that substance."¹³ Once again we are faced with one of Locke's fundamental mistakes. He believed that because it is logically true that attributes must always be attributes of something, the attributes must be attributes of something which is unknown. But the attributes of something are just attributes of that thing which is identified as the subject of the proposition which attributes the property. "The desk is six feet long" is a proposition which attributes a property of length, not to some unknown entity, but to the object on which I am now resting. But it is true that the desk is composed of many parts, and, as Locke maintained, is probably made of particles with which I am not directly acquainted individually in experience for they are each too small for me either to see or otherwise identify.

Notwithstanding Locke's error about the nature of subject terms, what he had to say about the structure of objects is largely both

true and in the context of seventeenth century science, important. We shall return to this point later, but for the moment we shall follow through Locke's account of particular substances.

Our conception of a corporeal substance or a material object, Locke said, is a composite one having three elements. First there are what Locke called the primary qualities of bodies. These qualities, Locke maintained, are in the object whether we perceive them or not; they are "bulk, figure, number, situation, and motion of the parts of bodies".¹⁴ Second are the "sensible secondary qualities, which, depending on these, [i.e. the primary qualities] are nothing but the powers those substances have to produce several ideas in us by our senses; which ideas are not in the things themselves, otherwise than as anything is in its cause".¹⁵ Third, are the "active and passive powers" of a substance, such as the power that a loadstone has to alter "the minute particles of iron", or the power of gold to be melted, but not consumed, by fire.

Our conception of any particular object, Locke argued, is limited by our experience of that object, and what we take an object's properties or powers to be will, no doubt, be affected by our experiences. "I doubt not" Locke wrote "but that there are a thousand changes, that bodies that we daily handle have a power to cause in one another, which we never suspect, because they never appear in sensible effects."¹⁶ And if our senses were different, then the same objects as we now perceive would no doubt cause us to have very different ideas from the ones which we normally have just as "sand or pounded glass, which

is opaque, and white to the naked eye, is pellucid in a microscope".¹⁷

Our particular conception, then, of any particular material object, Locke held, is determined by our experiences of that object, and undoubtedly our experiences are only a small selection from the full range of logically possible experiences that beings (of any sort) might have of that object. Once again Locke points up our limitations; but these limitations are neither inconsistent with our station nor inadequate to the tasks that man has been created to fulfil : in the words of Pope:

"Why has not man a microscopic eye?

For this plain reason - man is not a fly".¹⁸

Real and Nominal Essences.

Locke's account of our knowledge of the essences of material objects is a product of his theory of language and his empiricist position. As we have seen Locke was committed by his account of particular substances to the position that we can have no knowledge of those substances other than that supplied by experience. His theory of language was also substantially grounded in his empirical outlook.

Locke's account of language and meaning is in essence very simple. He maintained that words in general stand for ideas. As he expressed it: man uses sounds "as signs of internal conceptions; and to make them stand as marks for the ideas within his own mind, whereby they might be made known to others, and the thoughts of men's minds be conveyed from one to another."¹⁹

Since words are signs, if they are to have any significance they must be signs of ideas which the person using them has actually

experienced; as Locke put it: "Words being voluntary signs, they cannot be voluntary signs imposed by him on things he knows not. That would be to make them signs of nothing, sounds without signification".²⁰ Language, then, according to Locke, could not transcend experience, any more than knowledge could transcend experience.

We shall not consider Locke's account of language in any detail, nor press the difficulties which it raises. But the two central features which we have just noted, that words are signs of ideas, and that a word cannot be significant without a corresponding experience, are fundamental to Locke's account of our knowledge - and more especially the limitations of that knowledge - of particular substances.

Some words in our language, Locke pointed out, are the names of particular things. But most of our words (and here Locke was thinking particularly of nouns) are general terms.²¹ But how do we come by general words? To this Locke gave a celebrated answer. "Words", he said, "become general by being made the signs of general ideas: and ideas become general, by separating from them the circumstances of time and place, and any other ideas that may determine them to this or that particular existence."²²

Much philosophical energy has been spent attacking Locke's conception of general ideas. Under the influence of Berkeley's account of Locke's theory,²³ philosophers have generally accepted that Locke meant by an "abstract idea" a kind of mental image, despite the fact that nowhere in Locke's writings is there any evidence to

suggest that Locke understood the term in that way. But this aside, the effect of Berkeley's attack has been to mislead philosophers about the intentions lying behind Locke's account. One of the targets which Locke had in his sights when he gave his account of general ideas was the doctrine of substantial forms. His thesis was that a general word does not stand for some abstract Platonic entity, but for something which is in some sense present in the mind of the person who uses the word. As he put it: "this whole mystery of genera and species, which makes such a noise in the schools, and are with justice so little regarded out of them, is nothing else but abstract ideas, more or less comprehensive, with names annexed to them".²⁴ The point emerges again when Locke considers the nature of essences. He said that we identify essences by reference to our abstract ideas which themselves are founded upon experience, but which are also dependent upon the intellect which has identified certain properties as being necessary for a thing to be of a certain kind: "From whence it is easy to observe, that the essences of the sorts of things, and , consequently, the sorting of things, is the workmanship of the understanding that abstracts and makes those general ideas."²⁵

Locke went on to argue that he was not denying the obvious fact that "Nature, in the production of things, makes several of them alike". But he was asserting that "the sorting of them under names is the workmanship of the understanding, taking occasion, from the similitude it observes amongst them, to make abstract general ideas".²⁶ And Locke adduced in support of his account cases where

there is disagreement amongst men in classification.²⁷

The issue turns on the distinction between real and nominal essences. The real essence, Locke said, is "the real internal, but generally (in substances) unknown constitution of things, whereon their discoverable qualities depend".²⁸ The nominal essence, on the other hand, is the essence we attribute, in terms of the properties we regard as essential, on the basis of our (limited) experience, and which are labelled by a word which stands for this abstract idea.

Of the real essences of corporeal substances there are two views, Locke said. One of them assumes that there are fixed and immutable essences "according to which all natural things are made, and wherein they do exactly and every one of them partake".²⁹ This view, Locke held, makes it impossible to know real essences, and is practically useless. "The other and more rational opinion", Locke said, "is of those who look on all natural things to have a real, but unknown constitution of their insensible qualities which serve us to distinguish them one from another, according as we have occasion to rank them into sorts, under common denominations."³⁰

The first view is a quasi-Platonic conception of essences. Essences, on this view, are a sort of mould, and natural objects are copies from this mould. It is not surprising that Locke, with his aversion to a priori theorising, rejected such a theory. But the rejection was more than this, for it was also an implicit abandonment of one of the Baconian ideals, an ideal which was born of Bacon's optimism rather than his empiricism. It will be remembered that Bacon had recommended that science should pursue, and would

eventually obtain, knowledge of the form of nature. It was the goal of Baconian method to achieve certain knowledge by coming to understand these forms, albeit by an empirical method. Locke was opposed to the Baconian ideal in this respect, (though clearly much else of Bacon's programme he accepted) for he held that no observations can lead us to a knowledge of such essences or forms. To have such knowledge would require men to have knowledge of substances, and this, Locke held, man was not capable of achieving for we can never be in the position of knowing that the nominal essence which we postulate in fact is matched in reality to the object. Whether or not we agree with Locke on that point, he was undoubtedly right to draw attention to the fact that there can be no logical guarantees about any identification of essence such that we can, given some set of properties f, g, h , deduce that they must be accompanied by some further property j , in any particular case; the only way to discover whether or not an object has a certain property is by observation, and observation contains no guarantee for future instances of that property being again present when other apparently similar objects appear.

Locke's views about essences did not commit him to denying that we can improve our knowledge of substances. To improve our knowledge is to make empirical discoveries about them, to discover, for example, that gold is dissolved in aqua regia. But to make such a discovery is not to discover some eternal necessary property intrinsically linked to the form of gold, or if it is, we could never

know it to be so. What we take the essence of any substance to be, Locke maintained, is our conception of it, and there can be no guarantee that our conception is right. The general words which we use to identify substances simply stand for those ideas which are "that abstract idea whereof that name is the sign". Nominal essences, therefore, are products of our minds, the consequences of our experiences of particular instances of any particular substance.

The consequence of this analysis of the connection between abstract ideas and nominal essences, and the rejection of the possibility of our having knowledge of real essences is of considerable importance in Locke's philosophy of science, and indeed for seventeenth century conceptions of the possibilities of science generally, for Locke's argument shows that our knowledge of material objects can never be other than of contingent facts about the world. Locke brought out the point very clearly, using gold as his example:-

"Supposing the nominal essence of gold to be a body of such a peculiar colour and weight, with malleability and fusibility, the real essence is that constitution of the parts of matter on which these qualities and their union depend; and is also the foundation of its solubility in agua regia and other properties accompanying that complex idea. Here are essences and properties, but all upon supposition of a sort or general abstract idea which is considered as immutable; but there is no individual parcel of matter to which any of these qualities are so annexed as to be essential to it or inseparable from it. That which is essential belongs to it as a condition whereby it is of this or that sort; but take away the consideration of its being ranked under the name of some abstract idea, and then there is nothing necessary to it, nothing inseparable from it. Indeed, as to the real essences of substances, we only suppose their being, without precisely knowing what they are; but that which annexes them still to the species is the nominal essence, of which they are the supposed foundation and cause." 31

In other words, the so-called necessary properties of any particular substance are only necessary because we construe it to be a substance of a particular sort of essence. And this is something which we do. It is not in any way inherent in the substance. Thus Locke wrote: "particular beings, considered barely in themselves, will be found to have all their qualities equally essential; and everything in each individual will be essential to it; or which is more, nothing at all".³² The distinction between necessary and contingent properties of a substance, therefore, is not a dispute about the substance, but about how we are going to classify the substance, or even if we are going to classify it as a substance at all.

On this important point Locke was undoubtedly correct. He recognised as possibly no other philosopher had done before him that it is our conception of the world which determines what properties are necessary, what contingent, rather than the world itself.³³ But this is not to assert, as it might be taken to assert, that we can arbitrarily describe the world in any way we like, for experience shows us that it is profitable to have certain concepts, rather than alternative ones, and, given these concepts, they have the entailments and incompatibilities which define them wholly independent of all and any human choice. Locke summed up his position thus:-

Nature makes many particular things, which do agree one with another in many sensible qualities, and probably too in their internal frame and constitution: but it is not this real essence that distinguishes them into species; it is men who, taking occasion from the qualities they find united in them, and wherein they observe often several individuals to agree, range them into sorts, in order to their naming, for the convenience of comprehensive signs; under which individuals,

according to their conformity to this or that abstract idea, come to be ranked as under ensigns: so that this is of the blue, that the red regiment; this is a man, that a drill; and in this, I think, consists the whole business of genus and species." 34

Given this, largely correct, analysis of words for the sorts of material things, it is not surprising that Locke is sceptical of our ever reaching knowledge about material things. Given the analysis, Locke must reject both the Cartesian and Baconian programmes for science, for neither can lead to knowledge as their protagonists claimed. Neither will clear and distinct ideas in themselves supply us with knowledge of the external world, nor, given Locke's definition of knowledge, will bare observation in the Baconian fashion ever lead to knowledge of the forms of nature.

Primary and Secondary Qualities.

One of the most famous aspects of Locke's philosophy, and one of the most heavily disputed, is his account of the qualities of bodies, and, in particular, his distinction between what are generally called the primary and secondary qualities. We have already seen that some such distinction was a commonplace in contemporary science, especially in the work of Galileo, Descartes, Boyle, and Newton. Much of what Locke had to say on this topic must be seen against this background, for Locke himself, in discussing these matters, believed that what he was doing was physics and not epistemology. Thus he wrote at the end of the section in the Essay in which he discussed the distinction: "I have in what just goes before been engaged in physical enquires a little further than perhaps I intended I hope I shall be pardoned this little excursion into natural philosophy."35

That Locke construed his remarks about the qualities of bodies as a piece of natural philosophy is significant. It indicates that Locke believed that the distinction was one that turned on empirical considerations rather than on logical ones (though this is not to imply that he believed that logical considerations were always irrelevant) and that the truth of the distinction was grounded in nothing further than contingent facts about the world.

This aspect of Locke's thinking stands in marked and important contrast to those earlier thinkers in the scientific revolution who, for largely metaphysical reasons, had seen the qualities of bodies in terms of mathematics. There is nowhere in Locke any hint of that idea that the language of the universe is mathematics, that the forms of nature must subscribe to a mathematical model. There was nothing of the Neoplatonist in Locke's attitude towards the physical world. Nor would Locke have been at all willing to go along with the Cartesian preconception of physics expressed in the remark that "my physics is here jointed to pure mathematics which I particularly wish it to resemble".³⁶ As with Boyle, as with Newton, the distinction between primary and secondary qualities turned for Locke on what he took to be the empirical evidence.

This said, we are immediately presented with a difficulty. Locke used two quite different criteria for drawing a distinction between primary and secondary qualities, one of them empirical, but the other a logical one. Locke began his account of qualities in this way:-

"Qualities thus considered in bodies are:

First, such as are utterly inseparable from the body, in what state soever it be; such as in all the alterations and changes it suffers, all the force can be used upon it, it constantly keeps; and such as sense constantly finds in every particle of matter which has bulk enough to be perceived; and the mind finds inseparable from every particle of matter, though less than to make itself singly be perceived by our senses." 37

The difficulty in this passage is not generally noticed. It is that Locke uses two quite different criteria to identify primary qualities. First, there is a logical criterion: 'bodies, by definition, must have the following properties'. Second, there is an empirical criterion: 'all bodies that we have ever experienced have the following properties' Locke runs these two criteria together without appearing to notice how different they are. The list of properties which Locke so identifies are solidity, extension, figure, and mobility.

Locke was undoubtedly right to believe that there are some defining characteristics which anything must have in order to be a body, a material object. And no doubt he was right to single out solidity, extension, and figure, though it might be held, surely, that it was unnecessary to include all three, because it is necessarily true that if anything is solid, then it must be extended.³⁸ Mobility is a much less clear case of a necessary condition for something being a body. Certainly it seems odd to postulate an immovable body, for it always seems to make sense to say of any conceivable object that it could be at another location from where it is, but it is not at all obvious that, in order to understand what a body is I must also understand the notion

of mobility, whereas I could not possibly understand the notion of body without an understanding the other three primary qualities listed by Locke.

But even with solidity the issue was not as clear cut as all that, for had not Descartes defined 'body' simply as extension? And did not Locke admit that general ideas were the product of the intellect? So in what sense could anybody be wrong who denied that solidity was part of the essence of body? Locke in fact gave an answer to this challenge, and his reply is instructive. If the Cartesians "mean by body and extension the same that other people do, viz., by body something which is solid and extended, whose parts are inseparable and movable different ways; and by extension, only the space that lies between the extremities of those solid coherent parts, and which is possessed by them, - they confound very different ideas one with another."³⁹ For the idea of extension does not include solidity. Further, spaces cannot be separated, "neither really nor mentally", and therefore we cannot have a concept of motion from the idea of space, but we can from the idea of body.⁴⁰ Locke's reply, therefore, was to point out that if body is defined as Descartes would have it, then that conception is very, very, difference from our ordinary conception of body, and what we should in fact be concerned with is that ordinary conception.⁴¹

It appears, then, that Locke identified a list of qualities which any body must have if it is to match up to our ordinary conception of body: he has supplied a definition of body which is in keeping with the way in which we ordinarily use the term. But

it does not follow from this that the qualities identified by Locke are the only primary qualities, it does not follow, that is, that the list might not be added to, and, indeed, there is nothing in Locke which suggests that such additions might not take place, which is not surprising, for Locke undoubtedly believed that the defining characteristics were dependent on our experiences. And if our experiences were such that new properties of objects were discovered which were universally manifested by physical objects, there would be strong pressure on us to change our general idea of body, and thus our definition. Concurrently with the production of the Essay just such a discovery was being made by Isaac Newton: the phenomenon of universal gravitation.

At this point it will be worth reminding ourselves of Newton's conception of primary qualities. Newton, it will be remembered, had defined the primary qualities as those "which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments". These, he said, "are to be esteemed the universal qualities of all bodies whatsoever".⁴² With Newton, as with Locke, it was experience that indicated which the primary properties are. But Newton's list of such properties was in fact longer than Locke's. For Newton they were: extension, hardness, impenetrability, mobility, and inertia.⁴³ Inertia, then, was a new primary property of objects, discovered by Newton; but it is not a defining property of body, and certainly it was not a defining property in 1690.

Newton's discovery of inertia as a universal property of

matter, or the discovery of any other universal properties of matter, has implications for Locke's account of primary qualities, but not particularly damaging ones. The implications are that new discoveries may very well lead us to say that there are other primary qualities of bodies than those that are entailed by our present conception of a body. It follows from this that we cannot establish what the primary qualities of a body are simply by introspection of our idea of body. All that we can discover by such introspection is what we take the defining properties of body to be. We can only discover what are the fundamental properties of an object in the way that Newton did.

From all of this we can see, then, that in Locke's account of primary qualities, the conflation of two separate criteria is an important confusion. It shows that Locke did not satisfactorily distinguish between two questions:-

- 1). What is our nominal essence of a body?
- 2). What are the fundamental causal properties which bodies have, and upon which other properties depend?

The first question is one calling for a philosophical analysis of our concept of body, which is to say a linguistic analysis of how the word 'body' is used. The second question is an empirical question, requiring the attention of the scientist. It is worth adding, however, that the scientist will be able to attempt an answer only after some more or less correct answer has been found to the first question. Equally the discoveries of the scientist may well lead to a revision of our conception of body in general, or of any substance in particular.

Locke's failure to distinguish the two questions leads to further confusion. Because Locke identified certain properties as both defining properties of body and also as the primary properties in a physical sense, then it was natural to conclude that the so-called secondary qualities, qualities not required for a definition of matter, could be causally accounted for by the primary qualities already identified. In contrast, the impartial Baconian should surely identify only all those properties which are found to be in common amongst all physical bodies, and then, by a physical investigation, establish which amongst all properties are causally more fundamental than others. Although no doubt Locke would have acknowledge that this was the correct procedure (his concern for the importance of empirical evidence for the primary qualities is witness to this) he did not succeed in putting his method into practice.

In this connection it is worth considering Newton's discussion of what is basically the same question, the distinction between primary and secondary qualities, in the third Rule of Reasoning in the Principia. Like Locke, Newton accepted that universality was a necessary condition for a property being a candidate for primacy. But besides universality Newton offered another necessary condition for primary qualities. It was that "those qualities which admit neither intensification nor remission of degrees" should be so deemed. Newton explained this criterion by an example. The gravity of a body is not as essential property of bodies, Newton said, because

although it appears that gravitation of bodies towards one another is universal, the gravity of bodies "is diminished as they recede from the earth".⁴⁴ In other words, the weight of an object is not a primary quality because the weight of an object is a function not only of the body itself but of its environment: the weight of an object is subject to "intensification and remission of degrees". By contrast, its inertial mass is not. And thus an object's "vis insita" is "immutable" and therefore a primary quality.

Implicitly, then, Newton appears to limit primary qualities to those properties which an object has which are causally independent of any factors external to the body qua body. Secondary qualities, on this showing, would be either those properties which are not universally manifested, or those which are a result of a causal interaction between one body and another, as weight is.

Newton offered us a reason for accepting his first criterion of primary qualities. He said that qualities which "are not liable to diminution can never be quite taken away".⁴⁵ And the point here is that Newton's criterion emerges as a special case of his more general one of universality. Necessarily, Newton was saying, a property which cannot be taken away must always be a property of body, and must, therefore, be universally manifested. But this is not to imply that we can always establish a priori which properties cannot suffer diminution. It is only on the basis of experience that we come to know that inertia is not so subject and that weight is.

Locke never fastened on to this Newtonian criterion for primary qualities, which is hardly surprising as the third Rule of Reasoning appeared for the first time only in 1713, in the second edition of the Principia, nine years after Locke's death. Nor is it surprising that Locke never thought of it himself, because the criterion offered by Newton is itself a very subtle one which emerged from Newton's deep thought about the nature of matter in general, and gravity in particular. There is, indeed, some reason to think that the rule was actually designed to exclude gravity as a universal quality of bodies.⁴⁶

Newton's criteria illustrate that there were competing accounts and bases in the period we are considering for drawing the distinction between primary and secondary qualities. The competing accounts arose largely under the pressure of contemporary physical theories. If one rejects all theories about matter, whether, as with Boyle's, based upon substantial empirical evidence, or, as with Descartes', arrived at a priori, then there remains no basis for the distinction between primary and secondary qualities. It was exactly this point that Berkeley made in The Principles of Human Knowledge. If one examines the world without any preconceptions about its contents or structure, then any quality is on a par with any other. It is only if one (wrongly) asserts that the nominal essence of body is necessarily its real essence, as Locke, in his unguarded moments, was inclined to do, or if one believes (correctly)

that observation and experiment give one reason to believe that some properties are different from others in that some properties are causally dependent on others, that any distinction can be made.

The distinction between primary and secondary qualities is bound to be made once it is agreed that explanations in nature, as Boyle expressed it, rested on the two great principles of matter and motion. Matter was understood to have certain properties, those listed as the primary qualities, and all other properties could, in principle be accounted for causally by reference to these. Thus heat, according to Galileo, was not a primary property of any object but was rather "a multitude of minute particles having certain shapes and moving with certain velocities" which "meeting with our bodies their touch as felt by us as they pass through our substance is the sensation we call heat".⁴⁷ Or, as Locke expressed it, "For to speak truly, yellowness is not actually in gold, but is a power in gold to produce that idea in us by our eyes, when placed in a due light: and the heat which we cannot leave out of our idea of the sun, is no more really in the sun, than the white colour it introduces into wax. These are equally powers in the sun, operating, by the motion and figure of its sensible parts, so on a man, as to make him have the idea of heat; and so on wax, as to make it capable to produce in a man the idea of white."⁴⁸

But the fact that many properties of objects can be explained or accounted for in terms of other properties is not sufficient to

establish that a secondary property is not a real property of the object. That crystal glass is brittle is accounted for in terms of the arrangement of the molecules of such glass. But the brittleness is no less a property of the glass because it can be explained in such a way. It is indeed a dispositional property of the glass, or, as Locke preferred to call it, a power.

Locke recognised, perhaps more clearly than Galileo, and in a way very similar to Boyle, that a power is a real property of an object. When we have an idea, say of yellow, although there is no actual yellowness in the object, there is, nevertheless something about the object which is the direct cause of our having that particular idea. It is this power, itself no doubt inseparable from the arrangement of the parts of the object which was for Locke the secondary quality, and the cause of the corresponding idea of that quality. As Locke put it, the secondary qualities are "nothing but powers to produce various sensations in us, and depend on those primary qualities, viz. bulk, figure, texture, and motion of parts." 49

Locke, then, gave roughly a correct account of how physically and physiologically we come to know that certain objects are hot or yellow. And he correctly saw that this entailed a distinction between the causal account which we would give of the primary and secondary qualities of objects. But he did make an important error in his account. He assumed that the words in our language for secondary qualities, words for colours, sounds, and so on, name private objects, only open to our individual inspection. What Locke failed to notice was that it does not follow from the fact that our awareness is the product of a causal chain in normal perception, such as he described, that what we see, hear, feel, or touch is to be identified with the last stage of that causal chain. If Locke had been correct in his belief then Berkeley's

charge that as we are only aware of ideas, then "if there were external bodies, it is impossible we should ever come to know it; and if there were not, we might have the very same reasons to think there were that we had now"⁵⁰ would be true. But if it were the case that yellow was simply the name of my idea or sensation then there could never be any dispute about what the colour of gold, or anything else, is. But that I can misidentify the colour of an object shows that the correct identification depends upon factors other than my experience. For this reason we cannot identify colours solely with ideas in my mind.

What emerges from this is that Locke's account of primary and secondary qualities contains three main theses, two of which are true, whilst the third is false. The first true thesis is that the properties of bodies can be arranged in a causally hierarchical order. Some properties are a causal consequence of other properties. The strength of a bar of iron is dependent on the thickness of the bar, and the nature of the minute parts of the bar; the colour of gold is a function of the molecules of gold and their effect on white light which is reflected from them. The second true thesis is that both primary and secondary qualities are real properties of objects, but that the secondary qualities are to be identified with the actual arrangement or "texture" of the primary matter.

The false thesis is that the names we have for our ideas of secondary qualities are the names only of private sensations, not of public objects. For, as we have seen, if this were the case that the ideas of these qualities simply were sensations then there could never be any dispute about them in the way in which it is perfectly possible to dispute, be mistaken, and be correct, about the colour, taste, or smell of an object. The person having

the sensation would be the sole arbitor as to what sensation he was having at any time, and whilst this may be true about sensations it is not true about colours and other secondary qualities. Ripe strawberries are sweet, English pillar boxes are red, and people who judge otherwise are mistaken about a matter of fact, which fact is a fact about strawberries or pillarboxes and not about their private sensations.

Conclusions.

We have seen how Locke's thinking on both material substance as such, and on the distinction he drew between primary and secondary qualities, was deeply influenced by his scientific preconceptions. Indeed, I have suggested that it was Locke's commitments to the scientific discoveries of his day which led him to both some fundamental truths and also some errors. Both his acceptance of substance as such and his distinction between primary and secondary qualities were influenced by his understanding, and his misunderstanding, of the import of the scientific facts which he drew upon to support his position. But in such misunderstandings Locke was not alone, and more to the point, the accounts which he offered themselves clarified various aspects of the connection between science and philosophy which stood in great need of such clarification.

Chapter XII

Locke, Knowledge and Science

Introduction.

It is in Book Four of the Essay that Locke at last comes to a positive statement of his own views on the nature and extent of human knowledge. In this chapter we shall review Locke's answers to these questions not only to assess them in their own right, but also to see how it was that they fitted in to the developments in science and in philosophy which had occurred during the scientific revolution. It hardly needs to be said that we shall find that Locke's views are very much in keeping with the English scientists with whom he was closely connected, which emphasised the contingency, not the necessity, of the discoveries of the natural scientist, a point which raised difficulties for Locke who both wished to maintain that all knowledge was of necessary truths, but that also the scientists could and did discover truths (i.e. did come to know truths) about the world which were only contingent. Locke's solution to this dilemma was unsatisfactory, but it was very much a sign of the times that the dilemma itself emerged so clearly in his work.

Locke's Conception of Knowledge.

Given that the mind is only aware of ideas, it is evident, Locke said at the beginning of Book Four, "that our knowledge is only conversant about them."¹ From hence, Locke held, knowledge is "nothing but the perception of the connexion and agreement, or disagreement and repugnancy, of any of our ideas."²

Much has been said about Locke's definition and it is not my intention to subject it to detailed scrutiny. But there are several

aspects of it which cannot be allowed to pass unacknowledged. First, we must recognise the psychologism inherent in Locke's definition. It is the perception, by the mind, of a connexion between ideas which constitutes knowledge, and, ultimately, Locke held, this perception was an act of intuition. Significantly, Locke never challenged the validity of these perceptions. He never doubted that is, that such perceptions occur, and that such perceptions give knowledge: in short, he never subjected his examples of knowledge to that sceptical scrutiny to which Descartes had subjected his. Locke presented as paradigms of knowledge, without further ado, "white is not black" and "the three angles of a triangle are equal to two right ones,"³ which we see to be true because we perceive that the ideas respectively do not and do agree one with another. We perceive such agreement, he said, "by the natural power of perception and distinction".⁴

There is both merit and defect in Locke's account thus far. He was right to recognise that knowledge can and should be defined in the last analysis by appeals to paradigm cases: his definition of knowledge is derived from particular examples of knowledge which he rightly recognised are not open to serious dispute. But Locke's psychological criterion for identifying knowledge did not allow him to make the crucial distinction between contingent and necessary truths. The implication of Locke's position is that only necessary truths can and are known, for in all other cases there is no agreement or repugnancy of the ideas which themselves are necessary conditions for knowledge, but he did not appear either fully to see this implication, or to stick rigidly to his own definition.

The consequence of Locke's definition was to make the concept of knowledge excessively narrow, for obviously all statements which purported to convey knowledge about ideas which were not in the relevant respects either in agreement or repugnant could not be accepted as such, yet Locke himself, and often in the Essay itself, claimed to know things which did not match up to his own rigorous demands. The most obvious and blatant example of this is his firm commitment to the possibility of our knowing of the existence of objects in the world external to our minds. Almost equally important, and just as inconsistently, Locke held that Newton had given knowledge of the world in the discoveries conveyed in the Principia. "Mr. Newton, in his never enough to be admired book," Locke wrote, "has demonstrated several propositions, which are so many new truths before unknown to the world, and are further advances in mathematical knowledge"⁵ A similar view was expressed by him in Some Thoughts Concerning Education where he said:-

"Though the systems of physicks, that I have met with, afford little encouragement to look for certainty, or science, in any treatise, which shall pretend to give us a body of natural philosophy from the first principles of bodies in general; yet the incomparable Mr. Newton has shown, far far mathematicks, applied to some parts of nature, may, upon principles that matters of fact justify, carry us in the knowledge of some, as I may so call them, particular provinces of the incomprehensible universe. And if others could give us so good and clear an account of other parts of nature, as he has of this our planetary world, and the most considerable phenomena observable in it, in his admirable book 'Philosophae naturalis Principia mathematica' we might in time hope to be more furnished with some true and certain knowledge in several parts of this stupendous machine, than hitherto we could have expected. And though there are very few who have mathematics enough to understand his demonstrations; yet the most accurate mathematicians, who have examined them, allowing them to be such, his book will deserve to be read, and give no small light and pleasure to those, who, willing to understand the motions, properties, and operations of the great masses of matter in this our solar system, will but carefully mind his conclusions, which may be depended on as propositions well proved." 6

The inconsistency between Locke's definition of knowledge and such examples is of considerable importance because it shows that Locke was not only very tempted, but actually sometimes succumbed, to treating the discoveries of science, such as the general truths discovered by Newton, as knowledge, when, by his own definition, they had no such entitlement. Nor was it only the first two books of the Principia which Locke accepted gave knowledge, the books which were only to be understood as a branch of mathematics,⁷ it was also the physics of Book III, for it gave knowledge "of the great masses of matter in this our solar system". Further, and most important, Locke was quite right to see Newton's work as one which did reveal new discoveries, did indeed give knowledge of the world. It was Locke's definition of knowledge which was at fault, not his inclination.

We shall return to Locke's definition of knowledge and its implications for the possibility of scientific knowledge in a later section. For the present let us just note that Locke's definition raises grave difficulties for some positions which he obviously wished to accept, such as the truths of Newton's mechanics. We shall now consider the more fundamental question, more directly raised by Locke's account of knowledge, namely his claim that we could have knowledge of the "particular existence of finite beings without us".⁸

Locke said that there were three sorts or degrees of knowledge: intuitive, which he placed highest, and upon which all other forms of knowledge ultimately depended; demonstrative, "where the mind perceives the agreement or disagreement of any ideas, but not immediately,"⁹ and, third, the sensitive knowledge of the existence of particular external

objects. This last form of knowledge Locke conceded was not as certain as either of the other kinds, but "going beyond bare probability passes under the name of knowledge."¹⁰ The first two degrees of knowledge are not without their problems, but it is the third kind which has always presented the most difficulty to commentators. It is rightly pointed out that Locke held that " We can have knowledge no further than we have ideas."¹¹ Since external objects are ex hypothesi not ideas, it follows that we can have no knowledge of them, either that they exist, or what they are if they do exist.

Locke's position with regard to sensitive knowledge is one so obviously contradictory that it has often led to a dismissal of Locke's whole epistemology. Undoubtedly it is a very major fault in his whole scheme, but it hardly warrants such harsh judgement. From what we have seen of the background to Locke's Essay it is clear that the very last thing that Locke could possibly have denied was the existence of an independent physical world, for the underlabourer would fail to be an underlabourer if there were indeed no edifices under whose shadow he could work. Indeed it is fairly certain from the whole manner of Locke's approach that if his philosophy was indeed incompatible with the existence of such a material world, then, for him, it would have been so much the worse for his philosophy. Indeed, it was such an impossible notion for Locke that there might not be a material world that I believe he just could not have appreciated that his account of knowledge might in fact entail that we could not know whether or not there was.

Demonstrative Knowledge.

We have already had occasion to note that Locke believed that natural philosophy was far from being a science.¹² Locke understood by a science, a body of demonstrative knowledge. The paradigm of a science was mathematics, and for Locke, as for many of his contemporaries, mathematics was in fact the only science, or at least the only large body of demonstrative knowledge.

Why Locke thought that mathematical knowledge was possible is something of central importance for an understanding both of Locke's views on the conditions necessary for a knowledge of general, as opposed to particular, truths, and also to explain why Locke thought that knowledge of many general truths about the physical world was impossible for man.

Central to Locke's account of our knowledge of mathematical truths was his theory of nominal and real essence, which we have already had occasion to consider in Chapter Eleven. Locke argued, with regard to material objects, that all that we could hope to know of them was their nominal, not their real, essence, for we can never be sure that the properties which we identify as the essential properties of an object are in fact so essential. But it was not at all the same with mathematics, Locke held, for the real essences of the entities named in mathematical propositions are identical with their nominal essences. Mathematical propositions, then, which state relations between ideas, are not about ideas which are more or less imperfect copies of some real essence, they are that real essence. Mathematical ideas are

themselves abstractions, made by the mind, from particular experiences, and there is, therefore, no question of having to attempt to match the ideas which we have against some more perfect entity. Locke explained his account of mathematical knowledge thus:-

"Essence being thus distinguished into nominal and real, we may further observe, that, in the species of simple ideas and modes, they are always the same; but in substances always quite different. Thus a figure including a space between three lines is the real as well as the nominal essence of a triangle; it being not only the abstract idea to which the general name is annexed, but the very essentia or being of the thing itself, that foundation from which all its properties flow, and to which they are all inseparably annexed." 13

Locke did not consider the difficulties which can easily be raised against his account of mathematics. He seems to have been quite sure that his analysis was correct. But one implication of his position is obvious: Locke was as far away as possible from being a Platonist with regard to mathematics as he was with all other aspects of his epistemology, for a Platonist must and does hold that the entities which are identified in mathematical propositions are entities independent of the mind, just as much as Locke held that the entities named in propositions about substances refer to independent objects.

Granted that Locke held that only in those cases where the nominal and real essences of objects were known to coincide could we talk of knowledge of the essences, and therefore knowledge of general truths about objects, then it was certain to be the case that there were large areas of human enquiry where knowledge had not, nor was ever likely to be obtained. But Locke did not believe that there was no possibility of demonstrative knowledge outside of mathematics and ethics (which he also held was like mathematics in so far as nominal and real essences

coincided). Locke, in one chapter in the Essay appears to suggest that there is at least some possibility that demonstration might well be increased in the natural sciences. Locke wrote:-

"It has been generally taken for granted, that mathematics alone are capable of demonstrative certainty; but to have such an agreement or disagreement as may intuitively be perceived, being, as I imagine, not the privilege of the ideas of number, extension, and figure alone, it may possibly be the want of due method and application in us, and not of sufficient evidence in things that demonstration has been thought to have so little to do in other parts of knowledge, and scarce so much aimed at by any but mathematicians."¹⁴

It has been supposed, for example, by Fraser,¹⁵ that Locke had in mind here the possibility of a demonstrative system of ethics, but this is not so. Rather Locke was thinking of the possibility of demonstration in the natural sciences. (It is interesting to compare Locke's remarks here with Newton's first paper on light and colours which was presented to the Royal Society in 1671/2 and copies of which Locke owned.¹⁶)

What Locke said was this. Some properties, such as numbers and figures, are capable of being demonstrated, because "the modes of numbers have every the least difference very clear and perceivable", and, "the mind has found out ways to examine, and discover demonstratively, the just equality of two angles or extensions or figures".¹⁷ But there are many cases where demonstration is not possible: "in other simple ideas, whose modes and differences are made and counted by degrees, and not quantity, we have not so nice and accurate a distinction of their differences as to perceive, or to find ways to measure, their just equality, or the least differences".¹⁸ Locke's explanation for this impossibility was that we cannot know the causes of these simple ideas which are "particles of matter whereof each is too subtile to be

perceived, it is impossible for us to have any exact measures of the different degrees of these simple ideas".¹⁹ He went on to explain this further:-

"For, supposing the sensation or idea we name whiteness be produced in us by a certain number of globules, which having a verticity about their own centres, strike upon the retina of the eye, with a certain degree of rotation, as well as progressive swiftness; it will hence easily follow, that the more the superficial parts of any body are so ordered as to reflect the greater number of globules of light, and to give them the proper rotation, which is fit to produce this sensation of white in us, the more white will that body appear, that from an equal space sends to the retina the greater number of such corpuscles, with that peculiar sort of motion. I do not say that the nature of light consists in very small round globules; nor of whiteness in such a texture of parts as gives certain rotation to these globules when it reflects them: for I am not now treating physically of light or colours". 20

We cannot, then, Locke held, prove that two white objects are equally white unless we can find out whether or not the two whites are caused by the same number of particles hitting the retina, for in the ideas of white which we have there is no difference, even though there may in reality be a difference which we cannot detect. In such cases there is no possibility of demonstrating the equality or non equality of the two shades.

However, "where the difference is so great as to produce in the mind clearly distinct ideas, whose differences can be perfectly retained, there these ideas or colours, as we see in different kinds, as blue and red, are as capable of demonstration as ideas of number or extension."²¹ The implication here is that ideas of secondary qualities, which, being ideas in the mind, having no external existence, are capable of being fully perceived, and that as a result it is perfectly possible to deduce conclusions from our experiences of colours which are truths about our ideas of colours, and are not just "trifling propositions" about words.

But they are about our ideas, and not about anything in the external world. As Locke expressed it: "What I have here said of whiteness and colours I think holds true in all (ideas of) secondary qualities and their modes".²²

The extent of demonstrative knowledge, then, was for Locke extremely limited. It was indeed almost completely confined to the knowledge which we have of the relationship between our own ideas and where those ideas are not taken as copies of some further entity but are the entities themselves, or, as Locke expressed, where the nominal and real essence coincides. The implications of this for the possibility of knowledge in the natural sciences Locke well realized.

Our Knowledge of the World.

As we have already noted, notoriously Locke held that, although we are only aware of ideas, it is possible for us to know that objects exist independently and externally from us. He held that we can demonstrate the existence of God (though this was the only case of demonstrating the truth of an existential proposition which Locke allowed), and he held that it was possible to know by means of our senses that particular objects exist: "for I ask anyone whether he be not invincibly conscious to himself of a different perception, when he looks on the sun by day and thinks on it by night, when he actually tastes wormwood or smells a rose or only thinks on that savour or

odour?" There is, said Locke, "a very manifest difference between dreaming of being in the fire and being actually in it."²³ Such knowledge was clearly of the particular and not of the general. Our knowledge of the general, Locke held was almost non-existence, and this for two, rather different, reasons.

The first reason was based on the nature of empirical knowledge and was as important a statement as any in the scientific revolution about the nature and limits of scientific enquiry. The second arose out of Locke's general world view, or, as I earlier named it, cosmological considerations.

Locke argued that our knowledge of general propositions about the world was virtually non-existent because none of them stated connexions which we could see to be necessary, even though we accepted that they existed. Thus Locke held that our knowledge of the properties of objects is such that the properties are not known to be logically related "e.g. our idea of flame is a body hot, luminous, and moving upward; of gold, a body heavy to a certain degree, yellow, malleable, and fusible When we would know further concerning these, or any other sorts of substances, what do we inquire, but what other qualities or powers these substances have or have not?"²⁴

The only way in which we can discover these other properties is by empirical investigation because "the simple ideas whereof our complex ideas of substances are made up are, for the most part, such as carry with them, in their own nature, no visible necessary connexion or inconsistency with any other simple ideas, whose co-existence with them we would inform ourselves about".²⁵ And the reasons why

we do not discover any visible necessary connection between the properties of objects are (a) because we do not know the "primary qualities of their minute and sensible parts",²⁶ if we did, the implication is, we might be able to deduce or demonstrate the secondary qualities: and, (b) a "more incurable part of our ignorance", and that is that "there is no discoverable connexion between any secondary quality and those primary qualities which it depends on".²⁷ And thus:-

"Though we see the yellow colour, and, upon trial, find the weight, malleableness, fusibility, and fixedness that are united in a piece of gold; yet because no one of these ideas has any evidence dependence or necessary connexion with the other, we cannot certainly know that where any form of these are, the fifth will be there also, how highly probable soever it may be for this co-existence can be no further known than it is perceived, and it cannot be perceived but either in particular subjects, by the observation of our senses, or, in general, by the necessary connexion of the ideas themselves." 28

This, then, was Locke's answer to the problem of induction: we cannot have certainty of generalizations about the properties of physical objects. Nor can we have certainty about the powers of objects "to change the sensible qualities of other bodies", which is "a great part of our inquiries about them".²⁹ Which is to say that we cannot have general knowledge of cause and effects, because, of course, once again we can establish no necessary connection. Thus, although on several occasions particular experiments produce constant results "whether they will succeed again we cannot be certain. This hinders our certain knowledge of universal truths concerning natural bodies".³⁰ That is to say, knowledge of the future behaviour of objects is for

us impossible , "certainty and demonstration are things we must not, in these matters pretend to".³¹

This then was Locke's belief. Whatever it was that the natural philosopher discovered, his knowledge did not go beyond the particular, and although no doubt generalization is inevitable, few, if any generalizations could be accepted as giving knowledge. ".... as to a perfect science of natural bodies ... I conclude it as a lost labour to seek after it".³²

Although Locke held that as a matter of fact we cannot arrive at certain knowledge of natural objects in the way which we have considered he suggests or implies several times that this short-coming in our abilities is a contingent, not a necessary, fact about ourselves. And the contingency, he suggested, hinged upon our inability to obtain knowledge of the nature of the fundamental particles out of which the physical world was composed. If our faculties were better, Locke suggested, and we could see, or some other sensory way come to know the nature of the fundamental particles of matter, then we could come to know the powers of those particles and from such knowledge we could deduce the physical consequences of, say, chemical interaction. Unfortunately Locke never considered how it would be possible , even if we had such super senses, for us to know that we were right about the powers that we could identify in the particles of matter. And indeed, on his own principles, we find that we could not be so sure as Locke implies.

Locke, it was said earlier, had two, rather different, reasons for rejecting the possibility of our obtaining general knowledge of

natural events. The first reason we have just considered. The second arose out of his conception of the place of man within the total cosmological framework. It was, he maintained, our "mediocrity"³³ which was our hinderence. Our mediocrity was for Locke a necessary consequence of our particular place on the ladder of the Great Chain of Being³⁴. Our knowledge can never approach that of the angels, still less that of God. Indeed, Locke's belief that we cannot have knowledge of large areas of physical enquiry was intimately linked with his whole conception of man's ultimate purpose, about which Locke is very Aristotelian. Because our faculties cannot and are not designed to comprehend the real essence of bodies, he said, but we can know that there is a God and what our moral duties are, it follows that the real purpose of mankind in general is to discover what his duties are, rather than to reach knowledge of the physical world, which must be left to particular men who have the special abilities required.³⁵ An implication of Locke's position here is to suggest that there is almost something morally wrong in expecting to reach absolute certainty in the physical sciences, for to have such aspirations was tantamount to an identification of oneself with a being higher on the scale of creation.

There is also another implication of Locke's remarks which he was very quick to dispel, and that was the hint that Locke was opposed to science as an enterprize. In reply to this Locke gave as comprehensive a statement of his attitude as he ever did:-

"I would not be thought to disesteem or dissuade the study of nature. I readily agree the contemplation of his works gives us occasion to admire, revere, and glorify their Author and, if rightly directed, may be of greater benefit to mankind than the monuments of exemplary charity that have at so great charge been

raised by the founders of hospitals and alms-houses
All that I would say is that we should not be too forwardly
possessed with the opinion or expectation of knowledge where it
is not to be had, or by ways that will not attain it; that we
should not take doubtful systems for complete sciences, nor
unintelligible notions for scientific demonstrations. In
the knowledge of bodies, we must be content to glean what we
can from particular experiments, since we cannot from a discovery
of their real essences grasp at a time whole sheaves, and in
bundles comprehend the nature and properties of whole species
together. Where our inquiry is concerning co-existence or
repugnancy to co-exist, which by contemplation of our ideas we
cannot discover, there experience, observation, and natural
history must give us by our senses and by detail an insight into
corporeal substances. The knowledge of bodies we must get by
our senses warily employed in taking notice of their qualities
and operations on one another; and what we hope to know of
separate spirits in this world we must, I think, expect only
from revelations. He that shall consider how little general
maxims, precarious principles, and hypotheses laid down at
pleasure have promoted true knowledge or helped to satisfy
the inquiries of rational men after real improvements, how
little, I say, the setting out at that end has for many
ages together advanced men's progress towards the knowledge of
natural philosophy, will think we have reason to thank those
who in this latter age have taken another course and have trod
out to us, though not an easier way to learned ignorance,
yet a surer way to profitable knowledge." 36

Undoubtedly, Locke held, science was worth pursuing not only because
its revelations added to the glory of God, but also because the util-
itarian benefits which could and did flow from scientific discoveries
were themselves of great consequence. But the scientist should not
have exaggerated expectations for his findings.

Locke's views that it was a consequence of our mediocrity
that we did not have any demonstrative natural philosophy is not
easy to square with his views on the contingency of the laws of nature.
Locke held that the laws of nature are the product of "the arbitrary
will and good pleasure of the wise Architect"³⁷. God was free,

therefore, to modify the laws whenever he felt so inclined, for example, by performing miracles. And if this was so, then nature could not be understood as being necessary in any important sense. To this I believe that Locke would have replied that this is to misunderstand how it was that he construed the possibility of a demonstrative science of natural philosophy. He would, I think, have said something on the following lines: 'We are all agreed that some properties of objects are necessarily connected with other properties. We are all agreed, for example, that material objects must be extended. Granted that there are some necessary connections between some properties, are we in a position to decide definitively that there are no necessary connections between other properties of bodies simply because we do not comprehend them? No, we are not. There may therefore be other necessary connections obvious to angels and God of which we are not aware.'

Locke's analogies in connection with his discussions of the limits of our physical knowledge are very illuminating, thus he wrote:-

"Did we know the mechanical affections of the particles of rhubarb, hemlock, opium, and a man, as a watchmaker does those of a watch, whereby it performs its operations, and of a file, which by rubbing on them will alter the figure of any of the wheels, we should be able to tell beforehand that rhubarb will purge, hemlock kill, and opium make a man sleep: as well as a watchmaker can that a little piece of paper laid on the balance will keep the watch from going till it be removed; or that some small part of it being rubbed by a file, the machine would quite lose its motion, and the watch go no more. The dissolving of silver in aqua fortis and gold in aqua regia, and not vice versa, would be then perhaps no more difficult to know than it is to a smith to understand why the turning of one key will open a lock and not the turning of another." 38

Locke failed to distinguish the cases where the knowledge which the expert has is a consequence of constant conjunction, as it is in the

case of the fitting of some part of the mechanism of the watch, and the knowledge which is dependent on the agreement of ideas, as it is in the case of the key and the mechanism of the lock. In the latter case there is a clear sense in which the idea of the key matches or is in agreement with the idea of the lock, but there is no such agreement in the case of the watch being stopped, for it is only by experience that the watchmaker is able to say what amount of weight will have that effect. But even if it is plausible to argue that the example of the lock and key is a case of agreement between ideas, even here, in the last analysis, it is the lockmaker's past experience which has led him to a knowledge of what shapes will in fact open what locks, how near the matching shapes must be for them to work, and so forth. In other words the knowledge which indeed the locksmith and the watchmaker possess is only contingent knowledge, and if such knowledge were to be characterised as being the perception of the agreement between ideas, following Locke's definition, then the criterion of agreement would have become so subjective (any man would be able to claim knowledge about anything of which he felt sure) as to be useless. Although Locke's criterion for knowledge does on occasion seem to slide towards such an emptiness, (for example, compare Essay IV IV 18, where Locke's words actually commit him to such a position), he certainly believed that knowledge was dependent on something more than the psychological feelings of certainty of the claimant.

Indeed, it was for just this reason that Locke wrote Chapter IV of the fourth Book, entitled 'Of the Reality of Knowledge'. It followed

from Locke's definition of knowledge that if we perceive agreement between our ideas then we have knowledge. But he also maintained that our ideas must themselves agree with reality, as Locke expressed it: "Our knowledge, therefore, is real only so far as there is conformity between our ideas and the reality of things."³⁹ But, Locke never explained how it was that we could know that such conformity existed, because, as we have already considered in Chapter X when considering Locke's account of perception, as we are only aware of ideas there is no way in which we can compare our ideas with things directly to see if there is the required conformity, except in those cases where our ideas are the things, as Locke held they were in mathematics and ethics, but which he did not hold was the case in our conception of substances.

Locke did not conclude from our being aware only of ideas that we could not know if our ideas corresponded to anything at all external to us, the conclusion that most philosophers from Berkeley on have believed that Locke ought to have come to; rather, he maintained only that our ideas of particular substances might never be known to correspond to their archetypes, thus reinforcing his much argued claim that we cannot expect to have knowledge of substances to any further extent than we have found properties to coexist in nature.⁴⁰ Thus Locke used his definition of knowledge, not to establish the non-existence of the physical world or the lack of our ability to know that it exists, but rather to reinforce his much more particular claim that we cannot be sure of any claims to the essences of particular substances.

For the same sort of reasons, Locke held that we can only know the truth of general propositions in so far as their was both agreement and known reality:-

"To conclude: General propositions, of what kind soever, are then only capable of certainty when the terms used in them stand for such ideas whose agreement or disagreement, as there expressed, is capable to be discovered by us. Whence we may take notice that general certainty is never to be found but in our ideas. Whenever we go to seek it elsewhere, in experiment or observations without us, our knowledge goes not beyond particulars. It is the contemplation of our abstract ideas that is alone is able to afford us general knowledge." 41

There was, then, for Locke, no question of us being able to know the laws of nature, though he did believe that such lack of knowledge was not harmful to the possibility of rating generalizations more or less probable.

Rational Expectation.

Granted that natural philosophy "is not capable of being made a science," Locke considered it important to identify what sort of information was conveyed by the natural philosopher. His position was that natural philosophy was in general a collection of empirical data and a series of hypotheses to explain that empirical data. He implied just this conception when he considered the role of hypotheses in explanation. We may, Locke held:-

"to explain any phenomena of nature, make use of any probable hypothesis whatsoever: hypotheses, if they are well made, are at least great helps to the memory and often direct us to new discoveries. But my meaning is that we should not take up any one too hastily (which the mind, that would always penetrate into the causes of things and have principles to rest on, is very apt to do) till we have very examined particulars and made several well

experiments in that thing which we would explain by our hypothesis and see whether it will agree to them all, whether our principles will carry us quite through and not be as inconsistent with one phenomenon of nature, as they seem to accommodate and explain another. And at least that we take care that the name of principles deceive us not, nor impose on us, by making us receive that for unquestionable truth which is really at best but a very doubtful conjecture, such as are most (I had almost said all) of the hypotheses in natural philosophy." 42

It is important to see the difference there is between the position which Locke urged here and the attitude of Bacon's as we have already seen it expressed. Locke accepted the Baconian view that the mind was indeed too eager to accept hypotheses as true which were only conjectures,⁴³ but, unlike Bacon, Locke did not, for that reason, eschew hypotheses altogether. Hypotheses, he held, could be, and indeed obviously were, useful in natural philosophy, even though, as we have already seen, he elsewhere expressed his contempt for speculative hypotheses which "fill the world with useless, though pleasing visions".⁴⁴ There was, then, for Locke a place for hypotheses in natural philosophy, as long as they were of the right sort, even though in themselves they were not, and could not be, accepted as knowledge.

Locke's attitude in this was entirely at one with his two friends Boyle and Newton. The whole tenor of Locke's conception of the natural sciences was entirely in keeping with that expressed by Boyle in his account of the conditions required for both "good" and "excellent" hypotheses. (Cf. p.159 ff.) It was entirely in keeping too with the position expressed by Newton at the end of the 31st. Query to the Opticks (Cf. above, page 189 ff). All firmly agreed that demonstration of necessary truths about the world was a vain enterprise.

But with Locke , as with the other scientists of the period, the lack of demonstration was not sufficient to cause the abandonment of the programme, for where demonstration was not possible it was still reasonable to assess the probability of the truth of any proposition, and to the importance and rationality of probability in our dealings with the world Locke was heavily committed.

The historical origins of Locke's account of probability are most closely associated with the views expressed by Antoine Arnauld and Pierre Nichole, the authors of the Post Royal Logic or The Art of Thinking (first edition, Paris, 1662). Although the bulk of the book is concerned with formal deductive inference, there are important sections on probability at the end of the work which have a great deal in common with Locke's views as we find them in the Essay, and there is reason to believe that Locke was himself involved in the production of the English edition of the Logic which appeared in 1674.⁴⁵ But Locke's discussion of probability is indeed much more full and rewarding than the earlier writers' work.

Locke's account of probability is to some extent adversely infected with the same error as his treatment of knowledge, namely his tendency to psychologise the subject. He failed to distinguish between a man's judgement that a proposition is probably true, and the grounds for that judgement.⁴⁶ But in practice the error is not of great consequence, for it is on the acceptability or otherwise of the grounds for judgements of probability that Locke rightly concentrates.

To say that something is probable, Locke said, is to say that it is likely to be true, and this implies that there are grounds for

believing it to be so.⁴⁷ The grounds themselves were fundamentally of two sorts: "First, the conformity of anything with our own knowledge, observation and experience.

"Secondly, the testimony of others, vouching their observation and experience."⁴⁸ Locke laid down six factors which he believed of importance in assessing testimony, all relating to the weight of the evidence in favour of the proposition under consideration, and he then went on to state that:-

".... the mind, if it will proceed rationally, ought to examine all the grounds of probability and see how they make more or less for or against any probable proposition, before it assents to or dissents from it; and, upon a due balancing the whole, reject or receive it, with a more or less firm assent, proportionably to the preponderancy of the greater grounds of probability on one side or the other." 49

Thus Locke was committed to the rationality of the acceptance of propositions which nevertheless were only known to be probable, and the probability, and the rationality, was entirely a factor of the weight of empirical evidence in favour of the proposition, for:

"as the frequency and constancy of experience and the number and credibility of testimonies do more or less agree or disagree with it, so is any proposition in itself more or less probable."⁵⁰

Locke argued that our assessment of probabilities was limited by our experience. But he recognised that there were other areas of human enquiry where "the things be such that, falling not under the reach of our senses, they are not capable of testimony."⁵¹ These included "the manner of operation in most parts of the works of nature wherein, though we see the sensible effects, yet their causes are unknown and we perceive not the way and manner in which they are produced." In such areas, he said, "Analogy is the only help

we have, and it is from that alone we draw all our grounds of probability."⁵² He then went on to consider some examples which might plausibly lead us to believe that, say, "heat and fire consists in a violent agitation of the imperceptible minute parts of the burning matter" and concluded: "This sort of probability, which is the best conduct of rational experiments, and the rise of hypothesis, has also its use and influence; and a wary reasoning from analogy leads us often into the discovery of truths and useful productions, which would otherwise lie concealed."⁵³ Locke's recognition of the importance of analogical arguments in science is very relevant to an assessment of Locke's attitude to the possibility of knowledge in natural science. Locke was committed to a large part of natural philosophy always remaining conjecture of varying degrees of probability, and the conjectures themselves should be based upon what we have ourselves experienced, for there is nothing else upon which probability can be based. It is only by the use of analogical arguments that we can advance our understanding of the nature of those parts of the physical world which are not directly open to empirical observation, such as in all our reasonings about sub-microscopic entities.⁵⁴

Throughout Book Four of the Essay Locke constantly returns to the theme of the difficulty of our ever having knowledge, according to Locke's definition of knowledge, of the sub-microscopic entities. Again and again he emphasises the difficulties, and, indeed, the impossibilities, involved in the attainment of knowledge beyond our direct experience.⁵⁵ That Locke saw this issue as an important one should not go unnoticed

for it is indicative of the contemporary state of discussion of the status of contemporary theories about the nature of matter. For the most part the advocates of the varying positions were concerned to make their position the accepted one. Especially was this true of the Cartesians, for undoubtedly a very large part of Cartesian physics presupposed the nature of matter which Descartes had prescribed. Locke's emphasis, therefore, on the unknowability of matter's essence was a direct challenge to Cartesian physics in almost all its aspects. The effect was also to discourage exaggerated claims for the truth of Boyle's corpuscular philosophy. Locke expressed his attitude clearly in Concerning Education where he wrote:-

".... though the world be full of systems of it, yet I cannot say, I know any one which can be taught a young man as a science, wherein he may be sure to find truth and certainty, which is what all sciences give an expectation of I think the systems of natural philosophy, that have obtained in this part of the world, are to be read more to know the hypotheses, and to understand the terms and ways of talking of the several sects, than with hopes to gain thereby a comprehensive scientific and satisfactory knowledge of the works of nature: only this may be said, that the modern Corpuscularians talk, in most things, more intelligibly than the Peripateticks." 56

Certainly for Locke there was no room in science, as there was no room anywhere else for that "enthusiasm" which he so roundly condemned in Chapter XIX of the Essay. A man who loves truth, Locke said, will not entertain "any proposition with greater assurance than the proofs it is built on will warrant."⁵⁷

Locke's recognition of the limits of science in this direction, and his more general recognition of the importance of judgements of probability proportional to the evidence of experience was fundamental to the new attitude to science which had arisen during the course of the scientific revolution.

Knowledge, Religion and Morality.

It is not my intention to offer extensive comments on Locke's religious and moral theories, but it would be wrong in a work of this sort to ignore them entirely, for they are typical of the man and bring out both the strengths and the weaknesses of Locke's approach to philosophical problems.

It is a sign of the times, both Locke's and ours, that whilst today the general attitude is one in which whilst it is allowed that science can and indeed does give knowledge, to make such claims for religion and morality is much more likely to be challenged. In Locke's day, however, the situation was entirely the opposite: it was religion and morality which were accepted as certain; it was science about which there was general scepticism. It is clearly a very significant shift in general outlook, and one which makes the seventeenth century, for all its modernity, one very distant from our own. Locke expressed the general point in his journal entry for 24th June 1681:-

".... he that has a true idea of God, of himself as his creature, or the relations he stands in to God, and his fellow-creatures, and of justice, goodness, law, happiness, &c. &c., is capable of knowing moral things, or have a demonstrative certainty in them"

whereas:

"....Physique, polity, and prudence, are not capable of demonstration, but a man is principally helped in them by the history of matter-of-fact, and a sagacity of inquiring into probable causes, and finding out an analogy in their operations and effects." 58

God's existence could be proved, Locke held, and in the Essay he offered such a proof. The argument is this:

- (a) Man knows he is something which actually exists.
- (b) "Nothing can no more produce any real being than it can be equal to two right angles."
- (c) "from eternity there has been something, since what was not from eternity had a beginning, and what had a beginning must be produced from something else."
- (d) "Next, it is evident that what had its being and beginning from another must also have all that which is in and belongs to its being from another too."
- (e) "so this eternal being must be also the most powerful".
- (f) Similarly, man has perception and knowledge, and, therefore must be some knowing intelligent being.
- (g) Such things could not be caused by something less than themselves; "it is repugant to the idea of senseless matter that it should put into itself sense, perception, and knowledge."
- (h) Therefore "there is an eternal, most powerful, and most knowing being...."⁵⁹

The crucial stages of the argument are contained in (d) and (g). Without pursuing the argument at length, it is enough to say that it is by no means obvious that either of the statements is true. First it is not at all clear why it should be assumed that there is just one thing from which all else has arisen; second, Locke takes for

granted that "senseless matter" is "less" than sense, perception, and knowledge, but it is not at all clear in what sense, if any this can be said to be true. It is certainly not true in a physical sense, i.e. in the sense that, for example, there is not enough energy in the brain to account for the brain states which accompany intellect mental activity. But any other sense of "less" would appear to be a value judgement about the desirability or otherwise of mental over physical happenings, and not a factual matter at all. In the light of these points, therefore, it does not appear that Locke's argument does in fact establish the existence of God by demonstration as he believed.

This said, there are some other considerations about Locke's position which are worth noting. First, Locke never accepted the ontological argument as it had been presented by Descartes as a proof for the existence of God. He made this very clear in a paper, first published in King's Life, entitled "DEUS. - Descartes' Proof of a God from the Idea of necessary Existence, examined. 1696." In it Locke attacked the ontological argument because he said that by it "senseless matter might be the first eternal being and cause of all things, as well as an immaterial intelligent spirit."⁶⁰ His argument was that all that the ontological argument can be said to establish is that there must have been something which has existed from eternity, but it does not establish what that something is. Whereas, said Locke, the difference between an atheist and a theist is not whether something or other has so existed, for both accepted that something had, the question was whether it was an all-knowing

mind or physical matter. Locke concluded with the following typical paragraph:-

"By ideas in the mind we discern the agreement or disagreement of ideas that have a like ideal existence in our minds, but that reaches no further, proves no real existence, for the truth we so know is only of our ideas, and is applicable to things only as they are supposed to exist answering such ideas. But any idea, simple or complex, barely by being in our minds, is no evidence of the existence of anything out of our minds, answering that idea. [To which one can almost hear Berkeley saying "Quite so."] Real existence can be proved only by real existence; and therefore the real existence of a God can only be proved by the real existence of other things. The real existence of other things without us can be evidenced to us only by our senses; but our own existence is known to us by a certainty yet higher than our senses can give us of the existence of other things, and that is internal perception, a self-consciousness, or intuition; from whence therefore may be drawn, by a train of ideas, the surest and most incontestable proof of the existence of a God." 61

It is worth nothing further that Locke bases his argument for the existence of God on the empirical premise of his knowledge of his own existence, albeit an empirical premise which is known intuitively to be true. The fact that Locke believed that one could know some empirical propositions to be certainly true by intuition is an interesting, some might say, peculiar, aspect of his thought. The point is relevant to Locke's thinking on the possibility of moral demonstration as well.

Although Locke did hold that God's existence could be proved formally it was to the Bible that Locke turned as the source of true Christian knowledge. As he wrote to Richard King in 1703, barely a year before his death: "you ask me 'what is the shortest and surest way, for a young gentleman to attain a true knowledge of the Christian religion, in the full and just extent of it?' and to this I have

a short plain answer: 'Let him study the holy scripture especially the New Testament.' Therein are contained the words of eternal life. It has God for its author; salvation for its end; and truth, without any mixture of error, for its matter."⁶²

Like Boyle and many others, Locke saw the truth of the Christian religion vouchsafed for by the miracles described in the Bible, once again illustrating the importance of Hume's examination of the argument from miracles in his first Enquiry. In The Reasonableness of Christianity as Delivered in the Scriptures (1695) Locke wrote:-

"The evidence of our Saviour's mission from heaven is so great, in the multitude of miracles he did before all sorts of people, that what he delivered cannot not be received as the oracles of God, and unquestionable verity. For the miracles he did were so ordered by the divine providence and wisdom, that they never were, nor could be denied by any of the enemies, or opposers of Christianity." 63

And like Newton, with whom Locke had much correspondence on theological topics,⁶⁴ Locke thought that "the works of nature, in every part of them, sufficiently evidence a Deity."⁶⁵

In many respects Locke's attitude towards knowledge of morality matched his approach to religion, which is hardly surprising. He believed that morality could be demonstrated, but he preferred to refer people to the Bible and the sermons of such divines as Dr. Isaac Barrow, Archbishop Tillotson, and Dr. Whichcote.⁶⁶ In the Essay Locke often committed himself to the possibility of producing a deductive system of ethics, though he thought that the production of such a system might be more difficult than geometry. His most clear and famous statement on this topic is to be found in the fourth Book of the Essay

in which Locke suggests both that from a knowledge of God's existence it would be possible to deduce man's duties, and also that there were many self-evident moral propositions. Locke wrote:-

"The idea of a supreme Being, infinite in power, goodness, and wisdom, whose workmanship we are and on whom we depend, and the idea of ourselves as understanding rational beings, being such as are clear in us, would I suppose, if duly considered and pursued afford such foundations of our duty and rules of action as might place morality amongst the sciences capable of demonstration: wherein I doubt not but from self-evidence propositions, by necessary consequences as incontestable as those in mathematics, the measures of right and wrong might be made out to anyone that will apply himself with the same indifferency and attention to the one as he does to the other of these sciences Where there is no property there is no injustice is a proposition as certain as any demonstration in Euclid: Again, no government allows absolute liberty: the idea of government being the establishment of society upon certain rules or laws which require conformity to them, and the idea of absolute liberty being for anyone to do whatever he pleases, I am as capable of being certain of the truth of this proposition as of any in mathematics." 67

Locke never produced the work on ethics hinted at in this passage though he was often asked to do so, but in the Lovelace collection of Locke manuscripts there are a few notes which suggest that Locke at least toyed with the idea.⁶⁸

There are major difficulties in Locke's ethical programme which to consider in detail would require a work of their own.⁶⁹ However it is possible briefly to indicate some of the main difficulties and to draw attention to some of the implications of Locke's position.

Although Locke held that ethics was capable of demonstration he was also a Hedonist. As Locke expressed it in the manuscripts already referred to "Morality is the rule of man's acting for the attaining happiness."⁷¹ A similar outlook was expressed in the Essay when he

wrote "what has an aptness to produce pleasure in us is that we call good, and what is apt to produce pain in us we call evil, for no other reason but for its aptness to produce pleasure and pain in us, wherein consists our happiness and misery."⁷² Locke seems to have believed that men always act for the attainment of happiness, he wrote in his notes "Axiom I. All men desire the enjoyment of happiness and the absence of misery and that only and always"⁷³ Yet, at other times, he seems committed to the view that it is man's duty to pursue happiness: thus Locke wrote: "It is a man's proper business to seek happiness and avoid misery", in a paper entitled "Thus I so Think"⁷³ which is a statement of Locke's very Aristotelian ethical views. But clearly if man cannot help pursuing happiness it is very odd to think of him as having a duty to pursue it. Further it is by no means clear how Locke came to know the truth of either of these statements. That men desire happiness might be discovered empirically, but Locke is so confident of its truth that it appears to be more than this, thus he wrote: "If it be further asked, what is it moves desire? I answer: happiness, and that alone."⁷⁴ He treats the proposition as if it were one of the self-evidently true propositions from which the deduction of ethics could begin, as indeed is made clear from his labelling it an axiom in his manuscript notes.

Certainly we can agree with Locke that certain moral propositions are necessarily true. But, as Berkeley was to point out in his Commonplace Book:-

"To demonstrate morality it seems one need only make a dictionary of words, and see which included which. At least, this is the greatest bulk of the work. Locke's instances of demonstration in morality are, according to his own rule, trifling propositions." 75

It is worth remembering, too, that Locke, in the Essay, held that the reason why we could know the truth of moral propositions with certainty was because there were no entities against which we have to match up our moral notions to make sure that they correspond. (See the section on Demonstrative Knowledge above.) But such an attitude is totally incompatible with an objective ethics, and yet surely Locke, who believed in the Law of Nature, cannot really have subscribed to such a subjectivist position. The comparison with mathematics here is rather misleading, for, whereas we can agree with Locke that there are no Platonic mathematical entities, but nevertheless we can also agree that mathematical propositions are true in a perfectly objective sense, it is by no means obvious that the comparison with ethical propositions can be taken so far. Further, it is just in so far as we might wish to challenge the unquestionableness of a mathematical system, for example, as has occurred with the introduction of alternative geometries, that we are inclined to think of truth as being perhaps an inappropriate dimension of assessment for the propositions of geometry.

All in all Locke's treatment of moral philosophy is far from satisfactory. But if his thinking was muddled the effect of that thinking was not altogether bad, for Locke's emphasis on the importance for morality of happiness, a concept not altogether lacking

in empirical content, was the first clear sign of the rise of the utilitarianism which was to come in the eighteenth and nineteenth centuries. Furthermore, it was, I believe, an awareness of the difficulties of obtaining knowledge in any areas of enquiry which was itself much emphasised by the new science which encouraged that attitude of toleration on matters of private belief which was such an important feature of Locke's general attitude, and which was to become a cornerstone of English liberalism.

Conclusions.

The implications of Locke's positive account of the nature and limits of knowledge in Book Four for the contemporary scientific enterprise, and indeed for all succeeding enterprises, is very far reaching. The wider implications of Locke's findings we shall turn to in the next section; here let us just take notice of some direct consequences without attempting to assess their full cosmic significance.

From what we have seen it emerges that Locke's definition of knowledge, with its psychological link with certainty, leads him to include within the scope of knowledge only a very limited number of the sorts of things which we are normally quite happy to say that we know. Certainly, therefore, Locke's concept of knowledge was far narrower than we are content to work with in our ordinary dealings with the world, and for that reason it can rightly be maintained that whatever concept of knowledge it was that Locke examined it was not our concept, nor, indeed, in his less guarded moments, was it even Locke's. But, be that as it may, the concept that Locke identified

is certainly identifiable, and Locke was right to stress that there is an important distinction to draw between that which is necessarily so and that which is not. In emphasising this distinction Locke immediately forced attention on the grounds which we can produce for justifying claims which are not themselves immediately conceded as necessary, and it was here that he was entirely right to argue that in such areas it is experience and experience alone, which could give any grounding for rational probability. But Locke's point was in fact a great deal stronger than this. He argued not only that where we had no agreement of ideas then we also had no ground for knowledge, but also that agreement in ideas was not, in itself, sufficient to give knowledge over important areas of our investigations, for in so far as our ideas referred or stood in place of objects external to us, then we could only be sure that the necessary agreements between our ideas were truths about the world if our ideas were indeed true copies of the entities which they represented, or, in more modern idiom, only if our concepts correspond to reality can we have knowledge.

Granted that knowledge was thus limited, granted that its limitations arose out of the nature of man's relation to the total creation, the implications of Locke's conclusions for the future of science were fundamental and far-reaching.

CONCLUSIONS

Conclusions

It now remains to attempt some appraisal of the significance of Locke's philosophy in relation to the scientific revolution. Locke's two most important effects were, I believe, first, to damp down enthusiasm for unbridled speculation in both natural philosophy and other areas of human enquiry, and, second, to discredit the optimism of those who expected the new science to bring certainty in its train. It might be held that two such negative points could hardly be rated of great importance, but this is surely incorrect. The task of the underlabourer is to clear the ground, not to erect large buildings. More particularly, the amount of ground which is cleared very much determines the kind of building which can be placed upon the site. It was indeed just this which Locke successfully did. After his work certain intellectual edifices which might have arisen were seen to be no longer possible projects. No doubt the landscape of Europe was saved some monstrous enterprizes.

Locke's message was that our intellect is bounded by our experience, and what we can know is therefore bounded by our experience. The implications of this thesis are wide reaching and fundamental, for it spelled the end of several traditions in philosophy and science which had, until the end of the seventeenth century, great influences on contemporary thought. Foremost amongst these were (a) the Platonism which we have seen was central to much of the thought of the early scientific revolution; (b) the rationalism of the Cartesian programme, and, (c) connected with both of these movements, but also extending

beyond them into, for example, the work of Bacon, the optimistic hope that scientific research in its various forms could lead to certainty.

It is true that by the time that Locke wrote Platonism was not a marked feature of the science of the period, but there was an important indirect influence which Locke's thesis did much to undermine. It was the thesis that mathematics had a privileged status amongst all the scientific disciplines. It was Locke's view that mathematics, far from being able to supply insight into the perfect world of forms, was in fact a product of the intellect, and although Locke's conventionalist position was not presented with much sophistication, it stands in very marked contrast to the position of the Neoplatonic thinkers of the early part of the century. Even if, therefore, Platonism in science, and Platonism in philosophy generally was not currently a powerful force, the effect of Locke's approach was to rule out of court all pretensions to that particular metaphysics. Indeed Platonism in either English science or English philosophy has had little influence since. Although it is difficult, perhaps impossible, to estimate such influences it seems not unlikely that the tradition of empiricism in the form we have found it in the English science of the later seventeenth century, with its intellectual support in the shape of the Essay were important causal influences towards that outcome.

Much more certainly can we see the impact of Locke's empiricism on the rationalist programme of Descartes. The road to knowledge,

Locke argued, cannot lie along the route mapped out in the Discourse on Method for there is no such road to be charted. Knowledge as Descartes and Locke understood it, was not simply or only a function of our recognition of clear and distinct ideas, but depended also, for most areas of inquiry, upon those ideas correctly representing reality external to the mind. Natural philosophy, therefore, could not with certainty extend beyond experience, and the rationalist's distant, but none the less real, hope that science could be reduced to mathematics was deeply discredited.

Equally important, and no less damagingly, Locke's account of the nature and limits of knowledge undermined the faith of those who saw the goal of natural philosophy as any form of certainty which would never need revision in the light of future experience. The generalizations of the natural philosopher, no matter how well grounded in past experience, and no matter how certainly deducible from some given principle, might always have to be given up in the light of future experience; they could never be other than morally certain or highly probable.

Locke's commitment to the lack of absolute certainty, however, never led him into any deep scepticism about rationality. Although in most areas of human enquiry deductive certainty was not possible, it did not follow from this that rational belief was not possible. Rationality, Locke held, consisted in proportioning one's belief to the testimony of either our own senses, or where that was lacking, that

of other people's. Where no such testimony was available, then conjecture might lead us to plausible analogy, itself uncertain, but not, for that reason, to be reckoned useless.

Undoubtedly Locke's epistemology contains many errors: his lack of clarity on the nature of logical necessity, his thesis that all that we are ever aware of are ideas, and his representationalist theory of perception, his narrow definition of knowledge, and his downgrading of the imagination, were all, in their way deep and important influences on subsequent philosophy. But his positive achievement was to set men straight on the possible goals which could be achieved. His statements of the limits of the possibilities of the scientific enterprise were largely true, historically important, and very relevant.

At the beginning of this work I have quoted a passage from one of the oldest of the extant scientific writings. To a very large extent Locke's Essay was a reaching back across the intervening centuries to the writer of those words. The scientific revolution was to a large extent an anomaly in the history of thought: the mysticism of the Neoplatonists, the rationalism and the optimism of the practitioners of the new science stood in need of cool appraisal. It was just this appraisal which Locke so ably supplied.

NOTES

Notes

Introduction.

1. All references to the Essay Concerning Human Understanding will be to the Everyman edition (London, 1961) edited by John W. Yolton, this being by far the most accurate readily available full edition. Ed. cit. p. xxxv.
2. Stated thus, my position clearly takes for granted that there was a scientific revolution in the seventeenth century. This proposition I think few would, and nobody should, doubt. I also believe that a core aspect of that revolution was conceptual. I have some sympathy, for the account of science offered by T. H. Kuhn in his The Structure of Scientific Revolutions (Chicago, 1962) though I think that Kuhn tends to overstate his case. For discussion of Kuhn's views see I. Lakatos and A. Musgrave (eds.): Criticism and the Growth of Knowledge. (Cambridge, 1970).
3. Op. cit., (London, 1971). Vol. V, p. 71.
4. The works of Sextus Empiricus were first published in modern times in 1562, but they were undoubtedly influential before that. On the scepticism of the period see especially R. H. Popkin's The History of Scepticism from Erasmus to Descartes, (Assen, revised edition, 1964.) Richard A. Watson in his The Downfall of Cartesianism (The Hague, 1966) has argued that it was largely sceptical arguments which undermined the position of Cartesians in the much later period from 1673 to 1712.

5. On this see, for example, Paul H. Kocher's Science and Religion in Elizabethan England, (San Marino, Cal., 1953.) especially Ch. III.
6. History of England, Ch. LXXI.
7. Augustine to Galileo, Second edition, (London, 1957) Vol. 2, p. 116.
8. Essay, IV III 6.
9. The extent of this influence is sometimes exaggerated by historians of science. For example J. D. North in his Isaac Newton (London 1967) writes that Newton's Opticks had a great influence on Locke. Yet the Opticks was not published until 1704, in fact the year in which Locke died! It is true, however, that Locke and Newton clearly talked together of optics, perception and such phenomena. In June 1691 Newton wrote to Locke describing the formation of an after-image. Newton's description of the self-experiment arose out of a discussion on Boyle's book Experiments and Considerations Touching Colours (1664), (first published as Experimental History of Colours in 1663). "It deals with a large variety of phenomena and contains many generalizations which were subsequently adopted by Newton in his memorable treatise on 'Optics'". (J. S. Fulton: A Bibliography of the Honourable Robert Boyle F.R.S., Oxford, 1961, p. 43.
10. Essay, II VIII 9.
11. Essay, II VIII 10.
12. University of Toronto Studies. Philosophy II No. 1. (Toronto, 1923.)

13. Cambridge, 1917.
14. John Locke, (Oxford, 1937, second edition 1955).
15. It is perfectly possible for a philosopher not to fall into any of these categories. He may remain agnostic or believe it to be a muddled issue. Anthony Quinton has pointed out that there is currently a reluctance on the part of many philosophers to give a definite answer to the problem of mind and matter. 'Mind and Matter', Brain and Mind editor J. R. Smythies, (London, 1965) especially pp. 202-203.
16. In labelling Spinoza a materialist it might be thought I do him an injustice. In one sense this is true. Spinoza's system did not involve, in fact expressly denied, that mind could be reduced to matter. Spinoza saw matter and mind as two attributes of one single substance which could not be identified with either of them. On this see Stuart Hampshire's Spinoza (London, 1951) p. 81. Spinoza's views, or alleged views, placed him in considerable danger. At the time of his excommunication from the Jewish religion (1656) he was assaulted by a fanatic, but the dagger used only pierced his coat. (cf. Sir Frederick Pollock's Spinoza, His Life and Philosophy (second edition, London, 1899) pp. 16-17.

It is worth pointing out that Spinoza seems to have had little or no influence on Locke's philosophy. Although Locke owned all the available published works of Spinoza, including the Ethics, published posthumously in Opera Postuma, (Amsterdam, 1677). Locke never once, so far as I can

establish, refers to Spinoza in his writings. There is undoubtedly a great gulf in their approach to philosophy, at least at first acquaintance, for, while Spinoza saw his task as that of the great metaphysician, producing the unified account of knowledge, Locke's whole intellectual approach was much more piecemeal. Locke owned, besides Spinoza's works, various criticisms of Spinoza's philosophy, so he was clearly aware of him as an intellectual figure.

About Hobbes's materialism there can be no question. In Leviathan he wrote:-

"The World, (I mean not the earth only ... but the Universe, that is the whole mass of all things that are) is corporeal, that is to say, body; and hath the dimensions of magnitude, namely, length, breadth, and depth: also every part of the body, is likewise body, and hath the like dimensions; and consequently every part of the Universe is body; and that which is not body is no part of the Universe: And because the Universe is All, that which is no part of it, is Nothing; and consequently no where." (Part IV, Ch. 46.)

The contemporary reaction to the "Monster of Malmesbury" is recounted in Samuel I. Mintz's The Hunting of Leviathan (Cambridge, 1962).

The rejection of materialism by Newton and his followers is illustrated by some remarks of Samuel Clarke in his correspondence with Leibniz.

Clarke wrote:-

"That there are some in England, as well as in other countries, who deny or very much corrupt even natural religion itself, is very true, and much to be lamented. But (next to the vicious affections of men) this is to be principally ascribed to the false philosophy of the materialists, to which the mathematical principles of philosophy are the most directly repugnant. That some make the souls of men, and others even God himself, to be a corporeal being; is also very true: but those who

do so, are the great enemies of the mathematical principles of philosophy; which principles, and which alone, prove matter to be the smallest and most inconsiderable part of the universe." (The Leibniz-Clarke Correspondence, edited by H. G. Alexander, (Manchester 1956) p. 12.)

17. In 'The Refutation of Idealism', Mind, N. S. Vol. XII, 1903. Reprinted in Philosophical Studies (London, 1922) pp. 5 and 6.
18. The clearest statement of the central thesis in Berkeley is found in Section 3 of The Principles of Human Knowledge, (1710).
19. Again, compare Berkeley in The Principles of Human Knowledge, especially Section 66 and Sections 101-135.
20. Kepler published his theory in the work known generally as the Mysterium Cosmographicum (1597). Cf. Max Casper's Kepler (New York, 1959) Ch. 2, and Arthur Koestler's The Sleepwalkers (London, 1959) pp. 247-267.
21. For further discussion of this see Chapter Two.
22. All quotations from Newton's Principia, will be from Andrew Motte's translation, revised by Florian Cajori, published as Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World, (Berkeley and Los Angeles, 1962). Rule IV is to be found on p. 400.
23. All references to Descartes's works will be to The Philosophical Works of Descartes, edited by Elizabeth Haldane and G. R. T. Ross, (Cambridge 1911, corrected edition 1931,) in two volumes, unless otherwise stated. The quotation is to be found in Vol. I, p. 262.
24. All references to Hobbes will be to The English Works of Thomas Hobbes, edited by William Molesworth, (London, 1839) in eight volumes, unless

- otherwise stated. The quotation occurs in Part IV, Ch. XXX Section 15 of the Elements of Philosophy. Works, Vol. I, p. 531.
25. Ed. cit., pp. 544-545.
 26. Essay, ed. cit. p. xxxv.
 27. Pierre Gassendi: Syntagma Philosophicum. Quoted in Aaron: John Locke, p. 35.
 28. Quoted from Selections from the Notebooks of Leonardo da Vinci, edited by Irma A. Richter. (London, 1952) pp. 4 and 5.
 29. Gilbert Ryle has distinguished five senses in which Locke uses the term 'idea' in the Essay., 'John Locke and the Human Understanding', published in Tercentenary Addresses on John Locke, edited by J. L. Stocks (London, 1933). Ryle's paper is reprinted in Locke and Berkeley, edited by C. B. Martin and D. M. Armstrong, (London, 1968).
 30. In Charleton's "Physiologia Epicuro-Gassendi-Charltonia, or a Fabric of Science Natural Upon the Hypothesis of Atoms". (1654) passim.
 31. Cf. The Principles of Human Knowledge, Section 50.
 32. Essay, IV I 2.
 33. Cf. Leibniz: New Essays Concerning Human Understanding, Bk. IV, Ch. I.
 34. January 17 1692/3. Published in The Correspondence of Isaac Newton, (Cambridge 1961 -), Vol. 3, edited by H. W. Tumbull, p. 240. Hereinafter referred to as Correspondence.

35. Opticks, second English edition 1717. Query 31. In the Dover Publication edition of the Opticks (1952) from which quotations are taken the reference is p. 400.
36. On Education was first published in 1693, collected from a series of letters Locke had written from Holland to Edward Clarke of Chipley about the education of Clarke's son. Reprinted in Locke's Works (1768 edition) Vol. IV, pp. 1 - 135. The actual reference is to be found on p. 121. All references to Locke's Works will be to the 1768 edition.
37. In the General Scholium added to the second edition of the Principia, published in 1713. Ed. cit., p. 547.
38. Cf. Newton's Correspondence, Vol. I, p. 209.
39. Some Familiar Letters between Mr. Locke and Several of his Friends. Locke to Thomas Molyneux, Nov. 1 1692. Published in Locke's Works, ed. cit., Vol. IV, p. 384.
40. The most obvious example is Essay, II VIII II. See also Locke's reply to Stillingfleet, Works, Vol. I, p. 754,; A. C. Fraser's note on p. 17 of Vol. I of his edition of the Essay, and A. Koyré's Newtonian Studies (London, 1965) pp. 154-155.
41. Locke's influence appears discernible in Newton's projected (and rejected) Rule V of the Rules of Reasoning in Philosophy. Cf. Koyré's Newtonian Studies esp. pp. 271-272

42. Compare Ryle's story:-

"A good many years ago, I happened to be sitting with Earl Russell in a restaurant-car of a train to North Wales. Somehow our conversation turned to John Locke and I put to Russell this very question, perhaps with some hyperbole, - 'Why is it that although nearly every youthful student of philosophy both can and does in about his second essay refute Locke's entire Theory of Knowledge, yet Locke made a bigger difference to the whole intellectual climate of mankind than anyone had done since Aristotle?' Russell agreed that the facts were so, and suggested, on the spur of the moment, an answer which dissatisfied me. He said 'Locke was the spokesman of Common Sense.' Almost without thinking I retorted impatiently 'I think Locke invented Common Sense.' To which Russell rejoined 'By God, Ryle, I believe you are right. No one ever had Common Sense before John Locke - and no one but Englishmen have ever had it since.'" Gilbert Ryle: 'John Locke'. Critica, Vol. I, No. 2, May 1967, pp. 3.4.

Chapter I

1. I shall refer to Copernicus's On the Revolutions of the Heavenly Spheres by its abbreviated Latin title, De Revolutionibus, as is customary. There is only one available English translation, from which quotations will be taken. It is published by Encyclopedia Britannica Inc., in Volume 16 of the Great Books of the Western World series (Chicago, 1952). The work was first published in 1543 weeks before Copernicus's death.
2. On this see A. Koestler: The Sleepwalkers (London, 1959) pp. 165-172.
3. The letter is quoted in Kepler's Apologia Tychonis contra Ursum, published in Kepler's Opera Omnia, ed. Frisch, Vol. I, pp. 236-276. The translation is that published in A. Koestler's The Sleepwalkers, (London, 1959) p. 167.
4. Ed. cit., p. 505.
5. Ibid, pp. 505-506.
6. Simplicius, De Caelo . 488a. Quoted in S. Sambursky: The Physical World of the Greeks. (Second edition, London 1960.) p. 59. (The reference to Simplicius in Sambursky reads incorrectly '498a'.)
7. Plato, in the Republic, seemed not very interested in observation. We find Socrates saying:

"If we mean, then, to turn the soul's native intelligence to its proper use by a genuine study of astronomy, we shall proceed, as we do in geometry, by means of problems, and leave the starry heavens along."
Republic VII 530 B. (F. M. Cornford's translation, Oxford, 1941.)

8. Cf. A.C. Crombie: Augustine to Galileo, Vol. I, p. 82, ff.
9. Ia XXXII, 1 and 2. The translation is that of the Blackfriars edition of the Summa Theologiae, Vol. 6, p. 107. (London, 1965).
10. Why 'Neoplatonism' is to be preferred as a term to 'Platonism' to characterise these Renaissance philosophies is discussed by Nesca A. Robb in her Neoplatonism of the Italian Renaissance (London, 1935), Introduction.
11. Cf. Republic passim, but especially V 476, VII 514 a - 521 b, and VII 529.
12. Cf. Republic, VII 521 c - 541.
13. Cf. Republic, VII 521 c - 531 e.
14. The arguments to substantiate this claim could be several. One would be: Knowledge is knowledge of the Forms. The Forms cannot change, so any proposition about the Forms which is true could never become untrue. Therefore, any true proposition about the Forms is necessarily true, as it is necessarily impossible for its negation to be true.
15. For a discussion of the importance of Cusanus for the Renaissance see Ernst Cassirer's The Individual and the Cosmos in Renaissance Philosophy, especially Ch. I. (First German edition 1927; first English edition, Oxford, 1963.)
16. Of Learned Ignorance (London, 1964) p. 11.
17. Ibid, pp. 25-26.

18. On this see P. O. Kristeller: 'Florentine Platonism and Its Relations with Humanism and Scholasticism'. Church History, VIII, (1939). p. 201 ff.
19. The Commentariolus. Published in Three Copernican Treatises, trans. and ed. by E. Rosen, (New York, 1959), pp. 57-58.
20. Ibid, p. 59.
21. Narratio Prima. Published in Rosen op. cit., pp. 147-148.
22. De Revolutionibus, ed. cit., pp. 526-527.
23. Ibid, pp. 527-528.
24. Rosen, op. cit., p. 139.
25. Quoted from Frances A. Yates: Giordano Bruno and the Hermetic Tradition (London, 1964), pp. 152-153.
26. Quoted from Yates, ibid, p. 236.
27. The Commentariolus, Rosen, p. 59.
28. Copernicus did not quite identify the centre of the universe with the sun's centre. The centre of the universe was for him the centre of the earth's orbit, which was a little distance away from the sun.
29. Published in English translation in the same volume as the translation of De Revolutionibus, p. 857.
30. Quoted in Max Casper: Kepler. First German edition 1948; English edition, New York, 1959. All quotations from the Collier paperback edition, 1962, p. 303.

31. Ibid, p. 49. Interestingly, Kepler did not think that Copernicus advocated physical or metaphysical reasons for his system. He presumably accepted Osiander's Preface as expressing Copernicus's view.
32. Quoted in Casper's Kepler, pp. 65-66.
33. The fact that further observation of the orbit of Mars led Kepler to abandon his theory, re-inforces, rather than detracts, from the point. The geometrical forms had to exist in nature, Kepler was not interested in vague approximations.
34. Epitome of Copernican Astronomy, ed. cit., p. 861.
35. Ibid, p. 863.
36. Of Learned Ignorance, ed. cit., p. 16.
37. 'Necessity, Contingency, and Natural Law'. Melanges Alexandre Koyré, Vol. 2, L'Aventure del'Esprit, ed. I. B. Cohen and R. Taton, (Paris, 1964) pp. 458-470.
38. On Grosseteste and Bacon see A. C. Crombie: Grosseteste and the Origins of Experimental Science (Oxford, 1953.) On the continuity of English thought from Roger Bacon to the Renaissance see F. R. Johnson: Astronomical Thought in Renaissance England (Baltimore 1937) p. 76 ff.
39. Recorde: The Castle of Knowledge (1556) pp. 98-99.
40. Astronomical Thought in Renaissance England, pp. 82-83.
41. Unfortunately, the work promised by Recorde in The Castle of Knowledge in favour of the Copernican system was never written. But Recorde's discussion of Copernicus certainly implies at least a very sympathetic attitude.

42. Giordano Bruno and the Hermetic Tradition, p. 150.

43. Quoted from Henry Billingsley's translation of The Elements of Geometrie of the most ancient Philosopher Euclide of Megara (1570). The pages of the Preface by Dee are not numbered. All quotations from Dee are from this work.

44. Cf. Popkin: The History of Scepticism from Erasmus to Descartes, Ch. 2.

Chapter II

1. 'Galileo and the Scientific Revolution'. Published in Koyré's Metaphysics and Measurement (London, 1968) p. 14.
2. The much debated question 'Was Galileo a Platonist?' would, I believe, gain from the employment of Wittgenstein's concept of 'family resemblance'. (Cf. Philosophical Investigations (Oxford, 1953) I p. 65 ff.) There are undoubtedly some important links which show a traceable historical connection between the outlook of the Florentine Academy - and, indeed, the Academy of Plato, - and the attitudes of Galileo. But what is probably not at all helpful is to assume that there are a set of necessary and sufficient beliefs in virtue of which it is possible definitively to identify somebody as a Platonist. For further discussions of Galileo's Platonism see the papers by Eric Cockrane, Ernst Cassirer, Edward W. Strong, and Thomas P. McTighe in Galileo, Man of Science, edited by Evvan McMullin, (New York, 1967.)
3. Published as 'The Starry Messenger' in Discoveries and Opinions of Galileo (New York, 1957), edited and translated by Stillman Drake.
4. Ibid, p. 24.
5. Ibid, p. 24, in a footnote.
6. Johannes Kepler: Gesammelte Werke, ed. by W. V. Dych and Max Casper. Vol. XIII, p. 130-131. Quoted in Koestler's The Sleepwalkers p. 356.

7. References will be to Stillman Drake's edition. (Berkeley and Los Angeles, 1962.)
8. Ibid, pp. 5-6.
9. Ibid, p. 6.
10. 'Letter to the Grand Duchess Christina' (1615). Published in Discoveries and Opinions of Galileo, p. 179.
11. From the Forward to Drake's edition of the Dialogue Concerning the Two Chief World Systems, p. xix.
12. The Assayer, published in Discoveries and Opinions of Galileo, pp. 237-238.
13. Sarsi was a critic of Galileo's views who was specially singled out for attack in The Assayer.
14. Discoveries and Opinions of Galileo, p. 241.
15. Two Chief World Systems, p. 19.
16. Ibid, p. 11.
17. Two Chief World Systems p. 406. Francis Bacon, for different reasons, shared Galileo's assessment of the vigour of Gilbert's work. Bacon's reasons for this judgement are discussed in Chapter IV.
18. Ibid, p. 207.
19. The Dialogues Concerning Two New Sciences, trans. Henry Crew and Alfonso de Salvio, with an introduction by Antonio Favaro, first published New York, 1914. Reprinted by Dover Publications Inc. New York. All

references will be to this edition.

20. Ibid, p. 170.
21. Two Chief World Systems, p. 31
22. Ibid, p. 32.
23. Ed. cit., p. 513.
24. It is in fact incorrect to attribute this view to Plato. On this see Koyré's Newtonian Studies, pp. 217-219.
25. Two New Sciences, p. 261.
26. Two Chief World Systems, Forward, p. xvii.
27. On this see Karl Popper's 'Three Views Concerning Human Knowledge'. First published in Contemporary British Philosophy, Third Series, ed. by H. D. Lewis (London, 1956). Reprinted in Popper's Conjectures and Refutations (London, 1963) p. 97 ff.
28. Discoveries and Opinions of Galileo, p. 274.
29. Ibid, p. 275.
30. Ibid, p. 277.
31. Ibid, pp. 275-277.
32. Two New Sciences, p. 19.
33. 'The Atomic View of Matter in the XVth, XVIth, and XVIIth. Centuries', Isis, 10, (1928), p. 457.
34. Published in 'On Motion' and 'On Mechanics', trans. and ed. I. E. Drabkin and Stillman Drake (Maddison, 1960) p. 15.

35. Two New Sciences, p. 18.
36. Ibid, p. 160.
37. Ibid, pp. 166-167.
38. Ibid, p. 243.
39. Discoveries and Opinions of Galileo, p. 197.
40. Two New Sciences, p. 179.

Chapter III

1. I therefore disagree with Richard A. Watson's thesis that Cartesianism was abandoned because of insoluble epistemological problems which it raised, e.g. that no satisfactory account of the relationship between mind and body was possible in Cartesian terms. (Cf. The Downfall of Cartesianism (1673 - 1712), (The Hague, 1966). Watson's explanation is to me not very convincing because many of the same epistemological problems are to be found, unanswered, in Locke, yet Locke's influence grew rapidly in the early eighteenth century.

The main reasons, I would urge, for the rejection of Cartesianism was the success of the empirical method as exemplified by Newtonian science and Locke's epistemology.

2. It perhaps says something about the two men that whilst Newton certainly, though somewhat surreptitiously, aimed his Mathematical Principles of Natural Philosophy at Descartes's Principles of Philosophy (compare the titles), Locke did not attack any particular work or author in his Essay Concerning Human Understanding.

Newton was very explicit in his attacks on Descartes's Principles in his early unpublished works. There are, for instance, glimpses of a polemical style not usually associated with Newton in his denunciation of Descartes's physics in his paper 'De Gravitatione et Aequipondio Fluidorum', probably written between 1664 and 1668. "I shall venture to dispose of his (Descartes's)

fictions", he wrote, and, "we now see how absurd is this doctrine of Descartes."

This paper is now published in Unpublished Scientific Papers of Isaac Newton, ed. A. R. Hall and M. Boas Hall (Cambridge, 1962).

3. On this see A. Koyré: 'Newton and Descartes'. Published in his Newtonian Studies.

4. In a letter to Lady Masham. Mss. in Remonstrants' Library, Amsterdam. Lady Masham to Le Clerc, 12th January 1704-5. Quoted in Fox Bourne: Life of John Locke (London, 1876) Vol. I, pp. 61-62, and also in M. Cranston: John Locke, A Biography (London, 1957) p. 100.

5. An attitude which I have in some way attempted to correct in my article 'Boyle, Locke, and Reason', Journal of the History of Ideas, Vol. XXVII, No. 2, 1966.

6. The Two New Sciences, then circulating in manuscript. Cf. C. Adams and P. Tannery, Oeuvres de Descartes (Paris, 1898) Vol. II, p. 402, Note 1.

References to this work will in future be abbreviated to 'A. and T.'.

7. A. and T., Vol. II, p. 380.

8. A. and T., Vol. II, p. 433.

9. Cf. Aubrey's story of Hobbes:-

"He was 40 years old before he looked on Geometry; which happened accidentally. Being in a Gentleman's library, Euclid's Elements lay open, and 'twas the 47 El libri I. He read the Proposition. By G-, said he (he would now and then swear an emphatical Oath by way of emphasis) this is impossible! So he reads the Demonstration of it, which referred him back to such a Proposition; which proposition he

read. That referred him back to another, which he also read. Et sic demceps (and so on) that at last he was demonstratively convinced of that truth. This made him in love with Geometry." John Aubrey, Brief Lives (Penguin edition, London, 1962) p. 230.

10. Haldane and Ross, Vol. I, p. 208.
11. Haldane and Ross, Vol. I, p. 158.
12. Discussion on this topic is to be found throughout the vast literature on Descartes. Some important recent articles are: L. Miller: 'Descartes, Mathematics, and God', Philosophical Review, LXVI (1957) pp. 451-465, reprinted in Meta-meditations, ed. A. Sesonske and N. Fleming, (Belmont, 1965); Jaako Hintikka: 'Cogito ergo sum: Inference or Performance?', Philosophical Review, LXXI (1962) pp. 3-32, reprinted in Meta-meditations, and also in Descartes, ed. Willis Doney, (London, 1967). Also see the articles and extracts in the same volume by A. J. Ayer, Bernard Williams, A. K. Stout, Harry G. Frankfurt, and Alan Gewirth. See also the related articles in the bibliography of that volume.
13. On this see especially Harry G. Frankfurt's article 'Descartes' Validation of Reason', referred to above, originally published in American Philosophical Quarterly, Vol. II, No. 2, April 1965, pp. 149-156.
14. It is, I imagine, fairly obvious from this that I find none of the arguments which I have seen, exonerating Descartes from the charge of circularity, convincing, despite their ingenuity.
15. Haldane and Ross, Vol. I, pp. 208-209.

16. Ibid, p. 255.
17. A. and T. Vol. IX, p. 212.
18. Principles of Philosophy, Part II, IV, Haldane and Ross, Vol. I, p. 256.
19. Ibid. Haldane and Ross, p. 256.
20. Principles of Philosophy, Part I, LIII, Haldane and Ross, Vol. I, p. 240.
21. In his article 'The Creation of the Etemal Truths in Descartes's System'. First published in Reue Philosophique de la France et de l'Etranger, Vol. CXIII, Nos. 5-6, 7-8, (May-August, 1937) pp. 15-29. Reprinted in Descartes, ed. Willis Doney, p. 202.
22. See especially Haldane and Ross, Vol. II, pp. 248, 250 and 251.
23. Meditations, III.
24. God's existence, Descartes said, is necessary. All other existences, he said, are contingent. Cf. Principles of Philosophy, Part I, XIV and XV.
25. A. and T., Vol. III, p. 39.
26. Principles of Philosophy, Part III, IV, A. and T., Vol. IX, pp. 104-105.
27. A. and T., Vol. II, p. 141.
28. This remark needs some qualification, this Newton did not believe that general mathematical propositions stood in need of empirical confirmation, but as a general statement it will do to indicate the vastness of the gap separating Cartesian from Newtonian method.

29. Haldane and Ross, Vol. I, p. 121.
30. Cf. Principles of Philosophy, Part III, XLIII, XLIV, XLV, XLVII; Part IV, I. A. and T., pp. 123-124, and p. 201.
31. The same point can be made about Descartes' account of the function of the heart in the circulation of the blood. His explanation is wrong, but it is an explanation which fits in with his general preconceptions. Descartes' mistake is more reprehensible as he was familiar with Harvey's work on the circulation of the blood which correctly identified the heart's function. Cf. Discourse on the Method, Part V. Haldane and Ross, Vol. I, p. 112 ff.
32. Principles of Philosophy, IV, CCIII. Haldane and Ross, Vol. I, pp. 299-300.
33. Haldane and Ross, Vol. I, p. 300.
34. Ibid, Vol. I, p. 300.
35. Ibid, Vol. I, p. 301.
36. Ibid, Vol. I, p. 302.
37. Rohault's Treatise on Physics was the standard textbook in Cambridge between 1680 and 1700, until replaced by Newton's Principia.
38. Bodleian Library Msc. Locke f. 3, Locke's journal for 1678. An entry for 4th March quotes from Rohault's Traité de Physique.
39. Rohault was not alone in this attitude. Huygens, for example, wrote:
"M. des Cartes has found the means of getting his conjectures and fictions accepted as truths ... I have recovered a great deal from the

infatuation I had of it." Oeuvres Completes, Vol. X, p. 403.

See also the only "probable" explanations of phenomena offered by Huygens in the Traite de la Lumiere (1690). Cf. Treatise on Light, translated by Silvanus P. Thompson, London, 1912, reprinted by Dover Publications Inc. p. 2.

40. Quoted from John Clarke's translation: Rohault's System of Natural Philosophy Illustrated with Dr. Samuel Clarke's Notes Taken mostly out of Sir Isaac Newton's Philosophy. (London, 1723). Two volumes. There are no page numbers to the Author's letter. All quotations will be from this edition of Rohault's work.

41. Ibid.

42. It would be worth making a comparison with the method recommended by Rohault and the method actually practised by Newton as described in his first paper on light and colours submitted to the Royal Society in February 1671-72.

43. Treatise on Physics, Vol. I, pp. 13-14.

44. Ibid, p. 14.

45. Except in the sense that we could know for certain what were not the causes of various effects.

46. Treatise on Physics, Vol. I, p. 14.

47. Ibid, Vol. I, p. 14.

48. Cf. Ibid, Vol. I, p. 203 ff.

49. Ibid, Vol. I, p. 12.

Chapter IV

1. I think here of R. F. Jones: Ancients and Moderns (2nd Edition, St. Louis, 1961), Benjamin Farrington: Francis Bacon, Philosopher of Industrial Science (New York, 1961), Christopher Hill: The Intellectual Origins of the English Revolution (London, 1965), and Marjorie Purver: The Royal Society: Concept and Creation (London, 1967).
2. F. R. Jones, op. cit. p. vii.
3. Benjamin Farrington, op. cit., p. 113. Cf. p. 138 where the views of other writers on Bacon are noted.
4. Henry G. Van Leeuwen: The Problem of Certainty in English Thought 1630-1690. (The Hague, 1963.)
5. Ibid, pp. 1-2.
6. Van Leeuwen can, I believe, be faulted on another count. Amongst the English scientists of the later seventeenth century there was not one problem of certainty, there were several. Further discussion of this occurs in Chapter V.
7. Of the Advancement of Learning (1605), Bk. III, 1. All references to Bacon, unless otherwise indicated, will be to The Works of Francis Bacon, edited by James Spedding, R. L. Ellis, and D. D. Heath (London, 1870). The passage quoted is in Vol. IV, p. 337.
8. The Royal Society: Concept and Creation, especially pp. 143-150.
9. Works, Vol. IV, pp. 65-66.

10. Ibid, p. 66.
11. For example, by Spedding, cf. Works, III, p. 516.
12. On this see Duane H. D. Roller: 'Did Bacon know Gilbert's De Magnete?', Isis (44) June, 1953.
13. See above, p. 35.
14. De Magnete, translated by P. Fleury Mottelay, (republished New York, 1958), p. xlvi.
15. Ibid, p. xlviii.
16. Novum Organum, Works, Vol. IV, p. 65.
17. Ibid, p. 65.
18. Of the Advancement of Learning, (Everyman edition) p. 104. This particular passage is not included in the standard edition of Bacon's Works.
19. Cf. Ibid, p. 106, Novum Organum, Works, IV, pp. 183-184, Descriptio Globi Intellectualis, Works, V, p. 511.
20. Novum Organum, Works, IV, p. 71.
21. Descriptio Globi Intellectualis, Works, V, p. 511.
22. The Great Instauration, Works, IV, p. 25.
23. Ibid, p. 25.
24. Ibid, p. 25.
25. Novum Organum, Works, IV, p. 50.
26. Novum Organum, Works, IV, p. 149.

27. For example, in The Great Instauration he wrote:-

"The sense fails in two ways. Sometimes it gives no information, sometimes it gives false information. For first, there are very many things which escape the sense, even when best disposed and no way obstructed; by reason either of the subtlety of the whole body, or the minuteness of the parts, or distance of place, or else slowness or else swiftness of motion, or familiarity of the object or other causes. And again when the sense does apprehend a thing its apprehension is not much to be relied upon. For the testimony and information of the sense has reference always to man, not to the universe; and it is a great error to assert that the sense is the measure of things." Works, IV, p. 26.

28. However, many of the early atomists were good mathematicians, for example, Thomas Hariot. On the introduction of atomism into England see especially R. J. Kargon: Atomism in England from Hariot to Newton, (Oxford, 1966).

29. De Augmentis Scientiarum, Book III, Works, IV, p. 370.

30. Ibid, p. 371.

31. De Augmentis Scientiarum, Book II, Works, IV, p. 292.

32. Ibid, p. 292.

33. Ibid, pp. 341-342.

34. Though this did not mean that natural theology, properly understood, did not have a place in man's inquiries; Bacon quite definitely held that it did.

35. The downgrading of the imagination as a key to knowledge was very much part of the seventeenth century ethos. Imagination played a central

role in such thinkers as Bruno. (On this see Francis A. Yates: The Art of Memory (London, 1966) pp. 293 ff.) But Bacon, and Descartes too, saw it as a hindrance rather than a help to knowledge.

Chapter V

1. On this topic see R. F. Merton's important work: 'Science, technology and society in XVIIth-century England'. Osiris (IV) 1938. See also Christopher Hill: Intellectual Origins of the English Revolution (London, 1965).
2. For a discussion of this see Kocher's Science and Religion in Elizabethan England, pp. 58-59, and E. A. Strathmann: Sir Walter Raleigh (New York, 1951) especially Ch. 7.
3. Bacon's attitude towards atomism has been the subject of much discussion. In his earlier writings Bacon shows that he was strongly attracted towards atomic explanations of matter, and, among the ancient philosophers, praised Democritus most highly. Cf. On Principles and Origins, Works, Vol. V, pp. 463-464. But in the New Organon he was much more cautious:

"Nor shall we thus be led to the doctrine of atoms, which implies the hypothesis of a vacuum and that of the unchangeableness of matter (both false assumptions; we shall be led only to real particles, such as really exist ..." Works, Vol. IV, p. 126.

On Bacon's atomism, or lack of it, see: C. T. Harrison: 'Bacon, Hobbes, Boyle, and the Ancient Atomists', Harvard Studies and Notes in Philology and Literature XV (1933); Maria Boas: 'The Establishment of the Mechanical Philosophy', Osiris 10 (1952); F. H. Anderson: The Philosophy of Francis Bacon (Chicago, 1948); F. R. Jones: Ancients and Moderns, especially pp. 57-58.

4. On this see W. Kargon: Atomism in England from Harriot to Newton.

5. Of Bodies. (First published in Paris, 1644. All references will be to the London edition of 1669) p. 439.
6. Of Bodies, p. 1.
7. Ibid, p. 5.
8. Ibid, p. 24.
9. Ibid, p. 60 ff.
10. There is a curious circularity about Digby's account of the loadstone because he only introduced the notion of floods of atoms moving from the north pole to the equator to explain why loadstones line up north and south. Cf. Ibid, p. 288.
11. Ibid, p. 434.
12. On the introduction of Cartesian views into England see Marjorie Nicholson: 'The Early Stages of Cartesianism in England', Studies in Philology, XXVI, (1929) pp. 356-374; and R. T. Peterson: Sir Kenelm Digby (London, 1956) Part II, Ch. 2.
13. Seven Philosophical Problems, (London, 1662) Dedication.
14. Cf. Novum Organum, Book II, VIII. Works, IV, p. 435.
15. Cf. Principles of Philosophy, II, XVI.
16. Elements of Philosophy (1655) in English Works, edited by William Molesworth (London, 1839) Vol. I, p. 414.
17. English Works, Vol. I, pp. 426-427.

18. Ibid, p. 444.
19. The Works of Sir Thomas Browne, edited by Geoffrey Keynes (London, 1964) Vol. II, p. 37.
20. Ibid, pp. 37-38.
21. Ibid, p. 38.
22. The Mathematical and Philosophical Works of John Wilkins, (London, 1802) p. 92.
23. Ibid, p. 93. It is worth noting that Bacon is not so attacked. Those who criticise Bacon for not paying enough attention to mathematics might note that even the most mathematically inclined of his followers did not see this fault in him.
24. Ibid, p. 255.
25. Mathematical Magic, Works, p. 97.
26. On this see Grant M. McColley: 'The Debt of Bishop John Wilkins to the Apologia pro Galileo of Tommass Campenella.' Annals of Science, IV (1939) pp. 150-168.
27. John Aubrey: Brief Lives, article on John Wilkins. Ed. cit., p. 369. On Wilkins' role in the foundation of the Royal Society see Margery Purver: The Royal Society: Concept and Creation, passim.
28. Experimental Philosophy. No page number.
29. Experimental Philosophy, p. 82.

30. In the Lovelace Collection of John Locke's manuscripts in the Bodleian Library is a notebook of Locke's in which he quotes Power's opinions on various people as expressed in his Experimental Philosophy. Bodleian Library Msc. John Locke f. 14.
31. The Posthumous Works of Robert Hooke, edited by Richard Waller, (London, 1705).
32. Ibid, p. 1.
33. Ibid, p. 3.
34. Ibid, p. 7.
35. In The Problem of Certainty in English Thought 1630-1690, already referred to.
36. Ibid, p. 5.
37. The Posthumous Works of Robert Hooke, p. 7
38. Van Leeuwen is probably correct to see a connection between this scepticism and the theological writings of men like Tillotson and Chillingworth. But that is a separate issue.
39. For an account of this see Jackson I. Cope: Joseph Glanvill, Anglican Apologist (St. Louis, 1956), Ch. I.
40. On this again see Cope, op. cit., Ch. VI.
41. Vanity of Dogmatizing, (London, 1661) p. 5.
42. Ibid, Preface.
43. Ibid, p. 193.

44. Ibid, pp. 189-190.
45. Ibid, pp. 189-190.
46. Ibid, p. 211.
47. Ibid, p. 212.
48. Ibid, p. 69.
49. Ibid, p. 95.
50. Ibid, p. 104.
51. Ibid, p. 73.
52. Ibid, p. 87.
53. Ibid, p. 89.
54. Ibid, p. 90.
55. Ibid, pp. 28-29.
56. Henry More held that space and time were absolute. It is possible that Newton was influenced by More on this topic, though the more likely source for his preference was his mentor Isaac Barrow. Newton edited Barrow's Geometrical Lectures: Explaining the Generation, Nature and Properties of Curve Lines (1669) which contained Barrow's views on absolute space and time. As we have already seen, Glanvill admired More's writings, as did Robert Boyle.
57. Outlined in The Immortality of the Soul, pp. 7-8.
58. In The Works of Robert Boyle, edited by Thomas Birch (1744) in six

volumes. Vol. I, p. cxxii. All references to Boyle's Works will be to this edition.

59. For a discussion of their views on innate ideas, and their connection with Locke, see John W. Yoton: John Locke and the Way of Ideas (London, 1956) especially pp. 26-48.

60. Elements of Philosophy, English Works, Vol. I, p. 85.

61. Cf. Maurice Cranston: John Locke (London, 1957), pp. 466-469.

Chapter VI

1. The Christian Virtuoso, Works, Vol. V, p. 524.
2. On this see Kargon: Atomism in England from Harriot to Newton, Ch. IX; and his 'Charleton, Boyle, and Epicurean Atomism in England, Isis, 1964 (55) pp. 190-191. For a different view see Maria Boas: 'The Establishment of the Mechanical Philosophy', Osiris 1952 (10) p. 461.
3. Certain Physiological Essays, Works, Vol. I, p. 302.
4. Published in Annals of Science, 1956, Part I, pp. 63-73, Part II, pp. 103-117.
5. Ibid, p. 110.
6. Works, Vol. I, p. 591.
7. Westfall, op. cit., p. 108.
8. These manuscripts are in the Royal Society collection of Boyle papers.
9. Westfall, op. cit., quotes these in full, pp. 116-117.
10. A. C. Crombie, in Augustine to Galileo writes (Vol. II, pp. 295-296):
"The only scientist of the period who saw himself as a complete Baconian was Boyle: 'designed by Nature to succeed' to the fame of the great Verulam as the Spectator described him in 1712."

This verdict on Boyle does not sufficiently indicate the scope of his speculative thinking, although it is certainly true that Boyle was much less speculative than the scientists of the Cartesian tradition.
11. Works, Vol. II, p. 45.

12. Cf. The Second Part of the Christian Virtuoso. Not published until the collected Works. Works, Vol. VI, pp. 722-723.
13. Works, Vol. II, p. 45.
14. Ibid, p. 46.
15. His Free Inquiry into the Vulgarly Received Notion of Nature, Works, Vol. V, pp. 158-254.
16. Works, Vol. III, p. 439.
17. Cf. The Usefulness of Mathematics to Natural Philosophy, Works, Vol. III, pp. 428 and 431. Galileo had impressed Boyle as a young man.

Boyle wrote in his autobiography that on his visit to Italy in 1641-42 he read:

"the new paradoxes of the great star-gazer, Galileo whose ingenious books, perhaps because they could not be so otherwise, were confuted by a decree from Rome; his highness the Pope, it seems presuming, and that justly, that the infallibility of his chair extended equally to determining points in philosophy as in religion, and loth to have the stability of that earth questioned, in which he had established his kingdom." Works, Vol. I, p. xxiv.

The Pope's decree apparently did not prevent the circulation of Galileo's works in Florence.

18. Works, Vol. V, p. 521.
19. Works, Vol. V, p. 139.
20. Works, Vol. V, p. 140.
21. Ibid, p. 140.
22. Works, Vol. IV, p. 454.
23. Ibid, pp. 460-461.

24. Cf. for example, the obvious influence of Descartes in his discussion of ideas and the imagination in A Discourse of Things above Reason, Works, Vol. IV, p. 415.
25. Works, Vol. III, p. 9.
26. Ibid, p. 15.
27. Ibid, p. 15.
28. Ibid, p. 15.
29. Ibid, p. 16.
30. Ibid, pp. 18-19.
31. Ibid, p. 20.
32. Ibid, p. 23.
33. Ibid, p. 24-25.
34. Ibid, p. 38.
35. Ibid., p. 38.
36. Ibid, p. 47.
37. Ibid, pp. 49-50.
38. Ibid, p. 50.
39. This correspondence is now most easily available in The Correspondence of Henry Oldenburg, edited by A. R. Halls, and M. Boas Hall (Madison and Milwaukee, 1965.) The relevant letters between Oldenburg and Spinoza were written between 1661 and 1663. For an interesting discussion of this topic see A. R. Hall and M. B. Hall: "Philosophy and Natural Philosophy: Boyle and Spinoza", Melanges Alexandre Koyré, Vol 2.

Chapter VII

1. Newton's Preface to the first edition of the Principia. Ed. cit., Vol. I, p. xvii.
2. The Philosophical Transactions of the Royal Society (80) 1671-2, pp. 3075-3087. Reprinted in: Isaac Newton's Papers and Letters on Natural Philosophy, edited by I. B. Cohen, (Cambridge, 1958) pp. 47-59.
3. It was Hooke who was least well treated by Newton in this controversy, and not for the last time; they were to clash later over the priority of the inverse square law. Cf. T. S. Kuhn's introduction to Newton's optical papers in Isaac Newton's Papers and Letters on Natural Philosophy, especially pp. 35-40. On the later controversy see A. Koyré: 'An Unpublished Letter of Robert Hooke to Isaac Newton', Isis (43) 1952.
4. Philosophical Transactions of the Royal Society, (85), p. 5014. Translated and published in Isaac Newton's Papers and Letters on Natural Philosophy, p. 106.
5. Published in Newton's Correspondence, Vol. I, pp. 187-188.
6. Principia, ed. cit., Vol. II, p. 547.
7. Ibid, p. 547.
8. Ibid, p. 398.
9. Ibid, p. 398.
10. In fact theology was never far away from Newton's science. Thus in the 28th Query to the Opticks Newton said that there were a vast number of phenomena which argued the intervention of God in the universe. Indeed, Newton actually

invoked his rejection of hypotheses as a part of the justification for inferring God's existence from natural phenomena! He concluded:

"And though every true step made in this Philosophy brings us not immediately to the knowledge of the first Cause, yet in bridges us nearer to it, and on that account is to be highly valued." Opticks, reprint of the fourth edition, ed. cit., p. 370.

11. Ibid, p. 398
12. Ibid, p. 400.
13. See Koyré's 'Pour une édition critique des oeuvres de Newton', Revue d'Histoire de Sciences 8, (1955) pp. 19-37; his 'Newton's Regulae Philosophandi', first published in Archives Internationales d'Histoire des Sciences 13 (1960) pp. 3-14, and reprinted in Newtonian Studies (London, 1965) pp. 261-272; and his 'Concept and Experience in Newton's Scientific Thought', originally published as 'L'Hypothèse et L'expérience chez Newton', Bulletin de la Société Française de Philosophie 50 (1956) pp. 59-79, and reprinted in Newtonian Studies, pp. 25-52.
14. See especially 'Concept and Experience in Newton's Scientific Thought', *passim*.
15. J. Edleston (ed.): Correspondence of Sir Isaac Newton and Professor Cotes (London, 1850) pp. 154-156.
16. Opticks (London, 1704) p. 1.
17. Opticks, reprint of the fourth edition, ed. cit., pp. 404-405.
18. Ibid, p. 405.
19. Principle XLIV of Part III of the Principles of Philosophy reads:

"That I yet merely desire to assert that those (causes) which I set forth are to be regarded as hypotheses." Principle XLV reads:

"That I shall also here assume some propositions which are agreed to be false." And Principle XLVII reads: "That the falsity of these propositions does not prevent what may be deduced from them from being true and certain."

Where or not Descartes really believed that he had assumed false hypotheses is a matter of some doubt.

20. System of Natural Philosophy, ed. cit., Vol. I, p. 13.

21. The Leibniz-Clarke Correspondence, ed. cit., p. 94.

22. Ibid, p. 118.

23. Correspondence, Vol. II, p. 295.

24. Query 21, Opticks, fourth edition, ed. cit., p. 350. On this topic

Henry Guerlac has suggested that Newton was encouraged towards an acceptance of the ether especially by the electrical experiments of Francis Hauksbee.

See his: 'Sir Isaac Newton and the Ingenious Mr. Hauksbee', Melanges

Alexandre Koyré: Vol. 2, pp. 228-253; and "Francis Hauksbee, experiment-

ateur au profit de Newton', Archives Internationales d'Histoire des Sciences,

16 (1963) pp. 113-128.

25. Opticks, fourth edition, ed. cit., p. 405. Newton's reference to the light of Nature as the course of moral knowledge is an indication that he, like St. Paul, believed that we have a natural knowledge of what is right and wrong.

Cf. Romans 2 14:

"When Gentiles who do not possess the law carry out its precepts by the light of nature, then, although they have no law they are their own law, for they display the effect of the law inscribed on their hearts." (The New English Bible translation.)

It is possible that Newton never really accepted the full implications for morality that are implicit in Locke's rejection of innate ideas. In September 1693, at a time of great emotional and intellectual crisis in Newton's life, he wrote to Locke:-

"Being of opinion that you endeavoured to embroil me with woemen & by other means I was so much affected with it as that when one told me you were sickly and would not live I answered twere better if you were dead. I desire you to forgive me this uncharitableness. For I am now satisfied that what you have done is just & I beg your pardon for my having hard thoughts of you for it & for representing that you struck at ye root of morality in a principle you laid down in your book of Ideas (i. e. the Essay Concerning Human Understanding) & designed to pursue in another book & that I took you for a Hobbist. I beg your pardon also for saying or thinking that there was a designe to sell me an office, or to embroil me." (Correspondence, Vol. III, p. 280).

The principle to which Newton had taken exception was almost certainly Locke's rejection of innate ideas, and with it the rejection of a common morality known to all men innately. This is, however, an inference. Apparently Locke himself was not completely clear about Newton's meaning; cf. Newton's letter to Locke October 1693, Correspondence, Vol. III, p. 284. Certainly Newton appears to have rejected innate ideas in science. Cf. Note 37 below. The expression 'the light of nature' was a commonplace in seventeenth century moral writings. It was indeed used more than once by Locke. Thus, in his Common-Place Book he wrote:

"A ploughman that cannot read, is no so ignorant but he has a conscience, and knows in those few cases which concern his own actions, what is right and what is wrong. Let him sincerely obey this light of nature, it is the transcript of the moral law in the Gospel; and this, even though

there be errors in it, will lead him into all the truths in the Gospel that are necessary for him to know." Lord King: Life and Letters of John Locke, new edition (London, 1858). pp. 283-284.

See also Locke's Essays on the Law of Nature, edited by W. Von Leyden, (Oxford 1954, reprinted, 1958) p. 123 ff.

26. Four Letters from Sir Isaac Newton to Dr. Bentley containing some Arguments in Proof of a Deity. (London, 1756) Reprinted in Isaac Newton's Papers and Letters On Natural Philosophy, p. 280.

27. See the letters to Bentley, ibid, passim.

28. The point is that if we accept that there is design in the universe, then we must necessarily accept a designer, so the crucial issue is whether or not the universe exhibits design, and not, given design, there is - or was - a designer.

29. See Newton's manuscript: "A Short Scheme of the True Religion", printed in Sir David Brewster's Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton, (Edinburgh, 1850) Two volumes, Vol. II, pp. 347-348.

30. Isaac Newton's Papers and Letters on Natural Philosophy, p. 298.

31. Principia, ed. cit. Vol. II, p. 398.

32. Ibid, p. 398.

33. Ibid, p. 399.

34. Fourth edition, ed, cit., p. 400.

35. Ibid, p. 400.

36. Berkeley's attack on dualism was directed at Locke rather than Newton. It is only in the Queries that Newton appears uncompromisingly committed to dualism and these were not published in full until 1717, seven years after Berkeley's Principles of Human Knowledge.

37. Some evidence to indicate Locke's influence on Newton is to be found in the wording of the projected (and rejected) Rule V of the Rules of Reasoning in Philosophy, which was never included in Newton's published works but which survives in manuscript. Attention to this Rule has been drawn by Koyré in 'Newton's "Regulae Philosophandi"', Newtonian Studies, pp. 261-272. In Koyré's translation the Rule reads:

"Whatever is not derived from things themselves, whether by the external senses or by the sensation of internal thoughts, is to be taken for a hypothesis. Thus I sense that I am thinking, which could not happen unless at the same time I were to sense that I am. But I do not sense that any idea whatsoever may be innate. And I do not take for a phenomenon only that which is made known to us by the five external senses, but also that which we contemplate in our minds when thinking: such as, I am, I believe, I understand, I remember, I think, I wish, I am unwilling, I am thirsty, I am hungry, I rejoice, I suffer, etc. And those things which neither can be demonstrated from the phenomenon nor follow from it by the argument of induction, I hold as hypotheses." (Newtonian Studies, p. 272.)

The rejection of innate ideas by Newton was entirely Lockian, as was the acceptance of ideas of sensation and what Locke called ideas of reflection, i.e. ideas in our minds "when thinking". Newton certainly read Locke's Essay; see Note 25 above.

38. On this see E. A. Burtt: The Metaphysical Foundations of Modern Physical Science, pp. 230-237.

39. Berkeley's system required - as also did Leibniz's - that standard examples of causation in Newtonian physics be treated only as constant conjunctions. Hume's analysis of causation was most ably presented in An Inquiry Concerning Human Understanding, Section VII.

40. Duration, Descartes held, was simply a mode of existence. (Principles of Philosophy Part I, Principle LV. Haldane and Ross, Vol. I, p. 241). On space Descartes wrote: "And hence if we say that a thing is in a particular place, we simply mean that it is situated in a certain manner in reference to certain other things" Principles of Philosophy, Part II Principle XIV. Haldane and Ross, Vol. I, p. 261.

41. Locke's views on space and its relational nature are to be found in Book II Chapter XIII of the Essay. His conception of time is to be found in the following chapter.

42. See The Leibniz-Clarke Correspondence, ed. cit., especially pp. 25-27, 31-33, 36-42, 66-78, and 81 ff.

Chapter VIII

1. Cf. Kenneth Dewhurst: John Locke: Physician and Philosopher, (London, 1963) p. vii.
2. Ibid, Ch. II, especially pp. 38-40.
3. Cf. M. Cranston: John Locke, a Biography, (London, 1957), p. 117.
4. Besides Dewhurst's book, the following articles are also relevant:
E. T. Withington: 'John Locke as a Medical Practitioner', Medical Magazine (1898) No. 7, pp. 47-50, 375-387, 573-579; Janus (1899) No. 4, pp. 393-398, 457-463, 527-532, 579-587; Sir William Osler: An Alabama Student and Other Biographical Essays, (Oxford, 1926), reprinted from a special issue of the Lancet, ii, 10, 20th October 1900, entitled 'John Locke as a Physician'.
See also the articles by Dewhurst listed on pp. viii-ix of his book.
5. Cf. Dewhurst, pp. 11-12.
6. R. M. Yost, Jr.: "Sydenham's Philosophy of Science", Osiris, IX, (1950), p. 84.
7. The Works of Thomas Sydenham, M. D., translated from the Latin edition of Dr. Greenhill by R. G. Latham, M. D., 2 volumes, (London, 1848), Vol. I, p. 15. Quoted in Yost, op. cit., p. 91.
8. Familiar Letters between Mr. John Locke and Several of his Friends, (1742), 4th edition, p. 278. Quoted in Dewhurst, op.cit., p. 309.
9. It is to be remembered that Newton's most famous rejection of hypotheses, "Hypotheses non fingo", occurs for the first time in the General

Scholium of the second edition of the Principia of 1713. The first edition was not nearly so explicit in its condemnation.

10. Familiar Letters, pp. 223-224, 20th January, 1692/3. Quoted in Dewhurst, op. cit., pp. 310-311
11. Cf. Dewhurst, Chapter X.
12. The recognition by Locke of the limitation of the logical force of analogical arguments was a point of considerable significance for the true recognition of the possibilities of certainty in the natural sciences. This recognition was to come much later in the social sciences; thus Emile Durkheim, in 1898, pointed out both the use and limitations of analogical arguments in social investigations in his 'Individual and Collective Representation', now published in Sociology and Philosophy (London, 1953), pp. 1-2. Clearly, an understanding of the logic of analogical arguments is central to the successful employment of models both in the natural and social sciences.
13. Dewhurst, op. cit., p. 311.
14. Douglas Guthrie: A History of Medicine (London, 1945), p. 203.
15. G. H. Tumbull: 'Peter Stahl, the first public teacher of chemistry at Oxford', Annals of Science (1953), 9, pp. 265-270.
16. The Life and Times of Anthony Wood (1632-95), Vol. I, p. 472.
17. Op.cit., p. 10.
18. Ibid, pp. 19-20.
19. Ibid, p. 20.

20. Bernier's Abregé was published at Lyon in 1678 (eight volumes).
Locke's library catalogue of 1697 shows that Locke then owned Bernier's work. Bodleian Library MSS., Locke e. 3.
21. M. Cranston, op. cit., pp. 169-170.
22. The letter is now published in Newton's Correspondence, Vol. III, p. 215.
23. Sir David Brewster: Memoirs of the life, writings and discoveries of Sir Isaac Newton, ed. cit., Vol. I, p. 339.
24. Cf. Lois Trenchard More: Isaac Newton, (New York, 1934), p. 299.
25. Ed. Cit., Vol. II, p. 392.
26. On this see H. R. Fox Bourne: The Life of John Locke, (London, 1876) Two Volumes, Vol. I, p. 44 ff.
27. Ibid, p. 55.
28. The entry reads:-

"Mathematica The 3rd Volume of Mersennes is worth the having because therein he has joined mathematics and physics and has many experiments. But his other two volumes are not worth reading not because they are not good but Vorellus in the same kind is much better. There are also many experiments in Schottus who was a good mathematician. E. Pembroke.

Mathematica The best Trigonometry is V. Lack on 8^o. And the best mechanicks is Descartes. Dr. Wallis is too long. E. Pembroke."
Bodleian Library MSS., John Locke, c28 fol. 114.

It is interesting that as late as 1693 Descartes was apparently preferred to Newton for his mechanics.

29. First published in 1693, and collected from a series of letters Locke had written from Holland to Edmund Clarke of Chipley about the education of Clarke's son. Reprinted in Works (1768), Vol. IV, pp. 135 ff. References will be to this edition.
30. Works, IV, p. 113.
31. Ibid, p. 114.
32. Ibid, p. 580.
33. Ibid, p. 581.
34. Ibid, p. 581.
35. Bodleian Library MS. John Locke c.31, f.101. The manuscript is printed in Lord King's biography of Locke, and is now republished in Newton's Correspondence, Vol. III, pp. 71-76. For a discussion of the significance of this manuscript see John Herivel: The Background to Newton's Principia, (Oxford, 1965), pp. 108-117.
39. Ibid, p. 18.
40. Ibid, p. 18.
41. Cf. Dewhurst, op. cit., p.7.
42. Cf. my 'Boyle, Locke, and Reason', Journal of the History of Ideas, Vol. XXVII, (1966) pp. 206-207.
43. Cf. W. von Leyden (ed): John Locke. Essays on the Law of Nature, Introduction, p. 20.
44. Bodleian Library MS, John Locke, e.3.

45. Cf. J. Gibson: Locke's Theory of Knowledge and its Historical Relations, pp. 250-254.
26. Cf. Note 40 to the Introduction.
47. Thomas Birch: History of the Royal Society of London in Four Volumes (1756), Vol. II, p. 406.
48. Ibid, Vol. II, p. 497.
49. Ibid, Vol. III, p. 64.
50. The Diary of John Evelyn Esq., F.R.S., 24th October, 1672.
51. Royal Society Additional MSS. 4441. Published in C. R. Weld: A History of the Royal Society with Memoirs of the Presidents, two volumes, (London, 1848), Vol. I, pp. 146-147.
52. Cf. Chapter VII, pp. 157-158.
53. Bodleian Library MS. John Locke, fol. 14.
54. Bodleian Library MS. John Locke, b.2 fol. 85.
55. MS. in the Remonstrants's Library, Amsterdam. Lady Masham to Le Clerc, 12th January, 1704/5. Quoted in Fox Bourne, op. cit., Vol. I, pp. 61-62, and M. Cranston, op. cit., p. 100.
56. On Locke's debt to Descartes see especially Gibson, op. cit., Chapter IX.
57. Fox Bourne, op. cit., Vol. I, p. 91.
58. Cf. John Lough (ed.): Locke's Travels in France. As Related in His

Journals, Correspondence, and other papers. (Cambridge, 1953), Note 1 on p. 177. It is also worth noting that the only work of Gassendi that Locke owned was Viri Illustris Nicolai Claudii Fabricii de Peiresc, which, as the title indicates, was not one of Gassendi's scientific works. For a brief discussion of Gassendi's influence on Locke see Aaron: John Locke, pp. 31-35.

Chapter IX

1. In many editions of the Essay, Chapter I is what Yolton in his edition identifies as Chapter II.
2. Essay, II I 2. All future references to the Essay will simply list Book, Chapter, and section number.
3. In the first draft of the Essay there is only the scantiest attention paid to the problem of innate ideas. There is a much fuller discussion in the second draft, and, of course, the published work contains a still longer account, suggesting that during the course of the Essay's production Locke came to see this as a much more important question than it at first appeared. The first draft of Locke's Essay is published as: An Early Draft of Locke's Essay Together with Excerpts from his Journals, ed. R. I. Aaron and J. Gibb, 'Draft A'. (Oxford, 1936). The second draft is published as An Essay Concerning the Understanding, Knowledge, Opinion, and Assent, ed. Benjamin Rand, 'Draft B'. (Harvard, 1931).
4. John W. Yolton: John Locke and the Way of Ideas, (Oxford, 1956), Chapter 2.
5. I IV 25.
6. Although I do not intend in this work to consider the connection between Locke's Essay and his politics, many important links do exist, and I believe it to be a grave error in assessing Locke either to construe these two areas as being either unconnected or, that they are inconsistent in a fundamental way.

Cf. Peter Laslett: Introduction to his edition of the Two Treatises of Government (Cambridge, 1964) pp. 79 ff. where Laslett makes much of the supposed inconsistencies between the Treatises and the Essay without any attempt to suggest positive links.

7. I II 3.

8. J. Edleston: Correspondence of Sir Isaac Newton and Professor Cotes, p. 155.

9. I III 9.

10. I IV 8.

11. I II 5.

12. I II 5.

13. Cf. Principles of Philosophy, Principle IX: "By the word thought I understand that of which we are conscious as operating in us." Haldane and Ross, Vol. I, p. 222.

14. I II 22.

15. I II 23.

16. Meditation III, Haldane and Ross, Vol. I, p. 160.

17. Haldane and Ross, Vol. I, p. 433.

18. Ibid, p. 442.

19. Most clearly expressed in The Discourse on the Method, Part IV, Haldane and Ross, Vol. I, pp. 102-103.

20. New Essays Concerning Human Understanding Book IV, Chapter XI.

The Open Court edition, translated by Alfred G. Langley, (La Salle, 1949), pp. 516-517.

21. Arthur Pap; Semantics and Necessary Truth, (Yale, 1958), p. 22.

22. I. Kant: Critique of Pure Reason, Introduction II.

23. I I 2.

24. I refer to the following passage in Lewis Carroll's Through the Looking Glass:

" "I don't know what you mean by 'glory'," Alice said. Humpty Dumpty smiled contemptuously. "Of course you don't - till I tell you. I meant 'there's a nice knockdown argument for you!'" "

"But 'glory' doesn't mean 'a nice knockdown argument'," Alice objected.

"When I use a word", Humpty Dumpty said, in rather a scornful tone, "it means just what I choose it to mean - neither more nor less."

"The question is", said Alice, "whether you can make words mean so many different things."

"The question is", said Humpty Dumpty, "which is to be master - that's all."

25. I IV 26.

26. I IV 26.

27. I IV 26.

28. I IV 26.

Chapter X

1. I I 8.
2. I I 8.
3. II I 2.
4. II I 10. Cf. II I 21.
5. II VI 1.
6. This is not to say that Locke was unaware of the possibility of a man not seeing the rose or not hearing the bird even though the one was before his eyes and the other singing only some few yards away. Cf. II IX 4.
7. II I 25.
8. Cf. II XI 11.
9. See on this: Aaron's John Locke, especially p. 107 ff., and D. J. O'Connor's John Locke, (Dover edition 1967) p. 47 ff.
10. II II 1.
11. II II 2.
12. An Enquiry concerning Human Understanding, Section II.
13. II II 2.
14. Query 31.
15. Op. Cit., Works, Vol. I, p. 562.
16. Ibid, p. 493.
17. IV III 26.

18. The most obvious alternative to an atomic theory of experience is a gestalt theory.
19. As a general characterization of Locke's account of language this will do, but it would be wrong to think that this was all there was to Locke's theory of meaning, thus his chapter 'Of Particles', (Essay III VII) might seriously be used as evidence that Locke held similar views about meaning to those of the late J. L. Austin.
20. II VIII 11 and 12.
21. IV XI 2.
22. General Scholium to the Principia, ed. cit., Vol. II, p. 547.
23. Objection V. Haldane and Ross, Vol. II, p. 200. The quotation is a misquotation from Lucretius.
24. II XXIII 26.
25. Cf. Galileo's account of the strength of materials in Two New Sciences, ed. cit., p. 11.
26. II XXIII 28.
27. Works, ed. cit., Vol. I, p. 754.
28. II XXVI 1.
29. Fraser makes the point in his edition of the Essay, Vol. I, p. 433. Hume makes his in the Treatise of Human Nature, Book I, III, ii.
30. II XXVI 1.
31. II XXI 1.

32. Cf. IV VIII 4.

33. IV I 2.

34. II XXVI 1.

35. Works, Vol. I, p. 755.

36. Cf. William Ockham: Predestination, God's Foreknowledge, and Future Contingents, translated into English by Marilyn M. Adams and Norman Kretzmann, (New York, 1969), passim. Cf., also, Frederick Copleston: A History of Philosophy, Vol. III, pp. 43-95, but especially pp. 94-95.

37. II IX 3.

38. Cf. Siris 303:

"There runs a Chain throughout the whole system of beings. In this Chain one link drags another. The meanest things are connected with the highest."

Interestingly, Arthur Lovejoy in his classic work The Great Chain of Being (Harvard, 1936) did not mention Berkeley in connection with the theory, though he gave quite a lot of attention to Locke.

39. III VI 12.

40. III VI 12.

41. IV XI 8.

42. II IX 12.

43. IV XI 8.

44. The Argument from the Paradigm Case (APC) might be expressed, aphoristically, in the following way: (1) When a word is correctly used to identify a situation then that situation must exist at the time of the identification. (2) If in standard cases of its use, such as when under normal conditions an English pillar box is described as red, then anybody who challenges that the word is correctly used could not allow that the word could ever be used correctly for such uses are, paradigmatically, correct uses. (3) Therefore, it must be correct that pillar boxes are red, because 'red' is the word whose function is to identify the colour of such objects as pillar boxes.

Discussions of the APC has in the recent past been very wide. Some of the key references are: Norman Malcolm: "Moore and Ordinary Language", in Paul Arthur Schilpp, ed. The Philosophy of G. E. Moore, (Evanston and Chicago, 1942); J. W. N. Watkins: "Farewell to the Paradigm-Case Argument", Analysis, 18 (December, 1957); A. G. N. Flew: "'Farewell to the Paradigm-Case Argument' A Comment", Analysis 18, (December 1957); and several subsequent articles in Analysis; Robert J. Rickman: "On the Argument of the Paradigm Case", Australasian Journal of Philosophy, 39 (1961); Antony Flew: "Again the Paradigm", in Paul K. Feyerabend and Grover Maxwell eds. Mind, Matter, and Method. Essays in Philosophy and Science in Honor of Herbert Feigl, (Minneapolis, 1966).

45. II IV 4.

46. A Treatise of Human Nature, Book I Part IV Section II.
47. An Essay Concerning Human Understanding, edited and abridged with an introduction by A. D. Woozley. Fontana Collins, (London, 1964).
48. Ibid, pp. 24-35.
49. Ibid, p. 28.
50. III I 2.
51. III I 2.
52. It is here worth remembering the points made by G. E. Moore in his paper 'Proof of an External World', Proceedings of the British Academy, Vol. XXV, 1939. Republished in Moore's Philosophical Papers, 1959, pp. 127-150. It is especially worth remembering Moore's famous proof of a world external to himself when he held up his hands and said: "Here is one hand" and "here is another", and thereby claimed to have proved that external things, and therefore an external world, exists. It is interesting to remark that Locke and Moore have much in common, both as regard their common sense approach to philosophy and their particular epistemological theories. Similar comparisons can also be made with the views put forward by Bertrand Russell in Chapter II of his My Philosophical Development, (London, 1959).

Chapter XI

1. II XXIII 3.
2. II XXIII 6.
3. II XXIII 2.
4. Works, Vol. I, p. 744.
5. Ibid, p. 745.
6. II XXI 73. See also the following passages in the Essay: II IV 4, II VIII 13-17, II XXIII 11, II XXXI 6, III VI 6, IV III 16, IV X 10.
7. Works, Vol. IV, p. 21.
8. IV III 16.
9. IV III 25.
10. W. D. Joske: Material Objects (London, 1967) pp. 14-15. The offending passage cited by Joske is Essay II IV 1.
11. II XXIII 3.
12. See Essay II XI 9.
13. II XXIII 3.
14. II XXIII 9.
15. Ibid.
16. Ibid.
17. II XXIII 11.
18. Quoted in A. C. Fraser's edition of the Essay, Vol. I, p. 403.
19. II I 2.

20. III II 2.
21. Cf. III III 1-5.
22. III III 6.
23. Principles of Human Knowledge, Introduction, especially II ff.
24. III III 9.
25. III III 12.
26. III III 13.
27. III III 14.
28. III III 15.
29. III III 17.
30. III III 17.
31. III VI 6.
32. III VI 5.
33. It would be interesting, but in this context not appropriate, to compare what Locke had to say about nominal essences with the remarks of Wittgenstein in Philosophical Investigations about language games. A case could be made to show that Locke anticipated some of Wittgenstein's ideas. Cf. Philosophical Investigations 65 and 116, as examples of passages which share some of the spirit of Locke's remarks.
34. III VI 36.
35. II VIII 22.
36. Quoted above, Chapter III, p. 84.

37. II VIII 9.
38. Locke acknowledged that solidity was inseparable from extension in II XIII 11.
39. II XIII 11.
40. II XIII 12-14.
41. It is perhaps worth noting again a similarity between Locke and G. E. Moore. This is true in a general sense but also in particular. Thus Moore wrote in his paper 'A Defence of Common Sense' (1925):

"In what I have said, I have assumed that there is some meaning which is the ordinary or popular meaning of such expressions as 'The earth has existed for many years past'." Philosophical Papers, p. 36.
42. Principia, ed. cit. p. 398.
43. Ibid, p. 399.
44. Ibid, p. 400.
45. Ibid, p. 398.
46. Cf. Koyré: Newtonian Studies, pp. 149-163, and 261-272.
47. The Assayer. Discoveries and Opinions of Galileo, p. 277.
48. II XXIII 9.
49. IV VIII 14.
50. Principles of Human Knowledge, Section 20.
51. II XXIII 9.

Chapter XII

1. IV I 1.
2. IV I 2.
3. IV I 2.
4. IV I 4.
5. IV VII 3.
6. Works, Vol. IV, p. 122.
7. Newton himself clearly distinguished in the Pri ncipia between mathematics and physics. Thus, at the beginning of Book Three he wrote:-

"In the preceding books I have laid down the principles of philosophy; principles not philosophical but mathematical". (ed. cit. p. 397).
8. IV II 14.
9. IV II 2.
10. IV II 14.
11. IV III 2.
12. Cf. Chapter XI, p. 299-300 above.
13. III III 18.
14. IV II 9.
15. Cf. A. C. Fraser's edition of the Essay, Vol. II, p. 182, Note 3.
16. Bodleian Library MSS. Lovelace Collection Locke e. 3 is a catalogue of Locke's library in 1697 and in it is entered "Philosophical Transactions (of the Royal Society) Vols. 1-19." Newton's paper appears in Vol. 7.

17. IV II 10.

18. IV II 11.

19. IV II 11.

20. IV II 11.

21. IV II 13.

22. It is worth comparing Locke's remarks here with Newton's comments in the science of colours quoted above, p. 184.

23. IV II 14.

24. IV III 9.

25. IV III 10.

26. IV III 11.

27. IV III 12.

28. IV III 14. Also see IV XII 9.

29. IV III 16.

30. IV III 25.

31. IV III 26.

32. IV III 29.

33. IV XII 10.

34. III VI 12.

35. IV XII 11.

36. IV XII 12. Locke's use of "knowledge" in the last sentence is an example of how he slips from consistency with his own definition.

37. IV III 29.
38. IV III 25.
39. IV IV 3.
40. IV IV 12.
41. IV VI 16.
42. IV XII 13.
43. Cf. Chapter IV, p. 88 ff.
44. Cf. Chapter VIII, p. 187.
45. On Locke's possible connection with the production of the English edition of the Art of Thinking see Charles W. Hendel's Forward to the Bobbs Merrill edition of The Art of Thinking, (New York, 1964), p. XIX. See also John Maynard Keynes: A Treatise of Probability (London, 1921, reprinted 1948) p. 80 and p. 308, for a brief discussion of Locke's debt to the Port Royal Logicians.
46. Cf. his definition of probability, as offered in IV XV 1.
47. IV XV 3 and 4.
48. IV XV 4.
49. IV XV 5.
50. IV XV 6. Compare J. P. Day's comments on the connection between "probable" and "believe" in Inductive Probability (London, 1961) p. 31 ff.
51. IV XVI 12.
52. IV XVI 12.

53. IV XVI 12. This last passage would alone seem sufficient to show that R. M. Yost Jr. is wrong to claim that

"Locke did not believe that the employment of hypotheses about sub-microscopic events would accelerate the acquisition of empirical knowledge." Cf. 'Locke's Rejection of Hypotheses About Sub-Microscopic Events', Journal of the History of Ideas, 12, (1951), p. 111.

Yost fails to see the distinction which Locke made between the usefulness of conjectures and our ability to be absolutely certain of their truth. It was the latter which Locke denied, not the former.

54. For a further discussion of this and the importance of Locke's insight see Mary B. Hesse: Forces and Fields (London, 1961), pp. 121-125.

55. Thus the following passages of Book IV all, in different ways, emphasise this lack of knowledge: II 11 ff., III 5 ff., III 9 ff., III 24 ff., IV 11 ff., V 8, VI 5-16, VII 12-14, IX 13, XII 9-15.

56. Works, Vol. IV, p. 121.

57. IV XIX 1.

58. Lord King: The Life and Letters of John Locke, New edition, (London, 1858) pp. 121-122.

59. IV 10 2-6.

60. King: Life and Letters, p. 314.

61. Ibid, p. 316.

62. Works, Vol. IV, p. 638.

63. Works, Vol. III, pp. 84-85.

64. See on this the letters exchanged between Newton and Locke in Newton's Correspondence, Vol. III, letters numbered 355, 358, 359, 360. Locke had a considerable admiration for Newton as a theologian.
65. The Reasonableness of Christianity, Works, Vol. III p. 85.
66. Cf. Works, Vol. IV p. 638.
67. IV III 18.
68. Bodleian Library MS. John Locke, c. 28 f. 114, 139, 141. See also J. W. Gough: John Locke's Political Philosophy, (Oxford 1950, reprinted 1956) Ch. I.
69. Good accounts of Locke's moral philosophy are to be found in Sterling P. Lamprecht: The Moral and Political Philosophy of John Locke, (reissued, New York 1962), and in the introduction to John Locke: Essays on the Law of Nature, (Oxford, 1954, reprinted 1958) by W. von Leyden.
70. Bodleian Library MS. John Locke, c. 28 f. 139.
71. II XXI 42.
72. Bodleian Library MS. John Locke, c. 28 f. 139.
73. Lord King: Life of Locke, p. 306.
74. II XXI 41.
75. G. Berkeley: Complete Works, edited by A. C. Fraser, (Oxford, 1901) 4 Vols. Vol. I, p. 39.

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The list below is limited to those works which have been referred to either in the text or in the notes with the exception of some of Locke's works which I have listed although I have not actually referred to them. The list is divided into two parts: Primary and Secondary sources. Some works might well be included in both lists but I have preferred to have only one entry for each work. All the books are published in London unless otherwise stated. Articles as well as books are included. The name for the Journal for the History of Ideas is abbreviated throughout to "J.H.I."

The most important manuscript source for work on John Locke is the Lovelace Collection in the Bodleian Library. A catalogue of that collection has been prepared by P. Long: A Summary Catalogue of the Lovelace Collection of the papers of John Locke in the Bodleian Library, (Oxford Bibliographical Society Publication, New Series, vol.8, Oxford 1959.) There is also a very useful list of the manuscripts in the Lovelace Collection dealing with philosophical subjects in the bibliography of John Locke by R. I. Aaron, (Oxford, 1955) pp.309-310.

Until recently the main bibliographical source for the study of Locke was H. O. Christopherson: A Bibliographical Introduction to the Study of John Locke (Oslo 1930). However, in July 1970, Roland Hall and Roger Woolhouse published "Forty Years of Work on John Locke (1929 - 1969) A Bibliography," The Philosophical Quarterly Vol.20 No. 80, July 1970, and later: "Addenda to 'Forty Years of Work on John Locke'", The Philosophical Quarterly, Vol. 20, No. 81, October 1970.

These form an important supplement to the Christopherson work. The best brief bibliography to Locke is that in Aaron's book already mentioned.

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