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THE *VIS VIVA* CONTROVERSY: DO MEANINGS MATTER?

1. Introduction

IN 1686 Leibniz published an article entitled *Brevis Demonstratio erroris memorabilis Cartesii et aliorum circa legem naturalem...*[1]. According to Leibniz, the memorable error in question was the identification of the "motive force" (*vis motrix*) of a body with its "quantity of motion", that is, with the product of its speed by its mass.[2] The argument Leibniz presents purports to show that the "force" of a body in motion must instead be measured by the product of its mass with the *square* of its speed.

The publication of Leibniz's article marks the beginning of the "*vis viva* controversy", a dispute which occupied the attention of most European natural philosophers for about fifty years. By and large, affiliations in the dispute went by nationalities, with English Newtonians and French Cartesians following the "old opinion" (that "force" is proportional to mass times velocity), while Dutch, German and Italian scientists favoured the "new opinion" put forward by Leibniz.

The *vis viva* controversy has always been something of a puzzle to historians of science. In his short textbook, *A History of Physics,* Florian Cajori calls it "a curious dispute".[3] He accounts for the episode by endorsing the view expressed by d'Alembert in 1743, that "...this is a dispute of words, too undignified to occupy the philosophers any longer".[4]

Most traditional historians of science have agreed with Cajori's judgment.[5] This is, they have maintained that the expression "force of a body in motion" was ambiguous, in that it was used by the participants in the *vis viva* controversy to stand for two completely different quantities, one of which was proportional to mass times velocity, while the other was proportional to mass times velocity squared. And thus they have concluded that the dispute was simply due to insufficient care in specifying the meaning of the term "force".

There have been a number of variations on this theme of an undetected

[1] *Acta Eruditorum,* (1686), pp. 161–3. Also in G. Leibniz, *Philosophical Papers and Letters,* edited and translated by L. Loemker (Chicago: Chicago University Press, 1956), pp. 455–63.

[2] In common with many of the other thinkers discussed in this paper, Leibniz did not have a clear concept of mass. For ease of exposition I shall ignore this where it is of no relevance to the issues being discussed.

[3] F. Cajori, *A History of Physics* (New York: Macmillan, 1929), p. 58.

[4] J. d'Alembert, *Traité de Dynamique,* 1st edn (Paris, 1743), p. xxi.

[5] For a list of those holding this view see L. Laudan "The *Vis Viva* Controversy, a Post-Mortem", *Isis,* **59** (1968).

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ambiguity. Cajori follows Ernst Mach[6] in claiming that the "force of a body's motion" simply meant the body's ability to produce effects, and consequently meant different things when different effects were considered. In particular he points out that if one has in mind as the effect in question the *time* for which a body will continue to move if uniformly retarded, one will reckon its initial "force" to be proportional to its initial *velocity,* whereas if one is considering the *distance* through which it will continue if uniformly retarded one will think of its "force" as proportional to the square of its velocity. The dispute, according to Cajori, was the unfortunate result of not distinguishing these two ways of considering a moving body's ability to act.

Max Jammer sees things somewhat differently, in his *Concepts of Force.*[7] He reads "force of a body's motion" as that *applied force necessary to produce (or destroy) that motion in the body.* (Where by "applied force" is intended the familiar notion of modern physics, that is, something continuously exerted on a body from without, which at any instant produces an acceleration proportional to its magnitude.) Jammer points out that the question of what applied force is required to produce or destroy a given motion in a body is indeterminate — for in order to produce or destroy a given velocity in a body an applied force must act for some finite interval, not just for an instant. But the interval can either be considered in terms of *time,* or in terms of *space.* Given an interval of *time,* the applied force required is proportional to the given velocity; but given an interval of *space.* it is proportional to the square of the velocity. So Jammer concludes that the problem was a lack of clarity in formulating the question "What applied force is required to produce a given velocity?"

Others, again, have not distinguished between the Mach–Cajori line and Jammer's analysis, and have felt it sufficient simply to refer to the asymmetrical way in which time and distance are related to velocity in the equations of uniformly accelerated motion.[8]

There is one obvious difficulty with all these versions of the traditional approach. Namely that it is not easy to see why the best minds of the seventeenth and eighteenth centuries should have been incapable of detecting an ambiguity which the modern historian apparently has no difficulty in uncovering. The natural philosophers of the time certainly knew the relationship between "applied force" (usually called "pressure") and acceleration,[9] and the one thing there had been no disagreement about since

[6] E. Mach, *The Science of Mechanics,* translated by T. McCormack, 6th edn (LaSalle, Illinois: Open Court, 1960), p. 365.

[7] M. Jammer, *Concepts of Force* (Cambridge, Mass.: Harvard University Press, 1957), p. 165.

[8] See, for example, the explanation given by H. Alexander in the introduction to his edition of *The Leibniz–Clarke Correspondence* (Manchester: Manchester University Press, 1956). p. xxxi.

[9] See T. Hankins, "Eighteenth Century Attempts to Resolve the *Vis Viva* Controversy" *Isis,* **56** (1965), 286.

Galileo was the behaviour of bodies in uniformly accelerated or retarded motion.

Contemporary work on the episode has to some extent improved on the brisk diagnoses mentioned so far. A number of writers have discussed the subject in some detail and sought to draw attention to some substantial issues involved.[10] One suggestion has been that the real point at issue was the question of what quantity is conserved in impacts between moving bodies.[11] Other commentators have argued that different theories about the ultimate nature of matter were at the centre of the dispute.[12]

As we shall see, these matters of substance were indeed crucially involved in the *vis viva* controversy. However, it seems to me that something needs to be added to the uncovering of these substantial areas of disagreement before we can claim to have a satisfactory understanding of the episode. For then, as now, it was universally agreed that MV, considered as a vector, is conserved in all impacts, and that MV^2 is conserved in impacts between elastic bodies. It needs to be explained what the disputants found to disagree about in connection with these quite compatible generalisations.

It is true that the Leibizians would not admit the existence of hard inelastic fundamental particles, and were thus in a position to maintain that MV^2 is conserved in *all* impacts, which their opponents denied. But what still needs to be elaborated is what a controversy about the correct measure of "force" has to do with an argument about whether or not matter is such that MV^2 is conserved in all impacts.

Ultimately even the more recent commentators have tended to return to the traditional diagnosis that the participants in the debate were "confused", arguing that the real differences between the disputants were obscured by the use of equivocal terminology. The implication is thus again that the unnatural and unfruitful continuation of the debate is to be attributed to the participants not realising soon enough that they were using words with different meanings.

But this simply raises once again the puzzle encountered earlier. If part of their disagreement was simply due to their referring to quite distinct things with the same name, why did the participants not quickly come to realise this and dispel the confusion? Recent work on the episode has done much to provide the materials for a full understanding of the dispute — but there is still a need to fit those materials into a pattern which will make satisfactory sense of the arguments involved.

[10] In particular should be mentioned T. Hankins, *op. cit.* and C. Iltis, "D'Alembert and the *Vis Viva* Controversy" *Studies in History and Philosophy of Science,* 1 (1970); "Leibniz and the *Vis Viva* Controversy" *Isis,* **62** (1971); "The Decline of Cartesianism in Mechanics", *Isis,* **64** (1973); "The Leibnizian-Newtonian Debates: Natural Philosophy and Social Psychology" *British Journal for the History of Science,* **6** (1973).

[11] See T. Hankins, *op. cit.* p. 282; and C. Iltis, "Leibniz and the *Vis Viva* Controversy", p. 22.

[12] See T. Hankins, *Jean d'Alembert* (Oxford: Clarendon Press, 1970), p. 206; and W. Scott, *The Conflict between Atomism and Conservation Theory* (London: Macdonald, 1970), p. 25.

2. Do meanings matter?

In both the traditional and in the more recent work on the *vis viva* controversy it is possible to detect an underlying presupposition concerning the meanings of scientific terms and the resolution of scientific disputes. It seems to me that it is this presupposition, rather than any lack of historical information or accuracy, which has stood in the way of a satisfactory understanding of the dispute.

The implicit presupposition in question is that scientific disputes can in general be divided into two kinds. On the one hand are taken to be those normal issues which can in principle be decided reasonably unproblematically by reference to the empirical facts. On the other hand, there is supposed to be a quite different kind of dispute, characterised by the existence of semantic or conceptual divergences, and which therefore cannot be resolved by reference to the observational data in any straightforward way.

This presupposition is a natural consequence of an atomistic picture of meaning, of the all too plausible idea that the meaning of a term is some *thing* (an idea, an object) which, as it were, stands behind the word and imbues it with the power it has to represent some aspect of the external world. This picture allows only the following two possibilities for a scientific dispute. If the parties to the dispute signify the same things by the words they use, they will be able to decide objectively by reference to experience of the empirical world which sentences are to be accepted. If, on the other hand, they attach different meanings to the words involved, they will disagree about which assertions the observed data supports or discredits; and since their disagreement rests on convention there will be no non-arbitrary way of resolving it.

Once this dichotomy is accepted, however, there is little room for a satisfactory understanding of such episodes as the *vis viva* dispute. The possibility that the two sides of the *vis viva* controversy attached the same meanings to the words they used seems to be ruled out, for that ought to have precluded their continuing the debate without resolution over such a long period. The only alternative then is to conclude that they attached different meanings to the words they used. Which, of course, leaves us with the puzzle of why the equivocation was not detected and removed.

The way out of this dilemma is to be found in modern work on the theory of meaning for scientific terms — or, rather, in modern work that undermines the whole notion that scientific terms have meanings as normally conceived. An important strain in Wittgenstein's later philosophy is his criticism of the idea that meanings are *things* attached to words. Instead words get a significance from the use to which they are put in various practices.[13] And Quine, in his well-known attack on the analytic–synthetic distinction,

[13] L. Wittgenstein, *Philosophical Investigations* (London: Macmillan, 1953).

concludes that our scientific terms get what semantic content they have simply from their place in our total fabric of theory, not from any special association with sensory ideas or "meaning postulates".[14]

To elaborate, the following view seems in a Quinean spirit, even if put less behaviouristically than he would like. Any scientific theory consists of a set of generalisations connecting the terms it uses. There will also be (necessarily unarticulated) principles governing the observational use of some of these terms, which in certain circumstances will allow them to be applied directly in response to sensory stimulation. Each term in a theory must somehow get its semantic content from such a structure of generalisations and observational procedures. What Quine has shown is that we must take this *whole* structure as determining semantic content; that we cannot separate out those bits which constitute meanings from those which don't.

The adoption of such a holist perspective on the meanings of scientific terms enable us to escape from the constricting dichotomy which has hampered the analysis of the *vis viva* controversy. We will no longer wish to ask whether or not the advocates of the different opinions attached the same "meanings" to their words, and, in particular, to the term "force". For, given the perspective proposed, there is no possibility of dividing theoretical disputes into those involving differences in meanings and those involving factual differences only. *All* theoretical disputes will involve some differences in theoretical structure between the two sides. Since we do not distinguish the meaning-constitutive from the empirical components of theoretical structures, such differences will all be similar in kind.

It might be thought the adoption of a holist view, in which the significance of a term depends on the total theoretical structure in which it is used, implies an essentially relativistic view of science. For, it might well be asked, what is there to ensure that the empirical data will lead to appropriate theoretical revisions and to the objective resolution of scientific disputes, once we have denied a clear sense to the possibility of scientific terms retaining constant meaning across theoretical change?

But this particular worry is well-founded only as long as we remain within the atomistic model which depicts empirical objectivity as requiring fixed and shared units of meaning behind the words we use. Once we shift to the holist perspective I am recommending we can perfectly well understand scientific objectivity independently of any assumptions about constancy of meanings. There is nothing in the holist perspective which precludes our recognising that there can be empirically objective grounds for revising a given scientific theory,

[14] W. Quine, "Two Dogmas of Empiricism" in *From a Logical Point of View* (Cambridge, Mass.: M.I.T. Press, 1960).

The approach to scientific terms I advocate in this paper is not uncontroversial. For the defence lacking here see my unpublished PhD. dissertation, *A Theory of Meaning for Scientific Terms* (Cambridge University).

nor our maintaining that amongst possible revisions mooted in such circumstances some will objectively be better than others. For it certainly allows that a theory consisting of generalisations plus observational principles can be shown to be empirically deficient by the emergence of observable anomalies, as when a prediction based on certain of its observational principles and generalisations fails to be borne out by observations based on other observational principles. And there is nothing in the holist view which prevents us comparing different modifications of a theory made when such a deficiency is exposed along such dimensions as their simplicity and generality and the extent to which they retain parts of the original theory which have previously been corroborated by the empirical data.

Thus, even though the holist approach undercuts questions of whether or not scientific terms stand for the same things in different theories, it by no means forces us to relativism about scientific disputes. In my analysis of the *vis viva* controversy I intend to show that it can be seen as a perfectly rational competition between two theoretical systems, a competition as to which system dealt with the empirical data better, according to such criteria as simplicity, generality and retention of corroborated content. Thus, by ignoring the fruitless question of whether or not "force" was univocal, I hope to show that the *vis viva* controversy was in essence no different from any other scientific dispute about which of two alternative positions is empirically preferable.

Another objection to the holist approach might be that it cannot give any account of what it is for two theoretical systems to be competing accounts of the same aspect of reality rather than different but quite compatible descriptions of different subject matters. Traditionally what is required in order for them to be rival accounts is that two theoretical systems must be framed in terminology with common meanings. But holism precludes our ever judging that scientists with different theories are using words to stand for the same things, and consequently seems to stop us ever judging that apparently conflicting assertions are indeed giving incompatible descriptions of the same phenomenon. There thus seems to be a danger that we are going to be forced to conclude after all that in every scientific dispute (and, *a fortiori,* in the *vis viva* controversy) the two sides are not in active disagreement but merely talking about different things.

However, I think it is still perfectly possible, within the holist view, to have a conception of two theories being *rival* accounts of the same aspect of reality, rather than non-competing views about different subject matters. In the first place we can note that the holist perspective leaves untouched the possibility that two different theories may have *some* terms whose observational use is governed by *some* common rules. However, such common observational content, though it would mean that the two theories could issue in predictions which conflict in the sense that observations cannot bear them both out, will not suffice for two theories to be considered competing accounts of a given subject area. For disparate theories from quite separate branches of science (such as thermodynamics and biology) will often share some observational rules without our wanting to say they are competitors. Even if the same observations are sometimes relevant to two such theories, we will not see the data as supporting one *against* the other. There will be no question of choosing *between* the two theories — merely problems about how each is best to be developed in the light of the evidence. Genuinely competing theories call for a decision for one *over* the other (even if the correct answer is not immediately obvious).

What then, over and above mutual observational content, is required for two theories to count as *competing* accounts of a given aspect of reality? A more illuminating requirement (and one also admitted by the holist view) would be that the overall structures of generalisations in, and observational rules associated with, the two theories should display a significant degree of similarity. For, in those cases where this requirement is satisfied, not only will the parties to the dispute share a stock of agreed premises by reference to which they can argue deficiencies and inconsistencies in each other's view, but, moreover, there will be a reason for deciding (at some stage) *between* the two theories rather than continuing to develop both. For the fact that two views are small variations on a common theme provides a rationale for discarding one — it is only with two radically different positions that it is worth agreeing to elaborate both, for only then will the double effort involved hold any promise of producing something like double returns. Real ambiguity, then, in the sense which would support a diagnosis that apparently contrary views are not really in conflict, should be deemed to occur only when the terms involved are set within theoretical systems which are quite dissimilar in structure.

It is true that this talk of different theoretical systems containing "similar structures" is irremediably vague. And so there will be no precise criteria for deciding when two theories are actually competing accounts of the same domain. But this conclusion seems to me not inappropriate — why should such questions be capable of conclusive decision? What decides, as much as anything else whether two theories are competitors is their temporal and cultural proximity. Unless they are near in these respects there will be no question of their competing; if, on the other hand, they are both current in the same historical context, it seem generally to be the case that any similarity will suffice to generate a clash between them.

Another point worth noting is that the account of competing scientific theories put forward here leaves it open that the result of such competition may take considerable time to emerge, even if the theories are genuine competitors and the disputants are concerned to resolve their disagreement rationally. This is because the holist approach rejects the view that scientific disagreements consist of easily decidable conflicts between isolated assertions mutually formulated in words standing for the same aspects of empirical reality. Rather it takes each theory to face the totality of relevant data as a whole. A theory which at one time deals less well with the data than a rival might well in time come to be modified and elaborated in such a way as to show it superior. A decision as to which of competing theories is ultimately preferable will generally wait until it becomes clear how such modifications and elaborations will turn out, which might (but need not) take some considerable time.

My intention in this paper is to show that the *vis viva* controversy was indeed a serious debate between two genuinely rival frameworks of physical thought. In order to support this conclusion I shall not seek to establish that the two sides to the controversy associated the "same ideas" (or external physical entities) with their words. Rather I shall take it to suffice if I can demonstrate the existence of a degree of structural similarity between the two theoretical systems involved. This I will attempt to do by showing how the two sides to the dispute represented alternative modifications of a common system of mechanical thought handed down from the early part of the seventeenth century. As such they naturally continued to share a large proportion of the structure of theoretical presuppositions and observational principles which constituted that earlier structure of thought.

The explanation of the longevity of the controversy will not be that the participants were talking about different things, nor that they ignored objective grounds for choosing between their views, but simply that it required time to find which of the alternative frameworks could best be developed to cope with the totality of relevant empirical data.

My exposition will proceed as follows. I shall first briefly describe the 17th framework of mechanical thought, in particular as embodied in Descartes' theory of impact, and I shall mention various critiques of this theory. Then I shall show how Leibniz's publication of *Brevis Demonstratio* can be seen as a response to the problems raised by these critiques; and I shall describe the initial reaction to Leibniz's views. After this the respective views of the Leibnizians and their opponents on the central problem of impact are described, and it is shown that the two sides to the dispute can be seen as alternative strategies for revising the flawed Cartesian theory of impact. And finally the understanding of the dispute thus gained is used to make some sense of some of the apparently fatuous faces of the controversy.

Inevitably there are a number of respects in which my analysis is over-simplified. I generalise about groups of scientists quite freely, and under-emphasise the often many differences between the members of such groups. And I concentrate on certain theoretical aspects of the debate to the exclusion of others. This is because my aim has been to depict just those configurations and commitments which are necessary to an understanding of the controversy, even if other interesting issues are thereby ignored. In particular I say little about the social or psychological factors which might have affected the development of the controversy; nor about the influence of such theological questions as whether there is divine intervention in the physical world; nor about debates on such basic metaphysical issues as whether motion is a state or an active power (though, as will become clear, the controversy was to a large extent sustained by an implicit acceptance of the latter view). This is not because I consider such factors to be irrelevant to an appreciation of the historical episode in question, but rather because it seems to me that their relevance can only be estimated against a background in which it is made clear what, if anything, the controversy amounted to as a substantive scientific dispute.

3. The 17th century background

The dominant philosophy of nature in the middle of the seventeenth century was mechanism. The differences between different versions of mechanism need not concern us here. What is relevant is that something like the "principle of inertia" was generally assumed, in that it was accepted that a body would continue in any motion, with the same speed in the same direction, until subject to some external action. It was also assumed that the only way in which such action could be exercised was by *impact;* by matter with some motion coming into contact with matter with a different motion. "Action at a distance" was considered to be absolutely impossible.

As a result, the laws governing the changes of motion which result from impacts were considered to be of fundamental importance in the seventeenth century. It was in the problem of deriving these laws that the notion of "force of motion" had its most important application.

Descartes was the first to attempt a systematic treatment of impact. The main components of the concept of "force of motion" can be seen in his approach to the problem. He took it that any body had a "force", proportional to its "quantity of motion" (its "mass" times its speed). And he assumed that in any impact between hard bodies, any "force" lost by one would be gained by the other. Since impact was the only possible means of changing motion, it followed that the total "quantity of motion" in the universe was at all times conserved.

The rules that Descartes derives strike the modern eye as extremely strange.[15] This is largely because he considered "force of motion" as a scalar quantity, and not as a vector like the modern concept of momentum. For him, an impact did not necessarily produce oppositely directed, equal-sized changes in directional motion. Instead, he saw what would happen to a body

[15]See R. Descartes, *Principia Philosophiae* (Holland, 1644); in *Oeuvres de Descartes,* C. Adam and P. Tannery (eds.) (Paris, 1905), vol. 8, pp. 68–70.

in impact as determined by the relative sizes of its "force of motion" and the *resistance* to this force offered by the other body.[16] If the resistance of the struck body exceeds the force of the striking body then no transfer of motion takes place and the latter rebounds with its original speed in the opposite direction. If the force exceeds the resistance then the two bodies move together in the original direction of the impacting body at such a speed as to conserve the total quantity of motion. A moving body resists the impression of the force of another proportionately to its own force of motion. Somewhat anomalously, a body at rest does not have zero resistance but resists proportionately to its mass and the speed *with which* it is struck.

Only a few of the solutions these premises imply are now considered empirically correct (some for elastic bodies, some for inelastic). But the important things to note once again are that for Descartes all impact phenomena, and hence all changes of motion, were to be explained in terms of the "forces of motion" of the bodies involved. All impacts involved the transference of "force" from one body to another, in such a way that the total "force" was conserved. The precise amount depended, as above, on the contest between "force" and "resistance".

As the first systematic treatment of the problem, Descartes' rules of impact had considerable authority for his successors. However, the deficiencies of his analysis soon became apparent, once it was subject to criticism.

Huygens realised that Descartes' rules were inconsistent with something else both Descartes and he himself presupposed — the relativity of motion. Huygens considered what would happen if a series of impacts took place on a barge moving down a Dutch canal. He pointed out that an impact which seemed to a man on the barge to occur according to Descartes' rules, would not necessarily seem to do so to a man on the bank.

Huygens assumed that any rules of impact should remain true under such uniform transformations of motion. This form of argument (which came to be known simply as "the method of the boat") is of course extremely powerful. In Huygens' case, it enabled him to dispense almost entirely with dynamic assumptions in deriving the results which we now consider to describe cases of elastic impact.

Huygens' article in the *Journal des Scavans* of 1669, which presented his results on impact, was reprinted in the *Philosophical Transactions of the Royal Society* of the same year. This was as a response to the Royal Society's invitation for accounts of the "laws of motion". There were two other important replies to this invitation, both published in the same year. One was by Christopher Wren, the other by John Wallis, Wren got the same results as Huygens, whereas Wallis derived the results we now accept for inelastic impacts.

[16]This aspect of Descartes' views is emphasised in A. Gabbey, "Force and Inertia in Seventeenth-Century Dynamics", *Studies in History and Philosophy Science,* **2** (1971).

Wren applied the model of the lever to impact. He argued that if two bodies have oppositely directed speeds inversely proportional "to the bodies", they will be in "equilibrium" in impact, and after impact each will therefore be reflected with its original motion. And if two bodies with unequal "forces" meet, he argued that the stronger will cause the other to be reflected with just such an increase of speed that it is as much stronger than the first after impact, as the first was stronger than it was before. This approach reflects the then common reliance on the understanding of the simple machines in the analysis of impact. I shall return to this later.

Wallis, as I said, derived the results which are today accepted as describing inelastic impacts. There are two significant features of his analysis. First, he denied that *hard* bodies rebound in impact. This was an innovation. Previous thinkers, including Descartes, Huygens and Wren, had all assumed that perfectly hard bodies *can* rebound, and had considered that their theories of impact applied to such "hard-elastic" bodies. But within some few years it was Wallis' position on this question which was generally accepted. That is, it was agreed that hard, inflexible bodies did not rebound in impact.

The second significant feature of Wallis' analysis is the way he derives his conclusions. He proceeded as follows. He first imagined one of the two bodies to be at rest, and supposed it to be struck by the second, moving wih its actual motion. They would then move together, with the second transferring some of its "force" to the first, so that their common velocity is given by dividing the second body's original "force" by their joint mass. Wallis then reversed the situation, and imagined the second body to be at rest, and the first to strike it with *its* actual motion, with a similar putative result. He then added the two resulting motions vectorially, to derive what would actually happen in the impact. What is significant about this analysis is that it implies a conception of "force of motion" as a vectorial quantity — "force" in one direction is supposed to be "cancelled out" by an equal amount of "force" in the contrary direction.

The importance of the analyses given by Huygens, Wren and Wallis was soon recognised. But there was one factor which weighed heavily against their findings. This was the point on which their various analyses were in surprising agreement: they all agreed that the total "force of motion" in the universe, measured by mass times speed, is *not* conserved. For, as mentioned above, Huygens and Wren both put forward the formulaé now considered to describe cases of elastic impact; and in such cases the quantity given by mass times speed can either increase or descrease in impact (depending on what frame of reference is taken). And Wallis, in putting forward the formulae for inelastic impacts, implied that this quantity decreases whenever an impact is considered in a frame of reference in which one of the bodies changed direction.

Given the basic framework for the analysis of impact displayed in Descartes' work, these conclusions constituted a striking anomaly. For central to that framework was the principle that all impacts involved the transference of "force" from body to another, with the total "force" being conserved. Insofar as the common run of natural philosophers based their understanding of impact on this principle they did not find it easy to stomach the theories put forward by Huygens, Wren and Wallis, A. R. Hall has described[17] the initial rejection of these theories by the members of the Royal Society, on the grounds that they implied that "motion" (mass times speed) is not conserved. Amongst others he mentions William Neile, who entered into a controversy with Wallis on the question of whether "motion" can be destroyed in impact; and Francis Willughby, who presented some *Animadversions* on the results of Huygens and Wren. These began:

> As it seemed to me absurd that motion should be destroyed or created from nothing, and more than that, incredible, I suspected that there was some other sense concealed in Dr. Wren's words, which I long sought to discover, in vain. At length Huygens by publishing the same hypothesis as also discovered by himself quite freed me from that suspicion, for he expressly allowed that the quantity of motion of two bodies might be increased or diminished by their collision.

And Willughby concluded that

> The hypothesis from which these paradoxes follow should by no means be admitted unless it is confirmed by store of experiments.[18]

These reactions show that the principle that total force is conserved was of fundamental importance within the common framework for thinking about physical action inherited from Descartes. It is relevant that both Huygens and Wren, in deriving conclusions which contradicted this principle, managed to proceed by arguments drawn from outside this framework of thought. Huygens dispensed almost entirely with dynamic assumptions; while Wren based his reasoning on the analysis of the lever.

Wallis, on the other hand, can be seen as working within the Cartesian framework, as proposing a modification to the standard analysis of impact rather than trying to derive his conclusions independently of the general theory. For, by treating "force" as a *vectorial* quantity equal to mass times velocity, Wallis could continue to assume that all impacts consist of a transference of "force" from one body to another in such a way as to conserve the total "force".

As we shall see, it was this modification of the Cartesian position which was adopted by those who opposed Leibniz's views on "force". But before going onto this it is time to examine Leibniz's own views.

[17]A. R. Hall, "Mechanics and the Royal Society, 1668–70" *British Journal for the History of Science*, 3 (1966).

[18]*Royal Society Classified Papers*, vol. (iii) (1), No. 54. Quoted in A. R. Hall, *op. cit.* p. 35.

4. "Brevis Demonstratio" and the early period of the controversy

Leibniz was one of those who was perturbed by the possibility that the quantity of motion is not conserved in collisions between bodies. In 1680 he wrote

> M. Descartes physics has a great defect; it is that his rules of motion are for the most part false. This is demonstrated. And his great principle, that the same quantity of motion is conserved in the world, is an error. What I say here is acknowledged by the ablest people in France and England.[19]

But, while admitting in this way the non-conservation of the *quantity* of motion, Leibniz was profoundly resistant to the idea that the total "*force* of motion" in the universe is not conserved. As he put it in *Brevis Demonstratio*

> It conforms to reason to say that the same sum of motive force is conserved in nature; this sum does not diminish, since we never observe any body lose any force that is not transferred to another; nor does this sum increase since perpetual motion is unreal to such a degree that no machine and in consequence not even the entire world can conserve its force without new impulsion from without.

The natural conclusion for Leibniz to draw was that "motive force" could not be the same thing as "quantity of motion". The argument of *Brevis Demonstratio* was designed to provide an independent proof of this conclusion. Leibniz first stated two premises which he maintained are admitted by "Cartesians as well as other philosophers and mathematicians of our times". These were:

(i) that a body, in falling from a certain height, acquires just that "force" necessary to raise itself to that height again, and

(ii) that the same "force" is required to raise a body of 4 lb. 1 yard as is required to raise a body of 1 lb. 4 yards.

From these two premises, together with the Galilean analysis of free fall, Leibniz deduced that "force of motion" cannot equal quantity of motion (mass times speed).

Leibniz concluded:

> It seems from this that *force* is rather to be estimated from the quantity of effect which it can produce; for example, from the height to which it can elevate a heavy body of given magnitude and kind, but not from the velocity which it can impress upon the body.

(In *Brevis Demonstratio* Leibniz did not explicity say that "motive force" should be taken as proportional to velocity squared: but of course this would follow directly from the estimate of "force" in terms of height.)

Thus the strategy adopted by Leibniz in *Brevis Demonstratio* was to turn

[19]Letter to Filippi, January, 1680. Quoted in R. Dugas, *Mechanics in the Seventeenth Century*, translated F. Jacquot (Neuchatel, Switzerland: Editions da Griffon, 1958).

from the analysis of impact, and to argue instead from principles accepted in other areas of natural philosophy. This should not be taken, however, to imply that the problem of impact was not of central concern for Leibniz. He was as much a mechanist as any of his contemporaries in that he thought the possibility of action at a distance absurd: as such he could not but consider the analysis of impact as central to physics. The reason for his strategy in *Brevis Demonstratio* was simply that, with the acknowledged anomaly of quantity of motion not being conserved in impact, the basic principles by which the phenomenon of impact was to be analysed had become problematic. So, if Leibniz was to discredit Descartes' measure of "force" and suggest a measure of his own, it was essential that he draw his assumptions from other areas.

The initial responses to *Brevis Demonstratio* followed Leibniz's strategy in focussing on the premises of his argument rather than the implications of his conclusion for the analysis of impact. It was the second premise that was seen as most vulnerable: that is, the assumption the same "force" is needed to raise 4 lbs. 1 yard as 1 lb. 4 yards.

In *Brevis Demonstratio* Leibniz does not state the basis for this assumption. But from the ensuing discussion it seems that he was counting on the law of the lever to get it accepted. The issue was thus what the law of moments shows about "forces". On Leibniz's side was the argument that the same "force" on a lever is required to raise 4 lbs. 1 yard as to raise 1 lb. (four times as far from the fulcrum) 4 yards. His critics[20] in response insisted that the *time* of the action has to be taken into account — clearly a force on a lever which lifts 4 lb. through 1 yard can lift the 1 lb. four times further from the fulcrum through any distance desired, provided it can act for a greater or lesser time. They concluded that the correct measure of a "force" was the product of the weight it can lift and the *velocity* with which it can lift it.

Involved here is an understanding of the lever according to which a lever operates when the "force" of a body or one arm overcomes the contrary "force" or "resistance" of the body on the other. This thinking was extremely common in the seventeenth century, and persisted well into the eighteenth. (It was this model of the lever that Wren used for his analysis of impact; and intimations of this model can be seen even in Descartes' theory.)

In fact it was Leibniz himself who first clearly perceived the inappropriateness of analysing the simple machines in terms of "forces of *motion*". He gives some idea of this in *Brevis Demonstratio*, where in considering the law of the lever, he says that one

> ...need not wonder that in common machines...there is equilibrium...when the same quantity of motion is produced on either side. For in this special

[20]See, for example, Abbé Catalan, "Courte Remarque de M. l'Abbé D.C.", *Nouvelles de la Republique des Lettres* **8**, (1686), pp. 1000–1005.

> case the *quantity of effect,* or the height risen or fallen, will be the same on both sides, no matter to which side of the balance the motion is applied. It is therefore merely accidental here that the force can be estimated from the quantity of motion.

Leibniz elaborated this insight in response to the criticisms of his argument in *Brevis Demonstratio*. In his *Essay on Dynamics*[21] (written in 1691) he expressed the view that

> What has contributed the most to confound force with quantity of motion is the abuse of the static doctrine.

In this work he explained in some detail how this confusion should be avoided. This he did by first introducing the notion of "*dead* force" which he distinguished from "*living* force" ("force *vives*"). It was the latter which he identified with "motive force", and urged should be measured by mass times velocity squared. "Dead force", on the other hand, is not conditional on an actual motion, but is

> the infinitely small motion which I am accustomed to call *solitication,* which takes place when a heavy body tries to commence movement, and has not yet conceived any impetuosity.

In fact because of his general principle of continuity in nature Leibniz considered that "dead force" is involved in all alterations of velocity. Moreover, he used the concept so as to imply that acceleration is proportional to dead force. Thus his concept of "dead force" is by no means dissimilar to the "applied force", equal to mass times acceleration, of classical physics. But it only played a subsidiary role in Leibniz's thought, as the means by which "living force" is transferred from one body to another.

It did however enable him to give a clear account of what is involved in the analysis of the lever. There are no "living forces" to be considered in connection with the law of moments, for "when bodies are in equilibrium, and, trying to descend, are mutually hindered", no actual motion arises, and we have a case concerning only "dead forces". Thus the conditions of equilibrium specify and equation between "dead" rather than "living forces", and have no immediate relevance to the question of how to measure "motive force".

Having disposed of the argument from the simple machines as the basis for his claim that the *height* to which it can raise a body against gravity is the measure of a "force of motion", Leibniz turned to another generally accepted principle, namely, that "the effect is always equal to the cause". In his *Specimen Dynamicum* of 1695, he maintained that "motive force" is to be estimated

> ...by the effect which it produces in consuming itself... Thus those things

[21] "Essay on Dynamics", G. Leibniz, in *New Essays Concerning Human Understanding,* edited and translated A. Langley (La Salle, Illinois: Open Court, 1949), pp. 657–670.

> which by themselves could not easily be compared, by their effects at least might be compared accurately. I assumed, moreover, that the effect must be equal to its cause if it is produced by the expenditure or consumption of the entire force.[22]

The principle stated here by Leibniz, that the effect must be equal to the cause which is consumed in producing it, is something which played a central role in the *vis viva* controversy. In nearly every contribution to the dispute, prior to d'Alembert's *Traité* in 1743, the author makes a point of affirming his commitment to the equality of cause and effect. Jean Bernoulli goes so far as to say

> To try and demonstrate this law would be to obscure it. Indeed, everyone regards this as an incontestable axiom, that an efficient cause cannot perish, either as a whole or in part, without producing an effect equal to its loss.[23]

However, in spite of its universal acceptance, this principle by no means sufficed to win acceptance for Leibniz's measure of force. For he had no conclusive argument to show that the *distance* to which a force could raise a body against gravity should be considered as the *effect* produced by its consumption. What he says in *Specimen Dynamicum* is less than convincing.

> I chose that effect of the violent effects which is especially capable of homogeneity or division into similar and equal parts, such as exists in the ascent of a body possessed of weight: for the elevation of a heavy body two or three feet is precisely double or triple the elevation of the same heavy body one foot.

As will be shown in Section 7 the partisans of the "old opinion" argued that the distance traversed in retarded motion is *not* the relevant measure of the effect produced by the "force" which has been consumed in that retarded motion. Their view was that it was the *time* for which a body continues to move that measures its "force", and that consequently "force" is proportional to MV, not MV^2.

5. The "old" and the "new" opinions

If there had been nothing more to the "*vis viva* controversy" than a dispute as to what should count as the "effect" produced in the "consumption of a force" in retarded motion, then clearly there could have been no real substance to the controversy. The general insistence that "effects equal causes" would have been no more than the mouthing of an empty slogan, which could only have served to obscure the pointlessness of the debate. And if that had indeed been

[22] "Specimen Dynamicum", G. Leibniz, *Philosophical Papers and Letters*, pp. 435–452.
[23] J. Bernoulli, "Discours sur les loix de la Communication du Mouvement" *Recueil des Piéces qui a Remporté les Prix de l'Academie Royale des Sciences* (Paris, 1752), vol. 1, Ch. X, Section 1.

the case the two sides in the controversy, by considering two different "effects", would simply have been using "force" in two quite different ways, and disputing about nothing but words.

However, retarded motion was not considered around 1700 to be the only, or even the most important, case in which effects are produced by the consumption of "force". More central in the minds of the mechanistic natural philosophers of the time was, of course, what happened in impacts between particles in motion. And it is in connection with this problem that the agreement on the dictum "effects equal causes" can be seen in its true significance. For in an impact the effect produced by the annihilation of a "force" in one body was simply assumed to be the production of the same quantity of "force" in another. So what the general adherence to the universal "equality of cause and effect" really indicates is that all the participants in the *vis viva* controversy continued to approach the problem of impact through the framework originally proposed by Descartes. That is, they all took for granted that all impacts are to be accounted for by the transference of "force" from one body to the other, with the total force being conserved. Where they disagreed from Descartes, and from each other, was, of course, on the questions of how exactly "force" should be measured, and what determined exactly how much "force" would be transferred in a given impact.

It is precisely by attending to this common framework of presuppositions inherited from Descartes that we will be able to gain a proper understanding of the disagreement between the two sides in the *vis viva* controversy. For if we do attend to it we can see that it was *because* there was this common framework for approaching the problem of impact that the controversy amounted to a serious dispute between competing views. Far from being a mere "dispute of words", their debate was at basis a perfectly sensible one between two alternative suggestions as to how the Cartesian theory of impact could best be modified and elaborated.

In this section I shall attempt to substantiate this view of the *vis viva* controversy by examining in some detail the views of the two sides on impact. After that I shall return to the question of retarded motion and show that even this aspect of the debate can be seen to have had some substance.

Let us first look at Leibniz's views on impact. As argued earlier, it was probably the theories of impact of Wallis, Wren and Huygens which originally caused Leibniz to be dissatisfied with the Cartesian measure of "motive force". For in none of these theories is the "quantity of motion" generally conserved. That is, it was not held to be true that the annihilation of motion in one body in an impact was always accompanied by the production of an equivalent quantity of motion in the other body.

In the *Essay on Dynamics* Leibniz notes the general recognition of this failure of conservation of quantity of motion, and observes

> ...that we have been thrown too far into the other extreme, and do not recognise the conservation of anything absolute which might hold in the place of the quantity of motion.

What Leibniz thought should be recognised as conserved in impact was of course his "living force", or "*vis viva*", measured by mass times velocity squared. In the *Essay on Dynamics* Leibniz specifies three equations which he believes describe all impacts between two bodies. The first is the conservation of the relative velocities; the second conservation of momentum, considered as a *vector* quantity; and the third the conservation of mass times velocity squared. Leibniz notes that any two of these equations would suffice to derive the third.

Leibniz refers the reader to Huygens and his "method of the boat" for the derivation of these equations. However, there is the important difference that, while Huygens, as noted earlier, thought himself to be deriving theorems about perfectly hard, inflexible bodies, Leibniz intended his theory to describe the behaviour of flexible bodies, whose perfect elasticity consisted in their ability to return to their original shape after being deformed in collisions. From this assumption of the elasticity of matter Leibniz derived an analysis of the detailed mechanics of impact. This involved the "dead forces", equal and oppositely directed, which arise *during* an impact, and act to alter the velocity of a body proportionately to the body's mass and the time for which they act.

These notions of Leibniz's were later elaborated with great sophistication by Jean Bernoulli in his important essay *Discourse on the Laws of the Communication of Motion.* After deriving the laws of elastic impact from the relativity of motion and the elasticity of matter, he uses his considerable facility with the differential relations between "pressure" (or "dead force"), velocity, distance and time to produce what he considered to be additional support for Leibniz's measure of "living force". What he does is deduce from the "familiar law of acceleration" (pressure = mass × acceleration) the conclusion that the mass times velocity squared produced, or destroyed, by the action of a spring (elastic surface) is proportional to the length through which it expands, or contracts.

Later in the essay Bernoulli produces another argument, based on the directional resolution of velocities, to show that the number of similar springs which a body in motion can "close" successively before being brought to rest is proportional to its mass times its velocity squared. In Bernoulli's view, these conclusions about the ability of springs to accelerate and retard bodies in motion provide an additional proof for Leibniz's measure of force. For he maintained that the "cause" of the "force" imparted to a body impelled by a spring must be the length through which the spring expands; and that, conversely, the effect of a "force" consumed in closing a compound spring, or a series of springs, must be the length through which the spring or springs are closed. And so, from the necessary equality of "causes" and "effects", it had to follow that the "force of a body in motion" is given by its mass times its velocity squared.

Thus we see how Leibniz, and following him, Bernoulli, articulated their theory of "motive force" and the communication of motion, by reference to the "pressures of elasticity", or "dead forces", which occur in impacts between perfectly elastic bodies. It is worth emphasising, however, that for both of them it was "living force" which was the more important quantity; "dead force" was simply the means by which "living force" acted, and, indeed, "dead force" manifested itself only in situations where "living force" was transferring itself from one body to another. The real cause of any change in the motion of a body was its receiving the "living force" of some other body, and, since effects had to be equal to causes, the change of motion had to be such that the "living force" received was equal to that lost by the other body. As Bernoulli was at pains to emphasise

> It is then clear that when the living force of a body diminishes or increases in its meeting with some other body, the living force of this other body must in exchange increase or diminish in the same quantity; the increase of one being the immediate effect of the diminution of the other, from which follows necessarily the conservation of the total quantity of living force: thus this quantity is itself absolutely unalterable in the impact of bodies.

There was, of course, one problem which this commitment to the conservation of *vis viva* raised for the Leibnizians. This was the undeniable phenomenal fact that certain bodies were such that *vis viva* was lost in their impact. For it is clearly absurd to suppose that all medium-sized physical objects are perfectly elastic. But Leibniz had no doubt as to how this should be explained:

> ...in the impact of such bodies a part of the force is absorbed by the small parts which compose the mass, without this force being given to the whole; and this must always happen when the pressed mass does not recover perfectly. (Essay on Dynamics.)

Leibniz repeats this view in his fifth paper of *The Leibniz–Clarke Correspondence,* where he says:

> The author objects, that two soft or un-elastic bodies meeting together, lose some of their force. I answer, no. 'Tis true, their wholes lose it with respect to their total motion; but their parts receive it, being shaken (internally) by the force of the concourse. And therefore that loss of force is only apparent.

In general, the proponents of *vis viva* as a measure of "motive force" adopted this account of inelastic impacts, where "*vis viva*" appeared to be lost at the phenomenal level.

One might think that those who measured "force" by mass times velocity would have had the same problem with hard bodies as did the Leibnizians. For not only is the sum of mass times velocity squared not conserved in hard body impacts, but the sum of mass times the magnitude of the velocity is not always conserved either. (Some is lost whenever either body changes direction.)

But hard bodies were not a problem for the proponents of the "old opinion". This was not because they denied the equality of cause and effect. They affirmed this as energetically as the Leibnizians, and it would have been equally inconceivable to them that "force" could be created or destroyed in an impact. Instead they adopted the approach used by Wallis, and accommodated the phenomenon of inelastic impacts by conceiving of "force" as a *vector* quantity, at least insofar as it was transmitted in impact. The effect of the annihilation of a certain "motive force" in an impact was for them the production of an equal amount of "force" *in the same direction*. Thus, the fact that two bodies do not rebound in an impact was argued not to imply an inequality of cause and effect. For, considered as a vector quantity, mass times velocity *is* conserved in inelastic impacts (and, for that matter, in elastic impacts as well). And so they maintained that the quantity of motion which one body loses in an impact is always transferred to the other, and that that motion, continuing to act in the same direction, combines vectorially with the "motive force" that the latter body originally had.

The essay submitted to the Academie des Sciences competition on hard body impact in 1724 by Colin Maclaurin, the English Newtonian, is typical of this point of view.[24] (This was the same competition to which Bernoulli's *Discours* discussed above was originally submitted. Bernoulli was disqualified; not unjustifiably, since he denied the existence of hard, inflexible bodies in a competition on hard bodies. Maclaurin won the prize.) Amongst the "uncontested" axioms which Maclaurin states at the beginning of his essay is one to the effect that

> The change of force, that is to say, its increase or decrease, is always proportional to the impressed force, and is made in the direction of that force. By *impressed force* is intended that which consumes itself entirely in increasing or decreasing the motion of a body.

Maclaurin also thinks it an "uncontested axiom" that the "forces of bodies" are proportional to their masses. After arguing against the new opinion that "force" is proportional to velocity squared, he reaffirms its proportionality to velocity. He then proceeds to deduce his laws of impact.

Hard bodies moving in the same direction have no spring, and so move together after impact, in such a way that

> ...all that one of these bodies loses in impact the other gains.

[24]C. Maclaurin, "Demonstration des Loix du Choc des Corps", *Recueil des Piéces qui a Remporté les Prix de l'Academie Royal des Sciences* (Paris, 1752), Vol. 1.

With hard bodies moving in opposite directions he notes there is a loss of "force", taken absolutely, but points out that

> ...the force which body *B* [with smaller*MV*] gains on the side towards which both bodies move after impact is that which the body *A* loses.

After dealing with hard bodies, Maclaurin proceeds to elastic impacts. Here his conception of "force" as a *vectorial* substance transferred from one body to another in impact is particularly clear. He argues that in restoring the shapes of the bodies, their elasticity

> ...doubles the exchanges of the forces which would have been produced in the bodies if they had no spring.

and that therefore

> ...there is a double augmentation produced in the force of the body which gains by the impact and a double diminution in the force of the body which loses by the impact.

Maclaurin's meaning is clear. In inelastic impacts one body loses, and the other gains, a quantity of "force", taken in one direction. In an elastic impact, the body losing some "force" is also reflected in the direction opposite to its original motion, and "loses" as much force again. Thus a "double diminution". Similarly, the body which gains "force" has a "double augmentation" in elastic impacts.[25]

The importance with which Maclaurin viewed the conservation of "force" is shown by the discussion with which he concludes his analysis of elastic impact. He refers to an argument of William 'sGravesande's, who had pointed out that, according to those who measured "force" by *mv*,

> If the [elastic] body *A* strikes a bigger [elastic] body *B* at rest, the body *B* will have a greater force after the impact, than the body *A* had before impact.

'sGravesande had compounded this effect, considering ten elastic balls with masses in geometric progression, the first striking the second with one degree of force, the second then striking the third, and so on. He concluded that the last ball will, after impact, have 394 times the "force" the first had. For 'sGravesande this seemed to imply that the measure of "force" by *mv* committed its proponents to the possibility of an "effect" exceeding its "cause", and, hence, to the ultimate absordity of perpetual motion.

> But [retorts Maclaurin] one can only be extremely surprised that the author has forgotten that the other ten bodies are reflected in the other direction with 393 degress of force, and that the sum of all the forces, taken in one direction, is by one degree; which entirely reverses his reasoning.

[25] In *The Conflict between Atomism and Conservation Theory,* W. Scott claims that, according to Maclaurin's analysis, "force", as *MV* taken *absolutely,* is *not* lost in elastic impacts (p. 28). This certainly does not follow from Maclaurin's equations (the correct ones for elastic impact), nor is there any indication Maclaurin believed it to be true.

To summarise, then, we can see that in dealing with impact Maclaurin took it for granted, as did the Leibnizians, that any "force" lost by one body would have to be transferred to the other. This presupposition, together with some simple mechanical assumptions about elastic and inelastic bodies, sufficed for him to derive his equations of impact. The difference between him and the Leibnizians was simply that, where they took "force" as measured by the scalar mass times velocity squared, he took it as the vector mass times velocity.

The position adopted by the Cartesian opponents of Leibniz on this point was the same. The issue is clearly put in a controversy between the influential Cartesian J. J. de Mairan and Madame du Châtelet, who defected to the Leibnizian camp in 1740. She used an argument similar to 'sGravesande's, showing that in an elastic impact mass times velocity taken as a scalar quantity could increase. Mairan replied, with a certain lack of courtesy, that

> It is against all the rules for addition, when adding increases of which some have a *plus* sign and others a *minus* sign, as is the case here with the increases [in "force"] after the impact, to add, as you do, those with the plus sign to those with the minus sign, instead of subtracting them, which would never give you anything but the sum of the forces in proportion to the masses times the simple velocities.[26]

6. The Newtonians and the controversy

It will be illuminating to digress at this point and consider something which in one way is the most surprising fact about the *vis viva* controversy. This is that followers of Newton were prepared to use, in a quite uncritical way, the notion of "force of motion" involved in the controversy. For Maclaurin was by no means alone amongst English Newtonians in contributing to the *vis viva* controversy. Samuel Clarke, for instance, Newton's close associate and spokesman on controversial philosophical matters, took a vigorous part in the debate. L. L. Laudan has noted that a significant number of other English scientists were involved, including J. T. Desaguliers, H. Pemberton, C. L'Abelye, and J. Eames.[27]

Why this participation of English Newtonians in the *vis viva* controversy is puzzling is that there is no place in the structure of Newton's mature thought, as expressed in the *Principia,* for the notion of the "force of a body in motion". To see this, compare the second law of motion in Newton's *Principia* with the version that Maclaurin gives in his essay: where Maclaurin says that it is the "change of *force*" which is proportional to the "impressed force", for Newton it is simply the "change of *motion*". There is a corresponding divergence in the ways in which they explain what they mean by

[26] *Lettre de M. de Mairan à Madam***, sur la question des force vives,...* (Paris, 1741), pp. 25–26.
[27] L. Laudin, *op. cit.* p. 131.

"impressed force". For Maclaurin the source of the "impressed force" lies *in* the motion of an impacting body, and it "consumes itself entirely in increasing or decreasing the motion" of another body. Newton, on the other hand, takes pains to insist, in Definition IV of the *Principia,* that an impressed force

> ...consists in the action only, and remains no longer in the body when the action is over.

In the first edition of the *Principia* the idea that a body in motion *has* a "force", of the kind which is impressed on bodies to change their motion, appears only in a few isolated phrases.[28] Why then did Newton's disciples involve themselves to such an extent in the *vis viva* controversy which was about the nature of just such a "force"?

One rather easy explanation which offers itself is that this involvement was something of an historical accident, a by-product of the more general antipathy between the followers of Newton and those of Leibniz. In this way it might be argued that the motivation for Newtonians to maintain that "force" is measured by mass times velocity was simply that Leibniz had said that it was not.

This seems to me a plausible account only in the case of Newton himself. So far as we know, Newton was only involved in the controversy on one occasion (and then not publicly). A. Koyré and I. Cohen have shown that one of the arguments put forward against Leibniz's measure of "force" in the *Leibniz–Clarke Correspondence* is taken, almost *verbatim,* from a manuscript draft in Newton's own hand.[29] The argument in question is to the effect that since "gravity is uniform" it must "impress and communicate to the falling body equal impulsive forces in equal times", and hence the "impulsive force" of a body must be proportional to its velocity, and not its velocity squared. In view of the rather extreme language with which Newton formulates this argument, and given the history of his relationship with Leibniz, I do not think that one need look for any further rationale for Newton's interest in the "force of a body", other than the possibility that it promised to provide him with a stick to beat Leibniz with.[30] However, I think a deeper and more satisfactory explanation can be found for the far more thoroughgoing involvement of later English Newtonians in the *vis viva* controversy. In order to show this I shall first suggest an explanation of why Newton himself did not, in his mature thought, use the concept of "force of motion".

[28]See the remarks following the Third Law of Motion, and also the phrases introducing the section on simple machines, at the end of the Scholium to the Laws of Motion. I. Newton, *Mathematical Principles of Natural Philosophy and his System of the World,* F. Cajori, (ed.) (Berkeley: University of California Press, 1960).

[29]A. Koyré and I. Cohen, "Newton and the Leibniz–Clarke Correspondence", *Archives Internationales d'Histoire des Sciences,* **15** (1962).

[30]This episode explains the insertion of an anomalous passage in the 3rd edition of the *Principia,* at the beginning of the Scholium to the Laws of Motion, which begins:

As has been shown, the participants in the *vis viva* controversy agreed, following Descartes, that a body in motion had a "force" which measured its potentiality to produce changes in the movement of other bodies it might collide with: on coming into contact with another body, any loss of "force" that one body suffered would produce an equal amount of "force" in the other. And it is my contention that this agreement gave enough common content to the notion of "force of motion" to make the *vis viva* controversy a meaningful debate. But, at the same time, because the "force of motion" of a body expressed that body's power to produce changes of motion in *impacts,* the notion of "force" shared by the participants in the *vis viva* controversy could be held to be a fully adequate foundation for physics only as long as it was assumed that all changes of motion had to be explained as due to contact between matter in motion. It is unlikely that Newton shared this assumption. As he says in Definition IV of the *Principia*

> ...impressed forces are from different origins, as from percussion, from pressure, from centripetal force.

It seems probable that it was Newton's desire to produce laws of motion that would deal with the action of attractions and repulsions, as well as the phenomenon of impact, that explains his rejection of the concept of "force of motion", and his replacement of it by the concept of "force" ultimately defined as *anything* which acts on a body to produce a change in its inertial motion, whatever its source.

Indeed, the notion of "force" constructed by Newton is in a sense better suited to dealing with action at a distance, such as attraction or repulsion, than it is for the problem of impact. For the analysis of impact which results from Newton's theory is simply that in every impact the two bodies will be subjected to equal and oppositely directed "impulsive forces". This conception of impact is, if viewed from outside the conceptual scheme of Newtonian physics, difficult to grasp intuitively: for it is scarcely natural to view all impacts as symmetrical in this way, with both bodies, whatever their respective masses and velocities, acting in the same way on the other. And, in addition, the

> When a body is falling the uniform force of its gravity acting equally, impresses in equal intervals of time, equal forces upon that body, and therefore generates equal velocities; and in the whole time impresses a whole force, and generates a whole velocity proportional to the time...

As E. J. Dijkterhuis notes in *The Mechanization of the World Picture,* translated C. Dikshoorn (Oxford: Clarendon Press, 1961), in this passage:

> Newton uses the term 'impressed force' in an altogether different sense from that he had assigned to it in Definition IV. In the latter case it was a force exerted on a body from without, and it was expressly stated that this force did not remain in the body (p. 476).

Dijkterhuis suggests that Newton inserted this passage because he realised that his Laws of Motion, framed in terms of impulse, did not strictly imply that gravity produces a constant acceleration. But, in view of the fact that the inserted passage is almost word for word identical with the argument he drafted for Clarke to use in the *Leibniz-Clarke Correspondence,* it seems more plausible that the point of the insertion was simply its anti-Leibnizian implications.

analysis of impact based simply on the equality of action and reaction does not lead to any very informative results. For instance, Newton's brief treatment of the question of impact in the *Principia* does not imply any determinate solutions for different kinds of bodies; it is interesting to note that for all he says there is nothing to rule out the possibility that MV^2 might increase in some impacts, to any degree whatsoever.

It is this feature of Newton's position that suggests a plausible explanation for many of Newton's nominal followers showing a continued interest in the *vis viva* controversy. For while Newton himself was relatively uninterested in impact, and far more concerned with the analysis of action at a distance, this cannot be said of his disciples: they continued to devote a significant amount of attention to analysing the phenomenon of impact. Because of this the traditional framework for thinking about impact, involving the notion of "force of motion", would have exerted a powerful hold on their minds. And, in consequence, it would have been relatively unlikely that they would have used Newton's concept of "force", designed for action at a distance, in order to deal with impact. I am not, of course, contending that Newton's followers did not accept action at a distance and the associated notion of "force" — for it was their acceptance of this that distinguished the Newtonians from other natural philosophers. What I am saying is merely that they did not apply the notion of "force" involved with action at a distance to their study of impact, but instead continued to work with the old concept of "force of motion" in this area.

Even when Newton's followers acknowledge the "equality of action and reaction" in impact it was, for most of them, only a matter of paying lip-service to Newton's own thinking on the subject. For they were inclined to read this dictum as expressing the "equality of cause and effect" in impact; that is, the fact that any "force of motion" one body lost in acting on another would be entirely transferred to the latter. Thus Maclaurin, in discussing impact between oppositely directed hard bodies, says

> The larger force...destroys the smaller, and in destroying it is itself diminished by a quantity equal to that smaller force, by the third principle [the "equality of action and reaction"]. There remains the difference between the two forces.[31]

Thus Newton's disciples differed from him in that they continued to see impact in terms of that concept of "force of motion" which was constituted in part by the assumption that all impacts consist of the transference of such "force" from one body to another. As such they shared a common framework of thinking about impact with the continental Cartesians and Leibnizians. For notwithstanding their differences about how to measure "force of motion",

[31] C. Maclaurin, *op. cit.* Section III.

they all agreed that any theory of impact would comprise the principle that all changes of motion produced in impact are due to the loss of "force" by one body and the gain of an equivalent amount by the other.

7. Time or distance?

I have argued that the *vis viva* controversy should be understood as focused on the problem of impact. The set of common assumptions about how this problem should be dealt with, and the importance with which the participants viewed it, yield an explanation of what it was that made the continued debate possible. Now that the centrality of impact in the minds of those involved in the *vis viva* controversy has been established, we are also in a better position to understand that aspect of the controversy concerned with the behaviour of bodies in retarded and àccelerated motion.

To a large extent the apparently futile question of whether it is the time or the distance for which a body is accelerated (or decelerated) which measures the "force of motion" acquired (or lost), can be seen as focussing on the possible physical *mechanisms* by which such accelerations are produced. It was generally assumed that the uncovering of the details of such mechanisms would come to show whether uniformly accelerating bodies undergo equivalent physical actions during succeeding units of time, or over succeeding units of space. The former discovery would show "force" to be proportional to MV, the latter that it was proportional to MV^2. Thus,given the general assumption that all action involved physical contact, the apparently empty question of whether time or distance measured force was transformed into a potentially substantial dispute about the properties of the physical mechanisms producing accelerations.

Consider first the arguments about gravity. The Leibnizians said that the height to which a body can elevate itself (which is proportional to its velocity squared) is the effect which measures its "force". Their opponents' response was that since gravity is uniform it takes away equal forces in equal times, and so it is the *time* of ascent (proportional to initial velocity) which measures "force". But to this the Leibnizians replied by simply denying that gravity acts uniformly in destroying "force" (though of course they admitted it produced a uniform deceleration). Clarke himself, in the *Correspondence* refers to Jacob Hermann, who

> ...in his *Phoronomia* (arguing for Mr. Leibniz against those who hold that the forces acquired by falling bodies are proportional to the times of falling) represents that this is founded on a false supposition, that bodies thrown upwards receieve from the gravity which resists them, an equal number of impulses in equal times. Which is as much as to say, that gravity is not uniform;...I suppose, he means that the swifter the motion

> of bodies is upwards, the more numerous are the impulses; because the bodies meet the (imaginary) gravitating particles.[32]

This argument, attributed to Hermann by Clarke, was extremely common. A body ascending against gravity would meet more particles when it was moving faster, at the beginning of its ascent. So, since it is progressively being subject to fewer impacts per unit time, it must lose decreasing amounts of "force" per unit time in its ascent — which, by the laws of free fall, supported the measure of MV^2, rather than MV.

A closely analogous debate took place in connection with the decelerations of heavy bodies dropped into some such substance as clay.[33] 'sGravesande showed experimentally that the volume of the cavity made by a heavy body of given shape dropped into clay is proportional to its MV^2. This was held to support the Leibnizian measure of force (cause must equal effect). But it was objected, in particular by the Swiss mathematician, Calandrin, that in general the resistance of the clay would be uniform, and that therefore it would consume equal "forces" in equal times: from which it followed that

> ...the times during which two forces act on tenacious material until these forces are destroyed will always be proportional to these forces.

'sGravesande was equal to this. He granted the uniformity of the resistance (or "pressure"). But since the "effort of a pressure" depends on both "the pressure *and* the speed of the points or surfaces being struck", more "force" is taken away at first than later, and the total "force" consumed is proportional to the distance. ('sGravesande argued that: "force consumed" in time dt = pressure $\times$ velocity $\times$ dt. And so, by integrating, he concluded that: "total force consumed" = pressure $\times$ distance.) 'sGravesande's argument here can be seen to be essentially the same as that used by other Leibnizians in connection with gravity.

The Leibnizian view received further support from experiments involving dropping weights onto layers of tissue paper. The number of tissues broken was proportional to MV^2. Again the Leibnizians argued that since each tissue must remove an equal amount of "force", it must be MV^2 which measures force. But even to this the Leibnizians' opponents could find an answer. Thus, J. J. de Mairan, arguing for the "old opinion", in 1728, admits that a uniformly retarded body "meets more obstacles" or "received more impressions" per unit time at the beginning of its retardation, when it is moving faster, than at the end. But, nevertheless, he argues, the "force" lost per unit time is constant:

> ...the reason is, that the contrary impulsions, the resistances, or if one wishes, the contrary forces, act against those which surmount them, and

[32] *The Leibniz-Clarke Correspondence*, p. 125.

[33] This debate is described in some detail in T. Hankins, "Eighteenth Century Attempts to Resolve the *Vis Viva* Controversy", pp. 288–291.

> which they consume, so much the more or so much the less, ..., according as they are applied to them for a longer or shorter time.

That is, Mairan suggests that although a faster moving body "meets more obstacles" than a slower, this is compensated for by the fact the obstacles have less time to act on a faster moving body.[34] This response to the Leibnizian analysis became general amongst proponents of the "old opinion", including the English Newtonians.

In the end, of course, the hope of establishing satisfactory detailed mechanistic models for the production of uniform accelerations was frustrated. In particular gravity was a problem. For, unlike clay, gravity does not only decelerate bodies moving against it, but also accelerates bodies moving "with" it. Impact models of gravity were impotent to explain this. Bernoulli to some extent circumvented this difficulty in his *Discours*, by likening graviety to an "elastic matter which extends vertically to infinity" (and arguing for MV^2 *via* his lemma that springs produce forces proportionally to the distance through which they act). But even this more promising reduction of gravitation to action by physical contact had nothing to say about why gravity accelerates large masses as much as small ones. (Bernoulli ignored this problem.)

In the absence of any agreed conclusions about the mechanisms behind uniform accelerations the "time or distance" argument inevitably tended to end up going round in circles. But we have seen that this aspect of the *vis viva* controversy was at least something more than just a muddle about the equations of uniformly accelerated motion. As long as there remained the metaphysical conviction that all changes of motion must be explicable in mechanistic terms, it was perfectly coherent to discuss whether those mechanisms acted uniformly over time, or over space.

8. The End of the Controversy

In the 1740's interest in the question of how to measure "force of motion" waned, and eventually the *vis viva* controversy died away.[35] There were two developments which were influential in bringing this about.

In the first place there was the kind of thinking that led d'Alembert to conclude that "this is a dispute of words". D'Alembert denied that "effects are always equal to causes", in the accepted sense. His reason for thinking this was simply that he did not believe in the existence of any "forces" at all, and so did not accept there was anything to be the "cause" of physical effects. His approach to mechanics was distinctly positivistic: he felt that there was no

[34] J. J. de Mairan, "Dissertation sur l'Estimation et le Mesure des Forces Motrice des Crops", *Histoire de l'Academie des Sciences* (1728), §32.

[35] L. Laudan (*op.cit.*) has pointed out that some scientists continued to debate the subject after the 1740's, thus disproving the view that the controversy stopped entirely in the 1740's.

possibility of access to the "causes" of mechanical phenomena — all we have coherent ideas about are the effects that occur in different situations. As he explains in the preface to his *Traité de Dynamique,*

> ...I have kept away from *motive causes* to consider only the effects they produce...we have no precise and distinct idea of the word *force* unless we restrict this term to express an effect...arguments concerning the measure of forces are entirely useless in mechanics and even without any real object.[36]

It is this view, that the only proper use for the term force is "to express an effect", that led d'Alembert to condemn the "*vis viva* controversy" as a "dispute of words". In his *Traité* he lists a number of effects, which he considers lead to different meanings, and therefore different measures, of "force", (Uniform motion, equilibrium on the lever, and the time in retarded motion, given the measure mv; the "number of springs closed" gives the measure mv^2.)

In a sense, d'Alembert's denial of the existence of "forces" amounts to an epistemological scepticism about the possibility of identifying any abstract entity which could fit into a theory according to which it was a "cause" which necessarily produced proportional "effects". Doubts about the existence of such "causes" were not peculiar to d'Alembert. It is interesting to note similar passages in other writers of the same philosophical bent. Thus Hume, in the *Treatise:*

> ...when we talk of any being...as endowed with a power or force, proportioned to any effect; when we speak of a necessary connection betwixt objects, and suppose that this connection depends on an efficacy or energy, with which any of these objects are endowed; in all the expressions *so applied,* we have really no distinct meaning, and make use of only of common words, without any clear and determinate ideas.[37]

Another development, perhaps more influential amongst scientists in ending the "*vis viva* controversy", did not deny the existence of forces altogether, but sought to replace the concept of "force" associated with the potentiality of matter in motion to act by impact, with that of an "applied force" first introduced by Newton.

The clearest statement of this view is found in the work of the Jesuit, Roger Boscovich. On the question of "force" Boscovich regards himself as a follower of Newton; he admits the existence of attractive and repulsive forces. But he goes beyond Newton in that he denies the possibility of matter *ever* coming into contact. So for him all action is action at a distance. He realises that there are those who "consider that all the phenomena must be explained by impluse

[36] J. d'Alembert, *op. cit.* p. xxi.

[37] D. Hume, *A Treatise of Human Nature* (London, 1738), Book I, Part III, Section XIV.

and immediate contact" (and who "add, by way of a joke in the midst of a serious argument...that a stick would be useful for persuading anyone who denied contact")' But he feels that the "method of explaining phenomena by employing forces acting at a distance" is far superior.[38]

Boscovich is an extreme case of the then growing tendency to accept action at a distance as an irreducible cause of changes in motion. As I argued at the end of Section 6 when discussing Newton, concentrating on action at a distance would inevitably lead scientists to look for a notion of "force" which could deal with all changes of motion, not only those produced in impact. And this meant the abandonment of the notion of "force of motion" placed *within* a body, and its replacement by the idea of something exerted *on* a body to the extent that its motion is changed.

What this meant for Boscovich's analysis of impact is well illustrated by a passage from his *Philosophiae Naturalis Theoria* (1758) where he is discussing an argument designed to show the "force" of bodies in impact is MV^2:

> But in my theory this argument has no weight at all. The sphere *A* does not transfer to the sphere *C*...part of its velocity...; and with it part of its force. There acts on the spheres a new mutual force in opposite directions, which gives the velocity *CE* to one sphere and velocity *BD* to the other; ...and there is not any...transference of living force.[39]

Because Boscovich took external applied forces as primitive he had no difficulty with the behaviour of accelerating bodies. In his early dissertation *De Viribus Vivis* (1745) he showed that change in a body's MV is given by the integral over time of the "applied force" acting on it, and its increase of MV^2 is given by the integral over space. Since he denied the existence of physical mechanisms transferring internal "forces of motion", and indeed that there were such "forces" at all, he did not see there was any further question raised by these results, and concluded that the *vis viva* controversy was merely a dispute of words.[40]

Boscovich illustrates the way in which the growing acceptance of action at a distance as the fundamental mode of physical action in the middle of the eighteenth century contributed to the dissolution of the *vis viva* controversy. It is possible that this hyper-Newtonian trend was interlinked with the positivist trend exemplified by d'Alembert, with the arguments against the epistemological superiority of explanations in terms of transference of "forces" in impact aiding the acceptance of action at a distance. Thus Maupertius, who, along with Voltaire, was one of the earliest proponents of action at a distance in

[38] R. Boscovich, *A Theory of Natural Philosophy,* translated by J. M. Child (London: Open Court, 1922), p. 56.

[39] R. Boscovich, *ibid.* p. 115.

[40] See P. Costabel, "Le *De Viribus Vivis* de R. Boscovich", *Archives Internationales d'Histoire des Sciences,* **41**, (1961).

France, argued in his *Discours sur les Figures des Astres* (1732) that action at a distance was no less logical than impluse, as follows:

> What is this impulsive force? How does it reside in bodies? Who would ever have been able to discover it resided there before seeing bodies in collision? (p. 65)[41]

9. Summary and conclusions

According to the system constructed by Descartes, all changes of motion were to be explained as due to contact between matter in relative motion. Any changes resulting from such contact involved the transference of "force" from one body to another, in such a way that the total amount of "force" would be conserved. The measure of a body's "force" was given by the product of its mass by its speed, taken as a scalar quantity. Exactly how "force" gets transferred in an impact depended on whether the "force" of one body can overcome the "resistance" of the other.

Once Descartes had proposed his theory, some compelling arguments were formulated which showed that there are impacts in which mass times speed is not conserved, and this was confirmed by experimental evidence. This constituted a serious anomaly for the Cartesian framework. There were essentially two alternative proposals as to how the framework should be revised.

On the one hand Leibniz and his followers suggested replacing Descartes' measure of "force" by the quantity mass times velocity squared. In support of this they developed a theory of "living" and "dead forces" which enabled them to explain exactly how "force" is transferred in elastic impacts (and which also enabled them to give a satisfactory analysis of the simple machines).

The "Newtonians" and Cartesians, on the other hand, proposed that mass times velocity, considered as a vector quantity, should be taken as the measure of the "force" of a body in motion. This enabled them, too, to derive equations of impact. Though their views on the nature of matter were less articulated than the Leibnizians', they were not restricted to considering elastic bodies only.

Common to both the Leibnizians and their opponents was the basic structure of the original framework put forward by Descartes. For they all accepted that, whatever the measure of "force", it had to be the case that any "force" lost by one body in an impact would be gained by the other. This bas' agreement meant, firstly, that there was a perfectly real conflict between the ' wo positions in the *vis viva* controversy: for, in that there was agreement on the fundamental structure of the concept of "force", all the participants in the controversy were committed to establishing which was the best amongst the alternative proposals for elaborating that structure. And, at the same time, this agreement meant that there was a stock of common premises which could be used in arguments between the two sides.

[41]Quoted in T. L. Hankins, *Jean d'Alembert,* p. 160.

By avoiding the question of whether "force" meant the same thing for the two sides, we have managed to understand the *vis viva* controversy as a normal scientific dispute about which of two competing views was best equipped to deal with the empirical data. Our procedure has not been to look for the hidden content behind the words used by the disputants, but simply to chart the components of the theoretical space they moved in. It is interesting in this connection to compare the traditional analyses of the controversy given by Cajori and Jammer with the arguments put forward by d'Alembert and Boscovich in the middle of the eighteenth century. In the works of d'Alembert and Boscovich we can see the source of the alternative versions of the theory that the *vis viva* controversy was a dispute of words. Cajori follows d'Alembert in his insistence that "force of motion" can only properly express the various effects of motion, and concludes like him that "force of motion" contains an obvious ambiguity. And Jammer is little different from Boscovich, in maintaining that "force of motion" must be understood as that "applied force" required to produce the motion, and detecting the not quite so obvious ambiguity which depends on whether the applied force is integrated over space or time. These comparisons, however, do not in any way justify the views of Cajori and Jammer. For the historian (as opposed to the theoretical innovator of the time) is scarcely justified in *stipulating* what words should mean. Rather he must find their significance in the theoretical framework actually adopted by the scientists he is studying. Neither Boscovich nor d'Alembert shared the theoretical context within which the *vis viva* controversy took place: for, as we have seen, the precise thrust of their work was to overthrow the existing structure of thought — Boscovich sought to replace it by a more flexible system of physics, while d'Alembert in effect denied the possibility of any systematic physics at all.

The long persistence of the dispute presents no problem for the approach adopted here. It was due to no confusion, or lack of objectivity. It was simply that two alternative modifications of the Cartesian theory of impact were proposed when the latter was seen to be inadequate. Both these alternatives merited consideration, and time was needed for their implications and possible refinements to be explored and evaluated.

Whether there would eventually have been an agreed conclusion to the debate must remain an unanswered question. For, as we have seen, the *vis viva* constroversy closed, not with the victory of one side, but with a fundamental revision of physical thought resulting in the repudiation of both.

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