Jean Le Rond D Alembert | Encyclopedia.com

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(*b*. Paris, France, 17 November 1717; *d*. Pairs, 29 October 1783)

physics, mathematics.

Jean Le Rond d’Alembert was the illegitimate child of Madame de Tencin, a famous salon hostess of the eighteenth century, and the Chevalier DestouchesCanon, a cavalry officer. His mother, who had renounced her nun’s vows, abandoned him, for she feared being returned to a convent. His father, however, located the baby and found him a home with a humble artisan named Rousseau and his wife. D’Alembert lived with them until he was forty-seven years old. Destouches-Canon also saw to the education of the child. D’Alembert attended the Collège de Quatre-Nations (sometimes called after Mazarin, its founder), a Jansenist school offering a curriculum in the classics and rhetoric–and also offering more than the average amount of mathematics. In spite of the efforts of his teachers, he turned against a religious career and began studies of law and medicine before he finally embarked on a career as a mathematician. In the 1740’s he became part of the *philosophes*, thus joining in the rising tide of criticism of the social and intellectual standards of the day. D’Alembert published many works on mathematics and mathematical physics, and was the scientific editor of the *Encyclopédie*.

D’Alembert never married, although he lived for a number of years with Julie de Lespinasse, the one love of his life. A slight man with an expressive face, a high-pitched voice, and a talent for mimicry, he was known for his wit, gaiety, and gift for conversation, although later in life he became bitter and morose. D’Alembert spent his time much as the other *philosophes* did: working during the morning and afternoon and spending the evening in the salons, particularly those of Mme. du Deffand and Mlle. de Lespinasse. He seldom traveled, leaving the country only once, for a visit to the court of Frederick the Great. D’Alembert was a member of the Académie des Sciences, the Académie Française, and most of the other scientific academies of Europe. He is best known for his work in mathematics and rational mechanics, and for his association with the *Encyclopédie*.

D’Alembert appeared on the scientific scene in July 1739, when he sent his first communication to the Acdémie des Sciences. It was a critique of a mathematical text by Father Charles Reyneau. During the next two years he sent the academy five more *mémoires* dealing with methods of integrating differential equations and with the motion of bodies in resisting media. Although d’Alembert had received almost no formal scientific training (at school he had studied Varignon’s work), it is clear that on his own he had become familiar not only with Newton’s work, but also with that of L’Hospital, the Bernoullis, and the other mathematicians of his day. His communications to the academy were answered by Clairaut, who although only four years older than d’Alembert was already a member.

After several attempts to join the academy, d’Alembert was finally successful. He was made *adjoint* in astronomy in May 1741, and received the title of *associé géometre* in 1746. From 1741 through 1743 he worked on various problems in rational mechanics and in the latter year published his famous *Traité de dynamique*. He published rather hastily (a pattern he was to follow all of his life) in order to forestall the loss of priority; Clairaut was working along similar lines. His rivalry with Clairaut, which continued until Clairaut’s death, was only one of several in which he was involved over the years.

The *Traité de dynamique*, which has become the most famous of his scientific works, is significant in many ways. First, it is clear that d’Alembert recognized that a scientific revolution had occurred, and he thought that he was doing the job of formalizing the new science of mechanics. That accomplishment is often attributed to Newton, but in fact it was done over a long period of time by a number of men. If d’Alembert was overly proud of his share, he was at least clearly aware of what was happening in science. The *Traité* also contained the first statement of what is now known as d’Alembert’s principle. D’Alembert was, furthermore, in the tradition that attempted to develop mechanics without using the notion of force. Finally, it was long afterward said (rather simplistically) that in this work he resolved the famous *vis viva* controversy, a statement with just enough truth in it to be plausible. In terms of his own development, it can be said that he set the style he was to follow for the rest of his life.

As was customary at the time, d’Alembert opened his book with a lengthy philosophical preface. It is true that he was not always faithful to the principles he set down in the preface, but it is astonishing that he could carry his arguments as far as he did and remain faithful to them. D’Alembert fully accepted the prevailing epistemology of sensationalism. Taken from John Locke and expanded by such men as Condillac, sensationalism was to be d’Alembert’s metaphysical basis of science. The main tenet of this epistemology was that all knowledge was derived, not from innate ideas, but from sense perception. In many ways, however, d’Alembert remained Cartesian. The criterion of the truth, for example, was still the clear and simple idea, although that idea now had a different origin. In science, therefore, the basic concepts had to conform to this ideal.

In developing his philosophy of mechanics, d'Alembert analyzed the ideas available to him until he came to those that could be analyzed no further; these were to be his starting points. Space and time were such. So simple and clear that they could not even be defined, they were the only fundamental ideas he could locate. Motion was a combination of the ideas of space and time, and so a definition of it was necessary. The word "force" was so unclear and confusing that it was rejected as a conceptual building block of mechanics and was used merely as a convenient shorthand when it was properly and arbitrarily defined. D'Alembert defined matter as impenetrable extension, which took account of the fact that two objects could not pass through one another. The concept of mass, which he defined, as Newton had done, as quantity of matter, had to be smuggled into the treatise in a mathematical sense later on.

In the first part of the *Traité*, d'Alembert developed his own three laws of motion. It should be remembered that Newton had stated his laws verbally in the *Principia*, and that expressing them in algebraic form was a task taken up by the mathematicians of the eighteenth century. D'Alembert's first law was, as Newton's had been, the law of inertia. D'Alembert, however, tried to give an a priori proof for the law, indicating that however sensationalistic his thought might be he still clung to the notion that the mind could arrive at truth by its own processes. His proof was based on the simple ideas of space and time; and the reasoning was geometric, not physical, in nature. His second law, also proved as a problem in geometry, was that of the parallelogram of motion. It was not until he arrived at the third law that physical assumptions were involved.

The third law dealt with equilibrium, and amounted to the principle of the conservation of momentum in impact situations. In fact, d'Alembert was inclined to reduce every mechanical situation to one of impact rather than resort to the effects of continual forces; this again showed an inheritance from Descartes. D'Alembert's proof rested on the clear and simple case of two equal masses approaching each other with equal but opposite speeds. They will clearly balance one another, he declared, for there is no reason why one should overcome the other. Other impact situations were reduced to this one; in cases where the masses or velocities were unequal, the object with the greater quantity of motion (defined as mv) would prevail. In fact, d'Alembert's mathematical definition of mass was introduced implicitly here; he actually assumed the conservation of momentum and defined mass accordingly. This fact was what made his work a mathematical physics rather than simply mathematics.

The principle that bears d'Alembert's name was introduced in the next part of the *Traité*. It was not so much a principle as it was a rule for using the previously stated laws of motion. It can be summarized as follows: In any situation where an object is constrained from following its normal inertial motion, the resulting motion can be analyzed into two components. One of these is the motion the object actually takes, and the other is the motion "destroyed" by the constraints. The lost motion is balanced against either a fictional force or a motion lost by the constraining object. The latter case is the case of impact, and the result is the conservation of momentum (in some cases, the conservation of *vis viva* as well). In the former case, an infinite force must be assumed. Such, for example, would be the case of an object on an inclined plane. The normal motion would be vertically downward; this motion car. be resolved into two others. One would be a component down the plane (the motion actually taken) and the other would be normal to the surface of the plane (the motion destroyed by the infinite resisting force of the plane). Then one can easily describe the situation (in this case, a trivial problem).

It is clear that the use of d'Alembert's principle requires some knowledge beyond that of his laws. One must have the conditions of constraint. or the law of falling bodies, or some information derived either empirically or hypothetically about the particular situation. It was for this reason that Ernst Mach could refer to d'Alembert's principle as a routine form for the solution of problems, and not a principle at all. D'Alembert's principle actually rests on his assumptions of what constitutes equilibrium, and it is in his third law of motion that those assumptions appear. Indeed, in discussing his third law (in the second edition of his book, published in 1758) d'Alembert arrived at the equation $\phi = dv/dt$, which is similar to the standard expression for Newton's second law, but which lacks the crucial parameter of mass. The function ϕ was to contain the parameters for specific problems. For example (and this is d'Alembert's example), should the assumption be made that a given deceleration is proportional to the square of the velocity of an object, then the equation becomes $-gv^2=dv/dt$. The minus sign indicates deceleration, and the constant g packs in the other factors involved, such as mass. In this fashion d'Alembert was able to avoid dealing with forces.

It has often been said that d'Alembert settled the *vis viva* controversy in this treatise, but such a view must be qualified. In the preface d'Alembert did discuss the issue, pointing out that in a given deceleration the change in velocity was proportional to the time. One could therefore define force in terms of the velocity of an object. On the other hand, if one were concerned with the number of "obstacles" that had to be overcome to stop a moving body (here he probably had in mind 'sGravesande's experiments with objects stopped by springs), then it was clear that such a definition of force depended on the square of the velocity and that the related metric was distance, not time. D'Alembert pointed out that these were two different ways of looking at the same problem, that both methods worked and were used with success by different scientists. To use the word "force" to describe either mv *or* mv^2 was therefore a quarrel of words; the metaphysical notion of force as a universal causal agent was not clarified by such an argument. In this way d'Alembert solved the controversy by declaring it a false one. It involved convention, not reality, for universal causes (the metaphysical meaning of the idea of force) were not known, and possibly not even knowable. It was for this reason that d'Alembert refused to entertain the possibility of talking of forces in mechanics. He did not throw the word away, but used it only when he could give it what today would be called an operational definition. He simply refused to give the notion of force any metaphysical validity and, thus, any ontological reality.

In this way d'Alembert was clearly a precursor of positivistic science. He employed mathematical abstractions and hypothetical or idealized models of physical phenomena and was careful to indicate the shortcomings of his results when they did not closely match the actual events of the world. The metaphysician, he warned in a later treatise, too often built systems that might or might not reflect reality, while the mathematician too often trusted his calculations, thinking they represented the whole truth. But just as metaphysics was suspect because of its unjustified claim to knowledge, so mathematics was suspect in its similar claim. Not everything could be reduced to calculation.

Geometry owes its certainty to the simplicity of the things it deals with; as the phenomena become more complicated, the results become less certain. It is necessary to know when to stop, when one is ignorant of the thing being studied, and one must not believe that the words *theorem and corollary* have some secret virtue so that by writing QED at the end of a proposition one proves something that is not true *[Essai d'une nouvelle thèorie de la rèsistance des fluides*. pp. xlii-xliii]. D'Alembert's instincts were good. Unfortunately, in this case they diverted him from the path that was eventually to produce the principle of the conservation of energy.

A major question that beset all philosophers of the Enlightenment was that of the nature of matter. While d'Alembert's primary concern was mathematical physics, his epistemology of sensationalism led him to speculate on matter theory. Here again, he was frustrated, repeating time after time that we simply do not know what matter is like in its essence. He tended to accept the corpuscular theory of matter, and in Newton's style; that is, he conceived of the ideal atom as perfectly hard. Since this kind of atom could not show the characteristic of elasticity, much less of other chemical or physical phenomena, he was sorely perplexed. In his *Traité de dynamique*, however, he evolved a model of the atom as a hard particle connected to its neighbors by springs. In this way, he could explain elasticity, but he never confused the model with reality. Possibly he sensed that his model actually begged the question, for the springs became more important that the atom itself, and resembled nothing more than a clumsy ether, the carrier of an active principle. Instead of belaboring the point, however, d'Alembert soon returned to mathematical abstraction, where one dealt with functional relations and did not have to agonize over ontology.

In 1744 d'Alembert published a companion volume to his first work, the *Traité de l'équilibre et du mouvement des fluides*. In this work d'Alembert used his principle to describe fluid motion, treating the major problems of fluid mechanics that were current. The sources of his interest in fluids were many. First, Newton had attempted a treatment of fluid motion in his *Principia*, primarily to refute Descartes's *tourbillon* theory of planetary motion. Second, there was a lively interest in fluids by the experimental physicists in the eighteenth century, for fluids were most frequently invoked to give physical explanations for a variety of phenomena, such as electricity, magnetism, and heat. There was also the problem of the shape of the earth; What shape would it be expected to take if it were thought of as a rotating fluid body? Clairaut published a work in 1744 which treated the earth as such, a treatise that was a landmark in fluid mechanics. Furthermore, the *vis viva* controversy was often centered on fluid flow, since the quantity of *vis viva* was used almost exclusively by the Bernoullis in their work on such problems. Finally, of course, there was the inherent interest in fluids themselves. D'Alembert's first treatise had been devoted to the study of rigid bodies; now he was giving attention to the other class of matter, the fluids. He was actually giving an alternative treatment to one already published by Daniel Bernoulli, and he commented that both he and Bernoulli usually arrived at the same conclusions. He felt that his own method was superior. Bernoulli did not agree.

In 1747 d'Alembert published two more important works, one of which, the *Réflexions sur la cause générale des vents*, won a prize from the Prussian Academy. In it appeared the first general use of partial differential equations in mathematical physics. Euler later perfected the techniques of using these equations. The pattern was to become a familiar one: d'Alembert, Daniel Bernoulli, or Clairaut would pioneer a technique, and Euler would take it far beyond their capacity to develop it. D'Alembert's treatise on winds was the only one of his works honored by a prize and, ironically, was later shown to be based on insufficient assumptions. D'Alembert assumed that wind patterns were the result of tidal effects on the atmosphere, and he relegated the influence of heat to a minor role, one that caused only local variations from the general circulation. Still, as a work on atmospheric tides it was successful, and Lagrange continued to praise d'Alembert's efforts many years later.

D'Alembert's other important publication of 1747 was an article in the *Mémoirs* of the Prussian Academy dealing with the motion of vibrating strings, another problem that taxed the minds of the major mathematicians of the day. Here the wave equation made its first appearance in physics. D'Alembert's mathematical instincts led him to simplify the boundary conditions, however, to the point where his solution, while correct, did not match well the observed phenomenon. Euler subsequently treated the same problem more generally; and although he was no more correct than d'Alembert, his work was more useful.

During the late 1740's, d'Alembert, Clairaut, and Euler were all working on the famous three-body problem, with varying success. D'Alembert's interest in celestial mechanics thus led him, in 1749, to publish a masterly work, the *Recherches sur la précession des équinoxes et sur la nutation de la terre*. The precession of the equinoxes, a problem previously attacked by Clairaut, was very difficult. D'Alembert's method was similar to Clairaut's, but he employed more terms in his integration of the equation of motion and arrived at a solution more in accord with the observed motion of the earth. He was rightly proud of his book.

D'Alembert then applied himself to further studies in fluid mechanics, entering a competition announced by the Prussian Academy. He was not awarded the prize; indeed, it was not given to anybody. The Prussian Academy took this action on the ground that nobody had submitted experimental proof of the theoretical work. There has been considerable dispute over this action. The claim has been made that d'Alembert's work, although the best entered, was marred by many errors. D'Alembert himself viewed his denial as the result of Euler's influence, and the relations between the two men deteriorated furhter. Whatever the case, the disgruntled d'Alembert published his work in 1752 as the *Essai d'une nouvelle théorie de la résistance* *des fluides*. It was in this essay that the differential hydrodynamic equations were first expressed in terms of a field and the hydrodynamic paradox was put forth.

In studying the flow lines of a fluid around an object (in this case, an elliptical object), d'Alembert could find no reason for assuming that the flow pattern was any different behind the object than in front of it. This implied that whatever the forces exerted on the front of the object might be, they would be counteracted by similar forces on the back, and the result would be no resistance to the flow whatever. The paradox was left for his readers to solve. D'Alembert had other difficulties as well. He found himself forced to assume, in order to avoid the necessity of allowing an instantaneous change in the velocity of parts of the fluid moving around the object, that a small portion of the fluid remained stagnant in front of the object, an assumption required to prevent breaking the law of continuity.

In spite of these problems, the essay was an important contribution. Hunter Rouse and Simon Ince have said that d'Alembert was the first "to introduce such concepts as the components of fluid velocity and acceleration, the differential requirements of continuity, and even the complex numbers essential to modern analysis of the same problem." Clifford Truesdell, on the other hand, thinks that most of the credit for the development of fluid mechanics must be granted to Euler; thus historians have continued the disputes that originated among the scientists themselves. But it is often difficult to tell where the original idea came from and who should receive primary recognition. It is certain, however, that d'Alembert, Clairaut, Bernoulli, and Euler were all active in pursuing these problems, all influenced one another, and all deserve to be remembered, although Euler was no doubt the most able of the group. But they all sought claims to priority, and they guarded their claims with passion.

D'Alembert wrote one other scientific work in the 1750's, the *Recherches sur différens points importants du système du monde*. It appeared in three volumes, two of them published in 1754 and the third in 1756. Devoted primarily to the motion of the moon (Volume III included a new set of lunar tables), it was written at least partially to guard d'Alembert's claims to originality against those of Clairaut. As was so often the case, d'Alembert's method was mathematically more sound, but Clairaut's method was more easily used by astronomers.

The 1750's were more noteworthy in d'Alembert's life for the development of interests outside the realm of mathematics and physics. Those interests came as a result of his involvement with the *Encyclopédie*. Denis Diderot was the principal editor of the enterprise, and d'Alembert was chosen as the science editor. His efforts did not remain limited to purely scientific concerns, however. His first literary task was that of writing the *Discours préliminaire* of the *Encyclopédie*, a task that he accomplished with such success that its publication was largely the reason for his acceptance into the Académie Française in 1754.

The *Discours préliminaire*, written in two parts, has rightly been recognized as a cardinal document of the Enlightenment. The first part is devoted to the work as an *encyclopédie*, that is, as a collection of the knowledge of mankind. The second part is devoted to the work as a *dictionnaire raisonnée*, or critical dictionary. Actually, the first part is an exposition of the epistemology of sensationalism, and owes a great deal to both John Locke and Condillac. All kinds of human knowledge are discussed, from scientific to moral. The sciences are to be based on physical perception, and morality is to be based on the perception of those emotions, feelings, and inclinations that men can sense within themselves. Although d'Alembert gives lip service to the truths of religion, they are clearly irrelevant and are acknowledged only for the sake of the censors. For this reason, the *Discours prèliminaire* came under frequent attack; nevertheless, it was generally well received and applauded. It formed, so to say, the manifesto of the now coalescing party of *philosophes;* the body of the *Encyclopédie* was to be the expression of their program.

The second part of the *Discours préliminaire* is in fact a history of science and philosophy, and clearly shows the penchant of the *philosophes* for the notion of progress through the increased use of reason. As a history, it has often quite properly been attacked for its extreme bias against the medieval period and any form of thought developed within the framework of theology, but this bias was, of course, intentional. At the end of this history, the *philosophes'* debt to Francis Bacon is clearly acknowledged in the outline of the organization of knowledge. A modified version of Bacon's tree of knowledge is included and briefly explained. All knowledge is related to three functions of the mind: memory, reason, and imagination. Reason is clearly the most important of the three. Bacon's emphasis on utility was also reflected in the *Encyclopédie*, although more by Diderot than by d'Alembert. D'Alembert's concept of utility was far wider than that of most people. To him, the things used by philosophers–even mathematical equations–were very useful, even though the bulk of the public might find them mysterious and esoteric.

In the midst of this activity, d'Alembert found time to write a book on what must be called a psychophysical subject, that of music. In 1752 he published his *Élémens de musique théorique et pratique suivant les principes de M. Rameau*. This work has often been neglected by historians, save those of music, for it was not particularly mathematical and acted as a popularization of Rameau's new scheme of musical structure. Yet it was more than simply a popularization. Music was still emerging from the mixture of Pythagorean numerical mysticism and theological principles that had marked its rationale during the late medieval period. D'Alembert understood Rameau's innovations as a liberation; music could finally be given a secular rationale, and his work was important in spreading Rameau's ideas throughout Europe.

As time went on, d'Alembert's pen was increasingly devoted to nonscientific subjects. His articles in the *Encyclopédie* reached far beyond mathematics. He wrote and read many essays before the Académie Française; these began to appear in print as early as 1753. In that year he published two volumes of his *Mélanges de littérature et de philosophie*. The first two were reprinted along with two more in 1759; a fifth and last volume was published in 1767. The word *mélanges* was apt, for in these volumes were essays on music, law, and religion, his treatise on the *Élémens de philosophie*, translations of portions of Tacitus, and other assorted literary efforts. They make an odd mixture, for some are important in their exposition of Enlightenment ideals, while others are mere polemics or even trivial essays.

In 1757 d'Alembert visited Voltaire at Ferney, and an important result of the visit was the article on Geneva, which appeared in the seventh volume of the *Encycloédie*. It was clearly an article meant to be propaganda, for the space devoted to the city was quite out of keeping with the general editorial policy. In essence, d'Alembert damned the city by praising it. The furor that resulted was the immediate cause of the suspension of the license for the *Encyclopédie*. D'Alembert resigned as an editor, convinced that the enterprise must founder, and left Diderot to finish the task by himself. Diderot thought that d'Alembert had deserted him, and the relations between the men became strained. Rousseau also attacked d'Alembert for his view that Geneva should allow a theater, thus touching off another of the famous controversies that showed that the *philosophes* were by no means a totally unified group of thinkers.

D'Alembert's chief scientific output after 1760 was his *Opuscules mathématiques*, eight volumes of which appeared from 1761 to 1780. These collections of mathematical essays were a mixed bag, ranging from theories of achromatic lenses to purely mathematical manipulations and theorems. Included were many new solutions to problems he had previously attacked–including a new proof of the law of inertia. Although the mathematical articles in the *Encyclopédie* had aired many of his notions, these volumes provide the closest thing to a collection of them that exists.

As Carl Boyer has pointed out, d'Alembert was almost alone in his day in regarding the differential as the limit of a function, the key concept around which the calculus was eventually rationalized. Unfortunately, d'Alembert could never escape the tradition that had made geometry preeminent among the sciences, and he was therefore unable to put the idea of the limit into purely algorithmic form. His concept of the limit did not seem to be any more clear to his contemporaries than other schemes invented to explain the nature of the differential.

It has often been said that d'Alembert was always primarily a mathematician and secondarily a physicist. This evaluation must be qualified. No doubt he sensed the power of mathematics. But, as he once said, "Mathematics owes its certainty to the simplicity of the things with which it deals." In other words, d'Alembert was never able to remove himself to a world of pure mathematics. He was rather in the tradition of Descartes. Space was the realization of geometry (although, unlike Descartes. d'Alembert drew his evidence fró;m sense perception). It was for this reason that he could never reduce mathematics to pure algorithms, and it is also the reason for his concern about the law of continuity. In mathematics as well as physics, discontinuities seemed improper to d'Alembert; equations that had discontinuities in them gave solutions that he called "impossible," and he wasted no time on them. It was for this reason that the notion of perfectly hard matter was so difficult for him to comprehend, for two such particles colliding would necessarily undergo sudden changes in velocity, something he could not allow as possible.

It was probably the requirement of continuity that led d'Alembert to his idea of the limit, and it also led him to consider the techniques of handling series. In Volume V of the *Opuscules* he published a test for convergence that is still called d'Alembert's theorem. The mathematical statement is:

If and $r < 1$, the series converges. If $r > 1$, the series diverges; if $r = 1$, the test fails.

But in spite of such original contributions to mathematical manipulation, d'Alembert's chief concern was in making this language not merely descriptive of the world, but congruent to it. The application of mathematics was a matter of considering physical situations, developing differential equations to express them, and then integrating those equations. Mathematical physicists had to invent much of their procedure as they went along. Thus, in the course of his work, d'Alembert was able to give the first formulation of the wave equation, to express the first partial differential equation, and to be the first to solve a partial differential equation by the technique of the separation of variables. But probably the assignment of "firsts" in this way is not the best manner of evaluating the development of mathematics or of mathematical physics. For every such first, one can find other men who had alternative suggestions or different ways of expressing themselves, and who often wrote down similar but less satisfactory expressions.

More important, possibly, is the way in which these ideas reflect the mathematicians' view of nature, a view that was changing and was then very different from that of a mathematical physicist today. D'Alembert's very language gives a clue. He used, for example, the word *fausse* to describe a divergent series. The word to him was not a bare descriptive term. There was no match, or no useful match, for divergence in the physical world. Convergence leads to the notion of the limit; divergence leads nowhere–or everywhere.

D'Alembert has often been cited as being oddly ineffective when he considered probability theory. Here again his view of nature, not his mathematical capabilities, blocked him. He considered, for example, a game of chance in which Pierre and Jacques take part. Pierre is to flip a coin. If heads turns up on the first toss, he is to pay Jacques one *écu*. If it does not turn up until the second toss, he is to pay two *écus*, If it does not turn up until the third toss, he is to pay four *écus*, and so on, the payments mounting in geometric progression. The problem is to determine how many *écus* Jacques should give to Pierre before the game begins in order that the two men have equal chances at breaking even. The solution seemed to be that since the probability on each toss was one-half, and since the number of tosses was unlimited, then Jacques would have to give an infinite number of *écus* to Pierre before the game began, clearly a paradoxical situation.

D'Alembert rebelled against this solution, but had no satisfactory alternative. He considered the possibility of tossing tails one hundred times in a row. Metaphysically, he declared, one could imagine that such a thing could happen; but one could not realistically imagine it happening. He went further: heads, he declared, must *necessarily* arise after a finite number of tosses. In other words, any given toss is influenced by previous tosses, an assumption firmly denied by modern probability theory. D'Alembert also said that if the probability of an event were very small, it could be treated as nothing, and therefore would have no relevance to physical events. Jacques and Pierre could forget the mathematics; it was not applicable to their game.

It is no wonder that such theorizing caused d'Alembert to have quarrels and arguments with others. Moreover, there were reasons for interest in probability outside games of chance. It had been known for some time that if a person were inoculated with a fluid taken from a person having smallpox, the result would usually be a mild case of the disease, followed by immunity afterward. Unfortunately, a person so inoculated occasionally would develop a more serious case and die. The question was posed: Is one more likely to live longer with or without inoculation? There were many variables, of course. For example, should a forty-year-old, who was already past the average life expectancy, be inoculated? What, in fact, was a life expectancy? How many years could one hope to live, from any given age, both with and without inoculation? D'Alembert and Daniel Bernoulli carried on extensive arguments about this problem. What is significant about d'Alembert's way of thinking is that he expressed the feeling that the laws of probability were faint comfort to the man who had his child inoculated and lost the gamble. To d'Alembert, that factor was as important as any mathematical ratio. It was not, as far as he was concerned, irrelevant to the problem.

Most of these humanitarian concerns crept into d'Alembert's work in his later years. Aside from the *Opuscules*, there was only one other scientific publication after 1760 that carried his name: the *Nouvelles experiences sur la résistance des fluides* (published in 1777). Listed as coauthors were the Abbé Bossut and Condorcet. The last two actually did all of the work; d'Alembert merely lent his name.

In 1764 d'Alembert spent three months at the court of Frederick the Great. Although frequently asked by Frederick, d'Alembert refused to move to Potsdam as president of the Prussian Academy. Indeed, he urged Frederick to appoint Euler, and the rift that had grown between d'Alembert and Euler was at last repaired. Unfortunately, Euler was never trusted by Frederick, and he left soon afterward for St. Petersburg, where he spent the rest of his life.

In 1765 d'Alembert published his *Histoire de la destruction des Jésuites*. The work was seen through the press by Voltaire in Geneva, and although it was published anonymously, everyone knew who wrote it. A part of Voltaire's plan *écraser l'infâme*, this work is not one of d'Alembert's best.

In the same year, d'Alembert fell gravely ill, and moved to the house of Mlle. de Lespinasse, who nursed him back to health. He continued to live with her until her death in 1776. In 1772 he was elected perpetual secretary of the Académie Françise, and undertook the task of writing the eulogies for the deceased members of the academy. He became the academy's most influential member, but, in spite of his efforts, that body failed to produce anything noteworthy in the way of literature during his preeminence. D'Alembert sensed his failure. His later life was filled with frustration and despair, particularly after the death of Mlle. de Lespinasse.

Possibly d'Alembert lived too long. Many of the *philosophes* passed away before he did, and these who remained alive in the 1780's were old and clearly not the vibrant young revolutionaries they had once been. What political success they had tasted they had not been able to develop. But, to a large degree, they had, in Diderot's phrase, "changed the general way of thinking."

BIBLIOGRAPHY

I. Original Works. There have been no collections made of d'Alembert's scientific works, although reprints of the original editions of his scientific books (except the *Opuscules mathématiques)* have recently been issued by Éditions Culture et Civilisation, Brussels. There are two collection of d'Alembert's *Oeuvres* which contain his literary pieces: the Bélin ed., 18 vols. (Paris, 1805); and the Bastien ed., 5 vols. (Paris, 1821). The most recent and complete bibliographies are in Grimsley and Hankins (see below).

II. Secondary Literature. The following works are devoted primarily to d'Alembert or accord him a prominent role: Joseph Bertrand, *D'Alembert* (Paris, 1889); Carl Boyer, *The History of the Calculus and Its Conceptual Development* (New York, 1949), ch. 4; René Dugas, *A History of Mechanics* (Neuchâtel, 1955), pp. 244–251, 290–299; Ronald Grimsley, *Jean d'Alembert* (Oxford, 1963); Maurice Müller, *Essai sur la philosophie de Jean d'Alembert* (Paris, 1926); Hunter Rouse and Simon Ince, *A History of Hydraulics* (New York, 1963), pp. 100–107; Clifford Truesdell, *Continuum Mechanics*, 4 vols. (New York, 1963–1964); and Arthur Wilson, *Diderot: The Testing Years* (New York, 1957). Of the above, Boyer, Dugas, Rouse and Ince, and particularly Truesdell, deal specifically and in detail with d'Alembert's science.

Three recent doctoral dissertations on d'Alembert are J. Morton Briggs, *D'Alembert: Mechanics, Matter, and Morals* (New York, 1962): Thomas Hankins, *Jean d Alembert, Scientist and Philosopher* (Cornell University, 1964); and Harold Jarrett, *D'Alembert and the Encyclopédie* (Durham, N.C., 1962).

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