Philosophy and science in Berkeley's de motu

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ABSTRACT

My main intention in writing this thesis is to give a greater insight into Berkeley's philosophical concerns with eighteenth century science as expressed in *de Motu*. I have written this thesis in four parts, covering the scientific background, the philosohical background, an exposition of *de Motu* itself, and a more modern treatment of *de Motu*.

In the scientific background I cover such topics as the relation between Descartes and Newton, Newton's achievement, Newton's position regarding such matters as the status of gravity and motion.

In the philosophical background I discuss such matters as Berkeley's sources, the nature of Occasionalism, Occasionalism's alternative to realism, and Berkeley's divergence from Malebranche.

In the third section I deal with Berkeley's anti-realist construal of scientific language, his treatment of absolute space, his attack on Descartes causal paradigm, the meaning of scientific terms, the connection between explanation and realism (if any), and between cause and explanation.

In the fourth section I compare and contrast Berkeley and Popper, discuss instrumentalism and its relation to explanation, Popper's muddled beliefs about Berkeley's philosophy of language and its implications for Berkeley's philosophy of science. I also discuss such topics as Berkeley's conception of explanation, the difference between Berkeley's and Popper's realism; and the the rôle of the scientific test. I will also defend instrumentalists and Berkeley from the charge that their view of scientific theories reduces them to mere computation rules.

PHILOSOPHY AND SCIENCE

IN

BERKELEY'S *DE MOTU*

by

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CONTENTS

	Page
Copyright Statement	6
Introduction	8

The Scientific Background

Section 1:	Introduction	12
Section 2:	Descartes and Newton	13
Section 3:	Newton's Achievement	15
Section 4:	Criticisms of Newton	17
Section 5:	Newton's Position (i)	20
Section 6:	Newton's Position (ii)	24
Section 7:	Newton's Position (iii)	28
Section 8:	Atheism Vanquished	33

The Philosophical Background

Section 1:	Introduction	38
Section 2:	Berkeley's Sources	39
Section 3:	Method: On Being Clear and Distinct	45
Section 4:	What is Occasionalism?	48
Section 5:	What Occasionalism is Not	49
Section 6:	The Occasionalists' Question	51
Section 7:	Rival Views of Causation	52
Section 8:	The Occasionalist' Solution	55
Section 9:	Source of Our Mistaken Views about Causation	58
Section 10:	God and Science - a Criticism	59
Section 11:	God and Essences - Further Criticism	64
Section 12:	Origins of Malebranche's Occasionalism	67
Section 13:	An Important Divergence	69

de Motu

Section 1:	Introduction	74
Section 2:	The Structure of *de Motu*	74
Section 3:	The General Thrust of Berkeley's Argument	76
Section 4:	Source of the Muddle	78
Section 5:	Against Inferring to the Best Explanation	79
Section 6:	Elucidating the Terms	81
Section 7:	The Force of Percussion	84
Section 8:	Berkeley's Construal of the Problem	87
Section 9:	A Missed Opportunity - Conservation	88
Section 10:	Descartes and Conservationism	90
Section 11:	Leibniz' Criticism	91
Section 12:	Boscovitch	93
Section 13:	Berkeley's Clarification	94
Section 14:	What Does "Gravity" Mean?	95
Section 15:	What Can and Cannot Cause Motion	97
Section 16:	The Origins of Our Knowledge	100
Section 17:	Cause and Explanation	102
Section 18:	A Linguistic Model	105
Section 19:	"Force" as Metaphor	110
Section 20:	What is Motion?	115
Section 21:	A Substitute for Absolute Space	118
Section 22:	Berkeley's Methodological Rules	125
Section 23:	Berkeley Attacks Descartes' Paradigm	127
Section 24:	The Nerve of Berkeley's Argument	130
Section 25:	A Possible Objection	132
Section 26:	An Apparent Failure of Nerve	133
Section 27:	The Limits of Science	135
Section 28:	Conclusion	136

Berkeley and Popper

Section 1:	Introduction	139
Section 2:	Popper's Position	140

Section 3: Instrumentalism - What is it? 142
Section 4: Explanation and Instrumentalism 144
Section 5: Popper and a Muddle about Forces 146
Section 6: More About Force 150
Section 7: Berkeley and Explanation 155
Section 8: Dispositions - An Ambiguity 160
Section 9: Realism About What? 162
Section 10: Berkeley's Realism 166
Section 11: Theories and Computation Rules 169
Section 12: Testing For Truth Or Applicability 174
Section 13: Obscurantism 175
Section 14: Conclusion 176

Conclusion 179

Key to Berkeley References 183

References and Bibliography 185

None of this material has been submitted for a degree in this or any other university.

INTRODUCTION

INTRODUCTION.

The aim of this thesis is, as the title indicates, to make the science and philosophy of Berkeley's *de Motu* explicit and to make the ideas accessible to the modern reader. [DM. For an explanation of the coventions used in referring to the works of Berkeley, see the Key to Berkeley References on page 183.]

In *de Motu*, a scientific background is presupposed. There are two aspects to the scientific background: first, reference to specific scientific issues is made, eg, vis viva, force of percussion, absolute space, conservation of motion; second, reference to the work of specific natural philosophers is made, eg, Leibniz, Borelli, Torricelli, and, especially, Newton. Both these aspects are clear and near the surface of the text. This requires a considerable investigation into science, certain construals of which are criticized in *de Motu*. Regarding the philosophical background there are again two aspects: first, reference to important conclusions of individual philosophers is made, eg, Democritus, Anaxagoras, Plato, Aristotle, Descartes, Leibniz; second, though Berkeley never mentions him by name, the philosophical influence of Malebranche is everywhere apparent: it is especially discernable in Berkeley's treatment of causation and scientific terms. So it will be necessary to say something about Malebranche whose writings Berkeley clearly found so suggestive when he was a student at Trinity. This historical research takes up the first third of this thesis.

It should not be thought - because I have treated the scientific and the philosophical backgrounds in separate sections - that there were in the seventeenth and eighteenth centuries two correspondingly separate enterprises, namely philosophy and physics, each having exclusive membership - as is very much the case now (except for the occasional maverick). In that period there was only philosophy practised by people calling themselves "philosophers"; it is a mere accident of history that some are remembered for what we now call "philosophy" and some for what we now call "science". In the period under discussion, they were all part of the one, single enterprise: increasing human knowledge. The subsequent split was perhaps inevitable, and for two reasons: first, specialization occurred because massive advances made it impossible for one person to be competent in all areas; second, a sceptical attitude - in some metaphysical quarters at least - towards science as being a bearer of truth, tended to force people into taking sides.

This division had not yet taken place when Berkeley wrote *de Motu*; it was still possible for someone to be an expert in both fields. It is not therefore surprising to note that it was one person who was behind the advances in metaphysics, mathematics and geometry, scientific methodology, epistomolgy, physics, etc - Descartes. This man, more than any other, is the father of the modern intellectual enterprise: I shall have something to say about him.

The historical investigation successfully completed, we will be better able to understand *de Motu*, and a detailed analysis of it takes up the second of the first two thirds of this thesis.

The method in the first two thirds of this thesis is historical; and, as far as possible, I attempt to describe Berkeley in eighteenth century terms, which, once the meaning of his terms has been grasped, is quite easy. It is not my intention in the first two thirds to present Berkeley as a contemporary, nor my wish to impose a shape on Berkeley's thought such that he seems a disputant in a war currently being waged in the pages of the learned press. But then neither do I want to excuse Berkeley's views by placing them in the context of a philosophical dark age and say that given the period in which he was writing he could do no better.

My aim then, to restate my opening sentence, is to make Berkeley's position clear; and to do it in such a way that I do not ascribe an opinion to him that he could never be brought to accept as a true description of his own position.

In the final third of my thesis my method changes: here, I treat Berkeley as a contemporary, if not of myself, at least of Popper. This is not to revoke my earlier method: it is to some extent forced upon me, since it is the one Popper himself adopts in his articles on Berkeley. Furthermore, it just so happens that Berkeley's concerns are in part ours also: he too has interests in language and science. With respect to science, the language he employs is not so far removed from ours that it requires torturing before it can be shown to express a view of interest to the modern philosopher of science.

THE SCIENTIFIC BACKGROUND

Section 1: Introduction.

In 1687 Newton published *Philosophia Naturalis Principia Mathematica* [Newton (1729)]. It remained the most notable event in the history of the physical sciences until Einstein published his *Special Theory of Relativity* in 1903 [Lorentz *et al*, (1923)] and his *General Theory of Relativity* in 1916 [Lorentz *et al*, (1923)]. In the *Principia* can be found the culmination of of the work of many men, spanning many hundred of years, who had spent their energies trying to comprehend the system of world. Newton's achievement was to synthesize the work of his predecessors by creating an order out of the chaos of notions and terms. Hitherto, scientists had spoken of gravity, levity, force, power, velocity, impetus, quantity of motion, mass, the centrifugal force of a revolving body, and the force of an impact, without ever having clear definitions of these terms and notions. Newton would radically simplify this state of affairs: he did this by selecting a number of statements which are evident or at least can be rendered plausible and which can serve as a starting points. These are preceded by a group of definitions in which the meanings of the other words used in the so-called axioms are defined with the aid of these terms which may be considered to require no further explanation. "Axioms", in this usage, is not to be understood as meaning a number of non-contradictory propositions that implicitly define the terms appearing in them nor as the basis for theorems to be deduced from them. Although Euclid's *Elements* had long served as the model for the systematization of a subject matter, its logical elegance was never achieved by Newton, partly as a result of his character and partly as a result of the nature of the subject matter.

Section 2: Descartes and Newton.

In the *Principia*, Newton adopts an extremely anti-Cartesian stance: his aim is to oppose to the Cartesian philosophy, with its apriorism and its attempt at global deduction, another and rather different philosophy, a philosophy more empirical and at the same time more mathematical than that of Descartes, a philosophy which restricts itself to the effects, the phenomena, the surface of things, and which sees its goal in the study of the nature's mathematical frame, and the mathematical laws of natural forces. Or as Newton himself put it:

> ... the whole burden of philosophy seems to consist in this - from the phenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the other phenomena. [Newton (1975), p XVII-XVIII.]

But despite Newton's criticisms of Descartes' physics - mathematical physics without mathematics - despite even Newton's failure to admit it, despite even "Error, error non est Geom", it seems reasonable to suppose that Newton found in Descartes valuable insights. After all, Descartes was the first to formulate a consistent set of rules of motion (they do, however, suffer from the rather serious defect of being wrong!); and Decartes' rational cosmology was an identification of celestial and terrestial mechanics - for the first time centrifugal forces had been seen in the heavens. It is only in Descartes, for example, that we find not only the clear assertion of the uniformity and rectilinearity of inertial motion, but also the explicit definition of motion as a "state." [Descartes (1983), p 52.]

Descartes insists that it is a vulgar error to put motion and rest on different levels of being and to think that more power is needed to put in motion a body that is at rest than is needed to put at rest a body that is in motion. He is right: the ontological equivalence of motion and rest is at the very centre of the new mechanics. All this Newton tacitly recognizes by using Decartes' term "status." [Newton (1729), p 13.]

It is precisely the institution of the concept of "status of motion" for actual motion that enables Descartes, as it was to enable Newton, to assert the validity of his first law or rule of motion. Actual motion is essentially temporal: a body takes a certain time to move from one place to another place, and during that time, however short, the body is necessarily subjected to the action of forces "*qui cogent* it *statuum suum mutare*" ("which compel it to change its state"). [Descartes, quoted in Koyré, (1965) p 69.] The status, however is connected with time in a very different way: it can either endure, or last only an instant. Hence a body in curvilinear or accelerated motion changes its status every instant because at every instant it changes either its direction or its speed; it is nevertheless every instant "*in statu movendi uniformiter in directum*" ("in a uniform state of motion in a given direction"). [Descartes, quoted in Koyré, (1965) p 69.] Thus a way of atomizing motion had been found which complemeted physical atomism. Descartes expresses the same notion clearly when he says that it is not the the actual motion of a body but its inclination, its "conatus", that is rectilinear. Newton, in Definition III, put it more briefly, using only the Cartesian formula "quantum in se est" ("as much as in it lies"). [Newton (1729), p 2.]

Newton and Descartes explain in very different ways how bodies persevere in their states. Newton does it by ascribing a certain "vis insita" to matter, that is,

> ... a power of resisting, by which every body, as much as in it lies, continues in its present state, whether it be of rest, or of moving uniformly forwards in a right line. [Newton (1729), p 2.]

This is taken from Definition III, the Latin original of which has been much argued over. This power or force Newton calls "vis inertiae." However, Newton himself admits - still in Definition III - that,

> ... it is resistence so far as the body, for maintaining its present state, opposes the force impressed; it is impulse, so far as the body, by not easily giving way to the impressed force of another, endeavors to change the state of that other. [Newton, (1729) p 2.]

It is resistence if the body is at rest, impulse if it is in motion. But this distinction can only be an apparent one, since motion and rest, as Newton himself admits at the end of this section of the *Principia*, are only relatively distinguished.

<u>Section 3: Newton's Achievement</u>.

In Propositions I and II of Book I of the *Principia*, Newton first of all succeeds in proving that for all motions resulting from the operations of a central force, irrespective of the law which states how the magnitude of the force depends on the distance from the centre, Kepler's law of areas applies, and that conversely it also follows from the applicability of this law that the force is directed towards the centre from which the radius vector describing the areas has been drawn. From the second of Kepler's laws of planetary motion it thus results that the planet is acted upon by a force directed towards the sun. Newton subsequently shows that if a material point describes an ellipse under the influence of a force directed towards one of the foci, the magnitude of the force is inversely proportional to the square of the distance from the centre of force, so that it can be concluded on the strength of Kepler's first law that the planents are attracted by the sun in accordance with this law. According to Newton's third law or axiom the planets must therefore attract the sun with an equal but opposite force. Newton now extends this result to all material bodies in the universe, and thus arrives at the formulation of the general principle of gravity, according to which every particle of matter attracts every other with a force whose magnitude is directly proportional to the product of the mass of these particles and inversely proportional to the square of the distance between them. It is now possible for Newton to treat all motions in the universe mathematically, while at the same time considering the effects of terrestial gravity to be a special case of general gravitation, so that the motion of falling bodies on the earth, and the motions of the planets about the sun, could be regarded as examples of the applicability of the same law.

And this simplifying unity of conception of the physical universe that Newton attained - surely a remarkable feat by any standards - made it possible to reconcile what had hitherto been assumed to be wholly disparate phenomena, ie, the celestial and terrestial phenomena of motion, formerly believed antithetical, were now found to be subject to the same universal law of gravity. The natural motion of a free falling, heavy body and the enforced motion of a projectile or, in non-Aristotelian language, the motion of bodies falling, rising or thrown to the earth and that of the planents could now be shown subject to the same universal law. Even those things that seemed to offer not a single point of comparison, such as tidal phenomena, were now seen to be examples of the same law.

But note already the language employed in the this brief summary of the *Principia*: "acted upon by a force", "under the influence of a force", "force directed towards the centre", and "the action of forces" have all been used in this outline and all suggest that the forces in question are something over and above motion itself. Yet all of them are quite natural ways of speaking in science.

Section 4: Criticisms of Newton.

To those natural philosophers who adhered to a Cartesian conception of mechanical science - and in this period surely many, if not most, did; adhered, that is, to a conception that admitted only explanations that were truly mechanical, namely physical ones based on the belief that the only real force was the force of impact, Newton's views seemed a relapse into the scholastic physics of qualities and powers, and into animistic explanations employing such terms as "antipathy" and "sympathy".

Descartes' continuing influence cannot be doubted; his physics received a popular exposition in the famous text book on physics written by Rohault, and published in 1674. This book was translated into Latin from the original French and appeared in England in 1682. It was widely accepted as the best general treatise on physics, and was still available when Berkeley was writing *de Motu* in 1720 (ie, thirty-three years after the first edition of the *Principia*). In 1697 a new Latin translation of Rohault's book appeared in England by Samual Clarke; this in its turn was translated into English in 1723. These facts indicate the endurance of Cartesian physics - even in the face of severe Newtonian criticisms.

Descartes' enduring contribution to physics is that he managed to free physics from explanations employing such terms as "antipathy" and "sympathy" and had taught scientists to consider the operation of every force as the effect of the motion of the material particles; henceforth, science would be truly mechanical. Furthermore, Descartes had invented systems to explain the motion of the earth round the sun and the fall of heavy bodies by the action of material particles which, when organized into vortices, drew any object placed in one, be it planet or satellite, nearer to its centre. But now Descartes' followers were asked to put all this to one side and to accept a theory that explained gravity as a mysterious, attractive force mutually exerted - without any intermediary mechanism - by two bodies and separated by empty space. In a sense this was even worse than scholastic physics: that at least had always rejected action at a distance.

But the reaction was not entirely hostile: Newton's critics agreed that he had found that the moon moves as if the same cause that causes weight or heaviness on earth also causes the motion of the moon; and they even went so far as to agree with Newton in assigning the same cause to the motion of the planets around the sun, and of satellites around planets. But there were limits, as we can see from the following passages from Huygens:

> I have nothing against the vis centripeta, as M Newton calls it, by which he makes planets gravitate towards the sun and the moon towards the earth [it is] known by experience that there is in nature such a manner of attraction or impulsion. Indeed nothing prohibits that the cause of this vis centripeta towards the sun be similar to that which makes the bodies that we call heavy descend towards the earth. [Huygens, quoted in Cohen (1980) p 79.]

Huygens could see well enough that there must be some cause acting on the planets preventing them from flying off at tangents according to the law of inertia. But planetary motion apart, attraction was something else:

> Concerning the cause of the tides given by M Newton, I am by no means satisfied [by it], nor by all the other Theories that he builds upon his principle of attraction, which to me seems absurd.... [Huygens, quoted in Cohen (1980) p 80.]

No beating about the bush here; and furthermore:

> That is something I would not be able to admit because I believe that I see clearly that the cause of such an attraction is not explicable by any of the principles of mechanics, or of the rules of motion. Nor am I convinced of the necessity of the mutual attraction of whole bodies, since I have shown that, even if there were no earth, bodies would not cease to tend towards a centre by that which we call gravity. [Huygens, quoted in Cohen (1980) p 81.]

On reading the *Principia*, Huygens was forced to admit that "Vortices [have been] destroyed by Newton", and thought that this was a considerable advance. But he eventually substituted for the destroyed Cartesian vortices a new kind of vortex so that the effect of gravity might still be explained by matter and motion, according to the principles of the mechanical philosophy.

It seemed to many like Huygens that at the bottom of the *Principia* there was a metaphysical position akin to that espoused by the peripatetics with their talk of occult forces: terms such as "force", "gravity", and "attraction" seemed to function as the scholastic "powers" and "qualities" had. But were these not the very same sort of terms as those assiduously removed from scientific discourse by such men as Descartes, Malebranche (the *Search* is full of anti-Aristotelian abuse); and in England by Francis Bacon. I doubt if Newton's critics failed to recognise the importance of many of his results - as we have seen Huygens was no exception - but they refused to believe that these results had a physical basis.

Berkeley was to question this need: since science could do all that was wanted without one, why be troubled by misleading and irrelevant questions, which fall outside the scope of physics anyway?

But had Newton's critics fully grasped the contents of the *Principia*?

Section 5: Newton's Position (i).

It is well known that Newton did not believe that gravity was an "innate, essential and inherent property of matter"; indeed, in 1679 he had made attempts to explain gravity by mechanical means, ie, by the motions of either subtile matter or an etherial medium. He did not pursue these speculations for some time and he asked Richard Bentley, in a letter of 1692, not to ascribe to him that Epicurean notion:

> It is inconceivable that inanimate brute matter should, without the mediation of something else which is not material, operate upon and effect

> other matter without mutual contact, as it must be if gravitations, in the sense of Epicurus, be essential and inherent in it. And this is one reason why I desired you would not ascribe innate gravity to me. That gravity should be innate inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. [Newton, in Thayer (1953), p 54.]

This notion was Epicurean because Epicurus had,

> ... laid down atoms, space and gravity, as the first principles of all things. The Universe, he taught, consisted of atoms or corpuscles, of various forms, magnitudes, and weights. [Chambers (1741); unpaginated edition.]

These atoms became organized into various worlds or systems which from time to time collapsed leaving the constituents atoms to reform at some future date,

> ... without the intervention of any deity, or the intervention of any providence. [Chambers (1741); unpaginated edition.]

In Cicero's *The Nature of the Gods*, we find Velleius' summary of the Epicurean position:

> Our master [ie, Epicurus] has taught us that the world was made by a natural process, without any need of a creator: and that this process, which you say can only be effected by divine wisdom, in fact comes about so easily that nature has created, is creating, and will create, worlds without end. But as you cannot see how nature can do this without the intervention of mind, you follow the example of our tragic playwrights and take refuge in a divine intervention to unravel the intricacies of your plot. You would have no need of such divine handiwork, if you would only consider the infinite immensity of boundless space in all directions. The mind may

> plunge into space and in thought traverse it far and wide, yet never find that further shore where it may come to rest. In this immensity of breadth and lenght and height there swarms the infinite power of atoms beyond number. And although they move in a vacuum, they cohere amongst themselves, and then are held together by mutual attraction. Thus are created all the shapes and forms of natrue, which you imagine can only be creaed by some divine blacksmith with his anvil and his bellows! [Cicero (1972), p 91.]

The consequence of all this is that God, if he exists at all, exists in haughty detachment, far removed from any involvement with the mundane: in any case, it could look after itself.

Yet Bentley, in spite of Newton's admonitions, could write that,

> ... a constant Energy [is] infused into matter by the Author of all things... [Bentley, in Cohen (1958), p 363.]

And that,

> ... Gravity may be essential to matter... [Bentley, in Cohen (1958), p 363.]

As for Newton himself, he does not - at least in the *Principia* - express his own views about the nature of gravitation; nor did he tell his readers that action at a distance without mediation was an impossibility, and that bodies could not in this sense attract each other. But he did explain that the forces of attraction and repulsion dealt within natural philosophy, forces by which bodies either approach one another or recede from one another are not to be taken as causes of such movement, but as "mathematical forces", the cause of which is unknown. This remark is clearly echoed in

Berkeley's *de Motu*, (see for example *de Motu*, paragraph 22). As Newton says in Definition VIII of the *Principia*:

> I ... use the words attraction, impulse, or propensity of any sort towards a centre, promiscuously, and indifferently, one for the other; considering those forces not physically, but mathematically... [Newton (1729), p 5.]

That statement seems clear and unambiguous and so it is: it is the use he makes of ideas contained in it that make Newton's thoughts subtle and sophisticated - not to mention difficult to understand. So, the forces with which he is dealing are mathematical, that is, in so far as they can be treated mathematically, he can deal with them. But he does not care or even enquire, at least in the *Principia*, what they are in themselves, what either their true nature or their true causes might be: "Hypothese non fingo" ("I feign no hypothese"). Newton confines himself to an investigation of the manner in which these forces behave, of what can seen and measured. It is odd therefore, given Newton's repeated and decided pronouncements on the matter, that he was interpreted as positing action at a distance by an attractive force residing in bodies.

An example of this misunderstanding can be seen in a letter written to Hartsoeker by Leibniz in 1711:

> If you allege only the will of God for it, you have recourse to a miracle.... For example, if any one should say, it is God's will that a planet should move round in its orb without any other Cause of its Motion, I maintain that it would be a perpetual miracle: for by the nature of things, the planet going round tends to move from its orb along the tangent, if nothing hinders it; and God must constantly prevent it, if no natural cause does....
>
> [Those], who own that gravity is an *occult quality*, are in the right, if they mean by it

> that there is a certain mechanism unknown to them, whereby all bodies tend towards the centre of the earth. But if they mean that the thing is performed without any mechanism, by a simple *primitive quality*, or by a law of God, who produces that effect without using any intelligible means, it is an unreasonable occult quality, and so very occult, that 'tis impossible it should ever be clear, tho' an angel should undertake to explain it. [Leibniz, quoted in Koyré (1965) p 141.]

Section 6: Newton's Position (ii).

Cotes, while preparing the *Principia* for its second edition in 1713, corresponded with Newton, seeking, among other things, clarification of the problems mentioned at the end of the previous section. In 1712/13 he wrote to Newton enclosing for approval his outline of the preface. Cotes suggested that the difference between Newton's method and Descartes' should be made clear by showing that Newton proceeded by demonstrating or deducing from the phenomena of nature the principle this phenomena is based on (the principle of universal gravity), and that he did not merely assert it. The demonstration will be based on,

a) the first law of motion (the law of inertia) which states that moving objects will move in a straight line if no force acts upon them; and on,

b) the observed fact that planets do not move in straight lines, but in curves.

The planets are acted upon by a force,

> ... which Force may ... not improperly be called centripetal in respect of ye revolving bodies, and attractive in respect of the Central. [Newton (1975), p 392.]

Cotes however met with a difficulty:

> ... in the first Corollary of the 5th Proposition [of Book III] I meet with a difficulty, it lyes in these words *Et cum attractio omnis mutua sit* I am persuaded they are then true when the Attraction may properly be so called, otherwise they may be false. You will understand my meaning by an Example. Suppose two Globes *A* & *B* placed at a distance from each other upon a Table, & that whilst *A* remains at rest *B* is moved towards it by an invisible Hand. A bystander who observes this motion but not the cause of it, will say that *B* does certainly tend to the centre of *A*, & thereupon he may call the force of the invisible hand the centripetal force of *B* & the Attraction of *A* since the effect appears the same as if it did truly proceed from a proper & real Attraction of *A*. But then I think he cannot by virtue of this Axiom [the mutual actions of two bodies upon each other are always equal] conclude contrary to his sense & Observation that the Globe *A* does move towards the Globe *B* & will meet it at the common centre of Gravity of both bodies. This is what stops me in the train of reasoning by which I would make out as I said in popular way Your 7th Prop. Lib III. I shall be glad to have Your resolution of the difficulty, for such I take it to be.... For till this objecton be cleared I would not undertake to answer any one who would assert that You do *Hypothesim fingere*, I think You seem tacitly to make this supposition that the Attractive forces resides in the Central Body. [Newton, (1975), p 392.]

Newton's reaction to all this is rather interesting; he first enlightens Cotes about the meaning of the word "hypothesis"; tells him that attraction is not a hypothesis but a truth established by induction, and that the mutual attraction of bodies is a case ot the third fundamental law or axiom of motion:

> Sr
> I had yours of Feb 18th, & the Difficulty you mention wch lies in these words [et cum Attractio omnis mutua sit] is removed by considering that as in Geometry the word

> Hypothesis is not taken in so large a sense as to include the Axiomes & Postulates, so in Experimental Philosophy it is not to be taken in so large a sense as to include the first Principles or Axiomes wch I call the laws of motion. These Principles are deduced from Phaenomena & made general by Induction: wch 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 Phaenomenon nor deduced from any Phaenomenon but assumed or supposed wthout any experimental proof. Now the mutual & mutually equal attraction of bodies is a branch of the third Law of motion & how this branch is deduced from Phaenomena you may see in the Corollaries of ye Laws of Motion, pag. 22. If a body attracts another body contiguous to it & is not mutually attracted by the other: the attracted body will drive the other before it & both will go away together wth an accelerated motion in infinitum, as it were by a self moving principle, contrary to ye first law of motion, whereas there is no such phaenomenon in all nature. [Newton (1975), p 275.]

The third law states that every action has an equal and opposite reaction, and is formulated in the *Principia* like this:

> Law III. *To every action there is always opposed an equal reaction: or, the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.*
>
> Whatever draws or presses another is as much drawn or pressed by that other. If you press a stone with your finger, the finger is also pressed by the stone. If a horse draws a stone tied to rope, the horse (if I may say so) will be equally drawn back towards the stone; for the distended rope, by the same endevour to relax or unbend itself, will draw the horse as much towards the stone as it does the stone towards the horse and will obstruct the progress of the one as much as it advances that of the other. If a body impinges upon another, and by its force change that motion of the other, that body also (because of the equality of the mutual pressure) will undergo an equal change, in its own motion, towards the contrary part. The

> changes made by these actions are equal, not in the velocities, but in the motions of the bodies; that is to say, if the bodies are not hindered by any other impediments. For, because the motions are equally changed, the changes of velocities made towards contrary parts are inversely proportional to the bodies. [Newton (1729), p 13*f*.]

Newton's reply is, it appears, to direct Cotes to appropriate sections of the *Principia* where Cotes will find all the clarification he needs. But as a matter of fact, Newton did not stop there: he made certain additions to the text, which are not mentioned in his correspondence with Cotes. Newton adds to the text of the third law; in the Scholium to the laws he adds a paragraph in which he argues that it is not only magnets and iron that attract each other in conformity with the third law but also the earth and its parts; he also changed the wording of Proposition V, and added to it a corollary in which the mutual character of attraction is again asserted. Here is what he wrote in the Scholium that concludes Section XI of Book I:

> I here use the word *attraction* in general for any endevour what ever, made by bodies to approach to each other, whether that endevour arise from the action ot the bodies themselves, as tending to each other or agitating each other by spirits emitted; or whether it arises from the action of the ether or of the air, or of any medium whatever, whether corporeal or incorporeal, in any manner impelling bodies placed therein towards each other. In the same general sense I use the word *impulse*, not defining in this treatise the species or physical qualities of forces, but investigating the qualities and mathematical proportions of them; as I observed before in the Definitions. [Newton (1729), p 192.]

Note that both "impuse" and "attraction" are to be interpreted as being devoid of any reference to the means of there production. Newton, for whom impulse is the only acceptable mode of action for a physical force, gives "impulse" a neutral meaning so as not to concede anything to the Cartesians: both "impuse" and "attraction" are to be understood as having only a mathematical meaning. It may be noted in passing that in the fifth section of *de Motu*, Berkeley makes a similar move when giving his solution to the problem of whether causal explanation is the more fundamental explanation to which all other explanations have to be reduced if they are to count as explanations at all. In other words: Do all explanations have to be causal?

Section 7: Newton's Position (iii).

Cotes did not debate these points further with Newton; but I do not think we can conclude on that basis that Cotes was convinced: if anything he believed he was right when he made the above points, but blundered in treating gravity as a supposition or hypothesis. And surely Cotes *was* rigth when he pointed out that it is only if bodies do act upon on other that it can truly said of them that they really attract on another; and he was right when he suggested that Newton's whole reasoning was based on that very assumption. Indeed, if a body pulls or draws, it is drawn or pulled by it and just as strongly. On the other hand, if a body is only pushed towards another, then the action and reaction take place between the pushing and the one pushed, and not between the latter and the one towards which it is pushed: their reaction is just not mutual. If for instance we assume that the planets are deflected from their rectilinear path not by means of something which binds them together in the way a string prevents a whirling stone flying off at a tangent, but by, eg, some external force such as a Cartesian vortex, then we can conclude that the sun will not be pushed towards the planets.

It could be said, as it was by Leibniz in "Tentamen de Motuum Coelestium Causis", that "the planets are attracted by the sun" [Leibniz, quoted in Koyré (1965), p 279]: but it would be wrong to say that the attraction was mutual. Newton's experiments with magnets and iron had shown him that the third law is valid in the case of magnetic attraction; from this he rightly concludes that magnets really act upon iron. Cotes would have said that this was a genuine case of "attraction properly so called." The planets present us with rather a different problem: we do not know if they really do attract one another - we have no way of measuring the force, if any, involved; attractive force maybe the result the result of pressure or impulsion. All we know is that they are subject to centripetal forces, that is , they do not fly of at a tangent. By asserting that the forces involved are mutual merely because the law that says all attraction is mutual is true and as such is contained in the third law, Newton was arguing in a circle. If Newton did not commit such an elementary mistake, and surely he did not, then his reasoning can only be explained if we accept Cotes' criticisms and admit that Newton's "attr-action" was the name of some sort of real force, though not necessarily a physical one, by which forces really acted upon each other by means of some immaterial link, and that this force was somehow connected with these bodies and was proportional to their masses. This indeed would be "attraction properly so called."

None of this appears directly in the *Principia*, in which Newton constructs the system of Books I and II from a purely mathematical standpoint, in terms of a series of imagined systems or constructs whose physical reality or lack of reality was not at issue at that stage of the inquiry. Later, he found that certain forms of the basic construct led to an agreement with the phenomena to an extent that gave him confidence that the construct was capable of prediction and retrodictions, that is, capable of explanations of known phenomena, and even of new and as yet unknown effects which were later confirmed by observation. Finally, his mathematics was applied to natural philosophy - with amazing results. This is the view of I B Cohen [Cohen (1980) p 110*f*], and it is also very much the view of Berkeley who held that scientists use a construct - like those used by geometers, constructs whose reality was never assumed - which, when applied to the system of the universe, enabled the scientists to understand the phenomena of terrestial and celestial motion. (Cf *de Motu*, pargraphs 38 and 39.)

In private, Newton struggled on and off for years with the problem of gravitational phenomena. To the second edition of the *Opticks* (published in 1717), Newton added "Query 31", from which the following quotations come; Newton can be seen wrestling with the problem:

> Have not the small particles of bodies certain powers, virtues, or forces by which they act at a distance?... For is is well known that bodies act one upon another by the attractions of gravity. [Newton, in Thayer (1953), p 159*f*.]

However,

> How these attractions may be performed, I do not here consider. What I call "attraction" may be performed by impulse, or by some other means unknown to me. I use the word here to signify only in general any force by which bodies tend toward one another, whatsoever be the cause. [Newton, in Thayer (1953), p 160*f*.]

This is the offical theory and close to what Newton said in the *Pricipia*. But in the closing pages of the "Queries", Newton seems less than completely sure of his official theory:

> It seems to me further that these particles have not only a *vis inertiae*, accompanied with such passive laws of motion as naturally result from that force, but also that they are moved by certain active principles, such as is that of gravity .... These principles I consider ... as general laws of nature by which the things themselves are formed, their truth appearing to us by phenomena, though their cause be not yet discovered. [Newton, in Thayer (1953), p 176*f*.]

None of Newton's mechanical models worked, and at the time of writing to Bentley he was forced to conclude that,

> Gravity must be caused by an agent acting according to certain laws, but whether this agent be material or immaterial I have left to the consideration of my readers. [Newton, in Thayer (1953), p 54.]

One wonders if occasionalism was ever entertained by Newton: there is, of course, no way of knowing whether it was, but occasionalism certainly provides for the *Principia* a referent for the term "attraction properly so called." In section 8, I will consider the extent to which Newton thought that God was the cause of gravitational phenomena; this question will also demand attention when I come to consider the famous exchange of letters between Leibniz and Samuel Clarke. [Alexander (1956).]

Cotes' only error, then, was to assume that this conception was a hypothesis made by Newton in order to permit him to subject attraction to the provisions of the third law, whereas for Newton attraction is an empirically ascertained and demonstrated fact of which only the cause is unknown: but it is definitely not a hypothesis. Cotes seems to have understood this perfectly; his only mistake was to take attraction to be a supposition - but he became convinced that attraction was a property of bodies and even an essential one. For this last belief he was taken to task by Clarke to whom he submitted the draft preface. Cotes replied that he only wanted to indicate that as we do not know what matter really is, we may ascribe to it all kinds of properties which by experience we learn it has:

> For I understand By Essential Propertys such propertys without which no other belonging to the same substance can exist: and I would not undertake to prove that it were impossible for any of the other Propertys of Bodies to exist without even Extension. [Cotes, quoted in Koyré, (1965) p 282.]

Cotes corrected his text to say that attraction was a primary property of bodies:

> Since, then, all bodies, whether upon earth or in the heavens, are heavy, so far as we can make any experiments or observations concerning them, we must certaily allow that gravity is found in all bodies universally. And in like manner as we ought not to suppose that any body can be otherwise than extended, movable, or impenetrable, so we ought not to conceive that any body can be otherwise than heavy. The extension, mobility, and inpenetrability of bodies become known to us only by experiments; and in the very same manner their gravity becomes known to us. All bodies upon which we can make any observations, are extended, movable, and impenetrable; and thence we conclude all bodies, and those concerning which we have no observations of, to be heavy also, If anyone should say that the bodies of bodies of the fixed stars are not heavy because their gravity is not yet observed, they may say for the same reason that they are neither extended nor movable nor impenetrable, because these properties of the fixed stars are not yet

> observed. *In short, either gravity must have a place among the primary qualities of all bodies, or extension, mobility, and impenetrability must not.* And if the nature of things is not rigthly explained by the gravity of bodies, it will not be explained by their extension, mobility, and impenetrability. [Cotes, in Newton (1729), p XXVI; italics mine.]

The italicized section of that extract gives the impression that despite Clarke's and Newton's correction, Cotes believed that "gravity" should be given an essentialist interpretation or, at least, that the argument in Newton's text warranted that concluson. Surely it does not: it is part of our understanding of the concept "body" that it includes that of "extension"; it is not part of our understanding of the concept of "body" that it also includes that of "gravity." It is just a matter of fact, if it is, that all objects tend towards one another in accordance with the inverse square law; but it is manifestly not just a matter of fact that all objects are extended. It is surely conceivable that two objects in close proximity do not tend towards each other; it is inconceivable that an object could be extensionless.

Section 8: Atheism Vanquished.

It is evident that while one remains a supporter of atomism and mathematical philosophy, that is, while one believes that matter is nothing other than what entirely and adequately fills space, one cannot include forces, repulsive or attractive, in the essence of body because this is already fully determined by extension, hardness, impenetrability, mobility, etc. This being so, it provides a basis for those with religious, or even just theistic, convictions to attack the Epicurean materialism of Lucretius, Hobbes and even Descartes: All held that the cosmic system was the result of the chance collisions of atoms and that gravity was an innate force of matter and as such responsible for gravitational phenomena. Descartes did not believe that gravity was such a force but held that God was not responsible for ordering the universe into a cosmos but only for sustaining it in being.

But a number of questions now become acute, namely, How did the cosmic system originate? Why did the planets, once formed, not fall into the Sun? Why is the position of each orbit relative to the Sun not different to what it in fact is? In short: If the cosmic system does not have the necessary resources, who or what does? In the first series of Boyle Lectures, delivered in 1692, Richard Bentley - helped by Newton, who wrote a number of letters to Bentley (while the last two lectures were being prepared for the press) elucidating some of the theological consequences of the *Principia* - came to the conclusion that it must be God.

Bentley was to show that if gravity had been the only force active at the moment of creation, the planets of the solar system would have fallen into the Sun. Therefore, a specific intervention of some force must be assumed to be responsible for preventing the planets plunging to their doom and for placing them in their respective orbits. Again, when the spacing of the orbits is considered, it is clear that no principle of science could determine the relations of the distances, except that "the Author of the system though it convenient." Bentley seemed to Newton to be on the right track when he argued that the operations of gravity over empty space could only mean that the agent was constantly guiding the planets according to certain laws. Clearly, this agent must have volitions, and must be very skilled in mechanics and geometry. Bentley called it "God"; Newton did not object. In the General Scholium, first published in the second edition of the *Principia* (ie, in 1713, more than twenty years after the correspondence between Newton and Bentley) Newton wrote,

> ... planets and comets will constantly persue their revolutions in orbits given in kind and position, according to the laws [of planetary motion]; but though these bodies may, indeed, continue in their orbits by the mere laws of gravity, yet they could by no means have at first derived the regular position of the orbits themselves from those laws. [Newton (1729), p 543.]

A nice point that, disentangling matters of fact from matters of method. And,

> This most beautiful system of the sun, planets, and comets, could only proceed from the counsel and dominion of an intelligent and powerful Being. And if the fixed stars are the centres of other like systems, these, being formed by the like wise counsel, must be all subject to the dominion of One; especially since the light of the fixed stars is of the same nature with the light of the sun, and from every system light passes into all the other systems: and lest the system of the fixed stars should, by their gravity, fall on each other, he hath placed those systems at immense distance from one another. [Newton (1729), p 544.]

But, as we saw, Bentley wrote something that implied that he thought that gravity was an inherent property of matter, implicit in the very substance, a sort of occult quality. We also saw the severity with which Newton put Bentley straight: under no circumstances could gravity be deemed to have a material existence. But this oversight notwithstanding, Bentley was now in a position to conclude that mutual gravitation can operate at a distance only because it was simultaneously regulated by the agent and not by the system itself. This is what both Bentley and his era wanted: "a new and invincible argument for the being of God." It was wanted because it repudiated the atheism affected during the Restoration by coffee house and tavern wits - who got a pretty severe pasting at the hands of Jonathan Swift. (It is surely not without significance that Berkeley and Swift were both Irish, both ousiders in a corrupt society and friends of one another.) The principle source of this atheism was Hobbes who had been attacked for his impiety by the orthodox for forty years. But to the guardians of Christianity it seemed that the tide of atheism had not yet been checked; Bentley's achievement was to show that the new science, in particular the natural philosophy of Issac Newton, was not only no threat to the Christian religion but was also the basis for a demonstration and certain proof of the existence of God. Thus, the new wisdom was accommodated and too severe a change averted; what the era wanted after a century of unprecedented civil, religious, and intellectual turmoil was peace of mind, a period of stability, of consolidation and a chance to build on the revolutionary foundations. But revolution itself had come to end.

THE PHILOSOPHICAL BACKGROUND

Section 1: Introduction.

In the "Scientific Background" I considered the scientific kowledge relevant to my inquiry; I must now turn my attention to philosophy and explore the ideas which Berkeley found suggestive and those which he was combatting. I have already touched on some of these ideas; this is unsurprizing since the previous part, being an introduction to the science of *de Motu*, determined that the contents should also reveal somthing of the philosophical concerns of *de Motu* because it was, after all, the philosophy of that science that pre-occupied Berkeley. The sort of questions he found pressing are, for example, What is the cause of gravitational phenomena? What are the limits of mechanistic explanation? What is an explanation anyway? What is the rôle of God? What is the status of theoretical terms? I now wish to investigate the source of the ideas Berkeley used to answer these questions; in some cases the investigation will continue to the end of this thesis - in a sense they are what this thesis is about. The ones I want to look at now are those that derive from occasionalism, for without doubt the occasionalism of Malebranche exerted a considerable influence on Berkeley's philosophy of science.

Berkeley found Malebranche very instructive - *de la Recherché de la Vérité* [Malebranche (1980a)] offered a solution to Berkeley's main concern regarding science: How to admit its usefulness without at the same time conceding that it truely described a reality behind the phenomena of our every day experience? Berkeley's feelings were that many people - not just scientists - were choosing either science or religion because they thought that science and religion were mutually exclusive alternatives: it was Berkeley's intention that such a choice does not have to be made. Behind this concern lay the greater concerns of metaphysics, religion, and morality: these and these alone dealt with the truth. Note, for example, the full title of the *Principles*: *A Treatise Concerning the Principles of Human Knowledge wherein the Chief Causes of Error and Difficulty in the Sciences, with the Grounds of Scepticism, Atheism, and Irreligion, are Inquired into*. This was first published in 1710, ie three years before the second edition of the *Principia* and its attempt, in the General Scholium, to reconcile science and religion.

An equally important concern was for the correct philosophical method; this is more than a nod in Descartes' direction: from clear and distinct perceptions being the basis of all right science we move to the clear and distinct use of language as the basis of a correct philosophy of science.

<u>Section 2: Berkeley's Sources</u>.

Berkeley read Malebranche's *Search* [Malebranche (1980a)] sometime between 1700 and 1704, that is, when he was a student at Trinity College, Dublin. Berkeley probably read it in Thomas Taylor's translation, the second edition of which had been published in 1700 [Taylor (1700)]. Luce, in his *Berkeley and Malebranche* [Luce (1967)], notes that some of Berkeley's phrases are the same as those that Taylor uses: eg, "outness" for "des dehors"; "illustration" for "eclaircissment"; "mediums" for the signs of distance, all seem to derive from Taylor's translation. There are many references, direct and indirect, to Malebranche in Berkeley's *Philosophical Commentaries*. [PC. For details of the conventions I have used in referring to the works of Berkeley, see GEORGE BERKELEY in *References and Bibliography.*] Berkeley, while accepting much of what Malebranche says about secondary causes, does not accept Malebranche's thorough-going occasionalism. Malebranche believed that God is the cause of the determinations of our wills as well as the cause of events in the world. Not so Berkeley:

> We move our legs our selves. 'tis we that will their movement. Herein I differ from Malebranche. [PC, p 304.]

Berkeley's belief that minds were the only genuine causal agents, is one he makes important use of in *de Motu*. The motion of objects cannot be caused by other moving objects (because objects are causally inefficacious); therefore a mind must cause such movements; we do not cause it ; therefore, there must be a mind which does - call it "God". This is Berkeley's version of the argument for occasionalism.

The basic point made by Malebranche is this: there is no power or force in bodies which accounts for their motion. He argued as follows: we lack a clear idea of the supposed power or force in bodies; nor can the examination of an idea of a body reveal under what circumstances it will move or in what way it will move other bodies. We observe only a constant and regular conjuction of events, not a necessary connection between them. Furthermore, since nothing can move a body that God wills to be at rest or impede or alter the movement of a body that God wills to be in motion, bodies themselves are inefficacious. And since there is a necessary connection between the volition of an omnipotent being and the execution of that volition, God is the true cause of motion in bodies, and the action of his will is their moving force. So, when one ball strikes another and the latter moves, God is the real or true cause of its motion. Since God acts, not at random, but in accordance with the general laws of motion which he as enacted, the impact of the first ball may be called the occasional or secondary cause of the second ball's movement.

Malebranche's inflence on Berkeley is clearly discernable:

> The cause of all natural things is onely God. Hence it is trifling to enquire after second Causes. [PC, p 290.]

And,

> Those may be more properly be Called occasions yet (to comply) we may term them Causes. but [sic] then we must mean Causes yt do nothing. [PC, p 334.]

Both of these quotations give a clear indication of the direction in which Berkeley's mind was moving in the early seventeen hundreds.

Berkeley, while accepting some of Malebranche's arguments, developed them to more radical conclusions. As we saw, Malebranche believed that material things are inert, powerless things: they, and whatever else may exist, depend on God. Berkeley thought that this contention could and should be pressed further; for if it is true, then what useful rôle is left to either material substance or material things in the divine scheme things? The question is not now: Whether there is anything distinct from spirit and idea? But: Whether there are certain ideas in God's mind which direct him how and in what order to produce ideas of sense? Ideas produced,

> ... much after the same maner as a musician is directed by the notes of music to produce that harmonious train and composition of sound, which is called a tune; ... [Principles, p 99.]

According to Berkeley, this is all that remains of the notion of matter once it has become dead, inert occasion for our ideas.

Berkeley's conclusion needs no mention: but it is interesting to consider the fact that in adopting this conclusion he may have been influenced by Pierre Bayle's *Dictionary* [Bayle (1710)]; of special interest in this resect is the article "Zeno of Elea". Remark F of this article deals with the impossibility of extension and motion.

> There is no extension, therefore there is no Motion. The Consequence is good, for what hath no Extension takes up no room, and what takes up no room cannot possibly pass from one place to another, nor consequently move. [Bayle (1710), p 3077.]

According to Bayle this was "incontestable", hence the thing to do was to prove that there is no extension and that would be enough to prove that motion is impossible. So an important part of this Remark was devoted to proving that there is no extension:

> ... extension cannot be composed either of Mathematical Points, of Atoms, or Parts divisible *in infinitum*. [Bayle (1710), p 3077.]

Mathematical points were easily dispensed with:

> ... several nullities of Extension joined together will never make an Extension. [Bayle (1710), p 3077.]

Regarding Epicurean atoms, which were characterized as extended and indivisible, Bayle observed that any atom of any extent, no matter how small, will have a left side, a right side, an upper and a lower side; it is therefore,

> ... a conjunction of distinct bodies; and I may deny of the right side what I affirm of the left side. [Bayle (1710), p 3078.]

Now, since the left and right side are not in the same place they must be separable:

> ... and therefore the Indivisibility of an Atom is meerly Chimerical. [Bayle (1710), p 3078.]

Bayle concluded:

> If therefore there is such a thing as extension, its parts must be divisible *in infinitum*. [Bayle (1710), p 3078.]

Therefore no two points could touch because for any two parts there must be an infinite number of parts between them; hence no two parts could touch, and so there can be no extension since extension requires immediate contact of parts. This does not seem the best that Bayle could have said; he could have said, simply, that if there is an infinite number of parts between any two parts, then motion is impossible because in order to move one would have to cross an infinite number of parts and so on ad infinitum. However, it is doubtful that there is such a thing as an actual infinite between any two parts as Bayle supposed. Bayle continued:

> But on the other side, if they cannot be divisible *in infinitum* we must conclude that the existence of extension is impossible, or at least incomprehensible. [Bayle (1710), p 3078.]

This follows from the fact that whatever cannot be divided cannot be composed of parts; therefore it cannot have a left side, nor a right side, etc. Hence it is not extended.

We can see in these considerations a possible source for Berkeley's worries about atomism, infinity, extension and motion. Bayle's comments in Remark H of the same article are also of some interest: for example,

> The proofs which reason furnishes us, of the existence of matter, are not evident enough to furnish a good demonstration on that head. [Bayle (1710), p 3078.]

To arrive at this conclusion Bayle employed two principles: Nature does nothing in vain; and, It is vain to do by several means what can be done by fewer with the same ease. These principles imply that, whether bodies exist or not, God could exite in us all the perceptions which we have of an external world, without using bodies as his instruments; "bodies" is here understood to mean extended and coloured objects that exist independently of perceiving minds and are like the ones we perceive. We have no way of proving from the evidence of our senses that there are no such bodies outside our minds.

Bayle commented on Descartes' appeal to the trustworthiness of God, that he does not deceive us regarding the existence of bodies. According to Bayle, Descartes provided no demonstrative reason, since not only must God's existence and trustworthiness be demonstrated, but also that he has assured us that there are bodies, and this he has not done. God would not be a deceiver if there were no bodies; in that case the error would be ours, not God's. Bayle's appeal to the divine economy would doubtless have caught Berkeley's eye.

(Berkeley refers to Bayle in the *Philosophical Commentaries* where he argued that Bayle's arguments work against bodies but not against space [PC, pp 283 and 290].) An appeal to divine economy would, if true, rule out the creation of a useless material realm: useless because God could create our sensations without the interposition of objects as the occasion of them.

Section 3: Method: On Being Clear and Distinct.

Berkeley began both *de Motu* and the *Principles* with a brief treatment of language and the errors mistaken views about words can and do cause. Malebranche did not do this: he began the *Search* with a treatment of the cause and nature of error, reserving until later such treatment of words as he will give. But the parallel is clear: both authors began with a treatment of the cause of erroneous thinking and its cure. For example, in the context of a discussion about the importance of clear and distinct ideas, using the relative terms "pure" and "impure" as his examples, Malebranche wrote,

> Philosophers should refrain from saying that matter is "pure" or "impure" unless they know exactly what they mean by these words, for one should never speak without knowing what one is saying, ie, without having distinct ideas corresponding to the terms one is employing. Now, had [the scholastics] affixed clear and distinct ideas both these words, they would see that what they call pure is often quite impure, and what seems impure is often quite pure. [Malebranche (1980a), p 83.]

The important lesson here is *We must reason only on the basis of our clear ideas*. In physics, a body must be thought of as extended, divisible, numerable and mobile; and this will make it possible to treat natural phenomena both mathematically and geometrically, that is, mechnically. Henceforth, science must avoid all accounts of natural phenomena employing the Aritotelian terminology of what Malebranche called,

> ... those lovely words: genus, species, act, potency, nature, form, qualities, cause in itself, and accidental cause. [Aristotles's] followers have trouble in understanding that these words signify nothing, and that they are no more learned than before just because they are heard to say that fire dissolves metals because it has a dissolving faculty. [Malebranche (1980a), p 443.]

What clear ideas correspond to those terms? According to Malebranche, none: the only ideas that correspond are vague ones of cause and being in general; are they are used tautologically, as in the above example of the dissolving faculty of fire. In Elucidation Twelve, Malebranche wrote,

> ... it is especially in matters of physics that we take advantage of vague and general terms that do not call up distinct ideas of being or modes. [Malebranche (1980b), p 642.]

Malebranche went on to give as an example the way the scientists use the term "gravity" and similar terms which,

> ... call up the idea of neither a being nor a mode. They are terms devoid of all sense, which wise people ought to avoid. [Malebranche (1980b), p 643.]

Echoes of this remark can be found in *de Motu*, paragraphs 3, 4, 6, 7 and 23. Later, in his next paragraph, Malebranche wrote,

> It seems to me that this is certain and easy to understand. Yet most men speak freely of all things without troubling themselves to examine whether the terms they use have a clear and distinct meaning. [Malebranche (1980b), p 643.]

Again, echoes of this remark can be found in *de Motu*, paragraphs 1 and 3.

Malebranche concluded this Elucidation by reflecting that it is only because we use a term thoughtlessly many times that we come to think of it as being perfectly understood; this belief was espoused by Berkeley in *de Motu*, paragraph 7.

The elimination of all this metaphysical baggage from physics does not render it useless because in physics,

> ... it is necessary to admit only notions common to all men, ie, the axioms of geometers and the clear ideas of extension, figure, motion and rest. [Malebranche (1980a), p 484.]

In passing, Malebranche met the objection that the essence of matter is not extension: surely it does not matter one way or the other provided our conception of the world is similar to the one we perceive - even if it is not made of matter "of which we know nothing, and about which they nevertheless make so much fuss." [Malebranche (1980a), p 484.] And what matters most of all in this respect is:

> We should only be careful that the reasonings we make about the properties of things are in agreement with our sensations of them, ie, that what we think is in perfect agreement with experience, because in physics we try to dicover the order and connection of effects with their causes, either in bodies, if there are any, or in our sensations, if they do not exist. [Malebranche (1980a) p 483.]

Much of what is contained in these two passages will be familiar to anyone reading *de Motu*, especially paragraphs 22, 28, 31, 49 and 71. It is fair to say that Berkeley is arguing for the truth of these paragraphs, especially paragraph 49 which sums up so much of what Berkeley has to say in *de Motu*, paragraphs 35 to 42.

Section 4: What is Occasionalism?

What then is the doctrine of occasionalism? It can be defined as the doctrine that things or events are caused only by God, never by other things or events. Ont his view apparent causes are what God uses as the occasion ("occasional causes" ,ie, instruments) for creating their apparent effects. For example, when one ball strikes a second and this one moves, God is the real cause of the second ball's motion - the first ball is just the occasion or instrument of motion in the second ball; this understanding of "occasional cause" Berkeley captured when he explained it as:

> ... something that is observed to accompany, or go before it, in the ordinary course of things. [DM, p 98.]

The view occasionalists opposed is the view that causal agents have within themselves the power to bring about change, to make a particular event inevitable. Occasionalists argued that there are no such powers in nature but only regularity, as defined above. This regularity is the work of God, who has, it is true, linked phenomenal events together; but he has not done it by creating any linking-entities between them. Malebranche replaced the notion of causal power with the scientific notion of law:

> All natural forces are therefore nothing but the will of God, which is always efficacious. God created the world because He willed it...; and he moves all things, and thus produces all the effects that we see happening, because He also willed certain laws according to which motion is communicated upon the collision of bodies; and because these laws are efficacious, they act,

> whereas bodies cannot act. There are therefore no forces, powers, or true causes in the material, sensible world; and it is not necessary to admit the existence of forms, faculties, and real qualities for producing effects that bodies do not produce and for sharing with god the force and power essential to Him. [Malebranche (1980a), p 448-9.]

It is clear from this passage that Malebranche was rejecting the opinion of those who held that between God and phenomenal events there is a layer of forces or qualities and that it was these that were the proximate cause of the events perceived: to understand occasionalism more fully it is necessary to know something of this opinion, which it rejected.

Section 5: What Occasionalism is Not.

This view, which dominated seventeenth and eigtheenth century European thought, presented the world not as a mere random assemblage of parts but as a structured and coherent whole: it was a world view amplified and to some extent created by scientists. According to this view, events are consequences of general laws, eg, the motion of the earth round the Sun is explained by Kepler's three laws; these in turn are explained by yet more general laws, such as gravity and inertia. Finally, these basic laws which govern bodies are explained as the immediate effect of God's will: bodies attract one another, Why? Because God so wills it. Cudworth, trying to escape mechanism without falling into occasionalism, wrote,

> Wherefore, since neither all things are produced fortuitously, or by the unguided mechanisms of matter, nor God himself may reasonably be thought to do all things immediately and miraculously; it may well be concluded, that there is a plastic nature under him, which, as an inferior and subordinate instrument, doth drudgingly execute that part of his Providence,

> which consists in the regular and orderly motion of matter. [Cudworth; quoted in Jammer (1957), p 151.]

In his *The Immortality of the Soul*, Henry More came to a similar conclusion: this plastic nature, this spirit of nature is

> ... a substance incorporeal, but without sense and animadversion, pervading the whole matter of the universe, and exercising a plastic power therein according to the sundry predispositions and occasions in the parts it works upon, raising such phenomena in the world, by directing the parts of the matter, and their motion, as cannot be resolved into mere mechanical powers. [More (1659); quoted in Jammer, p 151*f*.]

In his *Enchiridion Metaphysicum*, More concluded "Nam sic mobilia omnia moventur a Deo" ["for in this way are all moving objects set in motion by God"]. [More (1671); quoted in Jammer, p 152.]

Similar views are found in some of Newton's followers; witness Cotes in his preface to the second edition of *Principia*:

> Without all doubt this world, so diversified with that variety of forms and motions we find in it, could arise from nothing but the perfectly free will of God directing and presiding over all.
>
> From this fountain it is that those laws, which we call the laws of Nature have flowed, in which there appear many traces indeed of the most wise contrivance, but not the least shadow of necessity. [Cotes, in Newton (1729), p XXXII.]

In short particular events are effects of nature's laws, and those laws are the effects of God's will, whose vicarious power is somehow embodied in plastic nature;

such a view can even be detected in some of the works of Newton himself:

> The main business of natural philosophy is to argue from phenomena without feighning hypotheses, and to deduce causes from effects, till we come to the very first cause, which certainly is not mechanical; and not only to unfold the mechanism of the world, but chiefly to resolve these and such like questions. What is there in places almost empty of matter, and whence is it that the Sun and the planents gravitate towards one another, without dense matter between them?... To what end are comets, and whence is it that planets move all one and the same way in orbs concentric, while comets move all manner of ways in orbs vey eccentric and what hinders the fixed stars from falling upon one another? [Newton in Thayer (1953), p 156.]

For Newton the answer must be God because whatever non-dense matter is, it must be, at least by implication, something God supplies. But, be this as it may, "vicarious power", "plastic nature", and "non-dense matter" are precisely the sort of terms the occasionalists were to oust from science and philosophy.

<u>Section 6: The Occasionalists' Question</u>.

The question raised by the occasionalist philosophers, men such as Malebranche, de la Forge, Cordemoy, Geulincx, was If God's will is responsible for nature's laws, and those laws cause particular events, is it not now being supposed that these basic laws constitute something which intervenes between God's will and particular events? And, more importanly, what purpose could such a layer serve? The proposals we have seen say, in effect, that there are causal intermediaries between God and the world, ie, that there are forces or powers or a plastic nature created by God and that this intervening layer determines all events. Hence gravitational attraction is a result of a gravitational force or some power embodied in this plastic nature, and so on.

Descartes and his followers, men such as those listed above with the addition of Rohault and Huygens, had opposed this view of force and powers on the grounds that we can have no clear and distinct idea of them, save the abstract one that "they are the cause of" whatever effects they are postulated to explain. Moliere's well known example of such an occult power is opium's dormitive power: we have no immediate are independent knowledge of this power other than that we know it is the cause of the effects it purports to explain. It tells us no more than that opium has the power to put us to sleep: that much we knew already.

If that was the best science could do, then it was as guilty as scholasticism had been of concocting empty explanations. The Newtonians, that is such men as Cotes, Bentley, Clarke, Whiston, Derham, Horsley, and Baxter incurred much criticism for their belief in gravity: surely this was *the* occult force par exellence. In fairness it must be admitted that some of Newton's followers did deserve such criticism because they were misinterpreting Newton; but Newton himself was quite clear about where he stood on the matter: as we have seen, he states repeatedly in the *Principia* that gravity is not an occult force or quality but an effect whose cause is unknown to him.

Section 7: Rival Views of Causation.

There were in Malebranche's view two concepts of nature competing with one another. One has it that nature, plastic or otherwise, is a dynamic storehouse of force, powers and efficacious secondary causes:

> ... ordinary philosophy furnishes [atheists] with enough to blind themselves and to support their mistakes, for it speakes to them of certain impressed virtues, of certain motor faculties, in short, of a certain nature that is the principle of motion in each thing; and although they have no distinct idea of this thing, they are complacent enough, because of the corruption of their hearts, to substitute it for the true God, imagining that it causes all the wonders we see. [Malebranche (1980a), p 466,]

The other view has it that phenomena, though behaving in a perfectly uniform and regular manner, are connected by the temporal relations of before and after; but we must not allow ourselves to be deceived by this fact:

> Finally, because God resolves from all eternity to create certain things in a certain time, one could also say that these times would be the causes of the creation of these beings; just as one claims that a ball collides with another is the true cause of the movement it communicates to it, because God willed through His general will, which causes the order of nature, that when two bodies collide such a communication of motion occurs. There is therefore only one single true God and one single cause that is truly a cause, and one should not imagine that what preceeds an effect is its true cause. [Malebranche (1980a), p 451.]

The notion that a correct anylysis of causation involves its being analysed into the temporal relations of before and after was shared by Berkeley and is clearly expressed in *de Motu*, paragraph 71. It is clear that both men believed that the true cause of an effect was not some earlier event, but God.

Malebranche held that the idea of cause involved not the idea of force or power at all, but the idea of necessary connection: if one event occurs, so *must* the other. Such a connection is not found to exist in nature between any two events, but it is found to exist between God's will and the motion of bodies,

> ... since the idea we have of all bodies makes us aware that they cannot move themselves, it must be concluded that it is minds which move them. But when we examine our idea of all finite minds, we do not see any necessary connection between their will and the motion of any body whatsoever....
>
> But when one thinks about the idea of God, ie, of an infinitely perfect and consequently all powerful being, one knows there is such a connection between His will and the motion of all bodies, that it is impossible to conceive that He wills a body to be moved and that this body not be moved. [Malebranche (1980a), p 448.]

What is found is that a certain type of of event is invariably linked to another type of event in accordance with the temporal relations of before and after. What type of event are so linked is not given a priori but is discovered a posteriori and proved experimentally wherever possible - rendered certain by induction as Berkeley and Newton would say; but the only element common to all pairs is the temporal relation of before and after. God does not will that this particular event follows some other event; instead, God rules all things by a few general volitions; eg, he has a general volition to the effect that *every* body will tend to move in a straight line; that *every* body at rest will remain at rest, etc.

According to Malebranche, neither our senses nor our reason give us any idea of causal power: our senses reveal no transfer of moving force; our reason demonstrates that the characteristics of body contain nothing that suggests to us any idea of power:

> When I see one ball strike another, my eyes tell me, or seem to tell me, that the one is truly the cause of the motion it impresses on the other, for the true cause that moves bodies does not appear before my eyes. But when I consult my reason I clearly see that since bodies cannot move themselves, and since their motor force is but the will of God that conserves them successively in different places, they cannot communicate a power they do not have and could not communicate even if it were in their possession. For the mind will never conceive that one body, a purely passive substance, can in any way whatsoever transmit to another body the power transporting it. [Malebranche (1980b), p 660.]

It will be obvious when we come to inspect *de Motu* itself that Berkeley found such passages as the one just quoted highly suggestive, offering him a way of refuting scientific realism, while at the same time giving God a fundamental rôle in the philosophy of science.

Section 8: The Occasionalist' Solution.

The solution the occasionalists offered as a way of preventing the reintroduction of occult forces into science was to suppose that God's will is *identical* to the fundamental laws of natural science, but not their cause:

> ... the study of nature is false and vain in every way when true causes are sought in it other than the volitions of the Almighty, or the general laws according to which He constantly acts. [Malebranche (1980b), p 662.]

And,

> God does everything in all things, and nothing resists Him. He does everything in all things, His volitions produce and regulate all motion,... [Malebranche (1980b), p 664.]

On this view, particular events are explained by general laws, but these laws are not now supposed the mysterious effects of the divine will, but the most general of God's volitions:

> God does not multiply His volitions without reason; He always acts through the simplest ways, and this is why He uses the collision of bodies to move them, not because their impact is absolutely necessary for their motion, as our senses tell us, but because with impact as the occasion for the communication of motion, very few natural laws are needed to produce all the admissible effects we see. [Malebranche (1980b), p 663.]

But there are no intermediaries falling between God's will and the world of phenomenal events:

> ... there is only one true cause because there is only one true God; that the nature or power of each thing is nothing but the will of God; that all natural causes are not true causes but only occasional causes....
>
> We must therefore say that only His will can move bodies if we wish to state things as we conceive them and not as we sense them. The motor force of bodies is therefore not in the bodies that are moved, for this motor force is nothing other than the will of God. Thus bodies have no action; and when a ball is moved, collides with, and moves another, it communicates to it nothing of its own, for it does not itself have the force it communicates to it. Nevertheless, a ball is the natural cause of the motion it communicates. A natural cause is therefore not a real cause but only an occasional cause. [Malebranche (1980a), p 448.]

So instead of saying, "The apple fell because it was drawn towards the earth by a force directly proportional to the product of the masses of the earth and the apple, and inversly proportional to the square of the distance between them," we should say, "On the occasion of the apple's, God wills that it is drawn towards the earth at a rate directly proportional to etc, etc." In the first case, we attribute the fall to some mysterious force; in the latter, we attribute it to the immediate effect of God's will, now identified with the most general of nature's laws.

The question posed by the occasionalists was: What need has an all powerful God for such occult intermediaries as the above mentioned forces, powers and even, for that matter, secondary causes if all the effects that can be observed are ultimately the product of his will? Secondary causes, defined in Ephraim Chamber's *Cyclopaedia* as being, "those which derive the power, and faculty of acting, from a first Cause," [Chambers (1741)] seem especially open to occasionalist criticism. Berkeley may have been well aware of this fact - the definition continues:

> Such causes do not properly act at all; but are acted on: and therefore are improperly called Causes: of which kind are all those that we call Natural Causes. [Chambers (1741).]

And so the occasionalists, mindful of Ockam's Principle, answered their question with the reply that God has no need of such things at all. And, it may be added, since they are unnecessary, we need not postulate them; this amounts to saying that they do not exist at all. I take this to be one of the main points that Berkeley is arguing for in *de Motu*.

Section 9: Source of Our Mistaken Views about Causation.

So where does our idea of causal efficacy come from? We know that bodies cannot move themselves; we also know that there are two fundamental sorts of things, minds and bodies; hence, given the foregoing, we should conclude that the origin of motion should be minds. We are misled into believing that there are such things as secondary causes by the fact that we always observe one event to be invariably preceede by another; and this invarience we never see violated.

Because all natural phenomena follow one another in predictably regular patterns, we are prone to suppose that the events that follow are produced by those that invariably preceed them. But the connection is not a necessary one:

> A true cause ... is one such that the mind perceives a necessary connection between it and its effect. Now the mind perceives a necessary connecton only between the will of an infinitely perfectly being and its effects. Therefore, it is only God who is the true cause and who truly has the power to move bodies. [Malebranche (1980a), p 450.]

As we have seen, Malebranche supposed that the number of God's general laws to be few, and that by their collective operation they produce all the events occuring in nature. Hence God's will is the sufficient and necessary condition of the occurance of any event; but when we suppose the existence of forces, powers, and secondary causes, we do something for which there is no need: talk of forces, etc will only entagle us in unnecessary problems relating to occult forces. Clarity and economy of thought demand that all talk of secondary causes and forces be given an anti-realist construal.

Section 10: God and Science - a Criticism.

An objection brought against occasionalism was that it required God to intervene constantly in the workings of his creation: God, it seems, must be busy at every instant with the production of nature's effects. This objection was part of the greater controversy over whether religion was the only authoritative means of reaching the truth, which fact science must acknowledge; or, Is science an independent, though limited, means which religion must acknowledge? This controversy had its modern origins in the works of Copernicus, Osiander, Galileo, and Bellarmine. If science could rid itself of religion and claim for itself a large degree of epistemic independence, it would then be in a position to say what is or is not in the world. Science would become a purveyor of truth and falsity; God would be edged off stage and the only forces would be the so-called secondary ones. The view seems to be that of science trying to come of age and of being stopped by religious orthodoxy: this is a travesty. The argument was at bottom about what could count as knowledge and understanding: Berkeley tried to accommodate both a metaphysical and a scientific view of understanding in the *Principles* and *de Motu*. Genuine understanding of the world requires knowledge of God's ends and purposes; science cannot supply this - it can only deal in increasingly more general concepts from which particular events are deduced and, as we shall see, by which they are explained. Berkeley was trying to accommodate these two concepts of explanation; but Leibniz and Clarke in the *Leibniz-Clarke Correspondence* [Alexander (1956)] had no such intention. Leibniz held that Newton's *Principia* was non-explanatory: *everything* that science needs to know in order to understand why things happen, in the way that they do, is present in nature. If this is not accepted, then recourse to miracles is necessary. Clarke, for his part, was quite unmoved: scientific explanation appeals ultimately to a few basic regularities whose explanation requires an appeal to the workings of the divine mind. The argument was about the grounds and limits of explanation: that explanation has a limit was not in dispute; what was in dispute was where exactly the limit is. Leibniz wanted to include in science things more properly belonging to theology and metaphysics; Clarke wanted to exclude such things, which exclusion will leave mechanics, mathematics, motion and geometry - these he thought would be all that is required in order to explain any natural phenomena.

Does this mean that Newton had occasionalist leanings? I thimk it may, though Newton was careful to conceal the fact from the readers of the *Principia*. But it is clear from his refusal to accept ad hoc explanatory hypotheses and from his failure to generate a causal model relying solely on the three laws to explain gravity, that his only alternatives were to follow either Leibniz or the occasionalists: obviously only the latter was open to him. This is the source of the interest and the relevance of the *Correspondence* to this section.

In the following extracts, the dispute is about the nature of miracles and about the status of scientific laws. Let Leibniz begin:

> If God would cause a body to move free in the aether round a certain fixed centre, without any other creature acting upon it: I say, it could not be done without a miracle; since it cannot be explained by the nature of bodies.
> [Alexander (1956), p 30.]

Berkeley might not have disagreed with this as it stands. However, he would certainly have balked at Leibniz' solution, namely, that God created the ultimate constituents of reality - monads - in such a way that they had from the moment of their creation the power and the tendency to develop spontaneously all their future states in succession according to God's initial plan, without any interaction with other things and without any further special action of God. This was the theory of the Pre-Established Harmony. This, according to Leibniz, ensured the regular, continued, uniform and continued operation of the universe. In his attempt to refute Leibniz' position, Clarke employed the notion of what it is for something to be usual; in other words, he attempts to show that a body's continued motion was not miraculous because it was perfectly usual and unremarkable. This is reminiscent of Malebranche's insistence, already noted, on clear and distinct conceptions, and it also looks forward to early passages in *de Motu*. It is worth noting that the *Correspondence* was published in 1717, and that *de Motu* was written in 1720. Usualness is activity according to a law:

> For a body to move in a circle round a centre *in vacuo*; if it be usual (as the planets moving about the sun,) 'tis no miracle, whether it be affected immediately by God himself, or mediately by any created power: but if it be usual, (as, for a heavy body to be suspended, and move so in the air,) 'tis equally a miracle, whether it be affected immediately by God himself, or mediately by any invisible created power. [Alexander (1956), p 35.]

Missing the important point - even a monster's construction obeys the most general physical laws - Leibniz replied,

> The nature of a miracle does not at all consist in usualness or unusualness; for then monsters would be miracles. [Alexander (1956), p 43.]

And continued in a later section,

> 'Tis also a supernatural thing that bodies should attract one another at a distance without any intermediate means and that a body should move around without receding in the tangent, though nothing hinder it from so receding. For these effects cannot be explained by the nature of things. [Alexander (1956), p 43.]

The phrase, "cannot be explained by the nature of things" indicates that Leibniz was an essentialist: for him, explanation must rest on the essential, ie, monadic, properties of objects, and ultimately by showing how the fact is in accord with the Pre-Established Harmony. So, on this view, a body's continued motion is explained when and only when this motion is shown to be the inevitable consequence of the body's essential properties.

Clarke did not think that his own understanding of "miracle" committed him to the belief that monsters are miracles, if what he said about being unusual was true: usualness must be included in the notion of a miracle because if it were not, natural things, such as planetary motion would, indeed, be miraculous; but they are not miraculous because they are perfectly usual. That is, monsters can be shown to be in accord with the general laws of science, even though they are themselves rare effects.

In a later passage, referring to a body in motion round a fixed centre, Clarke wrote,

> ... the means by which two bodies attract each other, may be invisible and intangible, and of a different nature from mechanism; and yet, acting

> regularly and constantly, may well be called natural, being much less wonderful than animal-motion, which yet is never called a miracle. [Alexander (1956), p 53.]

Leibniz would have none of Clarke's employment of "usualness" in the latter's clarification of "miracle" because if the difference between the miraculous and the natural is only apparent, so that we call events miraculous only when those events are seldom seen, then,

> ... there will be no internal real difference, between a miracle and what is natural. [Alexander (1956), p 91.]

Clarke said that he could not understand what Leibniz meant by a "real internal difference". In the *Correspondence*, Leibniz defined the miraculous as that which surpasses the powers of creatures. But if a thing has something done to it which surpasses its powers, it undergoes a state not built into it at the moment of its creation. But how can this be, if God, in choosing its states, ie, willing the possible world of which it is a member, incorporates at the moment of its creation, *everyone* of its future states? The only alternative is that it can have a state not built into it to its concept and God can do what is impossible.

It is not known whether Leibniz received Clarke's fifth reply: it has not been found among his surviving papers; but despite this, Clarke, at least in the published correspondence, allowed himself the last word. With regard to an object's continuing in a regular orbit round a fixed centre, he concluded his argument with these reflections:

> That this phenomenon 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; it 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,) nevertheless true? Or is a manifest quality to be called occult, because the immediate efficient cause of it (perhaps) is occult, or not yet discovered?... if in some cases [a phenomenon] be not mechanically explicable, or be not yet discovered, what that something is; does it therefore follow, that the phenomenon itself is false? [Alexander (1956), p 118*f*.]

This is nothing if not perfectly orthodox Newtonianism: surely Newton was peering over Clarke's shoulder when it was being written - no talk here of gravity's being reified, or of the *Principia*'s being given a realist interpretation. This is clear evidence that Berkeley's beliefs about Newton are not without distinguished precedent.

Section 11: God and Essences - Further Criticism.

Another objection to occasionalism can be stated thus: States of objects are real causes because they determine when, where, in what direction, etc, God is to act; this is to say that they determine the precise character of his actions. For example, given the impenetrability of two objects and that they collide, then their direction after the collision must be different to what it was prior to it and the resulting change in their direction makes one of the bodies the real cause of at least the determination of the change in the other. Bayle had suggested that either God would stop both bodies, since he had no decree concerning the case, and nothing can exist without God's decree, or he would issue an appropriate decree on the spot. But neither alternative reaches the objection that given their impenetrability there is a necessary connection between the state of colliding bodies and some future state, whatever that state might be. Bayle replied that a real cause must be capable of actually producing the effect; and it is this and only this that is precluded from the notion of an occasional cause - but an occasional cause may still determine the real cause, God, to act and in only one way.

What the above objection finally amounts to is that occasionalism is incompatible with essential characteristics of any sort because to the extent that a thing has essential properties and ceteris paribus conditions can be met, it enters into necessary connections that limit God's actions, perhaps to a single possibility, thus qualifying it as a cause of what God produces.

The weakness of this objection is that *any* chosen consequence may be derived from the conjunction of an initial premise and a ceteris paribus clause if the ceteris paribus clause is chosen with care. Another weakness is that the objection equivocates on the meaning of the word "necessary". On the one hand it is necessary that the universe behaves in a regular and law-like way - necessary, that is, if people are going to be able to live in it. But does that mean that this or that *specific* law is necessary? If, for example, it is said that water could not have been otherwise than that it boiled at 100°C rather that at 125°C unless compensatory changes were made in the material world, then an oblique appeal is being made to realism. The assumption is that the world is - more or less - as our theory says it is, and that facts about theories reflect facts about the world. Berkeley would perhaps say that necessity is a feature of language (if it's a feature of anything) not of the phenomenal world. Berkeley would cetainly say that God could do as he pleases regarding the temperature he decides water should boil; we, on the other hand, would have to make compensatory changes in our theory. If we consider the first alternative, we find that without regularity of some sort it would be, given the nature of the regularity to be found in this universe,

> ... useless to plow [sic], water, and dispose bodies to prepare them for what we hope will happen to them. [Malebranche (1980b), p 663.]

Without some sort of regularity the Universe would be a chaos and we could not live in it. This is precisely Berkeley's point [see, for example, Principles, pp 86 and 95-6]. And, further, I do not think Berkeley would have balked at denying that impenetrability was an essential characteristic of objects; rather, he would say that this concept is derived from the observed fact that bodies do not occupy the same place. Whether Berkeley would in fact have agreed that an object need have no essential characteristics, including extension, is hard to say. Clearly, our concept of an object includes that of extension and this makes extension an essential characteristic; so when two objects meet something must happen and this seems to limit God. Perhaps Berkeley would have said that God need not have created extended objects: he could have created us in such a way that we dealt directly with the volitions of the divine mind, which are non-extended. This may seem a little fanciful, but it does offer a way out for Berkeley.

Section 12: Origins of Malebranche's Occasionalism.

It is interesting to note that Descartes was the historical source of the ideas which both Newton and Malebranche found so suggestive and stimulating. It seems that behind *de Motu* lie both Newton and Malebranche; behind them *both* lies Descartes, which say something of the extent of Descartes's influence - not for nothing is he called "the father of modern philosophy". He could also be called "the father of modern science".

Although Descartes exerted great influence on Malebranche, he denied that occasionalism was true. Yet its origins can be detected in some of his early writings. This denial took the form of a declaration in favour of secondary causes, in whose existence Descartes firmly believed. Descartes believed that if God is the immediate cause of our sensations, there would be nothing to prevent us from thinking of him as a deceiver because,

> ... we clearly understand this supposed thing to be completely distinct, not only from God, but also from us or our mind. Moreover, we seem to see clearly that the idea of it comes from external things, which it perfectly represents; and,... it is completely contrary to God's nature to be a deceiver. [Descartes (1983), p 39.]

There is more to this argument than there appears to be. The fact that our sensations incline us to believe that there are material objects whose nature is as we clearly and distinctly understand it to be does not in itself prove that material objects exist, or that God would be deceiving us if they did not. Rather, it is the fact that we cannot verify or refute this belief by using our reason that would make it a deception if it were untrue:

see *Principles* II, section lx. Many of the beliefs that arise from our senses are false according to Descartes but these can be known to be false: see for example *Principles* II, sections iii and iv. Yet Descartes could write to Princess Elizabeth that it would be contrary to God's perfection if something could happen uncaused by him:

> ... God is the universal cause of everything in such a way as to be also the total cause of everything; and so nothing can happen without His will. [Descartes (1970), p 180.]

We have already noted Bayle's objection to this proof, namely that not only must Descartes first demonstrate that God exists and that he is trustworthy, but also that God has assured us that there are bodies. And this Descartes has not done; God would not be a deceiver if there were no material bodies: the mistake would be ours, not God's. Descartes came especially close to occasionalism in his treatment of the problem of the communication of substances; that is, the means by which a determinate substance communicates one of its modes to another such substance. This problem was not a problem just about the relation between mind and body, it was a problem also about the relation between minds and minds and between bodies and bodies. According to Descartes, all communication between bodies was by way of impulsion, that is, by either pulling or pushing. Now, since motion is a mode of a body at a certain time and because Descartes accepted the scholastic principle, "Accidentia non migrant e substantis in substantias" ("Modes do not go from substance to substance"), the question, "How can one moving object be the cause of motion in a second one?" became accute. If the motion qua mode is not numerically identical with the motion qua mode of the second object how can the motion of the first be said to be the *cause* of the motion in the second?

> The translation which I call motion, is a thing of no less entity than shape: it is a mode in a body. The power causing motion may be the power of God Himself conserving the same amount of translation in matter as he put in it the first moment of creation;... but because this is not easy for every one to understand, I did not want to discuss it in my writings. I was afraid of seeming inclined to favour the views of thoses who consider God as a world-soul united to matter. [Descartes (1970), p 257.]

It is easy to see why a number of thinkers thought that Descartes was an occasionalist.

Section 13: An Important Divergence.

This section will serve as an introduction to the main body of this thesis, namely my treatment of *de Motu* itself.

An important divergence between Berkeley and Malebranche was their conflicting beliefs about human action. Berkeley held that all spirits are capable of voluntary acts and hence are true causal agents. Malebranche denied this: God is the cause of both the determinations of our will and the movement of our bodies.

We know that Malebranche held that causation involved a necessary connection and that the mind perceives such a connection "only between the will of an infinitely perfect being and its effects". Berkeley believed that human beings are responsible, some how or other, for the motion in their limbs:

> ... That there is in [thinking things] the power of moving bodies we have learned by personal experience, since our mind at will can stir and

> stay the movement of our limbs, whatever be the ultimate explanation of the fact. [DM, p 215-6.]

Quite what the "ultimate explanation of the fact" is, Berkeley never tells us. And in anycase, how is the view reconcilable with holding that bodies are mere collections of ideas and that God is the cause of those ideas? What does it mean to say that "our minds at will can stir and stay the movement of our limbs"? It appears to mean no more than that my volition is accompanied by certain internal sensations of muscular effort and certain visual and tactile sensations of my limbs in motion. According to Berkeley, a mind cannot be the cause of those muscular, visual and tactile sensations because God is the cause of them as he is the cause of all ideas of sense. It seems that Berkeley should say that minds cause their volitions: this would not remove the distinction between Berkeley's view of causation and Malebranche's, since it was Berkeley's aim to guarantee the liberty of the will - something that is assured if minds are able to determine their own choices.

If Malebranche were right and Berkeley wrong, it would mean that we do nothing, that we are lifeless instruments, that God does everything in us, that he plies our minds and wills as a workman wields his tools which can have no motion except that given them by the workman. No room here for human agency, decision or choice, and this makes a nonsense of any morality; this is clearly a positon that is unacceptable to Berkeley.

Berkeley makes the contrast between the efficacy of the human will and the inefficacy of objects an important one in *de Motu*. According to Berkeley our only knowledge of force, effort, etc comes from human physical activity:

> ... *solicitation* and *effort* or *conation* belong properly to animate beings alone. When they are attributed to other things, they must be taken in a metaphorical sense;... Besides, anyone who has seriously considered the matter will agree that those terms have no clear and distinct meaning apart from all affection of the mind and motion of the body. [DM, p 211.]

And,

> While we support heavy bodies we feel in ourselves effort, fatigue and discomfort. [DM, p 211.]

This effort or force is not itself occult because while,

> Animal effort and corporeal motion are commonly regarded as symptoms and measures of this occult quality, [DM, p 211.]

the terms "force", etc,

> ... have no clear and distinct meaning apart form all affection of the mind and motion of the body. [DM, p 211.]

In other words the term "force" is not the name of some unobservable mental cause of human effort and human action; it is the name we give to human motion and to certain dispositions or attributes of mind. The term "force" in its non-metaphorical and concrete sense means no more than the effort involved in resisting, pushing, etc. When I make an effort pushing a heavy object, I experience force not as a cause but as an effect of the effort I make: "force" is the name I give to certain muscular sensations and to certain visual and tactile sensations of motion. This inner experience of force will be repeatedly contrasted with physical force which is nothing over and above a body's motion. It is useful for scientists to attribute force to objects in the same way that we attribute agency to the human mind; this enables them to explain why one object can apparently push another one out of its way. This last remark contains the seeds of the problem which Berkeley was anxious to clear up in *de Motu*.

DE MOTU

Section 1: Introduction.

Every year the Academie Royal des Sciences ran an essay competition whose subject was a scientific topic: in 1720 the subject of the essay was motion - in response, Berkeley wrote *de Motu Sive Principio & Natura et de Causa Communicationis Motuum.* Berkeley's first editor, A C Fraser, found no evidence of the essay's being submitted. Possibly, Berkeley did not think it worth entering the competition: his anti-realist, pro-Newtonian views would not have endeared him to many scientist or philosophers on the continent at that time. It is somewhat ironic that the then permanent secretary, Bernard le Bovier de Fontanelle - a Frenchman and a Cartesian - would be the first biographer of Newton. In any event, the prize was won by M Crousaz, professor of philosophy at Lausanne. Because the competition was an international one, *de Motu* was written in Latin. It was published by Berkeley in Latin in 1721 but it was not translated into English until A A Luce did so for his 1948 edition of Berkeley's complete works.

Section 2: The Structure of *de Motu*.

Berkeley's *de Motu* is written in seventy-two numbered paragraphs and divides naturally into five parts. The first part, paragraphs 1 to 18, deals with the construal of scientific language; the second, 19 to 34, with the origins of motion; the third, paragraphs 35 to 42, with mechanical principles; the fourth, paragraphs 43 to 65, with the nature of motion; and the fifth, paragraphs 66 to 72, with the communication of motion.

The first four parts form a coherent and structured whole; the third part is central and the first, second and fourth parts relate to it directly. The fifth part does not so relate to it and seems to have been added as an afterthought. I have two reasons for saying this:

> a) the first paragraph of the fourth part (paragraph 65) is a summary of the preceding four parts, not just of part four itself; and,
>
> b) in the first four parts, Berkeley elaborates the thesis that explanation need not be causal, ie, need not involve impulsion.

I do not think that a) is either problematical or controversial: it is, I think, clearly true. But b) is more complicated than that; as we have seen, to many mechanical philosophers on the continent the thesis expressed in the first four parts would be absurd - all explanation had to be in terms of impulsive causation and demonstrate that events were inevitable. Hence, in the fifth part, Berkeley argued that the account developed in the first four sections holds equally against the apparently unproblematic cases involving impulsion. Is this the reason why Berkeley did not submit *de Motu*, that the fifth part would have been unacceptable to the competion judges, all of whom, we may assume, were well schooled in Cartesian physics? I think that this is very probable; Berkeley knew he had not got a winning formula - after all, the *Principles* had already received an unsympathetic reception from continental critics. The reviewer in the *Mémoires de Trévoux* for May, 1713 said:

> Berkley [sic] ... has pushed without discretion *the principles of his sect* greatly beyond common sense,... [Quoted in Bracken (1965), p 16; italics mine.]

Things got worse; by 1718 Fénelon wrote, in his *Oeuvres Philosophiques*,

> A philosophy directly opposite to [Spinoza's], has been taking its place for some years past. The English book of one Berkey [sic], has published these new attempts at incredulity. The blasphemous of this sect say not that all is matter; they say all is spirit. [Quoted in Braken (1965), p 26.]

No wonder Berkeley did not enter *de Motu* in the competition!

Section 3: The General Thrust of Berkeley's Argument.

Berkeley begins the *Principles* with an analysis of language: he does the same at the beginning of *de Motu*, opening, and note the Baconian spirit of this emphasis, with this warning:

> In the pursuit of truth we must beware of being misled by terms which we do not rightly understand. That is the chief point.... It is not so difficult to observe, where sense, experience, and geometrical reasoning obtain, as is especially the case in physics. Laying aside ... all prejudice, whether rooted in linguistic usage or in philosophical authority, let us fix our gaze on the very nature of things. For no one's authority ought to rank so high as to set a value on his words and terms unless they are found to be based on clear and certain fact. [DM, p 211.]

Berkeley is doing two things here. First, he is drawing our attention to the danger of assuming that there are theoretical entities corresponding to the theoretical names used in science, much as he had done in the *Principles* where he points out that abstract general names do not name abstract entities. Second, Berkeley is suggesting the possibility of a linguistic analogy with Descartes' clear and distinct ideas: if a clear and distinct understanding of terms such as, eg, "force", "gravity", "solicitation of gravity", "conatus", "effort", "attraction", "dead force", and "living force" can be achieved, then muddles can be avoided. And of course if no such understanding is possible, then such terms must be eliminated. There is here a point of further comparison with the *Principles*: that, too, was written with the intention of eliminating muddles from human knowledge. When the terms have been reduced to "clear and certain fact", Berkeley will show that the above terms mean nothing over and above "motion", "moving", "moved" and "rest": a problem about theoretical terms becomes a problem about abstract ones. In *de Motu*, paragraph 3, Berkeley states his thesis:

> Besides, anyone who has seriously considered the mattter will agree that those terms have no clear and distinct meaning apart from all affection of the mind and motion of the body. [DM, p 211.]

Evidently, "effort" or "force" when used by physicists has a meaning different to the one it is when used by some one discussing human endeavour: such terms, he reminds us,

> ... belong properly to animate beings alone. When attributed to other things, they must be taken in a metaphorical sense. [DM, p 211.]

What exactly this "metaphorical sense" is I will attempt to show later; but for the moment we should note the important point, which Berkeley is making, that even when applied to human mental activity a term like "force" does not refer to some occult mental item or entity, but to human dispositions and motion itself:

> *Force* ... is attributed to bodies; and that word is used as if it meant a known quality, and one distinct from motion, figure, and every other sensible thing and also from every affection of the living thing. But examine the matter more

> closely and you will agree that such force is nothing but an occult quality. [DM, p 212.]

And concludes that,

> Animal effort and corporeal motion are commonly regarded as symptoms and measures of this occult quality. [DM, p 212.]

Berkeley is saying, rightly in my view, that objects do not have dispositions to act; to say that objects have such dispositions is to speak metaphorically, and philosophers should avoid metaphor; the failure to observe this injunction results in "darkening works otherwise very learned" by a misconstral of scientific terms. How has this misconstrual come about?

Section 4: Source of the Muddle.

Berkeley's answer to the question asked at the end of the last section is that a number of physicists were guilty of making the referent of theoretical terms real substantial entities which are the cause and therefore the explanation of the observed phenomena:

> While we support heavy bodies we feel in ourselves effort, fatigue and discomfort. We perceive also in heavy bodies falling an accelerated motion towards the centre of the earth; and that is all the senses tell us. By reason, however, we infer that there is some cause or principle of these phenomena, and that is popularly called *gravity*. But since the cause of the fall of heavy bodies is unseen and unknown, gravity in that usage cannot properly be styled a sensible quality. It is, therefore, an occult quality. But what an occult quality is, or how any quality can act or do anything, we can scarcely conceive - indeed we cannot conceive. And so men would do better to let the occult quality go, and attend only to the sensible effects. Abstract terms (however useful they maybe in arguments) should be discarded in meditation, and the mind should be

> fixed on the particulars and the concrete, that is, on the things themselves. [DM, p 211*f*.]

The basic import of this passage is right: physicists infer - infer to the best explanation - that the unobservable realm that they postulate to explain the observable realm is as their theory says that it is because their theory successfully explains the observable realm. This amounts to the claim that the unobservable realm is described truly by their theory; so, if we say a theory explains, we must also say it is true.

Section 5: Against Inferring to the Best Explanation.

Berkeley says, in effect, that to have good reasons for accepting a theory is *not* ipso facto to have good reasons for asserting that the entities, namely gravitational and impulsive forces, postulated by mechanics do exist as separate substantial entities. The realists had argued as follows: motion we experience as something originating within ourselves, that is, within us as a force that causes motion in our limbs; we infer that all motion, whether of animate beings or not, is also the result of such a force; regarding animate beings that are also articulate we can ask of them if their movement was forceful or not. Hence, in those cases involvng human beings, we can ascertain, independently of any inference, the truth of our belief that forces explain this or that human movement. The canons of rational inference, eg, consistency, simplicity, etc demand that we follow this rule everywhere, even when we cannot independently ascertain the truth of our inference; and this leads to scientific realism. If I say that real forces are responsible for the movements of your arm, then I must also say that real forces are responsible for the movement of the moon. Berkeley blocks this by pointing out that their corresponding terms have,

> ... no clear and distinct meaning apart from all affection of the mind and motion of the body. [DM, p 211.]

And in any case, motion, being an object of sense, is caused by God. As we have seen, we are not obliged to be realists about scientific theories if we accept a theory as explanatory - being a scientific explanation is no great feat; in effect it amounts to no more than "talking with the vulgar". According to Berkeley, the analysis of scientific explanation reveals that the explanation involves an implicit appeal, via the explaining theory's fundamental principles or axioms, to known regularities. But no appeal need be made to unknown causal qualities or entities. Berkeley, in the third and fifth parts of *de Motu*, is concerned to show that to accept theory as explanatory does not require that I must accept that the entities postulated by it do exist.

A more persuasive objection against Berkeley might go like this: I concede what you say about forces being nothing over and above motion, but that does not mean that I have to concede that objects themselves are unreal because surely I can still say that if a moving object hits another object something *must* happen. Whatever happens must be a consequence of certain essential characteristics of the objects themselves; if I say this in the context of impulsive causation, then the canons of rational inference demand that I say this about *all* those events that I say can be explained. Regarding gravity, therefore, I must either postulate a causal mechanism for it or say it is an essential characteristic of all bodies. Firstly, Berkeley would object that this begs the question; predictions claiming to demonstrate what *must* happen are parasitic upon descriptions of what *does* happen. Secondly, he might object that it is conceivable for objects not to gravitate towards one another. This is in the case of a universe whose only two objects are spinning round a common centre and prevented from moving towards that centre by tangential forces; but they can be spinning relative only to absolute space. But absolute space is an incoherent notion because the arguments for it make sense only if the distincton between it and relative space is assumed in the first place.

It may be noted in passing that in one sense Berkeley admits that the scientist is right: something is responsible for the phenomana of gravity which we see, something that *makes* them happen. Berkeley's contention is that it is not the job of science to discover what it is; rather it is the job of theology and metaphysics. But some of the details of the passage quoted in the previous section are unclear: is it true that all non-sensible qualities are occult? And is not Berkeley discussing theoretical terms, not abstract ones? Answering these questions will be the substance of the next section.

Section 6: Elucidating the Terms.

Regarding the first question the answer must be, at least for Berkeley, yes. The following was written by Daniel Sennert in 1632:

> Qualities are divided in respect of our knowledge into Manifest and Occult. The manifest are those which easily, evidently and immediately are known to us, and judged by the senses. So light in the Stars, and Heaviness and Lightness.... But occult or hidden qualities are those, which are not immediately

> known to the senses, but their force is perceived mediately by the Effect, but their power of acting is unknown. So we see the Load-Stone draw the Iron, but that power of drawing is to hidden and not perceived by the senses.... By our Senses ... we perceive Heat in the Fire, by means whereof it heats: but it is not so in those operations which are performed by occult qualities. We perceive the Actions but not the qualities whereby they are affected. [Sennert, quoted in Hutchison (1982), p 234.]

Berkeley would doubtless accept the substance of this passage: qualities which are occult are insensible by definition. But he is also making a stronger claim, namely that occult qualities are unintelligible. Which is Berkeley more concerned with: the unintelligibility or the insensibility of occult qualities? He wants his arguments to be based on the unintelligibility of occult qualities, but surely occult qualities can be made intelligible - we now know why opium sends us to sleep. Gravity, too, would be made intelligible if someone could devise a causal mechanism that explained how gravitational phenomena came about. Berkeley is generalizing carelessly from the failure of physicists to construct a mechanistic model to the impossibility of all such models. In truth, Berkeley is making his *esse est percipi* principle do the work: that which cannot be sensed cannot exist and cannot do anything. Hence, insensibility gives the desired conclusion; but to convince his readers he conflates it with a thoroughly false and misleading argument about unintelligibility. This point is by no means as flippant as it may seem. Berkeley, it should be remembered, was writing with a continental reader in mind, and one therefore deeply schooled in the Cartesian principle of clear and distinct ideas and likely, therefore, to accept this sort of criticism of occult qualities, though not one employing

Berkeley's *esse est pricipi* principle. Furthermore, it should be remebered that the controversy between the Newtonians and those accusing Newton of re-introducing occult qualities into physics had just reached a zenith with the publication of the *Leibniz-Clarke Correspondence*. Leibniz and most philosopher-scientists rejected Newton's notion of gravity as unintelligible because he did not construct a mechanical hypothesis to explain it. Newton's response to this was: "Hypotheses non fingo" and promptly to demolish their vortical hypotheses by demonstrating that such hypothese did not have the appropriate observational consequences. To Newton, however, intelligibility of this sort was not essential provided gravity satisfied other criteria, notably that the phenomena had been reliably detected, accurately measured and admitted of mathematical presentation. Clarke, insisting that observed effects must be accepted, even if their causes are unknown, replied to Leibniz' charge that gravity is a "chimerical thing, a scholastic occult quality", with a rhetorical question which allows the possibility that gravity may have an occult cause:

> [Is] a manifest quality to be called ... occult because the immediate efficient cause of it (perhaps) is occult? [Alexander (1956), p 119.]

Clearly, an epistemic separation is being insisted on between a discussion of effects and a discussion of causes; it is being maintained that one can detect effects reliably, whether or not one knows what the efficient cause is.

Berkeley was alive to the unintelligibility aspect of "gravity" when construed as some causally efficacious entity, and to the dangers of so construing it:

> ... *force*, *gravity*, and terms of this sort are more often used in the concrete (and rightly so) so as to connote the body in motion, the effect of resisting, etc. But when they are used by philosophers to signify certain natures carved out and abstracted from all these things, natures which are not objects of sense, nor can be grasped by any force of intellect, nor pictured by the imagination, then indeed they breed errors and confusion. [DM, p 212.]

Note how he weaves considerations of intelligibility and insensibility together. Furthermore,

> ... nothing enters the imagination which from the nature of the thing cannot be perceived by sense, since indeed the imagination is nothing else than the faculty which represents things either actually existing or at least possible. [DM, p 222.]

But the real crux of the matter is yet to come: if the cause of gravity is forsworn, is not explanation forsworn also? Berkeley thought not, as we will see when we come to look at at what he wrote concerning mechanical principles, in the third part of *de Motu*. But first we will see Berkeley's methods in action against some of the problems that were current when he was writing.

Section 7: The Force of Percussion.

The problem raised in *de Motu*, paragraph 9, and discussed further in paragraphs 10, 11 and 14, is the problem of how to measure the force of a hammer striking a nail; no matter how large the nail is, the hammer will always produce some noticeable effect. Yet if a heavy weight is placed on the nail, the weight would need to be very heavy indeed, if it is to produce the same effect. Is the weight the measure of the force? No, because if the nail receives a similar hammer blow, another similar effect will be observed; but if the weight is taken off and replaced on the nail, no effect like the first will be observed.

The source of Berkeley's interest in this problem is that its central issues - as articulated in Galileo's experiments [Galileo (1974)] - all impinge on Berkeley's central concern: the construal of scientific terms. Here, as always, the general import of Berkeley's treatment of this subject is that facts about theories do not entail facts about the world.

This problem, known as "The problem of the force of percussion", had had its origins in the works of Borelli, Torricelli, Mariotte and Huygens, all of whom had written about the problem. But historically the most important is Galileo. Galileo made and described several experiments, some actual and some thought, that were aimed at the discovery of the force of a body in motion. All his experiments revolved around the attempt to compare this force with the pressure of a weght at rest, hoping thereby to measure the force of percussion.

Galileo's experiments regarding the force of percussion are described in "On the Force of Percussion" [Galileo (1974)], in which Galileo tries, unsuccessfully, to find "the means of finding and measuring its great force" [Galileo (1974), p 323]. A large part of the discussion is give over to proving this dictum: *The force of percussion is infinitely great.* Some of the experiments described are very elegant and include experiments with a water balance, a pile driver, two suspended spheres, and two weights joined by a chord and then hung over a triangular prism. Galileo's arguments are similar to those he employed in the proof of what is now known as

Newton's first law. In should be noted in passing that Galileo's concept of inertia is different to Newton's. Galileo wrote:

> ... all external impediments being removed, a heavy body being placed on a sherical surface concentric with the earth will be indifferent to rest or to movement towards any part of the horizon. And it will remain in that state in which it has onced been placed; that is, if placed in a state of rest it will conserve that; and if placed in movement towards the west, for example, it will maintain itself in that movement. [Galileo, quoted in Drake (1970), p 251.]

Note that this is a very limited account of inertia, unlike Newton's which is universal in its applicability.

An experiment yielding the dictum is set up as follows. Two balls are suspended from the same point and in such a way that the distance between the centre of each and the point of suspension is the same; if the smaller if the two is allowed to fall along the arc of the circle which has as its radius the distance between the centre of the balls and their common point of suspension, then no matter how small the distance travelled, nor how great the difference in the weight of the two balls, the smaller will produce some motion in the greater. The only weight capable of resisting such a percussive force would have to be infinitely great; hence the formula *The force of percussion, however small, is infinitely great.* Galileo soon became convinced that he could never counteract by a vis mortua (ie, a dead weight), the effect of an instantaneous impact. Perhaps the source of Galileo's problem is his belief in instantaneous changes of velocity which would require infinite forces to effect.

Section 8: Berkeley's Construal of the Problem.

Berkeley's problem is to construe the dictum in such a way that the scientific enterprise is neither repudiated nor shown to be a metaphysical nonsense.

According to Berkeley the dictum requires those who affirm it to suppose that gravity is a real and substantial entity distinct from motion and that gravitational phenomena are somehow the effect of this entity. This supposition is required because,

a) the ball is stationary yet it resists a moving object; and,

b) it must get this capability from something.

Why Berkeley believed that gravity rather than inertia is be thought of as a real entity, distinct from motion and rest, is not clear.

Because vis mortua is not itself an effect involving motion it cannot be, according to Berkeley, a force at all. Hence, there is no force for the percussive force to be proportional to; for there to be a proportion which is neither an infinite nor a zero magnitude requires the vis mortua to be something other than motion. I take this to be Berkeley's argument in *de Motu*, paragraph 10: there is no dead weight, nor any force at all, because there is no motion and force is nothing over and above motion itself. So vis mortua cannot be, by definition, a force. Furthermore, there can be no ratio of forces, if there is only one force. And so, argues Berkeley, vis mortua is a non-existent force and not merely a force of zero magnitude. That this is Berkeley's view can be seen from *de Motu*, paragraph 14: for the ratio to be infinite, a finite part must be shown to be contained in the whole an infinite number of times. But the vis mortua/force of percussion is not such a relationship because it is a finite entity involving a finite movement. As Berkeley points out, the relationship is like that existing between a point and line: a geometrical point is extensionless and can be deemed to fit a line an infinite number of times. Berkeley's remark is close to Mach's:

> In reality, therefore, pressure [ie, vis mortua] is related to momentum of impact [ie, force of percussion] as a line is to a surface. [Mach (1893), p 402.]

Section 9: A Missed Opportunity - Conservation.

The problem of the force of percussion bears on another problem, which was current when Berkeley was writing, namely the controversy between the atomists and the conservationists. the atomists conceived the ultimate constituents of the universe to be perfectly hard and indivisisble atoms. That such a conception led to the belief in instantaneous changes of velocity and hence in infinite forces was cited by the conservatonists as an argument against the existence of atoms. It is striking, given his views about such entities, that Berkeley did not make some use of this argument in *de Motu*, paragraphs 9 to 14. And it is even more striking that he did not further employ the same argument to attack the distinction between primary and secondary qualities, a theoretical basis for which is found in atomism. Berkeley does comment on a related problem, namely the controversy that existed among the conservationists themselves about what it was exactly that was conserved: was it quantity of motion or was it vis viva?

The debate about whether it was quantity of motion or vis viva which was conserved, existed through the seventeenth and eighteenth centuries, and principally involved Descartes (and his followers) and Leibniz (and his); at bottom the dispute was about,

a) what were the effects produced by a moving body when stopped by either gravity or a collision? And,

b) what is the correct measure of a force of a body in motion and does the total amount of this force in the universe remain the same?

Descartes argued for the conservation of motion (what is now called "momentum") in the *Principles*, Part II, 36; Leibniz replied, arguing for the conservation of vis viva (related to what we now call "kinetic energy"), in "A Brief Demonstration of a Notable Error of Descartes and Others Concerning a Natural Law" [Leibniz (1969a)] and in "Critical Thoughts on the General Part of the *Principles* of Descartes." [Leibniz (1969b).] It is not known whether Berkeley read either of these works. Although it is known that "Critical Thoughts" was written in 1692, it is not known when it was published; "A Brief Demonstration" was published in the *Acta Eruditorum* for March 1686. Berkeley may have seen this; there is evidence in *de Motu*, paragraph 8, that he had seen at least one edition of the *Acta*: the reference seems to be to "Specimen Dynamicum" [Leibniz (1969c)], which was published in 1695. [See for example, p 436*f* of Leibniz (1969c).]

The controversy is relatively easy to explain today; "force of a body in motion" is ambiguous because it can be taken to refer to either the momentum or the energy of a moving body, and both these quantities are in fact conserved. Leibniz sought some active principle that was conserved and which therefore prevented the running down and eventual halt of the universe. He found this in the vis viva of matter which he measured by the product of mass and the square of the velocity; this is the conclusion Berkeley criticises in *de Motu*, paragraph 15. Descartes on the other hand insisted that quantity of motion, the product of mass and simple, non-vectoral speed was the quantity that was conserved. Thus Leibniz came close to stating the law of conservation of kinetic energy in mechanics, whereas Descartes came close to stating the law of the conservation of momentum. Huygens and others pointed out that momentum is only conserved when considered as a vector quality, and with this one correction the conservation of momentum was accepted by both sides in the dispute. But vis viva was not so easily accommodated.

Section 10: Descartes and Conservationism.

Descartes starts his treatment of the conservation of motion in the *Principles*, Part II, 36:

> That God is the primary cause of motion; and that he always maintains an equal quantity if motion in the universe.
>
> ... God Himself, who, ... in the beginning created matter with both movement and rest; and now maintains in the sum total of matter, by His normal participation, the same quantity of motion and rest as He placed in it at that time. [Descartes (1983), p 57f.]

It is important to note that by "quantity of motion" Descartes does not mean "momentum", ie, the product of mass and velocity. Rather, he intends quantity of motion to be given by the product of size (or volume) and speed. This is, of course, a result of his view that extension is the essential property of matter. Thus the behavior of bodies should be determined entirely by their extension. Descartes' preference for speed over velocity may be a consequence of his belief that the direction in which a body is said to be moving depends upon which other bodies are considered at rest. Therefore there is nothing in the body itself, which enables the body's direction of motion to be determined. Descartes continues:

> ... although motion is only a mode of the matter which is moved, nevertheless there is a fixed and determinate quantity of it;... [Descartes (1983), p 58.]

From the complete immutability of God's nature and the immutability and constancy of the way he acts, Descartes could deduce, or thought he could, the belief "completely consistent with reason" that,

> ... solely because God moved the parts of matter in diverse ways when He first created them, and still maintains all this matter exactly as it was at its creation, and subject to the same law as at that time; He also always maintains in it an equal quantity of motion. [Descartes (1983), p 58.]

While this may be "completely consistent with reason", it clearly does not follow. What follows. even on the most generous interpretation, is that the total quantity of something must remain constant. Quite what that something is, was the subject of the debate between Descartes' followers and those of Leibniz.

Section 11: Leibniz' Criticism.

In his criticism of the *Principles*, Part II, 36, Leibniz writes:

> [The Cartesians] have given no demonstration of [the conservation of the quantity of motion],... for no one can fail to see the weaknees of their argument derived from the constancy of God. For although the constancy of God may be supreme,

> and he may change nothing except in accordance with the laws of the series already laid down, we must still ask what it is, after all, that he has decreed should be conserved in the series - whether the quantity of motion or something different, such as the quantity of force. [Leibniz (1969b), p 393*f*.]

Leibniz follows this with a detailed argument aimed at establishing that,

> ... the quantity of motion is known to be the product of mass and velocity, while the quantity of force is,..., the product of mass and the altitude to which it can be raised by force of its power, altitudes being proportional to the square of the velocities of ascent.... Meanwhile this rule can be set up: The same quantity of force as well as of motion is conserved when bodies tend in the same direction both before and after their collisions, as well as when the colliding bodies are equal. [Leibniz (1969b), p 395.]

Leibniz' acknowledgement that the quantity of motion is conserved in the case of bodies that do not reverse their direction anticipates his own more general principles of the conservation of progress, which differs from Descartes' principle in considering the algebraic, not the arithmetic, sum of motions.

Leibniz' conclusion in "A Brief Demonstration" can be best summarized as follows. Let d be the distance, g a gravitational constant, v the velocity, and t the time. According to the law of falling bodies, $d = \frac{1}{2}gt^2$, but $v = gt$; hence $v^2 = 2gd$, or distances vary as the square of the velocities. More generally, Leibniz held that work accomplished, measured by the motion of a body through a horizontal distance, is proportional to a quantity of force accumulating through time and is therefore an integral or summation of successive initial impulses whose effects in velocity are conserved and accumulated. It is therefore proportional to v^2 rather than to v.

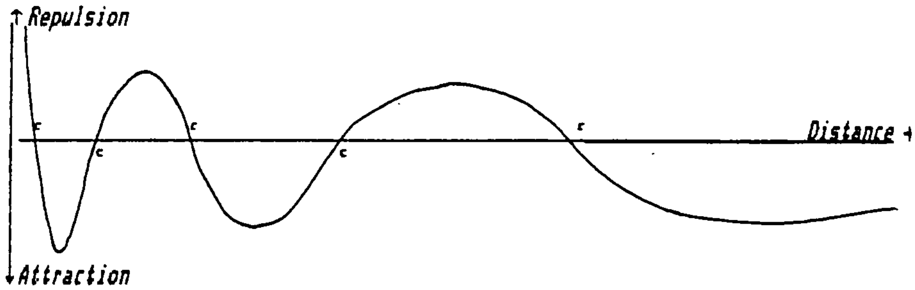
One of the most obvious criticisms that can be aimed at Leibniz' law of the conservation of vis viva is the frequency with which it is apparently violated as in the case of inelastic collisions. Leibniz had enough faith in his theory to postulate in his correspondence with Clarke that motion was retained in the small parts of the distorted inelastic body:

> 'Tis true, their wholes lose (some force) with respect to their total motion, but their parts receive it, being shaken internally by the force of the concourse. And therefore that loss of force is only in appearence. The forces are not destroyed but scattered among the small parts. The bodies do not lose their forces, but the case here is the same as when men change great money into small. [Alexander (1956), p 97-88.]

This view is, in effect, the one that Roger Boscovitch developed in his *Theoria Philosophia Naturalis* [Boscovitch (1961)].

Section 12: Boscovitch.

Leibniz' belief that an inelastic body's parts absorb the force lost when the body distorted is the essence of Boscovich's theory. According to his theory all matter is composed of non-extended mass points; these points exert forces on each other which vary with the distance between them; at very short distances there is a strong force of repulsion which increases to infinity as the points are brought together, preventing them from coming into contact. As the distance is increased, the force alternates between repulsion and attraction and finally follows the inverse square law of gravitational attraction for large distances. Boscovitch illustrated his theory with his famous curve:



At certain distances where the forces change from repulsion to attraction the mass points will be in stable equilibrium. Boscovitch called these distances "points of cohesion" (marked "c" on the diagram) and he used them to explain cohesion and the structure of matter. Such a theory elaborates Leibniz' and produces an explanation of how the motion of bodies in collision can be "scattered among the small parts".

There was no satisfactory model available for explaining and demonstrating the conservation of vis viva until Boscovitch invented his system; but even he did not see the significance of vis viva to his theory. The law of conservation of energy had to wait until the physicists of the nineteenth century established energy equivalents in a variety of phenomena and finally established the law on the basis of experimental evidence.

<u>Section 13: Berkeley's Clarification</u>.

This is the debate about which Berkeley argues, but he seems unaware of its full ramifications; nor does he participate as a physicist. In *de Motu*, paragraph 15, Berkeley is discussing this controversy. But what is not immediately clear is why the opinion of those who hold that forces are proportional to the squares of velocities must suppose that "the force of the body is distinguished from momentum, motion, and impetus and without that supposition it collapses." [DM p 214.] So how does Leibniz' view, discussed earlier, require that motion and force be distinguished? Berkeley thought that the notion of a force being literally conserved or stored - this does seem to be how Leibniz thought of it - is repulsive since it requires "force" to be the name of a substance and something over and above motion itself. What makes the conservation of momentum attractive is that it does not involve this notion but only the product of two figures, ie, m and v (mass times velocity). What Berkeley fails to comment on is whether vis viva must be treated in the way Leibniz did; could it not be legitimately treated as just a mathematical expression, just another mathematical hypothesis?

Berkeley made no comment on the fact that Descartes' rules make no mention of force, but if he thought he had settled the dispute in Descartes' favour by eliminating Leibniz, he was surely mistaken. If he wants to be convincing, he has surely more work to do. There is after all no reason to construe vis viva in the literal way in which Leibniz did.

Section 14: What Does "Gravity" Mean?

So far Berkeley has told us what "force" does not mean: qua abstract term it can mean nothing over and above the fact that an object is either moving, in motion, moved or at rest; this is true of "gravity" also. But what does "gravity" qua theoretical term mean? This is part of his answer:

> *Force*, *gravity*, *attraction*, and terms of this sort are useful for reasonings about motion and bodies in motion, but not for understanding the simple nature of motion itself or for indicating so many distinct qualities. As for attraction, it was introduced by Newton, not as a true physical quality, but only as a mathematical hypothesis. [DM p 214.]

In other words, scientific terms make calculation, predicting and explaining motion easier and more precise than would otherwise be the case; but they do not reveal the truth about motion or name any distinct entities causally responsible for motion. To achieve this simplicity and precision, a mathematical hypothesis is required. A mathematical hypothesis is the attribution of a force to an object, the supposition that forces reside in objects; this supposition enables the physicist to treat a body's motion in a mathematically useful way by assigning a number to it and hence give the body's mathematicized description a rôle to play in mathematical physics. It is just a methodological convenience. But for physics to work, it is not required that such forces actually exist; the force that does bring about the motion need not be enquired into, at least not by physicists.

Berkeley draws a parallel between this method and the composition and resolution of direct forces into an oblique one by means of the diagonal and sides of a parallelogram. If the force is impressed along the sides, the motion will be along the diagonal of the parallelogram. But in fact no force is impressed along the diagonal. Berkeley is saying that gravitational phenomena may be the result of two or more forces; indeed, it may even be the result of a non-physical force. Of such devices, he says,

> They serve the purpose of mechanical science and reckoning; but to be of service to reckoning and mathematical demonstrations is one thing, to set forth the nature of things is another. [DM p 214.]

The remainder of Berkeley's answer is an attempt to block the objection that if this is true, then science can no longer give explanations, but must henceforth confine itself to describing what does happen. Berkeley does not complete his answer here but waits until the third part of *de Motu* which deals with mechanical principles. For the moment his principle concern is to tell us what theoretical terms are not, and this he continues to do in the second part of *de Motu* where he considers what the origin of motion can be and is.

Section 15: What Can and Cannot Cause Motion.

Early in *de Motu*, Berkeley writes,

> *Solicitation* and *effort* or *conation* belong properly to animate beings alone. When they are attributed to other things the must be taken in a metaphorical sense. [DM p 211.]

How "metaphorical sense" is to be interpreted will be discussed when I come to deal with the third part of *de Motu*; the non-metaphorical usage suggests that only a mind, a soul or a spirit can be the source of motion, and that a body cannot. This is the topic which he takes up now.

In paragraph 21, Berkeley makes the time honoured distinction:

> ... there are two supreme classes of things, body and soul. [DM p 215.]

The strategy in this part is clear: after reiterating the statement that all that is true about motion is what can be perceived about it, and that all things belong to one of two classes, he establishes that body is passive, our idea of it containing nothing in it that could be active or an origin of motion. Hence, the origin and cause of motion must be minds, soul or spirit, and the origin and cause of motion in the world is God. Berkeley starts his argument against body thus:

> All that which we know to which we have given the name *body* contains nothing in itself which could be the principle of motion or its efficient cause; for impenetrability, extension, and figure neither include nor connote any power of producing motion; nay, on the contrary, if we review singly those qualities of body, and whatever other qualities there may be, we shall see that there are all in fact passive and there is nothing active in them which can in any way be understood as the source and principle of motion. [DM p 215].

This is the same point Berkeley made ten years earlier in the *Principles*:

> All our ideas, sensations, or the things which we perceive, by whatsoever names they may be distinguished, are visibly inactive, there is nothing of power or agency included in them. So that one idea or object of thought cannot produce, or make any alteration in another. [Principles p 84.]

So body is not the cause or origin of motion; what of mind?

> Besides corporeal things there is the other class, viz. thinking things, and that there is in them the power of moving bodies we have learned by personal experience, since our minds at will can stir and stay the movement of our limbs, whatever be the ultimate explanation of the fact. This is certain that bodies are moved at the will of the mind, and accordingly the mind can be called, correctly enough, a

> principle of motion, a particular and subordinate principle indeed, and one which itself depends on the first and universal principle. [DM p 215*f*.]

But whose mind is responsible for the observed events? Not mine, surely:

> Plato [affirms] that this corporeal machine, or visible world, is moved and animated by a mind which eludes all sense.... Cartesian philosophers recognize God as the principle of natural motions. And Newton everywhere frankly intimates that not only did motion orginate from God, but that still the mundane system is moved by the same actus. [DM p 217.]

No doubt Plato, Newton and the Cartesians did say what Berkeley claims they did, but this appeal to authority is no argument - and indeed as far as *de Motu* is concerned there is no argument for the conclusion that there must be a universal spiritual cause of motion, namely God. The argument can be supplied from the *Principles*; it is a variation of Berkeley's passivity argument:

a) I, that is, my mind qua agent, did not bring about the phenomenal events I am now experiencing.

b) The occurance of any phenomenal event must be brought about by some mind.

Therefore,

c) Phenomenal events are brought about by some mind other than mine.

That this conclusion falls sadly short of Christian monotheism need hardly be stated. Berkeley in all probability did not seriously think that his conclusion required detailed comment: that God exists as universal agent was just a fundamental assumption of Berkeley's thinking; this was also a fairly wide spread assumption anyway. So Berkeley's argument is in large measure an ad hominem argument: *What need has God of intermediaries? None. In that case do not postulate them. Hence, forces or material bodies do not exist as separate mind independent entities.* It is interesting to note, in passing, that in the paragraphs of the *Principles* dealing with the passivity argument, the argument from design is also dealt with, though not fully. But it is extremely curious that that argument is not appealed to, or even alluded to, in *de Motu* as an argument for the existence of God. He may have thought that the argument was too obvious to need stating; that would be consistent with God's existence being a fundamental assumption. The passivity argument is not at all worked out in full because it proceeds via a refutation of material substance and was therefore hardly likely to win much support in France where the mechanical philosophy of Descartes was still very influential, and where Berkeley's idealism had already received an unsympathetic reception.

But all we need to remember in this section is that motion requires an agent that must be a mind, and that objects cannot be initiators of motion.

Section 16: The Origins of Our Knowledge.

After Berkeley has discussed the construal of scientific knowledge (part one of *de Motu*) and the origin of motion (part two), he discusses what he calls the mechanical principles. In this part, Berkeley blocks the objection that what he has said hitherto entails that science must be rejected because it does not assign the efficient mechanical causes of events and cannot therefore explain those events. Berkeley now turns his attention to this problem via a treatment of the origin of our *knowledge* about motion. In the Latin original, Berkeley makes a play on words, which is lost in translation: in "principium motus", "principium" means "origin or cause"; but in "principia mathematica physicae", "principia" means "principles". Berkeley is suggesting that explanations fundamentally rest on, originate with, the mechanical principles:

> ... in mechanical philosophy those are to be called principles, in which the whole discipline is grounded and contained, those primary laws of motion which have been proved by experiment, elaborated by reason and rendered universal. These laws of motion are conveniently called principles, since from them are derived both general mechanical theorems and particular explanations of the phenomena. [DM p 218.]

I think it is clear that by *principles* Berkeley means Newton's three laws and the mathematical law about gravity (cf *de Motu*, paragraph 69).

Berkeley is anxious to rebut the Cartesian charge that Newtonion physics is not explanatory: gravity qua mathematical hypothesis simply cannot explain. According to Cartesians, mechanical explanation has to be given in terms of of efficient causation which involve knowing, by definition, what actually made the event take place; and so the extent to which Newtonian mechanics does not supply that knowledge is the extent to which it is not explanatory. Since it does not supply it at all, it must be rejected. Berkeley rejects this view on the grounds that it is not the business of science to give that sort of explanation in any case; such explanations are the province of metaphysics, theology and morality. The aim of science is to establish the rules of impulsion and attraction, that is, the fundamental laws of motion with which particular events can be explained. The phrase "primary laws of motion, which have been proved by experiment, elaborated by reason and rendered universal" echoes something Newton wrote in the General Scholium:

> In this philosophy particular propositions are inferred from the phenomena and afterwards rendered general by induction. [Newton (1729), p 547.]

And note the very next sentence:

> Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered. [Newton (1729), p 547.]

So much for the Cartesian view that mechanics is an a priori science!

Section 17: Cause and Explanation.

It is clear that Berkeley does not believe that only so-called causes can explain; he also believes that non-causal reasons can explain. But now Berkeley does something rather clever by introducing, by implication at least, his notion of a cause as thing that signifies an effect (the thing signified):

> A thing can be said to be explained mechanically ... when it is reduced to those most simple and universal principles.... For once the laws of nature have been found out, then it is the philosopher's task to show that each phenomena is in constant conformity with those laws, that is, necessarily follows from those principles. In that consists the explanation and solution of phenomena and the assigning their cause, ie, the reason why they take place. [DM p 218f.]

Hence, a particular event has been explained when it has been reduced to the fundamental mechanical principles and shown to be in agreement with them. Interestingly, "reduco", here translated as "reduce", means "to lead or take back to origins". From this it follows that a scientific explanation can go no further than the mechanical principles; for an explanation of *them* one has to go outside physics - the scientist's job is merely to demonstrate that each phenomenon is in strict accordance with the mechanical principles.

Berkeley has reached this point very quickly; the business of science is not to discover the efficient causes of things but to formulate the the fundamental laws or axioms of mechanical science with which physical events accord. By using these mechanical laws or axioms, mechanics locates not the efficient cause of an event, but the sign that signified it. In order to explain, that is, to give reasons why an event took place, an appeal is made to fundamental scientific laws or axioms; from these, a non-causal explanation is derived. This derivation makes it possible to isolate a specific item or event as the reason for the occurance of some other item or event; this specific event or item is called, by the vulgar at least, a "cause". In other, Berkeleyan, words, it is the sign that signified the event. To give a "causal" expanation means to isolate the sign that signifies; but a scientific explantion involves knowledge of the fundamental laws or axioms.

Berkeley is here expanding his theory of causation to accommodate those aspects of mechanics not dealing with impulsive causation, ie, gravity. (He will make some critical remarks about impulsion in the last paragraphs of *de Motu.*) His next step is to show how science has come to be thought of as explanatory:

> The human mind delights in extending and expanding its knowledge; and for this purpose general notions and propositions have to be formed in which particular propositions and cognitions are in some way comprised, which then, and not till then, are believed to be understood. [DM p 219.]

The phrase "are believed to be understood" is surely significant; coupled with what he has already said about inferring to the best expalanation and what he says towards the end of *de Motu*, he draws an interesting conclusion:

> The scientist studies the series or successions of sensible things, noting by what laws they are connected, and in what order, what precedes as cause, and what follows as effect. And on this method we say that that body in motion is the cause of motion in the other, and impresses motion on it, draws it also or impels it. [DM p 227.]

Again, surely the phrase "And on this method we say that the body in motion is the cause" is significant. Berkeley is drawing our attention to the fact that we use the word "explanation" in two ways. On the one hand, we think of scientific theories and concepts as explanatory because specific phenomena are shown to be in accord with increasingly general concepts, that is, shown to be examples of a general rule or law of nature; this is just a fact about the way we use language and about the way we use the word "explanation". This is the point of the remark about human understanding requiring general concepts. In this connection note what he wrote in the *Principles*:

> [The] mutual tendency towards each other, which [the philosopher] denotes by the general name *attraction*, whatever can be reduced to that, he thinks justly accounted for. [Principles p 109.]

Note that Berkeley does not write "is justly accounted for", but "thinks justly accounted for". But the truth or falsity of scientific theories need not arise, and indeed Berkeley will say that this is because they are not *capable* of being true or false. Part of the reason why such an explanation will satisfy a questioner is that there is about such expalanations an air of necessity; but it is a gross mistake to think that this necessity is anything more than verbal necessity or that there is anything in the world corresponding to this necessity, which makes events *themselves* inevitable.

On the other hand, there is a sense of explanation - and this is thinking with the learned - that does involve an appeal to efficient causation, principally in connection with God being the true and efficient cause of the regular succession of phenomena which we see. But efficient causation is not the province of science but of metaphysics, theology, and morality. (By "morality" I take Berkeley to mean not only the collection of principles that guide our actions, but also the treatment of the relationships between the will of a spirit and the associated bodily actions, what we would now call "action theory".)

Section 18: A Linguistic Model.

So far Berkeley has discussed several important concepts: explanation, making intelligible, understanding, assigning causes, giving reasons why, laws, and a number of other related concepts.

Berkeley's thoughts on these topics can be made clearer if we use his language-model of physical reality:

> ...I think we may fairly conclude that the proper objects of vision constitute an universal language of the Author of Nature, whereby we are instructed how to regulate our actions in order to attain those things that are necessary to the well-being of our bodies, as also to avoid whatever may be hurtful and destructive of them. It is by this information that we are principally guided in all the transactions and concerns of life. [NTV p 51*f*.]

As a sentence unfolds according to syntactical principles, so phenomena develop according to special syntactical ones: the mechanical philosophy. Prose is a web of syntactical relations, while the whole of phenomena is a web of relations capable of being described in the language of mechanical science. Berkeley is suggesting that science does not need hidden entities to account for phenomena but can give a perfectly adequate account by establishing an inferential pattern among events. These patterns are not the result of an underlying causal mechanism, any more than the inferential pattern of, say, ancient Greek is the result of an underlying causal mechanism. This is the importance and relevance of the language model in Berkeley's conception of science: science is just the description or verbal embodiment of the set rules or laws according to which a wise and benevolent spirit directly produces ideas in us.

Berkeley's model can be amplified as follows. A passage of Greek written in Plato's time would have been written entirely in capitals with neither gaps between the words nor any form of punctuation. There would be no words in this passage until they were read into it; this is to say, there would have been no already individuated linguistic units because there would be no gaps in the text to indicate the beginning and end of the words.

Suppose we have such a text before us now; then unless we can read the language we will not be able to individuate the words; our knowledge of Greek will help us to chop up the text into its various linguistic units, and show how the verbs, adverbs, nouns, clauses, etc relate to each other in a coherent and structured whole. Likewise, the mechanical philosophy will enable us to chop up physical reality into its individual units and discover what each unit's rôle is in the coherent and structured whole we call reality. We do not say that these units must actually exist, rather we say that they represent the most *convenient* way to chop up reality. The knowledge required to understand the Greek text is a knowledge of Greek grammar expressed in a suitable technical language; to explain is to show the relation between the individual word (the explanandum) and the remainder of the passage via the grammar of the Greek language. For the purpose of Berkeley's analogy, the semantics of ancient Greek is irrelevant; it is enough that a word can be shown to be a noun or a verb, etc.

If a particular word is out of place - it makes no sense where it is - we have the linguistic equivalent of a rogue phenomenon. We can, by appealing to the grammar, say why, and what sort of word should have been used and why; aided by our knowledge of the grammar we can point to specific syntactical features of the passage and point to specific clues, or, as Berkeley might have said, to signs which make the replacement word the correct sort to use. All this we can do because we have a fairly precise knowledge of Greek grammar expressed in a suitable technical language. To make the parallel perfect, the grammar would need to be axiomatized. Berkeley could not be aware of the difficulties that have been encountered in trying to axiomtize natural languages: we can see that while "brittle yolk" is not ungrammatical, it is still not good English and it is difficult to see how a purely axiomatic approach to grammar could generate, without reference to semantics, only good English. This problem need not be fatal to Berkeley's intention, indeed, it may have just the opposite effect: I take this problem to be parallel to the problem of the empirical import of Newton's three laws.

Berkeley is saying that mechanics is the expression in a suitable technical language of the grammar of the universe; that the way God goes about ordering phenomena embodies the mechanical principles and even that his will is identical to these basic principles. This throws further light on some of the problematic passages which seem to identify reason and cause, namely *de Motu*, paragraphs 36 and 71; this is consistent with the belief that Berkeley was influenced by Malebranche who came to the same conclusion.

Berkeley's language model gives him the basis of his account of a law's nomic necessity. Just as a sentence develops according to grammatical rules, so phenomenal events unfurl in accordance with Newtonian mechanics; certain events ought to happen or not to happen, that is, we can derive as conseqeuences of Newtonian mechanics, descriptions of certain events and these we say could happen; other descriptions cannot be so derived and these we say cannot happen. So, in ancient Greek we can say that a certain word ought to have been used or ought not to have been used, that is, we can derive as consequences of Greek grammar (hopefully axiomatized by now) certain words and sentences and these we say make good sense and preserve the text's intelligibility; other words and sentences cannot be so derived and these we say make no sense in this context or at all. I do not deny that there are ways in which nature fails to be a language; there is, as has already been observed, no semantics, except in as far as nature speaks as a whole of its author. But it is a legitimate move to construct a model suitable to the immediate task and preserve those features that are useful and discard those that are not. There are a number of ways in which gases fail to be ideal, yet much of thermodynamics is an attempt to bring data into agreement with equations based on the ideal gas model.

What does Berkeley mean when he writes "assigning their causes, ie, the reason why they take place"? [DM p 219.] A cause is a reason for an event taking place but it does not follow from this that all reasons are causal. It is easy to see how, in the case of tides, the moon may be thought of as being a reason for tidal phenomena, because these phenomena can be reduced to the fundamental laws of Newtonian mechanics and the inverse square law. This being so, we are able to to pick out the moon as the most significant feature in the matrix of events that admit of a mechanical description. This is how we have come to think of the moon as a cause of tidal phenomena. But this is not thinking with the learned, this is talking with the vulgar. Causes, properly so called, are the provenance of metaphysics, not physics; secondary causes, such as the moon, can be thought of as causes because the mechanical principles pick them out as the most significant; it can do this because the mechanical principles are founded on regularities. To point to the moon as a cause is to make a tacit appeal to these regularities as the validating basis of mechanics; to this extent and only to this extent are mechanical explanations true (cf *de Motu*, paragraph 71).

But if the assumption made earlier is correct and Berkeley does identify the fundamental laws or axioms with God's most general volitions then there is an even stronger sense in which reason and cause are identified. This identification is not damaging to Berkeley's anti-necessitarian position; all it means is that having decided to make a world that can be understood by us, God must do things in a settled and regular manner - he is, of course, under no obligation to do so. Something of this view can be seen in the *Principles*:

> ... though the fabrication of all those parts and organs be not absolutely necessary to the producing any affect, yet it is necessary to the producing of things in a constant, regular way, according to the Laws of Nature. [Principles p 95.]

Section 19: "Force" as Metaphor.

I mentioned earlier Berkeley's suggestion that the word "force" when used of corporeal objects is used in a metaphorical sense: I am now going to elaborate Berkeley's rather meagre hints and try to establish what exactly he means by "metaphorical". And from the outset, I would like to aknowledge my indebtedness in what follows to Lawrence Mirarchi [Mirarchi (1982)].

If force in all its guises is neither an essential attribute of matter, nor a primary quality of objects, nor some occult mechanism, then what is it and how is the word "force" to be construed? One part of the answer is to construe "force" in the instrumental sense of what Berkeley, following Newton's usage in the *Principia*, called a "mathematical hypothesis":

> *Force*, *gravity*, *attraction*, and terms of this sort are useful for reasonings and reckonings about motion and bodies in motion, but not for

> the understanding the simple nature of motion itself. [DM p 214.]

In other words, the above terms, when given an appropriate mathematical expression, are useful for making inferences and predictions, but this is not the same as understanding or explaining in the full blown sense described earlier. Later in the same paragraph, Berkeley says that Newton himself introduced attraction "not as a true, physical quality, but only as a mathematical hypothesis". Later, in the third part of *de Motu* (paragraphs 35 to 42), when dealing with mechanical principles, Berkeley tells us what the physicist is doing when he uses such hypotheses: the physicist,

> ... makes use of certain abstract and general terms, imagining in bodies force, action, attraction, solicitation, etc which are of first utility for theories and formulations, as also for computations about motion, even if in the truth of things, and in bodies actually existing, they would be looked for in vain.... [DM p 219.]

Whether the terms "force", "attraction", etc name any existent other than motion is quite irrelevant to their instrumental use: science describes the motions given in direct experience and describes them as simply as possible in rules or formulas, and it may be useful to refer to "force", etc as elements in calculations based on such rules or formulas, but then,

> ... we are not able to separate the action of a body from its motion. [DM p 213.]

Yet this account of "force", etc can only partially exhaust the meaning of such terms.

Berkeley has spoken of "imagining in bodies force"; I take this to indicate the metaphorical sense of force mentioned in *de Motu*, paragraph 3, and the manner in which force is to be attributed to bosies in conformity with the definition of "mathematical hypothesis" that Berkeley gives.

This procedure of attributing force to bodies suits the instrumental purpose of mechanical science, that is, it is a methodological convenience, but it is not to be supposed on the strength of that, that scientific theories are the sort of thing capable of being true or false, except in as they deal with observed and know regularities. What follows is an attempt to work out the metaphorical ascription idea, hinted at in *de Motu*, more fully than Berkeley did.

In *de Motu*, paragraph 39, Berkeley draws our attention to the fact that scientific terms have two functions: first, they facilitate calculation about motion; and, second, they facilitate the construction of scientific theories. The first I have dealt with; regarding the second: if new theories are to be developed then they must make or include some reference to experience, since it is only through the reconstruction of elements of experience that new theories *can* be developed. Hence terms such as "force", ie, those that name mathematical hypotheses, must have some reference to experience if they are to have more than a purely instrumental function. There must therefore be some further sense of "force"; the clue to what it is is found in *de Motu*, paragraph 4:

> While we support heavy bodies we feel in ourselves, effort, fatigue, and discomfort. [DM p 211.]

This inner experience of force is an experience of action exerted or of resistence felt. If Newton's second law is viewed as only a mathematical hypothesis it would amount to only a stipulative definition, and the relation, F = ma, would be analytic. If the law is only analytic, it is difficult to see what its value to physical science could be. More importantly, if it does not have some empirical consequences, it is arguable that it lacks a necessary condition of meaningfulness within natural science. [cf Hanson (1965a), p 13.]

So, since a law of nature must make some empirical claim, the term, "force", or the symbol, "F", on the left hand side of the equal's sign must have some empirical referent that differs from the acceleration symbolized by the "a" on the other side. The only empirical referent that Berkeley admits is the above mentioned inner experience of force; but this force belongs properly only to sentient beings, not to inanimate objects. The relation between such an inner experience and a visually observable acceration is an association of heterogeneous signs. The general idea of acceleration stands as a sign, and the thing it signifies is a potential inner experience of force. The visually observable signs do not contain the forces, that is, the forces are nothing over and above motion itself, they merely signify the forces that we are to experience. This is translated into the signs of an artificial language by introducing the signs, "F" for force, "a" for acceleration, and, "m" for a numerical factor that quantifies the relation F = ma; but heterogeneity of signs stops the process here.

The problem now becomes how to relate a variable representing a tactual quality with one representing a visual quality in a mathematically functional way.

Robert Hooke's *Lectures de Potentia Restitutiva* provides the answer:

> The Power of any Spring is in the same proportion with the Tension thereof: That is, if one power stretch or bend it one space, two will bend it two, and three will bend it three, and so forward. [Hooke (1678), p 333.]

The solution offered by Hooke is to replace the inner experience of force with another associated visual concept: we can, for example, express the force as a function of a visual position. So, if the force is due to a stretched piece of elastic, then the force can be assumed to be proportional to the displacement, x, of the elastic from its stretched position when unstretched. If the function, F = kx, is introduced, where "k" is a numerical factor and "x" is the displacement, then a new empirical relation, kx = ma, has been established. Acceleration and position are visual signs of a natural language that admit of translation into the mathematics and geometry of Newton's mechanics.

Hence, the mechanical philosopher is involved in a search among objects of experience for syntactical relations of natural signs given to us in the natural language of phenomena. The relations of signs and things signified are then translated into the relations of artificial signs, that, in conjunciton with the mechanical system, constitute the artificial language of the mechanical philosopher. The physicist no longer deals with the phenomena but deals with artificial signs manipulated according to the rules of mechanical philosophy.

Section 20: What is Motion?

In the fourth part of *de Motu*, that is paragraphs 43 to 66, Berkeley concerns himself with the nature of motion and considers among other things, the question of absolute space, but he first repeats a point central to *de Motu*:

> Motion never meets our senses apart from corporeal mass, space, and time. [DM p 220.]

Motion is not therefore some certain, simple and abstract idea, separate from all things; but the desire to see motion as such an idea has resulted in absurdities such as the definitions of "Aristotle and the Schoolmen" who defined "motion" as the act,

> ... 'of the moveable in so far as it is moveable, or the act of a being in potentiality in so far as it is in potentiality'. [DM p 220, and *cf* Aristotle (1984), 201^a, 10-201^b, 15.]

And such as asserting that,

> ... 'there is nothing real in motion except that momentary thing which must be constituted when a force is striving towards change'. [DM p 220 and *cf* Aristotle (1984), 202^a, 12-18.]

These definitions are absurd because they are attempts to understand motion apart from every consideration of time and space, and to do this, Berkeley rightly contends, is impossible. And further, there are those who compound these absurdities by attempting to separate from one another the parts of motion themselves and to conceive of each as though it were a distinct entity. As an example of this, Berkeley draws our attention to those,

> ... who distinguish movement from motion, looking on movement as an instantaneous element in motion. [DM p 220.]

This is a criticism of Descartes and Newton both of whom, it will be recalled from Section 2 of The Scientific Background, believed that it was impossible to distinguish actual motion from the status of motion. According to them, a body in motion had at every instant a rectilinear tendency, regardless of whether its actual motion was rectilinear or curvilinear. Berkeley is again making the point that because something is a methodological convenience is no reason to suppose that that something is in any sense true.

There are those who would even,

> ... have velocity, conation, force, and impetus to be so many things differing in essence, each of which is presented to the intellect through its own abstract idea separated from all the rest. [DM p 220.]

And finally there are those who,

> ... define motion by *passage*, forgetting indeed that passage itself cannot be understood without motion, and through motion ought to be defined. [DM p 220.]

Berkeley's contention here is that a definition throws no light on those things we perceive by sense; believing that it could has resulted in philosophers ensnaring their minds in unnecessary dificulties and saying with Aristotle that motion,

> ... 'is a certain act difficult to know'.... [DM p 221 and *cf* Aristotle (1984), 201^{b}, 4.]

Berkeley has attacked the foregoing views in the ususal Berkeleyan manner, that is, he has reproached those who would separate the truly inseparable, those who would deal in abstract ideas, and those who land themselves in a muddle by allowing themselves to become victims of the language they employ. And Berkeley, it should be noted, has spent 42 paragraphs out of a total 72 discussing his general metaphysical position; it is not until paragraph 43 do we get any detailed remarks about motion. In paragraph 47, Berkeley makes the following remark:

> ... it has become usual to confuse motion with the efficient cause of motion. Whence it comes about motion appears, as it were, in two forms, presenting one aspect to the senses, and keeping the other aspect covered in dark night. [DM p 221.]

Berkeley, however, spends most of his time in this part (ie, paragraphs 43 to 66) dealing with views then current about motion; an important aspect of these views is the need to distiguish between absolute and relative space. Absolute space is postulated as being measureless, immovable, insensible, permeating and containing all bodies; it is what would be left if all bodies were destroyed. Because all its qualities are privative or negative, it seems a mere nothing; yet it is extended and that is a positive quality. Berkeley considered the notion that absolute space is something extended very dubious blecause what sort of extension is it that can neither,

> ... be divided nor measured, no part of which can be perceived by sense or pictured by the imagination? [DM p 222.]

Berkeley concludes that absolute space is a mere nothing; it is at this point that Berkeley mentions that,

> ... all things which we designate by means of names are known qualitites or relations.... [DM p 222.]

Having successfully dealt with the qualities of absolute space - it has none - Berkeley now turns his attention to its relations, and comes to the same conclusion: space devoid of all bodies is the pures idea of nothing. We are sometimes duped because we imagine that our bodies would still remain when every thing else had gone and that we are able to move our limbs freely on all sides; but the truth of the matter is that space is a construct made out of the objects already existing, without which there could be no space. Understandably, Berkeley refers us to the *Priciples* where he has already dealt with nature of space. Here, he points out that we need not hesitate to accept his arguments; the fact that many important theorems in mechanics are based on the distinction between absolute and relative space is certainly no reason because the mechanics based on this distinction will work just as well without it. He devotes much of the remainder of this section of *de Motu* to establishing this conclusion. It is clear that he had to do so in a work whose ostensible purpose is to make the concept of motion clear, but it is Berkeley's evident intention to do so while preserving what is good and useful in Newton's *Principia*. Clearly, this must be done in such a way that the mathematics of the *Principia* remains intact: a substitute for absolute space must be found.

Section 21: A Substitute for Absolute Space.

Berkeley now develops his positive account of motion. All motion is relative, that is, motion is always motion in some direction and that requires that there are other objects because "up", "down", "left", "right" etc express some relation and necessarily indicate that there is some body other than the body in motion; hence, if there were only one object, no motion could be ascribed to it. Even if there are two objects we cannot conceive of them revolving around a common centre; that would require some third object and this is supplied in reality by the so-called fixed stars:

> ... let two globes be conceived to exist and nothing corporeal besides them. Let forces then be conceived to be applied in some way; whatever we may understand by the application of forces, a circular motion of the two globes round a common centre cannot be conceived by the imagination. Then let us suppose that the sky of the fixed stars is created; suddenly from the conception of the approach of the globes to different parts of the sky the motion will be conceived. [DM p 224]

The moral is: No relation can be given without an appropriate correlation. This example of two objects spinning around a common centre is a reference to a similar example in the *Principia* [Newton (1729), p 12]; Berkeley, however, continues with the more famous example of the bucket whirling around on the end of a cord [Newton (1729), p 10f].

The purpose of Newton's bucket experiment had been to establish the existence of absolute space. Unfortunately the experimental facts do not support Newton's case: there is no reason, except habit or convenience, for considering the parabolic shape of the water's surface deformed; it is just as conceivable that the surface of the water when it is level is deformed. What we in effect do is just assume that the water in the bucket is absolutely accelerated because the equations for the motion receive a simple and invarient form when we assume that a parabolic shape is the deformed shape. The fundamental point is that even if the water in the bucket is declared to have an acceleration when its surface is parabolic, it is still not necessary to conclude, as

Newton did, that this rotation takes place with respect to absolute space.

But if one does not accept Newton's association of inertial forces and absolute acceleration with the existence of absolute space,

> ... one must still account somehow for this important lawlike association of inertial forces with observable states of relative motion. [Sklar (1977), p 190.]

Berkeley argues that the frame of the fixed stars would make a perfectly adequate frame of reference, and would form the basis of just such an account.

These comments sum up Berkeley's general critique of absolute space and motion. In paragraph 60, a difficulty is encountered; this paragraph is worth quoting in full:

> As regards circular motion many think that, as motion truly circular increases, the body necessarily tends ever more and more away from its axis. This belief arises from the fact that circular motion can be seen taking its origins, as it were, at every moment from two direcitons, one along the radius and the other along the tangent, and if in this latter direction only the impetus be increased, then the body in motion will retire from its centre, and its orbit will cease to be circular. But if the forces be increased equally in both directions the motion will remain circular though accelerated - which will not argue an increase in the forces of retirement from the axis, any more than in the forces of approach to it. Therefore we must say that the water forced round in the bucket rises to the sides of the vessel, because when new forces are applied in the direction of the tangent to any particle, in the same instant new equal centripetal forces are not applied. From which experiment it in no way follows that absolute circular motion is necessarily recognized by the forces of retirement from the axis of motion. Again, how those terms *corporeal force* and *conation* are to

> be understood is more than sufficiently shown in the foregoing discussion. [DM p 224.]

Berkeley's argument appears to be this:

> a) many people believe that absolute circular motion reveals itself in the deformity of the rotating water;
>
> b) this belief is explained by noting that circular motion is a compound of two other motions, tangential and centripetal;
>
> c) if forces in both directions are increased, the motion will remain accelerated, but because there has been no movement relative to the axis there has been no increase in force, since force is known only through motion;
>
> d) if force is increased in only one direciton, namely tangentially, the water will travel up the side of the bucket.

Therefore,

> e) it does not follow from the experiment that absolute circular motion is recognized by the deformation of the water.

The *truth* of e) is, owing to the work of Mach, well established. The difficulty is this, Does e) *follow* from b), c) and d)? It is not immediately clear that it does; reading between the lines a little his argument would seem to be this:

> f) many people believe that absolute circular motion reveals itself in the deformation of the water {from a)};
>
> g) if the deformation is constant we must say that there is no third force acting, since force is known only through motion {from c)};
>
> h) it follows at once {from g)} that there is no force to be explained, and therefore no acceleration: hence the question of acceleration in this case simply does not arise;

i) if, on the other hand, the deformation is not constant but is increasing or decreasing, this is explicable by reference to an increase or decrease in tangential forces (from d));

j) it follows at once (from i) and from what Berkeley has said already in this part of *de Motu*) that tangential forces are explicable - being nothing over and above motion along a straight line - in terms of relative motion.

Therefore,

k) it does not follow from the experiment that absolute circular motion is recognized by the deformaton of the water (from h) and j)).

In the following paragraph, Berkeley points out that although thinking of a circle as made up of an infinity of straight lines is a useful convenience in geometry, the convenience itself corresponds to nothing in the physical realm. Likewise, a circular motion can be thought of as resulting from the integration of an infinite number of rectilinear directions which is again a useful convenience in mechanics; but it must not be supposed that this means that it is true. To be useful is one thing; to be true is quite another. This seems to have suggested to Berkeley's mind the thought that because any motion, including a curvilinear one, is compounded of, say, the motion of the earth's daily revolution, of its and the moon's monthly revolution around common centre of gravity and of the earth's annual orbit around the sun, we cannot say with certainty that the motion of the water in the bucket is circular. This does not amount to a proof; Berkeley is saying tht the onus of proof is still on those who believe that absolute space exists. More importantly, Berkeley considers only the circular motion of the water particles, which is strictly speaking irrelevant, when he ought to have considered the deformation which is important even if static.

John Earman would probably say that the whole of the above is irrelevant because Newton's concept of absolute space is not the only one available to us. We could, for example say,

> Space and time are absolute in the sense that for any pair of points of [Newtonian space-time] there is a uniquely defined spatial separation and a uniquely defined temporal separation. [Earman (1970), p 290.]

This, however, seems hardly fair. Berkeley was arguing against a particular account of absolute space and cannot be criticized for failing to take into consideration the scientific theories of the twentieth century!

In paragraphs 63 and 64, Berkeley again discusses his phenomenalistic creed: motion can be recognized through sensible things only, and since absolute space does not affect the senses, it follows that it cannot be employed for determing motion whether absolute or not. And Berkeley again makes the point that the frame of the fixed stars would do since relative space cannot be distinguished from absolute space by any feature or effect. Berkeley has missed something here, namely that the difference between relative space and absolute space is a logical one and not, as he seems to think, an empirical one. The point I am trying to make can be best made with the help of an analogy. Perfectly straight lines, perfectly true triangles, infinite space are all requirements of Euclidean geometry; but it is no where suggested that this geometry is unworkable or inconsistent if there are no actual and perfectly true triangles, no perfectly straight lines. All that geometry can do is to establish the criteria the world would have to meet if the theorms, etc were to give mathematically exact results regarding the world.

Likewise, absolute space is a theoretical requirement of Newton's physics; Newton is saying that if the world were to embody his theory perfectly, it would need to be exactly as his theory says it is. Of course, the world is not a perfect realization of either Euclid's geometry or Newton's mechanics; but neither is inconsistent or rendered unworkable by the world's failure to be a perfect realization of either of these sciences. Berkeley may have had an inkling of this (see *de Motu*, paragraph 61); he has certainly no wish to show that Newton's mechanics is a failure. But perhaps Newton is saying that absolute space is more than a theoretical requrement of his science; perhaps he is saying that for experience to be intellectually manageable absolute space is required and that space devoid of all objects is a real possibility. In this case, Berkeley's remarks in *de Motu*, paragraph 63 are very much to the point if they are taken to mean that absolute space is a logical impossibility as well as being both undetectable empirically and theoretically unnecessary:

> No motion can be recognized or measured, unless through sensible things. Since then absolute space in no way affects the senses, it must necessarily be quite useless for the distinguishing of motion. Besides, determination or direction is essential to motion; but that consists in relation. Therefore it is impossible that absolute motion should be conceived. [DM p 225.]

Section 22: Berkeley's Methodological Rules.

In *de Motu*, paragraph 66 (the last paragraph of the fourth section) Berkeley briefly states his anti-realist manifesto for the philosophy of science in the form of four rules, each of which summarizes a conclusion reached in the preceding four parts of *de Motu*. Was Berkeley thinking of Newton's "Regula Philosophandi" when he formulated these rules? I have no way of knowing, but the parallel is quite striking. The first rule is, Do not confuse mathematical hypotheses whith the nature of things, that is, it must not be supposed that there are in the world real, mind-independent entities that correspond exactly, or even approximately, to our theoretical terms. The second rule is, Beware of abstraction; this caveat is aimed at those who thought it possible to remove from individual ideas of motion all that is particular, believing that what remained was the abstract general idea of motion, and that the word "motion" referred to this idea. A principle target would be Locke. Another target would be Newton who thought that absolute space would be left if all the objects in it were annihilated. The third rule is, Understand motion as either a perceivable or, at least, an imaginable phenomena. In other words, motion is neither more nor less than what it is perceived to be; it is not some obscure nor some difficult to understand object of intellection - it is as intelligible as it is perceivable. The fourth rule is, Accept that there is no need for absolute space in the new mechanical philosophy. In other words, it is not a theoretical requirement that an object's motion be considered as other than its change of space relative to some other object or objects, eg, the sphere of the fixed stars.

A striking feature of Berkeley's rules is the development and interrelation clearly discernable among them.

> The first rule reveals Berkeley's general intention, ie, to argue against the reification of scientific terms; it finds expression in the remaining three.
>
> The second rule concerns the reification of
>
> a) abstract general terms, which must be understood to include theoretical terms as well; and,
>
> b) absolute space.
>
> The third rule emphasizes a), and the fourth emphasizes b).

If these rules are observed, two chief benefits will accrue: first, the theorems of mechanics which have made more precise our knowledge of the interrelatedness of phenomena and which have been given precise mathematical form will still serve to provide a basis for explanations and predictions about phenomena; and, second, science will be rid of all metaphysical haze.

But why did Berkeley write this recapitulation here? Its final sentence really does smack of finality:

> And let these words suffice about the nature of motion. [DM p 225.]

Yet Berkeley now procedes in the next paragraph to begin a discussion of the communication of motion; it is this mix of finality and restart that convinces me that the remaining paragraphs are an afterthought.

Section 23: Berkeley Attacks Descartes' Paradigm.

If the conclusion reached at the end of Section 25 is correct, this question immediately arises: Why did Berkeley feel the need to add a short section dealing with impulsive causation? In answering this question it must be remembered that Berkeley wrote *de Motu* with a continental audience in mind: and what, he would have asked himself on re-reading the first 66 paragraphs, will the Cartesians think of all this? The Cartesians, he continues in his speculations, will think that this is nonsense. This seems to me to be Berkeley's most likely conclusion: How, they will ask, can any explanation be a genuine explanation if it does not involve impulsive causation. And in this context, this must mean that they would think that Berkeley's conception of causation was a fraud, especially so since no attempt is made to supply the force of gravity with a workable physical model. According to the Cartesians, all explanation was causal explanation and the only acceptable reason for an event's happening was a causal agent; hence, a tacit appeal to known regularities, no matter how general the basic axioms were, was considered an insufficient basis for the validation of an explanation. Berkeley's achievement was to demonstrate that even the Cartesian paradigm of explanation shares important formal features with the concept of explanation with which they took issue. The important formal feature common to both sorts of explanation is that both make an implicit appeal to known regularities. Berkeley is saying that this is true even when implusive causation is involved. Hence the causal net, at whatever level, is whatever structure of relations causal language describes; and with respect to explanation, impulsive causation does not have a priviledged position. The scientist must always refer back to the known regularities those theorems and axioms that explain the regularities; the theorems and axioms alone cannot tell us what will happen - that requires statements of fact also - but what sort of events are possible and what sort impossible, ie, what sort are consistent with and what sort inconsistent with mechanics. The result is a possible causal net. But for the scientific enterprise, the all important facts are the regularities be they called "gravity" or "inertia". This brings us to another interesting problem in the dispute between the Newtonians and the Cartesians, namely did Newton's theory of gravity involve action at a distance?

How, it was very properly asked, can a thing act where it is not? Newton, as we have seen, never said that gravity was a real entity, only that it was a real phenomena or effect whose cause was unknown to him. Newton seems to have been placed in a dilemma by his critics: either he was postulating action at a distance or his theory fails to explain the very phenomena it sets out to explain. Unless therefore one posits some causal mechanism (of the sort hypothesized by Descartes) between objects said to be mutually exerting gravitational influence, one cannot properly be said to be offering a mechanical explanation. Nor indeed can it be said that objects exert a gravitational influence which is *mutual*. The demand was for something like a Cartesian vortex; Newton vigorously eschewed that hypothesis because it failed to fit the facts. In part the issue was about what could count as a mechanical explanation: the only thing that can determine another object to move is another moving object, only this can make movement inevitable. Berkeley's response is to show that even causal explanations involving impulsion are just as problematical as those made possible by the *Principia*; and if the former are not problematic, then neither are the latter. Both types of explanation are validated by an appeal to already known regularities, not to obscure forces, substances, qualities or essences somehow knowable independently of any consideration of their effects. Hence according to Berkeley, immediate causation properly understood does not involve the belief that the causal relation is a relation of necessity, which result is not surprising given the sources of Berkeley's inspiration.

Berkeley's first step is to draw our attention to what every one believes - mistakenly in Berkeley's view - about impulsive causation,

> Most people think that the force impressed on the movable body is the cause of motion in it. However that they do not assign a known cause of motion, and one distinct from the body in motion is clear from the preceding argument. [DM p 225.]

Berkeley now points out that force is not a determinate thing because great men such as Newton, Borelli, and Torricelli advance mutually exclusive views about it; but this is not important because each view is internally consistent and each explains as well as the other. Berkeley accounts for this as follows:

> For all forces attributed to bodies are mathematical hypotheses just as are attractive forces in planets and sun. But mathematical entities have no stable essence in the nature of things, and they depend on the the notion of the definer. Whence the same thing can be explained in different ways. [DM p 226.]

Again, it can be seen that Berkeley is relativizing explanations involving secondary causes: forces attributed to bodies are mathematical hypotheses; but how we use mathematics to describe phenomena is arbitary and conventional - the mathematics is not so to speak peeled of phenomena. The nature of our mathematical physics depends on how we decide to carve up phenomena in the first place. How we carve up the world may depend on certain objective constraints, eg, the state of our mathematics; but that sort of constraint is not a feature of the world we are trying to describe.

Section 24: The Nerve of Berkeley's Argument.

In paragraph 68, Berkeley begins his argument about causal explanations making a tacit appeal to known regularities: it does not matter whether we *say* that a body remains at rest or in motion because of inertia, or moves because of the force it receives when it is struck - the difference is only verbal. Likewise it is only a verbal problem deciding whether the motion caused is numerically the same as the effecting motion or is generated anew. No matter how we convey our meaning, it amounts to the same thing, namely, that,

> ... one body loses motion, and another acquires it, and, besides that, nothing. [DM p 226.]

"Motion", "moved", "moving" and "rest" are among the basic concepts that form the basis of our descriptions of the physical realm; the others, eg, "gravity", "inertia", etc involve the attribution of forces to objects; the latter type of term is used to "abbreviate" (Berkeley's term) more complex descriptions using the former type of term. Hence the latter type of term is the type predominately used in mechanics; they are the ones to be clarified by philosophers using the former type of term.

Berkeley, after pointing out that God is the true cause of both motion and its communication, makes the central point of this section: even in cases involving impulsive contact the explanation, prediction, calculation etc will still make an appeal, tacit or otherwise, to the fundamental laws or axioms of mechanics. As Berkeley said:

> ... a thing is explained ... by showing its connecton with mechanical principles, such as *action and reaction are always opposite and equal*. [DM p 226.]

It is surely significant that Berkeley quoted Newton's third law as an example of a relevant mechanical principle; any case of physical contact, ie, of Cartesian impulsion, would involve reference to this law. Berkeley concludes that,

> From such laws as from the source and primary principle, those rules for the communication of motion are drawn.... [DM p 226.]

What Berkeley means by "as from the source and primary principle" is not immediately clear; "source and primary principle" could mean the three basic laws of Newtonian mechanics, or it could be a further statement of Berkeley's relativization of explanation thesis; or perhaps it looks forward to a puzzling remark in paragraph 71:

> ... even the primary axioms of mechanical science can be called causes or mechanical principles, being regarded as the causes of the consequences. [DM p 227.]

It is not the first alternative because the Latin "principio" can only mean "God" in this context - "principio" being in the singular means "cause" or "origin", not "principle". So, the phrase can only mean "first cause", ie, "God". Strangely enough, this points us in the right direction; I will postpone further discussion of this until I come to discuss paragraph 71.

Returning to paragraph 69, we can see that Berkeley is saying that even events involving immediate causation must make, if they are to be explanatory, at least a covert appeal, via the laws of mechanics, to the known regularities of phenomenal events. Saying something happened is parasitic upon what is aready known to happen. Finding out what does happen and extending language to express these findings is very much the business of physicists when they study the order of sensible events, formalize, generalize and provide precise descriptions; one of the events is described as a "a causal relation". It is clear only on this basis that we are able to say, "Event A is the cause of event B." Berkeley's strategy is perfectly clear: since even those cases which involve impulsive causation make a tacit appeal to precisely described regularities they must be just as non-explanatory as the Newtonian system criticized for its inability to to offer genuine, ie, impulsive, causal explanations. But, if it is still insisted that impulsive causation is explanatory, then Newton's mechanics as a whole must be accepted as explanatory.

<u>Section 25: A Possible Objection</u>.

It might be objected against Berkeley that he excluded too much; the Cartesians probably thought so. The dispute can be seen as a dispute over what the locus of necessity is: is it in the realm of objects with their essential properties guaranteeing precisely describable events and outcomes; or is it a characteristic of language and therefore related to argument, logic and inference? Berkeley, believing that we can know nothing about the way phenomenal events take place prior to our having experience of the world, opted for the latter. It matters little if you work outward to laws, etc or inward to the so called essential properties; you will always start from the facts of experience. Berkeley's epistemological arguments against the latter mean that he must accept the former; postulating the unknowable as true does not help at all. I do not think that Berkeley believed that Newton's *Principia* was in any sense definitive; he may on occasion seem to to assert that Newtonian mechanics is the last word, he is doing so to block an attack from Cartesian doubt. Berkeley is well aware that scientific theories, including Newton's, are underdetermined (see *de Motu*, paragraph 67).

<u>Section 26: An Apparent Failure of Nerve</u>.

Yet despite all this, Berkeley's nerve does seem to fail him because he does try - so it seems - to accommodate Cartesian scruples; after taking necessity out of causation he tries to put causation into both verbal necessity and his theory of explanation. This bring us to paragraph 71 and its already quoted phrase. This paragraph is worth quoting in full:

> In physics sense and experience which reach only to apparent effects hold sway; in mechanics the abstract notions of mechanics are admitted. In first philosophy or metaphysics we are concerned with corporeal things, with causes, truth, and the existence of things. The physicist studies the series or successions of sensible things, noting by what laws they are connected, and in what order, what precedes as cause, and what follows as effect. And on this method we say that the body in motion is the cause of motion in the other, and impresses motion on it, draws it also or impels it. In this sense second

> corporeal causes ought to be understood, no account being taken of the actual forces or the active powers or the real cause in which they are. Further, besides body, figure, and motion, even the primary axioms of mechanical science can be called causes or mechanical principles, being regarded as the causes of the consequences. [DM p 227.]

In this pargraph, Berkeley sums up some of what he has already said: mechanical science deals with only those effects that can be given mathematical expression, it does not deal with the efficient cause of phenomena as a whole - this is the province of metaphysics which deals with incorporeal things, causes and truth. All that the physicist does is observe the way things happen, frame laws which classify, sum up, abbreviate, present in as general a way as possible, his observations; he will always attempt to reduce these laws still further to axioms or fundamental principles. This will be the basis of our ability to assign causes; cause and effect as far as mechanical science goes is just another sort of regularity that can be observed, and impressed force is another. The passage already quoted from paragraph 69 and the final sentence of the above quoted paragraph add weight to the claim that scientific explanation is explanation in only a limited relativised sense: it is relative to certain human conventions, one of which is that we do as a matter of fact accept science as explanatory; but that is a fact about the way in which we use the word "explain" and its cognates, not about explanation as understood by the theologically scrupulous Berkeley. A further interpretation that can be placed on the final sentence of paragraph 71 is that it refers to the occasionalist belief that the fundamental laws of physics are identical with God's general volitions. Perhaps Berkeley meant that as the scientific laws formulated by Newton depend on convention, so the laws embodied in God's will depend on his determining to use them, if the world is to be intelligible at all. This may be the insight Berkeley is struggling to make clear in the passages quoted from paragraphs 69 and 71.

Section 27: The Limits of Science.

In paragraph 72, Berkeley delimits science and metaphysics; particularly to be noted is that the concerns proper to the latter are not the proper concern of the former. Berkeley's main concern had been to show which discipline is involved in the pursuit of truth; he concluded that it is metaphysics, morals, and theology. Regarding our use of causal language, we can either speak with the vulgar, or think with the learned - so long as we know which it is that we are doing. In the descriptive part of his analysis, Berkeley showed us what the mechanical philosopher is as a matter of fact up to when he gives what he calls an explanation. This is speaking with the vulgar and it is not true, metaphysical explanation because such explanation involves by definition reference to necessity whereas a scientific explanation does not. A metaphysical explanation - the only genuine explanation accepted by Berkeley - does involve necessity because it involves the productive origins of motion, that is , it involves God. This necessity is not absolutely necessity; as already noted, it is a necessity dependent upon God's intention to create a world intelligible to its inhabitants. But this, indeed, is thinking with the learned.

Section 28: Conclusion.

Berkeley's chief point, which lay behind everything he wrote in *de Motu*, is that individual scientific terms need not always name individual things; to suppose that they do will generate all sorts of obscurities. For example, "force" is just such a term for it does not name some discrete entity, and is nothing over and above motion itself.

Berkeley used this principle to good effect in his discussion of the force of percussion, less successfully in his discussion of the conservation of vis viva, and extensively in his treatment of gravity.

"Gravity" is just a general term for a particular class of phenomena which obey a particular law. Or, to put it the other way round: phenomena obeying the inverse square law are called "gravitational". Berkeley warns us to beware of confusing cause with effect. It is easy to slide from thinking of gravity as an effect (eg, an object's falling the cause of which we still seek) to thinking of it as the cause of gravitational phenomena itself. This slide is the result of the mistaken beliefs that if a theory explains, it must be true; and that all explanation is causal. Berkeley's reply to this is predictably straightforward: a theory does not have to be true in order to explain - questions of its truth or falsity do not arise - nor does an explanation have to be causal. Berkeley makes the same point when he observes that the source of our understanding is one thing, the cause of the understood events is quite another. This perhaps reinforces his contention that causal explanation is not the only sort of explanation. In order to be explained an event must be shown to be in conformity with the general principles of physics and this is the case even with impulsive causation. Newton's *Principia* gives an adequate account of all explanations, including causal ones; infact the *Principia* offers a wider notion of explanation since it can be used to explain tidal phenomena whereas there is no causal explanation for it. Newton's theory being more general is to be preferred because being more general it is more powerful.

This basis of scientific explanation is the observed orderliness of the universe. The explanation of this orderliness is the divine mind which makes things happen as they do. No event can be truly said to make an other event inevitable, only God can do that.

BERKELEY AND POPPER

Section 1: Introduction.

While Berkeley could - quite reasonably - expect his readers to be familiar with the early eighteenth century science and philosophy required to understand *de Motu*, I cannot do so, and have therefore spent a fair portion of this thesis trying to make clear just what the science and philosophy presupposed in *de Motu* is. This done, it became possible to make clear exactly what Berkeley's arguments in *de Motu* are. And now, finally, I am in a position where I can compare and contrast Berkeley's philosophy of science with that of more modern philosophy of science, especially that of Karl Popper who has written about Berkeley's philosophy of science on two occasions. My strategy in this final section will be to see who of Popper and Berkeley has the philosophy more deserving of our our assent; it must be admitted at once that Popper does not emerge from this debate at all well. However, it should be conceded that this is not perhaps Popper's fault: in the last twnty-five years since he last wrote about Berkeley, there have been certain critical advances in the philosophy of science which were not therefore available to him. Berkeley himself does not emerge untouched: there have been some advances since his day too.

Popper wrote two articles two articles of interest in this connection: "Three Views Concerning Human Knowledge" [Popper (1961)] and "A Note on Berkeley as a Precursor of Mach" [Popper (1953/4)]. The first deals with essentialism, instrumentalism, and conjectural realism but is not primarily concerned with Berkeley - he is simply lumped together with the other instrumentalists. None the less it is of great importance as it reveals some interesting philosophical errors about Berkeley.

The second deals with Berkeley directly in as far as his ideas prefigure those of Mach; this article is also of great importance since it reveals more of Popper's errors and confusions about Berkeley's conception of science.

Section 2: Popper's Position.

Popper's general strategy in "Three Views Concerning Human Knowledge" is this: first, he accepts the instrumentalist critique of essentialism, that is, he rejects the view that,

> ... *the truly scientific theories describe the 'essences' or the 'essential natures' of things - the realities which lie behind all appearnces.* Such theories are neither in need of, nor susceptible of, further explanation: they are ultimate explanations, and to find them is the ultimate aim of the scientist.
> [Popper (1961), p 366.]

His criticism of essentialism complete, Popper uses what remains of essentialism both to criticise instrumentalism itself and to form the basis of what he calls the "the third view":

> The scientist aims at finding a true theory or description of the world (and especially of its regularities or laws), which shall also be an explanation of observable facts.
> [Popper (1961), p 366.]

But a scientist cannot, says Popper, succeed finally in establishing the truth of his theories beyond all reasonable doubt; this doctrine he thinks needs correction. What is needed is conjectural realism and falsificationism:

> All the scientist can do, ... is to test his theories, and to eliminate all those that do not stand up to the most severe tests he can design. But he can never be quite sure whether new tests

> (...) may not lead him to modify, or even discard, a theory. In this sense, all theories are, and remain, hypotheses - conjectures (doxai) as opposed to indubitable knowledge (episteme). [Popper (1961), p 366.]

In other words, we accept a theory as long as it remains unfalsified by the most severe test that the scientist can devise. Theories are either true or false, but we can only know when they are false. But the truth of the unrefuted theory remains conjectural. The only arguments which should be employed in science are those in which the premisses entail the conclusion; to advance premisses that do not entail the conclusion, but which incline one to think it reasonable to believe it, is to indulge in inductivism.

According to Popper inductivism is unacceptable because,

> ... it is far from obvious, from a logical point of view, that we are justified in inferring universal statements from singular ones, no matter how numerous; for any conclusion drawn in this way may turn out to be false: no matter how many instances of white swans we may have observed, this does not justfy the conclusion that *all* swans are white. [Popper (1980), p 27.]

If induction is to be justified it must be done by some principle which must either be a purely logical principle whose truth is self-evident, or a synthetic statement. Regarding the first: the problems associated with this are legion; none the less if such a principle *could* be found then the problem of induction would be eliminated. Regarding the second: in this case the problem re-emerges because the principle of induction must itself be a universal statement. Hence, to justify the principle we would have to employ inductive inferences, and to justify these we would have to employ a further principle, but of a higher order. To justify *this* principle further inductive inferences would have to be employed, but these in their turn would need the justification of some further principle. There would be no end to this process:

> Thus the attempt to base the principle of induction on experience breaks down, since it must leads to to an infinite regress. [Popper (1980), p 29.]

But note that Popper is from the outset a realist - though perhaps a sceptical one - that is, he assumes, almost takes it for granted in the articles under discussion, that a theory must be true or false.

Section 3: Instrumentalism - What is it?

But true or false about what? Being true or false about sensible effects was treated by Popper as a reason for saying that a theory is true or false about the unobservable realm. Berkeley did not agree: science deals with sensible effects only; see, for example *de Motu*, sections 4 and 6. It is Popper's realism which separates him from Berkeley and his falsificationism which separates him from traditional realists. But was Popper entirely fair to Berkeley? There is, I think, a good deal wrong with his interpretation of Berkeley.

The source of Popper's errors was his identification of Berkeley's philosophy with instrumentalism. True, there are interesting similarities, but it is also true that there are significant differences between the two philosophies. The principle difference is with regard to explanation - about which more later, but until I have made it clear what the difference is I will continue to refer to Berkeley as an instrumentalist.

By way of definition, Popper offers the following quotation taken from Osiander's Preface to Copernicus' *de Revolutionibus*:

> 'There is no need for these hypotheses to be true, or even to be at all like the truth; rather, one thing is sufficient for them - that they should yield a calculus which agrees with the observations.' [Popper (1961), p 358.]

We should notice at once that this is an example of what Newton-Smith calls epistomological instrumentalism, this is to say it is admitted by those adopting such an instrumentalism that a scientific theory is either true or false but denied that there is a way of determining which [Newton-Smith (1981), p 30]. Berkeley, being more radical, did not admit even that much: he denied that it makes sense to say that a scientific theory is true or false in the sense that there is an unobservable realm of which the theory could be true or false. Berkeley's instrumentalism is, again adopting Newton-Smith's terminology, semantic [Newton-Smith (1981), p 30]. The main point to note is that a scientific theory must save the phenomena; a theory is, to use Popper's own definition, "a convenient *instrument* for the calculation and prediction of phenomena or appearances" [Popper (1961), p 358]. Finally, and most importantly, according to instrumentalism scientific theories do not, indeed cannot, explain. Instrumentalists have this in common:

> They all assert that explanation is not the aim of physical science, since physical science cannot discover 'the hidden essences of things'. [Popper (1961), p 366.]

Compare this with what Duhem said about explanation:

> To explain (...) is to strip reality of the appearances covering it like a veil, in order to

> see the bare reality itself. [Duhem (1954), p 7.]

The sort of explanation which Popper was attacking is what he called "ultimate explanation", that is, theories that are "neither in need of, nor suseptible of, further explanation." [Popper (1961), p 366.] This sort of explanation, which Berkeley also criticised, is the causally efficaceous type whose existence is, it was claimed, demonstrably true. Berkeley thought that such things were not the concern of science of metaphysics, theology and morality, (cf *de Motu*, paragraph 42). Yet the essentialists had spotted something important: explanation must stop somewhere. Berkeley too spotted this: according to him, explanation finally stops in the divine mind.

Both Berkeley and Popper are in agreement that this sort of explanation is not possible for science; but this denial does not mean that they are denying that explanation of *any* sort is possible. Berkeley, just as much as Popper, asserted that science can and does explain; their beliefs differ with respect to what it is in science that does the explaining. Popper appealed to underlying mechanisms; Berkeley denied this and appealed to laws - if a productive mechanism is sought it can only be discovered by metaphysics or theology.

<u>Section 4: Explanation and Instrumentalism</u>.

Two questions immediately arise: Was Berkeley an instrumentalist? And, Did he give a persuasive account of explanation? The answer to the first is, *pace* Popper, that he is not an instrumentalist as understood by Popper because - and this answers the second question - Berkeley does allow that science can explain, and gives a persuasive account of explanation. In fact he gives two accounts of explanation: one normative and one descriptive. It is because Berkeley said that science can explain, that he is not an instrumentalist as understood by Popper. Berkeley is certainly a scientific anti-realist; that is, he consistently maintains that theoretical terms do not refer to theoretical entities, but he nowhere said that scientific theories cannot explain. But he is neither a phenomenalist nor a positivist; there is behind the observable the activity of the divine mind and to this extent he is not a precursor of Mach. (The parallels that do exist between them relate rather to their arguments regarding space and time - though Berkeley is much more obscure and less effective than Mach.) Popper's view of instrumentalism is that it is able to give only the feeblest of accounts that can possibly be given without giving up the claim to be explanatory all together; perhaps it cannot do even that much.

Now, according to Popper a scientific theory has explanatory power. According to Popper the aim of the scientist is,

> ... to find *explanatory theories* (if possible, *true* explanatory theories); that is to say, theories which describe certain structural properties of the world, and which permit us to deduce, with the help of initial conditions, the effects to be explained. [Popper (1980), p 61.]

For reasons he does not make evident - at least not in these articles - Popper thinks that a causal explanation appealing to underlying mechanisms is more persuasive than a non-causal explanation appealing to regularities observed to hold among phenomena. It is a great shame that Popper did not address himself to this question; all the more so as Berkeley offers, in the closing paragraphs of *de Motu*, a critique of the belief in the priority of causal explanation. Yet despite Popper's braketing Berkeley with the instrumentalists, Berkeley shares the view that a scientific theory has explanatory power; they differ about the source of this power. Popper's remarks suggest that he thought that Berkeley denied that a scientific theory could explain because he denied that a scientific theory could give what Popper calls an essentialist explanation.

We would expect Berkeley to have a problem with so called action at a distance; an appeal to either impulsive causation or occult qualities has already been ruled out. Berkeley is obliged to say that the explanation of such basic regularities is not the business of science but of metaphysics and theology; science can only accept these basic regularities and use them as the basis of an explanatory theory. But Berkeley is not obliged to give up explanation, but only *causal* explanation as understood by Popper; by appealing to the three laws of Newtonian mechanics, Berkeley could say that a theory gives *reasons*, and that to this extent can give an explanation. This will be Berkeley's descriptive account of explanation.

Section 5: Popper and a Muddle about Forces.

In order to make Berkeley's position on explanation clear, I must first consider what Popper said about Berkeley's treatment of both force and "force"; some muddles will have to be cleared up.

According to Popper, Berkeley's strongest argument against theoretical entities is based on his nominalistic theory of language:

> ... the expression 'force of attraction' must be a meaningless expression, since forces of attraction can never be observed. What can be observed are movements, not their hidden alleged 'causes'. [Popper (1961), p 373.]

Berkeley never said that such terms are meaningless because a force of attraction could never be observed. Berkeley admits that such terms do not help in,

> ... the understanding of the simple nature of motion itself or for indicating so many distinct qualities. [DM p 214.]

About the meaningfulness of such terms, Berkeley said that they are,

> ... useful for reasonings and reckonings about motion and bodies in motion. [DM p 214.]

Hardly an accusation of meaninglessness!

It seems that Popper thinks that the basis of Berkeley's instrumentalism is his account of how we classify particulars, which account is used to demonstrate that forces are nothing over and above motion itself. This is a confusion. Whether or not a force is some causally efficacious quality/entity over and above motion is a question quite irrelevant to the question of how we classify objects. The basis of Berkeley's nominalism is its rejection of abstract general ideas, not the rejection of the objects classified by using, *per impossible*, the appropriate abstract general idea. An argument against the existence of an abstract general idea of treeness is not an argument the existence of trees. Popper employs a similar argument in a "A Note On

Berkeley As A Precursor Of Mach": here he tells us that Berkeley's "ultimate argument" for instrumentalism is that,

> ... we know that there are no entities such as these because we know that the words professedly designating them must be meaningless. to have meaning a word must stand for an 'idea'.... Now the words here in question do not stand for ideas. [Popper (1953/54), p 32.]

The entities Popper has in mind are "occult substances and qualities, physical forces, structures of corpuscles, etc. absolute space, and absolute motion". In this connection, physical forces are of special interest: Popper seems not to have understood Berkeley's argument; "to have meaning, a word must stand for an idea" is not part of Berkeley's philosophy, rather it is the part of Locke's philosophy he was anxious to criticise. Note, for example, the following,

> ... in truth, there is no such thing as one precise and definite signification annexed to any general name, they all signifiy indifferently a great number of particular ideas. [Principles p 73.]

Of physical phenomena, Berkeley has this to say:

> ... when it is said *the change of motion is proportional to the impressed force*, or that *whatever has extension is divisible*; these propositions are to be understood of motion and extension in general, and nevertheless it will not follow that they suggest to my mind thoughts an idea of motion without a body moved, or any determinate direction and velocity, or that I must conceive an abstract general idea of extension, which is neither line, surface nor solid, neither great nor small, block, white, nor red, nor of any other determinate colour. It is only implied that whatever motion I consider, whether it be swift or slow, perpendicular, horizontal or oblique, or in whatever object, the axiom concerning it holds equally true. As does the other of every

> particular extension, it matters not whether line, surface or solid, whether of this or that magnitude or figure. [Principles p 69.]

It is true that Berkeley did not believe that there is a universally present entity called "gravity" resonsible for certain aspects of planetary motion, but it is not true that he thought this because he thought such terms as "gravity", "force", etc were meaningless - he simply did not believe that "gravity" was the name of a single determinate entity. If it does not refer to something like this, then what does "gravity" mean? Of gravity, he writes:

> ... a philosopher, whose thoughts take a larger compass of nature, having observed a certain similitude of appearences, as well in the heavens as in the earth, that argue innumerable bodies to have a mutual tendency towards each other, which he denotes by the general name *attraction*, whatever can be reduced to that, he thinks justly accounted for. Thus he explains the tides by the attraction of the terraqueous globe towards the moon, which to him doth not appear odd or anomolous, but only a particular example of a general rule of Nature. [Principles p 109.]

But Berkeley nowhere said that "force of attraction" or "gravity" is a meaningless term because forces of attraction cannot be observed. He said of such terms that they signify nothing but the effects; discovering the cause of such effects is not aimed at in physics:

> ... I do not perceive that anything is signified besides the effect itself; for as to the manner of the action whereby it is produced, or the cause which produces it, these are not so much aimed at. [Principles p 108.]

According to Berkeley, then, any idea of an object falling would do to classify a group of events as gravitational phenomena. We note as our comprehension grows that all these phenomena obey the inverse square law. The word "gravity" does not suggest any special idea of motion without a body moving in a determinate direction and with a determinate speed. It is only asserted that whatever gravitational event is considered the same law holds equally true.

This discussion is not irrelevant to my main concern because according to Berkeley, classifying, more than anything else, is the basis of our ability to explain.

Section 6: More About Force.

Briefly, Berkeley's argument is this: We observe objects falling to earth, tidal behaviour, the movements of the planets, of comets, etc; each has its appropriate idea. Or, to quote Duhem on the subject: from,

> Among the physical properties which we set ourselves to represent we select those we regard as simple properties, so that the others will supposedly be groupings or combinations of them. [Duhem, p 19*f*.]

The scientist, however, is able to see analogies and ways of abbreviating and generalizing these ideas and is able to reduce them to ideas of mass and distances; in other words "gravity" means the product of two masses divided by the square of the distance; the tendency of the objects to gravitate towards one another is given a precise mathematical form, namely $(M_1 \times M_2)/D^2$. Duhem made the same point when he said that by employing appropriate methods of measurement, the above mentioned combinations are made to correspond to certain groups of mathematical symbols, numbers and magnitudes. Furthermore, it seems that Popper has confused meaning with reference. True, "force of attraction" does not *refer* to any stuff existing between two objects, emanating from them and causing them to to tend towards each other's centres; but this, as we have seen, does not entail that the term "force of attraction" is meaningless. It was not Newton's view, at least in the *Principia*, that gravity is some etherial stuff whose mechanical operation brings about the movement of the objects according to the inverse square law. This confusion is apparent in Popper's remarks about Berkeley's treatment of Absolute Space and Absolute Time: it is not clear if Popper thinks that Berkeley thought that these ought to be rejected because they are false or because they are meaningless. This muddle is evident in the Popper quotes *de Motu*, paragraph 66:

> ... 'the study of motion will be freed from thousand pointless trivialities, subtleties, and [meaningless] abstract terms.' [Popper (1953/54), p 28; the translation is Popper's own.]

The words between the brackets in this quotation is not in the original; nor should it be. It is not a property of abstract ideas that they should be meaningful; that is a property of words - abstract ideas either do or do not exist. If they do not, and there is no alternative account explaining how we are able to use general terms, then general terms would, indeed, be meaningless. Berkeley of course has an alternative view and is not therefore obliged to think that that general terms such as "force" are meaningless, and nor is he suggesting as much in *de Motu*, paragraph 66. But I think it is pretty clear from Popper's insertion of "[meaningless]" that he thinks that it was Berkeley's intention to suggest just that. Berkeley's intentions are quite contrary to those Popper imputes to him; Berkeley is keen to show that "force" has a meaning, but that it involves nothing over and above the names of sensible effects, especially "motion" - "speed", "mass", and "direction" are also involved in giving meaning to the term "force". "Motion" gets its meaningfuless by being made the name of an idea of a particular motion,

> ... which considered in itself is particular, becomes general, by being made to represent or stand for all other particular ideas of the same sort. [Berkeley (1710), p 70.]

And it may be noted in passing that:

> ... it is not necessary (even in the strictest reasonings) significant names which stand for ideas should, every time they are used, exite in the understanding the ideas that they are made to stand for. [Berkeley (1710), p 73.]

Berkeley draws a parallel between the way words are used in writing and speech and the way letters are used in algebra:

> ... in which though a particular quantity be marked by each letter, yet to proceed right it is not requisitie that in every step each letter suggest to the thoughts, that particular quantity it was appointed to stand for. [Ibid, p 74.]

The idea here is one that Berkeley first conceived in "de Ludo Algebraico" [DLA pp 214-230] where he likens algebra to a game by showing that the rules of algebra permit certain inferences in very much the same way as game-rules permit certain moves. The meaning of the rules is, in both cases, exhausted by what they entitle a player to *do*. Of themselves they signify nothing. this is as thorough-going a conventionalist view as anything found in Wittegenstein.

Popper's confusion crops-up in a most interesting and suggestive way in a discussion of the physical world behind the world of appearences:

> But such a world *cannot* be described: for such a description would be meaningless. [Popper (1953/54), p 30.]

Granted such a description would be false if constructed literally because it would say, among other things, that there was such a world to be described; but such a world could be imagined by scientists as a methodological convevience - according to Berkeley it typically is. (Cf *de Motu*, paragraph 39.) Such a convenience might be atomism; atoms can be imagined as existing - it is at least logically possible that they do - and the scientist may attribute forces to them as Newton's theory of gravitation requires them to do. Berkeley argues against the existence of atoms on epistemological grounds; but there is nothing in the conception of an atom that obliges us or obliged Berkeley to say that it is self-contradictory or illogical. Berkeley's contention here is only that atoms are otiose.

In any event it seems misguided to claim that Berkeley believed the atomistic theory to be meaningless; rather he thought it was a very useful methodological tool which enabled the scientist to produce precise results about the world. This led many to believe that the theory was true, and hence that atoms really did exist. We have seen how Berkeley resisted this move. He thought, not so much that the theory was false if construed literally, but rather that there was no unobservable micro-structure for the theory to be true or false of. In other words the question of its truth or falsity does not arise. However, this question does arise: How are true consequences about phenomena to be derived using truth preserving rules of inference when the theory, it is claimed, is incapable of being true or false? What lies behind this question is the law of the excluded middle: either a proposition is true or its negation is; and hence any sentence in a theory must be either true or false - the very conclusion Berkeley wanted to deny. One way out of this is to deny the law of the excluded middle and adopt intuitionist logic. I do not think that Berkeley would have taken quite this way out: he would have said that theories are just elliptical ways of stating facts about the world. In other words, being meaningful does not entail being true or false about any micro-structure but about the experimental and observational consequences of the theory. Berkeley does talk of Newton's theory being true - indeed few could have been unimpressed by the success of Newtonian mechanics - but it can only be of the observable phenomena that a theory can, in the end, be true.

Berkeley is not attacking Newton or the *Principia*, but those who gave a realist construal of the *Principia*. Popper is quite right, then, to say: "What can be observed are movements" [Popper (1961), p 373] - after all this is the view of both Newton and Berkeley. At bottom the Newtonian method is this: a particular kind of regularity is observed to hold, which is given a precise mathematical expression. This expression figures in reasoning about celestial and terrestial mechanics; acting as a sort of non-formal rule of inference, it makes possible proofs, predictions, calculations and explanations regarding a multitude of mechanical phenomena.

But the phrase just quoted continues: "not their hidden alleged 'causes'." Berkeley is very critical of talking about hidden causes and warns us against doing so (*de Motu*, paragraphs 4 and 26). If we catch a falling object, or allow it to strike a stationary one we will either feel a force and our hand will move, or the stationary object will move. This makes force a consequence of motion. From this perfectly true proposition, we slide to the belief that this cause must be real because causes elsewhere are. This is popularly called "gravity". Berkeley's reply is this: Causes elsewhere are not real, even though they do involve impulsion because the real cause of motion is the divine mind; we think of them as real because our ability to predict an effect given the cause is rarely, if ever, seen to err. What lies behind impulsion, as much as behind celestial mechanics, is Newtonian mechanics with its three laws. Popper was not suggesting in the above remark that we can observe hidden causes only that we can have conjectural knowledge; but does it make sense to talk like this? I will return to this question later.

Section 7: Berkeley and Explanation.

In the *Principles*, Berkeley takes a straight forwardly covering law view of explanation:

> ... a philosopher, ... having observed a certain similitude of appearences, as well in the heavens as in the earth, that argue innumerable bodies to have a mutual tendency towards each other, which he denotes by the general name *attraction*, whatever can be reduced to that, he thinks justly accounted for. [Principles p 109.]

This view is elaborated in *de Motu*; an event is explained when,

> ... it is reduced to those most simple and universal priciples, and shown by accurate reasoning to be in agreement and connection with them. For once the laws of nature have beed found out, then it is the philosopher's task to show that each phenomena is in constant conformity with those laws, that is, necessarily follows from those principles. In that consists the explanation and solution of phenomena and assigning their cause, *ie* the reason why they take place. [DM p 218*f*.]

In the Latin original, the last sentence is,

> ... id quod est phaenomena explicare & solvere, causamque, id est rationem cur fiant, assignare. [DML p 20.]

If "id est" were translated as "or rather" the result would be less awkward; the result of this substitution would certainly be consistent with Berkeley's views. I suggest that this awkwardness is further evidence of haste in the composition of *de Motu*; but this, as we shall see later, is by no means the end of the story as far as this passage is concerned.

The phrase "thinks justly accounted for" in the first of the two Berkeley passages just quoted is surely significant, since it indicates that Berkeley is dealing with explanation as understood by the vulgar, be it lay or scientific. In other words, he is analysing our everday concept and activity of explanation, telling us what, as a matter of fact, we are doing when give what we call an "explanation". There is, it seems, no great mystery in trying to understand explanation: it is something language users do with language with its implicit appeal to classificatory schemes. It is especially something they are able to do with their scientific languages, which are an extension of natural languages, and which make the implicit appeal explicit in that that they provide basic principles that can be used to generate all the formed sentences of the language.

The similarity between Berkeley's views and Hempel's is striking:

> ... explanations may be conceived, ... as deductive arguments whose conclusion is the explanandum sentence, E, and whose premiss-set, the explanans, consist of general laws, ... and of other statements, ... which make assertions about particular facts....
>
> Explanatory accounts of this kind will be called explanations by deductive subsumption under general laws, or *deductive-nomological explanations*. The laws invoked in a scientific explanation will also be called *covering laws* for the explandum phenomenon, and the explanatory argument will be said to subsume the explanandum under those laws. [Hempel (1966), p 51.]

The explanandum phenomenon in such an explanation may be,

> ... a uniformity expressed by an empirical law such as Galileo's or Kepler's laws. Deductive explanations of such uniformities will then invoke laws of broader scope, such as ... Newton's laws of motion and of gravitation. [Hempel (1966), p 51.]

These are the fundamental laws or axioms of Newtonian mechanics; a fact is said to be explained if and only if it can be shown that it is a consequence of thses fundamental laws or axioms - which in their turn are validated by the known regularities.

This is also similar to Popper:

> To give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws*, together with certain singular

> statements, the *initial conditions.* [Popper (1980), p 59.]

Although Popper does sometimes, as here, express allegiance to a straight forward covering law law view of explantion, he does not accept - at least not in these articles - Berkeley's views about explanation; in this context, Popper's quest is for underlying causal mechanisms. Nor would Pierre Duhem in *The Aim And Structure Of Physical Science* (1954) have accepted Berkeley's views. In the *Aim* Duhem defined "explanation" as stripping,

> ... reality of appearences covering it like a veil, in order to see the bare reality itself. [Duhem (1954), p 7.]

This definition is surely both essentialist and too restrictive, and even though it is perfectly in order for a philosopher to be normative, Duhem's definition excludes too much that ordinarily and rightly passes for explanation. Duhem goes on to add what Berkeley agrees with and what Popper does not:

> The observation of physical phenomena does not put us into relation with the reality hidden under the sensible appearences, ... [Duhem (1954), p 7.]

Duhem says elsewhere that,

> A physical theory is not an explanation. It is a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible a set of experimental laws. [Duhem (1954), p 19.]

Berkeley while accepting the second sentence would most certainly not have accepted the first. Berkeley would accept that a scientific theory is an abbreviated way of stating what would otherwise be an unwieldy complex of observed facts. Duhem's great mistake was to consider explanation something special and irreducible: explanation is not the aim of science, only metaphysics can do that. And, according to Duhem, science and metaphysics are wholly different enterprises. But Berkeley, being an empiricist and not a rationalist, would have accepted Duhem's remark that,

> ... no metaphysics gives instruction exact enough or detailed enough to make it possible to derive all the elements of a physical theory from it. [Duhem (1954), p 16.]

This is precisely Newton's complaint against Descartes, already noted in "The Scientific Background", Section 2.

Duhem's attitude to explanation helped to foster the very view he was anxious to overturn: the "because" in explanations must denote some mysterious necessary relationship, and hence science must involve the attempt to attain something beyond the observable phenomena, namely determining causal relationships. Berkeley had seen what Duhem and Popper missed, namely that when a scientist is asked to explain something the information he uses is of the same kind as that used when he gives a description. The mistake was to see explanation as having a relationship to the phenomena like the one description has, ie a relation between only theory and fact.

But explanation requires a context and a person to do the explaining; hence Berkeley, in his descriptive treatment, repeatedly refers to what people *accept* as explanations. In this treatment, Berkeley held that a successful explanation - as far as information goes - involves nothing over and above a simple, general and informative description; this is why he said that force is nothing over and above motion itself. Berkeley sees the danger of being too restrictive and excluding too much from one's definition of explanation.

Section 8: Dispositions - An Ambiguity.

What is to be made of the following remark?

> I cannot but think that it is a mistake to denounce Newtonian forces (the causes of accelerations) as occult, and try discard them (as has been suggested) in favour of accelerations. [Popper (1961), p 386.]

My understanding of the matter is that the view which Popper thought was a mistake is the very view adopted by Newton himself who dealt with effects and tied his theory ultimately to the way things do in fact happen - according to Newton a force was nothing more than the way objects tended to act, though perhaps conceived differently, that is, as a change in velocity; but it is still nothing over and above the change in velocity itself. Popper's response to this immediately follows the passage just quoted:

> For accelerations cannot be observed any more directly than forces;... [Popper (1961), p 386.]

Popper's remark is surely false; that a body is moving faster now than it was a moment ago is something I can perceive directly - pulling away at traffic lights is something we have all experienced at some time or other. And what does it mean to say that accelerations are just as dispositional as forces? In a sense, of course, it is true; Newton defined a force in terms of change of velocity, ie, he wedded the concept of force to that of acceleration. But is not clear to me that Popper's analysis gave him what he wanted because as it stands the remark is ambiguous between semantics and a causal basis for the disposition. If Popper was doing semantics, then he was surely not entitled to derive ontological consequences. If he was giving a causal account, then it is not clear that he has succeeded because he must say either that force is the result of the activity of bodies - which is not really an account, or that force is an entity - which is precisely what he was arguing against. In a sense, of course, Popper's criticisms of the instrumentalist position were beside the point with regard to Berkeley; he, after all, believed that behind phenomena lay the reality of the divine mind and its volitions which are to be identified with the basic principles of mechanical science.

Popper seems to have thought that dispositional terms are descriptions of some reality or at least imply such a description; this issue is merely the realism/anti-realism debate revisited - it is hardly true therefore to call this the most "interesting difference" since it is the same difference.

According to Popper something is real if and only if a statement describing it is true. What has to be described truly to make a disposition real? Presumably a causal basis. But even here a description would involve dispositional predicates of the sort for which an explanation is sought - this seems to make dispositions alarmingly similar to occult qualities. I will not pursue again Popper, explanation, and realism; we have been here before.

Section 9: Realism About What?

I have suggested that Berkeley thought it unreasonable to reify the theoretical terms of science. Popper thought it was not only reasonable, but a demand of true science:

> *The scientist aims at finding a true description of the world* (and especially of its regularities or 'laws'), *which shall also be an explanation of the observable facts.* [Popper (1961), p 366.]

If all that Popper meant by this was that science aims at increasingly general descriptions of events, then the remark is quite innocuous. But this is not all that Popper meant and the question now becomes: Of what is a theory true?

According to Popper a theory is true of the unobservable events and micro-structures postulated by science, of whose existence we can be conjecturally sure:

> ... [the scientist] can never know for certain whether his findings are true, although he may sometimes establish with reasonable certainty that a theory is false. [Popper (1961), p 182.]

This is the basic ingredient of Popper's conjectural realism. Berkeley's reply to this is that failing to survive a critical test means merely that the theory is inadequate; and it if survives, we need need not say that its postulated entities exist, but only that the phenomena are as the theory as the theory says that they are. (Cf *de Motu*, paragaph 38.) Questions of truth and falsity do not arise. That a theory survives a critical test in no way obliges me to believe that science describes unobservable entities and micro-structures. Nor does its failure - conjectural realism or no conjectural realism. That Popper thought it an important function of science to explain, is clear from the adverse comments he made about instrumentalism's failure to give an account of explanation. Since he did not accept the instrumentalist view of the matter, and ignored Berkeley's, it must be presumed that he was seeking to make scientific explanations causal: he believed that every observed regularity needs some further (unobservable) causal regularity to explain it. But this is just the premiss that distinguishes the realist from the anti-realist. Berkeley accepted the major thrust of this argument: there must be some power responsible for the existence of phenomena. Berkeley's view differed form Popper's in that Berkeley did not believe it was the business of science to describe this power. Although,

> ... it is most true that the investigations of nature every where supplies the higher sciences with notable arguments to illustrate and prove the wisdom, the goodness, and the power of God. [DM p 218.]

Berkeley believed that it is the business of theology and metaphysics to tell us about this power. But the only knowledge we can have of this type of power is, he believed, the knowledge one has of one's own mind, and our own ability to determine the occurance of certain mental acts or events. Hence, the cause of observable motion is going to be the volitions of the divine mind. This would have enabled Berkeley to avoid the charge that he cannot give a causal explanation of the observable regularity of physical events, that they must be some vast cosmic coincidence. This is a charge that Hume cannot easily resist, and certainly not as Berkeley did.

It is not clear why we must accept, as Popper seemed to think we must, that the aim of science is to give a true description of the reality underlying phenomena.

Popper's reasons seem either unconvincing or to count for his opponents' views as much as for his own. What is required is a direct means of verifying the claims of science, which would enable to know these things other than by their effects. What is required is a direct means of verifying the claims of science. At this point Popper remarks that he does not think that the direct-indirect dichotomy will get us very far; this seems to amount to saying that the observable is at one end of a spectrum of observability whose other end terminates in the unobservable realm of scientific entities, and that no clear and unequivocal line can be drawn between them. Hence the viability of Berkeley's concept of direct but passive perception is thrown into doubt. This being so, a theory of truth that works for the directly observable must also work for the indirectly observable. Popper's theory of truth - "A state of affairs is real if and only if the statement describing it is true" - begs the question because it presupposes that the statement is capable of being true or false in the first place. Why should the above mentioned spectrum end with the theoretical entities? What this sort of contention needs is some direct but independent means of establishing the claims of science. Berkeley was aware of this; yet it is a frequent criticism of his philosophy that the arguments that he used against the existence of theoretical entities would be equally effective against a belief in God's existence. This is not so, because Berkeley used empirical arguments to establish the legitimacy of this or that entity's claim to be real; but he used a priori arguments to establish that there must be some means of producing phenomena. Popper's rejoinder is that there must be such a reality because if there were not, theories could not be falsified:

> ... if [the theory] is false, then it contradicts some real state of affairs (...). Moreover, if we test our conjecture, and succeed in falsifying it, then we see very clearly that there was a reality - something with which it could clash. [Popper (1961), p 384.]

This is true, but not in the way Popper intended it; theories are falsified, not by comparing their claims about, say, micro-structures directly, but by noting whether the observable consequences of a theory are as the theory says they should be. We cannot move from the correctness of a prediction based on a theory to the theory's truth; that would involve committing the fallacy of affirming the antecendent. There would be no fallacy and no circular argument here, if the means of determining whether the postulated entities of science did in fact exist were independent of determining whether a theory explains the phenomena. Sensible effects are one thing; their causes, another:

> As for gravity we have already shown above that by that term is meant nothing we know, nothing other than the sensible effect, the cause of which we seek. [DM p 215.]

And, again:

> We must ... admit that no force is immediately felt by itself nor measured other than by its effect. [DM p 213.]

Popper might say that he is not claiming categorically that a theory is true but only that it is conjecturally true until falsified. But of what is it true or false? I cannot agree with Popper that it is a,

> ... grave mistake to conclude from [the view that we should call a state of affairs 'real' if and only if the statement describing it is true] that the uncertainty of a theory, ie, its hypothetical or conjectural character,

> diminishes in any way its implicit *claim* to describe something real. [Popper (1961), p 384.]

Berkeley's objection is that this claim is simply unjustified because it begs the question whether there is something for science to describe. The critical test could be seen quite properly as a test of the theory's adequacy, and since this makes the weaker ontological claim, it is to be preferred. It is one thing to demonstrate that a theory is true; it is quite a different thing to to demonstrate that it explains. Yet Popper persited in confounding these two issues by assimilating the first to the second.

While it is fair to describe Berkeley as scientific anti-realist it is not fair to describe him as an out and out anti-realist; in fact it would be plain false. He had some views about metaphysics which were decidedly realistic, and to a consideration of these I now turn.

Section 10: Berkeley's Realism.

I mentioned earlier that Berkeley had two accounts of explanation: one normative, and the other descriptive. The descriptive one has already been dealt with; what Berkeley's normative account of explanation is will emerge by considering the realism that underpins much of the philosophy of *de Motu*.

Scientific realism, whatever its shortcomings, has one advantage over instrumentalism, namely it does provide us with a means of production expressed in terms of constituents or causes; this rôle is filled in realism by the theoretical entities of physical science. Denying that science provides such causes was the conclusion

Berkeley argued for; mere human beings, he seems to say, with their finite intellectual powers cannot hope to *discover* alone such causes. This sort of knowledge, if it is possible at all, is revealed and it involves knowledge of God and his will. So Berkeley - the supreme theological realist - is forced to postulate causes or constituents other than those required by a realistic construal of science; in this case they are the volitions of the divine mind. (Cf *de Motu*, paragraph 3.) Berkeley was conceding something both to the scientific realist and to those would infer to the best explanation: to the first he conceded that a causal basis is required for a genuine understanding of why an event happened; to the second he conceded that it is proper to infer to the best explanation - once it is clear what is meant by "best explanation". Berkeley disagrees about what can *make* something else happen and about what the *best* explanation is. An object, being entirely passive, cannot make anything happen, and accordingly, since our experience of something making something else happen is a mind, we ought therefore to infer that a mind must be resposible for the occurrences we see. Berkeley had to say that the best explanation is the divine mind and that a true cause is a volition of the divine mind. In the case of the human mind, its volitions can only bring about the occurrence of mental episodes, not the movement of a limb. The relationship between my mind and the ideas constituting my body is contingent, but the relationship between my mind and its volitions is a necessary one since they are, in part, constitutive of what my mind is - I cannot be said to cause them; but, the relationship between the will of God and the ideas constituting the physical realm, including my body, *is* a necessary one because God causes them to be manifest to the senses. All this he brings about with a few basic volitions. The nearest science can come to the discovery of these volitions is to formulate basic axioms and laws as found, for example, in Newtonian mechanics.

But do we have volitions, and, if we do, what are they? Wittegenstein asked in the *Investigations*,

> ... what is left over if I subtract the fact that my arm goes up from the fact that I raise my arm? [Wittgenstein (1953), p 161.]

The answer is,

> What is left over is whatever a paralytic does when he tries to lift his arm and fails.

It will not do to say that moving an arm is a basic action unanalysable into anything else, because this implies that the paralytic discovers that he cannot move his arm by intending to raise his arm at t_1, and noting at t_2 that it did not move. This is surely false because it makes the paralytic's knowledge of his own state inferential.

What are volitions? We can say that there is a class of conscious occurrences that are or express propositional attitudes, and the members of this class have the following in property: each has a tendency to cause an event that satisfies or fulfils its proposiitonal content.

All this tells us a great deal about what Berkeley meant by explanation that involves an appeal to "the nature of things": this is what expalanation means when one is thinking with the learned and saying what actually brings about the phenomena we see.

Since Berkeley traced all explanation of physical events to the volitions of the divine mind it cannot be objected against him - as it can against instrumentalism - that he cannot give any account of the means by which particular phenomena are produced. This objection clearly does not hold because Berkeley held that divine volitions are the only genuine causes.

Section 11: Theories And Computation Rules.

Popper is muddled and wrong about what he said about Berkeley: he told us that Berkeley's arguments depend on a certain philosophy of language which "hinges on the problem of meaning". As we saw, Berkeley's nominalism has no bearing whatsoever on his philosophy of science, the aim of which was to demonstrate that it is more reasonable to believe that there are no physical entities in the world named by the terms of physical theory. Berkeley's arguments against the reification of such terms are neither linguistic nor logical, but metaphysical and epistomological. Popper's arguments against Berkeley's nominalism are beside the point.

I think it is now clear that we should call Berkeley a "scientific anti-realist", in preference to "instrumentalist" because that term has a number of misleading connotations. Doing this is even more preferable than calling him an "empiricist" since Ayer, when calling Hume "a more consistent empiricist than Berkeley", made the remark sound like a criticism which it is not. Ayer's accusation sounds very much like an attempt to make terms do the work of an argument.

I want now to consider the significance for science of experimental tests, which subject touches on Berkeley's concerns in *de Motu*.

Popper said that, because instrumentalism construes science as a set of computation or inference rules, it could not therefore give an adequate account of the difference between high level theories and the more mundane, technologically oriented, applied sciences such as navigation. Popper believed - in my view, unjustifiably - that this failure is enough to bring about the collapse of instrumentalism, and, *a fortiori*, Berkeley's anti-realism also. Popper explained this the difference between science and a technologically oriented, applied science as follows:

> The way in which computation rules are *tried out* is different from the way in which theories are *tested*. [Popper (1961), p 377.]

This is perfectly true, but it does not count against Berkeley; navigation rules are tried out and high level theories are tested - but why could not Berkeley say exactly the same? Of course Berkeley could not have said that a theory is tested for its *truth* or its *falsity*, but he could have said that a theory is tested *for* its adequacy, generality and applicability. In other words, the only question that need have concerned Berkeley is extent to which a theory saves the phenomena:

> The human mind delights in extending and expanding its knowledge; and for this purpose general notions and propositions have to be formed in which particular propositons and cognitions are in some way comprised, which then, and not till then, are believed to be understood. [DM p 219.]

Theoretical terms serve this end:

> And just as geometers for the sake of their art make use of many devices which they themselves cannot describe nor find in the nature of things, even so the mechanician makes use of certain abstract and general terms, imagining in bodies force, action, attraction, solicitation, *etc.* which are of first utility for theories and formulations, as also for computations about motion, even it in the truth of things, and in bodies actually existing, they would be looked for in vain. [DM p 219.]

Berkeley was well aware that two contradictory theories could still explain a given phenomenon:

> But although Newton and Torricelli seem to be disagreeing with one another, they both advance consistent views, and the thing is sufficiently well explained by both. [DM p 226.]

It must not be supposed that Berkeley was therefore sympathetic to factitious explanations. At various points in paragraphs 16 to 35 of *de Motu*, Berkeley briefly surveys a number of conceptions about motion and comments that some are held by people who have "said something rather than thought it", and that such theories are "as difficult to explain as the very thing [they are] brought forward to explain"; they are all "too abstract and obscure." Realising that "of the unkown it is profitless to speak" and that he would be "ashamed to linger long on subtleties of this sort" he concludes that,

> ... to employ a term and conceive nothing by it is quite unworthy of a philosopher. [DM p 217.]

We have been warned!

In *de Motu*, paragraph 36, we are told that a principle is something "in which the whole discipline is grounded and contained," [DM p 218]; in paragraph 38, we get this:

> In mechanics ... notions are premised, ie definitions and first and general statements about motion from which afterwards by mathematical method conclusions more remote and less general are deduced. [DM p 219.]

Berkeley continued by making a comparison between the method of geometers and that of scientists, and concluded by pointing out that just as geometers are able, by applying geometrical theorms to measure the size of individual bodies,

> ... so also by the application of the universal theorems of mechanics, the movements of any part of the mundane system, and phenomena thereon depending, become known and are determined. [DM p 219.]

And leaving us in no two minds about the matter, he concludes:

> And this is the sole mark at which the physicist must aim. [DM p 219.]

Berkeley found the above mentioned explanations unacceptable because they derive from theories whose advocates lack a genuine understanding of the scientific enterprise. Hence a theory is properly constituted if and only if it is the result of observing, generalizing and formulating powerful axioms and principles; these activities are the basis of any well-formed theory. This is a bow from Berkeley in the direction of Newton and Bacon. The significance of this is clear: Berkeley had little sympathy with factitious, obscure and *ad hoc* arguments; in cases where there are two properly constituted but underdetermined theories, their undeterminatiorn is evidence of the further work that needs to be done before one or other (or neither) can be accepted as a genuine theory.

All this flies in the face of Popper's contention that Berkeley's ideas must prevent the advancement of genuine science because instrumentalism is a device whose rôle is to rescue contradictory theories which, having forgone all claim to veracity, still facilitate prediction, calculation, etc in their own limited sphere of applicability. Nor does this square at all well with the further related contention that genuine theories are testable and are not therefore mere computation rules; and only what is capable of being true or false, Popper implies, is capable of being tested. According to Berkeley, a scientific theory cannot be truthful about theoretical entities - they do not exist; about the world, then - this is doubtful because the "human mind delights in extending and expanding its knowledge" which means that we will always be revising or even discarding our theories, and if a theory is true, there can never be any grounds for discarding it. So, given that Berkeley is right and scientific theories are not capable of being true or false in the first place, then the claim that theories cannot be verified or falsified is trivial. Popper's argument is making a covert appeal to the very premiss Berkeley sought to call into question; Popper is merly supposing the truth of his own position. The sort of test Berkeley could have advocated would be exactly the same as those that Popper would advocate; there would be no difference whatsoever, and a Berkeleyan could claim to predict not just new phenomena but new *types* of phenomena:

> ... in mechanical philosophy those are to be called principles, in which the whole discipline is grounded and contained, those primary laws of motion which have been proved by experiment, elaborated by reason and rendered universal. [DM p 218.]

And "rendered universal" is not the language of someone who was peddling a philosophy of science whose sole aim is to rescue *ad hoc* theories. Popper failed to see this:

> A theory is tested ... by applying it to very special cases for which it yields results different from what we should have expected without the theory,... In other words, we try to select for our tests those crucial cases in which we should expect the theory to fail if it were not true. [Popper (1961), p 378.]

Change the last word of this quotation to "adequate", and Berkeley would have been very pleased to accept it.

Section 12: Testing For Truth Or Applicability.

Popper was quite right when he said that if instrumentalism is true, theories cannot be refuted; if truth or falsity is not claimed as a characteristic of theories in the first place, then the truth of Popper's remark is guaranteed. Of course there is nothing strictly corresponding to the attempt to refute a theory - and for the same reason as the one just given. Nonetheless a theory can still be tested for its applicability, and steps taken to render it universal; we can still reject a theory, not because its theoretical entities do not exist, but because the observational and experimental consequences of the theory are not as the theory says they should be.

If a theory is found wanting because it fails some test, we may not be forced to reject it *qua* instrument; it may still be of some use, and though of limited applicability, we may continue to use it until the scientists provide us with a better, ie, more general, theory. However, in these circumstances we *are* forced to reject it as a scientific theory and await the advent of an adequate theory because only theories "rendered universal" can be genuine theories. Theories incapable of being rendered universal must be give up their claim to explain phenomena. Here is the reason why navigation is not to be thought of as a scientific theory. Yet, I can imagine a situation in which it could be accepted as a scientific theory: suppose it represents the entire body of scientific and theoretical knowledge known to a particular sea-faring community; then *ex hypothesi* there is nothing more general it can be deduced form, and hence no higher level scientific theory with which it can be unfavourably contrasted. It would be a scientific theory in its own right; it would be used to predict, calculate, and explain - if any one thought it poor science, the onus would be on the critic to improve or replace it, perhaps by giving it a theoretical basis composed of axioms and a model which interpreted the axioms.

Section 13: Obscurantism.

Popper's final complaint against Berkeley was to accuse him of being obscurantist:

> ... *by neglecting falsification, and stressing application, instrumentalism proves to be ... obscurantist* .... For it is only in searching for refutations that science can hope to learn and to advance. [Popper (1961), p 360.]

About what we "can hope to learn" I have already dealt with; I want now to deal with Popper's remarks about the obscurantism of Berkeley's position; it is, I hope, clear by now why Berkeley neglected falsification. If he stressed application at the expense of every other aspect of a theory, then Popper's suggestion was quite true. But as we have seen, Berkeley did not stress application exclusively; he wanted physical science to be both rendered universal and free of any hindrance, obscure or otherwise, that would impede its advancement. For, as Berkeley points out in a passage I have quoted before:

> The human mind delights in extending and expanding its knowledge;... [DM p 219.]

After giving his four methodological rules in *de Motu*, paragraph 66, Berkeley wrote that if these rules are followed,

> ... all the famous theorms of the mechanical philosophy by which the secrets of nature are unlocked, and by which the system of the world is reduced to human calculation, will remain untouched. [DM p 225.]

These are just not the words of someone advocating obscurantism in science.

Section 14: Conclusion.

In this section of my thesis I have compared and contrasted Berkeley's anti-realism with Popper's realism.

Popper from the outset assumed that a theory is capable of being either true or false; that instrumentalism cannot explain; and that instrumentalism is obscurantist - charges which Berkeley's philosophy can resist, even if instrumentalism cannot.

Popper's attack on Berkeley is not quite the attack that he thought it was. Popper berates instrumentalism for its inability to explain; this, though true, is beside the point since Berkeley demonstrates that science does explain. They disagree about what can count as explanation in science: Popper argued for underlying causal mechanisms, whereas Berkeley argues for the fundamental laws of Newtonian mechanics. Berkeley believed that science could give no convincing reason to suppose that the theoretical entities of physical science did exist; all we can ever know are the effects "the cause of which we seek". True, there is nothing in Berkeley's view corresponding to falsifying a theory, but that is because a theory is not capable of being true or false in the first place. At the bottom of our ability to explain lies our ability to classify in increasingly general ways. Berkeley, aware that explanaton must stop somewhere, and that he must defend himself from the cosmic coincidence objection, is in the end a realist: he believed that God is the causal basis of the observed phenomena; precisely put, he believed that the basic laws of physical science are identical to the basic volitions of the divine mind.

To be a genuine scientific theory, a theory must be all embracing; it is this which distinguishes it from computation rules which are only of limited applicability. This also enables Berkeley to resist the charge of obscurantism. Popper said that instrumentalism allowed scientists to stop attempting to achieve increasingly general theories - Berkeley argued for the opposite. Theories are tested, not for their truth or falsity, but for their generality and applicability.

Berkeley's philosophy resists Popper's criticisms very well, especially with regard to explanation; and in this regard Berkeley showed himself not to be an instrumentalist.

CONCLUSION

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We have seen the science and philosophy presupposed in *de Motu*, seen the arguments and conclusions drawn in *de Motu*, and finally we have seen what Berkeley has to offer the modern philosopher of science. But apart from that - pretty obvious it is, too - what is left to write in this conclusion?

One possibility is to speculate on further applications of the principles Berkeley elaborated in *de Motu*. We might, for example, apply his results to the social sciences. I am sure he would have undertaken that enterprise with relish; the determinism of these sciences and their failure to keep matters of fact separate from matters of method, their naive realism about such terms as "society", "social pressure", etc are ideal targets for his anti-realist construal of scientific terms. The notion of a person's behaviour being determined by external social factors would have been anathema to him - Berkeley always maintained that people are free, moral agents and that to suppose otherwise is to suppose we are made in a way that frustrates God's purpose in creating us in the first place. Our moral confusions would, I am sure, have angered and alarmed Berkeley.

Another possibility is to speculate more generally about Berkeley's likely reaction to our attitude towards science: I am sure that our childlike faith in science and technology would have been treated by him with a mixture of amazement and contempt; he would have thought that we were little better than mediaeval catholics. As a convinced advocate of free-thinking in philosophical matters, he would have been sure to encourage us to think for ourselves.

What, as another possibility, would Berkeley have said about philosophers of science more modern than Duhem and Popper? What, for example would he have made of Newton-Smith's contention that the demand that science be rational is best met if we accept scientific realism? Berkeley would have been mystified. He would have been more sympathetic towards Van Fraassen, though he would have denied that a scientific theory is capable of being true or false. But the notion of a scientific theory being adequate would have been very congenial.

But the most interesting question is: Why did Berkeley's views have such little impact on the science of his day? Part of the answer is the very success of the *Principia*, which made radical discussions of its fundamental tenets likely to be met with deadly silence; or more likely such a discussion would not have been heard above the tumult of success. The remainder of the answer - the more important part - lies with Berkeley himself, who, for all his criticism of the metaphysics of the *Pricipia*, leaves untouched the content of the physical system: it had after all been his contention that we do not need to be scientific realists about the theoretical terms of Newtonian physics. It could stand well enough without that. But the *apparent* anti-Newtonianism could have been contributed to the lack impact *de Motu* had, as could the lack of an English translation of *de Motu*. Physics' growing indifference to metaphysical scruples has created yet another buffer between philosophy and physics. So would the changing atmosphere in the eighteenth-century with its increasing confidence, success, progress and optimism; an age with these characteristics would have been impatient with conceptual niceties.

With the benefit of hindsight, we can see that the various possibilities that become available once absolute space is denied, were not appreciated; no one spotted the significance and so no one investigated the matter further. Conceptual reformulation - prompted by metaphysical and theological interests - ran ahead and prevented a fertile contact with the basis of Newton's system. Berkeley's attitude is in very marked contrast to that of Kant and Hume, both of whom took Newton for granted. Although Berkeley was writing before the full impact of Newton was appreciated, this in no way diminishes the fact that Berkeley was one of the last people not to be over-awed by Newton.

Inevitably, Berkeley's views - via Mach - influenced modern physics; Einstein's theory is a specific interpretaton of Berkeley's general relativity principle. This shows that there is a looseness of fit between the general conclusions obtained from a metaphysical inquiry and those conceptual investigations into the fundamentals of the science which become pressing in a time of revolution. The moral is that we cannot predict, before the event, what the relevance of some philosophical theory will be to science. And that is the importance of philosophy and the reason why we do it.

KEY TO BERKELEY REFERENCES

Key To Berkeley References.

Throughout the thesis I have adopted the following conventions when referring to works by Berkeley.

DM = *de Motu*, English translation by A A Luce, in *Berkeley, Philosophical Works, Including the Works on Vision*, (ed M R Ayers), 1975. I have used the 1983 reprint.

DML = *de Motu*, Latin text in *The Works of George Berkeley, Bishop of Cloyne*, (ed A A Luce and T E Jessop), Volume IV, 1951. *de Motu* was first published in 1721.

Principles = *A Treatise Concerning the Principles of Human Knowledge* in *Berkeley, Philosophical Works, Including the Works on Vision*, (ed M R Ayers), 1975. Reprinted with minor revisions, 1983. *Principles* was first published in 1710

PC = *Philosophical Commentaries* in *Berkeley, Philosophical Works, Including the Works on Vision*, (ed M R Ayers), 1975. Reprinted with minor revisions, 1983. *Philosophical Commentaries* was written in 1707-8 and first published in 1871.

NTV = *An Essay Towards a New Theory of Vision* in *Berkeley, Philosophical Works, Including the Works on Vision*, (ed M R Ayers), 1975. Reprinted with minor revisions, 1983. *New Theory of Vision* was first published in 1709.

TVV = *The Theory of Vision Vindicated and Explained* in *Berkeley, Philosophical Works, Including the Works on Vision*, (ed M R Ayers), 1975. Reprinted with minor revisions, 1983. *The Theory of Vision Vindicated* was first published in 1733.

DLA = "de Ludo Algebraico" in *The Works of George Berkeley, Bishop of Cloyne*, (ed A A Luce and T E Jessop), Volume IV, 1951. "de Ludo Algebraico" was first published in Berkeley's *Miscellena Mathematica*, 1707.

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