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THE ASSIMILATION OF NEWTONIAN MECHANICS

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INTRODUCTION

The period extending from the publication of the _Philosophiae naturalis principia mathematica_ of Isaac Newton (1642-1728) in 1687 to the publication of the _Mechanica sive motus scientia analytice exposita_ of Leonhard Euler (1707-1785) in 1736 is one that shows no profound new developments in mechanical theory and is, for this reason, passed over in works dealing with the development of mechanical thought on a large scale.[1]

However, precisely because there seems to be so little advance, this period is of interest from the standpoint of the process through which ideas are assimilated by what might be termed second-rate thinkers. These are the men who perform the work of criticism and elaboration on the ideas provided by the men of superior creative insight. The particular period under consideration here is unusually illuminating for a number of reasons: the tension in the political atmosphere between England and France, the impact of the initial phases of industrial revolution, but especially because of the nature of Newton's innovations in

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[1]See René Dugas, _A History of Mechanics_ (New York: Central Book Company, Inc., 1955), Eugen K. Duhring, _Kritische Geschichte der allgemeinen Prinzipien der Mechanik_ (Leipzig: Fues's Verlag, 1887) Ernst Mach, _Die Mechanik in ihrer Eutwickelung historisch-kritisch dargestellt_ (Leipzig: F. A. Brockhaus, 1904).

mechanical thought and in the conception of the nature of scientific explanation.

In the Principia, Newton laid out an extremely impressive, but somewhat obscure theory of mechanics. It takes a good measure of hindsight and a considerable amount of analysis to make out the true nature of Newton's thought, so that the vast array of theorems presented in the Principia seemed to his contemporaries more an achievement inviting awe and belief than an understandable theoretical system. In contrast to this, Euler, in his Mechanics, elaborated mechanical theory in terms of analysis, as is indicated in the title of the book, and, in so doing, produced a work that represented an understandable system, provided of course that one understood analysis, i.e., the differential and integral calculus. Euler wrote that the use of geometrical demonstrations--the means of demonstration employed by Newton--serves to convince one of the truth of a statement of principle, but does not give understanding. This can only be achieved in analysis.[2]

Euler's use of the word "understanding" seems to imply more than comprehension of a merely logical system. It demands insight into the actual physical processes whose observable consequences are represented by theory as well as insight into the ultimate nature of the matter involved in these processes. This sort of understanding is what was provided by the calculus.

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[2]Leonhard Euler, Leonhard Euler's Mechanik oder analytische Darstellung der Wissenschaft mit Anmerkungen und Erläuterungen, herausgegeben von J. Ph. Wolfers (Greifswald: C. A. Kogh's Verlagshandlung, 1848), I, pp. 3-4.

However, since Euler's formulation of mechanical theory, as well as those of Joseph-Louis Lagrange (1736-1813) and William Rowan Hamilton (1805-1865), was mathematically equivalent to Newton's formulation,[3] it seems likely that both, or all, were founded on the same mathematical basis. Further, there is the age-old distinction between mathematics as a method of discovery and mathematics as a means of demonstration to consider: how did Newton come by the discoveries which he proved in the traditional, geometrical manner? Since Newton was the inventor of an infinitesimal calculus, the so-called calculus of fluxions, there is a possibility that calculus was the tool used in the construction of his theory. As such, the calculus would constitute the "understandable" structure of the theory.

It is upon this thesis that the present work rests; with the assumption that analysis is the implicit structure of Newton's mechanics the period under examination takes on, in one aspect, the form of a dissemination of a new type of mathematical thought, i.e., the infinitesimal calculus. This would suggest that Gottfried Wilhelm Leibniz (1646-1716), who was an independent inventor of the infinitesimal calculus, might have exerted a large influence on mechanical thought in the period. In fact, as will be shown, the influence of Leibniz, especially through John Bernoulli (1667-1748), was more decisive than that of Newton on the French mechanicians, who were, on the whole, far advanced over their English contemporaries.

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[3]Ernest Nagel, The Structure of Science, Problems in the Logic of Scientific Explanation (New York: Harcourt, Brace and World, Inc., 1961), p. 158.

With this in mind, the study of the history of mechanical thought between 1687 and 1736 can be broken down into a number of major problems: the mathematical nature and origins of Newton's theory of mechanics, the difficulties that that theory entailed for his English followers, the differences between English and French mechanical tradition, the nature and scope of Leibniz's contributions to mathematical-mechanical thought, grounds for the French acceptance and English rejection of Leibniz, and finally the basis on which a reconciliation of French and English mechanical thought could be made.

These subsidiary problems suggested by the mathematical aspect of the development of mechanical theory can be more easily and adequately treated when the relation of mathematics to physical theory is understood. According to Ernest Nagel, scientific theory can be analyzed into three major components: an abstract calculus, which is the logical skeleton of the theory; a set of rules that assign an empirical content to the abstract calculus (so-called rules of correspondence); and an interpretation for the abstract calculus, which gives "flesh" to the skeletal structure.[4]

From this analysis of the component parts of theory, the infinitesimal calculus invented by Newton and Leibniz should correspond to the logical structure of mechanics. That is, the system of postulates and definitions that constitute the basis of the theory should form an abstract relational structure for the terms of the theory which is the same as the infinitesimal calculus. For instance, the relationships between force, distance, velocity, and acceleration defined in the

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[4]<u>Ibid</u>., p. 90.

mechanical theory should correspond exactly to the relationships between similar terms in the infinitesimal calculus.

If the abstract calculus were the only significant aspect of scientific theory, the history of mechanics in the period would be simply a matter of tracing the spread of mathematical understanding; anybody who could operate with the calculus could understand and accept the corresponding theory of mechanics. However, the other two components of theory are of equal importance and are capable of producing controversy and even of obscuring the understanding or preventing the acceptance of the calculus as a proper relational structure for mechanics.

With regard to the matter of assigning empirical content to the theory, a long and acrimonious dispute was carried on between Newtonian and Leibnizian adherents over the proper empirical determination of force in a moving body; the Newtonians insisted that force was proportional to the velocity and the Leibnizians insisted that it was proportional to the square of the velocity. Both of these results could be derived from the theory and verified in experience, depending on whether one assumed that the time factor or the distance factor was of basic significance in the understanding of force.

The interpretation of the abstract calculus was, however, the popularly significant aspect of any mechanical theory, from the point of view of its acceptance or rejection. As has been stated, the interpretation gives "flesh" to the abstract calculus; it provides the physical and metaphysical elaboration that ties the theory to reality. The picture of reality carried in Newton's mechanics, for instance, offended many men because in it particles of matter could affect other particles of matter without being in contact with them; it offended others because it seemed to make the world too deterministic and to leave no place for the free action of God or the freedom of the human mind. Leibnizian ideas on mechanics, on the other hand, were associated with his theory of "monads," which had a somewhat mystical character and was simply unacceptable in an individualistic and materialistic age; matter as hard massy particles did not have real existence for Leibniz. Neither theory was really able to solve the great problem of the age, the elimination of the mind-matter duality that had become explicit in the writings of René Descartes (1596-1650).

The interpretative ideas associated with Newtonian and Leibnizian mechanics were both in conflict with physical and metaphysical notions, stemming from Descartes, which maintained their influence through most of the period. However, Cartesian mechanics had a completely different relational structure than Newtonian and Leibnizian mechanics, one that could not be forced to agree with experience, so that it died a natural death. The other two theories were left to contend on interpretative or "explanatory" grounds.

Pierre Duhem (1861-1916), in his The Aim and Structure of Physical Theory, wrote that such interpretative or "explanatory" ideas associated with physical theory are to be distinguished from what he terms the representational part of the theory, the mathematical and empirical parts. He found in this distinction a key to the understanding of a continuous tradition in science. Since the representational part merely represents the experiential content of the science, it is taken over virtually in tact into any new theoretical system, while the explanatory part of the old theory is discarded.[5]

This idea can be applied to the competition between the theories of Newton and Leibniz. They both contained essentially the same representational parts, but the explanatory aspects of Leibnizian mechanics were too heavily metaphysical for the taste of the age, whereas Newton, although he in fact made just as many and as far reaching metaphysical assumptions as Leibniz, explicitly disclaimed any such content to his theory. In the famous "I frame no hypotheses" statement, Newton seemed to indicate that his work was purely mathematical and empirical, and this concept of the nature of science was to become a guiding principle in the eighteenth century. By the end of the period scarcely a single reference to anything of a metaphysical nature can be found in the writing of the leading mechanicians, and, correspondingly, the name of Leibniz was no longer mentioned in connection with the science of mechanics.

The rejection of metaphysics in favor of mathematics and empiricism that occurred in the eighteenth century is part of the modern mentality, so that contemporary writers on science and the history of science tend to hold the belief that apriori metaphysical ideas, or religious notions having to do with the world of nature, are purely detrimental to productive scientific work. Duhem gave classic expression to this idea, in terms of his distinction between the explanatory and representative parts of physical theory.

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[5] Pierre Duhem, The Aim and Structure of Physical Science, trans. Philip P. Wiener (Princeton, New Jersey: Princeton University Press, 1954), p. 32.

> Now it is very far from being true that the explanatory part is the reason for the existence of the representative part, the seed from which it grew or the root which nourishes its development; actually the link between the two parts is nearly always most frail and most artificial. The descriptive part has developed on its own by the proper and autonomous methods of theoretical physics; the explanatory part has come to this fully formed organism and attached itself to it like a parasite.
>
> It is not to this explanatory part that the theory owes its power and fertility; far from it. Everything good in the theory, by virtue of which it appears as a natural classification and which confers on it the power to anticipate experience, is found in the representative part; all of that was discovered by the physicist while he forgot about the search for explanation. On the other hand, whatever is false in the theory and contradicted by the facts is found above all in the explanatory part; the physicist has brought error into it, led by his desire to take hold of realities.[6]

While this estimate of the role of metaphysical, explanatory ideas is born out to a certain extent by the history of mechanics in the period under consideration, a real question still remains as to the part played by such ideas in the inception of a physical theory such as Newtonian mechanics. In fact, an examination of the basic ideas upon which Newton raised his imposing theoretical structure will show that these ideas were almost all first expressed in distinctly metaphysical or religious writings of Newton's time or even earlier.

Whatever the source of the ideas that Newton used as the basis for his mechanics, his theory achieved a synthesis of a number of formerly distinct theories. Prior to Newton's work there were at least three separate sciences dealing with subjects of a mechanical nature, all with their own concepts and axioms: the science of simple machines, or "mechanics"; the science of impact phenomena; and the science of the motion of freely falling bodies, projectiles, and pendulums.

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[6] _Ibid_.

Although Newton was the first theoretician to successfully explain all the phenomena in these various areas by means of a single theory, he was by no means the first to attempt such a theoretical unification. Neither was he the last, even in his life time, for, as has been indicated, his theory was either incomprehensible or unacceptable to many. Thus, the history of mechanics in the period can also be viewed under the aspect of a process through which several separate theories are unified in a higher synthesis. It is helpful to define such a development in logical terms as a preface to the historical treatment, just as the logical analysis of theoretical structure is an aid to the historical analysis of theory development in the manner discussed above.

The explanation of a theory or set of experimental laws dealing with phenomena in a given field of inquiry by a theory formulated for some other field is known as the reduction of the former, or secondary, theory to the latter, or primary, theory.[7] Thus, since Newton's mechanics can be said to have been formulated primarily to deal with the motion of bodies under the influence of central forces, the third of the separate mechanical sciences listed above, the unification which his theory achieved can be described as a reduction of the secondary theories of simple machines and impact phenomena to the primary theory of the motion of bodies moving under the influence of central forces [--freely falling bodies, etc.] or "dynamics."

There are basically two types of reductions. In the first, deductive relations are established between two sets of statements--

[7]Nagel, p. 338.

theories--that employ the same vocabulary. In the second, where the vocabularies are dissimilar, a set of characteristics of one subject matter is assimilated into a set of characteristics which seem quite different; the primary theory then seems to wipe out familiar distinctions of the secondary theory as superfluous. That is, some of the characteristics assumed by the secondary science to be fundamentally significant will no longer be so.[8]

An example of what is meant by the first type of reduction, would be the use of the term "force" in the vocabularies of all of the separate theories mentioned. The unification of these theories involves the establishment of logical relationships between the various meanings of this term, and by means of these relationships, the other terms in the theories may be interrelated in a deductive system. An instance of the second type of reduction can be seen in the reduction of the science of impact to that of the motion of bodies under the influence of central forces. The latter science made no use of the notion of "collision" in its theoretical structure, and, following the reduction, the previously basic distinction between elastic and inelastic collisions became non-essential; these two phenomena were then only special cases of a more general phenomena.

From the point of view of the historian of science, the interesting aspect of the logical definition of reduction lies primarily in the conditions under which the reduction is possible;these conditions must point to the historically significant developments leading toward the unification

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[8] Ibid., pp. 338-340.

of separate theories. Such unification is one of the most important aspects of the history of science since it has long been the aim of scientific thought to attain to a unified understanding of the whole phenomenal world. (It is possible to argue that this is not the goal of science, that is, of all scientists, but virtually all that is vital in contemporary science can be seen as an attempt to reduce every science to fundamental-particle physics.)

The formal conditions for theory reduction are as follows. The axioms, special hypotheses, and experimental laws of the sciences involved in the reduction must be known as explicitly formulated statements whose various terms have fixed meanings. These meanings must be fixed either through generally recognized definitions or established experimental procedures.[9] For instance, before any synthesis of the mechanical sciences could take place, such terms as "quantity of matter" or mass, had to be given a fixed empirical meaning. This was one of Newton's greatest achievements, although it was obscured in the _Principia_.

Thus one part of any history of a theoretical unification must deal with the formulation of each of the secondary theories; the elaboration of their concepts as well as of their relational structure.[10] This leads to a second formal condition for reduction, in the case where the primary and secondary theories do not employ the same vocabulary.

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[9] _Ibid._, p. 345.

[10] Actually, the relational structure of a theory partially defines all the theoretical concepts by stating the way in which they are interconnected with other concepts. The empirical meaning of the concept is independent of this partial definition, but clearly must be in harmony with it. See _ibid._, pp. 91-93.

In that case the primary and secondary sciences usually have a number of terms in common, but, before its reduction the secondary science uses terms and asserts experimental laws with their help, that do not occur in the primary science.[11] Since the meanings of expressions in a theory are partially defined by its relational structure or abstract calculus, the relational structure of the primary theory must provide logical connections between theoretical terms in the primary science and terms peculiar to the secondary science. If, for instance, the primary science deals with microscopic and the secondary science with macroscopic phenomena, the abstract calculus of the primary theory must provide a means of making the transition between the two realms.

This condition for reduction of theory is particularly applicable to Newton's synthesis of mechanics since the study of the motions of falling bodies and of projectiles had, since the middle ages, made use of concepts like instantaneous velocity and acceleration, while the sciences of simple machines and impact phenomena were formulated entirely in terms of observable entities. Thus the Newtonian synthesis depended on the construction of an abstract calculus capable of drawing together into a single deductive framework all the expressions used in mechanics, both of instantaneous or infinitesimal and of finite character.

This gives some insight into why the infinitesimal calculus was described by Euler as providing "understanding." The calculus was necessary to the explanation of the secondary mechanical sciences; through the calculus, their laws and axioms could be reduced to instantaneous motions

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[11] Ibid., pp. 351-352.

of infinitesimal particles, or aggregates of these. That this particular sort of reduction appeared as "understandable" is in itself worthy of some investigation and analysis. Of course it did not appear so to many thinkers, at least at the beginning of the eighteenth century and earlier, but became more and more natural as time went on.

This increasing acceptability of reduction of all phenomena to events occurring at the infinitesimal level is of course related to what has been called the interpretive or explanatory part of theory. Clearly, even though Newton disclaimed any explanatory hypotheses, they are built into his theory in its calculus and constitute the strength of the theory. The question is, how was it possible that such a theory could eventually appear as one without metaphysical content. Or, how did the notion that everything is composed of infinitesimal bodies moving under the influence of certain forces become so common as to be a self-evident and uncritically accepted truth, rather than a metaphysical doctrine.

The answer to this sort of question must lie, in part, in considerations of a non-scientific character, in matters ranging from the moral and religious to the economic and political. Therefore, the history of the assimilation of Newtonian mechanics during the period following the publication of the Principia should deal with any social and idealogical factors contributing to or impeding that assimilation. Further, since, as has been stated earlier, almost all the basic ideas upon which Newton founded his theory had been expressed in non-scientific writings, the theory itself might be, in part at least, attributable to the same social and idealogical factors which contributed to its ultimate triumph.

Thus it appears that the key to the problem posed by this work lies in the translation into mathematical language of a certain approach to reality--essentially the atomistic conception of Lucretius--from which all the attendant problems outlined above with regard to specific prevailing conditions and specific personalities may be derived.

This is not to say that either Newton or Leibniz thought of themselves as followers of Lucretius, even though they both held that the world was composed of certain fundamental, irreducible parts. On the contrary, both men felt that their mathematical, mechanical systems were nothing more than representations of a world totally dependent on God. Therefore, that the formal system should survive, stripped of all its original meaning and clothed with significance completely foreign to the intentions of its originators, is an example of the irony of history.

Even though, from a modern scientific point of view the interpretative part of a theory is virtually a matter of indifference, on the broad stage of history it is of the utmost significance. The misunderstanding of Newton and Leibniz on this score, and the consequent distortion of their thought seem to represent a universal trend.

> Consider for a brief moment certain great names of our time, which prides itself on a dominant identity enhanced by scientific truth. Darwin, Einstein, and Freud . . . would certainly deny that they had any intention of influencing, say, the editorials or the vocabulary, or the scrupulosity of our time in the ways in which they undoubtedly did and do. They could, in fact, refute the bulk of the concepts popularly ascribed to them, or vaguely and anonymously derived from them, as utterly foreign to their original ideas, their methodology and their personal philosophy and conduct. Darwin did not intend to debase man to an animal; Einstein did not preach relativism; Freud was neither a philosophical pansexualist nor a moral egotist. Freud pointed squarely to the psychohistorical

> problem involved when he said that the world apparently could not forgive him for having revised the image of man by demonstrating the dependence of man's will on unconscious motivation, just as Darwin had not been forgiven for demonstrating man's relationship to the animal world, or Copernicus for showing that our earth is off-center. Freud did not see a worse fate, namely that the world can absorb such a major shock by splintering it into minor half-truths, irrelevant exaggerations, and brilliant distortions, mere caricatures of the intended design. Yet somehow the shock effects the intimate inner balance of many, if not all, contemporary individuals, obviously not because great men are understood and believed, but because they are felt to represent vast shifts in man's image of the universe and of his place in it--shifts which are determined concomitantly by political and economic developments. The tragedy of great men is that they are leaders and yet the victims of ideological processes.[12]

Thus the study of the assimilation of Newton's mechanics must deal with a number of widely variant but, nonetheless interrelated problems. The first of these is the logical, mathematical character of Newton's theory; the identity of the relational structure of Newton's mechanics with the calculus of fluxions must be demonstrated. The contrast between this new theory and previous theories in the "secondary" mechanical disciplines must then be brought out as well as the more or less continuous developments in mathematics and kinematics which culminated in Newton's synthesis and in that of Leibniz. It is in association with the development of this mathematical treatment of motion that the metaphysical concepts and problems of the period were generated, and these must be seen both in their influence on the formation of theory and on the process of assimilation itself.

Such metaphysical ideas, before they attain to expression, and certainly before they become influential in the thought of an age, are

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[12]Erik H. Erikson, Young Man Luther, A Study in Psychoanalysis and History (New York: W. W. Norton & Company, Inc., 1958), pp. 177-178.

implicit in the general cultural life of the age. Therefore, some attention must be given to social, political, and artistic forms in the period, especially since it is only on the basis of such consideration that any distortion of the thought of the great intellectual leaders that occurs during the process of assimilation can be really understood.

In this particular study, the process of assimilation takes place largely in the context of a dialogue between the followers of Newton and those of Leibniz, and to a lesser extent of Descartes. These two groups coincide almost exactly with a geographical grouping into Englishmen and Continentals, or a linguistic grouping into English and French-speaking people. Therefore, the social and especially the political differences between these groups should have some bearing on the problem. However, since respectable evidence for this sort of influence is rare, because of the very nature of scientific writing, political and social factors will only be mentioned where their influence can be seen explicitly, and the bulk of the study will be concerned with the internal development of mechanical theory as it appears in the writings of the period.

The writing on mechanics after Newton may be arranged for study along the lines already indicated in four major groups: English Newtonians and Anti-Newtonians up to about 1730; French thinkers in mechanics between Descartes and Pierre Varignon (1654-1722); Leibniz, Jean Bernoulli (1667-1748), and French mechanicians using the Leibnizian form of analysis; and finally English writers on mechanics making use of the calculus of fluxions, principally Colin Maclaurin (1698-1746).

Once the work of these men had been accomplished, the formal structure of the new mechanics was made clear and available for the surge of theoretical development in mechanics that took place in the second half of the eighteenth century. Nothing essentially new had been added to the theory, as it was conceived by Newton or by Leibniz and it is not clear that anything was taken away from it either, but still, the idea of the universe underwent a considerable change during the period under consideration; it was far more "mechanical" at the end that it had been at the beginning. That is, the universe became self-sufficient in proportion as the formal structure of mechanics was elaborated in an understandable fashion.

CHAPTER I

THE MECHANICAL THEORY OF NEWTON'S PRINCIPIA

The purpose of this chapter is to show explicitly the scope and nature of Newton's theory of mechanics, that is, to show that it was a general theory of mechanics whose logical structure was fundamentally that of the infinitesimal calculus. By "general theory" it is meant that the theory was designed to explain all mechanical phenomena: not only the motion of freely falling bodies, pendulums, and projectiles (which has been referred to as "dynamics" above), but also impact phenomena and the operation of the simple machines. The generality of the theory--the reduction of the secondary sciences of impact phenomena and simple machines to dynamics--is based on the shift from the traditional geometrical logical structure to the logical structure of the calculus of fluxions, a shift which Newton appears to have been at some pains to conceal.

The logical character of the <u>Principia</u> is in fact not immediately apparent because Newton wrote it "in geometry." However, even though the great bulk of calculation in the working out of the theory is cast in the framework of traditional geometry, the logical structure is that of the calculus. This structure was simply introduced into the theory in the form of certain lemmas which will be discussed later. This idea is supported in the preface to Newton's <u>Treatise of the Method of Fluxions</u>, where it is stated that "although the propositions in that book [the Principia] for the sake of elegance are demonstrated in the synthetic way according to the manner of the ancients . . . yet it is well known that they were first discovered by the use and application of some kind of analysis."[1]

The mathematical, logical character of the Principia, as has been stated, is the source of the generality of Newton's mechanical theory, and this generality centers on the concept of force. Each of the secondary mechanical sciences made use of the term "force," but the meaning of force in these sciences was not clearly fixed. Therefore, it is to the force concept that one must look in order to understand the process of reduction of all mechanical sciences to dynamics. Further, since Newton's concept of force will prove meaningful only in the context of the calculus, it is not surprising that there would be a great deal of confusion among the early Newtonians, who were not versed in the calculus, precisely over the meaning of force.

The approach to the concept of force in Newton's mechanics, and to its relationship with motion--the basic logical relation of the theory--must be made through the postulates of the theory and the accompanying definitions. Through an analysis of these statements the precise relationship between force and motion can be brought out.

The First Law states that "every body continues in its state of rest, or of uniform motion in a right line, unless it is compelled to

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[1]Isaac Newton, A Treatise of the Method of Fluxions and Infinite Series With Its Application to the Geometry of Curved Lines (Translated from the Latin original not yet published; London, T. Woodman and J. Millan, MDCCXXXVII), p. iv.

change that state by forces impressed upon it."[2] The Second Law states that "the change of motion is proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed."[3] For reasons to be brought out later, it is sufficient to examine only these two laws at present.

In the explanation accompanying the First Law, Newton attempted to make it appear as a simple empirical generalization drawn from everyday experiences; projectiles continue their motions in so far as they are not retarded by air resistance, tops spin, planets and comets preserve their motions. However, the explanation accompanying the Second Law is different and, from a modern point of view, bewildering. It states that if a given force generates a given motion, then double the force will generate double the motion, regardless of the time which elapses during the application of the force.[4] From the absence of the time factor it appears that Newton's "motive force impressed" does not coincide with the modern force concept (which is related to acceleration rather than to simple change of motion).

The definition of "impressed force" provided by Newton turns out to be of little help in providing understanding. "An impressed force is an action exerted upon a body, in order to change its state, either of

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[2] Isaac Newton, Sir Issac Newton's Mathematical Principles of Natural Philosophy and His System of the World. Translated by Andrew Motte, 1729. Translation revision and historical appendix by Florian Cajori (Berkeley, California: University of California Press, 1947), p. 13.

[3] Ibid.

[4] Ibid.

rest, or of uniform motion in a right line."[5] This definition, when applied to the two laws, yields two statements: every body continues in its state of rest or of uniform motion in a right line, unless it is compelled to change that state by actions exerted on it in order to change its state, either of rest or of uniform rectilinear motion; and, the change of motion is proportional to the action which causes it. These statements are merely specializations of the principle of sufficient reason and of the causality principle respectively, with the addition, not original with Newton, that uniform rectilinear motion is a natural state of a body.

Thus far there is no indication as to the meaning of force and nothing that gives any insight into the structure of the theory. However, it can be seen that both impressed force and its accompanying change of motion are produced by some agent. It follows from this that impressed force and change of motion are quantities of the same nature. This was necessary, for, as Newton wrote in his Treatise of the Method of Fluxions, "things only of the same kind can be compar'd together, and also their velocities of increase and decrease."[6]

Thus, while "motive force impressed" and change of motion are basic terms of the postulates of the theory and are proportional to each other, their real relationship must be contained in their common cause, the action which produces them both. That action should also be the "force," which is the cornerstone of the theory. In order to get at the

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[5] Ibid., p. 2.

[6] Newton, Treatise of the Method of Fluxions, p. 26.

action producing impressed force and change of motion, it is necessary to have an independent determination of impressed force, one that does not involve the change of motion.

The definition of the "motive quantity of a centripetal force" provides the necessary separate determination of impressed force. The motive quantity of a centripetal force is defined as the "measure" of centripetal force and is "proportional to the motion which it generates in a given time."[7] Newton immediately identified this quantity with weight, so that impressed force is generated by weight, or by some weight-like endeavor, in time. Thus, the relation between impressed force and weight is the same as the relation between change in motion and acceleration; the latter generates the former in time.[8]

But what is the nature of weight? Newton distinguished three aspects of centripetal force: its absolute quantity, its accelerative quantity, and its motive quantity. Of these he said,

> I refer the motive force to the body as an endeavor and propensity of the whole towards a center, arising from the propensities of the several parts taken together; the accelerative force to the place of the body, as a certain power diffused from the center to all places around to move the bodies in them; and the absolute force to the center, as endued with some cause, without which those motive forces would not be propagated through the spaces round about . . . .
>
> Wherefore the accelerative force will stand in the same relation to the motive, as celerity does to motion. For the quantity of motion arises from the celerity multiplied by the quantity of matter; and the motive force arises from the accelerative force multiplied by the same quantity of matter.[9]

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[7]Newton, Principia, p. 4.

[8]In this connection see E. J. Dijksterhuis, The Mechanization of the World Picture. Trans. by C. Dikshooru (Oxford: At the Clarendon Press, 1961), pp. 470-473.

[9]Newton, Principia, p. 5.

Thus the weight of a body is the product of its quantity of matter and the accelerative force which characterizes the space occupied by the body and which is propagated from some center. The impressed force in a body is then equal to its quantity of matter multiplied by an accelerative force characteristic of its place and by the time during which the accelerative force acts.

This statement is still not a separate determination of impressed force since it contains some terms not yet defined. Quantity of matter, according to Definition I, "is the measure of the same, arising from its density and bulk conjointly."[10] This definition is not satisfactory, but for the present it can be assumed that quantity of matter is a determinable quantity; the apparent circularity will be discussed later in connection with Newton's Third Law. This leaves accelerative force as a quantity not determined except through its effect.

However, according to the above, accelerative force is characteristic of a place, or of space, so that Newton's concept of space should give some notion as to the possibility of its separate determination. There must be some active principle associated with space that is capable of providing the logical link between it and accelerative force. In typical 17th century fashion Newton saw this connection in God.

In the general scholium at the end of Book III of the Principia, Newton wrote that God

> is eternal and infinite, omnipotent and omniscient; that is, his duration reaches from eternity to eternity; his presence from infinity to infinity; he governs all things, and knows all things that are or can be done. He is not eternity or infinity, but

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[10] Ibid., p. 1.

> eternal and infinite; he is not duration or space, but he endures and is present. He endures forever and is everywhere present; and by existing always and everywhere he constitutes duration and space.[11]

The evident relationship presented in this passage between space, time, and deity, which amounts almost to identity, is further enhanced by Newton's insistence on the absolute character of space and time.

> I. Absolute, true, and mathematical time, of itself, and from its own nature, flows equably without relation to anything external . . . .
>
> II. Absolute space, in its own nature, without relation to anything external, remains always similar and immovable.[12]

The form which these statements take is one that had been traditionally reserved for statements about God: the only being whose very nature it is to be and to act. Now there must be a clear relation between God and force in order that the connection between place or space and accelerative force be complete.

Newton was somewhat less explicit about the relationship between God and force, but there is evidence that he believed God to be directly and immediately responsible for the existence and action of forces such as gravity. In a letter to Richard Bentley--a chaplin to the Bishop of Worcester who had delivered a series of sermons entitled "A Confutation of Atheism" in 1692--Newton wrote "You sometimes speak of gravity as essential & inherent to matter: pray do not ascribe that notion to me, for ye cause of gravity is what I do not pretend to know . . . ."[13]

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[11] Ibid., p. 545.

[12] Ibid., p. 6.

[13] Isaac Newton, "Newton to Bentley, 17 January 1692/3," The Correspondence of Isaac Newton, ed. H. W. Turnbull (Cambridge: At the University Press, 1961), III, p. 240.

However, Bentley was less reticent, and in his reply to Newton's letter, stated that gravity "is above all Mechanism or power of inanimate matter, & must proceed from a higher principle and a divine energy & impression."[14] This remark is contained in the third of six propositions found at the beginning of Bentley's letter.

Newton began his reply to this letter of Bentley by saying "Because you desire speed I'l answer your letter wth what brevity I can. In ye six positions you lay down in ye beginning of your letter I agree with you."[15] Thus Newton gave his assent to the idea that the force of gravity, which is of course an accelerative force, is the product of divine energy. In the same letter he gave additional support to this notion by saying that "Tis unconceivable that inanimate brute matter should (without ye mediation of something else wch is not material) operate on and affect other matter wthout mutual contact; as it must if gravitation in the sense of Epicurus be essential & inherent in it."[16] On the basis of this evidence it seems safe to say that Newton traced the origin of accelerative forces directly to God.[17]

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[14] Richard Bentley, "Bentley to Newton, 18 February 1691/3," The Correspondence of Issac Newton, III, p. 247.

[15] Newton, "Newton to Bentley, 25 February 1692/3," The Correspondence of Isaac Newton, III, p. 253.

[16] Ibid., pp. 253-254.

[17] For further discussion of this point see A. Rupert Hall and Marie Boas Hall (eds.) Unpublished Scientific Papers of Isaac Newton from the Portsmouth Collection in the University Library, Cambridge ("Introduction to Part III, theory of Matter"; Cambridge: At the University Press, 1962), pp. 193-194. See also Alexander Koyré, From The Closed World to the Infinite Universe (New York, Evanston, and London: Harper and Row, [1958]), pp. 209-217.

Finally, in the General Scholium of the Principia, Newton wrote that "we know Him [God] only by His most wise and excellent contrivances of things, and final causes. . . . And thus much concerning God; to discourse of whom from the appearances of things, does certainly belong to Natural Philosophy."[18]

Now God constitutes duration and space and is also the origin of irreducible accelerative forces, so that these entities are in some sense unified in God. This unification can achieve a mathematical expression through the concept of mathematical function; force is a function of duration and space. This allows the writing of an equation for it constitutes an expression or determination of force which is independent of the effect of force, a change of motion.

Newton did introduce force into celestial mechanics in just this way, through the gravitational hypothesis. The gravitational hypothesis itself is stated in Corollary I to Proposition LXXV of Book I of the Principia. "The attractions of Spheres toward other homogeneous spheres are as the attracting spheres applied to the squares of the distances of their centers from the centers of those which attract."[19] Here Newton is talking about the motive quantity of a centripetal force, the product of the mass of a body with the accelerative force obtaining in its place. Thus, in the above proposition, the accelerative forces are represented by the reciprocal of the squares of the distances between the centers; they are functions of space.

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[18] Newton, Principia, p. 546.

[19] Ibid., p. 198.

In this manner, God, the logical and actual connection between space and force is suppressed in the mathematical formalism and the result is a theory that has an empirical character.

> Good empirical and experimental natural philosophy does not exclude from the fabric of the world and the furniture of heaven immaterial or transmaterial forces. It only renounces the discussion of their nature, and, dealing with them simply as causes of the observable effects, treats them--being a <u>mathematical</u> natural philosophy--as mathematical causes or forces, that is as mathematical concepts or relations . . . . As for Newton himself, he is so deeply convinced of the reality of these immaterial and, in this sense, transphysical forces, that this conviction enables him to devise a most extraordinary and truly prophetic picture of the general structure of material beings.[20]

The nature of force is however only partially elucidated by what has been brought out thus far. The meaning of force must be further determined by means of the logical relationships contained in the postulates of the theory, that is in the Laws. The Second Law now has the meaning that a weight-like force, expressible as a function of space, multiplied by a time during which it acts will produce a definite change of motion. But this could be strictly true only for a constant force. If the force varies with space, then the law could be approximately true only for very short time intervals.

In addition to this consideration, there is the notion that only quantities of the same kind, as well as their velocities of increase and decrease, can be set equal. It is not immediately obvious that a weight multiplied by a duration is of the same nature as a motion. But, if the matter is considered on an infinitesimal basis, the weight-like force then can be understood as the instantaneous increase of the impressed force and the change of motion as the instantaneous increment of motion. Or, it

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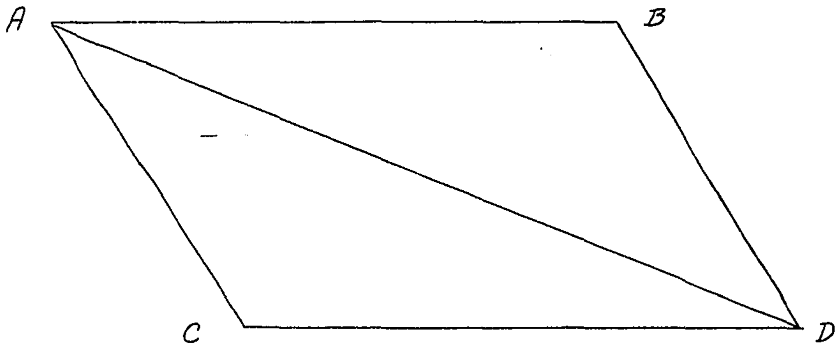
[20] Koyré, p. 213.

can be said that the action of a weight-like force through some time interval is equivalent to a series of instantaneous impressions of force.

Thus while the equality of impressed force and change of motion is the basic relationship of Newtonian mechanics, this equality is only understandable in terms of infinitesimals, that is, of instantaneous changes, which as has been shown, result from the action of divine energy. The infinitesimal character of the theory can be further demonstrated through the first two corollaries to the laws of motion. The first corollary states that "a body acted upon by two forces simultaneously, will describe the diagonal of a parallelogram in the same time as it would describe the sides by those forces separately."[21] The demonstration of the corollary is carried out through consideration of <u>constant</u> velocities, according to the following figure.



If a force M, "impressed apart in the place A," such as to cause a body to move with uniform velocity from A to B in a given time be applied simultaneously with a force N, also impressed in the place A,

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[21]Newton, <u>Principia</u>, p. 14.

but tending to cause the body to move uniformly from A to C in the same time, then by the action of both forces, the body will move in the diagonal AD of the parallelogram ABDC.

This is the case because N acts parallel to BD and will not affect the time required for the body to arrive at BD, by the Second Law. Similarly, M will not affect the time required for the body to arrive at CD, so that at the end of the time interval the body will be at D. It must move in the diagonal AD, by the First Law, since no forces act upon it during the motion.[22]

The significance of Corollary I is made clear in Corollary II: "And hence is explained the composition of any one direct force AD, out of any two oblique forces AC and CD . . . ."[23] Corollary I is the "proof" of Corollary II. However, the "direct forces" mentioned in Corollary II are clearly meant to represent weights while Corollary I deals with impressed forces in terms of constant velocities. The question is, in what sense is Newton justified in extending the result achieved in the first corollary to the second.

In the first, constant velocities are conceived of as lines generated by points moving with constant velocity. These lines AB and AC determine a parallelogram, and hence a diagonal AD, which expresses their relationship, their sum. This relation clearly holds good for the velocities thus represented and also for the impressed forces proportional to them. However, as has been pointed out, impressed force is

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[22] Ibid.

[23] Ibid., p. 15.

the product of a weight-like force and a duration, or weight is the instantaneous increment of impressed force. Therefore, by extending Corollary I to application in the situation of Corollary II, Newton clearly says that he is concerned with an instantaneous event. This conclusion receives support from the fact that the force M was "impressed in the place A."

The conception of impression of force in a place A, or of an instantaneous generation of impressed force leads to the idea of impact. Now, from what has been brought out thus far, weight is the instantaneous increment of impressed force, and, as such, is proportional to the instantaneous increase in motion. Weight, therefore, can be thought of as the result of impact; its action in time--the production of a finite change of motion--is then really the summation of an indefinitely large number of impacts occurring within a given time interval.

Thus Newton's force concept can be seen to be in line with the views of the age to the effect that the basic process of mechanics is that of impact. However, he has succeeded in constructing a mathematical representation that allowed him to treat a succession of impacts as a continuous process, that is, the result of the action of an agent that has a continuous character in space--weight, or the motive quantity of a centripetal force.

It is open to question whether or not Newton took seriously the idea that centripetal or gravitational force was really caused by the impacts of material bodies of some sort striking the gravitating body. There is, as has been shown, much evidence to support the contention that Newton thought God to be the force-producing agent in space. But he did, at times, indulge in speculations on the possibility that the motions of a material aether might be responsible for gravity. In this connection, the Halls have written that

> The very fact that he speculated at all on the aether as a mechanism to account for the forces attributed to material particles gratified the prejudice of an age that, lacking any concept of field-theory, loathed the notion of action at a distance and saw in the push-and-pull mechanism of an aether the only escape from it. Faced with a choice between a universe of Cartesian, billiard-ball mechanism rewritten in Newtonian terms and a universe requiring the inconceivable concept of action at a distance, the seventeenth, eighteenth, and nineteenth centuries preferred the former. But, because this was so, and because Newton himself shared the general contempt for the notion of action at a distance, we should not suppose that Newton was unaware of the distinction between an hypothesis and a theory; nor should we suppose that his speculations on the aether were the foundation of his theory of matter, when in fact they were at most no more than hypothetical ancillaries to it.[24]

In a sense, Newton had retained the Cartesian notion that motion is only imparted through impact, or, more specifically, a quantity of motion, an impressed force, or a momentum is transferred to a body only in discrete events. However, these discrete events, with the aid of the concepts of the calculus and his concept of weight could be treated as a continuous process. Both the continuity of the transfer of momentum and the relation between weight and momentum--weight is an instantaneous increment of momentum--were of extreme importance for the solution of the problem of the motion of the planets. The weight-momentum relation was necessary in order to relate the motion of the planets to the concept of gravity, and the continuity of the transfer of momentum was necessary to the treatment of motion in curved lines.

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[24]Hall and Hall, p. 193.

Also from these corollaries it can be seen in what manner Newton intended to represent the increment of motion to which impressed force was proportional. Both motion and increments of motion would be represented by the length of a straight line or by a combination of straight lines. Then, in order to make this representation suitable for the treatment of motion in curved lines, recourse must be made to infinitesimal durations between impressions of motion, which would be the same as motion under the influence of an accelerative central force.

Of course motion in curved lines was of supreme importance for Newton, as was the idea of motion under the influence of central forces, since he wished to explain the motion of the heavenly bodies. Therefore, it is likely that the mathematical methods of representation of curves had some bearing on the formulation of the two corollaries just discussed. It was of course the analysis of curves, or of motion in curved lines, that provided the field of development for the calculus, so that at this point the identity of the logical structures of the mechanics and the calculus must become more explicit.

Newton's first attempt at a finished exposition of the calculus was probably "To Resolve Problems by Motion," dated October, 1666.[25] In this work Newton attacked, as a first step in the treatment of curves, the problem of drawing tangents to "crooked lines." From the statement of the problem the means of representation of the curved line can be seen to correspond to the above representation of motion as a series of straight lines, or combinations of straight lines, each representing

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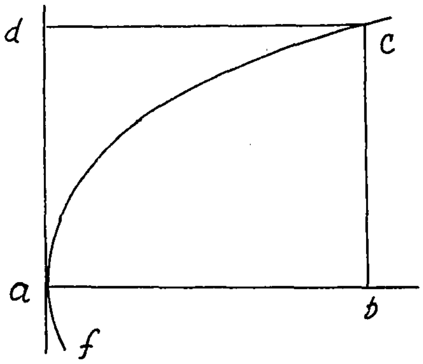
[25]Hall and Hall, p. 15.

an instantaneous impression of force.

> Seeke . . . ye motions of those streight lines to wch ye crooked line is chiefely referred, & with what velocity they increase or decrease: & they shall give . . . ye motion of ye point describing ye crooked line; wch motion is in its tangent.[26]

The solution to the problem is based on a description of the curve as the intersection of two straight lines each moving in a direction perpendicular to itself such that the lengths of the two lines, measured from some fixed frame or reference, are in a constant functional relationship. "If ye crooked line fac is described by ye intersection of two lines cb and dc ye one moveing parallely, viz:



cb//ad and dc//ab; soe yt if ab=x, and bc=y=ad, their relation is $x^4-3yx^3+ayxx-2y^3x+a^4=0$."[27] This relation between the lengths of the lines ab (x) and ad (y) represents the curve, and from it can be found a relationship between the velocities of increase of both x and y at any point of the curve. These two velocities can then be represented as a pair of perpendicular lines determining a small rectangle at the point on the curve. The diagonal of the rectangle is a

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[26]Isaac Newton, "To Resolve Problems by Motion," Unpublished Scientific Papers of Isaac Newton, p. 34.

[27]Ibid.

tangent to the curve at that point and its length represents the velocity along the curve, and thus, by the Second Law, also the impressed force in the direction of the motion. This is clearly similar to the kinematic situation of Corollary I to the Laws, and makes evident the manner in which force as a function of space can be related to the change of motion of a body moving along a curved path in a scientifically meaningful way.

This sort of representation of curved lines--a functional relationship between the lengths of two straight lines--is common to the Principia as well. For instance, in Book I, Section I, where Newton developed the method of the first and last ratios of quantities, the representation is basic. Section I consists of a series of lemmas which serve as a basis for the treatment of centripetal force. "These lemmas are premised to avoid the tediousness of deducing involved demonstrations ad absurdum . . . and now these principles being demonstrated, we may use them with greater safety."[28]

Lemmas II, III, and IV are all concerned with the relationship between curvilinear figures and figures composed of their inscribed and circumscribed parallelograms as the number of the parallelograms is increased and their breadth diminished "in infinitum." Here also, the curved, or "crooked" line is reduced to a set of intersecting straight lines. If the curve itself is represented as a functional relation between lengths of two sets of intersecting straight lines, reference lines, then, as stated above, the instantaneous motion along the curve at any point, which is represented by the tangent at the point, is

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[28] Newton, Principia, p. 38.

compounded of velocities, or rates of change of length, of the reference lines intersecting at the point.

However, it is the instantaneous change of motion along the curve that can be set proportional to the motive quantity of a centripetal force. The instantaneous change of motion is itself compounded of the instantaneous rates of change of the velocities of generation of the intersecting reference lines. The question now is, are the velocities of change of length of the lines, and the changes in these velocities, determinable from the functional relationship holding between the reference lines themselves? The answer to this question was given affirmatively by Newton in his calculus of fluxions.

Thus the calculus of fluxions can be seen to be an integral aspect of the theory of mechanics; the basic relationship of the theory the Second Law, is an infinitesimal one, that is, one dealing with motion occurring in a point of space and an instant of time, and, in order to have any empirical significance, this relation must be provided with a logical connection to the world of experience, that is, to observable positions in space.

In other words, an instantaneously impressed force, which is the same as the motive quantity of a centripetal force, or a weight, and can be expressed as a function of space, can be equated with an instantaneous change of motion along a curved line; the sum of the instantaneous values of the impressed force and the inertial force in a moving body yield its instantaneous motion along the curved path. This motion is itself a function of the lengths of the reference lines whose relationship is the mathematical representative of the curve, and is known, or can be ascertained from observation. In this way, a logical connection is established between the position of an object in space and its motion.

However, in order to fully explain the Laws of Motion it still remains to indicate the actual means by which Newton could deduce the necessary relation between the velocities of increase and decrease of lengths of lines from the functional relationship of those lengths. Then the solution to this problem, the core of the calculus of fluxions, must be related to the actual procedure of calculation used in the application of the theory. But before proceeding to the discussion of this problem, it is necessary to clear up the matter of the Third Law of motion and the question as to the meaning of the term "quantity of matter."

The definition of quantity of matter given above was that it is the product of density and volume, or bulk. At first sight this seems to be circular since density would seem to depend on quantity of matter. However, in his "De gravitatione et aequipondio fluidorum," Newton stated that "bodies are denser when their inertia is more intense, and rarer when it is more remiss."[29] This definition is repeated in the Principia in Corollary IV to Proposition VI of Book III which states that "by bodies of the same density, I mean those whose inertias are in the proportion of their bulks."[30]

Thus quantity of matter is dependent on inertia, or the innate force of matter. Once this has been established, then it is possible to

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[29] Isaac Newton, "De gravitatione et aequipondio fluidorum," Unpublished Scientific Papers of Isaac Newton, p. 150.

[30] Newton, Principia, p. 414.

determine quantity of matter through an application of the Third Law of Motion. "Those bodies are equipollent in their impact and reflection whose velocities are inversely as their innate forces."[31] For instance, if the impacts of two objects dropped from measured heights on to a lever at equal distances from the fulcrum balance each other--are "equipollent"--then the relationship $m_1/m_2 = v_2/v_1 = \sqrt{h_2/h_1}$ holds, where m_1 and m_2 are the innate forces, or masses, v_1 and v_2 the respective velocities, and h_1 and h_2 the respective heights.

Thus it is the Third Law which gives empirical content to the theoretical notion of mass and hence also of motion. As will be shown, it is the Third Law which makes Newtonian Mechanics a physical theory; for the First and Second Laws are purely mathematical in character. That is, they contain the abstract relational structure of the theory, which, as has been stated and is yet to be shown, is the same as that of the calculus of fluxions.

The calculus of fluxions is presented in the Principia in a compressed form, and then not as a complete exposition of the method. However, the answer to the problem mentioned above--from a functional relationship between lengths to deduce the relation between the velocities of increase and decrease of those lengths--is approached in Lemma II of Book II.

The Lemma states that "the moment of any genitum is equal to the moments of each of the generating sides multiplied by the indices of the powers of those sides and by their coefficients continually."[32] A

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[31] Ibid., p. 26.

[32] Ibid., p. 249.

genitum is defined as any quantity produced by the operations of multiplication, division, and extraction of roots, rather than through addition or subtraction of parts. These quantities are to be conceived as variable and indetermined, as increasing or decreasing by a continuous flux. The momentary increments or decrements of a genitum are called moments and are not to be conceived as finite in magnitude, but as the "just nascent principles of finite magnitudes," and can be thought of as the _velocities_ of the increments and decrements--"fluxions of quantities."

> Wherefore the sense of the Lemma is, that if the moments of any quantities A, B, C, &C., increasing or decreasing by a continual flux, or the velocities of the mutations which are proportional to them, be called a, b, c, &c., the moment or mutation of the generated rectangle AB will be aB + bA; the moment of the generated content will be aBC + bAC + cAB . . .[33]

The proof of the lemma is carried out in a number of cases, but Case 1 is basic to all the rest. There Newton considered a rectangle AB increasing by a continuous flux. The sides A and B have the moments a and b respectively. At the instant when the sides have the length A-1/2a and B-1/2b the area of the rectangle is AB-1/2aB-1/2bA+1/2ab. At the instant when the sides have the length A+1/2a and B_1/2b, the area is AB+1/2aB+1/2bA+1/2ab. The increment in area corresponding to the full increments to the sides is thus aB+bA.[34]

This lemma does not constitute an _explicit_ answer to the problem of finding the relationship of the "fluxions" of quantities since the fluxions, a and b, are given, but it contains the necessary relationships. In the scholium immediately following the lemma Newton made

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[33] _Ibid_., pp. 249-250.

[34] _Ibid_., p. 250.

reference to a "general method which extends itself . . . not only to the drawing of tangents to any curved lines, whether geometrical or mechanical . . . but also to the resolving other abstruser kinds of problems, . . ." and then stated that "the foundation of that general method is contained in the preceding Lemma."[35]

Therefore we may turn to that more general method in order to find the relation between the fluxions of functionally related quantities, which is necessary to the full understanding of the Laws of Motion as developed above.

In the above reference to a general method of dealing with problems related to curved lines, Newton was referring to a treatise composed in 1671.[36] The exposition of the calculus of fluxions to which we now turn was written in October, 1666, and "appears to be Newton's most complete exposition of his methods up to that time."[37] There is no reason to think that the method underwent any major conceptual changes between 1666 and 1671, especially since the method described in the earlier treatise, "To Resolve Problems by Motion," is perfectly compatible with the expositions of the method contained in the Principia.

"To Resolve Problems by Motion" begins with a list of eight propositions, of which the seventh is logically the most important: the proofs of the others are dependent on it. Proposition 7 states that,

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[35]Ibid., pp. 251-252.

[36]This reference is to the Methodus fluxionum et serierum infinitarum. Carl B. Boyer, The History of the Calculus and its Conceptual Development (New York: Dover Publications, Inc., 1949), p. 193. See below pp. 93-98.

[37]Hall and Hall, p. 5.

> Haveing an Equation expressing ye relation twixt two or more lines, x, y, z &c: described in ye same time by two or more moveing bodys A, B, C, &c: the relation of their velocities p, q, r &c may be thus found, viz: Set all ye terms on one side of ye equation that they become equal to nothing. And first multiply each terme by so many times p/x as x hath dimensions in yt terme. Secondly, multiply each terme by so many times q/y as y hath dimensions in it . . . . The summe of all these products shall be equal to nothing. Wch equation gives ye relation of ye velocities p, q, ...[38]

This proposition alone suffices to determine the relationship between fluxions in terms of the functional relationships between the lengths of lines, of which the fluxions are the velocities of generation.

In order to get at the relational structure of the calculus of fluxions, it is necessary to examine the demonstration of proposition 7. The demonstration itself is preceded by a lemma which states that if two bodies, A and B, move uniformly, from a to c, d, e, f and from b to g, h, k, l respectively, in the same time; then the lines ac and bg, cd and gh, etc. are as the velocities, p and q, of those bodies. And even if the motion of the bodies were not uniform, still the "infinitely little" lines which they describe each "moment" are as the velocities with which they describe them. That is, A with velocity p will describe the line (cd=) p X o in an instant (o represents an instant of time) and B with velocity q will describe the line (gh=) q X o, since p:q::po:qo. Then if the described lines are (ac=)X and (bg=)Y, in one instant, they will be (ad=) X+po and (bh=)Y + qo in the next.[39]

This lemma serves to relate instantaneous velocities to infinitesimal distances through the relationship of uniform velocities

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[38]Newton, "To Resolve Problems by Motion," Unpublished Scientific Papers, pp. 17-18.

[39]Ibid., pp. 31-32.

of equal duration to the finite distances which they describe. In other words, infinitesimal quantities stand in the same relationship to each other as the analogous finite quantities; they are qualitatively the same. This is explained in the lemma by saying that no matter how velocity may vary along a line, if distances described are taken to be infinitely small, the velocities with which they are described will have constant values over the distances. Then, since the time intervals are also infinitely small, they are "equal," and the ratio of the velocities will be equal to the ratio of the distances.

These basic ideas were formulated in the Principia in greater generality and conciseness in lemma I, Section I of Book 1. Section I deals with the "method of first and last ratios of quantities"[40] and provides a basis for the determination of centripetal force from the motions of bodies. Lemma I of this section states that "quantities, and the ratios of quantities, which in any finite time converge continually to equality, and before the end of that time approach nearer to each other than by any given difference, become ultimately equal."[41] This is a generalization of the notion that the ratio of velocities p/q approaches the ratio of the distances, $\frac{pt}{qt}$, as the times intervals are decreased in infinitum.

This lemma is fundamental to the determination of centripetal forces from the motion of bodies. Its similarity to the above lemma from "To Resolve Problems by Motion" is another indication of the role played

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[40]Newton, Principia, p. 29.

[41]Ibid.

by the calculus in Newtonian mechanics. The "method of first and last ratios," at first glance, appears to differ from the method developed by Newton in "To Solve Problems by Motion," but they will be shown to be equivalent.

Returning to the demonstration of proposition 7 of "To Solve Problems by Motion,"

> Now if ye equation expressing ye relation twixt ye lines x and y bee $x^3 - abx + a^3 - dyy = 0$. I may substitute x + po and y + qo into ye place of x and y; because (by ye lemma) they as well as x and y, doe signify ye lines described by ye bodys A & B. By doeing so there results
>
> $$\begin{array}{l} x^3 + 3poxx + 3ppoox + p^3o^3 - dyy - 2dqoy - dqqoo = 0 \\ \qquad\qquad -abx - abqo \\ \qquad\qquad\qquad\quad +a^3 \end{array}$$
>
> But $x^3 - abx + a^3 - dyy = 0$ (by supposition). Therefore there remains onely
>
> $$\begin{array}{l} 3poxx + 2ppoox + p^3o^3 - 2dqoy - dqqoo = 0. \\ \qquad\qquad - abpo \end{array}$$
>
> Or dividing it by o tis
>
> $$\begin{array}{l} 3px^2 + 3ppox + p^3oo - 2dqy - dqqo = 0 \\ \qquad\qquad -abp \end{array}$$
>
> Also, those terms are infinitely little in wch 0 is. Therefore, omitting them there rests 3 pxx - abp - 2dqy = 0. The like may be done in all other equations.[42]

This demonstration rests on three basic ideas. Firstly, that straight lines are generated by the uniform motions of points (preceding lemma). Secondly, any such line may be considered as a composite of at least two other lines X and Y, and thirdly, any curved line can be conceived as composed of infinitesimal straight segments such that it can be characterized by a functional relationship between X and Y--in the above demonstration by the function $X^3 - abx + a^3 - dyy = 0$. From these

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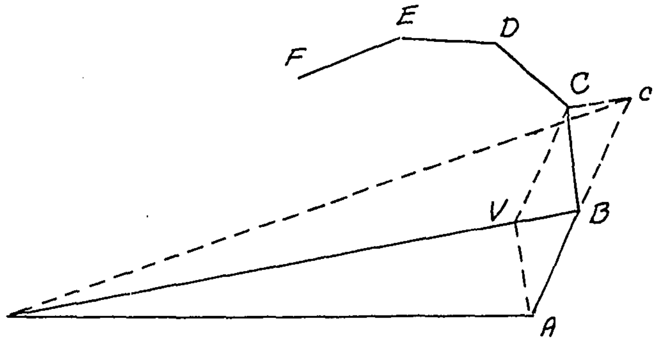
[42]Newton, "To Resolve Problems by Motion," Unpublished Scientific Papers, p. 32.

three postulates Newton was able to derive an expression for the ratio of the "fluxions," that is the instantaneous velocities of change, of x and y, or $\dot{x}$ and $\dot{y}$, ($\dot{x}$ and $\dot{y}$ replace p and q in later notation) from the expression for the curve as a function of x and y.

With $\dot{x}$ and $\dot{y}$, the instantaneous motion along any curved path can be calculated. However, it is the <u>change</u> in motion which is equivalent to impressed force. All that would be necessary to make this additional step would be to perform a similar calculation with a substitution of, for instance, $\dot{x} + \ddot{x}o$ and $\dot{y} + \ddot{y}o$ for $\dot{x}$ and $\dot{y}$, where $\ddot{x}$ and $\ddot{y}$ would be velocity <u>increments</u>, or "second fluxions," and o represents an instant. Thus, this mathematical theory constitutes a solution to the problem of establishing a connection between instantaneous velocities of increase and decrease of lengths of lines--$\dot{x}$ and $\dot{y}$--and the functional relation of the lengths, x and y, or between the instantaneous motion along the curve and the algebraic representation of the curve. The procedure can also be extended to represent instantaneous changes of motion, which in turn can be related to force. Thus from a knowledge of force in terms of space variables, the path of the motion may be deduced.

However, Newton did not in fact make use of precisely this procedure in the <u>Principia</u> in establishing the relationship between forces and curves, or orbits. Rather, he used the method of first and last ratios of quantities, mentioned earlier, which is a variation on the above method that permits a more conventional, geometrical treatment.

A typical example of Newton's use of this method is found in Corollary III to Proposition I, Theorem I, Section II of Book I. The theorem states that "the areas which revolving bodies describe by radii drawn to an immovable center of force do lie in the same immovable places, and are proportional to the times in which they are described."[43] The corollary states that "if the chords AB, BC, and DE, EF of arcs



described in equal times . . . are completed into parallelograms ABCV, DEFZ, the forces in B and E are one to another in the ultimate ratio of the diagonals BV, EZ, when those arcs are diminished in infinitum."[44]

Thus, the instantaneously impressed force, which is proportional to the diagonals BV, EZ, and becomes, in the "ultimate ratio," the continuous change of motion as the "arcs are diminished in infinitum," is related to a curve which has the verifiable property that a radius to a point moving along the curve sweeps through equal areas in equal times. Further, Corollary IV established a relationship between forces at various points along the curve.

> The forces by which bodies . . . are drawn back from rectilinear motions and turned into curvilinear orbits, are to each other as the versed sines of arcs described in equal times . . . when those arcs are diminished to infinity. For such versed sines are the halves of the diagonals mentioned in Cor. III.[45]

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[43]Newton, Principia, p. 40.

[44]Ibid., p. 41.

[45]Ibid., p. 42.

Now, the versed sine of an arc of a curve is one half of the vertical distance from the tangent, drawn at one end of the arc, to the curve at the other end. This quantity is however proportional, as the arc is decreased *in infinitum*, to the second derivative, or the ratio of second fluxions of the curve. Thus the two approaches to the problem of relating the force on a moving body to the curve it follows are equivalent, the one "algebraic" in form, the other "geometrical." Further, since the problems just discussed are fundamental to the treatment of the motion of bodies under the influence of centripetal or gravitational force in Newton's mechanics, the manner of their solution indicated the basic logical structure of the theory.

In both methods--that of the calculus of fluxions or that of first and last ratios--the fundamental notions are that straight lines are generated by the uniform motions of points, that these lines can be resolved into two or more other lines, and that curved lines can be reduced to an infinite succession of infinitesimal segments. These same ideas are implicit in the Laws of Motion and the accompanying definitions and corollaries, where they are cast into a physical framework by means of the concepts of mass and force. The Laws state first that a body in motion with no forces acting on it will generate a straight line. Secondly, that any force may be resolved into components is deduced as a corollary of the Laws, and the deduction depends on the concept of uninfluenced motion as the generator of straight lines. Finally, it is implied in the Laws that curvilinear motion requires the impression of a force proportional to the instantaneous change of motion at every point of the motion.

Newtonian mechanics, as presented thus far, is a highly articulated system of mathematical thought that moves "from the phenomena of motions to investigate the forces of nature, and from these forces to demonstrate the other phenomena."[46] The "other phenomena" with which the Principia is mainly concerned are the motions of the heavenly bodies, but, as stated earlier, the Principia contains a perfectly general mechanical theory which is equivalent to the "classical" formulations of mechanics. This equivalence has been elaborated in terms of mathematical structure, both formulations being based on the calculus, but there is still another aspect of equivalence between the Newtonian and "classical" formulations that is of significance, the unification of dynamics and statics, or the science of simple machines.

The unification of theories was described earlier in terms of the concept of reduction of theories, of a secondary theory to a primary one. Also, with regard to the theoretical development inherent in the Newtonian synthesis, the science of dynamics was designated as the primary theory while the other mechanical sciences, impact phenomena, statics, etc., were designated as secondary theories. The implication in this was primarily that the laws and concepts of the secondary theories, in this case statics, are subsumed under and explained by laws and concepts formulated in the realm of the primary science, dynamics.

The reduction of statics to dynamics involved nothing more than the clarification of the force concept, which has already been described, and the extension of the Third Law of Motion to include the concept of

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[46] Ibid., pp. XVII-XVIII.

static equilibrium. However, out of this arose the possibility of dealing with dynamic states of machines using the same formal principle that was traditionally employed in the purely static treatment of machines, the equilibrium principle.[47] The manner in which this was accomplished is laid out in the treatment of machines contained in the scholium following the Laws of Motion.

There Newton wrote that

> . . . the power and use of machines consist only in this, that by diminishing the velocity we may augment the force, and the contrary; from whence, in all sorts of proper machines, we have a solution to this problem: _To move a given weight with a given power_, or with a given force to overcome any other given resistance. For if machines are so contrived that the velocities of the agent and the resistant are inversely as their forces, the agent will just sustain the resistant, but with greater disparity of velocity will overcome it. So that if the disparity of velocities is so great as to overcome all that resistance which commonly arises either from the friction of contiguous bodies as they slide by one another, or from the choesion of continuous bodies that are to be separated, or from the weights of bodies to be raised, the excess of force remaining, after all these resistances are overcome, will produce an acceleration of motion proportional thereto, as well in the parts of the machine as in the resisting body. But to treat of mechanics is not my present business. I was aiming only to show by those examples the great extent and certainty of the third Law of Motion. For if we estimate the action of the agent from the product of its force and velocity, and likewise the reaction of the impediment from the product of the velocities of its several parts, and the forces of resistance arising from the friction, cohesion, weight and acceleration of those parts, the action and reaction in the use of all sorts of machines will be found always equal to one another. And so far as the action is propagated by the intervening instruments, and at last impressed upon the resisting body, the ultimate action will always be contrary to the reaction.[48]

The first thing of importance in this passage is Newton's explicit realization that an excess in the force of the agent over the

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[47]The development of the theory of simple machines and of the equilibrium principle will be discussed in Chapter IV.

[48]_Ibid_., pp. 27-28.

resistance of the load--a non-equilibrium condition--will accelerate the machine and the load. However, since the purpose in treating machines at all was to show "the great extent and certainty of the third Law of Motion," which is a generalized form of the equilibrium concept, the equality of action and reaction had to be somehow established even in this dynamic situation.

The equilibrium principle itself provided an indication of the solution to this problem, for Newton had based the equilibrium principle on the consideration already cited that "in the use of mechanic instruments those agents are equipollent . . . whose velocities, estimated according to the determination of the forces, are inversely as the forces."[49] From this notion of equilibrium was taken the concept of "action" as force times velocity.

Since Newton had at his disposal the concepts of instantaneous velocity and instantaneous change of velocity, or acceleration, which was associated with the instantaneous action of a force or weight, it was possible to conceive of an instantaneous reaction as proportional to the product of a velocity and an acceleration. Thus there was a "force arising from acceleration," or, what is now called an inertial force, that could be used to extend the idea of equilibrium to a state of accelerated motion.

If Newton's statement of the application of the Third Law of Motion to machines from the above quote were transcribed into a modern notation it would look like this:

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[49] Ibid., p. 26.

$$fv = \sum_i v_i (f_{friction_i} + w_i + f_{cohesion_i} + m_i a_i),$$

where f = force, w = weight, m = quantity of matter, a = acceleration, and v = velocity. The symbol $\sum_i$ indicates a summation over all i parts of the machine, including the load. This would be a normal equation of static equilibrium except for the term $m_i a_i$, which is treated as though it were of the same nature as a weight.

In a machine, all the velocity ratios, v/v_i, are constants of the machine, so that ignoring all but the last term on the right in the above expression results in the equation

$$f = \sum_i c_i m_i a_i,$$

where c_i represents the constant velocity ratio. The force in this equation is weight; this is clear from the context. Furthermore, it is equal to mass, or quantity of matter, times acceleration.

However, force and the product of mass and acceleration are disparate quantities, and their equality is, in any case, not an expression of the Second Law, which is concerned with impressed force and generated motion. The only way to solve these difficulties is to conceive weight as the instantaneous increment of impressed force and the product of mass and acceleration as the instantaneous increment of quantity of motion, as has been already indicated. Then if impressed force and generated motion are proportional, so are their velocities of increase.

Using the same argument as in the demonstration of proposition seven of "To Resolve Problems by Motion," if F is the impressed force;

f, its velocity of increase; v, the generated velocity; and a, its velocity of increase, then the Second Law states that $F + fo \sim m(v+ao)$. But, since $F \sim mv$, it follows that, after dividing through by the tiny time interval o, $f \sim ma$.

Thus Newton's treatment of machines, involving the extension of the Third Law of Motion to dynamic situations so that they may be conceived of as systems in equilibrium, depends on the concept of weight as the velocity of increase of impressed force, which, in turn, implies the application of ideas fundamental to the calculus of fluxions. This can be taken as an instance of the unification of the mechanical sciences and its dependence on the fact that the logical structure of Newtonian mechanics is identical with that of the calculus.

Thus far, Newton's theory has been presented simply as an accomplished fact, with no indication as to any historical development leading up to his vast accomplishment. That there must have been such a development is clear from the fact, already mentioned, of the independent discovery of the calculus by Leibniz. In fact, such a phenomenon as an independent and virtually simultaneous discovery of a complex mathematical logic suggests that the ideas fundamental to that logic must be the more or less common intellectual property of the age. If this were indeed the case it would be a fact of considerable importance from the standpoint of assimilation of the theory.

One prominent aspect of the common intellectual background of the seventeenth century was the complex of ideas that goes under the name of "mechanism;" very generally, the idea that everything is the result of the configurations and motions of material particles. An investigation of this basic approach to nature, through the writings of one of its most powerful exponents should show whether or not the fundamental ideas of the new mechanics, and of the calculus, were implied by the mechanistic approach to reality.

The essence of "mechanism" is expressed in the modern notion of causality, or the causal definition of an object or phenomenon. The causal definition provides understanding of something by telling how it is generated or made. This form of definition, the basis for the mechanical understanding of nature was, according to Ernst Cassirer, first fully understood by Thomas Hobbes (1588-1679). Hobbes was "the first modern logician to grasp this significance of the 'causal definition.'"[50] This suggests that the physical thought of Thomas Hobbes would yield the clearest insight into the question of whether or not the basic ideas of the calculus and the new mechanics are implicit in the mechanistic approach to the world.

There are other reasons for choosing Hobbes as an example par excellence of the mechanistic approach. According to his biographer, Sir Leslie Stephen, he "was the most conspicuous thinker in the whole period between Bacon and Locke . . . ."[51] Of course a great deal of his prominence, or notoriety, arose from non-scientific writings and his reputation as an atheist, but these things stemmmed from a basic mechanistic view of reality.

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[50]Ernst Cassirer, The Philosophy of the Enlightenment (Princeton, N.J.: Princeton University Press, 1951), p. 254.

[51]Leslie Stephen, Hobbes (Ann Arbor, Michigan: The University of Michigan Press, 1961), p. 1.

Hobbes' basic ideas on the nature of reality were formed on the basis of the most advanced scientific thought of his day--that of Galileo--and he enjoyed close contact with the development of physical thought during the years that he devoted to philosophy. This contact came about through the agency of Marin Mersenne (1588-1648), who served the European scientific community as "a central depot of information and a general channel of communication."[52] Through Mersenne, Hobbes was put into contact with such important natural philosophers as Descartes and Pierre Gassendi (1592-1655).[53]

Both Descartes and Gassendi were mechanistic thinkers, but Hobbes, while influenced by them, went beyond the position of either to one of extreme mechanism and materialism.[54] Hobbes carried the idea of mechanism to its logical conclusion, complete atheism, a conclusion that brought down upon him the condemnation of virtually everybody. In fact, his influence was, in a sense negative--there never were any "Hobbesians" in the sense that there were Baconians and Newtonians.[55] However,

> Hobbes exerted a subtle but powerful influence on his critics: he imposed upon them his own strict, rational standards of argument. He obliged them to meet him on his own grounds, to combat him with his own weapons of logical exactitude and severe reasoning. He caused them, for purposes of argument, to lay aside their theological presuppositions and moral predilections, and to try the

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[52]Herbert Butterfield, The Origins of Modern Science, 1300-1500 (New York: Macmillan Co., 1960), p. 71.

[53]Stephen, pp. 24, 32-33.

[54]J. Bronowski and Bruce Mazlish, The Western Intellectual Tradition from Leonardo to Hegel (New York, Evanston, and London: Harper and Row, 1962), p. 196.

[55]S. I. Mintz, The Hunting of Leviathan (Cambridge: At the University Press, 1962), p. 147.

> issues on their own merits. Thus by his very provocation, Hobbes endowed the thought of his critics with a strong rationalist impulse, . . . The critics were satisfied that they had cut Hobbes down to size; in fact they had yielded, slowly and imperceptibly but also very surely to the force of his rationalist method.[56]

Perhaps the controversy between Hobbes and John Wallis (1618-1703) is a case in point. Wallis was one of the first mathematicians of the day and the author of the Arithmetica Infinitorum, which was an important step towards the development of the calculus.[57] Thus, in the verbal and mathematical struggle that developed out of Hobbes' claim to have squared the circle, Hobbes, who was not much of a mathematician, was at a considerable disadvantage. Nonetheless, the controversy itself made clear the superiority of the mathematical methods of Wallis, which led to those of Newton, which in turn can be seen to be at least partially a logical outgrowth of the fundamental position of Hobbes.

Hobbes's ill-fated attempt to square the circle was contained in Chapter 20 of his De Corpore, which appeared in Latin in 1655 and in English translation in the following year.[58] The attention attracted by the book was due to the rather inept circle-squaring, and little or no attention was paid to "its contributions to logic and scientific method or . . . [to] the boldness of the metaphysical scheme for which it laid the foundation."[59]

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[56] Ibid., p. 149.

[57] Stephen, p. 52.

[58] Stephen, p. 50.

[59] Richard S. Peters, (ed.), Thomas Hobbes, Body, Man, and Citizen. Selections from Thomas Hobbes (New York: Collier Books, 1962), p. 16.

The basis of Hobbes's metaphysical and physical thought lay in his idea of causality, formulated in the De Corpore.

> For whatsoever is produced, in as much as it is produced, had an entire cause, that is, had all those things, which being supposed, it cannot be understood but that the effect follows; that is, it had a necessary cause. And in the same manner it may be shown, that whatsoever effects are hereafter to be produced, shall have a necessary cause; so that all the effects that have been, or shall be produced, have their necessity in things antecedent. . . .
>
> And from this, that whensoever the cause is entire, the effect is produced in the same instant, it is manifest that causation and the production of effects consist in a certain continual progress; so that as there is a continual mutation in the agent or agents, by the working of other agents upon them, so also the patient upon which they work, is continually altered and changed.[60]

Here Hobbes has indicated that the explanation of any phenomenon must be constructed in terms of necessary causes of the phenomenon. These causes and their effect are in a temporal relationship of a peculiar nature. An entire cause is simultaneous with its effect, so that any phenomenon is the result of a continuous process, that is, not only the effect, but also the causes are characterized by continuous change. If this is the case, then it is necessary to consider causes in terms of infinitesimals, a conclusion which Hobbes drew.

One of the concepts evolved by Hobbes to deal with the motion of bodies was that of "endeavor."

> I define ENDEAVOR to be motion made in less space and time than can be given; that is, motion made through the length of a point, and in an instant or point of time. For the explaining of which definition it must be remembered, that by a point is not understood that which has no quantity, or which cannot by any means be divided; for there is no such thing in nature; but that, whose quantity is not at all considered, that is, whereof neither quantity nor any part is computed in demonstration; so that a point is not to be taken for an indivisible, but for an undivided thing; as

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[60] Thomas Hobbes, Body, Man, and Citizen, p. 117.

> also an instant is to be taken for an undivided, and not for an indivisible time.[61]

"Endeavor" is a change of place conceived as occurring in an instant, and the velocity with which the change takes place, the "velocity of endeavor," Hobbes defined as "impetus."[62] These infinitesimal quantities were related to the idea of force, which was the

> impetus *or quickness of motion multiplied either into itself, or into the magnitude of the movent, by means whereof the said movent works more or less upon the body that resists it*.[63]

That is, force is equal either to the square of the instantaneous velocity of the movent or to the product of its instantaneous velocity and its magnitude.[64]

In these concepts, Hobbes has attempted to lay the groundwork for a form of explanation of natural phenomena which proceeds from infinitesimal, indetermined, and therefore fundamental motions as causes to observable phenomena as effects. This he termed the "compositive" method, by which

> we are to observe what effect a body moved produceth, when we consider nothing in it besides its motion; and we see presently that this makes a line or a length . . . and so forwards, till we see what the effects of simple motion are; and then, in like manner, we are to observe what proceeds from the addition, multiplication, subtraction and division of these motions, and what effects, what figures, and what properties, they produce . . . .[65]

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[61] *Ibid*., p. 132.

[62] *Ibid*., pp. 132-133.

[63] *Ibid*., p. 136.

[64] "The *extension* of a body is the same thing with the *magnitude* of it, . . .", *Ibid*., p. 103.

[65] *Ibid*., p. 76.

The "compositive" method bears a strong resemblance to Newton's brief statement of method cited above: "from the phenomena of motion to investigate the forces of nature, and from there to demonstrate the other phenomena." In both cases, the consideration of motion begins with motion in a point and proceeds to the explanation of finite motions.

Thus, to a considerable extent, the ideas fundamental to the calculus were present in Hobbes' thought, as logical implications of his concept of causality, which explicitly employed the notion of the continuity of causes and of effects, as well as their simultaneity. Hobbes was not enough of a mathematician to exploit these ideas, and, in any case there was an important omission in his system.

The missing element in Hobbes' analysis of motion was some way of obtaining a description of causes that was different from that of their effects. He saw the need for continuity in the causes producing continuously changing motion--an idea that was later to be raised to the level of a first principle of both mechanics and the calculus by Leibniz--but could conceive the cause of motion in a body, the exertion of force, only as the action of another body. In this connection Hobbes wrote that "*when any body is moved which was formerly at rest, the immediate efficient cause of that motion is in some other moved and contiguous body*."[66] Therefore, in order to handle the motion of a body along a curved path he must be able to mathematically describe the motions of the "movents" which cause it to deviate from a straight line motion.[67]

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[66] *Ibid*., p. 131.

[67] That Hobbes was aware that some action was necessary to cause a moving body to deviate from motion in a straight line is apparent from

This is the problem that Newton was able to overcome by making force a property of space and an action of God, that is, by virtually identifying space and God. Such a solution to the problem would have been out of the question for Hobbes even if he had thought of it; to conceive force as the action of God was not in line with the mechanistic view of reality. Newton's use of the idea thus suggests that yet another aspect of the intellectual background of the age entered into the synthesis represented by his theory of mechanics: namely, a tradition stemming from Plato and Aristotle which held that non-material entities were real and irreducible factors in the existence and functioning of the physical world.

This tradition had undergone a revival, or more accurately perhaps, a "renaissance," during the fifteenth century mainly through the activity of the Florentine Platonists Giovanni Pico della Mirandola (1463-94) and Marsilio Ficino (1433-99). For Pico and Ficino there existed a duality of mind and nature, as indeed with all Platonists. However in contradistinction to the mind-matter duality later developed by Descartes and incorporated in the mechanistic view of the world,

> this duality is not allowed to become an absolute dualism of the Scholastic-medieval variety. For the polarity is not an absolute, but a relative opposition. The difference between the two poles is only possible and conceivable in that it implies a reciprocal relationship between them. Here we have before us one of the basic conceptions of Florentine Platonism, one which was never completely submerged or extinguished by opposing currents of thought or by the tendency towards 'transcendence. . . .' Transcendence itself postulates and requires 'participation',

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his assertion that "when any body, which is moved in the circumference of a circle, is freed from the retention of the radious, it will proceed . . . in a tangent." Ibid., p. 139.

just as 'participation' postulates and requires 'transcendence'.[68]

This basic tenet of the Platonic approach to the physical world, the participation of the transcendent and immaterial in the natural and material, appeared in the writings of Henry More (1614-88), one of the so-called Cambridge Platonists. However, in the course of a dispute with Descartes, More gave this idea a new form in terms of space.

Henry More does not have a good reputation as a clear and systematic thinker in the history of philosophy. He seems almost to belong to the hermetic or occultist tradition rather than to the philosophical tradition proper, to be

> a spiritual contemporary of Marsilio Ficino, lost in the disenchanted world of the "new philosophy" and fighting a losing battle against it. And yet in spite of his partially anachronistic standpoint, in spite of his invincible trend towards syncretism which makes him jumble together Plato and Aristotle, Democritus and the Cabala, the thrice great Hermes and the Stoa, it was Henry More who gave to the new science--and the new world view--some of the most important elements of the metaphysical framework which ensured its development. . . . Henry More succeeded in grasping the fundamental principle of the new ontology, the infinitization of space, which he asserted with unflinching and fearless energy.[69]

The "infinitization" of space means an identification of space as the frame of reference for the action in the physical world of the transcendent, immaterial, and infinite God. This view of space was set forth in More's <u>Enchiridium metaphysicum</u> published in 1671.

> I have clearly shown that this infinite extension, which commonly is held to be mere space, is in truth a certain substance, and that it is incorporeal or a spirit. . . . This immense <u>locus internus</u> or

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[68]Ernst Cassirer, <u>The Individual and the Cosmos in Renaissance Philosophy</u>, trans. Mario Domandi (New York and Evanston, Ill.: Harper and Row, 1964), pp. 86-87.

[69]Koyré, pp. 125-126.

> space really distinct from matter, which we conceive in our understanding, is a certain rather . . . confused and vague representation of the divine essence or essential presence, in so far as it is distinguished from his life and activities.[70]

This identification of God and space fulfills the Platonic requirement of the participation of the transcendent in the natural in a rather peculiar way. More accepted the common mechanistic notions of the day as regards the ultimate construction of matter as homogeneous atoms. He also accepted the Cartesian notion of conservation of the quantity of motion in the universe. But, like Hobbes, More could not conceive of anything existing without extension.[71] Therefore, it followed that God, or spirit, is an extended being and his participation in the natural world is a matter of moving the homogeneous atoms and arranging them in various configurations.

> Whence, I ask if it be unworthy of a philosopher to inquire if there be not in nature an incorporeal substance which, while it can impress on any body all the qualities of body, or at least most of them, such as motion, figure, position of parts, etc. . . . would be further able, since it is almost certain that this substance removes and stops bodies, to add whatever is involved in such motion, that is, it can divide, scatter, bind, form the small parts, order the forms, set in circular motions those which are disposed for it. . . .[72]

Thus it can be seen that in the thought of Henry More, a friend to Newton,[73] living and writing at Cambridge University where Newton was also situated, there is a sort of synthesis of the mechanistic and

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[70] Quoted by Edwin Arthur Burtt, The Metaphysical Foundations of Modern Physical Science, a Historical and Critical Essay (rev. ed.; New York: The Humanities Press Inc., 1951), p. 141.

[71] Ibid., pp. 128-129.

[72] Ibid., p. 131.

[73] See below, p. 64.

Platonic approaches to the understanding of the physical world. The resulting combination of ideas bears a strong resemblance to Newton's solution of the problem of providing a causal description of the motions of bodies. The concepts of absolute space and time and their connection with the divinity in both writers are particularly striking in their similarity.

One of the most interesting aspects of the similarity of Newton's thought to the ideas of Hobbes and More, beyond the fact that they were antithetical figures,[74] is that both Hobbes and More were basically unacceptable to the English scientific community. Neither of them were really competent natural philosophers in either the experimental or mathematical sense; they were speculative thinkers in an age that demanded concreteness and exactness in its understanding of nature.

The speculative character of their thought carries over into Newtonian mechanics where, however, it is covered up with, or developed into, a system with both mathematical clarity and empirical significance. Of course there are other developments, both of a mathematical and physical character, that are presupposed by the Newtonian synthesis, but these are beyond the scope of the present study. It is Newton's dependence on the speculative, metaphysical thought of the age that has a direct bearing on such things as the formation of the force concept, the logical structuring (abstract calculus) of the theory, and the

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[74] "The warfare against Hobbes was undertaken primarily by the Neo-Platonist school of Cambridge whose chief literary representatives were Ralph Cudworth and Henry More." Wilhelm Windleband, A History of Philosophy, Vol. II: Renaissance, Enlightenment and Modern (New York, Evanston, and London: Harper and Row, 1958), p. 435.

interpretation of the theory, and these are more relevant to the process of assimilation than the concrete physical and mathematical discoveries that lead up to the theory.

There are two questions concerning Newton himself that arise from the dependence of his theory of mechanics on metaphysical speculations of the sort just discussed. First, was he personally involved to any extent in the metaphysical and/or religious problems of his day, and second, what sort of man is it that is capable of creating out of abstract and widely disparate thoughts on the nature of reality a coherent and verifiable description of man's experience of the physical world? A brief sketch of Newton's personality can yield answers to these questions that are significant with regard to the understanding and acceptance of his theory of mechanics by others of his era.[75]

The picture of Newton that appears in modern treatments of his life is usually one of extreme contradictions. For instance, Aldous Huxley wrote of him that he

> created the science of celestial mechanics; but he was also the author of _Observations on the Prophecies of Daniel and the Apocalypse of St. John_, of a _Lexicon Propheticum_ and a _History of the Creation_. With one part of his mind he believed in the miracles and prophecies about which he had been taught in childhood; with another part he believed that the universe is a scene of order and uniformity. The two parts were impenetrably divided one from the other. The mathematical physicist never interfered with the commentator on the Apocalypse; the believer in miracles had no share in formulating the laws of gravitation.[76]

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[75]For the following interpretation of Newton's personality the writer is largely indebted to Kent A. Higgins, "Isaac Newton," a paper submitted as partial fulfillment of requirements for a course in the History of Science at the University of North Dakota, May 1966.

[76]Aldous Leonard Huxley, "The Idea of Equality," _Proper Studies_ (London: Chatto and Windus, 1957), p. 6.

Huxley here depicts Newton as a personality harboring an irreconcilable conflict: the pious believer versus the mathematical physicist. However, from the foregoing analysis of Newton's theory of mechanics, it appears that Newton was able to integrate, to some extent at least, these two seemingly disparate aspects of his mental make-up. In fact the two really irreconcilable things about Newton are not his own inner characteristics, but rather the commonly accepted image of Newton as a scientific saint and the historical Newton.

In assuming that Newton's personality, and therefore his interests as well, were actually integrated, it is still necessary to take cognizance of John Maynard Keynes' observation that "in vulgar modern terms, Newton was profoundly neurotic, of a not unfamiliar type, but--I should say from the records--a most extreme example."[77] This is not to say that Newton's brilliance as a scientist was the result of any neuroses from which he may have suffered. Rather, the obviously neurotic aspects of his character--some of which will shortly be mentioned--can be seen to share a common cause with his genius.

It is not necessary to review Newton's entire life in order to bring out the factor in his inner make-up that lies behind his outwardly contradictory traits. One need only focus on one of the most obvious and hence most easily overlooked facts about Newton and at some of the problematic elements in his life that are directly related to it. That central fact is simply that Newton was a genius of an extremely high order.

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[77] John Maynard Keynes, "Newton the Man," Men and Numbers (New York: Simon and Schuster, 1956), p. 278.

The actual degree of Newton's intelligence--his I.Q.--can only be _estimated_ on the basis of biographical data, and I.Q. itself is perhaps best defined as that which I.Q. tests measure, rather than a real indicator of intellectual capacity. Nonetheless, an estimate of Newton's I.Q. can serve as the basis for a comparison between Newton and contemporary study groups of similar I.Q. rating. In this way, some light may be shed on the great man's relationships with his associates and on the quality of his work, as seen by his contemporaries.

A study of 300 geniuses, among them Newton, including estimates of their I.Q.'s on the basis of biographical data, has been done by Catherine Morris Cox and associates. The figure at which they arrived in Newton's case was an I.Q. of 190.[78] While the available data may not have been the best for such purposes, there is little doubt but what Newton had an intelligence of the very highest order. A rating of 190 I.Q. places Newton in the classification group of 170 I.Q. and beyond.

Psychological researches have shown that above average children and adolescents within an I.Q. range of 125 to 155 experience a very favorable development toward a successful and well-rounded personality. Their superior intelligence provides them with confidence and leadership capacity, and at the same time there are enough of them to make communication and mutual understanding possible.

> But those of 170 I.Q. and beyond are too intelligent to be understood by the general run of persons with whom they make contact. They are too infrequent to find many congenial companions.

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[78]Catherine Morris Cox, _et al_., _Genetic Studies of Genius_, Vol. II, _The Early Mental Traits of Three Hundred Geniuses_ (Stanford University: Stanford University Press, 1926), pp. 60, 365-366.

> They have to contend with loneliness and with personal isolation from their contemporaries throughout the period of their immaturity. To what extent these patterns become fixed, we cannot yet tell.[79]

In the case of Isaac Newton, there is reason to believe that personal isolation from his contemporaries did indeed become a fixed pattern of life. Newton's relationships with his acquaintances and scientific colleagues, as described by Louis Trenchard More, are something less than warm and personal.

> With the exception, perhaps, of Montague, Newton had no intimate and personal friends who penetrated the ivory tower in which he jealously guarded his inner life. How aloof he wished to be is epitomized in his almost agonized cry that he would publish nothing more as it would result only in attracting acquaintance, what he sought most to avoid. Towards Boyle and Wren he showed a deep respect, and next to Montague his most congenial friends were Henry More and John Locke; but even they regarded him as difficult and "nice" to approach. Men of science, such as Hooke, Flamsteed, and Leibniz, who ventured in the same field of work and who felt themselves competent to criticize him, were met by chilling rebuffs.[80]

A particular aspect of the isolation of children of very high intelligence is that, because they are physically unable to keep pace with older children and are indifferent to the play of children of their own age, their play tends to become lonely and sedentary.[81] This trait can be seen in Newton too, for, according to More, Newton "shunned all forms of physical exercise, played no games, and disliked boys." Even at Cambridge, consequently, he was completely out of touch with his fellow undergraduates.[82]

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[79]Leta S. Hollingworth, Children Above 180 I.Q. (Yonkers-on-Hudson, New York: World Book Co., 1942), pp. 94-95.

[80]More, p. 130.

[81]Leta S. Hollingworth, "The Child of Very Superior Intelligence As a Special Problem in Social Adjustment," Mental Hygiene, XV (1931), p. 8.

[82]More, p. 30.

The notion of social isolation as a direct result of great genius also helps explain Newton's penchant for becoming enmeshed in professional controversy while, at the same time, hating such embroilment. His exchanges with Robert Hooke (1635-1703), John Flamsteed (1646-1719), and Leibniz are the most famous of such incidents, and again there is close correspondence to modern observations on the very intelligent child. One of the main difficulties of such children, is, as Hollingworth puts it, to "suffer fools gladly."[83]

This characteristic was recognized in Newton by More as something that was a concomitant to his special genius, and thus More attempted to excuse Newton's quarrels, saying that

> they bulk too large in our estimate of his character as, after all, they occupied but a small part of a long life which was, on the whole, exemplary; and we must make allowances for his constitutional irritability when criticized, a trait which such inordinate flattery as was given to him could not fail to intensify.[84]

However, the point is not to judge Newton, but to see what effects his character might have on his contemporaries. From the above, it would appear that he was esteemed almost to the point of being worshipped--a notion to which we will return--and was correspondingly unapproachable. On the other hand, a case has been made for Newton's humility on the basis of the statement often attributed to him, "if I have seen further, it is by standing on the shoulders of giants."[85] Further support for this view

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[83]Hollingworth, Children Above 180 I.Q., pp. 258-259.

[84]More, pp. 135-136.

[85]Edward Neville da Costa Andrade, "Isaac Newton," Men and Numbers from the World of Mathematics, ed. James R. Newman (New York: Simon and Schuster, 1956), p. 271.

of Newton's character can be drawn from the simile he presumably used to describe his achievements in natural philosophy.

> I do not know what I may appear to the world; but to myself I seem to have been only like a boy, playing on the seashore, and diverting myself, in now and then finding a smoother pebble or a prettier shell than ordinary, while the great ocean of truth lay all undiscovered before me.[86]

This statement would only indicate Newton's awe of the universe, which to him, was pregnant with the divine presence. The statement, also shows him as being essentially alone, so that if he felt indebted to any "giants," he was probably not referring to any of his contemporaries, and certainly not to any of his critics.

Newton's awe of the universe was of a piece with his theological interests, and both of these things can be referred back to behavior patterns common to people of his intelligence group. Hollingworth wrote that

> when we observe young gifted children, we discover that religious ideas and needs originate in them _whenever they develop to a mental level_ past "twelve years mental age." Thus they show these needs when they are but eight or nine years old, or earlier. The higher the I.Q., the earlier does the pressing need for an explanation of the universe occur, the sooner does the demand for a concept of the origin and destiny of the self appear.
>
> In the cases of children who test above 180 I.Q. observed by the present writer, definite demand for a systematic philosophy of life and death developed when they were but six or seven years old. Similar phenomena appear in the childhood histories of eminent persons, where data of childhood are available. Goethe, for example, at the age of nine, constructed an altar and devised a religion of his own, in which God could be worshipped without the help of priests.[87]

Now, an interest in theology was not at all peculiar for men of Newton's time, not even among men of science. In Newton's case, however,

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[86] _Ibid_.

[87] Hollingworth, _Mental Hygiene_, XV, p. 13

this interest seems to be more than the reflection of a common preoccupation. It was with him a particular aspect of an overall behavior pattern; the reverse side, as it were, of his almost total isolation from human contact.

There is one last item that relates directly to Newton's social isolation; the somewhat perplexing fact that he spent a good deal of time simply copying things. With regard to this, I. B. Cohen has commented that:

> Whiston tells us that he wrote out "eighteen copies of the first and principal chapter of the Chronology with his own hand but little different from each other." A theological manuscript in the Keynes collection, the "Irenicum, or Ecclesiastical Polity tending to Peace," is found in seven separate autograph drafts, which are almost identical. Why did Newton copy out so much again and again? Many reasons have been advanced to explain Newton's copying, extracting, and summarizing the books that stood on his own shelf. In the Preface to the catalogue of the Portsmouth Collection it is remarked of the Newton manuscripts on historical and theological subjects; "Much is written out, as if prepared for the press, much apparently from the mere love of writing. His power of writing a beautiful hand was evidently a snare to him." Anyone who has read Newton manuscripts cannot fail to be impressed by the beauty of his handwriting and so this remark contains a grain of truth. But another possible reason is that Newton was a man who did not easily communicate his ideas to others, either by word of mouth or in print. Such a man, lacking close friends, might well satisfy his inner need of expressing himself by writing to himself and for himself and enjoying the experience of writing out that which he could not print. From the enjoyment of reading his own thoughts in the intimacy of his own handwriting, it might not have been so great a step to the habit of transforming portions of books into texts which he could likewise read in his own handwriting. In any event, here is another curious and bewildering aspect of the man Newton.[88]

In the light of what has been said of Newton thus far, Cohen's conjecture seems completely accurate and completes the picture of Newton as a man living in a profound social and intellectual isolation. From

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[88]I. Bernard Cohen, "Newton in the Light of Recent Scholarship," Isis., LI (1960), p. 504.

this it follows that Aldous Huxley was essentially correct when he wrote that "the price Newton had to pay for being a supreme intellect was that he was incapable of friendship, love, fatherhood, and many other desirable things. As a man he was a failure; as a monster he was superb."[89]

The question that now presents itself is, given that Newton's mechanics was logically structured according to a mathematical system almost completely foreign to his age, that its metaphysical content was synthesized from two opposing and relatively unpopular views of reality, and that Newton himself lived in a sort of splendid isolation, how could this theory be assimilated by the men of his times. The answer to this question, as will be shown, is simply that it could not, at least not directly.

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[89]Quoted in Men and Numbers, p. 277.

CHAPTER II

THE NEWTONIANS AND ANTI-NEWTONIANS

The Newtonian synthesis in mechanics effected a reduction of statics to dynamics, first of all by explaining the fundamental concept of statics, the idea of equilibrium, through the Third Law of Motion, and secondly, by extending the Third Law and hence the concept of equilibrium by means of the idea of "inertial force," to cover dynamic states of machines. In this way, even in complex mechanical systems, the state of rest was deprived of its special nature and became just a special case of motion. Fundamental to this reduction was the concept of force as weight acting in an instant of time, and the instantaneous action of weight was conceived by Newton to be the generation of motion. Therefore, the connection between statics and dynamics rested upon this particular connection between weight and motion.

Traditionally, statics, or the science of simple machines, had identified weight with "force," or "power." However, the action of the force or power had no particular relation to time. Similarly, in the science of impact phenomena, the idea of force was essentially independent of time, but there force was taken to be momentum, or quantity of motion. Newton, as has been shown, forged the connection between these two ideas of force, which are represented in the Principia by the terms "motive quantity of a force" and "impressed force" respectively, by saying that the former generates the latter in time.

Therefore, in looking at the mechanical writings of the so-called Newtonians, an important indicator of the degree to which they have assimilated his thought lies in whether they have been able to follow Newton in his unification of statics and dynamics or whether they still treat them as essentially separate sciences with separate and unreconciled notions of force. If the force concept remains unclarified, then it is clear that the logical structure of Newtonian mechanics, which has been identified with the calculus, cannot be present. Further, since Newton's interpretation of his mechanics, in terms of the nature of matter, space, and of physical reality in general, was intimately related to the concepts underlying the calculus, their absence in the writings of the Newtonians would imply a view of reality substantially different from that of Newton. Thus, even with writers who vehemently espoused "Newtonianism," it is conceivable that their ideas bore only a superficial relationship to those of the master.

One of the first influential Newtonians was John Keill (1671-1721), who is said to have been the first to publicly teach Newtonian philosophy, and in particular, to teach it on the basis of the experiments on which it was founded.[1] This by itself casts a shadow of suspicion on the depth of Keill's understanding of Newton, whose Platonic tendencies

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[1]"John Keill," The Philosophical Transactions of the Royal Society of London from Their Commencement in 1665 to the Year 1800; Abbridged with Notes and Biographic Illustrations, V (London: C. & R. Baldwin, 1810), pp. 417-418. This remark was made by J. T. Desagulier (1683-1744), a student of Keill's. "John Keill," Dictionary of National Biography, Vol. X.

have been made explicit. In any case, Keill spent virtually his entire adult life as a proponent of Newtonian philosophy. From 1691, when he followed his professor, David Gregory, to Oxford, until his death, Keill was an active lecturer, writer and polemicist in the Newtonian cause.[2] His major work in the field of mechanics, the Introductio ad veram physicam, went through at least four Latin and two English editions during Newton's lifetime.[3]

Keill was also a member of the Royal Society of London from 1701 until his death and, in this connection, became involved in the controversy between Newton and Leibniz over the question of priority in the invention of the calculus. It was Keill who prepared the refutation of Leibniz's accusation of plagiarism against Newton in 1708, and he also edited a report, called the Commercium Epistolicum, prepared by the Royal Society on the Newton-Leibniz controversy in 1712.[4]

Through all of this it is apparent that Keill was not only a longtime student of Newton's thought and one of his personal admirers, but that Newton had every chance to know the nature of Keill's work. Especially, he had ample opportunity to gain a knowledge of the depth of Keill's understanding of the principles upon which the mechanics of the Principia are based, that is, the principles of the calculus.

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[2]"John Keill," D.N.B., Vol. X.

[3]John Keill, Introductio ad veram physicam, seu lectiones physicae, habitae in schola naturalis philosophiae Academiae Oxonensis. Quibus accedunt C. Hugenii theoremata de vi centrifuga et motu circulari demonstrata (Oxoniae: T. Bennet, 1702). Latin editions two through four appeared in 1705, 1715, and 1719 respectively. English editions appeared in 1720 and 1726.

[4]"John Keill," D.N.B., Vol. X.

However, in turning to Keill's Introductio ad veram physicam, one is confronted with something of a paradox; while the book bears a surface resemblance to Newtonian mechanics, there is in it little attempt to exploit or even to elucidate the basic concepts of the new mechanics. This can be seen in the axioms listed by Keill.

I. There are no properties or affections of a nonentity or nothing.

II. No body can be naturally annihilated.

III. Every mutation induced in a natural body proceeds from an external agent; for every body is but a listless heap of matter, and it cannot induce any mutation in itself.

IV. Effects are proportional to their adequate causes.

V. The causes of natural things are such, as are the most simple, and are sufficient to explain the phenomena: for nature always proceeds in the simplest and most expeditious method; because by this manner of operating the divine wisdom displays itself the more.

VI. Natural effects of the same kind have the same causes: as the descent of a stone and a piece of wood proceeds from the same cause; and there is also the same cause of light and heat in the sun and in the kitchen fire, of the reflection of light in the earth and in the planets.

VII. If two things are so connected together that they perpetually accompany each other, that is, if one of them is changed or removed, the other likewise will be in the same manner changed or removed; either one of these is the cause of the other, or they both proceed from the same common cause.

VIII. Any body being moved in any direction, all its particles which are relatively at rest in it, proceed together in the same direction with the same velocity; that is, a relative place being moved, that which is placed therein will be also moved.

IX. Equal quantities of matter carried along with the same velocity, their Momenta or quantities of motion will be equal.

X. Equal and contrary forces acting on the same body, destroy their mutual effects.

XI. But from unequal and contrary forces there is produced a motion equivalent to the excess of the greater force.

XII. A motion produced from conspiring forces, that is, acting in the same direction, is equivalent to their sum.

XIII. If what is equivalent be either augmented, or its contrary diminished, then it becomes the greater.

XIV. All matter is everywhere of the same nature, and has the same essential attributes, whether it is in the heavens or on the earth, whether it appears under the form of a fluid body, or a hard, or of any other whatever; that is, the matter of any body, for example of wood, does not differ essentially from the matter of any other body whatever.

XV. But the different forms of bodies are nothing but the different modifications of the same matter; and depend on the various magnitude, figure, texture, position, and other modes of the particles composing bodies.

XVI. So likewise the qualities, or actions, or powers, of some bodies on other bodies, arise only from the former attractions and motion conjointly.[5]

Keill's axioms do not represent a single, coherent logical system. The set of axioms does not serve to clearly establish any sort of relational structure between theoretical terms which can themselves be given an empirical significance.

For instance, the axioms that most closely correspond to Newton's Laws are the third, the tenth, and the eleventh (relating to Newton's First, Third, and Second Laws respectively). These axioms of Keill's do not explicitly contain the theoretical terms that are basic to the mechanics. Axiom III mentions only "mutations" instead of changes in the state of motion, and "outside agents" instead of impressed force. Axiom XI makes use of the term "force" rather than the more specific motive force impressed, and Axiom X uses the same term, "force," in place of the mutual actions of bodies on each other, as in Newton's Third Law.

The use of the term "mutation" in Axiom III can be ignored in this connection since Keill evidently meant to include changes in the

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[5]John Keill, An Introduction to Natural Philosophy: or Philosophical Lectures Read in the University of Oxford, Anno Dom. 1700. To Which Are Added the Demonstrations of Monsieur Huygen's Theorems, Concerning the Centrifugal Force and Circular Motion (2nd ed.; London: J. Senex, W. & J. Innys, J. Osborn & T. Longman, 1726), pp. 89-92.

state of motion under the broader concept. However, the use of the unqualified term "force" in Axioms X and XI, along with the substitution of "agent" for impressed force in Axiom III indicates that Keill had no clear idea of the crucial distinction between motive and impressed force. In his definition of "moving" or "impressed" force Keill spoke of it as _causing_ or _changing_ the motion of a body.[6]

This was not the case in the mechanics of Newton, where impressed force, or the force of a moving body, _was_ a motion or an increment of motion, but seen from the _point of view_ of cause rather than of effect--a weight-like, or motive force was the _cause_ of impressed force = change of motion. This deficiency in the conception of force is, by itself, sufficient to preclude the possibility of Keill's mechanics having the same logical structure as Newtonian mechanics. That structure may have been somewhat obscured in the _Principia_, but in the _Introductio ad veram physicam_ it is absent.

The deficiency in the force concept becomes still more apparent in Keill's treatment of machines. The traditional core of the theory of machines was the fundamental theorem of the lever--the forces being inversely as their distances from the fulcrum, or center of motion of the lever, the lever will be in a state of equilibrium. Keill proved this theorem by showing that if the ratio of the power, or moving force, to the weight, or load, is inversely as their respective distances from the center of rotation, then the "momentum" of the power will be the same as the "momentum" of the weight, "and consequently the power will be

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[6] _Ibid._, pp. 85-86.

equivalent to the weight; which if it be ever so little increased will raise the weight."[7]

Keill has made use of Axiom X (that equal and contrary forces acting on the same body destroy their mutual effects) in his demonstration. The way in which the demonstration proceeds is that if the weights are inversely as their distances from the center of rotation and their velocities are directly as these distances, then the products of weights and velocities, the momenta, will be equal, and hence, by Axion X these will destroy each others effects, and the lever will be in equilibrium. Here Keill has identified "force" with "momentum," but "momentum" is weight times velocity rather than the product of quantity of matter and velocity as in Newtonian mechanics.

Thus the Newtonian causal relation between weight and quantity of motion, or momentum, is replaced in Keill's mechanics by saying that weight is simply one of the factors in momentum. From this it follows that Keill could have no understanding of the idea of "inertial force" mentioned earlier in connection with Newton's treatment of accelerated states of machines, or of the extension of the Third Law to mechanical systems in motion. Thus it is not surprising that Keill did not mention the subject in his book, but stayed within the traditional, that is, static, limitations of the mathematical-theoretical treatment of machines. Therefore, although a dynamic concept, that of momentum, lies at the core of Keill's discussion of static problems, he has not been able to effect a true synthesis in the Newtonian manner.

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[7] _Ibid_., pp. 122-123.

Keill's treatment of the phenomena of impact, like his treatment of machines, bears only a superficial resemblance to Newtonian mechanics. There too, the discussion is based on the Third Law, or on his own Axiom X, but resembles the work of earlier writers, John Wallis and Edme. Mariotte (1620-1684), which will be discussed later.

Typical of Keill's approach to impact phenomena, is his discussion of elastic collision. He used a taut string as the model for an elastic body, which, when deformed, "will restore itself with the same force wherewith it was first inflected . . . [which] was equivalent to the momentum of the impinging body . . . ."[8] Therefore, the body will be reflected with the same quantity of motion which it had formerly.

The forces that Keill refers to here are, as before, momenta, so that he is still concerned with motions rather than the causes of motion. In contrast to this, Newton, in his rather brief treatment of elastic collision, had written of the elasticity of a body as having the character of a cause of change of motion, and he therefore associated a force with it. With regard to any given body, that force was "certain and determined, and makes the bodies to return one from the other with a relative velocity, which is in a given ratio to that relative velocity with which they met."[9]

By making elastic force a characteristic of a body, Newton opened up the possibility of an expression of that force independent of the change

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[8] Ibid., p. 183.

[9] Isaac Newton, Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World. Trans. Andrew Motte, 1729. Translation revision and historical appendix by Florian Cajori (Berkeley, California: University of California Press, 1947), p. 25.

of motion which it produces. This is essentially the same thing that was accomplished by making the centripetal force a function of space. Keill, however, in limiting his conception of force to motion or momentum, passed up the possibility of finding a separate determination of force, and therefore a consistent mathematical treatment of motion, in the Newtonian sense, could not be effected.[10]

Such a difference in logical structure as existed between Keill's and Newton's mechanics should carry with it some differences in the interpretation of the theory. In particular, there should be differences in any ideas concerning physical reality that are connected with the concepts of force and weight. As has been shown, Newton was able to conceive of weight, or motive force, as the product of the "extensive" quantity of matter of a body with the "intensive" accelerative force that was a characteristic of space. Keill, on the other hand used weight in place of mass, which suggests that he regarded the gravitational force as a property of bodies themselves.

In his Introductio, Keill stated that although gravity is called an attraction, "it is not intended as a determination of the cause of motion, but is merely a naming of the cause in the manner of the

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[10]It is of significance that John Bernoulli, in 1723, had developed the notion of the elastic force of a body in terms of the calculus. The 1726 English edition of Keill's Introductio, however, contains the treatment just described. See John Bernoulli, "Discourse sur les Loix de la Communication de Mouvement, contenant la Solution de la premiere Question proposee par MM. de l'Académie Royale des Sciences pour l'Annee 1724," Reçueil des pieces qui ont remporte les Prix. Fondez dans l'Academie Royale des Sciences par M. Rouille de Meslay, Conseiller au Parlament: depuis l'Annee 1720 jusqu'en 1728. Avec quelques Pieces qui ont concouru aux memes Prix (Paris: Claude Jombart, MDCCXXVIII), pp. 12-15.

paripatetics."[11] This statement indicates that he was following Newton's lead in refusing to admit to the framing of hypotheses concerning the actual nature of the gravitational force. Nevertheless, he went on, virtually in the same breath, to speak of the propagation of qualities through space.[12] Also, in an article on attractive forces that appeared in the Philosophical Transactions in 1708, Keill laid down the attractive power of matter as a fundamental principle.[13]

The attribution of attractive force to matter itself rather than to a more or less direct action of God in space fits in well with an idea expressed in Axiom V of the Introductio. There Keill used the term "nature" as a subject, in the grammatical sense: "nature always proceeds in the simplest and most expeditious method; because by this manner of operating the divine wisdom displays itself the more."

The implication is that nature functions of itself, and that the economy and efficiency of its functioning displays the wisdom of its divine architect. Thus Keill's popularized Newtonianism reflects the deistic conception of God's relation to the world rather than the Platonic notion of immanence that lay behind Newton's idea of force, and was in harmony with the then existing movement toward liberal protestantism. "From this time on there developed in a remarkable way and with extraordinary speed the tendency to a new type of Protestantism . . . .

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[11] Keill, Introduction, pp. 4-5.

[12] Ibid.

[13] John Keill, "On the Laws of Attraction and Other Physical Principles," Phil. Trans. Abb., V, p. 417.

It was a Protestantism married to the rationalizing movement . . . ."[14] In this way, "Newtonian" mechanics could become laden with powerful religious overtones that were essentially foreign to its original content.

Still, the question may be legitimately raised as to why, if Keill had in fact departed so drastically from Newton's thought as has been here indicated, did not Newton correct him? A sufficient answer to this question can be found in Newton's profound sense of isolation. From all indications, he neither expected nor particularly wished to be understood by his contemporaries. Thus, lacking any instruction in Newton's thought beyond what was contained in the Principia, the Newtonians had no choice but to fall back on their own understanding and on what they could learn from each other. Under the circumstances a further degeneration in the understanding of the Newtonian system was all too possible.

J. T. Desagulier (1683-1744), one of Keill's pupils during the last series of lectures on experimental philosophy delivered by Keill at Hart Hall, Oxford, represents a further loss or confusion of Newton's thought. However, this did not prevent Desagulier from attaining to a certain degree of success as a representative of the new science. In 1710 Desagulier took over Keill's old lectureship at Hart Hall. In 1713 he left Oxford for London where he became famous for his public lectures in Newtonian science, and at about the same time he was made "curator" of experiments to the Royal Society.[15] It should be noted that Newton

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[14]H. Butterfield, The Origins of Modern Science, 1300-1800 (New York: Macmillan, 1960), p. 184.

[15]I. Bernard Cohen, Franklin and Newton. An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work in

was the president of the Royal Society at this time and must have at least given approval to the employment of Desagulier in this capacity.

Now, as indicated above, the measure of comprehension of Newton's mechanics lies in the degree to which a writer on the subject was able to give a unified treatment of statics and dynamics in terms of Newton's Laws. In Desagulier's main work, A System of Experimental Philosophy, there is little question of such a unification since he did not even see the direct applicability of Newton's Third Law to a general treatment of machines. Instead of making use of the laws in dealing with machines, Desagulier inserted a rather lengthy discussion of them as "laws of nature" following his theory of machines.[16] Also, while his statement of the laws adheres closely to Newton's own wording, his conception of quantity of motion, or momentum, is the same as Keill's, that is, the product of weight and velocity.[17]

As the basis for the treatment of machines, Desagulier listed ten definitions, three "suppositions" and four axioms.[18] Of all of these statements, only one definition, that of equilibrium, has any manifest connection with Newtonian theory. "Equilibrium," wrote Desagulier, "is, when there is the same quantity of motion in the power, as there is in

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Electricity as an Example Thereof (Philadelphia, Pa.: The American Philosophical Society, 1956), pp. 243-245.

[16] J. T. Desagulier, A System of Experimental Philosophy, Proved by Mechanicks. Wherein the Principles and Laws of Physicks, Mechanicks, Hydrostaticks, and Opticks Are Demonstrated (London: B. Creake and J. Sackfield, 1719), pp. 47-62.

[17] Ibid., pp. 12-13.

[18] Ibid., pp. 21-23.

the weight; because their motions being contrary, the one destroys the other."[19] This equilibrium idea is the same as Keill's, but, whereas Keill saw that it was directly applicable to any one of the machines, Desagulier applied it only to the lever and then reduced all the other machines to the lever. Such a treatment of machines is purely static; its emphasis lying solely on geometrical and structural properties of the machines rather than on the motions of their parts.

Desagulier was manifestly not of a mathematical turn of mind. He seems to have been more in the tradition of the craftsman-technician than that of the philosopher-mathematician, so that it is perhaps not so astonishing that he failed to grasp the mathematical subtleties of the new mechanics. Nonetheless, as Cohen has stated, he was regarded as an "ambassador of Newtonian thought,"[20] and thus his understanding of Newton must represent an at least respectable standard for his time. Thus one is confronted with a further degeneration of Newtonian thought while Newton himself was still very much on the scene.

Desagulier represents a low point in the understanding of Newton. Actually, as will be shown later, both Keill and Desagulier drew a great deal of their thought on mechanics from French work in the development of mechanics stemming from Descartes. The principal authors by whom they were influenced seem to have been Philippe de La Hire (1640-1718) and Jacques Rohault (1620-1675). Rohault in particular was very popular in England in the late seventeenth and early eighteenth centuries.

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[19] Ibid., p. 21.

[20] Cohen, p. 245.

His principal writings on mechanics were published there in both Latin and English translation.[21]

Both Rohault and La Hire were basically Cartesian in their approach to physics (which means that they started from the basic assumption that all phenomena result from either impacts or pressures and that matter has only geometrical or spatial characteristics), so that "Newtonian" thought on mechanics up into the 1720's is seen to be strongly related to Cartesianism. The difference between "Newtonian" and "Cartesian" work in this period is largely superficial, a mere use of Newton's terms without any deep understanding of what they meant in Newton's theory.

Even Newton himself felt a need to counter the influence of Descartes in 1713, the time of the publication of the second edition of the Principia. The preface to the second edition, written by Roger Cotes (1682-1716), had as its primary object the combating of Descartes theory of vortices. "The need of such discussion, twenty-six years after the

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[21]In 1697, Samuel Clarke, a Newtonian, published a translation of Rohault's Traite de Physique. The translation was accompanied by notes that explained the Newtonian view of the material covered, so that they constituted a virtual refutation of the text. However, the 1698 edition contained the notes as annotations at the end of the text rather than as footnotes, and they do not refute the idea of vortices. The third edition of 1710 contained much enlarged notes, appearing as footnotes at the bottom of the pages to which they were applicable. An English translation of Clarke's work appeared as late as 1723. Florian Cajori, "An Historical and Explanatory Appendix," Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World. Trans. Andrew Motte, 1729. Translation revision and historical appendix by Florian Cajori (Berkeley, California: University of California Press, 1947), pp. 630-631.

first appearance of Newton's Principia indicates the great popular attachment to the views of Descartes."[22]

By that time, however, the influence of Leibniz had made itself felt in French thought on mechanics, and Leibniz had expressed contempt for the "occult quality" of gravitation.[23] Thus a new challenge to Newtonian mechanical thought had appeared, significantly from the same quarter as the challenge to Newton's priority in the matter of the invention of the calculus. The ensuing controversies were eventually to merge into one: the question as to the nature of the "force" of a body in motion. From what has been said thus far, the confluence of the two arguments in the force concept was a matter of almost logical necessity.[24]

Leibniz' attack on the Newtonian system evoked a reaction from Newton through Cotes' preface to the second edition of the Principia and in the General Scholium added at that time. In his preface Cotes wrote:

> But shall gravity be therefore called an occult cause, and thrown out of Philosophy, because the cause of gravity is occult and not yet discovered? Those who affirm this, should be careful not to fall into an absurdity that may overturn the foundations of all philosophy. For causes usually proceed in a continued chain from those that are more compounded to those that are more simple; when we are arrived at the most simple cause we can go no farther . . . .
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> Some there are who say that gravity is preternatural, and call it a perpetual miracle. . . . It is hardly worth while to spend time in answering this ridiculous objection which overturns

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[22]Ibid., p. 629.

[23]A letter from Leibniz to Nicholas Hartsoeker (1656-1725) stating that Newton's mechanics was built upon miracles was published in a weekly paper, Memoires of Literature in May, 1712. Leibniz, prior to this, had attacked Newton in his Théodicée. Alexander Koyré, From the Closed World to the Infinite Universe (New York, Evanston, and London: Harper and Row, [1958]), p. 299.

[24]For an amplification of this point see below, pp. 226-229.

> all philosophy. For either they will deny gravity to be in bodies, which cannot be said, or else, they will therfore call it preternatural because it is not produced by the other properties of bodies, and therefore not by mechanical causes. But certainly there are primary properties of bodies; and these . . . have no dependence on the others.[25]

Here again there is an attribution of gravitational attraction to matter as one of its fundamental properties; the same misconception already noted in the work of John Keill. This seems to contradict the other addition to the second edition of the Principia, the General Scholium, which was intended to combat another of Leibniz' objections: that the doctrines of the Principia would tend to weaken religion and spread materialism.[26] It has already been pointed out that making force a property of matter led to Deism, a point of view quite foreign to Newton. In the General Scholium in direct contradiction to this idea Newton laid out his thoughts on the near identity of space, time, and deity that were so important to the logical structure of the mechanics.

Thus, even in the attempt to refute their great antagonist, Newton and Cotes exhibited a very significant ideological difference. The important thing at this point, however, is that Newton had been at least momentarily aroused to the defense of his own theory of mechanics, and possibly to the need to give some attention to what his defenders were saying in his behalf. While Desagulier's book (1719) certainly appears to have been written without any help from the master, a similar

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[25] Roger Cotes, "Preface to the Second Edition," Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World. Trans. Andrew Motte, 1729. Translation revision and historical appendix by Florian Cajori (Berkeley, California: University of California Press, 1947), p. XXVII.

[26] Koyré, p. 235.

work, A View of Sir Isaac Newton's Philosophy, written some years later by Henry Pemberton (1694-1771) seems to have received a certain amount of attention from Newton.

Pemberton helped Newton in the preparation of the third edition of the Principia and also performed an English translation of it which he intended to publish along with a "comment" on the Principia.[27] In the course of this activity Pemberton had ample opportunity to familiarize himself with Newton's thought, and he stated, in the preface of his View of Sir Isaac Newton's Philosophy, that Newton approved of the book, which he and Pemberton had read together in great part.[28]

Nevertheless, although, as will be seen, the work is definitely superior to that of Keill and Desagulier in the crucial matter of the conceptions of force and quantity of matter, it followed those writers in their failure to understand Newton's indications as to the proper basis for the treatment of machines.

Both Keill and Desagulier had obscured the concept of momentum and its relationship to weight by confusing weight and quantity of matter. This confusion, as pointed out earlier, arose partially through the attribution of gravitational attraction to matter as a fundamental property and partially through the inability to grasp the mathematical structure of Newton's mechanics. Pemberton, however, exhibited in his book a more proper Newtonian understanding of quantity of matter. In his explication of the concept of the "power of inactivity," *vis inertiae*, he stated that

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[27] Henry Pemberton, A View of Sir Isaac Newton's Philosophy (London: S. Palmer, 1728), pp.[2r-a2v].

[28] *Ibid*., p. [a2r].

it is that

> quality in bodies whereby they preserve their present state, with regard to motion or rest, till some active force disturb them. . . . By this property, matter, sluggish and inactive of itself, retains all the power impressed upon it, and cannot be made to cease from action but by the opposition of as great a power, as that which first moved it.[29]

The emphasis in the above definition, which itself is equivalent to the First Law of Motion, is on the inactive character of matter, rather than on any active force of matter. Furthermore, it was on the basis of the "power of inactivity" that the quantity of solid matter in a body was to be judged, and not directly by means of the weight of the body; the degree of the power of inactivity was assumed to be proportional to the quantity of solid matter. The proportionality between quantity of matter and weight, however, was not a matter of definition, but a consequence of the Second Law of Motion. Pemberton expressed this idea by saying that the power of inactivity of a body and therefore the quantity of matter was proportional to its weight, in support of which he cited Proposition XXIV of Book II of the *Principia*.[30]

In Proposition XXIV, Book II, Newton proved, on the basis of the Second Law, that the quantities of matter in pendulous bodies are as the weights of the bodies and the square of their periods of oscillation. Assuming that the total periods of oscillation of the pendulums are in the same ratio as the times during which they traverse any corresponding portions of their respective arcs, and, by the Second Law, that the velocities generated during these times by the motive forces, or weights,

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[29] *Ibid*., p. 42.

[30] *Ibid*., pp. 42-43.

are inversely proportional to the quantities of matter in the bodies, the above result follows easily.[31]

By the reference to the above proposition, Pemberton's work gives positive evidence of an understanding of the central concepts of mass and force that was lacking in his predecessors. At the same time, however, his explication of the Second Law itself is not very clear. The sense of the law, according to Pemberton, is that

> if any body were put into motion with that degree of swiftness, as to pass in one hour the length of a thousand yards, the power which would give the same degree of velocity to a body twice as great, would give the lesser body twice the velocity . . . .[32]

Here there is no explicit indication that the Second Law has any meaning in terms of motive force acting in time, unless the term "power" had that meaning for Pemberton in this context. While there is no definition of "power" in such terms in A View of Sir Isaac Newton's Philosophy, Pemberton had earlier (1722) in a piece in the Philosophical Transactions, given an indication that he understood the term power, or force, as an action in time producing change of motion.[33]

The article is concerned with experiments made by Giovanni Poleni (1683-1761) in which globes of equal size but different weights were allowed to fall on a yielding substance like soft wax.[34] The

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[31]Newton, Principia, p. 303.

[32]Pemberton, p. 36.

[33]Henry Pemberton, "A Letter to Dr. Mead, Coll. Med. Lond. & Soc. Reg. S. Concerning an Experiment Whereby It Has Been Attempted to Show the Falsity of the Common Opinion, in Relation to the Force of Bodies in Motion," Philosophical Transactions, XXXII (1722-1723), pp. 57-68.

[34]This experiment was described in Poleni's de Castellis per quae derivantur fluviorum aquae habentibus latera convergentia liber quo

The heights from which the globes were dropped were inversely proportional to their weights, so that the squares of the velocities of the globes at impact were also inversely as the weights. Under these conditions, the globes were found to make equal impressions in the wax, a result that led Poleni to infer that the forces of bodies in motion are proportional to the products of their weights and the squares of their velocities.[35]

Pemberton reacted to this interpretation of the experiment, which supported the Leibnizian view of the nature of force, by saying that the experiment should be interpreted in such a manner as to shed light on the _manner_ of penetration.[36] That is, he denied Poleni's assumption that the depth, or size, of the penetration is simply proportional to the force of the striking body.

In order to reinterpret the experiment in terms of the way in which a yielding substance resists penetration, Pemberton reasoned that if two bodies, A and B, are dropped, as before, from heights inversely as their weights, then the ratio of their momenta at impact will be

$$\frac{M_A}{M_B} = \frac{W_A V_A}{W_B V_B} = \frac{V_B^2 V_A}{V_A^2 V_B} = \frac{V_B}{V_A},$$

where M, W and V represent momentum, weight and velocity respectively. Next, Pemberton assumed that the resistance of a soft substance to penetration must be inversely proportional to the velocity of the penetrating

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etiam continentur nova experimenta ad aquas fluentes atque ad percussionis vires pertinentia (Patavii: J. Comini, 1718).

[35]Pemberton, _Philosophical Transactions_, XXXII, p. 57.

[36]_Ibid._, pp. 57-59.

body, or directly proportional to the time required to penetrate through some given distance. The effect of the resistance to penetration, according to Pemberton, was a "momentaneous" loss of force, or momentum, which, being proportional to its cause, must be inversely proportional to the *velocity*. However, the momenta of the striking bodies were inversely as their velocities at the moment of impact, so that the "momentaneous" losses of momentum in the instant of impact are as the momenta with which the globes strike the wax. From this, Pemberton went on to conclude that the bodies must always make equal penetrations if, at the moment of impact, their momenta are inversely as their velocities.[37]

The above interpretation, in spite of certain short-comings, does indicate that Pemberton understood force, or power, to cause a "momentaneous" change in force, or momentum (unfortunately Pemberton was somewhat loose in his terminology, so that the sense of a term like force often has to be determined from the context). That is, the resistance of the soft material represented a force whose action during a very short time interval was to cause a change in momentum, which is the true sense of the Second Law of Motion.

Beyond the sheer conceptual grasp of the Second Law, however, Pemberton's treatment of the problem presented by Poleni's experiment has little to recommend it. The notion that the resistance of the wax to penetration must be inversely as the velocity of penetration was obviously contrived to yield the desired result, since all experience with the motion of bodies in resistant media indicated that the resistance of the

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[37] Ibid., pp. 59-60.

media was directly, rather than inversely, proportional to the velocity. Thus Pemberton allowed himself to be led by his desire to refute an antagonist into some very bad physics. But if the physics was bad, the attempt at mathematical interpretation was worse.

The manner--an attempt to use infinitesimal quantities--in which the conclusion was drawn from the contrived relationship between "momentaneous" change in momenta in the soft medium and the momenta of the bodies at impact is very unclear, and the conclusion itself is not a refutation of Poleni's interpretation. This lack of clarity shows that, although Pemberton was aware that Newtonian mechanics was meant to deal with infinitesimal time intervals, the mathematical treatment was beyond his power.

The failure to really grasp the mathematical structure of the mechanics was basic to Pemberton's inability to apply the theory to anything but the simplest situations. This is apparent in the discussion of machines contained in A View of Sir Isaac Newton's Philosophy. The approach to a theory of machines was made through a notion of virtual velocities which Pemberton wrongly ascribed to Archimedes. His statement of the principle involved is:

> . . . when two weights are applied to any of these instruments, the weights will equiponderate, if when put into motion, their velocities will be reciprocally proportional to their respective weights.[38]

This principle served to explain the action of each of the machines in sustaining a given load with a given force.

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[38] Pemberton, A View of Sir Isaac Newton's Philosophy, p. 69.

However, as stated and applied, Pemberton's equilibrium principle is independent of the rest of the theory; that is, it bears no necessary relationship to the laws of motion. His statement of the Third Law, the basis for Newton's synthesis of statics and dynamics, is too specialized to have any possibility of direct application to the problems of statics. It deals exclusively with inelastic collision and is a direct consequence of the definition of the power of inactivity of matter. As seen by Pemberton, the content of the Third Law was that a body, no matter how small, striking a second body, no matter how large, will impart some motion to the second body and itself be deprived of just that much motion.[39] In this form, the law clearly has no direct application even to the equilibrium of simple machines, not to mention the analysis of dynamic states of machines. Therefore the union of static and dynamic theory achieved in the *Principia* is not present in Pemberton's book.

Thus, although there was, relative to the Newtonians discussed earlier, a certain conceptual improvement manifested in Pemberton's book, he was still liable to much of their misconception and unclarity. In fact, it seems as though the parts of *A View of Sir Isaac Newton's Philosophy* in which Newton could have manifested the interest indicated by Pemberton must have been confined to the concepts of mass and force, and to their relationship. One reason for this might have been a sensitivity on Newton's part to the religious implications that have been shown to be contained in the idea that force is a property of matter. Beyond that, his normal aloofness seems to have reasserted itself.

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[39] *Ibid.*, p. 46.

Pemberton himself provided an insight into Newton's position in regard to the spread of his own ideas when he wrote that "though his memory was much decayed, I found he perfectly understood his own writings, contrary to what I frequently heard in discourse from many persons."[40] If nothing else, this remark indicates that at the time--in the last few years before Newton's death--there was a certain amount of disagreement between Newton and others over the content of the Principia. In view of what has been said thus far, Pemberton's claim that Newton's understanding was still accurate seems plausible. Any disagreement over the meaning of Newton's mechanical writings must therefore have arisen from such misinterpretations as have been here brought to light and which were at that time, still deeply rooted. Even Pemberton, who had the benefit of professional association with Newton, was not able to lay out a general theory of mechanics giving full play to the conceptual structure forged by the great man.

Perhaps, however, it is unfair to judge Pemberton's book on this basis, since it seems to have been intended as something like a textbook rather than as an advanced or creative scientific work. It was designed, as were the books of Keill and Desagulier, to present an explication of Newtonian thought in language understandable to ordinary literate people and to students, and not to deal with questions of a difficult and abstract nature. Still, it is significant that Pemberton's treatment of mechanical problems is in large measure not Newtonian.

Considering the intended audiences of the works of Keill, Desagulier, and Pemberton, there is another factor that must have had

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[40] Ibid., p. a2r.

considerable impact on the way in which they presented the Newtonian system; the dissemination of the necessary mathematical techniques, that is, the calculus, was quite late and uneven in occurrence. The ideas basic to the calculus caused a great deal of controversy of a purely mathematical nature among academicians, as a consequence of which, the teaching of the calculus in the schools was held up even after the publication of a number of treatises on the method. Under such conditions, books on mechanics intended for a student and lay audience could not make use of the ideas of the calculus even if it lay within the power of the author to do so.

It is beyond the scope of this work to delve deeply into the strictly mathematical aspects of the development of the calculus and of the problems that arose concerning it.[41] Of significance here is the fact that such problems existed and that their effect was to produce a certain amount of confusion. Part of this confusion derived from Newton

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[41] A considerable amount of scholarship has been concentrated in this area. The following works, however, provide a good general coverage of the problems and issues surrounding the development of the calculus. Carl Boyer, The History of the Calculus and its Conceptual Development (New York: Dover Publications, Inc., 1949). Florian Cajori, A History of Mathematics (2nd ed. rev.; New York: The Macmillan Company, 1931). Florian Cajori, A History of Mathematical Notation, Vol. II (Chicago: Open Court Publishing Co., 1929). Florian Cajori, "Discussions of Fluxions: From Berkeley to Woodhouse," The American Mathematical Monthly, 24 (April, 1917), pp. 145-154. Eric Temple Bell, The Development of Mathematics (New York: McGraw-Hill, 1945). Ettore Çarruccio, Mathematics and Logic in History and in Contemporary Thought, trans. Isabel Quigly (Chicago, Ill.: Aldine Publishing Co., 1964). J. F. Scott, A History of Mathematics (London: Taylor and Francis Ltd., 1960). E. W. Strong, "Newton's Mathematical Way," The Journal of the History of Ideas, XII (Jan. 1951), pp. 90-110. E. W. Strong, "Newtonian Explications of Natural Philosophy," The Journal of the History of Ideas, XVIII (Jan., 1957), pp. 49-83.

himself; from the fact that at different times he held different views of a very basic idea of the calculus, that is, of the nature of infinitesimals. It should be noted that this question is essentially independent of the logical structure of the calculus, which was common to all its various formulations.

The first published account of the calculus was contained in the first edition of the Principia (1687). As already mentioned, the account of the calculus contained in the Principia was quite brief; in his attempt to give classical geometrical form to his work, Newton presented only the shortest possible justification of his mathematical methods. The first real presentation of the method came in 1693 when a portion of Newton's De Quadratura curvarum was printed in Wallis' Algebra.[42] In the De Quadratura, originally written in 1676, Newton attempted to avoid the use of infinitesimals, or "moments," that is, of indivisible but infinitely small quantities. In their stead, the method of "prime and ultimate ratios" was employed, the same method that was used in the exposition of the calculus contained in the Principia.[43] In modern terms, the method of prime and ultimate ratios represents a definition of the infinitesimal in terms of limits. In Newton's terms, it made use of a limiting ratio of so-called "nascent" or "evanescent" quantities, rather than indefinitely small but "atomic" entities. The complete De Quadratura was published in 1704 as an appendix to Newton's Optics.[44]

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[42] Boyer, p. 201.

[43] Scott, p. 51.

[44] Boyer, p. 201.

The next work on the calculus to be published was Humphrey Ditton's An Institution of Fluxions, which appeared in 1706.[45] The essence of the method of fluxions as stated by Ditton was that

> . . . the genuine sense and meaning of finding the fluxions of any flowing quantity, is as much as finding the nature and relation of the velocities of those motions, by which the said flowing quantities are generated and described.[46]

In the actual use of the method, however, Ditton treated the fluxions themselves as though they were very tiny increments, instead of velocities of increase, of a flowing quantity. This is apparent from the fact that the fluxions were not multiplied by the symbol "o," representing an instant of time.[47] The same omission was sometimes made in the 1704 publication of the De Quadratura, and it led a great many British mathematicians to regard fluxions as entities of infinitesimal magnitude.[48]

Newton's De Analysi per aequationes numero terminorum infinitas, appearing in 1711, was the next work on the calculus to be published in England. The use of infinitesimals in this treatise appeared as a direct contradiction to the definition of infinitesimals in terms of limits--prime and ultimate ratios--contained in the De Quadratura and the

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[45]Humphrey Ditton, An Institution of Fluxions: Containing the First Principles, the Operations, with Some of the Uses and Applications of that Admirable Method; According to the Scheme Prefix'd to his Tract of Quadratures by (its First Inventor) the Incomparable Sir Isaac Newton (London: James Knapton, 1706).

[46]Ibid., p. 15.

[47]As an example of this practice see Ditton's fluxional derivation of Galileo's laws of motion. Ibid., pp. 188-190.

[48]Boyer, pp. 201-202.

Principia. The method of the De Analysi which is completely dependent on the use of indivisible spatial and temporal increments, was devised, prior to that of the De Quadratura, in 1669 and is similar to the work of Isaac Barrow (1630-1677), Newton's teacher.[49]

In 1715 there appeared a work entitled Methodus Incrementorium directa et inversa by Brook Taylor (1685-1731) which was an exposition of Newton's calculus based on prime and ultimate ratios. The major contribution of the work to the development of the calculus was to attempt to derive the ratio of the fluxions from the finite differences of moments, or momentary increments, rather than from Newton's somewhat vague "nascent and evanescent" quantities. In order to make the transition from the finite differences of moments to the infinitesimal ratio of fluxions, Taylor was forced to conceive the ultimate ratio of Newton's method as a ratio "in which the quantities are already evanescent and are made zero."[50] This made the calculus neither clearer nor more rigorous.[51] No doubt Taylor's innovation merely added to the already existing confusion over the nature and reality of the infinitesimal.

The controversy over infinitesimals continued on into the 1730's, and in 1736, nine years after Newton's death, and in the midst of the confusion there appeared an exposition of the earliest and the most productive form of Newton's calculus, the Method of Fluxions.[52] The

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[49] Ibid., p. 191.

[50] Boyer, p. 234.

[51] Bell, p. 285.

[52] Isaac Newton, The Method of Fluxions and Infinite Series with its application to the Geometry of Curve-Lines, trans. with commentary by John Colson (London: Henry Woodfall, 1736). See above, p. 39.

method expounded in this work is in essence the same as that described in "To Resolve Problems by Motion," which was cited previously as the earliest (1666) of Newton's complete expositions of the calculus. The basic similarity of the methods can best be realized in the context of a specific problem.

The first problem of the calculus is, given the relationship between fluent quantities, to find the relation between their velocities of increase, or fluxions. The 1666 solution to the problem is given on pages 42-43 of the present work and can be seen to be, except for notation, the same as the solution that appeared in The Method of Fluxions in 1736. There Newton stated that the moment of a flowing quantity, x, can be represented as the product of its celerity, $\dot{x}$, and an indefinitely small quantity, o, that is, $\dot{x}o$. The same can be done for any other fluent quantity, y. Then x and y, after an infinitesimal time interval become $x + \dot{x}o$ and $y + \dot{y}o$. If these quantities are then inserted in the original relation between x and y and all terms containing o are eliminated, the result will be the desired relationship between $\dot{x}$ and $\dot{y}$.[53]

Thus, the method of fluxions depends on the use of an infinitesimal entity which is discarded in the final step. Newton was aware that such a procedure was not entirely rigorous, but it was effective; it was capable of providing the logical relationships necessary to the construction of a comprehensive mechanical theory and to the solution of problems not amenable to the methods of classical geometry. Over the years between the invention of the method of fluxions and the publication of the Principia Newton had attempted to improve the logical foundations of the

[53] Ibid., pp. 24-25.

calculus, and hence of the mechanics, and these various attempts appeared in print in the inverse chronological order of their conception. To make things worse, Newton presented the various forms of the calculus described above as being essentially equivalent. That almost god-like figure, in true god-like style, never admitted that he had undergone any change of mind with regard to infinitesimals.[54] It is no wonder that in the England of the early 18th century there was uncertainty among mathematicians and ignorance among non-mathematicians concerning the calculus.

The English universities reflect this state of affairs during the period. A Dr. W. Heberden of St. John's College, Cambridge, made the following comments on the examinations he recalled about 1730.

> Locke, Clarke, and the four branches of natural philosophy were studied; while Newton, Euclid, and algebra were only known to those who chose to attend the lectures of Prof. Saunderson, for the college lecturers were silent on them.[55]

At that time, the Clarke translation of Rohault was still the Cambridge textbook in mechanics.[56] Although, through its footnotes it contained an exposition of Newtonian philosophy, it was not a preeiminently mathematical work. As indicated in the above quotation, the mathematical aspects of Newtonian thought were taught <u>as mathematics</u>, and not

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[54] Boyer, p. 222. In the sense that infinitesimals in any of the methods, always entered into the same sort of relationships, Newton was justified in asserting that he had never changed his mind concerning them. All forms of the calculus performed the same sorts of operations in essentially the same fashion.

[55] Christopher Wordsworth, <u>Scholae Academicae. Some Account of the Studies at the English Universisites in the 18th Century</u> (Cambridge: At the University Press, 1910), pp. 68-69.

[56] Cajori, <u>Principia</u>, p. 631.

as an integral part of physics. Further, such teaching probably reached a relatively small number of students, even if after 1710, when Nicolas Saunderson (1682-1739) succeeded William Whiston (1667-1752) as Lucasian Professor of Mathematics, there was a rise in the price of copies of the Principia from ten shillings to two guineas.[57]

In general, the situation at the great English universities seems not to have been particularly favorable to abstract mathematical studies during the first half of the eighteenth century. The main subjects of instruction at both Oxford and Cambridge were the classics and theology, which were taught and examined with hardly any change from the medieval system. "At neither university was either the obsolete curriculum or the dons, mostly die-hard, port-drinking tories," likely to stimulate intelligent interest in any subject of an advanced or controversial nature.[58]

Such a situation with regard to the calculus explains to some extent the character of popular works on Newtonian thought such as those of Keill, Desagulier, and Pemberton. The mere fact that there was a good deal of confusion over the justification of the calculus, that is, over the nature and existence of the infinitesimal, which kept mathematicians and metaphysicians wrangling and possibly contributed to the failure of the universities to add the calculus to their curricula, should not, however, have prevented physicists from making use of a mathematical tool that, in any of its forms, obviously worked. One can only conclude that,

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[57] Wordsworth, p. 69.

[58] Basil Williams, The Whig Supremacy, 1714-1760 (Oxford: At the Clarendon Press, 1939), p. 135.

since a fair amount of material on the method of the calculus was published after 1693, and since the Newtonian writers examined thus far had committed serious conceptual errors that are typical of a finite, geometrical approach to mechanics as opposed to one making use of the calculus, the Newtonian mechanicians did not really grasp the dependence of mechanical theory on mathematical structure. Another way of saying the same thing is that there existed a separation, in the minds of most English thinkers of the day, between physics, as an empirical discipline, and mathematics; between what they believed to be the inductive method of science and the deductive method of mathematics. The English, at this time, had not yet overcome the Baconian influence toward simple empiricism, as a glance at the table of contents of any issue of the Philosophical Transactions in the period will confirm.

Thus, even though there was little chance of any effective use of the calculus in popular works on the mechanics due to the problems involved in its dissemination, it seems that the absence of the calculus in these works stems basically from the ignorance of the authors. It has been pointed out that Newton himself was in a sense responsible for this state of affairs. Of the three Newtonian writers mentioned, only Pemberton had grasped the mass-weight relationship and, to some extent, the notion that the calculus was applicable to mechanical phenomena, but he had the benefit of personal instruction by Newton.

The idea that Pemberton's insights were indeed somewhat unique for the period and the direct result of Newton's personal influence, can be supported by the consideration of a work by Andrew Motte (d. 1730) entitled A Treatise of the Mechanical Powers.[59] Motte, like Pemberton, was a translator of the Principia and so had an exposure to that work of the same nature as Pemberton's, except that Motte apparently did not work with Newton in the translation. Motte's own book on mechanics, first published in 1727 and in a second edition in 1733, unlike Pemberton's maintains the same conceptions of momentum, force, and weight as earlier writers, and like them exhibits no insight into the relation of the calculus to the mechanics.

Motte's work, as can be seen from the full title, was intended as lecture material for hearers of some ability in mathematics, that is, in geometry. Motte also hoped to reach "those gentlemen who have gone through courses of experiments . . ." and those just entering the study of natural philosophy, "as they will find the Laws of Motion, which are the foundations of that science, more largely explained than is commonly done . . . .[60]

The claim that the Laws of Motion were to be more fully explained than usual, and in the context of a "geometry lecture," would lead one to imagine that Motte intended a truly mathematical presentation of the mechanics. Motte, however, was in fact affected by the common notion of the separation of mathematical and physical principles even though in his definition of the subject of mechanics he seemed to

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[59]Andrew Motte, A Treatise of the Mechanical Powers, Wherein the Laws of Motion, and the Properties of Those Powers are Explained and Demonstrated in an Easy and Familiar Method. Being the Substance of Certain Discourses Delivered at the Geometry Lecture, at Gresham College (2nd ed.; London: Benjamin Motte, 1733).

[60]Ibid., pp. [A3r-A3v].

indicate otherwise. Mechanics, for him, dealt with the various ways in which force may be applied to bodies and with the different effects thus produced, with regard to velocity, direction, and quantity of motion. Further,

> . . . this science, being part of what is called the mixed mathematics, requires some axioms or general principles, found out by certain and indubitable experiment, joined with strict geometrical reasoning, whereon to found its conclusions.[61]

Here Motte seems to have grasped the notion that the axioms of a theory must be related to a mathematical deductive system, although, as will appear shortly, theory itself did not, for him, have the character of being the uniquely acceptable explanation of the phenomena within its scope. In any case, Motte was unaware that different mathematical deductive systems characterize various scientific theories; that is, he did not realize that Newtonian mechanics required a mathematical logic other than the classical geometry of finite quantities. This becomes apparent not only in his explication of the Laws of Motion, but also in his application of them to the analysis of machines.

In the Treatise of the Mechanical Powers the First Law of Motion was stated in straight Newtonian terms; matter continues at rest or in motion until some external cause alters its state. Motte presented this as a generalization of common experience, just as had Newton, but went on to discuss the impossibility of bodies moving or accelerating themselves on the basis of the principle of insufficient reason.[62] Such a discussion is superfluous, since the First Law is nothing more than a

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[61] Ibid., pp. 2-3.

[62] Ibid., pp. 3-5.

specialization of that principle in the first place, and the fact that Motte treated the law in this fashion shows up the way in which the empirical and logical elements of theory were related in his mind. The empirical elements were assumed to have significance and validity independently of the logical ones, which merely tended to support them.

The agent or cause producing changes of motion was force, the real Newtonian meaning of which Motte almost grasped in his statement of the measures of force.

> Now in order to limit and determine the quantity of these forces . . . they are expressed either by the velocity they produce in a body impelled by them, the weight they are able to sustain or move, or by a compound of both together.
>
> . . . .
>
> Force is also valued by the rectangle or product of these two, the velocity and the weight of a moving body . . . multiplied into each other, which is called the momentum of that body.[63]

Here Motte has seen that force, in the Newtonian system, has two determinations, weight and quantity of motion produced, but has not seen the causal connection between them. Instead, he has lumped them together as momentum in what seems to be an illogical fashion, and in so doing, has missed the meaning of mass and of the Second Law of Motion.

Motte's statement of the Second Law is that an increase or decrease of motion in a body is proportional to the force acting on it and in the direction of that force. The external force acting on the body must overcome or combine with an "active" force, the momentum, existing in the body, so that both of these forces must have the same nature.[64] Assuming that weight, or centripetal force, is the external force

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[63] Ibid., pp. 8-9.

[64] Ibid., p. 13.

producing change of momentum, or of "active" force, then weight must appear on both sides of the proportionality represented by the Second Law. In that case, the law is reduced to being nothing more than an identity. As has been pointed out, the only way in which the Second Law can be given any content is by means of the calculus and the independent conceptions of force and matter.

Motte did not see the meaning of Newton's distinction between impressed and motive force, which has been shown to be only understandable and meaningful in terms of the calculus, and to be based on the idea that motive force is a characteristic of space and not of bodies themselves. The simple geometrical approach that Motte attempted, along with the idea that weight is a fundamental property of matter, had led him into real confusion with regard to the Second Law.

In spite of the lack of clarity in his exposition of the Laws of Motion, Motte did attempt to base his treatment of machines on them. In the preface to his Treatise of the Mechanical Powers, he wrote of the work to follow that "it begins with the Laws of Motion. Not that this previous step is absolutely necessary, it being easy to have shown the properties of the mechanical powers without it."[65] The Laws of Motion were, to Motte, merely "convenient" to the treatment of mechanical powers. Thus, although he saw that the action of machines could be explained on the basis of the Laws of Motion, he did not feel that they provided the only valid explanation. Again the theoretical understanding was merely support for the empirically given.

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[65] Ibid., pp. [A2r-A2v].

As could be expected, the connection established by Motte between the Laws of Motion and the functioning of machines was tenuous at best. From the Second Law, he deduced that if a force contrary but equal to that of a moving body were to act on that body, then the body would lose its motion entirely.[66] This conclusion is equivalent to the Third Law of Motion, which is therefore no longer an axiom of the theory, but merely a corollary to the Second Law.

Now, since the force of a moving body was compounded of its velocity and its weight, two bodies, whose velocities were inversely as their weights, would communicate equal forces to one another and would come to a halt. Likewise, if two such bodies should simultaneously strike the ends of a balance, the equilibrium of the balance would not be destroyed.[67] From this point, Motte made the transition to "virtual" motions of the weights in place of real velocities, setting up a proof of the fundamental law of the lever.

> Therefore the weight at A which is as 3 tends to go over the space . . . 1; at the same time the weight at B which is as 1 tends to go over the space . . . 3. Now these two tendencies or efforts were shown to counterpoise each other; and therefore since the two weights tend contrary ways with equal forces, they . . . must remain in equilibrium.[68]

From this "proof" of the law of the lever it can be seen in what manner Motte conceived the identity of weight and momentum. Their sameness depends on the notion of a "virtual" velocity, that is, a velocity that is neither finite nor exactly zero, but is present only as a tendency.

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[66] Ibid., p. 14.

[67] Ibid., pp. 42-43.

[68] Ibid., pp. 56-57.

However, from his own formulation of momentum, as the product of weight and velocity, it follows that it would require an infinitely large static weight to counterbalance the force of any body with a finite motion. Therefore, Motte's treatment of the mechanical powers, in so far as it was supposed to be based on the Laws of Motion, was simply wrong, which probably would not have bothered him since such treatment was not really necessary to the description of the mechanical powers anyway.

A second edition of the book was published after Motte's death in 1733, almost at the end of the period under consideration, which indicates that the work was at least not universally recognized, at that time, as being incorrect and a basic perversion of Newton's theory of mechanics. One thing can be said for the Treatise of Mechanical Powers, however; even though fundamentally in error, it made an attempt to establish the relation between force as weight and force as momentum in terms of something approaching an infinitesimal increment of velocity. The proper relationship between the two aspects of force did appear in another Newtonian work which appeared in 1730; A Demonstration of Some of the Principal Sections of Sir Isaac Newton's Philosophy, by John Clarke (1682-1757).[69]

Clarke's work was, to a large degree nothing more than a paraphrase of parts of the Principia and made no attempt to apply Newton's theory of mechanics to any problems not treated in the Principia itself.

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[69]John Clarke, A Demonstration of Some of the Principal Sections of Sir Isaac Newton's Principles of Natural Philosophy in which his Peculiar Method of Treating that Useful Subject is Explained, and Applied to Some of the Chief Phenomena of the System of the World (London: James and John Knapton, 1730).

However, Clarke did elaborate on Newton's work to some extent and gave due emphasis to the mathematical structure of the key concepts of mechanics, that is, to the relation of the methods of the calculus to the concept of force.[70] For instance, in his discussion of Lemma X of Book I of the Principia, which states that the spaces described by a body acted upon by finite, variable or constant, forces are, at the start of the motion, to each other as the squares of the times, Clarke stated clearly that the action of force was the generation of velocity in time. In each time interval the force adds an increment of velocity.[71]

Force was thus associated with a velocity of increase of a quantity--velocity--and velocity of increase was a fundamental notion of all of the methods of the calculus. In this particular work, Clarke used the method of first and last ratios, as had Newton in the Principia: these ratios were of the velocities with which quantities begin to be generated or with which they decrease in the instant just before disappearing altogether.[72]

Clarke also added some elaboration of the force concept in its relation to momentum in his treatment of gravitational force.

> The force of gravity therefore is quite a different kind from the projectile force, and cannot strictly be compared with it, but only by a compound proportion made up of a finite and an infinite proportion . . . .[73]

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[70] Ibid., pp. 71-76.

[71] Ibid., pp. 75-76.

[72] Ibid., pp. 71-72.

[73] Ibid., pp. 125-126.

This statement represents an addition to what is contained in the Principia and seems designed to combat the erroneous identification of weight and momentum such as has been seen in Motte's work. The identification of weight and momentum was possible only if weight were considered to be a fundamental property of matter in place of mass, and Clarke, in his discussion of force, addressed himself also to the correction of this misconception. The centripetal forces--weight--were not to be considered as attractions;

> . . . physically speaking, they may more truly be called impulses; for it is not probable that there is any natural virtue or power in the common center of forces, but that the revolving bodies are some way impelled towards that center . . . .[74]

Thus, although Clarke was in no sense an original thinker capable of significant extension of Newtonian mechanics, he was at least a fairly faithful copyist who placed an emphasis on those key concepts that had been distorted by other Newtonian popularizers. His work represents the first popular work on Newtonian mechanics by an English author that did not contain some important misconceptions, and to this extent it represents a significant landmark in the assimilation of Newtonian mechanics in England.

The basic misconceptions in the mechanics of the Newtonians--the failure to recognize its dependence on infinitesimals and the imputation of the gravitational force to matter itself--that were corrected in Clarke's work had, at the time of publication of his Demonstration, long been recognized as significant errors and attacked by the philosopher George Berkeley (1685-1753). While Berkeley is by no means to be thought

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[74] Ibid., p. 262.

of as having been primarily interested in the science of mechanics, still, in the development of his philosophy of "immaterialism" the prevalent ideas on the nature of mechanical phenomena were of profound importance. He therefore, of necessity, took a position with regard to the true nature of matter and motion; one that, as will be pointed out, was in some respects closer to Newton's than that of Newton's professed followers.

Little is known of Berkeley's childhood and youth except that his family was comfortably well off and that he received a good education. He was first sent to Kilkenny school and then, in 1700, to Trinity College, Dublin. At the time, Trinity College was more progressive than either Oxford or Cambridge so that Berkeley could receive a thorough grounding in contemporary mathematics and physical science, particularly in Newton's work.[73]

Berkeley was among the most precocious of the great philosophers, and the philosophical position of his mature years was already largely elaborated in the notebooks written while a fellow at Trinity. His major work, The Principles of Human Knowledge, published in Dublin in May of 1710, already contained the essence of a philosophy which was never seriously modified throughout his life.[74] That philosophy did, however, require amplification in various directions in order to deal with specific problems, and, in this sense, Berkeley's De Motu, published in 1721, and his Analyst, published in 1734, are of special significance for this study.

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[73]G. J. Warnock, "Introduction," George Berkeley, The Principles of Human Knowledge and Three Dialogues between Hylas and Philonous, ed. G. J. Warnock (Cleveland, Ohio and New York: Meridian Books, 1963), p. 7.

[74]Ibid., pp. 7-8.

The occasion for the writing of the De Motu was apparently the prize question of the Paris Academy of Sciences for 1721 as to the cause of motion. Berkeley, at that time, was returning home from a continental tour, and during a stay at Lyons he drew up the tract and sent it to the Paris Academy. He published it in 1721 in England.[75]

The De Motu could not be considered a "scientific" work in the usual sense of the word; it is philosophical in character. That is, it is an application of Berkeley's general philosophy to the specific question of the cause of motion. Nonetheless, it does contain Berkeley's criticism of Newtonian mechanical thought and presages his later attack on the calculus of fluxions.

Since the De Motu is an application of a general philosophy to a specific area of thought, it is necessary to briefly outline the main aspects of the general philosophical position. It was one of Berkeley's main purposes to eliminate error and confusion by making a careful inquiry into the basic elements upon which all knowledge is based, especially scientific knowledge. This purpose is clearly revealed by the full title of his major work: A Treatise Concerning the Principles of Human Knowledge, Part I, Wherein the Chief Causes of Error and Difficulty in the Sciences, with the Grounds of Skepticism, Atheism, and Irreligion Are Inquired Into.[76] The cause of all these things, according

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[75]A. A. Luce, "Editor's Introduction," The Works of George Berkeley, Bishop of Cloyne, IV, eds. A. A. Luce and T. E. Jessop (London, Edinburgh, Paris, Melbourne, Toronto, and New York: Thomas Nelson and Sons Ltd., 1951), p. 3.

[76]George Berkeley, Berkeley's Philosophical Writings, ed. David M. Armstrong (New York: Collier Books, 1965), p. 42. All citations to Berkeley's works will be given in Berkeley's own section numbers.

to Berkeley, was generally thought to be "the obscurity of things, or the natural weakness and imperfection of our understandings."[77] However, that this was not really the case, but that the trouble lay with man's misuse of his own faculties and with difficulties of his own production was Berkeley's contention. "We have raised a dust, and then complain we cannot see."[78]

In order to see how Berkeley cleared the dust obscuring mechanics and mathematics, one must turn first to his metaphysics. The main metaphysical doctrine in both of these areas, as has already been emphasized, is that of causality. The understanding of Berkeley's view of causality is, however, dependent on his doctrines as to the nature and existence of sensible objects and the minds which perceive them.

Minds, according to Berkeley, were active substantial beings capable of acting as causes, whereas sense impressions--which correspond to what are normally considered to be sensible objects--were mind-dependent and passive. As such, sense impressions--ideas of sense--could neither be causes nor represent causes. The essential passivity of "things" thus rests on Berkeley's refusal to consider them as actually existing independently of mind.

> That neither our thoughts, nor passions, nor ideas formed by the imagination exist without the mind is what everybody will allow. And to me it seems no less evident that the various sensations or ideas imprinted on the Sense, however blended or combined together

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Citations to the <u>Principles of Human Knowledge</u> and the <u>De Motu</u> are from the Armstrong edition, those to the <u>Analyst</u> from the Luce and Jessop edition.

[77]Berkeley, <u>Principles of Human Knowledge</u>, "Introduction," 2.

[78]<u>Ibid</u>., 3.

> (that is, whatever objects they compose), cannot exist otherwise than in a mind perceiving them.
>
> . . . .
>
> For what are . . . objects but things we perceive by sense? and what do we perceive besides our own ideas or sensations? and is it not plainly repugnant that any one of these, or any combination of them, should exist unperceived.[79]

What we commonly call bodies, or objects, are no more than various combinations of sensible qualities, and hence so are the changes and motions occurring in them which we normally associate with an active cause.

Berkeley did not deny the existence of active causes, but he argued that, because causes are active, they can only be minds or spirits; the sensible ideas that constitute physical bodies are wholly passive. Thus there is in Berkeley's thought a duality that, in a sense, is like the Cartesian duality of mind and matter. Berkeley, however, explained the dualism by designating mind as the cause of sense impressions, or "matter."

> We perceive a continual succession of ideas, some are anew excited, others are changed or totally disappear. There is therefore, some cause of these ideas, whereon they depend, and which produces and changes them. That this cause cannot be any quality or idea or combination of ideas, is clear from the preceding section. It must therefore be a substance; but it has been shown that there is no corporeal or material substance: it remains therefore that the cause of ideas is an incorporeal active substance or spirit.[80]

Each man is an active, thinking substance, a cause. Man can, through an act of the will, cause an idea to rise up in his imagination. But it is different with sense impressions, or sense ideas.

> Whatever power I may have over my own thoughts, I find the ideas actually perceived by sense have not a like dependence on <u>my</u> will. When in broad daylight I open my eyes, it is not in my power to

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[79] Berkeley, <u>Principles of Human Knowledge</u>, 3-4.

[80] <u>Ibid</u>., 26.

> choose whether I shall see or no, or to determine what particular objects shall present themselves to my view: and so likewise as to the hearing and other senses; the ideas imprinted on them are not creatures of _my_ will. There is therefore some other Will or Spirit that produces them.[81]

It follows directly that the "other Will," or God, is the only cause operating in the "world;" that is, God is the cause of the motions and changes that we perceive.

In the assertion that God is the cause of the motions that take place in the sensible world, Berkeley's thought resembles that of Newton. Indeed both thinkers arrived at this conclusion by drawing the logical consequences inherent in the idea that matter is basically passive and inert. The difference in their thought is of course that Newton conceived of matter as independently existent and Berkeley made it dependent on God. Another similarity between the two is that they both knew that only effects are perceived by sense and that causes are inferred by reason. Since all causes were ultimately traceable to God, the causal structure of the natural world, natural law, which is the object of scientific investigation, was basically the set of rules by which God chooses to act. This idea, however, had different implications in the systems of Newton and Berkeley. As has been shown, the Newtonians exhibited a strong tendency to regard the theoretical terms of Newton's mechanics as representing real entities; invisible but material agents which produced observable effects in a unique and deterministic fashion. Thus natural law was thought to represent the absolute truth, right down to the finest detail.

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[81] _Ibid_., 29.

For Berkeley, the character of natural law was significantly different. Laws of nature are extracted from experience and

> . . . are by men applied, as well to the framing artificial things for the use and ornament of life as to the explaining the various phenomena. Which explication consists only in showing the conformity any particular phenomenon hath to the general laws of nature, or, which is the same thing, in discovering the uniformity there is in the production of natural effects. . . .[82]

Implied in this idea of scientific explanation is the notion that all of the theoretical terms are mere rational constructs having no necessary connection with any absolute reality. It was precisely this difference in the conception of scientific explanation that led Berkeley to attack the science of his day as false and demoralizing. Nonetheless, Berkeley held that theoretical treatment of nature does satisfy man's craving for knowledge, that is, for an understanding of the principles describing the uniformities in the workings of nature.[83] This was tantamount to seeing the action of God rather than knowing the actual mechanism by which He works, which is the next thing to dispensing with God altogether.

The Newtonians, by Berkeley's time had already eliminated God from the functioning of nature by simply attributing gravitational force to bodies so that Berkeley, in attacking them could claim alliance with the great Sir Isaac. In the De Motu Berkeley wrote that "Newton everywhere frankly intimates that not only did motion originate from God, but that still the mundane system is moved by the same actus."[84] Thus, as a

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[82] Ibid., 62.

[83] Ibid., 105.

[84] Berkeley, De Motu, 32.

sort of appeal to universally recognized authority, Berkeley placed emphasis on the similarity of his and Newton's ideas in his attempt to destroy the then prevalent notion of the new physics as ultimate truth, while preserving it as a research tool.

> Whereas the new philosophy, presuming on the name of mathematical physics, had subverted common-sense, destroyed the cosmos, called all familiar things in doubt; Berkeley, equally armed with the name of mathematical physics, reversed all this. He read man back into the focal point of nature; he dispelled the phantom 'matter' of the physicists. . . . At the same time, he was fully alive to the potentialities of the new science as a discipline, as a spear-head for the exploration of Nature in detail, and as a fertile source of inspiration for technical advance.[85]

In order to accomplish this aim, Berkeley had to discredit the idea that theoretical terms like force, mass, etc., used in Newtonian mechanics represented physical reality. In particular, it was necessary to show that an active principle like force could never be associated with matter as one of its properties. Force, according to Berkeley, was an occult quality of which the "symptons and measures" were commonly held to be animal effort and corporeal motion.[86] These "measures" of force correspond to those set up by Newton, but whereas Newton had linked them in a cause-effect relationship, Berkeley did not; they were both occult, and "what is itself occult explains nothing."[87]

It then followed that if such terms are used to signify real entities abstracted from sense perceptions they would breed error and confusion. From this source would arise absurdities like the statement

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[85] G. W. R. Ardley, Berkeley's Philosophy of Nature (Bulletin No. 63, Philosophy Series No. 3; University of Auckland, 1962), p. 10.

[86] Berkeley, De Motu, 5.

[87] Ibid., 6.

that "the force of percussion, however small, is infinitely great."[88] According to Berkeley, this statement supposes that gravity is a real quality of bodies, different from all others, and that it is distinct from motion. But a very small percussion produces a greater effect than the greatest gravitational force without motion, from which it follows that the force of percussion exceeds the force of gravity by an infinite ratio, or is infinitely great.[89]

This is an absurdity since no force makes itself known except through action which is inseparable from motion. Thus the so-called "dead force" of gravitation is really nothing at all. It is to the force of percussion as a point is to a line and not as a part to a whole.[90]

In this criticism, Berkeley was ridiculing the notion that weight, or gravitation, or "dead force" bears an infinitesimal relationship to the force of a moving body. If nothing else, his criticism shows that Berkeley had grasped the significance of the infinitesimal relationship between weight and momentum, which, as has been shown, is the basic relationship of Newton's mechanics and the point that most clearly exhibits its dependence on the calculus. In the same argument Berkeley also attacked a basic error of the Newtonians, that the force of gravity is a real property of bodies. Further, he suggested a causal connection between the notion of the reality of the gravitational force in bodies and the idea of the infinitesimal relationship of force to momentum.

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[88] _Ibid._, 9.

[89] _Ibid._, 9-10.

[90] _Ibid._, 11-14.

This connection, however, was not the correct one, for the attribution of gravitational force to bodies tended rather to obscure the infinitesimal character of the weight-momentum relationship.

All his objections to the new physics were summarized by Berkeley in three rules: "(1) to distinguish mathematical hypotheses from the natures of things; (2) to beware of abstractions; (3) to consider motion as being something sensible, or at least imaginable, and to be content with relative measures."[91] The adherence to these rules would leave the theoretical structure of mechanics untouched and "the study of motion will be freed from a thousand minutiae, subtleties, and abstract ideas."[92]

Berkeley had thus, in his attack on Newtonian physics, done a possible service to that science in pointing out the proper understanding of, or at least calling attention to, some of the basic ideas of Newton's theory that had been lost on the Newtonians. A similar service with regard to the calculus of fluxions was performed by Berkeley with the publication of the _Analyst_ in 1734.

The motives behind the production of this work were similar to those back of the _De Motu_, and can, as with the _De Motu_, be read from the subtitle of the work: "A Discourse Addressed to an Infidel Mathematician, Wherein it is examined whether the object, principles, and inferences of the modern Analysis are more distinctly conceived, or more evidently deduced, than religious Mysteries and points of Faith."[93] The actual

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[91] _Ibid._, 66.

[92] _Ibid._

[93] Berkeley, _The Works of George Berkeley, Bishop of Cloyne_, 53.

events leading up to the writing of the Analyst have been summarized by Eric Temple Bell.

> It seems that Newton's friend Halley, posing as a great mathematician, had proved conclusively to some deluded wretch the inconceivability of the dogmas of Christian theology. The converted one, a friend of Berkeley's, refused the latter's spiritual offices on his deathbed. Profoundly shocked by the soul-destroying savagery of the 'modern analysis', and mindful of his education in semi-civilized Rhode Island, the good bishop went after the scalp of fluxions.[94]

The attack on fluxions was a continuation or an extension of the attack on the mechanics both in that Berkeley saw the integral relationship between the two and because his aims and methods were the same in both cases. As to the relationship between the calculus of fluxions and the mechanics, Berkeley wrote that

> . . . the Method of Fluxions is the general key by help whereof the modern mathematicians unlock the secrets of Geometry, and consequently of Nature. And, as it is that which hath enabled them so remarkably to outgo the ancients in discovering theorems and solving problems, the exercise and application thereof is become the main if not the sole employment of all those who in this age pass for profound geometers.[95]

With regard to the method of attack, Berkeley again struck at the logical foundations of the theory in question, asking as to the conceivability and reality of the theoretical terms, in this case the fluxions. After developing the concept of the fluxion in terms of the velocity of generation of a flowing quantity, Berkeley went on to the description of the relation between the fluxion and any finite quantity, a procedure reminiscent of his treatment of force in the De Motu.

> The fluxions are celerities, not proportional to the finite increments, though ever so small; but only to the moments or

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[94]Bell, p. 287.

[95]Berkeley, The Analyst, 3.

> nascent increments. . . . And of the aforesaid fluxions there be other fluxions, which fluxions of fluxions are called second fluxions. And the fluxions of these second fluxions are called third fluxions: and so on . . . ad infinitum. . . . Certainly in any sense, a second or third fluxion seems an obscure mystery. The incipient celerity of an incipient celerity, the nascent argument of a nascent argument, i.e., of a thing which hath no magnitude.[96]

More important perhaps than this type of criticism of the calculus was a statement that has become known as Berkeley's Lemma:

> If with a view to demonstrate any proposition, a certain point is supposed, by virtue of which certain other points are attained; and such supposed point be it self afterwards destroyed or rejected by contrary supposition; in that case all other points attained thereby, and consequent thereupon, must also be destroyed and rejected, so as from thence forward to be no more supposed or applied in the demonstration.[97]

This applies to the methods of the calculus presented in the *De Analysi* and the *Methodus fluxionum*. In each of these works Newton had assumed an infinitesimal increment, o, at the beginning of the demonstration and then expanded the quantity (x + o) into the equation. Then, in order to obtain a result, the increment, o, was either rejected, as in the *De Analysi*, or allowed to vanish, as in the method of fluxions. But o was originally assumed to differ from zero so that setting it equal to zero later in the demonstration is contrary to the original assumption and invalidates the demonstration.

Berkeley's attack on the calculus was sufficiently powerful to draw fire from Newton's defenders. The resistance to his ideas led Berkeley to produce fresh attacks on the calculus which were, however, largely reiterations of the conclusions of the *Analyst*. All of this was

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[96] *Ibid.*, 4.

[97] *Ibid.*, 12.

in the context of the controversy over infinitesimals discussed earlier, which, by and large, was completely separated from mechanical, physical thought. Berkeley, however, as has been shown, did relate the calculus to mechanics, perhaps because from his point of view all theoretical knowledge of causes was of necessity hypothetical and mathematical. In any case, the relatedness of mechanics and the new analysis was further emphasized in the "Queries" at the end of the Analyst.

In Query 9, Berkeley asked if the doctrine of forces does not illustrate the involvement of mathematicians in disputes and paradoxes concerning things that cannot be conceived. Query 28 asks if the shifting of hypotheses--Berkeley's Lemma--is not a sophism that infects both mechanical philosophy and abstract geometry. In Query 30, it is asked whether motion can be conceived in a point of space. Query 48 asks if there may not be sound as well as unsound metaphysics and logic and whether modern analytics is not related to one of these. Finally, Query 56 asks

> . . . whether the corpuscularian, experimental, and mathematical philosophy, so much cultivated in the last age, hath not too much engrossed man's attention; some part whereof it might have usefully employed.[98]

Queries 48 and 56 indicate Berkeley's realization of the dependence of Newtonian mechanics on a specific logic and a specific metaphysic, and it is evident from the foregoing that his attacks on the mechanics and on the calculus arose out of his own metaphysics and logic. It is of some significance that Berkeley's metaphysical position was not a particularly startling one. Its core was contained in an essay by

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[98] Ibid., 50.

Michel Eyquem de Montaigne (1533-1592) entitled "Apology for Raymond Sebond." There he wrote that

> . . . to judge the appearances that we receive of objects, we should need a judicatory instrument; to verify this instrument, we need demonstration; to verify the demonstration, an instrument: there we are in a circle.
>
> Since the senses cannot decide our dispute, being themselves full of uncertainty, it must be reason. No reason will be established without another reason: there we go retreating back to infinity.
>
> Our imagination is not itself applied to foreign objects, but is conceived through the mediation of the senses; and the senses do not comprehend the foreign object, but only their own impressions. And thus the image and semblance we form is not of the object, but only of the impression and effect made on the sense; which impression and the object are different things. Wherefore whoever judges by appearances judges by something different from the object.[99]

Thus the thought of Berkeley can be seen as an outgrowth of a complex of ideas of considerable age. Furthermore, it had a great inner consistency and showed a mastery of logical and mathematical techniques that commanded respect. Berkeley himself, as bishop of Cloyne (1734), must have been a respected figure. All of this adds up to the fact that Berkeley's anti-Newtonianism could gain a hearing and command attention, whether or not it exerted any great influence on the course of English thought in mechanics. Such, however, was not the case with a contemporary of Berkeley's, Robert Greene (1678-1730), who is also known as an anti-Newtonian.

Greene's anti-Newtonianism, unlike Berkeley's, met with derision and contempt. He had difficulty in getting his work published, and one almost gets the feeling that he experienced something like persecution

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[99]Michel Eyquem de Montaigne, "Apology for Raymond Sebond," Selections from the Essays of Michel Eyquem de Montaigne, trans. & ed. Donald M. Frame (New York: Appleton-Century-Crofts, 1948), pp. 60-61.

for his ideas. In any case, Green's criticisms of Newton and the Newtonians, his own ideas on the nature of matter, and the resulting theories of mechanics are interesting in their own right. Greene's criticism of Newton started in the same place as Berkeley's--with Newton's notion of a homogeneous, passive matter, whose motions in absolute space and time form the basis of the phenomenal world. From this point their thought took different paths in overcoming the contradictions that seemed to arise from that position. Rather than assume with Berkeley that matter and force were not physical realities at all, Greene assumed that matter was identical with force, or, more accurately, with two forces which he called the "expansive" and "contractive" forces.

This placed Greene in a position much different from Berkeley's, for while Berkeley could still accept the whole Newtonian scheme as useful and meaningful in a limited sense, it was incumbent on Greene to develop a counter theory, one that could not "stand on the shoulders of giants" as Newton's theory did. Greene was not the heir to an established tradition, even though some of his ideas bear a certain resemblance to those of Leibniz. In effect he had to start from the beginning in the development of a comprehensive theory of mechanics, which, by the fact of its radical character, was doomed to failure.

The magnitude of such an undertaking is truly staggering and it should not be surprising if the final result turns out to be less than perfect in the sense of being free of inner contradictions and capable of accurately treating all known phenomena. Moreover, men who are already committed to one theory or doctrine do not take such things into consideration in their view of an opposing theory, especially when it is proposed by a man of little or no prior scientific or social distinction. Rather, they will simply discount the author as a crack-pot and assign his ideas to oblivion, no matter what their merit.

Such was the case with Robert Greene, and for this reason his story sheds a new light on the process of assimilation of Newtonian mechanics. Thus far it has appeared that the Newtonians, although they espoused the new mechanics, did not understand it in any depth. Through the career of Robert Greene one can gain some insight into the reasons behind that espousal, which, if not based on understanding and conviction, must have had strong extra-scientific elements.

Comparatively little has been written about Robert Greene except what has been excited by his eccentricities, since his thought ran counter to the main intellectual currents of his time. The few available details of his life shed no light on his development as an independent thinker. He was the son of a mercer of Tamworth in Staffordshire, who died when Robert was quite young. The responsibility for the boy's education was taken over by an uncle, John Pretty, who eventually sent the boy to Clare Hall, Cambridge. There Greene earned the B.A. in 1699 and M.A. in 1703. Subsequently, he became a fellow and tutor of his college and entered the ministry of the Anglican Church. In 1727 Greene served as proctor at Cambridge and in 1728 proceeded to the doctorate. He died in 1730 at Birmingham.[100]

Beyond this bare outline of Greene's activities almost the only information about him comes from his own writings, particularly from the

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[100] "Robert Greene," D.N.B., VIII.

prefaces to his two treatises on natural philosophy and his will. The first of the treatises on natural philosophy appeared in 1712 and was entitled The Principles of Natural Philosophy in Which Is Shown the Insufficiency of the Present System to Give Us Any Just Account of That Science.[101] In the same year, Greene published a work on solid geometry which occasioned some speculation as to his sanity. The historian Robert Sanderson (1660-1741) wrote to William Jones (1675-1749), a friend of Newton and Halley and vice-president of the Royal Society, that "the gentleman has been reputed mad for these two years last past, but never gave the world such ample testimony of it before."[102]

Augustus De Morgan (1806-1871) said of this sort of attitude that

> . . . it is the weakness of the orthodox follower of any received system to impute insanity to the solitary dissentient: which is voted (in due time) a very wrong opinion about Copernicus, Columbus, or Galileo, but quite right about Robert Greene. If misconceptions, acted upon by too much self-opinion, be sufficient evidence of madness, it would be a curious inquiry what is the least percentage of the reigning school which has been insane at any one time.[103]

If De Morgan's judgment of Sanderson's motives in imputing madness to Greene are correct, then Sanderson's opinion would have been strongly influenced by the remarks Greene made about Newton in the Principles of Natural Philosophy, the avowed purpose of which was to "evince

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[101] Robert Greene, The Principles of Natural Philosophy in Which Is Shown the Insufficiency of the Present Systems to Give Us Any Just Account of That Science and the Necessity There Is of Some New Principles in Order to Furnish Us with a True and Real Knowledge of Nature (Cambridge: Edm. Jeffery, 1712).

[102] Augustus De Morgan, A Budget of Paradoxes (2nd ed.; New York: Dover Publications, Inc., 1954), I, p. 135.

[103] Ibid., p. 136.

what little satisfaction we are to expect from reason, and even from those who have entered into the depths of it with the utmost genius and penetration."[104]

After this indirect assertion that even Newton could err, Greene became a bit more blunt. He wrote that there were basically two kinds of men who espoused the new philosophy. The first of these, "instead of pursuing truth without any bias, reason with inveteracy and design; . . ." men such as Hobbes, Locke, and Spinoza. The second kind consisted of men like Newton, Halley, Raphson, etc., for whom Greene professed the highest veneration. However, Greene felt himself "obligated to depart from their sentiments and apprehensions of nature in obedience to a just and . . . impartial inquiry into it. . . ."[105]

The first group had been willfully inimical to religion and virtue, and therefore had fallen into error. But that

> . . . the greatest and most exalted geniuses of their times should fall in with the same notions can no otherways be explained, than from their being unwarily led into them by the authority and impressions of those who writ before them.[106]

This must indeed have sounded like the babbling of a lunatic to Newton's friends and followers, for not only was Greene imputing error to Newton, but to the whole mechanistic tradition reaching back to Galileo. Only from the point of view of quite recent times does Greene's objection to mechanistic theory make a great deal of sense. Greene was, however, aware that he had taken on a huge task, one to which he most

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[104]Greene, Principles of Natural Philosophy, p. *2v.

[105]Ibid., pp. a2r-a3v.

[106]Ibid.

probably could not do justice.

> And as to any errors or mistakes, in respect of reason, it cannot be expected we should be entirely free from them, who are obliged to proceed in a different method from that which any philosophers have done, and therefore are deprived of those assistances from others, which might be some kind of direction to us in our inquiries . . . .[107]

Greene also seemed to feel that his work would have more to overcome than merely logical and scientific criticism; it would also have to overcome certain prejudices of a religious and political nature. An indication of this was given in the dedication of the Principles of Natural Philosophy, which was to Robert Harley, Earl of Oxford, who led the Tory government under Queen Anne (1702-1714) from 1710 to 1714.

England was at the time engaged in the War of the Spanish Succession which the Whig faction, under the leadership of Godolphin and Marlborough, had prosecuted with vigor. This war, along with all the others that had been fought against the France of Louis XIV, produced a considerable amount of aggressive national feeling, which, because the Whigs were, generally speaking, "Latitudinarians" in a religious sense, came to be associated with that religious position. In the period just prior to Harley's rise to power, the Whigs held most of the bishoprics while the High Churchmen, who were almost exclusively Tory, had an overwhelming predominance in the church as a whole. Thus, when in 1709 Marlborough requested of Anne that she make him Captain-General for life, many Englishmen felt that he was a Cromwell in disguise and that the established church as well as the crown was in danger. This feeling, in combination with the rising costs of the war and the slaughter at the

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[107] Ibid., p. b4r.

battle of Malplaquet, turned the tide of popular opinion against the Whigs.

Parliament was dissolved in 1710, and at the general election a strong church and Tory majority was returned, partially through the efforts of the Established clergy. Through their sermons, the clergymen inflamed their parishoners against the Whigs.[108] It was on this wave that Harley rode to power, and it was against this background that Greene saw Harley as a God-send, both to England and to the cause of true religion, both of which Greene wished to support with his own work. In his dedication Greene wrote that he could not but "believe . . . [Harley] to be raised by the providence of almighty God for the support and patronage of our most holy faith, against the insults of the several atheists, deists, Socinians, and . . . Arrians of our age."[109]

Greene later had reason to regret this support of Harley and Toryism, for by the time of the appearance of his second work on natural philosophy, _The Principles of the Philosophy of the Expansive and Contractive Forces_,[110] in 1727, the Whigs were long since back in power and the Tories were partially discredited in the eyes of many Englishmen because of efforts made on behalf of the Stuart pretender. Since the universities of Oxford and Cambridge were heavily church oriented, it was

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[108] _The Age of Louis XIV_, Vol. V of _The Cambridge Modern History_, ed. A. W. Ward, G. W. Prothero, and Stanley Leathes (New York: The Macmillan Co., 1908), pp. 466-469.

[109] Greene, _Principles of Natural Philosophy_, p. [*4r].

[110] Robert Greene, _The Principles of the Philosophy of the Expansive and Contractive Forces, or an Inquiry into the Principles of the Modern Philosophy, That Is, into the Several Chief Rational Sciences, Which Are Extant_ (Cambridge: Cornelius Crownfield _et al_., 1727).

natural that they also be Tory as regards their political sympathies. Indeed, there was Jacobite activity at both universities during the reign of George I (1714-1727).

For instance, on May 28, 1715, George I's first birthday since his accession, some bell's were rung at Oxford in celebration. This small show of loyalty to the Hanoverian so infuriated the mob that they tore down a good part of the Presbyterian meeting house.[111] This incident, which clearly shows the association of Whig politics and liberal religion, appeared originally in the writings of Thomas Hearne (1678-1735), a Tory and an anti-Newtonian.[112]

There is an account of some further events of May 28, 1715, stemming from Nicholas Amhurst (1702-1742), an ardent Whig, in which the Oxonians' actions appear even more treasonable. A group of Whigs, the Constitution Club, had met that evening to celebrate their monarch's birthday and had planned a bonfire for the occasion.

> But before the bonfire could be lighted, a very numerous mob, which had been hired for that purpose, tore to pieces the faggots and then assaulted the room where the club was sitting with brickbats and stones. All the time the mob was thus employed, the disaffected scholars, who had crowded the houses and streets near the tavern, continued throwing up their caps and scattering money amongst the rabble and crying out, 'Down with the Constitutioners; down with the Whigs; no G----e; Ja--s for ever. . . .[113]

This account was printed in Amhurst's periodical, the Terrae-Filius: Or, the Secret History of the University of Oxford in Several

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[111] Christopher Wordsworth, Social Life at the English Universities in the Eighteenth Century (Cambridge: Deighton, Bell and Co., 1874), p. 41.

[112] Wordsworth, Scholae Academicae, p. 71.

[113] Wordsworth, Social Life at the English Universities, p. 43.

Essays. The Terrae-Filius had a short existence, from January to July of 1721, [114] but during that period it sustained itself mainly through attacks on Oxford. Amhurst, writing in the Terrae-Filius,

> . . . claimed that he need 'not use any argument or produce any vouchers to prove' the existence of treason in Oxford, attacked the program of studies followed in the university, though admitting 'that Locke, Clarke, and Sir Isaac Newton begin to find countenance in the schools and that Aristotle seems to totter on his ancient throne,' adduced the usual charges of perjury, and compared the Oxford heads with the directors of the South Sea Company, whose fundamental crime had been to betray the trust reposed in them by the government and nation.[115]

Here the failure to teach Newtonian doctrines was virtually equated with treason, betrayal of the public trust. It appears that religion, politics, and science had become intermingled and that science had therefore picked up emotional overtones in the England of the 1720's, or at the very least in the mind of Nicholas Amhurst. Amhurst did apparently bear a special grudge against Oxford--he had been expelled for dissolute behavior--and his attacks may have been partly exaggerations and inventions, but they did achieve a popularity sufficient to warrant the republication of the Terrae-Filius in two volumes in 1727.[116]

Greene was sensitive to the emotional attachment to Newtonian mechanics and to the fact that it drew its strength from the force of nationalism. Therefore, in the preface to The Principles of the Philosophy of the Expansive and Contractive Forces, he attempted to divert this force to the support of his own ideas. He wrote:

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[114] Ibid., p. 612.

[115] W. R. Ward, Georgian Oxford. University Politics in the 18th Century (Oxford: At the Clarendon Press, 1958), p. 79.

[116] Wordsworth, Social Life at the English Universities, p. 612.

> I cannot here but aquaint the world that the present philosophers derive all their notions of nature from Italy and Galileus, or from Descartes and France, excepting what Kepler, a German, has done in respect of those sciences, and from whom Sir Isaac Newton is said to have taken his principle of gravitation, and who is esteemed to have been the most learned and sagacious man of his age; but in all other respects, our philosophy, as it is now received and embraced, is the product of popish countries. . . .
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> All therefore, which I design and intend is to propose a philosophy which is truly English, a Cantebridgian, and Clarensian one, as it was born and educated and studied in those places. . . .[117]

Greene's attempt at swinging public sentiment to his support by claiming to be the only really English philosopher and emphasizing the common foe of all the English religious factions was of course hopeless, and, if he was not mad before the publication of his magnum opus, its failure to make any impact may have deranged him slightly. The will that he left behind at his death in 1730, by its strange and pathetic character, certainly indicates a state of mind other than normal.

The main provisions of the will are all concerned with placing Robert Greene before the public eye. This goal is pursued in almost every conceivable fashion.

> Item, this frail and perishing body, which now continually cloggs the life and activity of the mind, weak and infirm at the best in its constitution, thin and consumptive in its frame and complection, and continually liable to rheums, catarrhs, and defluxions, I give and bequeath to the anatomist and physicians for the instruction and information of others . . . and if any observations occurr which may be of advantage to the world . . . it is my will and pleasure, that they should be communicated to it in the Philosophical Transactions or any other way the most extensive . . . .[118]

Further, the fragments of the carcass were to be buried as near the communion table as possible in All Saints, Cambridge, provided a new

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[117] Greene, Philosophy of the Expansive and Contractive Forces, p. [A4v].

[118] Gentleman's Magazine, LIII (1783), p. 657.

chapel were not erected at Clare Hall before his death. As for his bones, Greene willed that they be formed into a skeleton and placed in the library next to the books which he had written. The skeleton was to be called "Mr. Greene." Then the will goes on to specify the erection of monuments--each supplied with an extravagant description of himself--the preaching of sermons, and the making of awards to students in his name. Copies of his works, whether published before or after his death were to be presented to all the public libraries and to the libraries of each of the colleges of Cambridge and Oxford.[119]

Greene's will reveals something that must be considered as an obsession with the idea of making his work public and receiving recognition for it. This indicates that his desire for expression and recognition was frustrated during his life, which is understandable in terms of the above discussion, if not solely in terms of the intrinsic value of the system of thought that he produced.

The chief expression of Greene's natural philosophy is to be found in his Principles of the Philosophy of the Expansive and Contractive Forces, much of which was written even before the publication of his Principles of Natural Philosophy in 1712.[120] Therefore, the discussion of Greene's thought may be confined to the later work.

From Greene's point of view, the Newtonian and Cartesian philosophies were essentially the same; they both rested upon the fundamental notions of an inert matter and motion. In constructing a new

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[119] Ibid., pp. 657-658.

[120] Greene, Philosophy of the Expansive and Contractive Forces, p. [b_1r].

philosophy of nature, Greene would have to replace these ideas with his own basic concepts, which were those of the expansive and contractive forces.[121] To do this it was necessary to reduce the notions of matter, space, and time to the action of the two forces.

The substratum, or essence, of matter was force. Greene based this statement on the argument that the sensation of matter would be impossible unless some kind of action were "impressed upon our minds from it."[122] The gravity, or weight, of matter is one of our sensations of matter, and like solidity and extension, it could be different for equal quantities of matter. Such differences would arise from different innate forces, and since there is a doubly infinite number of possible combinations of the expansive and contractive forces, there must be a corresponding number of intrinsically different types of matter.

The concept of space as mere three dimensional extension, or the vacuum, Greene thought to be a misleading abstraction. Such an idea of space really says nothing of our experience of space, which includes light, heat, sound, etc.[123] Space was to be conceived as an actual sensation, and sensation is not possible without action or force. So far Greene's approach to the conception of space is reminiscent of that of Berkeley, but at this point, rather than attribute all action to mind, Greene pursued the notion of force as the basis of action in space. The intensity and combination of the forces he thought must be variable from point to

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[121] Ibid., p. [a_4v].

[122] Ibid., p. 286.

[123] Ibid., pp. 40-41.

point in space in order to account for the inhomogeneity of perceived space. The inhomogeneity of space was, in turn, expressed through the concept of a variable space density, which Greene expressed geometrically in terms of the dimensionality of the space. The dimensionality of space could be infinite; for instance, a point in Euclidian space might actually, that is in "greenian" space, have the dimensionality of a line, or a surface, or a solid, or any higher dimensionality.[124]

The dimensionality of the space occupied by a body was, for Greene, a means of representing the particular combination of intensities of its forces. Thus the normal distinction between "matter" and "void" disappears in Greene's system for both are represented by certain characteristics of space. Greene wrote that there is an infinite variety in the kinds of bodies, "whose different constipations of actions, or whose expansive and contractive forces, may be represented by these different extensions, which can never be reduced to an unvaried and abstracted one of mere length, breadth, and thickness . . . .[125]

The aether, as well as heat, or fire, and light, was one of the kinds of "bodies" represented by a certain dimensionality of space, so that both matter and space, in the usual sense, were seen to be only different manifestations of the expansive and contractive forces. In accordance with this conception, Greene denied the existence of solid, massy, inert, and impenetrable parts of matter.[126]

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[124]Ibid., pp. 229-230.

[125]Ibid., p. 230.

[126]Ibid., pp. 1-20.

The notion of time, like those of matter and space, had to be formed from experience. Therefore, Newtonian absolute time was seen as an unrealistic abstraction, and only time derived from some observed motion could have any significance.[127] But what motion should be chosen as the measure of time in any given situation? Greene's answer to this question is an essential part of his mechanics. He said that "real time is the same in its measure as the celerity or the space described by it . . . for the real times, in which spaces are performed are commensurate to those spaces which are performed . . . ."[128] That is, given a velocity, different "abstracted" lengths may be described in the <u>same</u> time, because "the first . . . [may be] more thin or diluted and the last . . . more constipated and dense," so that the same celerity, or force, will describe one as quickly as the other.[129] Or, velocity and <u>true</u> length, which is dependent on the density of the space traversed, are fundamental quantities rather than distance and time. Both velocity and space density, however, correspond to forces, so that Greene has indeed eliminated the notion of time from his philosophy and replaced it with forces.

The sense of Greene's notion of time and its measure can probably be seen best in terms of an example. If two bodies with the same force move through media of different densities, then the body in the denser medium will move through a smaller "abstract" distance in a given time than will the body in the lighter medium. The traditional mechanical

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[127] <u>Ibid</u>., p. 41.

[128] <u>Ibid</u>., p. 50.

[129] <u>Ibid</u>.

explanation for this would be that the velocity of the body in the denser medium is less since a part of its force is used in overcoming the resistance of the medium. Greene said, however, that velocities of bodies are the same if their forces are the same, but a body in a denser medium goes "further" in the same "abstract" distance. Time was a function of velocity and distance, rather than velocity being a function of distance and time, and both velocity and distance, or space, were conceived in terms of combinations of expansive and contractive forces.

Thus far, Greene's conceptions are internally coherent, but they have become quite divorced from the world of experience which they were to truly represent. He appears as something of a visionary in his assertion that all space is characterized by forces, and startlingly so in the notion that this can be represented in terms of a higher dimensionality, but he had no way of giving these forces any empirical significance.

Expansive force was made to "concur with velocity," and contractive force to act "counter to it and with gravity," and both could be either intrinsic to bodies or impressed upon them.[130]

These forces, and any combination of them, could exist in a point of space in accordance with the notion that a "point" may have any dimensionality. Since expansive forces corresponded to velocities, these too might exist in a point, a situation Greene thought to be manifest in the case where forces are applied to a body which cannot be moved by them.[131] As a consequence, the distinction between static and dynamic force did

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[130]*Ibid*., p. 59.

[131]*Ibid*., p. 288.

not exist in Greene's system; it was replaced by a distinction between different kinds of spaces--diluted or constipated ones.

Since static and dynamic forces were essentially the same, Greene was able to use the theory that he had developed for the treatment of simple static problems as the basis for a general treatment of dynamical situations. Greene's theory of the mechanical powers, or simple machines as such does not make use of all the ideas just developed, but rather handles the traditional problems in an almost traditional fashion. That is, his treatment is completely static, it sees all the machines as variations on the lever, and its key concept is that of equilibrium. The nature of equilibrium was, however, seen in a novel way due to the concepts of expansive and contractive force.

The equilibrium condition was seen as dependent on the fulcrum, or "center of detention" in Greene's terminology, as well as on the weights attached to the lever. The center of detention represented a force center of the opposite nature to that of the weights; it was contractive while the force of the weights was, in this application, expansive, since it tended to produce motion. If a weight is applied to the center of detention itself, the contractive force will completely predominate and no motion will result. But if the weight is applied at some distance from the center of detention, its expansive force will not be completely destroyed and some motion will result. In general, the weight of a body will exert an action proportional to its distance from the center of detention. Thus, if bodies are placed at the ends of a lever, each will exert an action proportional to its distance from the fulcrum and to its weight, so that, if the weights are in the inverse ratio of these distances, the system will remain poised.[132]

This "proof" of the fundamental theorem of the lever, although based on a new conception of the mode of action of force, still shows only a relationship between the static configuration of the system and the state of static equilibrium. In fact, Greene's proof is in the Archimedean tradition; its equilibrium principle is based on the idea of a center of gravity, which is a special case of the center of detention "on which forces are equally poised."[133]

However, because of the identity of static and dynamic force, which rests on the conceptions of matter, space, and time, Greene was able to treat dynamical states of machines with the same conceptual apparatus and to generalize still further to all problems involving centers of detention. In this regard, Greene first considered a lever with a single body of given gravitational force attached to it at some distance from the center of detention. The force of the body, according to Greene, is in such a case to be measured by the distance to which the body will move under its action, and is therefore dissipated as the motion progresses.[134] Thus the weight will move to a position such that it approaches the lowest point, but the total distance covered by the motion will be proportional to the original force of gravitation. If then that force corresponds to a distance greater than the length of the lever arm, the lever will

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[132]<u>Ibid</u>., p. 68.

[133]<u>Ibid</u>., p. 80.

[134]<u>Ibid</u>., p. 288.

oscillate or rotate until the entire force has been dissipated. If the gravitational force is permanent and constant, then the revolutions will be continuous and uninterrupted. A blow or percussion at some point of the lever would impart additional force to the system.

Greene gave a general mathematical expression to these ideas in a single relationship.

> The forces, or moments, or powers of bodies, which have a contractive or gravitating force, and one of percussion, and which act from a center of detention are greater, the greater that gravitating force is, the greater the sum or quantity of it is, the farther it is removed from the center of detention, and the greater the expansive force which is impressed, and the farther its distance is from the same center.[135]

If M and m represent the moments (Greene's equivalent of quantity of motion) of the bodies, C and c their contractive, or gravitational, forces, A and B the sums or quantities of these (that is, the volume of the bodies, since intrinsic forces are thought of as intensive quantities), R and r their distances from the center, and P and p the impressed expansive forces, then the following relation holds

$$\frac{M}{m} = \frac{ACPR}{Bcpr}\ .$$ [136]

If, in this relation, $A = B$ and $P = p$ and the forces of gravity increase with distance from the center, then $M/m = R^2/r^2$. That is, the gravitating "forces" will be as the squares of the distances from the center. Since, however, these forces "have the nature of an Expansive from it, their forces will be reciprocally as the square of the said

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[135] *Ibid.*, pp. 80-81.

[136] *Ibid.*, pp. 81-82.

distances. . . ."[137] This line of thought could then be applied to the motion of the planets around the sun through the substitution of an expansive, or repulsive, force in the sun for the rigid connection of the lever. That is, the expansive force of the sun is balanced by the contractive force of the planet with the result that the planets rotate rather than moving directly toward the sun. This idea Greene felt was implicit in Kepler's work.[138]

It was impossible to treat the sun simply as a gravitating mass. It must have a tremendous expansive power tending to repel the planets as well as to heat and light them. In fact, according to Greene, the expansive power of the sun is so great that were it not for the great contractive-cold power of the moon, the earth would be incinerated. The earth and the other planets are carried about the sun like a stone in a sling, their contractive forces acting analogously to the sling. Also, it is the rotation of the sun, combined with its expansive force, that produces the rotation of the planets on their axes.[139]

Thus Greene generalized the basic law of statics, the law of the lever, to the extent that it became the basis for the explanation of the motions of the planets around the sun. In that explanation, moreover, the sun was allowed to retain a character much more in keeping with its appearance rather than its function, in a completely abstracted fashion, as a huge conglomeration of inert matter. Greene's theory does, however,

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[137] Ibid., p. 82.

[138] Ibid., pp. 85-86.

[139] Ibid., pp. 177-178.

have the very considerable disadvantage of not being logically satisfactory. But then, as has been pointed out, much of the Newtonian thought of his contemporaries harbored large inconsistencies that did not noticeably detract from its popularity and for which there was far less excuse.

Another damaging aspect of Greene's philosophy was that its scope was forbiddingly large; in fact it was all-inclusive. Greene wrote that

> All properties of matter mentioned in philosophy are derived from . . . [the expansive and contractive forces], whether those which are termed accidental, or those which are called essential; all the principles in chemistry and the observations observable in it, as fermentation, precipitation, coagulation, crystallization etc. and all the phaenomenons which present themselves to us from the animal, the vegetable, or the mineral kingdom, all the principles of anatomy, and of the motion of the lungs, of the blood, and of the muscles, of the animal spirits, or the nervous juice, and the union of the human system with our minds, and the sympathy which the one has with the other, are likewise deducible from these forces of expansion and contraction.[140]

In fact, of course, Newtonian natural philosophy was just as broad in scope in so far as it postulated certain basic ideas concerning the nature of matter and of physical process in general, but neither Newton nor any of his followers attempted to apply their ideas to the entire spectrum of natural phenomena in a single volume. Greene, on the contrary, attempted, in his Philosophy of the Expansive and Contractive Forces, to say everything. The volume is divided into seven books titled as follows:

Book I. Concerning the Principles of the Mechanical Philosophy.

Book II. Concerning the Principles of the Physical Astronomy.

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[140] Ibid., p. 290.

Book III. Concerning the Chief Properties of Matter, as also Concerning the Principles of Chymistry, Anatomy, Pneumaticks and Hydrostaticks.

Book IV. Concerning Opticks, Dioptricks, and Catoptricks.

Book V. Concerning the Metaphysicks and Logicks, or the Systeme of Ideas of Mr. <u>Locke</u>.

Book VI. Concerning the Ethicks, or Natural Religion, of <u>Des-Cartes's</u> Meditations, Mr. <u>Locke's</u> Essay, of Dr. <u>Clarke</u>, and Mr. Wollaston.

Book VII. Concerning Algebra.[141]

It is easy to imagine what sort of reaction a work of this character must have evoked, and it is not surprising that Greene was ignored in the scientific writings of his contemporaries and all but forgotten in the succeeding years. Nonetheless, it is clear that Greene was aware that nothing but a complete system of knowledge could hope to replace the Newtonian system, and that his principle objections to that system have been vindicated in the course of development of physical theory. Thus, in some respects, Greene, like Berkeley, was more aware of the content and implicit scope of Newton's theory of mechanics than his Newtonian contemporaries. Beyond that, he saw the necessity of resolving the dichotomy between static and dynamic conceptions of force, which, as has been pointed out, was the basic failing of Newton's followers in their conceptual grasp of the new mechanics. If Greene's method of resolving this dichotomy was essentially different from Newton's, this does not alter the fact that he envisioned a unified system of thought growing out of fundamental ideas on the real nature of phenomena, in place of a number of unrelated and limited systems of thought applying to various specific areas of experience.

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[141] <u>Ibid</u>., pp. [b2r - b3r].

The Newtonians, throughout the period from the publication of the Principia to the 1730's, had not been able to grasp Newton's mechanics either as a complete and unified theory capable of handling all mechanical problems or as a program for the treatment of all problem areas in natural philosophy. The anti-Newtonians, however, did see Newtonian mechanics in this light and based their objections to it precisely on those features of the theory--the conceptions of homogeneous matter, space, and time, all capable of indefinite division--which gave it its great scope and power. Berkeley in particular seems to have realized that the logical framework of the theory was the calculus, which alone was capable of uniting these elements into a coherent, mathematical, explanatory framework.

Berkeley also objected strongly to the Newtonian error of attributing active force to matter, as opposed to Newton's actual understanding in which the only active substance was God acting in space. As has been pointed out, this error--making weight a property of matter--was a prime cause of the conceptual difficulties that stood in the way of a real grasp of Newton's theory. Consequently, English mechanicians throughout the period under consideration made little progress beyond the state of mechanical knowledge prior to Newton, except for a change in terminology. They were still largely confined to traditional methods of handling static problems which were not related to the methods used in other sorts of mechanical problems such as impact phenomena or the motion of pendulums.

In the same period of time on the continent, primarily in France, there was not only a good deal more work done in mechanics, but there was also a perceptible movement toward a unified theory of mechanics that reached a critical point even before the turn of the century.

CHAPTER III

FRENCH MECHANICS IN TRANSITION

The unified theory of mechanics that was produced by Newton and misapprehended by his followers was designed to deal with dynamic processes rather than solely with separate and distinct states of mechanical systems, as, for instance the equilibrium state of a simple machine or the states of a system of bodies before and after collision. The calculus of infinitesimals was crucial to the treatment of dynamic process, and, since the calculus was not generally understood by the Newtonians and certainly not associated with the mechanics, they did not understand Newton's mechanics as process. (Or perhaps it was their failure to conceive of the mechanics as process that caused them to miss the significance of the calculus.) In any case, the Newtonians rather consistently used Newton's terminology of force, quantity of motion, etc., in a static context, thereby distorting the mathematical relationship among these terms.

Basic to the idea of process is the idea of continuity. By the end of the 17th century the concept of continuity already had a long history in mathematical and metaphysical contexts, and it will be seen that French writers in mechanics made distinct attempts to apply continuity to the understanding of observable mechanical phenomena. Along with the very basic notion of continuity, another more specifically mechanical concept played a large role in the orientation of French mechanical thought toward process: the principle of virtual velocity. Newton made use of the principle as an application of the Third Law of Motion to explain the equilibrium condition, but his followers made very little of it. In French mechanical thought, on the other hand, the principle became a broadly accepted basis for the explanation of equilibrium, thus lending a dynamic character to static problems even before a really unifying theory of mechanics was at hand.

Still a third idea played an important role in impregnating French mechanical thought with the idea of process, namely elasticity. It has been pointed out that Newton included a conceptually adequate treatment of elasticity in the Principia, which, like the concept of virtual velocity, was not picked up and applied by the Newtonians. The reason for this was the confusion over the ideas of force and momentum. In their desire to make force a property of bodies, the Newtonians fused the conceptions of gravitational force and the force of a moving body and, in so doing, simply eliminated the notion of force as push or pull in favor of force as momentum. This step was in line with the prevailing conception of matter as composed of inert, impenetrable, perfectly hard atoms.

In such a system of thought, the treatment of impact phenomena was fundamentally concerned with inelastic collision and could do no more than state rules governing the velocities of bodies before and after impact. These rules, in turn, were based on the idea of the conservation of momentum or moving force, which could be a property of perfectly hard bodies. The problem of elastic collision, which implied the existence of active forces in bodies, was simply not accepted by the Newtonians as a significant one.

On the continent however, from the time of Huygens on, elastic collision was the subject of much investigation and exerted a considerable influence on mechanical thought toward the concept of continuous process. Elasticity clearly has the property of continuity, whereas perfect hardness has the opposite character. From the point of view of elastic collision the notion of an inert matter was an absurdity, and force appeared as the fundamental reality in the world of nature. This basic idea, which has already been seen in the work of Robert Greene, was elaborated by Leibniz into a philosophy of nature known as the theory of monads. This philosophy differed radically from that of Newton basically in that Newton ascribed the properties of continuity and force to the action of God in space while Leibniz made them the essential characteristics of actual phenomena. Thus both men, in spite of their differences, could and did incorporate the infinitesimal calculus, as the mathematics of the continuum, into their philosophies of nature.

Aside from the consideration of metaphysical questions, which will be further discussed in a later chapter, insofar as they have a bearing on mechanics, the problems and concepts that assumed importance in French thought on mechanics in the last decades of the seventeenth century tended to focus it on processes of change and therefore to make it more open to the assimilation of the calculus as its proper form of mathematical expression.

It has been stated that the mechanical thought of the early Newtonians was heavily influenced by the work of Jacques Rohault and that throughout the entire period from the appearance of the Principia until the 1730's little substantial progress was made beyond Rohault's views in the work of the Newtonians, in spite of the tremendous accomplishment of their master. Therefore, Rohault's mechanics constitutes a convenient starting point for a comparison of English and French work in the field.

His ideas on simple machines were expressed in the usual geometrical style, that is, in terms of sets of definitions, axioms, and theorems deduced from them. In order to give a complete representation of the theory of mechanics at this stage of its development, the full set of definitions, postulates, and axioms will be given, along with Rohault's proof of the fundamental theorem of the lever.

Definitions:

1. The absolute gravity of a body in a fluid medium is the force by which that body tends to descend when it touches nothing but adjacent parts of the fluid.

2. The relative gravity of a body is the force by which it tends to descend when it is in contact with something other than the medium, for instance, an inclined plane or some other machine.

3. The center of magnitude of a body is the point which is most nearly equidistant from all its extremities (and not at an infinite distance).

4. The center of motion of a body, or the fixed point, is the point upon which the body may rest, or about which it may revolve.

5. The <u>center of gravity</u> of a body is the point around which all the parts of a body are balanced, so that if the body is supported at that point in any situation, the parts on one side will have no more force than the parts on the other, and hence all the parts will be in equilibrium and hinder each other from descending.

6. <u>Power or moving force</u> is that by which a body may be sustained or moved.

7. The <u>quantity of a power</u> is determined by the quantity of the gravity of the body on which itacts, whether merely sustaining the body or drawing or pushing it in the line in which it tends to descend.

8. A <u>machine</u> is that by the help of which a body is either moved of hindered from moving. Machines are either simple or compound. The simple machines are the balance, the lever, the pulley, the wheel and axle, the wedge, the screw, and the inclined plane.

9. The <u>application</u> of a weight or power to a lever is the angle of the line of direction of the weight or power with the lever.

10. The <u>distance</u> of a weight or power is the distance from the point of application to the machine to the center of motion.

11. <u>Mechanics</u> is the science of the effects of powers insofar as they are applied to machines.

Postulates:

1. Heavy bodies tend to the center of the earth along straight lines which may be assumed to be parallel.

2. A power applied at right angles is capable of producing a greater effect than if it were applied obliquely.

Axioms:

1. The center of magnitude in a regular, homogeneous, horizontally situated body is also the center of gravity of the body.

2. The gravitities of homogeneous bodies are in the proportion of their bulks.

3. That which sustains any one point of a heavy body, sustains all its points which lie on the straight line passing through that point and the center of the earth.

4. A weight or power which pushes or draws a point of a body, pushes or draws all points of the body which lie in its line of direction.

5. If a power has its line of direction in a plane and tends to make that plane revolve around a fixed point, all the parts of the plane will receive an impression of the power in such a manner that all parts lying in a circle around the fixed point as center will tend to move about the fixed point with an equal force.

6. When a power applied to a machine is just able to sustain a weight, then just a little more power will both move and sustain the weight.

7. If the gravity diffused throughout all the parts of a body is able to move it, then if all the gravity is united at the center of gravity, the body will be moved as before.[1]

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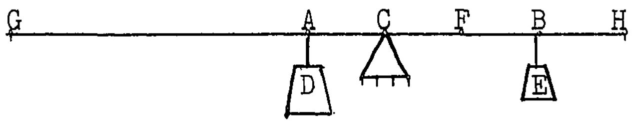
[1]Jacques Rohault, Oeuvres Posthumes de M. Rohault (Paris: Guillame Desprez, 1682), pp. 479-488. Jacques Rohault, A Treatise of Mechanicks: or, the Science of the Effects of Powers or Moving Forces As Applied to Machines, Demonstrated from Its First Principles, trans. Thomas Watts (2d ed.; London: Edward Symon, 1717), pp. 1-10.

The above definitions and axioms have a marked static and geometrical character as compared with those of Keill, at least insofar as terminology is concerned. The key concept is that of the center of gravity, which, however, contains the idea of oppositely directed but balanced forces which cancel one another. This idea, with the substitution of momentum for force, was the basis for Keill's proof of the fundamental theorem of the lever. (See pp. 74-75.) Also, at the end of his proof, Keill made use of Rohault's 6th axiom, which is a direct contradiction to Newton's thought. Thus Keill's proof, although framed in Newtonian words was not fundamentally different from that of Rohault, which is essentially the same as that of Archimedes (287-212 B.C.).[2]

Rohault's statement of the fundamental theorem of the lever is that "if two weights applied to the ends of a horizontal balance are in reciprocal proportion of their distances they will be in equilibrium."[3] That is, if the weights D and E, applied to the ends of the horizontal balance AB whose fixed point is at C, are in the proportion D:E :: BC:AC, then the balance is in equilibrium.



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[2]On the basis of the resemblance between Rohault's work and that of Archimedes it would seem possible to do without Rohault in explaining the particular form of the mechanics of the early Newtonians such as Keill. However, the sixth axiom of Rohault's system, which was used by Keill and others is not one of Archimedes axioms. See René Dugas, A History of Mechanics, trans. J. R. Maddox (New York: Central Book Co. Ltd., n.d.), p. 25.

[3]Rohault, A Treatise of Mechanicks, p. 12.

The proof proceeds as follows. A point F is determined such that FA=BC and FB=AC. Now, by axioms 1. and 7., the weights may be replaced by homogeneous bodies; D by GF and E by FH. Since D:E :: BC:AC and BC:AC :: AF:FB :: GF:FH by construction, then D:E :: GF:FH or D:GF :: E:FH. Thus the whole body, GH, is homogeneous and, since GA=BC and AC=BH, is supported at its center. By axiom 1. the center of gravity is thus at C and so, by the definition of center of gravity, the body will be in equilibrium.[4]

The essence of the above proof lies in the identification of the center of gravity of the system independently of the state of equilibrium. This is done through axioms 1. and 7. Thus a relation is established between the geometrical configuration of the system and the state of equilibrium. Or, what amounts to the same thing, force is replaced by extension in the mathematical treatment of the problem.

The notion of force, or power, is contained in definitions 1,2, 6, & 7 where it is identified with weight, or gravity; its effect is to sustain a weight or to move it uniformly along the line in which it tends to descend. Thus, it is easy to see how the identification of weight and momentum could have developed out of Rohault's force concept. There is a further suggestion in this direction in his explanation of the nature of gravity.

He wrote that all bodies on earth experience a centrifugal force due to their inertia and the rotary motion of the earth. Some bodies experience this to a lesser degree than others and hence are forced

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[4] <u>Ibid</u>., pp. 12-13.

downwards.[5] Hence what we experience as gravity is really the result of an inertial motion. The identity of weight and motion is further suggested by Rohault in his extension of the above ideas to explain the acceleration due to gravity. When a body begins to fall, its velocity is not very great because the subtle matter permeating the universe cannot at first make the body move with all the velocity with which the subtle matter tends to flee the center of the earth. But, once the body has been started, the subtle matter, which is trying to gain as much height as possible, continues pushing the body downwards and so continually adds new degrees of velocity to the body. Therefore, the fall will be more rapid in proportion to the height from which it began.[6]

The acceleration of gravity, according to this account, is due to a transfer of motion, which is what is experienced as weight. Transfer of motion, as pressure or impact, is therefore the fundamental process of the whole complex of mechanical phenomena as far as Rohault was concerned. This insight can of course be traced to Descartes, and, insofar, it is the same as Newton's approach to mechanics. The great difference between the Cartesian and Newtonian approaches to mechanics lies in the fact that Descartes, and Rohault after him, saw the fundamental process of the transfer of momentum as the instantaneous impact of inert bodies, that is, inelastic collision, and Newton transformed this into the action of force--pressure--in time, that is, into a continuous process.

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[5]Jacques Rohault, Traité de Physique (sixiéme edition; Paris: Guillame Desprez, ruë saint Jacques, MDCLXXXIII), II, 131-132.

[6]Ibid., pp. 137-138.

Unfortunately for Rohault, the Cartesian exposition of the phenomenon of impact was not understandable in any scientific sense.

> Impact and pressure--the only causes he is prepared to allow--must . . . be recognized as operating in a manner not dynamically explicable, not even when operating between entities all of which are physical . . . . Save on a metaphysical basis, and by this he means on a theistic basis, there can be no understanding, none at least that is genuinely scientific, of motion and of the laws to which it conforms.[7]

Thus Rohault had to confine himself on this all-important subject to an appeal to God.

The first consideration was that God had created a certain amount of motion at the beginning and that that original quantity is always absolutely conserved in the ordinary concourse of bodies. Therefore, if a body in motion meets one at rest and pushes it before itself, it must lose as much of its motion as it communicates to the other.[8] On this basis, Rohault was able to draw deductions concerning a few specific cases of the impact of bodies of various sizes, but could do nothing toward the production of a consistent explanation of the laws either of machines or of falling bodies in terms of the laws of impact.

In spite of obvious shortcomings, Rohault's views on mechanics do serve to point up the difficulties which had to be overcome in the creation of a unified theory, given the preconceptions common to the age. His theory further serves as a standpoint from which the work of succeeding writers may be seen in perspective, either as conforming to his thought or showing some significantly different traits. By its defects

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[7]Norman Kemp Smith, New Studies in the Philosophy of Descartes, Descartes as Pioneer (London: Macmillan & Co. Ltd., 1952), p. 194.

[8]Rohault, Traité de Physique, p. 71.

Rohault's mechanics indicates the direction that must be taken by future attempts at the production of a unified theory of mechanics. In particular, the laws of impact must be more thoroughly represented and correlated with the other areas of mechanical knowledge. That is, the conditions described earlier for the reduction of theories must be met. (See pp. 11-12.)

Even before the publication of Rohault's Traité de Mechanique the laws of impact had become the subject of much investigation. Almost simultaneously, in 1669, three authors produced sets of rules applying to the collision of bodies. In the issue of the Philosophical Transactions of January 1669 Dr. John Wallis and Dr. Christopher Wren published accounts of theories of impact.[9] Christian Huygens (1629-1695) published a similar work in the Journal des Scavans in March of the same year which was republished in the Philosophical Transactions in April, 1669.[10]

Wallis' theory of impact treats only inelastic collision and is based on fundamental algebraic laws. The laws that serve as postulates for his theory are that equals added to equals produce equals; that if A produces the effect E, the 2A produces the effect 2E, 3A produces 3E, and so on; and, lastly, the law of association, mPC = mPxC = PxmC. These

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[9]John Wallis, "A Summary Account Given by Dr. John Wallis of the General Laws of Motion, by Way of a Letter Written by Him to the Publisher and Communicated to the R. Society, Novemb. 26, 1668," Philosophical Transactions, III (1668), pp. 864-866. Christopher Wren, "Theory Concerning the Same Subject. . . ." Philosophical Transactions, III (1668), pp. 867-868.

[10]Christian Huygens, "A Summary Account of the Laws of Motion," Philosophical Transactions, IV (1669), pp. 925-928. Christian Huygens, "Regles du mouvement dans la rencontre des corps," Journal des Scavans, II (1667-1671), pp. 531-536.

statements amount to a mathematical assertion of the widely held notion that physical bodies and their motions only differ according to quantity. Specifically they state that the motion of a body M with a velocity of nC is the same as the motion of a body nM with a velocity C, where n is only a number. This Wallis saw as the principle of all machines for the facilitation of motion as well as the principle governing the collision of bodies.[11]

Not only did this strictly algebraic principle support the identification of force and momentum,[12] but it also yielded a very simple means of deducing the desired rules of collision. If it is also assumed that colliding bodies will remain together after impact, then, if a body P moving with velocity C--and hence a force PC--impinges on a body mP, the two bodies will move on together with a velocity $\frac{1}{1+m}C$. That is, $PC = \frac{1}{1+m} \times (P + mP)C = (P + mP) \times \frac{1}{1+m}C$.[13] Thus the conservation of motion in impact, in the algebraic sense, was simply a physical interpretation of the algebraic law of association, which, as the title of the article indicates, was seen as a "general law of motion."

Wallis' physical theory was thus clearly based on algebra, which deals with finite quantities, even though, as has been pointed out, his

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[11] Wallis, Philosophical Transactions, III, 864-865. See also John Wallis, "The General Laws of Motion," Philosophical Transactions Abridged, I (1665-1672), pp. 307-309.

[12] In his De Motu, appearing in 1669-70, Wallis defined force as that which is capable of causing motion, but used the term almost exclusively to mean momentum, i.e., only motion can cause motion. Momentum he defined as "that which tends to the production of motion," but it was also proportional to the product of force and time. J. F. Scott, The Mathematical Work of John Wallis, D.D., F.R.S. (1616-1703) (London: Taylor and Francis Ltd., 1938), pp. 108-110.

[13] Wallis, Philosophical Transactions, III, 865-866.

mathematical work provided considerable impetus to the development of the calculus, and his idea of force as the cause of motion came close to an anticipation of Newton's insight into the nature of force. This being the case, Wallis naturally treated the subject of elastic collision as a sort of addendum to the theory of inelastic collision.

> If the bodies be not absolutely hard, as is above supposed, but elastic, yielding to the stroke, and then restoring themselves to their figure again by an equal force, the bodies, instead of moving on together, may in that case recede from each other, and that more or less in proportion to the restoring force . . . .[14]

Presumably the "restoring force" referred to had the nature of a momentum, so that it was possible to think of elastic action in terms of the absorption and subsequent emission of momentum by the colliding bodies. It was on this basis, as will be shown, that much of the further study of elasticity developed.

In contrast to Wallis' theory of impact Wren's work dealt with elastic collision and was based on the assumption that there is a "natural velocity" for each body. The natural velocity of a body was taken to be reciprocally proportional to the body. That is, if there are two colliding bodies A and B, their respective natural velocities V_a and V_b are in the ratio $\frac{V_a}{V_b} = \frac{B}{A}$. After collision, the two bodies would retain their natural velocities. If, on the other hand, colliding bodies do not have their natural velocities before collision, then collision will correct this imbalance by transferring motion from one body to the other such that the velocities after collision are in the proper ratio to the bodies.[15]

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[14] Wallis, Philosophical Transactions Abridged, I, 310.

[15] Wren, Philosophical Transactions, III, 867-868. Philosophical Transactions Abridged, I, 310-312.

The way in which this must be understood is that if there are two bodies A and B in collision, with initial velocities V_a and V_b that are not "natural" velocities, then, assuming that V_a is greater than the natural velocity of A, the excess of V_a over the natural velocity of A will be transferred to B in the collision. By definition, the natural velocities are in the inverse ratio of the bodies so that A's natural share of the total velocity of approach, $V_a + V_b$, must be

$$\frac{B}{A + B}\,(V_a + V_b).$$

If V_a is greater than this quantity, then the excess is

$$V_a - \frac{B}{A + B}\,(V_a + V_b) = \frac{AV_a - BV_b}{A + B}\,.$$

This quantity is therefore added to the natural velocity of B, which B retains in the collision, to form the final velocity of B after collision. However, the excess of V_a over the natural velocity of A is the expression for the velocity of the center of gravity of the system. Thus, the rules for the velocities of bodies in elastic collision that could be derived from Wren's system were correct, from the modern point of view.

Wren's theory, although it yields correct results, is nonetheless inadequate. The postulates from which the rules are derived are equivalent to the rules themselves; their scope does not exceed the laws or rules which they were designed to explain. This can be seen clearly from the fact that the concept of natural velocity only has meaning in a collision situation. The theory was therefore of no consequence in the further development of mechanics.

Huygens's rules of elastic collision, as they appeared in the Philosophical Transactions, were the same as Wren's, but the theory behind them was much different. That theory did not appear with the rules in 1669, but considerably later, in 1703, in a work entitled Tractatus de motu corporum ex percussione.[16] In this work, five postulates are listed, chief among which is the statement that if two equal bodies with equal velocities come from opposite directions and meet each other directly, then they will both rebound with the same speed with which they came.[17] This statement corresponds to the equilibrium principles of a theory of statics, such as the statement that equal bodies at equal distances from the point of suspension of a lever will be in equilibrium. The structural similarity to static theory goes still further, since, in order to derive the theorem corresponding to the fundamental law of the lever, Huygens made use of an axiom relating the center of gravity of a system to its dynamic configuration. That is, the theorem that bodies whose masses are inversely proportional to their velocities will rebound from collision with the same velocities was proved on the basis of the axiom that the common center of gravity of a system of bodies moving only under the influence of gravity cannot ascend.[18] This basic theorem of elastic collision, which had the status of an axiom in Wren's theory, along with the assumption that a common

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[16]Christian Huygens, Über die Bewegung der Körper durch den Stoss. Über die Centrifugalkraft ("Ostwald's Klassiker der exakten Wissenschaften," Nr. 138; Leipzig: Wilhelm Engelmann, 1903), p. 64.

[17]Ibid., p. 3.

[18]Ibid., pp. 16-20.

motion of both bodies has no effect on the results of collision, forms the basis for calculating the velocities of bodies after impact.

Huygens then proved a number of theorems of a general nature. He showed that the sum of the products of the masses and the squares of the velocities before and after collision will be equal. The conservation of the quantity of motion was a further consequence of the theory, along with the conservation of the velocity of the common center of gravity. All of these conservation laws were presented in 1669 in the Journal des Sçavans, but the conservation of the velocity of the common center of gravity was singled out as "une loy admirable de la Nature."[19]

With the work of Wallis and Huygens, two theories of impact became available. While both of them yielded verifiable results in their limited fields of application, the two theories showed considerable differences in structure. The one derived from a strictly mathematical axiom and the other from statements with a direct physical meaning. The physical significance of Huygens' postulate concerning the center of gravity of a system of bodies moving solely under the influence of gravity was most clearly related to the motion of pendulums and to the motion of freely falling bodies and thus implied a logical connection between elastic collision and gravitational attraction.[20]

Such a connection was exploited, in a strictly experimental fashion prior to the publication of Huygens' theory of impact in a work

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[19]Huygens, Journal des Sçavans, II, 534.

[20]The principle in question was earlier enunciated by Huygens in his Horologium oscillatorium sive de motu pendulorum ad horologia aptato demonstrationes geometricae (Paris, 1673), Dugas, p. 187.

of Edme. Mariotte (1620-1684) entitled Traite de la Percussion ou Choc des Corps.[21] Like Rohault, Mariotte assumed that inelastic collision was the basic phenomenon of dynamics. However, since there were no hard bodies, that were also perfectly inelastic, available for experimental purposes, he built up his theory on the basis of experimentation with soft bodies. The experiments were performed with pendulums contrived so that the bobs--the colliding bodies--were just touching at the lowest point of their respective arcs. With this arrangement it was possible to measure velocities before and after impact by the length of the arcs traversed as the bobs descend before or ascend after collision. With this apparatus Mariotte was able to establish a series of propositions equivalent to the rules of impact derived on an a priori basis by Wallis. Principle among these was the law that colliding bodies move together after impact with a velocity equal to the algebraic sum of the momenta before collision divided by the sum of the masses.[22]

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[21]This treatise is contained in the Histoire de l'Academie Royale des Sciences, Tome I, Depuis son établissement in 1666 jusqu'a 1686 (Paris, 1726). The entry in the Histoire dates from 1674. The work went through three editions by 1679; the third being the basis for the edition appearing in the collected works of Mariotte. Oeuvres de M. Mariotte de l'Academie Royale des Sciences; comprenant tous les Traitez de cet Auteur, tant ceux qui avoient deja paru separément, que ceux qui n'avoient pas encore été publiés; Imprimées sur les Exemplaires les plus exacts & les plus complets; Revûes & corrigées de nouveau, I (The Hague: Jean Neaulme, 1740), p. [**2v].

[22]Mariotte, Histoire de l'Academie Royale des Sciences, I, 184-185. Mariotte's work was not totally independent of theoretical foundation. He needed three assumptions for the interpretation of his experiments: a statement equivalent to Newton's First Law; the statement that the heights to which bodies will rise are proportional to the velocities with which they begin the ascent; and the statement that the small oscillations of a pendulum may be assumed to be equal in duration even though the arcs through which they travel are different. Mariotte, Oeuvres, I, 4-5.

The principles of inelastic collision then served Mariotte as the basis for the explanation of elastic collision. The motion of colliding elastic bodies after their initial contact could be divided into two parts: the primary, or inelastic, and the elastic, or reflective. The primary, or inelastic, part of the motion was the motion they would carry out if they remained together. The secondary motion was regarded as one superimposed on the primary motion by the action of the <u>ressort</u>, or elasticity of the bodies.[23]

In order to establish the character of the "secondary" motion, Mariotte again had recourse to experimentation. His ninth experimental principle states that if an elastic body is struck by a hard and inflexible body, it will, upon regaining its original form, give back to the striking body its original velocity. The proof of this principle was an experimental demonstration making use of the same equipment as before.[24] Then follows the proposition that if two elastic bodies whose velocities are reciprocally as their weights strike each other directly each body will rebound with its initial velocity. Mariotte demonstrated this proposition on the basis of the above principle; since the primary motion is zero in this case, the shock will have the same effect as if each body had struck an inflexible body. Each body will then deform the other to the same degree, and, in resuming their original forms will regain their original velocities.[25]

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[23] Mariotte, <u>Oeuvres</u>, I, 28.

[24] <u>Ibid</u>., p. 23.

[25] <u>Ibid</u>., p. 29.

The first consequence of this proposition is that any two bodies pressed together so that they are in "tension" will, upon the release of the restraint, repulse each other in such a manner that each body takes an equal quantity of motion. That is, the reflective motion will always be such that the velocities of the two bodies are reciprocally as the weights. The second consequence is that the elastic bodies share the relative velocity of the collision (the mutual velocity of approach) according to the inverse proportion of their weights, whatever "proper" velocities they may have had before collision.[26]

In one sense, Mariotte's approach to elastic collision is similar to Wren's; both of them wrote in terms of a sharing of the relative velocity of collision by the colliding bodies in the reciprocal ratio of their masses. However, Wren's emphasis was placed on what looks like a metaphysical notion of "natural" velocity and a process whereby nature evens out excesses and deficiencies. Mariotte, on the other hand, focused attention on the elastic action of the bodies themselves as the key to the problem.

Mariotte also added new interest to the consideration of elastic impact through his speculation on a "paradox" that appeared as a consequence of the principles he had elaborated. Consider a body, A, at rest with a mass of one to be struck by a body, B, with a speed of one hundred and a mass of ninety-nine. Their primary velocity will then be $\frac{99 \times 100}{1 + 99} = 99$. Now the bodies share the relative velocity of 100 according to the inverse ratio of their masses so that A receives 99 units of

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[26] *Ibid*., p. 30.

velocity from the elastic action and B receives 1 unit. Thus A has a total velocity after collision of 198 and B a total velocity of 98.

> Il a donc donnè à un autre corps presque le double de la vitesse qu'il avoit lui-meme, & il a conservé la sienne presque entiere, & tout la vitesse qui etoit avant de choc est presque triplée par le choc.[27]

On the other hand, if the body at rest has a mass of ninety-nine and the body moving with a velocity of one hundred has a mass of one, after collision the large body will have a speed of 2, or a motion of 198, and the small body will have a motion of 98, totaling 296.[28] In this case the total number of degrees of velocity remains the same while the amount of motion is increased, while in the first case the opposite was true.

The paradoxical nature of these observations naturally disappears if they are analyzed on the basis of the algebraic conservation of motion, but that is a prejudice that Mariotte did not share with Wallis. He seems to have felt that his paradox might yield some profound insight into mechanical law, for he went on to further speculation on the subject that is of interest both for its form and for its physical significance. He conjectured that if the masses of the two bodies were made ever more unequal "to infinity," then when the small body is struck by the large one, the large body will preserve all its speed and will give twice as much to the small body. At least that speed would differ from "twice as much" by less than the smallest number one could imagine. In the other

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[27]Mariotte, Histoire de l'Academie Royale des Sciences, I, 189.

[28]Ibid.

case, the large body would not move at all and the small body would retain the same speed.

"Limits" of this nature were common to the mathematics of infinitesimals of Bonaventura Cavalieri (1598-1647), the so-called method of indivisables, which was contained in his Geometria indivisibilibus continuorum nova quadam ratione promota of 1635.[29] The significant thing about Mariotte's use of the idea is however that he attempted to use it to establish the relationship between weight and momentum; that is, he saw that this crucial dynamic relationship could be approached on the basis of infinitesimals. The context in which Mariotte tried to establish the weight-momentum relationship was the proof of a proposition necessary to the treatment of the problem of the center of percussion of a physical pendulum.

The proposition states that if the quantities of motion of bodies falling on the ends of a lever are inversely as their distances from the fulcrum of the lever, then there will be equilibrium at the instant of impact.[30] The proof of the theorem starts from the fundamental theorem of the lever--in the equilibrium condition the weights are inversely as their distances from the fulcrum. One of the weights on the lever is then replaced by a jet of water that can also sustain the equilibrium of the lever. Now each particle of water is a body much smaller than the body it strikes and is perfectly elastic. Thus the situation is

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[29] J. F. Scott, A History of Mathematics (London: Taylor and Francis, 1960), pp. 106-107. The example of Cavalieri's method given by Scott is concerned with a strictly geometrical problem, but the similarity to Mariotte's usage is still apparent.

[30] Mariotte, Oeuvres, I, 82.

a physical approximation to the second part of Mariotte's application of the "method of indivisibles" to his paradox. Since a continuous series of shocks of tiny elastic particles can sustain a stationary weight, each instantaneous shock must have the same nature as weight. Thus the sameness of weight and impact is established.

Mariotte did not pursue this line of inquiry further. Instead he replaced the remaining weight by another jet, so that now two jets falling through different heights maintained equilibrium across the balance. The next step was to replace the jets with equal solid bodies falling from the same heights as the jets; they too must maintain equilibrium at the moment of impact. From this point the proof was completed by means of the relationship between the velocity of a falling body and the height through which it has fallen.[31]

Mariotte now introduced a new expression, "solid quantity of motion," in order to achieve an economical expression of the above proposition.

> L'on voit par ces raissonemens qu'afinque deux corps étant en movement & tombant de part & d'autre du centre d'une balance en même tems, fassent equilibre au moment de leur choc; il faut que le nombre solide, produit par la multiplication du poids de l'un par sa vitesse & par la distance du point où il tombe jusques au centre de la balance, soit egal au nombre solide de l'autre poids multiplié de même . . . .[32]

Mariotte felt that this principle was a very important one. It formed the basis for his treatment of physical pendulums and embodied the basic laws of falling bodies, statics, and impact. It can be seen that

[31] Ibid., pp. 82-83.

[32] Ibid., p. 84.

the principle, as stated by Mariotte, bears a certain resemblance to the principle of virtual velocity since, in the instant of impact, the momenta of the bodies are the acting forces and their distances from the center can be taken to represent velocities. As has been stated, the principle of virtual velocities was to play a significant role in French mechanical thought; even in this crude and imperfect form it gave promise of providing the key to a unified theory of mechanics.

In another work, the Traite du mouvement des Eaux, Mariotte raised his principle, in a somewhat altered form, to the status of a universal principle of mechanics.

> Lorsque deux poids ou deux autres puissances sont disposées en sorte que l'une ne puisse se mouvoir qu'elle ne fasse mouvoir l'autre, si l'espace que doit parcourir un des poids selon sa direction propre & naturell est à l'espace que doit parcourir l'autre in même tems selon sa direction propre & naturelle reciproquement comme ce dernier poids est au premier; il se fera equilibre entre les deux poids; mais si l'un des poids est in plus grande raison à l'autre, il le forcera.[33]

Thus Mariotte, although starting from the same suppositions with regard to the nature of matter as were held by his contemporary Rohault, was able to produce a system of thought that cam very close to a unified theory of mechanics. Of course, in so doing, Mariotte had deviated from the notion that inelastic collision must be the fundamental dynamic phenomenon. The key relationship of his theory, that between weight and momentum, was based on the property of elasticity, which however was itself not understandable in terms of the current, that is Cartesian, idea of matter. Perhaps for this reason Mariotte's ideas were not accepted by the entire French scientific community or by English writers. However,

[33]Mariotte, Oeuvres, II, 360.

from this time onwards there were two main trends in continental mechanical theory; one continuing along the lines set in Rohault's mechanics, and the other drawing the implications contained in the work of Mariotte. (Perhaps it would be better to define these two trends in terms of the greater names of Descartes and Huygens, but both Rohault's and Mariotte's works contained extensions or modifications of the ideas of the former writers that were of significance.)

The main implications in Mariotte's work centered around the ideas of continuity and process. As has been pointed out, elasticity has the property of continuity, and elasticity forms the basis of Mariotte's synthesis. The type of speculation that he used to approach the weight-momentum relationship, the notion of limit, depends on the idea of continuity, and the resulting "universal principle," that of virtual velocities, represents the static situation in terms of motion, or process. On another level, Mariotte's work can be seen to suggest that the force of moving bodies is not to be measured by their quantity of motion. For instance, in the two situations that made up his paradox, there was a difference in the quantities that were conserved: total velocity in one case and quantity of motion in the other. As was pointed out, the idea of algebraic conservation of motion would resolve the paradox, but, for those who did not hold to the absolute character of space, that solution was not meaningful. On the other hand, the product of mass and the square of the velocity was conserved in both cases. This fact could be tied in with the proposition that the height to which a body will rise is proportional to the <u>square</u> of its velocity at the beginning of the ascent. This proposition, in turn, provided the link between impact and stationary weight in the experiment with the water jets maintaining equilibrium with a weight.

Most importantly perhaps, the idea of elasticity as a property of bodies and as a source of motion suggested a causal relation between weight, or pressure, and momentum. That is, bodies, by their very nature could be seen to be agents capable of producing motion. For instance, Mariotte had concluded as a first consequence to his law of elastic collision that when two elastic bodies are pressed together, they will repel one another in such a fashion that each will take an equal quantity of motion. Here motion had clearly been created--unless one wished to insist that since the motions were equal and oppositely directed they added up to no motion at all--out of a pressure, or tension, such as can be produced by weight and is equivalent to weight.

All of these implications were not seen by Mariotte. He did not attempt to elaborate the causal relationship between weight and momentum that was contained in his observation of the ability of a stressed body to produce motion or of a continuous series of shocks to sustain a weight. The reason that he did not do this, or see that it could be done, was simply that he was not in possession of the necessary logical tools. As has been shown in connection with Newton, the infinitesimal calculus was necessary to the treatment of the causal relation between weight and momentum.

While Mariotte did make an attempt to apply a mathematical idea associated with the beginnings of the calculus to his physics, his approach lacked, in particular, the important notion of causality as formulated by Thomas Hobbes. The Hobbesian concept of causality implied the existence of indefinitely small constituent parts of both matter and motion and, further, that the understanding of phenomena must be in terms of such parts. This idea was part of Newton's thought in the development of the new mechanics and the calculus, although he made no mention of any debt to Hobbes on this score. The other inventor of the calculus, Gottfried Wilhelm Leibniz, however, explicitly acknowledged his enthusiasm for Hobbes' ideas, and it was Leibniz who provided the basis for a consistent mechanical interpretation of all of the above implications in Mariotte's work.

In a letter to Hobbes dated July 1670, Leibniz wrote that

> There is nothing more polished and better adapted to the public good than your definitions. Among the theorems which you deduce from them there are many which will remain established. There are some who have abused them, but I believe that in most cases this occurred because the right principles of application were ignored. If one were to apply the general principles of motion--such, for example, as that nothing begins to move unless it is moved by another body, that a body at rest, however large, can be impelled by the slightest motion of a moving body, however small, and others--if one were to apply these by an ill-timed leap to sensible things, he would be derided by the common man . . . .[34]

This passage suggests that Leibniz had the important insight that Hobbes' ideas on motion were first of all concerned with the behavior of the indefinitely small, insensible elements of motion and that the transition from these elements to the explanation of sensible phenomena was one that required a particular technique. Hobbes' ideas seem to have impressed Leibniz deeply as the key to the understanding of the world and of God, for, in the same letter, Leibniz praised Hobbes in a rather unusual manner.

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[34]Gottfriend Wilhelm Leibniz, Philosophical Papers and Letters, trans. and ed. with an introduction by Leroy E. Loemaker (Chicago, Ill.: University of Chicago Press, 1956), I, 163.

> I shall always profess . . . that I know no one who has philosophized more exactly, clearly, and elegantly than you, not even excepting that man of divine genius, Descartes himself. I wish that you, my friend, who of all mortals could best do it, had taken into consideration what Descartes attempted rather than accomplished--that you had ministered to the happiness of mankind by confirming the hope of immortality.[35]

Thus there was by 1670 a combination of ideas in Leibniz's mind similar to that which has already been pointed out as the metaphysical ground from which the mechanics and mathematics of Newton developed. The Platonistic element in Leibniz's mentality, which insisted that mathematical investigation of nature would, if properly conducted, lead to the assurance of the existence and activity of God, stemmed from his formal education. Although his teachers have been described as Protestant Aristotelians, they were scholars of an especially eclectic variety and most were members of the Herborn school of encyclopedists.[36] Following in the tradition of the Florentine Academy, this school of thought sought its unifying principle in Christian Platonism. Their primary influence on Leibniz was to provide him with a "new Platonistic metaphysics of universal harmony governing a multitude of interrelated, vitalistically conceived individuals."[37] Beyond that, the Herborn school influenced Leibniz toward a rationalism in which experience, reason, and revelation were regarded as complementary sources of knowledge.[38]

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[35]Ibid., p. 166.

[36]Leroy Loemaker, "Leibniz and the Herborn Encyclopedists," Journal of the History of Ideas, XXII (1961), 332.

[37]Ibid., p. 324.

[38]Ibid., p. 331.

With this complex of ideas in mind, Leibniz produced, in 1671, a "Theory of Abstract Motion" which he dedicated to the Paris Academy. The theory postulated several principles that show a strong resemblance to Hobbes' ideas on matter and motion but also preserve the notion of the "vitalistically conceived individual," that is, an individual conceived, not in terms of inert matter, but in terms of active force. Motion was, first of all, continuous, which is to say that it is divisible into indefinitely small elements. Further, when a body is at rest, it will always remain at rest unless a new cause of motion occurs. Conversely, if a body is in motion it will maintain both speed and direction unless a cause for a change occurs.

The cause of motion was called conatus, which "is to motion as a point to space, or as one to infinity, for it is the beginning and end of motion."[39] Conatus was conceived by Leibniz as the action of one body on another, in accordance with the notion of causality. This implied that bodies could not be perfectly hard and impenetrable. In impact, the boundaries of the colliding bodies must either interpenetrate or be in the same point of space. This interpenetration, or beginning of union of the colliding bodies was accompanied by, or produced, a conatus which ended their motion relative to one another.[40]

Leibniz also ascribed conatus to curves or, conversely, a curve was generated by conatus. Just as curves could be compounded to form new curves, so could one conatus be compounded with another to form a third.

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[39] Ibid., pp. 218-219.

[40] Ibid., pp. 219-220.

Further, unequal conati that could not be compounded had to be subtracted and equal ones that could not be compounded were destroyed.[41] Conati were thus, according to their nature as both motion in a point and cause of motion, subject to given rules in their compounding. Then, any regular curve could be thought of as composed of or generated by two other curves--possibly straight lines--the conati of which were in some fixed relationship. If that relationship could be deduced, then the curve itself could be explained as the result of fundamental elements of motion. This, however, was a strictly mathematical problem, or could be treated as such, so that Leibniz's thought was directed by its own logic and conceptual structure, from considerations of motion to the study of the mathematics of curved lines.

In 1672 Leibniz was in Paris on a diplomatic mission and there began a study of mathematics.[42] Descartes' Geometry gave him some difficulty, but Christian Huygen whom he met in 1672, came to his assistance as his mathematics tutor. In the same year Leibniz read Cavalieri's Geometria indivisibilibus, and also in that year, on a visit to London, became familiar with the work of Isaac Barrow (1630-1677), concerning the problem of finding the tangent to a curve.

Barrows' conception of the nature of a curve coincided with Leibniz's earlier ideas on the relation of conatus and motion--a curve is composed of infinitesimal straight-line segments and, at the same time,

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[41] Ibid., p. 221.

[42] For an account of Leibniz's mathematical training see E. T. Bell, Men of Mathematics (New York: Simon and Schuster, 1957), pp. 117-130. See also Gottfried Wilhelm Leibniz, The Early Mathematical Manuscripts, trans. with notes by J. H. Child (Chicago: Open Court, 1920), pp. 11-15.

is generated by the motions of a point. Tangents to curves were then extensions of the infinitesimal line segments as well as the instantaneous directions of the motion of the moving point which generated the curve. The problem of finding tangents to curves was thus the same problem as finding the relation of the conati of which a curve is composed. Further, the inverse problem--given the tangent to a curve in some functional relationship, to find the curve--was the same thing as finding the curved path a body would follow under the influence of known conati. Barrow stated the reciprocal character of these two problems--Barrow's theorum--in his Lectiones opticae et geometriae of 1670,[43] and provided methods for their solution which differ from Leibniz's differential calculus chiefly in notation.[44]

From these and other authors Leibniz had, by 1675, acquired a thorough knowledge of the current state of mathematics, including the problem of tangents and quadratures.[45] It seems that there was little

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[43]Ettore Carruccio, Mathematics and Logic in History and in Contemporary Thought, trans. Isabel Quigly (Chicago, Ill.: Aldine Publishing Co., 1964), p. 216.

[44]Florian Cajori, A History of Mathematics (2nd ed.; New York: The Macmillan Co., 1931), p. 189. Cajori quotes J. H. Child, The Geometrical Lectures of Isaac Barrow, "Isaac Barrow was the first inventor of the infinitesimal calculus."

[45]The problem of quadratures refers to the finding of the area enclosed by some curved line, or generally integration. The relation between this and the "inverse" problem described above, and their connection with mechanical motion was implicit in a work of Evangelista Torricelli (1608-1647), the De Motu gravium. Torricelli considered the diagrams of space traversed and speed of a moving body as functions of time and pointed out that the ordinates of the space curve are proportional to the areas enclosed by the speed curve, while the ordinates of points on the speed curve are angular coefficients of the tangents of the space-curve. Barrow acknowledged Torricelli and Galileo as

left to do toward the invention of the infinitesimal calculus except to produce a notation and method of solution of the two problems that expressed their intimate relatedness. This task, however, proved to be as difficult as it was important. In a work of 1674, Leibniz had given an indication of the role which he expected the infinitesimal calculus, or Analysis, to fulfill.

> 1. The method of universality instructs us how to find by means of a single operation analytical formulas and general geometric constructions for different subjects or cases each one of which would otherwise need a particular analysis or synthesis. As a result its use may be considered as extending to algebra and analysis and as spreading to all the parts of pure or applied mathematics.
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> 2. Now as all the propositions of applied mathematical sciences may be stripped of their matter by means of a reduction to pure geometry, it will suffice to show its use in geometry. This boils down to two points; namely, first, the reduction of several different cases to a single formula, rule, equation, or construction, and secondly, the reduction of different figures to a certain harmony in order to demonstrate or resolve universally a number of problems or theorems about them . . . . if in time the Geometry of infinites might be rendered a little more susceptible of Analysis so that the problems of quadratures, of centers, and of the dimensions of curves could be solved by means of equations, . . . we should obtain a great advantage from the Harmony of the figures for the purpose of finding their quadrature as well as that of others.[46]

Although the "method of universality," or "characteristic," as Leibniz called, was to be a perfectly general science of which analysis and algebra were only branches, still, analysis would have the task of

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forerunners. Carruccio, p. 216. Leibniz, however, later remembered having received his insight into the problem not directly from Barrow but that it came as an inspiration while reading the Traité des Sinus du quart de cercle of Blaise Pascal (1623-1662). Leibniz, "Letter to Bernoulli, April 1703" Early Mathematical Manuscripts, pp. 15-18.

[46]Gottfried Wilhelm Leibniz, "On the Method of Universality," Leibniz Selections, ed. Philip R. Wiener (New York: Charles Scribner's Sons, 1951).

giving expressions to all the "harmonies" of the physical world. Analysis would be the mathematical framework and structure of all physical theory. It would contain and express all the important relationships that obtain in the phenomenal world. Thus, from the very beginning Leibniz had an idea of his calculus as the logical, relational structure of theory.

The symbols employed in the calculus, and the manner of their interrelationship should be truly representative of the structure of reality. Successful construction of theory would thus involve the choice of "real" characters qualified by their relationships in such a way as to reveal the organization of the world in their formulas.[47] This idea found expression in the notion of the mathematical function. The mathematical function represented a "law" expressing the dependence of one variable on other variables. A variable was a symbolic representation of a continuous series of particular values determined by the relationship expressed in the law, or function, to corresponding values of other variables. Thus the function was a mathematical analogy to Leibniz's later philosophical solution of the problem of the relationship of the individual to the whole. Every equation or functional relationship, $f(x,y,z, \ldots) = 0$ could be solved for any one of the variables. The resulting equation was then its principle, representing its dependence, through all its changes, on the rest of the world, so to speak.[48]

However, Leibniz also recognized that there are certain primitive elements of reality. In a work of the year 1679, entitled "On Universal

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[47] Leroy Loemaker, "Introduction," Philosophical Papers and Letters, p. 36.

[48] Ibid.

Synthesis and Analysis, or the Art of Discovery and Judgment," Leibniz gave expression to this idea in terms of the causal definition of Thomas Hobbes. His interpretation of the causal definition was, however, not exactly the same as that of Hobbes. In Leibniz's hands it yielded a new concept; that of "compossibility."

A nominal definition of a thing, according to Leibniz consisted in the enumeration of elements sufficient to distinguish it from everything else. These elements could be further resolved into primitive elements, which are understood of themselves. All definitions are thus combinations of primitive elements. But, in setting up definitions, it is necessary to establish their possibility--to show that all their elements are mutually compatible (compossible).

The best and easiest way to do this is to define a thing in terms of the elements by which it is generated, by which it is caused.

> But the concept of the circles set up by Euclid, that of a figure described by the motion of a straight line in a plane about a fixed end, affords a real definition, for such a figure is evidently possible. Hence it is useful to have definitions involving the generation of a thing, or if this is impossible, at least its constitution, that is, a method by which the thing appears to be producible or at least possible.[49]

Ultimately, then, all material phenomena were produced by combinations of basic conati linked together by certain relationships expressive of their compossibility. The expression of these relationships was the role that the calculus was to play in the understanding of the world.

Shortly after successfully devising his notation of sums, $\int$, and differences, dx, probably in 1680, Leibniz set down the basic

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[49]Leibniz, Philosophical Papers and Letters, I, 352-354.

concepts of his calculus in a treatise entitled Elementa calculi novi.[50] This treatise, which remained unpublished, was, as the title suggests, devoted to the solution of the problems of tangents and quadratures. The tangent was expressed through the ratio dx/dy and the area of the figure, the quadrature, by $\int y dx$.[51]

The basic character of the infinitesimal differences, dx and dy, was their fixity. The size of dx was constant but not determined, and dy bore a functional relationship to dx. Also both were conceived as "momentaneous increments" of their respective variables,[52] so that they indeed had all the characteristics of the earlier concept of conatus, and the new calculus was therefore intended as an expression of the real structure of phenomena, that is, of mechanics.

At the same time, Leibniz was aware that a mathematical system alone was insufficient for the complete understanding of natural phenomena; it could only provide a relational structure. In his "Introduction on the Value and Method of Natural Science" written between 1682-1684, he stated that

> The operation of a body cannot be understood adequately unless we know what its parts contribute; hence we cannot hope for the explanation of any corporeal phenomenon without taking up the arrangement of its parts. But from this it does not follow that nothing can be understood as true in bodies save what happens

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[50] J. M. Child, ed., Early Mathematical Manuscripts, pp. 135-136. The full title of the treatise is Elementa calculi novi pro differentiis et summis, tangentibus et quadraturis, maximis et minimis, dimensionibus linearum, superficium, solidorum, aliisque communens calculum transcendentibus.

[51] Leibniz, Early Mathematical Manuscripts, p. 138.

[52] Ibid., pp. 137-138.

> materially and mechanically, nor does it follow that only extension is to be found in matter . . . . We must recognize that there are two kinds of distinct attributes, one of which must be sought in mathematics, the other in metaphysics . . . . Metaphysics provides existence, duration, action and passion, force of acting, and end of action, or the perception of the agent.[53]

At this point, several questions arise. Did Leibniz himself construct a theory of mechanics on the basis of his calculus and, if so, was that theory essentially the same as Newton's. Finally, did such a theory have any influence on contemporary thought on mechanics? With regard to the first question, Leibniz did produce at least the beginnings of a theory of mechanics embodying his calculus. This did not take the form of a single unified treatise, perhaps because Leibniz was too much involved in other affairs to devote the necessary time to such an undertaking.

In the winter of 1685-1686 Leibniz wrote his Discourse on Metaphysics.[54] This work contains the background thinking on the determination of the measure of "force" in moving bodies and seems to have arise directly from Leibniz's contact with Huygens.[55] Force, as measured, is an effect--the product of a summation of *conati*--but its correct mathematical description was necessary to any physical theory that would give a causal account of motion. Huygens had, in effect, taken the position that elasticity is an essential property of matter,

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[53]Leibniz, *Philosophical Papers and Letters*, I, 447.

[54]Gottfried Wilhelm Leibniz, *Discourse on Metaphysics*, trans. Peter G. Lucas and Leslie Grint (Manchester, England: Manchester University Press, 1953), p. xiii.

[55]Kurt Huber, *Leibniz* (München: Verlag von R. Oldenbourg, 1951), pp. 204-205.

an idea that coincided with Leibniz'd own convictions. Further Huygens had shown, as had Mariotte,[56] that the conservation of the products of mass and the square of the velocity was a consequence of the elastic nature of matter, and, in the work of both men, this fact implied a logical connection between the various sorts of mechanical phenomena.

Leibniz accordingly turned to Huygens' principle (in its Galilean form) concerning the center of gravity of a system of bodies in motion in order to determine the measure of force.

> I suppose that a body falling from a certain height acquires the force to rise to it again, if its direction so causes it; . . . for example, a pendulum would rise again perfectly to the height from which it descended, if the resistance of the air and some other small obstacles did not diminish by a little its acquired force.[57]

Leibniz then assumed that as much force is required to lift a body of one pound to a height of four fathoms as is necessary to lift a body of four pounds to a height of one fathom.[58] That is, "force" was compounded of weight and distance according to the law of association: $nW \times S = W \times nS$, where W is weight, S is the distance, and n is a number. The force so compounded Leibniz also knew to be proportional to mass times velocity squared, and he went on to say that the "new philosophers" accepted this idea.[59]

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[56]Leibniz knew and corresponded with Edmonde Mariotte. W. H. Barber, Leibniz in France, From Arnauld to Voltaire. A Study in French Reactions to Leibnizianism, 1670-1760. (Oxford: At the Clarendon Press, 1955), p. 7.

[57]Leibniz, Discourse on Metaphysics, p. 29.

[58]Ibid., pp. 29-30. This follows from the principle of static equilibrium. It is significant that Leibniz brought together both dynamic and static ideas of force in his attempt to determine the measure of force.

[59]Ibid.

By "new philosophers," Leibniz was referring to those who had broken away from Cartesianism, those who had recognized the necessity for some metaphysical principle of action in mechanics, something beyond mere extension and motion. These "new philosophers" did not exist in great numbers in the France of the late 17th century due to the immense popularity of Cartesian thought, and so it was necessary that Leibniz's early works on mechanics should take the form of attacks on Cartesian mechanics. This being the case, it was only natural that Leibniz should attack Descartes at the most basic and yet the weakest part of his system, the laws of impact.

Accordingly, Leibniz, in March of 1686, published in the <u>Acta Eruditorum</u> "A Brief Demonstration of a Notable Error of Descartes and Others Concerning a Natural Law, According to Which God is Said Always to Conserve the Same Quantity of Motion; a Law Which They Also Misuse in Mechanics."[60] The "notable error" to which Leibniz referred was the identification of motive force and quantity of motion. In the <u>Discourse</u> Leibniz had shown that the measure of force was as the product of weight and distance, on the assumption that, if there are no external obstacles, a body will rise to the same height from which it has fallen. Force, as measured in this way must clearly be conserved due to the impossibility of a perpetual motion machine.

On the other hand, although a body A weighing one pound and falling through a distance of four yards has the same "force" as a body B

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[60]Gottfried Wilhelm Leibniz, "Brevis demonstratio erroris memorabilis Cartesii et aliorum circa legem naturalem, secundum quam volunt a Deo eandem semper quantitatem motus conservari; qua et in re mechanica abutuntur," <u>Acta Eruditorum</u> (1686), pp. 161-163.

weighing four pounds and falling through a distance of one yard, their momenta are different. Body A would have a momenta only half that of body B.[61] If momentum was not the measure of force, still force must be some function of velocity, and Leibniz easily concluded that it must be proportional to the square of the velocity and that its measure is mv^2,[62] a result that coincided with, and was no doubt suggested by, Huygens' work on elastic collision and pendulums, as well as that of Mariotte.

Leibniz then went on to explain why, in spite of this error, the Cartesian concept of force was satisfactory in the treatment of simple machines. In all such machines, in the equilibrium condition, the magnitudes of the bodies are reciprocally as their "virtual" velocities, their distances from the center of rotation. "It is therefore merely accidental here that the force can be estimated from the quantity of motion."[63]

Thus far Leibniz had only attacked the underlying principle of the Cartesian laws of impact and, as yet, it is not apparent that his objections to them or his own ideas on force have anything to do with a mechanical theory based on the calculus. In fact, the full elaboration of his force concept in terms of indivisibles and the relation of the force concept to the problems of tangents and quadratures did not appear for some time. Nonetheless, Leibniz did apply the concept of continuity, a basic idea of the calculus, to the study of impact at an early date.

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[61] Leibniz, Philosophical Papers and Letters, I, 455-457.

[62] See Max Jammer, Concepts of Force, a Study in the Foundations of Dynamics (New York: Harper Torchbooks, 1962), pp. 163-164.

[63] Leibniz, Philosophical Papers and Letters, I, 457-458.

In July 1687, there appeared in the Nouvelles de la Republique des Lettres a piece entitled "Extrait d'une lettre de M. Leibniz sur un principe general, utile à l'explication des loix de la nature, par la consideration de la sagesse divine; pour servir de replique à la reponse du R. P. Malebranche."[64] The general principle involved was that of continuity, which Leibniz stated in the following manner.

> When the difference between two instances in a given series, or that which is presupposed, can be diminished until it becomes smaller than any given quantity whatever, the corresponding difference in what is sought, or in their results, must of necessity also be diminished or become less than any given quantity whatever.[65]

As applications of this principle Leibniz cited the conic sections and the laws of impact. With regard to the conic sections, Leibniz observed that the ellipse approaches the parabola as one focus is removed to infinity.[66] The same principle applied to impact would imply that the state of rest was really only one of infinitely small velocity, so that there could be no qualitative difference between the properties of bodies at rest and of those in motion, just as there was no qualitative difference between the ellipse and the parabola. In general, the modes of behavior of bodies in motion in various circumstances should shade into one another in a continuous fashion.

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[64]Barber, p. 246.

[65]Leibniz, Philosophical Papers and Letters, I, 539.

[66]The first expression of this application of the mathematical idea of continuity occurred in the Ad vitellionem paralipomena of Johannes Kepler (1571-1630), published in 1604. In this work Kepler demonstrated that the conic sections form a continuous series. See C. Taylor, "The Geometry of Kepler and Newton," Transactions of the Cambridge Philosophical Society, XVIII (1900), 201.

Descartes' failure to see this had been the cause of his failure with the laws of impact. For instance, Descartes' first rule of impact stated that equal colliding bodies with equal speeds would be reflected with the same speeds, while the second rule stated that if the colliding bodies B and C had equal speeds but B was slightly larger, then C would be reflected with its former velocity, but B would continue its motion. There is a great difference between the behavior of the bodies under these two sets of circumstances that does not disappear as C and B are made more and more nearly equal. Thus they indicate a discontinuity in nature which is not, according to Leibniz, permissible.[67] Thus, while this particular idea of continuity is not exactly the same as the concept of continuity associated with causal relationships, still Leibniz's use of it indicates his conviction that mechanics must be structured by the principles of the calculus.

Leibniz was impelled toward further systematization of mechanics in terms of the calculus through Newton's Principia, which reached him in Italy around 1690.[68] This led Leibniz to further elaboration of the force concept. Another impetus in the same general direction came in 1694 from an attack on the Leibnizian calculus by the Dutch geometer Bernard Nieuwentijdt (1654-1718).

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[67]Leibniz, Philosophical Papers and Letters, I, 539-540. These same ideas were also expressed in a letter to Pierre Bayle (1647-1706) printed in July 1687 in the Nouvelles de la Republique des Lettres. Leibniz, Opera philosophica que exstant latina, gallica, germanica omnia, Ed. J. E. Erdman, reproduction of edition of 1840 (Meisenheim: Scientia Aalen, 1959), p. 105.

[68]Loemaker, "Introduction," Philosophical Papers and Letters, I, 58.

Nieuwentijdt argued that Leibniz could not explain how his infinitely small differences differed from absolute zero, or how a sum of these differences made up a finite magnitude (essentially the same objections later raised in England against the calculus of fluxions).[69] Leibniz's answer to these objections ("Reply to Nieuwentijdt") was based on the notion of continuity, but that concept itself underwent at this time a significant change. Up until Nieuwentijdt's attack, the infinitesimal differences, which were equivalent to the conati, were conceived as being of fixed, if indefinite, magnitude; they were velocities in a point. But now, possibly because of Leibniz's recent exposure to the Principia, the infinitesimal differences took on a more dynamic character.

> Of course it is really true that things which are absolutely equal have a difference which is absolutely nothing . . . . Yet a state of transition may be imagined, or one of evanescence, in which indeed there has not yet arisen exact equality or rest or parallelism, but in which it is passing into such a state, that the difference is less than any assignable quantity; also that in this state there will still remain some difference, some velocity, some angle, but in each case one that is infinitely small.[70]

Here the concept of conatus has taken on a more dynamic flavor; it has become something that produces change. Correspondingly the concept of continuity becomes one of a continuous flow rather than of a summation of infinitesimal but static increments. The notion of conatus as producing change was to be quickly assimilated into Leibniz's concept of mechanical force.

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[69]See pp. 93-98 above.

[70]Leibniz, Early Mathematical Manuscripts, pp. 148-149.

Leibniz was not alone in his defense of the calculus. Other defenders of the new method were the Bernoulli brothers, Jacob (1654-1705) and John (1667-1748), the Marquis de l'Hopital (1661-1704), Jacob Hermann (1678-1733), and Pierre Varignon (1654-1722),[71] all of whom were to do significant work in the development of the new mechanics based on the calculus. Christian Huygens also urged Leibniz to the defense of his ideas, as is evident from a letter from Leibniz to Huygens dated June 12, 1694.

> Your exhortation confirms me in the purpose I have of producing a treatise explaining the foundations and applications of the calculus of sums and differences and some related matters. As an appendix I shall add the beautiful insights and discoveries of certain geometricians who have made use of my method if they will so kind as to send them to me. I hope that the Marquis de l'Hopital will do me this favor if you judge it fitting to suggest it to him. The Bernoulli brothers could also do it. If I find something in the works of Newton which Mr. Wallis has inserted in his algebra which will help us get forward, I shall make use of it and give him credit. But I venture to ask that you yourself will favor me with what you judge appropriate, as, for example, your analysis of Mr. Bernoulli's problem by means of this kind of calculus. . . .[72]

From this letter and the "Reply to Nieuwentijdt," it can be seen that Leibniz had not only come a long way toward the basic insight of "Newtonian" mechanics--the idea of force as a cause of change which is fundamentally conceived in terms of infinitesimals--but that his mathematical methods had been accepted and applied by other eminent mathematicians. Since mathematics and physics had been kept in close association by Leibniz himself, it is only understandable that his associates would, as will be evidenced, themselves apply the new mathematical methods to physical problems along the lines suggested by Leibniz.

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[71]Loemaker, Philosophical Papers and Letters, II, 880.

[72]Leibniz, Philosophical Papers and Letters, II, 684-685.

A beginning to a new dynamics was made by Leibniz in two writings of the year 1695. In his "Systeme nouveau de la nature et de la communication des substances, aussi bien que de l'union qu'il y a entre l'ame et le corps," which appeared anonymously in the Journal des Sçavans, Leibniz attempted to reintroduce the notion of substantial forms into philosophy. Matter was to be conceived neither in purely geometrical terms as extension, nor in terms of impenetrable and indivisible material atoms moving in the void. Rather, matter was to be conceived in terms of "atoms" of substance or form.

The fundamental atoms of substance were called "primitive force." This force did not contain only act, but an "original activity" as well. This, along with extension was part of the essence of body.[73] The idea that primitive force is not only act, but also activity, reflects Leibniz's recent insight into the nature of the infinitesimal differences, the conati, of the calculus. Forces are not only velocities in a point, but are also activities producing velocity.

In a first draft of the "Systême nouveau" Leibniz wrote

> By force or power [puissance] I do not mean the power [pouvoir] or mere faculty, which is nothing but a near possibility of acting and which, being as it were dead, never produces an action without being stimulated from without, but I mean something between power to act [pouvoir] and action, something which includes an effort, an actual working [acte], an entelechy, for force passes of itself into action, in so far as nothing hinders it. Wherefore I regard force as constitutive of substance, since it is the source [principe] of action, which is the characteristic of substance.[74]

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[73] Leibniz, "Systême nouveau de la nature et de la communication des substances, aussi bien que de l'union qu'il y a entre l'ame et le corps," Journal des Sçavans, XXIII (1695), pp. 444-454.

[74] Leibniz, The Monadology and Other Philosophical Writings, trans. with introduction and notes by Robert Latta (Oxford: University Press, 1898), p. 300.

This new concept of force served to place dynamics squarely within the framework of the calculus. Force became a form, that is, a mathematical concept, that lay somewhere between "dead force," or weight, and the "living force" of a body in motion, whose measure, as has been shown, was proportional to the square of the velocity of the body. Leibniz developed this idea of force further in a work entitled "Specimen Dynamicum; in Behalf of the Admiration of Laws of Nature Concerning Corporeal Forces, the Discovery of Their Mutual Actions, and Their Reduction to Their Causes," which appeared in the Acta Eruditorum in 1695.[75]

In the "Specimen Dynamicum," the metaphysical status of force, and consequently of the calculus, was made clear. "There is nothing real in motion itself except that momentaneous state which must consist of a force striving toward change. Whatever there is in corporeal nature besides the object of geometry, or extension, must be reduced to this force."[76] Here Leibniz was referring to "primitive force" which was a species of what he termed "active force." Also under the classification of active force was the so-called "derivative force" that was exercised through the limitation of primitive force resulting from the conflict of bodies with each other. Derivative force would thus correspond to that force that can be sensed in a stressed elastic body or in the impact of a moving body.

The classification "active force" suggests the existence of a "passive force," which Leibniz also broke down into the species primitive

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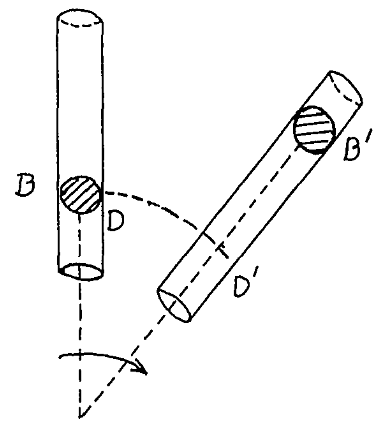
[75]Leibniz, Philosophical Papers and Letters, II, 712.

[76]Ibid.

and derivative. These corresponded to the power of suffering or resisting and constituted a "secondary matter." Bodies thus act by virtue of their form and suffer or resist by virtue of their "matter."[77] The passive, resisting force is equivalent to the Newtonian concept of mass, or <u>quantity</u> <u>of</u> <u>matter</u>.

"Derivative" force was the force connected with local motion, through which all material phenomena could be explained. In the actual explanation of local motion, however, Leibniz did not use this term, but rather those of "dead" and "living force." An example of his use of these terms should serve to clear up their meaning, as well as their relationship to "primitive" force.

Consider a rotating hollow tube containing a ball that is free to move along the length of the tube, as in the following figure. At



the beginning of the rotation the <u>conatus</u> of the radial movement (the instantaneous radially directed velocity) is infinitely small with

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[77]<u>Ibid</u>., pp. 714-715.

respect to the tangential impetus DD'.[78] However, if the centrifugal impulsion arising from rotation is continued for some time, then the ball must attain a

> . . . certain complete centrifugal impetus D'B' comparable to the impetus of rotation DD'. Hence the nisus is obviously twofold, an elementary or infinitely small one, which I call a solicitation, and one formed by the continuation or repetition of these elementary impulsions, that is, the impetus itself.
>
> . . . .
>
> Hence force is also of two kinds: the one elementary which I also call dead force because motion does not yet exist in it, but only a solicitation to motion, such as that of the ball in the tube; . . . the other is ordinary force combined with actual motion, which I call living force. An example of dead force is centrifugal force, and likewise the force of gravity or centripetal force; also the force by which a stretched elastic begins to restore itself.[79]

In this passage Leibniz has stated the fundamental relationship of the new dynamics; the summation, or integral of the elementary impulsions--primitive forces--is equal to the total acquired impetus of the motion. There was however a question remaining. Impetus, or quantity of motion was not the same as living force, which was proportional to the square of the velocity, yet both living force and impetus arose "from an infinite number of impressions of dead force."[80] How then was the transition from dead to living force to be understood?

Leibniz observed that many men had seen a proportionality between dead force, or weight, and the product of mass and velocity.

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[78] An impetus, or quantity of motion has existence only in time. It is a summation, or integral of impulsions over a given time interval. Ibid., p. 715.

[79] Ibid., pp. 716-717.

[80] Ibid., p. 717.

> This happens for a special reason, namely, that when, for example, different heavy bodies fall, the descent itself, or the quantities of space passed through in the descent are, at the very beginning of motion while they remain infinitely small or elementary, proportional to the velocities or to the conatuses of descent. But when some progress has been made and living force has developed, the acquired velocities are no longer proportional to the spaces already passed through in the descent, but only to their elements.[81]

By this Leibniz meant that there appears to be a proportionality between the force of a falling body and its velocity in the first instants of motion because, during that time, velocity and space traversed are proportional. However, as the duration of the fall increases, the velocities of descent are only proportional to the instantaneous elements of the space traversed (ds); while the total force of the motion must be calculated "in terms of the spaces themselves."[82] In modern (Leibnizian) notation, this would be expressed as $\int fds \sim v^2$ of $fds \sim 1/2vdv$.

This relationship can be seen to correspond to, or to contain, the two "measures" of force earlier determined by Leibniz, and it fully describes the transition from dead to living force in terms of the calculus. The relationship is also equivalent to Newton's Second Law,[83] the only difference being that the "evanescent" character of force is conceived in terms of space rather than of time, as is the case with Newton.

There is a certain sense in which Leibniz was more consistent than was Newton. Leibniz stated that force was something absolutely real in created substance, but

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[81] Ibid., pp. 717-718.

[82] Ibid.

[83] The relation, $fds \sim vdv$, can be transformed as follows: $fds \cdot dt/dt \sim ds/dt \cdot dv$ or $fdt \sim dv$ or $f \sim dv/dt$.

> . . . space, time and motion are of the nature of relations and are not true and real per se but only insofar as they involve the divine attributes such as immensity, eternity, and activity or the force of created sbustances.[84]

This is reflected in the relationship between dead and living force, since there appear in it no absolute spaces or times, but only increments, or elements thereof. He could accept the fact that velocity does appear as an absolute in the relationship because it is expressive of force. Newton on the other hand had asserted that space and time are absolute, and yet they do not appear as such in his Laws of Motion.

The relativity of motion led Leibniz to still another important conclusion, namely that, since motion consists in mere relationship, the

> . . . <u>equivalence of hypotheses is not changed by the impact of bodies upon each other</u> and that such rules of motion must be set up that the relative nature of motion is saved, that is, so that phenomena resulting from collision provide no basis for determining where there was rest or determinate absolute motion before the collision.[85]

Since the impact of two bodies is the same no matter which of them is assumed to have true motion, the effects of percussion must be equally distributed in both. Both bodies suffer and act equally. Thus it is possible to derive the effect in one from the action in the same one, or, what amounts to the same thing, bodies move under their own force. Since, further, "only force and the effort arising from it at any moment exist . . . and every effort tends in a straight line, it follows that all motion is in straight lines, or compounded of straight lines."[86] Finally,

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[84] <u>Ibid</u>., p. 728.

[85] <u>Ibid</u>., p. 729.

[86] <u>Ibid</u>., pp. 733-735.

there is no body, however large or small, that has no elasticity and is not permeated by still subtler substances. "There are no <u>elementary bodies</u>, . . . [and] analysis procedes to the infinite."[87]

Leibniz cannot be said to have delivered a fully articulated theory of mechanics to his colleagues and in this respect he fell far short of Newton's achievement. Of greater significance is the simple fact that Leibniz had colleagues with whom he could and did share his most profound insights on the nature of the physical world. As has been shown, these insights both begin and end with the idea of elasticity--the power of continuous action that is substantially present in every body--and include the fundamental insight into the causal relationship between weight and motion. The fact that Leibniz's expression of this relationship, although mathematically equivalent to Newton's, was based on entirely different metaphysical suppositions and dealt with "forces" rather than instantaneous increments of motion should serve to establish the originality of Leibniz's ideas. Furthermore, the form of the relationship used by other writers provides a clear indication as to which of the two men, Newton or Leibniz, had exerted the greatest influence on their thought. It was not until late in the period under consideration here that the mathematical equivalence of the two forms came to have greater significance than their differing metaphysical contents.

Newton and Leibniz were of course not the only sources of influence on French mechanical thought. More so than in England, the influence of Descartes and Rohault had to be overcome before it was possible

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[87]<u>Ibid</u>., p. 731.

to even see a causal relationship, of the kind set up by Newton or Leibniz, between weight and motion. As has been pointed out, the work of Mariotte represented a departure from Rohault and serves to define a trend in French mechanics that emphasized the notions of elasticity and continuity. Therefore, the men associated with it tended to assimilate the work of Leibniz and/or Newton much more easily than would any strict follower of Rohault and Descartes. Consequently, the latter group would be of interest, not because of any significant contributions to the development of theoretical mechanics, but because they would constitute one of those not so rare groups in the history of ideas that seems to prefer to put up with the inadequacies of an old system of ideas rather than to accept new ones.

However, in the France of the period, there were few "purists" of any sort in the field of mechanics. Most of those publishing treatises and articles in the field showed a good deal of eclecticism. Although the Cartesian system had great appeal, still there were areas where its failure was all too obvious.[88] As an example of the conflicting influence

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[88] "Pierre-Silvain Neges (1632-1707) . . . had become an ardent Cartesian under the influence of Rohault (1620-1675), whose Paris lectures did much to popularize Descartes' views, and had devoted his life to the propagation, with a few personal amendments, of the Cartesian system. He had lectured on Cartesianism with great success at Toulouse and elsewhere from 1665 to 1680, returning in that year to continue Rohault's lectures, and subsequently gave himself up to writing in the Cartesian cause. His reply to Leibniz, which appeared in the Journal des Savants in June 1697, was couched in the most indignant terms. He vigorously denied all Leibniz's assertions, maintaining that they were based on a misinterpretation of Descartes, and went on to launch a personal attack on Leibniz. Behind a façade of self-confident contempt, this reveals a good deal of uneasiness about the progress being made by Descartes' opponents, of whom Leibniz is clearly regarded as one of the leaders, and the inability of the Cartesians to check it." Barber, pp. 47-48.

of Rohault and Mariotte, the work of Philippe de La Hire (1640-1718) is of particular interest because La Hire wrote his Traité de Mecanique before the impact of Leibniz's thought was very widely felt.[89]

Since the theory of simple machines constituted the strongest portion of Rohault's mechanics, the greatest similarity between La Hire and Rohault is to be found in that area. The crucial definition of center of gravity as given by La Hire is almost the same as that of Rohault: a point within the body such that, if the body is suspended at that point, all the parts of the body will remain at rest. Also the center of gravity is a point which can be considered to be as heavy as if the weight of the entire body were concentrated there.[90]

However, whereas Rohault had included the idea of equilibrium in his conception of the center of gravity, La Hire explicitly avoided this. For him the basic idea of machines was that if "all things are equal on both sides" of the fixed point there will be equilibrium.[91] In the words "all things" there lies a significant departure from Rohault's treatment, for La Hire introduced into his mechanics the idea of the moment of a heavy body, a concept not found in Rohault. The moment is the "effort" with which a power can act on a body when applied to a machine. This effort, or moment, is composed of the absolute gravity of the power and the "force" with which it acts. "This composition is not

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[89] Philippe de La Hire, Traité de Mecanique ou l'on explique tout ce qui est necessaire dans la Pratique des Arts, et les Proprietes des Corps pesants lesquels ont un plus grand Usage dans la Physique (Paris: De L'imprimerie Royale et se vend chez Jean Anisson, MDCXCV).

[90] Ibid., pp. 8-9.

[91] Ibid., pp. 14-15.

a simple addition of the parts of the absolute gravity with those of its force, these parts being assumed equal, . . . but a compound addition which is a multiplication of the parts of the absolute gravity [of the power] . . . by those of its force."[92]

In the discussion of simple machines, the term "force" means lever arm, or distance from the fixed point, but in the discussion of impact, "force" means velocity. For instance, La Hire stated that the "effort" that a moving body can exert is equal to the product of its absolute gravity and its velocity.[93] Here La Hire has attempted to lay the groundwork for a synthesis of statics and dynamics--the concept of "moment" approaches both the ideas of virtual velocity and of momentum, except that his "effort" of a moving body involves weight rather than mass. In demonstrating the usual theorems concerning the lever, which he regarded as the basic machine, La Hire did not, however, make any specific use of the moment concept. Instead he followed a procedure essentially identical to that of Rohault.[94] Therefore, the relational structure of La Hire's mechanics, in spite of the idea of the effort of a power, or its moment, is one connecting static configurations of weights and lengths to the state of equilibrium. It does not embody any notion of causality.

Just as La Hire's work tends to resemble that of Rohault in the field of statics, it tends to draw on that of Mariotte in the field of impact phenomena. The usual rules of inelastic collision provided the

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[92] Ibid., pp. 10-11.

[93] Ibid., pp. 401-402.

[94] Ibid., pp. 13-23.

basic framework, and the rules for elastic collision were derived from them through the addition of the notion of elasticity. As with Mariotte, elasticity, for La Hire, simply meant that when two elastic bodies have been deformed through impact, at the instant when they have regained their original forms, they will have the same relative velocity as they had before collision. This relative velocity will be divided between the two bodies inversely according to their weights.[95]

Unlike Mariotte, however, La Hire did not make use of the notion of elasticity in attempting to establish the relationship between weight and momentum even though he made the attempt in the context of the same problem--the proposition that, if the "efforts" of two bodies falling on opposite ends of a balance are inversely as their distances from the fulcrum, there will be equilibrium at the moment of impact. La Hire reasoned that, since the same quantity of motion can be produced in an infinite number of combinations, the weights may be adjusted so that they are in the ratio of the momenta. Then their velocities would be equal. Now the same momenta may be maintained by increasing the weights and decreasing the velocity indefinitely. This process may be carried out until the velocity has become vanishingly small, and the weights (although now indefinitely large) may be assumed to be at rest. If the lever arms are in the inverse ratio of the momenta, the balance will now be in equilibrium, by the fundamental theorem of the lever.[96]

Thus, by considering weight and momentum, or static and dynamic "effort," to be the same, La Hire was able to reduce a dynamic situation

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[95] Ibid., p. 390.

[96] Ibid., pp. 401-403.

to a static one. But in so doing he also brought to light the infinite gap between them. Nonetheless, it was possible to solve many problems on this basis of the substitution of weight for momentum: for instance, that of the center or percussion.

In his treatment of the center of percussion of a compound body, La Hire simply assigned to each point of the body a static load which was proportional to the momentum of the point. Then the center of percussion of the body, according to his definition, would be identical with the center of gravity of his construction. This sort of problem caused no difficulties, since it involved no causal action and was, in spite of the motion of the body, essentially a static matter, one involving only geometrical configuration. As long as there is some proportionality between weight and momentum, the substitution of one for the other could be successfully made even though the exact nature of their relationship was not known.[97]

La Hire also attempted to derive laws of motion of falling bodies from his theory of impact, but did not succeed in doing more than recapitulating the work of Galileo, who he cited as its original author. Without the concepts of mass or force, La Hire could do no more than assume the occurrence of successive "blows" exerted on the body in equal time intervals. This sufficed for a loose derivation of the proportionality of distance traversed to the square of the elapsed time, but could lead to no rigorous dynamic relationships.[98]

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[97] Ibid., pp. 404-405.

[98] Ibid., pp. 409-418.

Thus La Hire's mechanics, as contained in the Traité de Mecanique falls far short of the unification of the mechanical disciplines. His failure to grasp the notion of causality led him to identify weight and momentum in the single concept, "moment." With this idea as its central concept, La Hire's theory of mechanics could do no more than loosely hold together the laws and rules produced in various fields. It could explain none of these in a satisfactory manner, and most importantly, it yielded no new insights.

Nonetheless, La Hire's work does serve to illustrate further that there was a drive among French mechaniciens toward the production of a unified, dynamic theory, and that the fulfillment of this drive was absolutely dependent on the acceptance of the calculus as the logical structure of physical theory. Even more illustrative of the truth of these assertions is the work of Pierre Varignon (1654-1722) over the period from 1687 to 1720. Varignon not only attempted his own novel synthesis of mechanics on the basis of a single principle, but eventually came under the influence of Leibniz and the new mathematics.

In 1687 Varignon published a work entitled Projet d'une nouvelle mechanique that proposed to base the theory of mechanics, that is, of machines, on an entirely new principle.[99] Varignon was impressed by the fact that all authors on mechanics reduced the action of all of the simple machines to that of the lever. This he rightly saw as an indication that their basic principles were not broad enough to demonstrate the properties

[99]Pierre Varignon, Projet d'une nouvelle mechanique avec un examen de l'opinion de M. Borelli sur les proprietez des poids suspendus par des cordes (Paris: chez la Veuve d'Edme Martin, Jean Boudot and Estienne Martin, 1687).

of the various machines separately. Whereas other authors had demonstrated the "necessity" of equilibrium, Varignon wished to understand its "nature." That is, he wished to study the "generation" of equilibrium.[100] This intention on the part of Varignon indicates that he had already moved in the direction of causal thinking in the modern, or Hobbesian sense.

In his investigation he first considered a body on an inclined plane and saw that the equilibrium between the sustaining force and the weight follows the proportion of the sine of their mutual angle. He considered also pulleys and levers and found always the same--equilibrium is to be understood from the point of view of the composition of motion.[101] It is significant that Varignon understood equilibrium in terms of the composition of <u>motion</u>. What was the relationship between motion and force? In the single axiom given in the <u>Projet d'une nouvelle mechanique</u>, Varignon stated that the spaces traversed by a body in equal times are in the proportion of the forces moving the bodies.[102] That is, moving force is proportional to velocity, the basic statement of Aristotelian dynamics. Indeed, Varignon "remained consciously faithrul to Aristotelian dynamics."[103]

Varignon's Aristotelianism, along with his search for causal understanding, both tended to make him open to Leibniz's ideas. Leibniz was, after all, endeavoring to restore a typical Aristotelian doctrine--

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[100] <u>Ibid</u>., pp. [ẽijr-ĩv].

[101] <u>Ibid</u>.

[102] <u>Ibid</u>., pp. 1-2.

[103] Rene Dugas, <u>A History of Mechanics</u>, trans. J. R. Maddox (New York: Central Book Co., 1955), p. 255.

that of substantial form--to its rightful place in physics within a causal framework.

Varignon attempted to apply his idea of force and motion to dynamics in his "Regles du Mouvement en général" of 1692.[104] In this work, the main principle of motion was that

> . . . dans toutes sortes de mouvemens, soit qu'ils se fassent en roulant ou en glissant, soit en ligne droit ou en ligne courbe, soit que ces mouvemens soient uniformes ou accelerez ou retardez, dans toutes les proportions et dans toutes les variations imaginables; la somme des forces qui font le mouvement dans tous les instants de sa durée, est toujours proportionelle a la somme des chemins ou des lignes que parcourent tous les points du corps mû.[105]

Here again is the Aristotelian idea that motive force is proportional to velocity. But since Varignon, according to the causal point of view, was attempting to understand motion in its basic, instantaneous elements, force was proportional to distance. Actually, his principle expresses the result of a summation of all the instantaneous elements of the motion. Translated into the symbolism of the calculus, it would read $\int f dt \sim \int m ds$, where f represents force, dt an instant of time, m the quantity of matter in the body, and ds the elementary path lengths.

This relationship, although incorrect from a modern point of view, nonetheless shows that Varignon's thoughts on force and motion had placed him in a position where further progress in the understanding of motion was dependent on the possession of a mathematical tool that would

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[104] Pierre Varignon, "Regles du Mouvement en général," Memoires de l'Academie Royale des Sciences, Tome X, pp. 225-233.

[105] Ibid., p. 226.

allow him to carry out the kind of summation he had indicated. In 1693 Varignon presented two articles to the Paris Academy that represented an attempt to reduce all known laws of motion to his general principle, and which embodied a means of summation of instantaneous values of force.

The first of these articles, entitled "Regles des Mouvemens Accelerez Suivant Toutes les Proportions Imaginables d'Accelerations Ordonnées," appeared in May of 1693.[106] In it Varignon set up a mathematical symbolism embodying his ideas which would allow the desired derivation of known laws from them. The symbols used are as follows:

Body	Mass	Space	Time	Initial Force	Exponent of Abscissa
M	e	f	c	r	
N	g	h	d	s	p

Final Abscissa	Final Velocity
v	x
y	z

Given these quantities, Varignon stated that the velocity increases as the power, p, of the times or the spaces, or more generally as the power, p, of the "abscissas" v and y, which may represent anything one wishes. Then, since in each body the forces at each instant are proportional, to the velocities they produce, and

> ... que (hyp.) les vitesses suivent ici la raison des puissances p des abscissas des grandeurs v et y; si l'on fait $1^p/_{v}p = r/_{rv}p$ et $1^p/_{y}p = {}^s/_{sy}p$, l'on aura rv^p et sy^p pour les plus grandes forces des corps M et N à la fin de leurs mouvemens ou des espaces f et h. Donc les sommes des forces qui se sont successivement trouvees dans chacun des corps M et N pendant les temps C et D ... sont entr'elles comme $\frac{rv^{p+1}}{p+1}$ et $\frac{sy^{p+1}}{p+1}$.[107]

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[106] Pierre Varignon, "Regles des Mouvemens Accelerez suivant toutes les proportions imaginables d'accelerations ordonnées," Memoires de l'Academie Royale des Sciences, Tome X, pp. 339-343.

[107] Ibid., p. 340.

In this passage, Varignon has created an expression for the force of a moving body in such a form as to be capable of "integration" of the simplest kind:

$$\int x^u dx = \frac{x^{u+1}}{u+1}.$$

With this expression for the total force of the motion, Varignon could now go on to the complete expression of his general principle. The total forces of M and N are proportional to ef and gh, the products of mass and distance. Thus follows the relationship

$$\frac{rv^{p+1}}{ef} = \frac{sy^{p+1}}{gh}, \quad \text{or} \quad \underline{efsy^{p+1} = ghrv^{p+1}} .$$

Varignon gave this relationship in yet another form: $xesy^p = zgrv^p$, which results from the above if x and z, the final velocities, are expressed as f/v and h/y respectively. In this case, v and y are apparently taken as times. Then, in order to find in these equalities the particular laws governing each possible assumption concerning acceleration, it is only necessary to sbustitute in place of y^{p+1}, v^{p+1} the parallel powers of the variable that is supposed to regulate the acceleration.[108]

Varignon's equation was not, in itself, a particularly valuable addition to theoretical knowledge of the phenomenon of motion. In a methodological sense, however, it is of some significance. He had recognized that the elementary force moving a body had to be an integrable function

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[108] Ibid., p. 341. Varignon applied his equation to the problem of falling bodies in "Application de la Regle Generale des Mouvemens accelerez à toutes les hypothesespossibles d'accelerations ordonnées dans la chute des corps," Memoires de l'Academie Royale des Sciences, Tome X (1693), pp. 354-360. The article appeared in June, and, in it, Varignon managed to derive all of Galileo's results with regard to falling bodies from his own general equation.

of some variable of the motion. It was natural to choose velocity, since according to his Aristotelian bias, motive force was proportional to velocity. Varignon further was able to employ the ideas of cause and effect; the sum of all the elementary causes, or forces, must be equal to the total effect, which he saw as the total distance traversed by every part of the body. Also, the causal relationship between force and distance traversed was contained in a specific operation of the calculus.

During the next few years Varignon published nothing in the field of mechanics. It seems as though he was in the process of assimilating the new mathematics of Leibniz, John Bernoulli and l'Hôpital. L'Hôpital was in contact with both Leibniz and Bernoulli in 1693,[109] and in 1696 he published the first textbook on the calculus, the Analyse des infiniment petits pour l'intelligence des lignes courbes, which was based in part, on earlier work by John Bernoulli.[110] Thus there was available to Varignon a complete treatise on the Leibnizian calculus at a time when his own thoughts on mechanics had led him to the need of a fuller understanding of the new mathematics.

But whatever the exact source of Varignon's knowledge of the calculus may have been, in January of the year 1700 he was fully able to handle problems connected with motion in terms of the calculus. At that time he published an article in the Memoires de l'Academie Royale des

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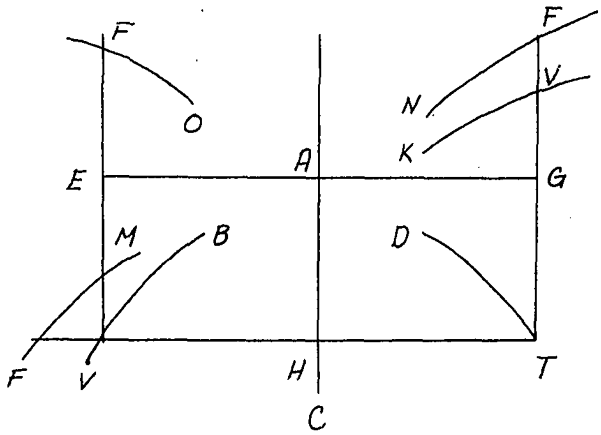
[109] See Eric Temple Bell, The Development of Mathematics (New York: McGraw Hill, 1945), p. 285, and Florian Cajori, A History of Mathematical Notations, Vol. II, Notations Mainly in Higher Mathematics (Chicago, Ill.: Open Court Publishing Co., 1929), p. 185.

[110] Carl Boyer, The History of the Calculus and Its Conceptual Development (New York: Dover Publications Inc., 1949), p. 238.

Science that dealt with finding the force, velocity, distance and time of variable rectilinear motion.[111]

The problem was stated in terms of the following figure.



TD, VB and FM represent, by their common abscissa, AH, the space travelled by a body along AC. The elapsed time in this motion is expressed by the ordinate HT of the curve TD, and VH and VG rxpress the velocity in each point H. The ordinates FH, FG and FE represent "de force vers C(je l'appelleray dorénavant Force Centrale à cause de sa tendence au point C comme centre)."[112] TD, Varignon called the "time curve," VB and VK were "curves of velocity," and the other three were called "curves of force."

The abscissa AH was set equal to x, the time HT = AG = t, the velocities at H, HU = AE = VG = v, and the central forces corresponding to HF = EF = GF = y. Then dx represented the space traversed each instant at velocity v, of which dv was the increment; ddx was the space traversed by virtue of the increment of velocity in the instant, and dt

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[111]Pierre Varignon, "Maniere generale de determiner les Forces, les Vîtesses, les Espaces et les Temps une seule de ces quatre choses etant donnée dans toutes sortes de mouvemens rectilignes variés à discretion," Memoires de l'Academie Royale des Sciences, 1700, pp. 22-27.

[112]Ibid., p. 22.

represents the instant. Velocity, according to Varignon, consisted only in a "rapport" of the space traversed by a uniform motion to the employed time, or $v = dx/dt$. Then $dv = ddx/dt$, where dt was taken as constant. These relations constituted a "first rule" for the solution of the problem.[113]

Then Varignon went on to state, as a "second rule," a relationship equivalent to Newton's Second Law of Motion.

> De plus les espaces parcourus par un corps mû d'une force constante et continuellement appliqué, telle qu'on conçoit d'ordinaire la pesanteur, etant en raison composée de cette force et des quarrés des temps employés à les parcourir; l'on aura aussi $ddx = ydt^2$, ou $y = \frac{ddx}{dt^2} = \frac{dv}{dt}$.[114]

Varignon did not give the source of this relationship, and it, at first glance, seems to conform neither to his own earlier ideas, nor to the work of Newton or Leibniz. The end result, $y = dv/dt$ looks more like Newton's force-motion relation than Leibniz's, but the derivation is not at all the same. Where Newton thought of force in terms of an instantaneous increment of motion and as a flowing quantity, Varignon has put force and the square of the time interval together to give the increment of distance due to the change in velocity. Since this combination bears no relationship to the basic ideas of Newton or Leibniz, it is likely that it is in some way derivable from Varignon's earlier general principle of motion. In fact, one would expect that Varignon might have attempted simply to cast that principle into the language of the new mathematics.

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[113] Ibid., p. 23.

[114] Ibid.

The general principle, as originally set up, already contained the summation of the elementary forces as the cause of the total displacement of the body. But, through the adoption of the new methods and notation, Varignon was impelled to relate the quantity ddx, the space traversed by virtue of the velocity increment, to force, rather than the total space traversed. Translated into the new symbolism, and with time substituted for the "abscissa," v, and p set = 1, Varignon's original principle relating the force at any time to its effect becomes $yt \sim dx$ (y, the centripetal force, replaces r, the earlier "initial" force, and dx is the element of distance traversed in an instant). The desired relationship between ddx and force can now be reached in a single step by differentiation, $ddx/dt \sim ydt$ or $ddx \sim ydt^2$.[115]

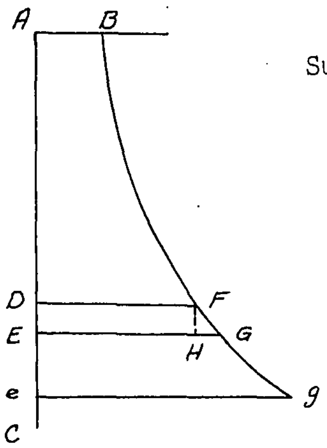
Thus, in a sense, Varignon never had to change his mind at all about the relationship of force and motion. He was able to keep the Aristotelian conception that motive force is proportional to velocity. Through the adoption of the calculus, however, the formerly unitary concept of motive force took on a two-fold character. The first attempt at the formulation of dynamical laws of motion in 1693, with its rudimentary use of the calculus, had brought with it the rather obscure "initial" force alongside of total force, the two being related through an exponential function of the time. With the full application of the calculus, the "initial" force emerged as the instantaneous rate of change of momentum on the one hand, and as centripetal or weight-like force on the other, while the "total force" was still related to the observable motion of the

[115]This is not intended as a rigorous deduction. It is the author's opinion that Varignon's deduction was not more rigorous at this point.

body. That is, the two ideas of force came to be related through differentiation, or integration with respect to time, and this sort of relationship, as has been pointed out, was the essence of the idea of causality.

One of the most interesting aspects of Varignon's work to this point is that his basic insights are clearly not too dependent on the logical rigor of his thought processes. A too scrupulous attention to infinitesimal differences would have prevented him from ever making the transition from his original idea to that which seemed correct in the light of the work of Newton and Leibniz. In fact, in the same article, Varignon showed that his two rules were equivalent to the principles of both Newton and Leibniz.

Varignon remarked that his two rules yield the same results as Proposition XXXIX of Book I of Newton's <u>Principia</u>.[116]



> Suppose the body E to fall from any place A in the right line ADEC; and from its place E imagine a perpendicular EG always erected proportional to the centripetal force in that place tending toward the center C; and let BFG be a curved line, the locus of the point G. And in the beginning of the motion suppose EG to coincide with the perpendicular AB; and the velocity of the body in any place E will be as a right line whose square is equal to the curvilinear area ABGE.[117]

If the ordinate EG, the centripetal force, is represented by y and the

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[116] <u>Ibid</u>., p. 27.

[117] Isaac Newton, <u>Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World</u>. Trans. Andrew Motte, 1729. Translation revision and historical appendix by Florian Cajori (Berkeley, California: University of California Press, 1947), p. 125.

abscissa AC by x, the area ABGE is represented by $\int ydx$. Then, according to Newton's proposition, the instantaneous velocity, v, is such that $v^2 \sim \int ydx$ or $v \sim \sqrt{\int ydx}$. Varignon's two rules give $dt = dx/v$ and $dt = dv/y$. Consequently, $dx/v = dv/y$ or $ydx = vdv$, which gives $\int ydx = \frac{1}{2}vv$ or $v = \sqrt{2\int ydx}$, which is Newton's result, but achieved with a minimum of effort. Also, the relationship achieved through the elimination of dt between Varignon's first and second rules is the basic relationship of Leibniz's mechanics.[118]

This performance on Varignon's part points up a number of significant things. First of all he seems to have felt that he had accomplished his results in a manner that was his own and significantly different from that of either Newton or Leibniz. Secondly, he was thoroughly familiar with the Principia and not only understood it in a manner far superior to the "Newtonians" of his day, but regarded it as a work of great authority. One might then say that Varignon's work was done under the influence of Newton, except that he made use of the Leibnizian form of the calculus. However, Varignon's results, as has been shown, represent a growth out of earlier, essentially different ideas about force and motion, that were shaped into their final form through adaptation to the calculus. Newton only represented substantiation of already achieved results.

Those results were extended to non-rectilinear motion in another memoire submitted to the Paris Academy in 1700,[119] in which Varignon

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[118] Varignon, Memoires de l'Academie Royale des Sciences, 1700, p. 27.

[119] Pierre Varignon, "Du Mouvement en General. Par toutes sortes

produced "une formule tres-simple des Forces Centrales, tant centrifuges que centripetes, lesquells sont le principal fondement de l'excelent ouvrage de M. Newton, De Phil. natur princ. Math."[120]

In this memoire, all things are the same as in the preceding one except that the body moves along a curved line rather than a straight one toward the center of force. Again there are two basic variables, x, the distance to the center of force, and the time, t. The velocity is again represented by v, and the absolute central force by y. However, Varignon had to introduce the distance traversed along the curve, s, and its element ds, the distance traversed at velocity, v, in the time interval dt. Then, dds was the distance along the curve traversed by virtue of the increment dv.

As the first rule, Varignon then defined v as ds/dt, from which vollows dv = dds/dt. The force along the curve, the component of y that is directed along the tangent to the curve, is $y\frac{dx}{ds}$, so that, according to the same argument used before, $dds = y\frac{dx}{ds} \times dt^2$ or $y = \frac{ds \times dds}{dxdt^2} = \frac{ds}{dt} \times \frac{dds}{dt} \times \frac{1}{dx} = \frac{vdv}{dx}$, or $\underline{ydx = vdv}$.

Again Varignon has made use of his own form of the fundamental dynamical relationship and arrived at a differential equation almost identical to Leibniz's equation for the transition from dead to living force. It is perhaps of significance that this more general rule for finding central forces was written in the Leibnizian form when it could

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de Courbes; et des Forces Centrales, tant centrifuges qui centripites, necessaires aux corps qui les décrivent." Memoires de l'Academie Royale des Sciences, 1700, pp. 83-101.

[120] Ibid., p. 83.

just as easily have been written $y\frac{dx}{ds} = \frac{dv}{dt}$.[121] In any case Varignon again referred to both Newton and Leibniz as sources tending to confirm his analysis of central forces and their effects;

> . . . encore la seconde de ces Regles suffira-t-elle pour cela, ainsi qu'on le va voir dans les exemples suivants par la conformite de mes solutions avec celles de M. Newton dans ceux qui nous seront communs. Quant à l'exemple de M. Leibniz, etant d'Astronomie, ce sera pour une autre fois.[122]

Thus far, all of Varignon's work in dynamics has concerned itself with the problem of bodies moving under the influence of central forces. Insofar, his work is specialized and cannot be considered as presenting a unified theory of mechanics. In particular, the force concept, while exhibiting the necessary internal structure, that is, the weight-motion relationship, has not been generalized beyond the action of gravity, or weight. However, in 1707, Varignon submitted a memoir to the Paris Academy in which the concept of force was considerably broadened.[123]

Varignon noted that Newton, in the Principia, Leibniz in the Actes de Leipsik of 1689, Huygens in his discourse on the cause of gravity, and Wallis in his Oeuvres Mathematiques had all treated the resistance of media to the motion of bodies passing through them. The results that all of these men had achieved on the basis of their hypotheses as to the nature of the resistance, as well as whatever results might follow from

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[121]Since, however, both y and v are given as functions of x through the "curves of force" and the "curves of velocity," the integrations indicated in the equation $ydx = vdv$ can be carried out immediately.

[122]Ibid., p. 88.

[123]Pierre Varignon, "Des Mouvemens faits dans des milieux qui leur resistent en raison quelconque," Memoires de l'Academie Royale des Sciences, 1707, pp. 382-398.

any other hypothesis, Varignon felt could be expressed in a single proposition of his devising.

For the expression and proof of this all-inclusive proposition, Varignon required a number of definitions and lemmas. The "instantaneous resistance" he defined as being proportional to the instantaneous decrements of velocity. These decrements he termed "successively continuous" if they occurred without interruption and were all of the same order of magnitude. In general, any quality which the moving body would have without the resistance of the medium was to be described as "primitive."[124]

With these basic ideas established, Varignon went on to the statement of two lemmas, the first being that the instantaneous and successively continuous resistances of any medium to any finite movement of finite duration are infinitely small with respect to the "persevering" force which produces the finite motion of the mobile. This statement serves to place the decrements of velocity resulting from the resistance in the same order of magnitude as the increments of velocity that would result from the action of the force in the absence of the medium. Its demonstration consists in the observation that if the instantaneous resistances were finite, their sum over any time interval would be indefinitely large and the assumed motion would be impossible.[125]

The second lemma states that the sum of the instantaneous velocities of a body moved in any manner is always proportional to the

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[124] _Ibid._, pp. 384-385.

[125] _Ibid._, pp. 385-387.

length of the path which they have caused it to traverse, one after the other by instants. The demonstration proceeds in terms of the calculus; if e is the distance traversed, t the time, and u the instantaneous velocity, then $u = de/dt$, $udt = de$, $\int udt = e$.[126]

The proposition itself was stated in the form of a problem, namely to find an expression for the resistance of a medium such that the sum of the velocity lost by the mobile due to the resistance and the velocity remaining to it would be, at every point, the same as its "primitive" velocity.[127] While this proposition looks innocent enough, there is a good deal more to it than at first meets the eye. Varignon was here attempting to set up a perfectly general differential equation for motion in resistant media that would be valid for any hypothesis concerning the nature of the resistance. The only condition imposed was that the sum of instantaneous lost and retained velocities be equal to the instantaneous "primitive" velocity at every instant of the motion.

The method of solution is somewhat complex and will be largely omitted here except for those parts that bear on the generalization of the concept of force. In general, the solution proceeds on the basis of curves of "primitive" velocity, "lost" velocity or resistance, "remaining" velocity, and resisting force, all plotted against time as their common abscissa. Thus, as in his earlier work, Varignon's aim in this problem is to elaborate the relationship between the ordinates of these curves.

The basic relationship among these curves is that the ordinate of the force curve, z, is always in a constant ratio to the instantaneous

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[126] _Ibid._, p. 386.

[127] _Ibid._

increment of the resistance, dr, which in turn is proportional, by the first definition, to the instantaneous decrement of velocity. Their constant ratio is equal to dt/a, where both dt and a are constant. Now, the increment of resistance, dr, is equal to the instantaneous increment of "primitive" velocity, dv, minus the change in the "remaining" velocity, du, or $dr = dv-du$. Thus results the differential equation $\frac{dv-du}{z} = \frac{dt}{a}$ which is the equation for the curve of resistance, z and v being given. The result is then $v-u = \frac{1}{a}\int zdt$.[128]

Z represents a cause of a change in motion that can be any function of the velocity, or of anything else for that matter. It is the hypothesis as to the nature of the resistance. Further, if dv is assumed to result from the action of some central force, f, then it may be replaced by fdt, so that the differential equation becomes $du = (f-\frac{z}{a})dt$. The quantity $f-\frac{z}{a}$ is then a "resultant" force, and the relationship between "force" and motion has been broadened to the point that it applies to any dynamical situation.

With this step, Varignon gave evidence of an almost complete understanding and acceptance of the theory of mechanics usually described as Newtonian. Not only has he shown that his methods are equivalent to the Newtonian system insofar as the treatment of central forces is concerned, but also he has taken the further step of generalization of the force notion to the point where it is a mathematical entity, a function, that represents a cause of a change in motion. The main things that could be considered as lacking in Varignon's mechanics are the connection

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[128] *Ibid.*, pp. 386-387.

of statics and dynamics through the principle of virtual velocities and the connection of dynamics and the laws of impact through an analysis of elasticity as a force with some functional dependence on factors in the collision of bodies. Both of these necessary additions, as will be shown in the next chapter, were to be supplied by John Bernoulli, who therefore can be said to complete the assimilation of "Newtonian" mechanics on the continent.

In this review of the transition period of French mechanics, from Rohault to Varignon, the influence of Leibniz, principally through his form of the calculus, has been apparent. Under the impact of the ideas of causality and continuity as expressed in the calculus, the Cartesian view of the physical world rapidly gave ground before the new methods and the metaphysical content attached to them. However, in Varignon there appears a deviation from Leibniz's metaphysical ideas that is perhaps a vestige of the Cartesian outlook. Particularly in his treatise on motion in resistant media, Varignon used the basic dynamic relationship in its Newtonian form. This implies that he considered momentum, mv, to be the "force" of a body in motion rather than mv^2, and that the action of force in time was somehow more basic than its action through space. These ideas are more compatible with the thought of Descartes than with the corresponding Leibnizian forms.

Thus there appears here something that might be regarded as a tendency to assimilate the insights opened up by Leibniz and the calculus into a Cartesian framework. The culmination of this tendency was to appear in a treatise by John Bernoulli which unsuccessfully attempted to reinstate the Cartesian vortices on the basis of the new dynamics.

Even among those who were most indebted to Leibniz there persisted a predilection for Cartesian metaphysics which, in its final demise, tended to obscure Leibniz's role in the development of mechanical thought and to deposit all the laurels on Newton's godly brow.

CHAPTER IV

FRENCH AND ENGLISH MECHANICS IN CONFLICT

The central figure in the further development of mechanics in France up to the end of the period under consideration was John Bernoulli. The closeness of his relationship with Leibniz has already been pointed out and is evidenced by the fact that their published correspondence runs to two volumes.[1] However, it is not the specific character of their correspondence but the fact of their long professional contact, along with Bernoulli's published writings in the field of mechanics, that is of significance for this study. Bernoulli's work stands by itself and represents not a recapitulation of any Leibnizian mechanical system, but rather an application of the Leibnizian method of analysis. That method, while applied to the solution of certain *specific* problems by Bernoulli and others, tended to draw all of its areas of application into a single theoretical structure.

This process of the unification of mechanics was carried a long way by the work of Varignon up to 1707, especially in the generalization of the force concept. Varignon, however, with his almost exclusive concern for the analysis of motion under the influence of central forces,

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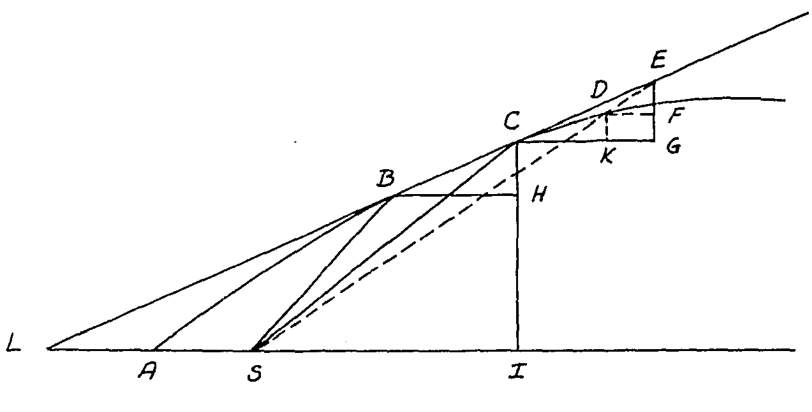
[1] *Gul. Leibnitii et Johannis Bernoulli Commercium Philosophicum et Mathematicum*, (Lausan. et Genev.: 1745), 2 volumes.

had departed from one of the major themes of French mechanical thought up to that time, namely, the property of elasticity in the context of the collision of elastic bodies. As has been indicated earlier, Bernoulli was to take up this question again, but only after the elapse of a considerable span of time. His early work in the field of dynamics dealt with the same problems that had been treated in Varignon's memoirs of 1700 and 1707. With regard to the problem of the dynamics of bodies moving under the impulsion of central forces, the expression of Bernoulli's thought came as a reply to a letter from Jacob Hermann (1678-1733) that was published in 1710 in the Memoires of the Paris Academy.[2]

Hermann's work is heavily mathematical, making use of integral and differential calculus to second order derivatives. This mathematical apparatus was brought to bear on the "inverse" problem of centripetal forces; that is, if an expression for the force is known, to find the trajectory of the motion. While Varignon's work on central forces dealt with this problem, Hermann's method of attack was considerably different and much more detailed.



[2]Jacob Hermann, "Extrait d'une lettre de M. Hermann à M. Bernoulli de Padoüe le 12 Juillet 1710," Memoires de l'Academie Royale des Sciences, 1710, pp. 519-520.

In the above figure, let SI = x and IC = y. Then SC = $\sqrt{x^2 + y^2}$, BH or CG = dx, CH or EG = dy, KG or DF = -ddx, and EF = -ddy. Hermann first showed that ydx - xdy is equal to twice the areas of the triangles BSC or CSD which are equal and remain constant throughout the motion. The similar triangles EDF and CSI yield the expression

$$ED = \frac{-ddx\sqrt{x^2 + y^2}}{x},$$

which, taken in a constant time interval, represents the deviation of the mobile from the position it would attain through motion at constant velocity. This distance Hermann identified as resulting from the action of the centripetal force, which was assumed to vary as

$$\frac{1}{x^2 + y^2}, \quad \text{or as} \quad \frac{(ydx - xdy)^2}{x^2 + y^2}$$

since the numerator is a constant of the motion.

The two expressions were then equated to produce a differential equation of the second order,

$$-addx = \frac{x(ydx - xdy)^2}{(x^2 + y^2)\sqrt{x^2 + y^2}},$$

where "a" is a constant of proportionality. The integration of the above equation yielded an expression for the trajectory, $a \frac{\pm cx}{b} = \sqrt{x^2 + y^2}$, which Hermann identified as an equation for three of the conic sections: a parabola if b = c, an ellipse if b > c, and an hyperbola is b < c.[3]

The key point of this mathematical _tour de force_ is the identification of the distance ED, traversed in an instant of time, with the action of the centripetal force, and in spite of the suppression of

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[3]_Ibid_.

the time factor, the solution appears to be quite Newtonian in character; the distance ED represents an instantaneous acceleration.

Bernoulli's reaction to Hermann's work was, first of all, critical. He remarked that Hermann had only found a particular solution to a problem where the result was already known. Also, if the force law were any other than $1/r^2$ the method of solution would be impossible, since the variables were so mixed up. And finally, Hermann had not shown that no orbits other than the three conic sections were possible.[4] In both Hermann's work and Bernoulli's criticism of it there appears an enormous degree of both mathematical and physical sophistication compared to the work that was done prior to the publication of Leibniz's calculus. Bernoulli's demand that a solution to the "inverse" problem should preclude the possibility of the existence of orbits other than those specifically indicated that satisfy the conditions of the problems, is truly remarkable. Not only does this demand indicate that Bernoulli had a profound understanding of the calculus, but also that he saw the calculus as the structure of physical reality. The notion of proving that a solution to a problem is absolutely unique is a much stronger one than simply finding one solution that works. Bernoulli's demand also seems to be an expression of Leibniz's notion of "compossibility;" those, and only those, orbits may actually exist as physical realities whose elements are mutually compatible, that is, whose elements conform to the basic differential equations generating the motion. It should then be possible

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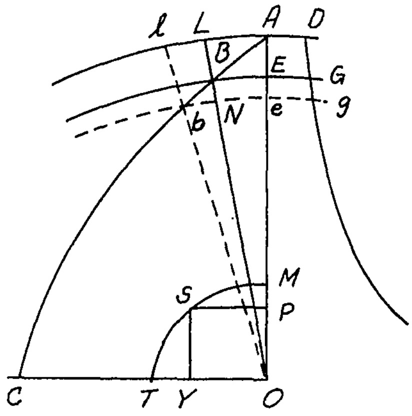
[4]John Bernoulli, "Extrait de la Réponse de M. Bernoulli à M. Hermann datée de Basle le 7 Octobre, 1710," Memoires de l'Academie Royale des Sciences, 1710, pp. 521-523.

to show mathematically that only a limited group of curves have this characteristic and to identify each member of that group.

Following his criticism of Hermann, Bernoulli ventured to present his own solution to the problem, one that would be perfectly general as to the force hypothesis, would not deliver an equation in which the unknowns were mixed, and which would be free from "differentio-differentielles."[5]

Preliminary to his solution of the problem, Bernoulli proved a lemma that is also to be found in Newton's Principia (Proposition XL of Book I), which is of some particular significance because Bernoulli remarked of Newton's proof, in comparison with his own, "mais elle y trop embarrassée: la voici plus simplement."[6] The lemma states that if two bodies whose masses are proportional to their weights begin their descent from the same point A, with equal velocities and equal forces



[5]Ibid., p. 523.

[6]Ibid. Abraham de Moivre (1667-1754) published an article entitled "Observations on Mr. John Bernoulli's Remarks on the Inverse Problem of Centripetal Forces in the Memoires of the Paris Academy for the Year 1710; With a New Solution of the Same Problem," Philosophical Transactions, N° 340 (1713), pp. 91-95, De Moivre's solution makes only scant use of the calculus.

toward the same point O, the one following a straight line AO toward O and the other moving along the curve ABC, then at all equal distances from the point of attraction, O, the two bodies will always have equal velocities.[7]

Bernoulli's proof of the lemma is based on two propositions. First the force acting along the curve, in the direction of the arc Bb, is to the force acting along Ee as Ee is to Bb. Secondly, the elementary times necessary to traverse Bb and Ee are in the ratio Bb to Ee. Then, since the increments of velocity are proportional to the acting forces and the elementary times, the velocity increments along ABC will be as Ee x Bb and those along the vertical as Bb x Ee. The same thing being true for any elements equidistant from O, it follows that the velocities at all points are the same.

As a corollary to the lemma, Bernoulli derived an expression for the velocity as a function of the distance from the center. In the above figure, DGg represents the velocity, v, along AO or ABC, and OE is the distance, x, from the force center. The force directed toward O is ϕ which is given as a function of x according to some law. Then, since the elementary time is equal to dx/v, and since the elementary time multiplied by the force gives the momentaneous augmentation or diminution of velocity, dv, with which the body approaches or departs from O, $\phi dx/v = -dv$, or $\phi dx = -vdv$. This yields the integral $\int \phi dx = ab - vv$, where ab is a constant quantity. Therefore $v = \sqrt{ab - \int \phi dx}$, a result

[7]Bernoulli, Memoires de l'Academie Royale des Sciences, 1710, p. 523.

which is substantially the same as Varignon's earlier conclusion, except for the constant of integration.

From this point, Bernoulli was able to find an expression for the trajectory, ABC, assuming that the above integration is possible. Let OA = a, AL = Z, Ll = dz, and Nb = xdz/a. Then, the time required to pass through Bb will be proportional to Nb x BO = xxdz/a. Since distance traversed is equal to the velocity multiplied by the elapsed time, Bb = xxdz/a x $\sqrt{ab - \int \phi dx}$. But Bb is also equal to

$$\sqrt{dx^2 + \frac{xxdz^2}{a^2}}\ ,$$

so that, equating these expressions, there arises a differential equation for the trajectory in terms of Z and x

$$dz = \frac{aacdx}{\sqrt{abx^4 - x^4 \int \phi dx - aaccxx}}.$$

This equation expresses the "nature" of the trajectory; every integral of it is a physical possibility.[8]

There are a number of things with regard to Bernoulli's work on the inverse problem that are of significance. Perhaps the most obvious of these is that his work constitutes an explicit challenge to Newton and his followers. Not only did he claim that his methods were simpler, and therefore, from a mathematician's point of view, more correct than Newton's; but he also exhibited, to advantage, the power of the Leibnizian calculus to handle physical problems. Considering that, just at this time, the dispute over the authorship of the calculus was just getting under way,

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[8]*Ibid.*, pp. 524-526.

it is easy to see that Bernoulli's successful use of its Leibnizian form could only add fuel to the fire.

With regard to the fundamental dynamic relationship, Bernoulli seems to represent a sort of compromise position between Newton and Leibniz. He starts from the position that force, multiplied by the elementary time, gives the instantaneous increment of velocity, but then substitutes the expression dx/v for the elementary time, which yields the Leibnizian dynamic relationship, $\phi dx = vdv$. Such a substitution is impossible within the framework of the calculus of fluxions. There all of the infinitesimal quantities are envisioned in relation to a "flow" taking place in absolute time. There is no symbol representing the elementary time itself, nor any way of representing an increment or decrement of a quantity as independent of time; there is no dx, but only $\dot{x}$. Thus, from a strictly Newtonian point of view, Bernoulli's work looks very Leibnizian, in spite of the fact that he began his calculations from the idea of force acting through time rather than through space. Aside from that, the algebraic juggling of elementary quantities must have appeared highly suspect to the English Newtonians--a sort of mathematical slight-of-hand employed by alien mathematico-metaphysicians.

In 1711 Bernoulli published another memoir that posed a challenge to the Newtonians. This memoir dealt with the problem of the motion of bodies in resistant media, as had Varignon's memoir of 1707.[9] The problem

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[9]John Bernoulli, "Extrait d'une Lettre de M. Bernoulli, écrite de Basle le 10. Janvier 1711 touchant la maniere de trouver les forces centrales dans des milieux resistans en raisons composée de leurs densites et des puissances quelconques des vitesses du mobile," Memoires de l'Academie Royale des Sciences, 1711, pp. 47-56.

was stated by Bernoulli in the following manner; find the central force required for a body to describe a given curve in a medium in which the density varies according to a given law, and which resists the body in the ratio of the density and any power of the velocity.[10]

The solution begins by resolving the forces acting on the body into components along the tangent to the path and perpendicular to it. A differential equation for the component perpendicular to the path is then found through the use of a lemma stating that the time, t, required to traverse a space, s, by a body moving under the influence of a constant force, p, is expressed by $\sqrt{2s/p}$.[11] Next the tangential component of the central force is calculated, and to this is added (or subtracted, depending on whether the body was ascending or descending) the resistance of the media. The resulting quantity, multiplied by $dt = ds/v$ is then set equal to the velocity increment, dv.[12]

In this solution, Varignon's generalization of the force concept has been given full play and even extended to a case of curvilinear motion. Bernoulli advanced still further beyond Varignon's work in performing the integration necessary to arrive at an expression for the force in terms of the given equation of the trajectory and law of the resistance of the medium. Also, as in his memoir of 1710, Bernoulli found occasion to indulge in criticism of Newton.

On this occasion his criticism was not that Newton's methods in this instance were unnecessarily complicated, but that they yielded absurd

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[10] Ibid., p. 47.

[11] Ibid. The demonstration of the lemma uses the familiar Leibnizian relation $pds = vdv$.

[12] Ibid., pp. 48-49.

results, a contention which he proceeded to demonstrate in a number of examples.[13] Such criticism must have clearly demonstrated to all those who were aware of it that the Leibnizian mathematical physicists, led by Bernoulli, were indeed threatening to eclipse the great English light.

The classic expression of Newtonian concern over the Leibnizian menace is to be found in the correspondence between Leibniz and Samuel Clarke.[14] Originating in the controversy over priority of invention of the calculus, the correspondence eventually gave expression to a dispute of much greater breadth and significance. Since, as has been shown, there was a distinct metaphysical background and content peculiar to each of the two forms of the calculus, the controversy was bound to bring these differences to light. The basic metaphysical differences between Leibniz and Newton all hinged on their respective ideas as to the nature of matter, a question which had, by this time, achieved a particular form in the context of the phenomenon of collision. In this way the Leibnizian-Newtonian dispute was to serve to rekindle interest in the theory of impact.

In his correspondence with Clarke, Leibniz attacked the metaphysical support of Newton's Laws of Motion, the concepts of absolute space and time. Once these were destroyed, the characterization of

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[13] Ibid., pp. 49-53. Newton's error was pointed out to him by Nicolas Bernoulli (1687-1759), a nephew of John, in 1712. Newton's acknowledgement of the correction was to propose John Bernoulli for membership in the Royal Society. Florian Cajori, "An Historical and Explanatory Appendix," Sir Isaac Newton's Principles of Natural Philosophy and His System of the World, p. 657.

[14] The Leibniz Clarke Correspondence together with Extracts from Newton's Principia and Opticks. Edited with introduction and notes by H. G. Alexander (New York: Philosophical Library Inc., 1956).

quantity of motion as the force of a moving body would fall as well, for Leibniz, with Newton, regarded force as something absolutely real. This being so, force could not be represented by motion, if motion were merely relative, as must follow from the relativity of space and time.

Leibniz argued, in his third letter of the correspondence, that, if space were something in itself, and absolutely uniform, then one point of space would not differ from any other point. Thus there would be no reason why God should have placed objects in space in one given manner rather than in another contrary way, for instance by changing East into West. If, however, space is nothing but the order of the objects, then those two states would not be different from one another, and the seeming arbitrariness of God's decision would disappear. The difference between two such states is "therefore only to be found in our chimerical supposition of the reality of space in itself."[15] Essentially the same argument was used against absolute time.

Another, to Leibniz, objectional result of the doctrines of absolute space, time, and motion was the consequent variability of the quantity of active force. In the same letter Leibniz also attacked this idea. God would have to be directly involved in any such process resulting in a change of motion, and a loss of something that directly represents God's action would imply an imperfection in His creation, which is impossible.[16]

Clarke replied to this in the same vein, saying that he did not consider the diminishing of active force to be a disorder at all and

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[15] Ibid., p. 26.

[16] Ibid., p. 29.

therefore not an imperfection in God's creation.[17] Such a reply was not only unanswerable but also unsatisfactory and, in his fourth letter Leibniz simply reasserted the constancy of the quantity of active force. Leibniz's obstinancy led Clarke, in his fourth reply, to bring in the example of the collision of inelastic bodies of equal force. In this case, both bodies, as is well known from the rules of impact, lose their motion, and thus the quantity of force is, in fact, diminished, no matter how you calculate it.[18]

Clarke, by bringing in this idea, had opened the way to a discussion of the nature of matter itself because, in this way, he forced Leibniz to reach beyond the known rules of impact for a defense of his position. Leibniz stated, in fifth letter, that when two soft inelastic bodies collide, they do, as wholes, lose some of their force. However, in that case, the parts receive it, "being shaken by the force of the concourse." The forces are thus not destroyed, but merely redistributed, and, while Leibniz agreed that the quantity of motion does not then remain the same, he still maintained the difference between it and force.[19]

In his fifth reply, Clarke admitted the possibility of a redistribution of force resulting from the collision of soft bodies but insisted that the question at issue was really concerned with the collision of perfectly hard inelastic bodies.[20] Clarke was in a sense correct

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[17] Ibid., p. 34.

[18] Ibid., p. 52.

[19] Ibid., pp. 87-88.

[20] Ibid., p. 111.

in insisting that their differences depended on the question of the collision of perfectly hard bodies. If perfectly hard, inelastic bodies did exist as the basic constituents of matter, and if the laws of inelastic collision were correct, then motion, or force, could be lost. Furthermore, space and time would then of necessity be absolute, the medium of action of absolute and irreducible force. Then the Newtonian dynamic relationship, the Second Law of Motion, would represent physical reality and, consequently the force of a moving body would be proportional to its velocity, or mv.

Leibniz, however, could not admit the existence of hard, inelastic bodies. Such an admission would have destroyed the entire structure of his thought, which was erected upon the basic idea of the innate activity of matter. That activity, as has been pointed out, was essentially activity in space, that is, within the simultaneous ordering of bodies, and its effect was proportional to the square of the produced velocity. Thus the question over the nature of matter led directly to the question as to the correct measure of the force of bodies. Both questions were closely related to the phenomena of impact, and so the rivalry between Newtonian and Leibnizian became focused on that one apparently crucial issue. Leibniz's death in 1716, the same year as his fifth letter to Clarke, seemed to produce a lull in the argument that had just achieved distinct form. There were perhaps also reasons of a totally different nature for the abatement of the controversy.

Louis XIV had died in 1715 and the government of France had passed into the hands of Philip, Duke of Orleans, who was regent during the minority of Louis XV, from 1715 to 1723. Orleans was a notorious and unpopular figure, and in his efforts to retain power he sought alliance with England. George I (1714-1727), who at that time occupied the English throne, was also an unpopular leader, troubled by the constant threat of a Jacobite rising (see pp. 128-129). Like the regent in France, George I felt insecure; so that both in England and France the dynastic situation contributed to a desire for European stabilization and harmony. It is impossible to state with certainty that the dynastic situation had any effect on the impending quarrel between English and French mechanicians, or, more accurately, between the mechanical philosophers of the English Royal Society and those of the Paris Academy. Both of these groups, however, existed under royal patronage and it is possible that they might have reflected their royal patron's desire for mutual accord by simply allowing a thorny issue to rest for a time.

It was not, in any case, until 1723, the year of the Duke of Orleans' death, that the question of the impact of hard bodies came into prominence again. In that year John Bernoulli wrote a treatise in answer to the prize question of the Paris Academy for 1724. The question was, "What are the laws according to which a perfectly hard body, put in motion, moves another body of the same nature, whether it be at rest or in motion, which it encounters either in the void or the plenum."[21]

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[21]Johann Bernoulli, Opera Omnia (Lausanne et Genevae: Sumptibus Marci-Michaelis Bousquet et Sociorum, MDCCXLII), III, 8. See also John Bernoulli, "Discourse sur les Loix de la Communication du Mouvement, contenant la Solution de la premiere Question proposée par MM. de l'Academie Royale des Sciences pour l'Année 1724," Recueil des pieces qui ont remporte les Prix. Fondez dans l'Academie Royale des Sciences par M. Rouille de Meslay, Conseiller ou Parlement: depuis l'Année 1720 jusqu'en 1728. Avec quelques Pieces qui ont concouru aux mêmes Prix (Paris: Claude Jombert, MDCCXXVIII), p. 5. Each section of the volume appears

The way in which the question was posed presents something of an enigma in the light of the Leibniz-Clarke correspondence. It seems that the phrasing of the question, where "perfectly hard" meant inflexible, biased the competition in favor of the Newtonian position. It is also a possibility, however, that the form of the question reflects a Cartesian bias. In any case, the question did call forth, from John Bernoulli, a highly significant treatise which, if it did not win the prize, represented another step in the unification of mechanical theory and a triumph for the Leibnizian doctrines of matter and force.

Bernoulli's first task in the "Discourse" was to dispose of the concept of hardness that constituted the basic principle of the Newtonian and Cartesian views of matter. He stated that such a view is a chimera

> ... qui repugne à cette loy generale que la nature observe constamment dans toutes ses operations; je parle de cet ordre immuable et perpetuel, etabli depuis la creation de l'Univers, qu'on peut appeller LOY DE CONTINUITE en vertu de laquelle tout ce qui s'execute, s'execute par des degres infiniment petits. Il semble que le bon sens dicte qu'aucun changement ne peut se faire par sault, ... rien ne peut passer d'une extremité a l'autre, sans passer par tous les degrez du milieu.[22]

Here Bernoulli had, as Leibniz before him, presented the principle of continuity as the underlying principle of all creation; the fundamental principle of the calculus was also that of physics. Continuity provided the logical framework for the understanding of events, because, without it, there could be nothing but arbitrary acts of creation and destruction.[23]

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to have been published separately as indicated by separate title page and pagination. Bernoulli's treatise was then first published in 1724, the prize year.

[22] Bernoulli, Recueil, p. 5.

[23] Ibid.

However, with regard to the nature of matter itself, Bernoulli exhibited distinctly Cartesian ideas.

Matter was to be considered as being by nature perfectly fluid so that while none of its parts had any necessary mutual cohesion, they could amass themselves into elementary molecules, from which would be formed sensible bodies of different qualities. The various qualities of bodies--liquidity, softness, hardness, etc.--resulted from different figures and motions of the elementary molecules and from the particles passing through their interstices, which held them either separated as fluids or compressed them more or less strongly. Bodies formed by the compressing action were, according to Bernoulli, called "hard" in proportion to the resistance made by the parts of the body to any force tending to separate them.[24]

The proper definition of hardness was then to be constructed in terms of force, or resistance to force. By Bernoulli's definition, a body is perfectly hard when, upon any change in the arrangement of its parts, a very quick and elastic "strength" restores them to their initial situation in an imperceptible time. The initial displacement, in accordance with the question at issue, was assumed to be occasioned by impact of another body.[25]

In this way Bernoulli linked up the idea of hardness with that of elasticity. Elasticity itself was still the property by which all the parts of a body regain their initial states following a displacement.

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[24] Ibid., p. 7.

[25] Ibid., p. 9.

Hardness was derived from this through the addition of a time factor; the degree of hardness was inversely proportional to the time required for restoration. Since a "perfectly hard" body was defined as one in which the restoration time was infinitesimal, even this extreme instance was brought within the framework of the principle of continuity. Consequently, the impact of bodies, even of perfectly hard ones, was now a problem to be understood in terms of the calculus.

Bernoulli's treatment starts from the axiom that bodies moving in the void will retain their velocity and direction in the same straight line unless some interference occurs. Then follows the "proposition" that a hard body, striking directly against a perfectly elastic spring that is immovably fixed at one end, must rebound along the same direction and with the same speed. The equality of speed and direction before and after collision was seen by Bernoulli as a consequence of the law of equality of action and reaction. "This proposition is clear and its truth springs to the eye if one gives the least bit of attention to the nature of action and reaction, which are always equal."[26]

The nature of action and reaction had proved to be one of the most obscure aspects of Newtonian mechanics. Not only had Newton used the terms in several senses, but the equality of action and reaction, the Third Law of Motion, had been used as the basic principle by which Newton drew together into a single dynamic structure the formerly separate sciences of impact phenomena and machines. Newton's own followers had consequently had great difficulties with the notions of action and

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[26] Ibid., pp. 11-12.

reaction; and so Bernoulli's understanding of those terms should shed further light on the origins of his thought as well as on its degree of maturity.

The nature of action and reaction, as seen by Bernoulli in the context of the above proposition, was as follows. In the first instant after contact, the spring is forced to contract a little, and consequently takes a little of the velocity from the striking body. This process continues, through a series of infinitely small diminutions of the force of the moving body, until the spring and the body are at rest, at which point the process is reversed. The same elements of force are returned to the body in the inverse order, so that at the end of the process the body has the same speed and line of direction as before impact.[27]

Here action and reaction appear at two different levels. At one level, they appear as total processes occurring successively in time: the compression followed by the extension of the spring, and the loss of motion of the body followed by its recovery. At the other level, the infinitesimal and simultaneous level, action and reaction appear as a diminution of the force of the moving body causing and caused by a contraction of the spring.

The mathematization of these ideas was carried out by Bernoulli in terms of the following situation. ABMN is a cylinder closed at AB and open at MN. ABDE is filled with compressed air retained by the moveable diphragm DE (which is kept from moving toward MN by a stop), and the ball G moves initially toward the diaphragm with speed GE.

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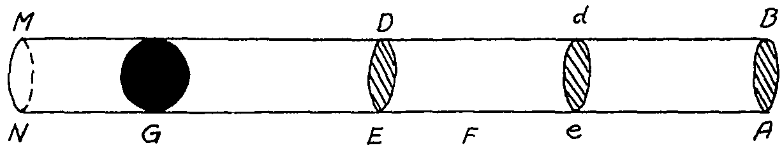
[27] _Ibid_.



Once the ball has struck the diaphragm, its speed will begin to diminish by degrees, while the density of the enclosed air will increase in proportion to the movement of the diaphragm until a point is reached where the velocity is fully destroyed. Then, once the motion has been stopped, the air will begin to accelerate the diaphragm and ball toward the rear at a rate always equal to the retardation suffered by the ball. Eventually, at DE, the ball will regain its initial speed.

Now let x equal AF, the distance of the diaphragm from the base, and v the instantaneous speed. If the resisting force is assumed proportional to the density, it is then proportional to $1/x$, from whence it follows that the element of velocity, dv, will be proportional to dx/xv, or $vdv = dx/x$.[28] It then follows by integration that v^2 is proportional to $\ln x$, or $v \sim \pm \sqrt{\ln x}$. Thus the speed in either direction, either by compression or distension of the spring, is the same for any given value of x, and the proposition is proven.

In this proof Bernoulli made use of the Leibnizian form of the fundamental dynamic relationship.[29] The way in which he applied the equality of action and reaction to the situation indicates, of itself, that his conception of the dynamic relationship was framed in terms of force rather than in terms of instantaneous changes in velocity. The

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[28] Ibid., pp. 12-15.

[29] See pp. 189-92 above.

action of the spring involved a force acting through an infinitesimal distance to produce a change in the force of the striking body.

In spite of the fact that Bernoulli's verbal description of the problem has a strictly Leibnizian character, in the mathematical formulation he made use of the more Newtonian dynamic relationship that he had also used in earlier treatises. However, from the substitution of dx/v for dt, the Leibnizian form emerges immediately and, as has been pointed out, even this substitution was not possible in a strictly Newtonian framework.

In order to apply the results of his analysis of elastic action to the collision of bodies, Bernoulli introduced another idea, the principle of virtual velocities. This principle had constituted a major theme in French mechanical thought, not only as a principle of equilibrium, but as a unifying principle of all mechanical disciplines. The title of the third chapter of Bernoulli's Discourse indicates that he too thought of it in this fashion: "Ce qui c'est que la vîtesse virtuelle. Principe de l'equilibre appliqué à la production du mouvement, par l'entremise d'un ressort entre deux corps en repos."[30]

Virtual velocities were defined by Bernoulli as

> ... celles que deux ou plusieurs forces mises en equilibre acquierent quand on leur imprime un petit mouvement; ou si ces forces sont deja en mouvement, la vitesse virtuelle est l'élement

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[30] Bernoulli, Recueil, p. 15. In a letter to Varignon of 1717 Bernoulli had described the principle of virtual velocities in detail. There the product of force and virtual velocity was termed energy and "in all equilibrium of any forces, in whatever way they may be applied and in whatever direction they may act . . . the sum of the positive energies will be equal to the sum of the negative energies taken positively." Quoted by René Dugas, A History of Mechanics, trans. J. R. Maddox (New York: Central Book Company, Inc., 1955), pp. 231-233.

> de vitesse, que chaque corps gagne ou perd, d'une vitesse deja acquises, dans un tems infiniment petits, suivant sa direction.[31]

This definition of virtual velocity made it possible for Bernoulli to link together the ideas of static equilibrium and the equality of action and reaction. Action, the product of a force and a distance, had already been applied on the scale of the infinitesimal so that, on that scale, the distance could also be considered a velocity, a "virtual" velocity. The other factor in action, the force, Bernoulli identified with the Leibnizian term, "force morte," as opposed to "force vive."

> La _force vive_ est celle qui réside dans un corps, lorsqu'il est dans un mouvement uniforme; et la _force morte_, celle qui reçoit un corps sans mouvement lorsqu'il est sollicité et pressé de se mouvoir, ou à se mouvoir plus ou moins vite, lorsque ce corps est deja en mouvement.[32]

With this complex of ideas Bernoulli was in a position to derive the fundamental theorem of elastic collision from the basic laws of dyanamics and statics, thus accomplishing the unification of these theories and the explanation, or reduction, of the theory of impact. From the principle of virtual velocities, Bernoulli drew the conclusion that two agents are in equilibrium when their "absolute" forces--_forces mortes_--are in reciprocal proportion to their virtual velocities.[33] Then, in order to extend this idea of equilibrium to the dynamic situation, he introduced the idea of an inertial force, or resistance, corresponding to the increment of velocity, i.e., the virtual velocity. Thus every

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[31] _Ibid_., p. 19.

[32] _Ibid_.

[33] _Ibid_., p. 20.

accelerating or decelerating body represented an equilibrium state between a _force_ _morte_ and an inertial force.

This being the case, if the same _force_ _morte_ acts on bodies of different masses, A and B, their respective inertial forces, in spite of their difference in mass, must be the same. If the inertial force, the force of resistance of a body to change, is assumed to be proportional to its mass, then the virtual velocities of bodies A and B must be in the inverse ratio of their masses.

Bernoulli presented these ideas in terms of a situation wherein the two bodies, A and B, are initially at rest, with a stressed spring between them. The spring, which represents the elasticity of the bodies, in expanding must exert an equal "effort" in both directions. Then,

> Il est visible que chacun de ces corps oposera aux mouvement du ressort par son inertie une resistance proportionelle à sa masse. Il faut donc, en vertu de l'hypothese prise de la mechanique, que les deux efforts opposez du ressort etant égaux, la force de l'inertie qui est en A, soit à la force de l'inertie qui est en B; ou que la masse A soit à la masse B in raison reciproque de ce que la vîtesse virtuelle du corps B est a la vîtesse virtuelle du corps A; et comme la chose continuë toujours pendant que le ressort en se dilatant accelere la vîtesse de ces corps, il est clair que leurs accelerations sont continuellement en raisons reciproques des masses A et B, ce qui forme une raison constant.[34]

It then follows that the velocities acquired by both bodies through the complete dilation of the spring must be in the same ratio as the virtual velocities at every point. Therefore they are in the reciprocal relation of their masses, which was to be proven and, from this point, the proof of the fundamental theorem of elastic collision follows easily.

In this treatment of elastic collision, Bernoulli exhibited a complete command of the conceptual apparatus of "Newtonian" mechanics,

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[34] _Ibid._

even to the point of recognizing the inertial force of accelerating bodies. He brought together in a consistent and usable synthesis the fundamental laws of the three main mechanical sciences, elaborating their relationships in terms of the calculus. Thus the ideas of continuity, causality, elasticity, virtual velocity, and generally the idea of process had opened the way to a unified mechanical theory that, while it was essentially the same as Newton's mechanics, had its own development, one which was largely independent of any positive influence from Newton or the Newtonians.

The failure of the Newtonians to grasp the new machanics was due largely to their identification of force as both weight and momentum and, to this point, the nature of force, Bernoulli devoted a great part of his treatise. The fifth chapter is entitled "De la force vive des corps qui sont en mouvement," and, as one might imagine, it constitutes a continuation of the now long dormant controversy that had first flared up in the Leibniz-Clarke correspondence. In his discussion of <u>force vive</u>, Bernoulli wrote that

> ... comme on a été long-tems dans la persuasion que la quantité de-mouvement, ou le produit de la masse d'un corps par sa vîtesse, étoit la mesure de la force de ce corps, on a crû faussement qu'il étoit necessaire qu'il y eut toujours un égal quantité de mouvement dans l'Universe.
>
> L'origine de cette erreur, ainsi que je l'ai deja insinué, vient de ce qu'on a confondu la nature des forces mortes, avec celle des forces vives; car voyant que le principe fondamentale de la statique, exige que dans l'equilibre des puissances, les momens soient en raison composée, des forces absolues, et de leurs vîtesses virtuelles. On a étendu mal à propos ce principe plus loin qu'il ne falloit, en l'appliquant aussi aux forces des corps qui ont des vîtesses actuelles.[35]

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[35] <u>Ibid</u>., p. 35.

Having pointed out the source of the error with regard to the measure of the force of moving bodies, Bernoulli went on to state that Leibniz was the first to realize that the true measure of the force of a body in motion was the product of its mass and the square of its velocity. Leibniz's adversaries had objected to his idea on the ground that he had not taken account of the time during which the motion was carried out and Leibniz had responded to their objections,

> ... mais il ne gagna rien sur des esprits prévenus en faveur du sentiment commun et erroné, que la force des corps en mouvement étoit égale à la quantité de leur mouvement ... Ce fut en vain qu'il fit voir à ses adversaires, que si l'opinion qu'ils soutennoient avoit lieu, on pouvoit executer un mouvement perpetuel purement mechanique, ce qui, selon M. de Leibnitz, étoit absolument impossible; ces adversaires aimerent mieux admettre la possibilité d'un mouvement perpetuel artificiel, que d'abandoner une opinion reçuë depuis long-tems, pour embrasser une nouvelle qu'ils regardoient comme une espece d'heresie en matiere de Physique.[36]

Thus, in Bernoulli's view Leibniz's ideas were subject to a conscious, and yet irrational, rejection on the grounds that they represented a sort of heresy. The supporters of the traditional idea of force clearly preferred to accept absurdities that happened to be logical consequences of their doctrines rather than to alter those doctrines. Bernoulli also cited Samuel Clarke as one who had attempted "à tourner en ridicule le sentiment de ce grand homme sur l'estime de la force vive, non sans une surprise extrême de la part de ceux qui reconnoissent la verité de ce sentiment."[37]

Nonetheless, Bernoulli was not, himself, totally convinced by Leibniz's arguments. They had however given him occasion to think; "ce

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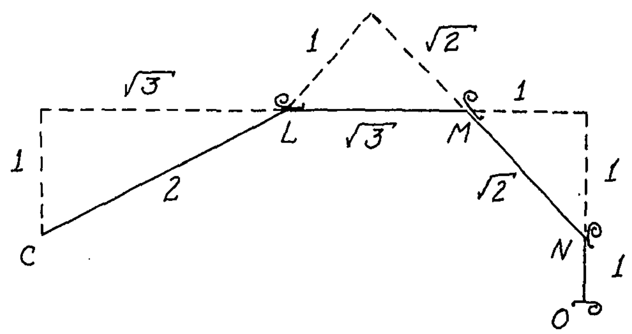
[36] Ibid., p. 37.

[37] Ibid.

n'est qu'après une longue et serieuse meditation que je trouvai enfin le moyen de me convaincre moi-même, par des démonstrations directes, et au-dessus de toute exception."[38]

The direct and unexceptionable methods to which Bernoulli referred had to do with the action of springs; springs provided the most ready means to the study of elastic action and, hence, "sur la production et la force du mouvement."[39] The reasoning that Bernoulli had employed started from the realization that springs of various lengths may exert the same "force morte" when compressed and yet impart very different velocities to identical bodies in fully distending themselves. Therefore, the total force, which corresponds to the force vive cannot be the same as force morte. On this basis it was possible to prove that the force vives of two equal balls impelled by different springs are as the squares of the resulting velocities.[40]

As a further proof that the force of a moving body is proportional to the square of its velocity. Bernoulli posed the following situation.



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[38] Ibid., p. 38.

[39] Ibid., p. 39.

[40] Ibid., pp. 43-45. The proof itself makes use of Bernoulli's usual form of the fundamental dynamic relationship $f(dx/v) = dv$, which he refers to as a "loi connue de l'acceleration."

A ball C begins its motion with a velocity of two degrees. At points L, M, N, and O it strikes springs which each absorb one degree of velocity, as indicated in the diagram. Thus the ball, with its initial velocity of two has bent four springs, each of which absorbs one degree of velocity.

> Puis donc les effets totaux sont entre eux, comme les forces qui ont produit ces effets, il faut que la force vive du corps C, mû avec deux degres de vîtesses, soit quatre fois plus grande que la force vive du même corps mû avec une degrée de vîtesse.[41]

This "thought experiment" is of interest because it implies that the determination of the measure of force as mv^2 is a direct result of the law of composition of velocities, a law which was beyond question. It was therefore necessary for Bernoulli's opposition to show that the experiment could also be interpreted in such a fashion that it would uphold the traditional idea of force.

Such an attempt was made by a John Eames (died 1744). Eames, in 1726, wrote an article for the Philosophical Transactions entitled "Remarks upon a Supposed Demonstration that the Moving Forces of the Same Body Are Not as the Velocities, but as the Squares of the Velocities."[42] Eames did not bother to mention the source of this "supposed demonstration," which seems to indicate a certain amount of hostility toward its author. Perhaps Eames would have been more magnanimous if his own arguments had been a little sounder.

The attack on Bernoulli's interpretation of the experiment centered on the fact that, in drawing his final conclusion as to the force

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[41] Ibid., pp. 51-53. Also, the total force absorbed by all the springs can be easily seen to be the sum of all the products vdv.

[42] Philosophical Transactions, XXXIV (1726-1727), pp. 188-191.

of bodies, Bernoulli had compared bodies with dissimilar motions; one body had suffered three oblique and one direct impact, and the other body, with one degree of velocity, had struck only one spring directly. A proper comparison must involve balls with similar motions.

In order to achieve such a comparison, Eames supposed that the same ball has an initial velocity of one degree and that the springs absorb one half of a degree rather than one. Then all things proceed as in the original experiment except that the total effect is four times one half, or two. Comparing these two trials, Eames "found" that reducing the velocity by one half also reduced the effect by one half, and therefore that the force was proportional to the velocity.[43]

It is hard to say that either Eames' or Bernoulli's interpretation of this experiment is correct to the absolute exclusion of the other. The two interpretations seem to deal with entirely different aspects of the phenomenon in question and are not really incompatible. Bernoulli's interpretation deals with the internal causal relation between force and velocity in a single process, and Eames' deals with a comparison of two similar processes. One can, however, scarcely escape the impression that Eames, like Pemberton in the earlier mentioned attack on Poleni's experiment, was simply trying to get the "right answer," and stubbornly refusing to admit that Bernoulli's thoughts were of any significance at all.

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[43] Ibid., pp. 190-191.

[44] Colin Maclaurin, Piece qui a remporté le prix de l'Academie Royale des Sciences, proposé pour l'année mil sept cens vingt quatre selon la fondation faite par M. Rouille de Meslay, ancien conseiller au Parlement de Paris (Paris: Claude Jombert, MDCCXXIV). Bernoulli's

When one considers the state of mechanical knowledge in England at the time it is not surprising that Eames' arguments bear no relationship to any particular physical or mathematical principal, but depend on a demand whose relevance is not at all clear. What does strike one as surprising is that such arguments were taken seriously not only in England but on the continent as well. This is evidenced by the fact that the essay that won the prize of 1724, for which Bernoulli's treatise was in competition, was of a character similar to Eame's work.

The prize essay of 1724 was written by Colin Maclaurin (1698-1746) and presented essentially the same view of collision phenomena as had Wallis and many others after him.[44] Maclaurin's approach to the problem reflected the influence of Newton only in that, like his English contemporaries, he stated Newton's Laws as a preface to calculations that had little, if anything to do with them.[45] The entire treatment of the collision of perfectly hard bodies, or of any bodies, was derived from the single proposition that the common velocities of the bodies after impact is the quotient of the algebraic sum of their momenta before impact and the sum of the masses.[46] Hard bodies were simply assumed to be inelastic; matter was by nature passive and inert.

Aside from its rather perfunctory and unoriginal treatment of the prize question, Maclaurin's treatise offers some polemic against the

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treatise seems to have been published at the same time as this one, and both are included in the single volume dated 1728. Each treatise has separate pagination.

[45] Ibid., p. 5.

[46] Ibid., pp. 16-17.

Leibnizian position with regard to the force of bodies in motion. Considering the amount of space devoted to this question by both Bernoulli and Maclaurin, it appears that the prize competition was used by both men as a platform upon which to debate the question of force.

Maclaurin's first attack on the Leibnizian idea of force was framed in terms of an hypothetical situation. A man on land throws an object with a certain effort, imparting to it a certain speed, v_2. A man on a boat moving relative to the man on shore with a velocity v_1 throws an identical object with the same effort. Now if one calculates the force in the Leibnizian fashion, a certain discrepancy arises.[47] If the force that the object has by virtue of the motion of the boat with respect to the land is proportional to v_1^2, and the force imparted to it by the man on a boat is proportional to v_2^2, then the total force is proportional to $v_1^2 + v_2^2$. However, of one takes the total velocity of the object with respect to the land as $v_1 + v_2$, then the force should be proportional to $(v_1 + v_2)^2$, which is not equal to $v_1^2 + v_2^2$. Whichever way you make the calculation, something is wrong; if the calculation of force is based upon the total velocity--force $\sim (v_1^2 + 2v_1v_2 + v_2^2)$--then the effort of the man in the boat appears to produce a greater effect than the same effort of the man ashore.[48] If, however, force is assumed proportional to simple velocity, no such difficulty arises.

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[47] Ibid., pp. 7-8.

[48] Ibid. This argument could have been answered in Bernoulli's terms if the "effort" is represented as $\int f dx = \int v dv$. If the calculation for the man on the boat is carried out with respect to the shore, then $v = v_1 + v_2$, $dv = dv_2$, $x = x_1 + v_2 t$, and $dx = dx_1 + t dv_2 + v_2 dt$. Substituting these values for dx and v yields an expression, which when integrated contains the factor $v_1 v_2$ on each side of the equation. Thus

Maclaurin raised yet another obje ion to the Leibnizian idea of force, this time in terms of elastic collision. Assume that two identical elastic bodies, A and B, moving in the same direction, with velocities of ten and five degrees respectively, come into collision. If they were inelastic, they would have a common velocity of seven and one half units, but being perfectly elastic they will change velocities so that A will have five units and B ten. That is, the spring will separate the bodies with five degrees of velocity, two and one half to each body, or, by Leibnizian reckoning 25/4 degrees of force. Without the action of the elasticity, A had 225/4 degrees of force, from which the spring withdrew 25/4 leaving 200/4 or 50 degrees of force. However A, after collision, is known to have only five degrees of velocity or twenty-five degrees of force. Again the difficulties are removed if the forces are measured by simple velocity.[49]

Both of these paradoxes really hinge on the same question, the question of transferring forces from a moving frame of reference to a stationary one. The complexities of such a transfer had not yet become apparent to the mechanicians of the early 18th century and so Maclaurin's objections must have appeared formidable indeed, just as formidable as Bernoulli's experiments and arguments supporting the Leignizian point of view. From the standpoint of the assimilation of Newtonian mechanics, however, Maclaurin's work shows little advance over his English

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the effort of the man on the boat, as seen from the shore, contains that factor, and he does appear to achieve a greater effect from the same effort than does the stationary man.

[49]Ibid., p. 9.

contemporaries, and is distinctly inferior to that of Bernoulli. In Maclaurin's arguments there is almost no attempt to understand the transmission of force as a process, no attempt to understand the relation between the tension of the spring and the production of motion, and, in short, no attempt at a unified theory of dynamics. Thus the really significant aspect of Bernoulli's treatise tended to become obscured by the apparently insoluble question as to the measure of the force of moving bodies.

If Bernoulli's insights into a general theory of mechanics, as contained in his Discours, were temporarily overlooked because of the force controversy, they were nonetheless on record and available to a new generation of mechanical philosophers. Pierre Louis Moreau de Maupertuis (1698-1759) was one of this new group, which could be characterized by a common and unquestioned assumption that mechanics was nothing more nor less than applied analysis.

There is no need to examine in detail the early work of Maupertuis even though it is of interest in its own right. What is of significance here is merely that, in those treatises that were written before 1735, the influence of Leibniz and Bernoulli is plainly visible. For instance, in 1730 Maupertuis published a memoire entitled "La Courbe Descensus AEquabilis dans un milieu resistant comme une puissance quelconque de la vitesse."[50]

The curve Descensus AEquabilis was the curve that Leibniz had proposed to illustrate the fact that the time during which a force acts

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[50] Memoires de l'Academie Royale des Sciences, 1730, pp. 233-242.

is an arbitrary factor, and that it is the distance through which it acts that is directly related to the effect produced. Maupertuis' treatment of the problem is of a character virtually identical with that of Bernoulli's memoire of 1711. To find the accelerative force, he constructed differential expressions for gravitational force and force of resistance. "Or la force accélératrice, multipliée par le temps, donne la difference de la vîtesse." Like Bernoulli, however, Maupertuis substituted dx/v for the time dt in order to arrive at a first order differential equation for the motion.

In a memoire of 1732, Maupertuis again revealed the Leibnizian influence on his thought. This memoire, "Sur les Loix de l'Attraction,"[51] attempted to use the principle of continuity in order to understand why the force of attraction between massy particles is reciprocally as the square of the distance rather than as some other power of the distance. Presumably God could have set it up in any fashion.

First Maupertuis set up a differential equation for the attraction exerted by a surface of revolution on a point mass. On this basis he calculated the total attractive force for a number of examples that parallel those treated by Newton in sections XII and XIII of Book I of the Principia. Finally, he concluded that, if the force were to decrease in any ratio greater than the square of the distance, any object touching the surface would experience a much greater attraction than one only infinitesimally removed from the surface. This would be a discontinuity, that is, something unintelligible, and therefore a rational God

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[51] Memoires de l'Academie Royale des Sciences, 1732, pp. 343-362.

would reject such a possibility.[52] Thus Maupertuis, like Leibniz and Bernoulli, saw the calculus, the mathematics of the continuum, as the structure of reality, and physics, therefore, as applied analysis.

It is of some significance that, in this memoire, the idea of attraction over large distances was simply taken for granted, the fact and mathematical form of the attraction being referred to God. Such an attitude was not at all inconsistent with the metaphysics of Leibniz, but it was incompatible with the assumptions of the Cartesian world-view. As has been indicated, there was, even in Bernoulli's writings, a tendency toward the resuscitation of Cartesianism within the context of the new physics, and this tendency found concrete and general expression in the French Academy's prize question for 1730.

> Quelle est la cause de la figure elliptique des Orbites des Planetes, et pourquoy le grand axe de ces Ellipses change de position, ou ce qui revient au même, pourquoy leur Aphelie ou leur Apogee repond successivement à differens points du Ciel?[53]

The prize went to John Bernoulli but his treatise, while exhibiting his usual mastery of mathematical technique and creative insight, contained some fatal inconsistencies. His results must have contributed considerably to a general acceptance by the new generation of mathematical physicists of the Newtonian attitude of "*hypothes non fingo*."

Bernoulli's attempt to rework the Cartesian system in terms of the new mathematical dynamics takes, as its starting point, Propositions

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[52] *Ibid.*, pp. 348-361.

[53] Quoted by John Bernoulli, *Nouvelles Pensées sur le Systême de M. Descartes et la Maniere d'en déduire les Orbites et les Aphelies des Planetes. Piece qui a remporté les Prix propose par l'Academie Royale des Sciences pour l'Année 1730* (Paris: Claude Jombert, MDCCXXX), pp. 1-2. The treatise is also contained in volume III of the *Opera Omnia*, pp. 133-173.

LI and LII of Book II of Newton's Principia. Newton had intended these as proofs of the impossibility of the Cartesian vortex theory, but Bernoulli showed them to be in error. Newton, in calculating the frictional force acting on cylindrical layers of fluid moving with respect to one another, had neglected the effect of the normal force between the layers. Newton's erroneous calculations had then led to the result that objects--planets--moving in such vortices cannot have a period of revolution proportional to the 3/2 power of the radius, which is indicated by Kepler's Third Law. Bernoulli called Newton's calculations "manifest sophistries."[54]

The calculation of the total frictional force as made by Bernoulli takes three factors into consideration: normal force, or pressure; relative velocity of translation between successive layers; and length of lever arm. The total moment of the force had to be equal to a constant in order that uniform circulation be maintained, and this led to the differential equation $(vxdx - xxdv) \cdot \int \frac{vvdx}{x} = cdx$, which determines the curve of velocity, v, with respect to radial distance x. The result is that the period is as the 4/3 power of the radius.[55]

Next, Bernoulli made a similar calculation for spherical layers of fluid instead of cylindrical ones, and obtained the result, in that case, that the period varies as the 5/3 power of the radius. (Newton had calculated the period in this case to vary as the square of the radius.) Thus the two cases calculated just straddle the correct value: $\frac{4}{3}, \frac{3}{2}, \frac{5}{3}$. This result was not altogether satisfactory, so further refinements were introduced.

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[54] Ibid., pp. 12-14.

[55] Ibid., pp. 18-20.

Since the density of the aetherial medium must be assumed to vary with the distance from the center of the vortex, the density can be introduced into the above equations as an additional variable and determined through the imposition of Kepler's Third Law as a condition of the problem. When this is done, it is clear that Kepler's Law will be satisfied and all that remains to question is the essentially unobservable aetherial density.

Proceeding in this fashion, Bernoulli showed the aetherial density to be reciprocally proportional to the square root of the distance from the center. Thus the density of the aetherial medium must be infinite at the center of the vortex and approach zero asymptotically as the distance becomes large.[56] However, pressure of the aetherial medium was then found to be directly proportional to the square root of the radius which is a manifest contradiction, since in any usual sense of the terms "pressure" and "density," the two are directly proportional to one another.[57]

To avoid this contradiction, Bernoulli fell back on the Cartesian idea that there are aetherial particles of various sizes. The larger particles, impelled by centrifugal force, wander outwards and the fine particles tend to gather at the center producing the high density. Thus the high density refers to the fine particles and the low pressure at the center to the larger particles, and all of these particles swim in an infinitely subtle fluid.[58] Such an explanation was an apparent departure

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[56] Ibid., pp. 25-26.

[57] Ibid., p. 26.

[58] Ibid., pp. 26-27.

from the strictly mathematical reasoning that had characterized Bernoulli's earlier work and seems to represent a regression to the "occult."

It was on the basis of such "occult" qualities as the aetherial density that Bernoulli attempted to explain the elliptical shape of the planetary orbits and the fact that the great axis of an orbit will change its position over a great number of planetary revolutions. The planets were assumed to be of a density not quite the same as that of the part of the vortex where they came into existence. For this reason there will be a radial oscillation of the planet as well as a circular motion due to the vortical motion. The correct combination of these two motions will produce the desired planetary motion.[59]

All of the calculations involved in Bernoulli's attempt to revive the vortex theory are based on the new mechanics, so that the treatise does not really represent a departure from the theoretical structure of mechanics that had been elaborated in Bernoulli's previous work. What the treatise does present is an interpretation of that theoretical structure, a metaphysical, or physical, garment with which to clothe the theoretical skeleton. Just why Bernoulli chose a Cartesian interpretation rather than a Leibnizian one is not clear.[60] But that he did make this choice was an important factor in so far as the recognition of Leibniz's part in the development of mechanics was concerned.

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[59] Ibid., pp. 28-29.

[60] For a detailed discussion of French reaction against Leibnizian metaphysics see chapters VI-X of W. H. Barber, Leibniz in France, from Arnauld to Voltaire. A Study in French Reactions to Leibnizianism, 1670-1760 (Oxford: At the Clarendon Press, 1955).

The result of Bernoulli's choice was that he committed himself to a fruitless interpretative enterprise that led him into a good deal of bad physics at the same time that he implicitly rejected the influence of Leibniz. Thus men like Maupertuis contented themselves with a mathematical physics virtually free of superfluous and controversial metaphysical appendages. In Euler's Mechanics, which, as pointed out in the introduction, treats mechanics simply as applied analysis, there is no mention of any causes of motion except unadorned theoretical terms defined in the context of their interrelationships and empirical significance.

Although Euler, as well as all succeeding continental mechanicians, made use of the Leibnizian form of the calculus and the dynamic relationship as worked out by Bernoulli, he was Newtonian in the sense that he eliminated any explanatory parts of the theory of mechanics. This meant, in a sense, a victory for the Newtonian cause. Leibnizian and Cartesian opposition to the Newtonians simply faded away. Even the force controversy was dropped, for Euler clearly saw that the action of force in time produced momentum and the action of force through space produced "vis viva," or as he put it, an increment in the square of the velocity.[61] Neither of these two modes of action of force was assumed to be more fundamental than the other since, by definition, $dt = \frac{dx}{v}$.[62]

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[61] Leonhard Euler's Mechanik oder analytische Darstellung der Wussenschaft mit Aumerkungen und Erläuterungen, ed. J. P. Wolfers (2 vols.; Greifswald: C. A. Koch's Verlagshandlung, 1848), p. 49.

[62] Ibid.

But whether or not the Newtonians can be said to have achieved a victory over their adversaries, the leadership in the field of mechanics had long since passed across the channel and was not to return. Their victory, such as it was, was truly empty.

CONCLUSION

This history of the assimilation of "Newtonian" mechanics brings to light a number of important insights, not only with regard to specific developments in its own field, but also in a broader sense. In the latter category, its most significant implications have to do with the rule of extra-scientific influences on the development of science. It is perhaps self evident that the scientific activity, seen as the production of explanatory, deductive systems of thought and their application to natural phenomena, must have some relation to intellectual and emotional currents prevalent in society. The postulates of any theory are, after all, *a priori* assumptions that by their very nature are incapable of proof.

Nonetheless, it has long been the attitude of most scientists, and of many historians and philosophers of science, that the "scientific method" guaranteees the scientist that the results of his endeavors shall be free of all those impurities which might result from his personal character, the biases built into his language, and the preconceptions of his social group. In this view, scientific knowledge appears as equivalent in truth value to the religious systems of earlier ages: a representation of ultimate reality or, at the very least, a close approximation to it.

The very fact that science has come to assume this character over the course of the past three or four hundred years would seem to indicate the action of a powerful tendency on the part of western culture to achieve some fixed point of reference in a world whose traditional modes of conception and systems of values had been demolished by the twin revolutions of the Renaissance and the Reformation. Thus it is conceivable that the idea of a "scientific method" itself represents an emotional need, that the precepts of such a method were heavily influenced by preconceptions generated in a particular historical situation, and that it therefore represents no "real" procedure by which real scientists have done their work, but rather an idealization bearing little relation to actual practice.

The present work, while it clearly cannot offer definitive answers to the questions that arise in this context, can at least provide provisional answers based on the materials it presents. The primary questions to be answered center on the nature of scientific method, both as conceived and as applied during the period. Was there in fact _a_ scientific method? What effect did scientific method have on the production and/or assimilation of Newtonian mechanics? And finally, was scientific method itself related to ideas of a specifically non-scientific character?

With regard to the question as to the existence of a scientific method, there certainly was a more or less explicitly formulated method common to most English natural philosophers of the period. It is this method to which Newton modeled his _Principia_ and which he himself formulated in his "Rules of Reasoning in Philosophy."[1] This method based itself on the notion that experience of the phenomenal world will yield to the careful observer those ideas or fundamental elements of experience on the basis of which all phenomena may be understood.

Since these basic ideas are drawn from experience they can have no necessary relationship to any _a priori_ logical system, with the possible exception of geometry, insofar as geometry represents a real science of space. From this it follows that any other mathematical system could have no true relationship to the physical theory. Certainly the infinitesimal calculus, a logical system dealing with essentially unobservable matters, had an _a priori_ character, and was therefore at best a short cut in the geometrical demonstration of propositions.

This conception of scientific method, as has been shown, was not only not the basis upon which Newton developed his theory of mechanics, but also it served to retard the understanding and acceptance of his work. In this instance then, "scientific method" indeed appears as an ideal that bears no relation to practice and which, like all such ideals, only serves to obscure understanding. But if not related to actual practice, to what was this ideal of scientific method related? In its elaboration in the writings of John Locke it was related to Deism. Also, the Newtonian's general confusion over the relationship between weight and momentum has been shown to have been a product not only of their inability to grasp the calculus, but also of their desire to place the

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[1]Isaac Newton, _Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World_. Translated by Andrew Motte, 1729. Translation revision and historical appendix by Florian Cajori (Berkeley, California: University of California Press, 1947), pp. 398-400.

source of gravitational attraction in bodies themselves. This in turn was related to the deistic notion that the world is a self-moving machine whose basic elements are accessible to reason based on experience.

However, the above "scientific method" was not the only approach to science current in Europe during the period. There was also a methodology, stemming from Galileo, that is sometimes given the name of "empirical Pythagoreanism." As the name suggests, this method combines the *a priori* and empirical approaches to knowledge of the physical world, and it does this in a particular way. The postulates of theory are the product of creative insights into the mathematical realities behind the phenomena and the logical consequences of the postulates found in this manner are then tested through controlled experimentation. That is, the phenomena produced in the experiment serve to validate the theory.

The method of "empirical Pythagoreanism" clearly leaves room for the action of extra-scientific currents in the matter of creative insight. This is evidenced by the rather heavy theological and metaphysical elements present in the thought of both Newton and Leibniz. Both men were convinced that the understanding of the world is based on the notions of dynamic causality and continuity. Both men embodied these ideas in a mathematical system that was to represent the reality behind phenomena and provide a logical relation between that ultimate reality and the phenomenal world. However, the ideas that surrounded and gave physical meaning to the notions of causality and continuity were quite different in the minds of Newton and Leibniz. At the same time these peripheral ideas appear to have been necessary to the inception of the mathematical theory. Newton's fluxion represented the flow of a physical quantity in absolute time; and Leibniz's idea of the differential, or momentaneous increment, was modeled after the conatus of a body.

With this in mind, the "method" of empirical Pythagoreanism hardly seems to be a method at all, or at least one that may be sucessfully practiced only by geniuses, for out of the most diverse elements, new concepts and relationships are created which place the entire field of inquiry in a new light. Both Newton and Leibniz provide substantiation for this idea in that their mathematical-physical theories can be seen as integral parts of general philosophical and religious syntheses, which, in turn, contained ideas that were common to their age.

The successors to Newton and Leibniz gradually eliminated the obvious metaphysical appendages to the new mechanics, priding themselves on a truly scientific approach and on a knowledge solidly based on reason and experience. Then David Hume (1711-1776) exposed the concept of causality as an uncritically accepted and unjustified notion. In more recent times the idea of continuity has lost its position as a constant characteristic of natural processes, and with this the Newtonian system fell.

The mere fact that the Newtonian system no longer holds sway over the entire physical world is proof enough that its underlying concepts were not the result of insight into the nature of things external and physical. Rather, those insights must have arisen out of the internal development of human thought in its effort to conceptually order human experience of the world.

These observations are not intended as a criticism of Newtonian mechanics as a great scientific achievement. They are meant only to indicate a general culture dependence of science, not to impugn its usefulness or even its validity as a description of human experience. Human experience of the world is, after all, only loosely related to what may or may not exist in the external world.

Thus the general import of this work tends to bear out Berkeley's view of Newtonian mechanics in particular and of science in general, with the modification that it is not God but man, as an historical and social being, that produces ideas in man's mind. In another sense too, Berkeley's thought is peculiarly applicable to this history. Newton's production of a new mechanical theory met with no direct comprehension and acceptance. Only after Leibniz's influence was felt did the European mechanicians show any understanding of Newton. Thus like the great tree falling in the forest, Newton's thought made no sound, because nobody heard it. And when it was discovered, almost fifty years later, that such a great event had occurred, everybody assumed that it must have made a great sound, and so the histories have recorded it.

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