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EIGHTEENTH-CENTURY SCIENCE AND THE *CORPS SONORE:* THE SCIENTIFIC BACKGROUND TO RAMEAU'S PRINCIPLE OF HARMONY

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There was nothing Jean-Philippe Rameau held more sacred in his music theory than the *corps sonore.* The *corps sonore* (literally the "sonorous body") was Rameau's term for any vibrating system such as a vibrating string which emitted harmonic partials above its fundamental frequency. Its importance in Rameau's theory can scarcely be exaggerated. Rameau was convinced, the good Cartesian that he was, that music was governed by rational laws, and that these laws could be deduced with geometric rigor from a single principle. He believed the theorist's most critical task was to identify this unique principle and to demonstrate its musical consequences. And in all of his theoretical publications save his first, the *corps sonore* served as this principle. In treatise after treatise, Rameau would attempt anew to prove that the *corps sonore* was the single unique principle of music, how it alone bore all elements and rules governing musical practice. Indeed, this theme became something of an *idée fixe* in his writings. Nothing else so dominated his thought. Rameau's fascination with the *corps sonore* became by the end of his life an obsession; the *corps sonore* assumed cosmic proportions in his writings as a veritable icon, the progenitor of all the arts, sciences, and even religion.

Notwithstanding these metaphysical excesses, Rameau always believed that the *corps sonore* was first and foremost a scientific verity, definable with mathematical language and confirmable by empirical observation. And he had good reason for this belief. We know that throughout his life, Rameau actively sought the support and criticism of leading scientists and academies for his music theory. Through these scientific contacts, Rameau learned of physical and mathematical theories which seemed to give credence to his principle of the *corps sonore*. It is true that Rameau did not evince any sophisticated understanding of these scientists's research, leading him at times to make unfortunate misstatements of fact and encouraging the widely-held view among his contemporaries that the composer was something of a scientist *manquée*, a view still quite common among historians today.[1] But this is an unfair judgment. As this paper argues, Rameau accurately reported—and effectively incorporated into his theory—much of the most progressive scientific research of his day.

This fact is critical for our own understanding of Rameau's music theory proper. Many of the composer's most sophisticated theoretical formulations (including his derivations of the minor triad, dissonance, and mode) were predicated upon—and thus can only be understood when analyzed alongside—the scientific definition he gave to the *corps sonore*. As the scientific research upon which Rameau relied changed during his lifetime (which it did at an unprecedented rate) Rameau was likewise forced to change his definition of the *corps sonore*, and as a consequence, those relevant portions of his theory. To be sure, some scientists objected strenuously to Rameau's appropriation of their research; but there were just as many who were more than willing to lend a hand to the composer in his quest for an acoustical explanation of tonal harmony. The noisy and at times heated debate that Rameau's theory provoked among these scientists is one which resounds even today.

At the beginning of the eighteenth century, overtones were a well-confirmed but ill-understood empirical phenomenon.[2] The most advanced scientific theory of the day, contained in the writings of the French scientist Joseph Sauveur, correctly analyzed the vibrating string as a composite of harmonically related "modes."[3] The fundamental mode of a string constitutes the oscillations of the entire string, while the higher modes comprise successive aliquot divisions of the string. But while Sauveur was correct in equating the upper modes with the higher frequencies of overtones, he could offer no plausible explanation mathematically or mechanically how such modes could coexist. According to the then-accepted formula proposed by the English scientist Brook Taylor (to determine the frequency of a given vibrating string), the shape of any vibrating string should be sinusoidal.[4] Thus, scientists in the eighteenth century were challenged to explain how the empirical phenomenon of overtones could be reconciled with the understood behavior of the vibrating string.

When Rameau wrote his first treatise of music, the *Traité de l'harmonie* of 1722, he was unaware of Sauveur's work. Located in the remote provincial city of Clermont, Rameau obviously had little chance to be informed of the research undertaken by scientists at the Parisian *Académie royale des sciences*. Had Rameau lived in Paris, in all likelihood he would have learned much earlier of Sauveur's work, as in fact he did soon after moving to Paris in 1722. As it was, Rameau based his *Traité* upon the time-honored tool of *musica theorica:* the monochord. By dividing a monochord string into successive aliquot divisions (specifically up to the eighth division, skipping the seventh), Rameau was able to construct a major triad. By invoking octave equivalence, he further claimed that any inversion, doubling, or spacing of this triad would not alter its identity. More significantly, though, he insisted that the undivided string had real musical significance as a generative fundamental, and that this fundamental remained the same for the chord in any form. This was the theoretical origin Rameau gave to his *basse fondamentale.*. Of course, Rameau's string divisions directly produced only the major triad. To produce the minor triad and various dissonant chords, while at the same time insisting that they, too, possess generative fundamentals analogous to the major triad, Rameau found it necessary to juggle the ratios of his initial monochord divisions. Notwithstanding his awkward and often laborious number manipulations, Rameau's musical premise was clear: all chords had definable roots, these roots remained constant even if the acoustical bass of the chord differs from it, and finally, roots succeeded one another by a small number of interval progressions, intervals essentially the same as those derived by aliquot string divisions.

The theoretical import of Rameau's fundamental bass, as well as its real compositional and pedagogical value was quickly recognized by musicians. One of Rameau's earliest admirers was the eccentric Jesuit, Louis-Bertrand Castel. In an extensive review written for the influential *Journal de Trévoux,* Castel enthusiastically reported on the fundamental bass. He noted in passing that the aliquot string divisions Rameau used as his principle of harmony occur naturally in any vibrating string:

> Not only may a string produce at the same time two sounds an octave apart, but additionally three and four [sounds], and without doubt the six [sounds] UT, UT, SOL, UT, MI, SOL. It is a fact attested to by M. Sauveur that when one plucks a long string in the still of the night, one hears the 12th UT, UT, SOL, and often even the 17th UT, UT, SOL, UT, MI, and in trumpets, one may hear even further, so that in physics, nature gives us the same system which M. Rameau has discovered in numbers. . .[5]

Needless to say, Rameau must have been delighted by this revelation. Had he known of this fact when writing the *Traité*, there is no doubt that he would have utilized it as his principle of harmony. Not only did the series of harmonic overtones offer a more "natural" origin for the major triad, it also provided a more secure definition of a chord root: a root was nothing less than an acoustical generator. Immediately upon learning of the overtone series, Rameau seized upon it as his long-sought principle of harmony.

Before the ink barely had time to dry in the *Traité*, Rameau was at work on a second treatise to announce his new principle of harmony. This was to be the *Nouveau Système de musique theorique* of 1726. Not coincidentally, the title he chose was taken from Fontenelle's report on Sauveur's work.[6] Rameau began his *Nouveau système* with these portentous words:

> There is actually in us a germ of harmony which apparently has not been noticed until now. It is nonetheless easily perceived in a string or a pipe, etc. whose resonance produces three different sounds at once. Supposing this same effect in all sonorous bodies, one ought logically to suppose it in the sound of our voice, even if it is not evident.[7]

The three sounds contained in the fundamental frequency of every "*corps sonoré*" Rameau insisted, are always the octave, the perfect twelfth and the major seventeenth.[8] With great self-confidence, he concluded, "We believe we are thus able to propose this experiment as a fact which will serve us as a principle for establishing all our consequences."[9]

Despite all the fanfare, the *Nouveau système* did not really develop his new principle of the *corps sonore.* The remainder of the treatise served as a supplement and elaboration of the *Traité.*[10] Important new theoretical ideas were introduced in the *Nouveau système,* among them the geometric proportion and the subdominant function. However, there was no substantial theoretical investigation of the *corps sonore.* Rameau evidently needed time to work out the musical implications of his new acoustic principle. It was not until 1737 that he published a treatise on harmony fully exploiting the *corps sonore* as a theoretical basis.

Shortly after arriving in Paris, Rameau had made the acquaintance of the scientist Jean Jacques Dortous de Mairan (1678–1771). Mairan had been censor for the *permis d'imprimes* at the time Rameau published his first book of *Pièces de clavecin* in 1724.[11] Rameau's friendship with Mairan would prove to be of great consequence in future years. Mairan was one of the leading members of the *Académie royale des sciences.* Succeeding Fontenelle as *secrétaire perpétuel* in 1741, Mairan had long interested himself in acoustics. As early as 1715 he had written on the subject. Although one of the last major defenders of Cartesian physics in the Academy, he was acquainted with much of Newton's work and in fact early in the century helped promote his optical theories.[12] He was particularly fascinated by a suggestion Newton had made in the *Opticks* equating the wave spectrum of colors to the ratios of the diatonic scale. Newton's "color-sound" analogy stimulated much discussion in the eighteenth century and was a subject to which Mairan made frequent reference, although he was dubious of its validity.[13]

Mairan's most original contribution to acoustics was a novel hypothesis of sound propagation outlined in a lengthy paper read before the Academy in 1737.[14] In his paper, Mairan was interested in explaining, among other things, the paradox of sound propagation. How was it that various sounds of differing pitch could be transmitted simultaneously through the air to the ear without any apparent interference? For a number of ill-conceived reasons, Mairan rejected the then-prevalent wave theory which explained sound waves as analogous to the small circles one sees when pebbles are thrown into a still pond. Instead, Mairan proposed an entirely mechanistic theory, one revealing his deep-rooted Cartesianism. His theory was based upon the atomistic theories held by such seventeenth-century scientists as Gassendi and de La Hire. Mairan hypothesized that air is a composite of different-sized atomic particles, each particle capable of vibrating at a single distinct frequency depending on its size. The propagation of sound, then, was essentially a chain reaction of sympathetically vibrating particles. Only in this way, Mairan argued, can sounds of differing pitch (as well as differing timbre and dynamic) reach the ear without interference.

Mairan was convinced that his hypothesis solved a host of acoustical paradoxes insoluable by competing theories. It showed not only how differing pitches can be propagated through the air, but it suggested how the ear can recognize these pitches. Mairan described the basilar membrane which carpeted the inner ear as a "veritable musical instrument." Each fiber of the basilar membrane is tuned—just like an air particle—to respond to a unique frequency.[15] Of importance to the present study, Mairan's hypothesis explained the mystery of harmonic overtones. He reported that, "in the presence of a very competent musician," he plucked a string and was able to hear an octave, twelfth, double octave, and seventeenth above the fundamental.[16] "What is the cause of this extraordinary effect?" he asked. "It is clear that this cause cannot reside in the *corps sonore* or in the sound itself, in the string, or in the air." Objecting that something cannot vibrate simultaneously at different frequencies, Mairan concluded that this phenomenon must originate in the sympathetic resonance of commensurate air particles. In other words, the vibrations of any particle will also agitate those particles whose frequencies are integrally related in harmonic proportion to that of the original sounding frequency, just as we observe in the sympathetic resonance of harmonically tuned strings. "This is why a string which by itself can only excite in the air a unison—or its octaves, may on occasion make heard the fifth and the third or their octaves." Mairan concludes:

> This is one of the experiences that in my opinion is inexplicable by any other system. One finds here at the same time the principle laws of harmony dictated by nature herself; the major triad founded on the correspondence that the harmonic particles of the air possess between themselves and a fecund source of rules, which art and calculation can extend, and which all philosophy will admit.[17]

Mairan's explanation of our instinct for harmony is surprisingly Lockian coming from such a strong Cartesian. Perhaps the triad is not implanted in our minds as an innate idea, he admits; hearing tones with their concomitant harmonics of the third and fifth, however, and "repeated millions of times since our birth, forms in us a habit that can justly be called a natural sentiment for harmony." This is clearly a basis for developing a theory of music, he adds, and very much worthy of contemplation.

> But I shall refrain from entering into this in detail, as a celebrated musician of our day, to whom my ideas and my hypothesis are not unknown, will imminently give to the public a treatise on music which aims at this goal, and is based upon these same principles.[18]

The treatise Mairan refers to is Rameau's *Génération harmonique* published in 1737. In this work, Rameau adopted Mairan's hypothesis as the explanation for harmonic overtones heard in the *corps sonore.* "Harmony," Rameau began his treatise, "which consists of an agreeable mixture of several different sounds is a natural effect, the cause of which resides in the air agitated by the percussion of each individual *corps sonore.*"[19] Rameau then proceeded to detail Mairan's hypopthesis in detail (giving due credit to the scientist). In the course of his presentation, Rameau augmented Mairan's theory with numerous of his own acoustical "hypotheses" and "experiments," ostensibly to serve as confirmation or consequences of the atomistic hypothesis. For the most part, these experiments consisted of the sounding of various instruments: trumpets, bells, organ pipes, and even a pair of tongs. Rameau reported that one hears in any tone of these instruments a strictly harmonic series of upper partials. The accuracy of Rameau's observations, though, are questionable. As we will soon see, many such instruments produce *inharmonic* partials. Nonetheless, Rameau found in Mairan's hypothesis reasonable grounds for believing that overtones were indigenous to all vibrating systems, and that these overtones were always uniformally harmonic. As far as he could see, his music theory was on firm scientific footing. Unfortunately, though, this would not prove to be the case. In parts of the *Génération harmonique,* Rameau quite blatantly misinterpreted Mairan's thesis in order to justify one or another of his theoretical arguments. The most glaring example of this is Rameau's discussion of the minor triad. Rameau used Mairan's atomistic hypothesis not only to account for the upper partials heard in a *corps sonore,* forming the major triad, but also to assert the existence of a series of lower harmonics forming the minor triad.[20]

In proposition five, Rameau suggested that a sounding string will not only activate higher partials through the agitation of air particles whose natural frequencies are integral multiples of the fundamental (the upper octave, twelfth, and so forth.), but it will also activate those air particles whose frequencies are integral divisors of the fundamental. This would produce the reciprocal (arithmetic) series of a lower octave, 12th, 17th, and so forth. Rameau's point with all this is that the minor triad is as much a product of "harmonic generation" as is the major triad. Of course, things were not quite as simple as that. By this reasoning, the generator of the minor triad is its fifth, for example, G in a C minor triad. Such a proposition, though, wreaks havoc with his rules governing the progression of roots in the fundamental bass. Most importantly, though, there was just no empirical evidence for the existence of lower harmonics. Rameau admitted that "the slowest vibrations have more power over the fastest than the latter have over the former, and in consequence, because the fastest vibrations can only agitate the slowest vibrations weakly, they cannot give those bodies sufficiently strong agitation for the sound to be transmitted to the ear."[21] In order to verify the existence of the slower vibrations and make them perceptible, Rameau tells us in his second "experiment" to tune two strings a twelfth apart. If you bow the higher sounding string, "you will see not only the lower sound vibrate as a whole, you will also see it divide itself into three equal parts, forming three anti-nodes of vibrations between two nodes or fixed points."[22] We can verify that the string is vibrating as a whole, says Rameau, by touching it at one of these nodal points. We will be able to feel that these nodes are not perfectly stationary, thus "proving" that the string is indeed vibrating as a whole. (I will term this Rameau's "resonance" theory of the minor triad.[23]) Rameau concluded from all these propositions and experiments that a single vibrating *corps sonore* is indeed the acoustic generator of both the major triad and the minor triad. He admits, though, that the minor triad possesses an origin "less perfect and less natural than the original harmony,"[24] requiring as it does "the artificial means of facilitating the perception of a sound imperceptible by itself."[25] But wishing to establish a theory of "harmonic generation" for both the harmonic and arithmetic proportions, Rameau must insist that the origin of the minor triad is as natural as that of the major triad.

Rameau's resonance theory, of course, is patently false, as he was soon to realize. Strings tuned a twelfth and seventeenth below a sounding string will not vibrate in their totality, but only in aliquot divisions producing a unison with the sounding string. Rameau's concern, though, was with harmony, not physics. Thus, whether willfully or out of sheer ignorance, he distorted his evidence in order to provide the minor triad with as firm an acoustical basis as the major triad.

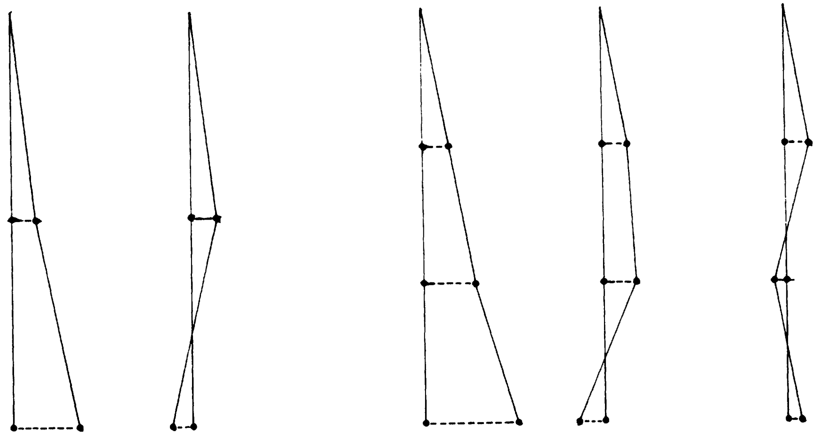
Notwithstanding the difficulties with the minor triad, though, Rameau was perfectly justified in defining the *corps sonore* using Mairan's atomistic hypothesis. Overtones were a well-confirmed property of most vibrating systems. As no scientist in 1737 was yet able to explain conceptually how a *corps sonore* such as the vibrating string could vibrate in such a manner as to emit several frequencies at once, Mairan's hypothesis was a reasonable one, if a bit naively mechanical. Around this time, though, a number of scientists working outside of France were taking the first steps towards an understanding of vibrational superposition, steps which would eventually lead to the correct explanation of overtones and the disproval of Mairan's theory.

As we have noted, Taylor's formula assumed that the fundamental shape of any vibrating string would be sinusoidal. This would account for the string's fundamental frequency, but could not account for its overtones.[26] It was precisely upon the basis of this assumption that Mairan sought the cause of overtones in a source *outside* of the vibrating string. Taylor was wrong, though; the shape of a vibrating string is much more complex than a single sine curve. As Sauveur discovered, any string possesses many possible forms (or "modes") of vibration. What Sauveur could not conceive (nor could anyone else at the time), was how these forms could be superimposed into a single complex motion without disturbing the individual component vibrations (frequencies). The answer to this puzzle was soon found through the study of simple vibrating systems.

While analyzing what scientists call the "hanging chain," the Swiss physicist Daniel Bernoulli (1700–1782) came to an important realization in 1733: a vibrating system possesses as many modes of vibration as the system has degrees of freedom.[27] This can be demonstrated by setting up a freely dangling string loaded with a small number of equally-spaced weights (the weights being analogous to the links of a "hanging chain"). With only one weight at the end of the string, there is but one mode of motion possible. A pendulum is a good example of this. With the addition of another weight, though, one additional degree of freedom is possible, and consequently one more mode of vibration. The process continues, theoretically, *ad infinitum.* Example 1 reproduces Bernoulli's representation of the respective modes of a string loaded first by two weights, and then by three weights.[28]

Bernoulli concluded from his observations that all upper modes of vibration contained nodes; for an object with k degrees of freedom, there would be $k - 1$ nodes. He further realized that his findings could be generalized to any flexible body. The vibrating string behaved like a hanging chain loaded with infinitely many small weights adjacent to one another. Thus, the vibrating string had potentially an infinite number of degrees of freedom, giving a correspondingly infinite number of nodes. Finally, Bernoulli realized that the frequency of each mode was directly proportional to the number of its nodes. At this point, though Bernoulli was not ready to say that such modes could coexist. The concept of vibrational superposition must have appeared improbable to Bernoulli. He considered, rather, modes to be discrete motions which a flexible body was capable of assuming.

Soon entering into this research along with Bernoulli was probably the most brilliant and unquestionably the most prolific scientist in the eighteenth century, Leonhard Euler (1702–1783), known to musicians for his



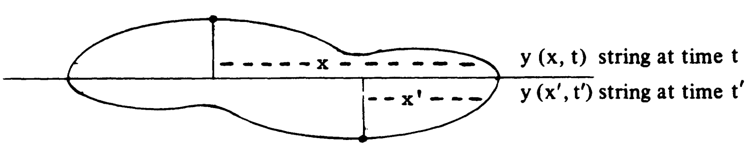
The two modes of a string loaded by two weights

The three modes of a string loaded by three weights

Example 1

Mode	Frequency	(Ratio to Fundamental)
1	6345	(1)
2	17,627	(2.78)
3	34,545	(5.44)
4	57,105	(9.00)
5	86,308	(11.36)

Example 2



Example 3

magnum opus of speculative *musica theorica,* the *Tentamen novae theoriae musicae* published in 1739. Euler and Bernoulli poured out a torrent of research on the behavior of flexible and rigid bodies which opened the way for unprecedented advances in the understanding of vibrational mechanics.[29] One finding relevant to music theory emerged from this research. In a study of the transverse vibrations of a rigid bar made in 1742, Bernoulli found that the initial vibrational modes of some flexible systems were not harmonic. That is to say, the frequencies of the upper modes did not necessarily relate in integral proportions to the frequency of the fundamental mode. Example 2 shows Bernoulli's calculations for the first five modes of a vibrating rod.[30] Only the frequency of the fourth mode stands in integral proportion to the fundamental. Similar inharmonic modes resulted no matter the clamping conditions of the rod (clamped on both ends, on one end only, pinned in the middle, and so forth). Again, there was no mention that such modes could coexist, although by now such a possibility had occured to Bernoulli.

Attention soon turned back to the vibrating string. Drawing upon their understanding of the hanging chain and elastic rods and bars, Euler and Bernoulli were able to show that Taylor's assumption was false; the motion of a vibrating string is much more complex than a simple sinusoid. Describing this motion mathematically was another problem, though. Fortunately, such a description was now possible with the help of a newly developed tool in calculus: partial differential equations.

The first scientist fully to develop and apply partial differential calculus was the great French scientist and *philosophe,* Jean Le Rond d'Alembert (1717–1785).[31] D'Alembert is today remembered by musicians as a propagandist and critic of Rameau's music theories, first in his role as coeditor of the great *Encyclopédie* along with Diderot, and later in his influential *Elémens de musique théorique et pratique.*[32] It was as a mathematician, though, that d'Alembert was known to his contemporaries. His first writings on partial differentials are to be found in a prize essay on wind entered in a competition at the Berlin Academy in 1746.[33] D'Alembert imaginatively approached the problem by analyzing wind as an "atmospheric tide." His findings, while completely abstract, were nonetheless of momentous importance for mathematics. In his paper we encounter for the first time the partial differential equation "as we understand it today."[34]

D'Alembert's success with his study of wind soon led him to consider the vibrating string. Although these phenomena may appear unrelated, an accurate mathematical description of the vibrating string, like that of wind, requires the use of partial differentials. In order to determine the position of a point y on the vibrating string illustrated in Example 3, one must determine it with respect to the x-axis, as well as time t. Its amplitude will then be represented by a function of two independent variables x and t such that $y = f(x,t)$. In two pioneering papers of 1747, d'Alembert derived a solution which could precisely determine y with respect to x and t. This is d'Alembert's famous wave equation, given below.[35]

$$\frac{\partial^2 y}{\partial t^2} = C^2 \frac{\partial^2 y}{\partial x^2}$$

As shown here, y is the displacement of a point on the x axis, over or under point x at time t. C represents the constant of string tension. D'Alembert's discovery was important, as his biographer has pointed out, "because it opened the way for the study of oscillations propagated in continuous media."[36] Field equations, as they are today called, have proved to be among the most powerful mathematical tools in modern physics.

In his paper, d'Alembert attempted to show how his wave equation could account for the transverse vibrations of any shaped string. According to his reasoning, such a system needed but one function to show its displacement from equilibrium at any moment. The function would be determined by the form of the stretched string just before being released.[37] For a number of misconceived reasons, though, d'Alembert believed that the displacement function could be of only certain restricted sorts. In a rebuttal to d'Alembert's paper, Euler argued that d'Alembert's restrictions were unnecessarily severe. He countered with his own equation, which he claimed was valid for any shaped string. Not be be left out, Daniel Bernoulli followed with a third solution differing fundamentally from both d'Alembert's and Euler's.

And thus began the great "vibrating string" controversy. It turned into the most noisy and vituperative scientific dispute of the mid-century, drawing into battle all the leading geometers of Europe, which is to say, d'Alembert, Euler, Daniel Bernoulli, and later, Joseph Lagrange and Pierre Laplace. We may wonder what all the fuss was about. After all, was it not possible to test any competing theories concerning the vibrating string through straightforward empirical analysis? The fact is, this is not the case. As an object of scientific inquiry, the vibrating string exists in a nebulous no-man's-land between physics and mathematics; it can be legitimately—and fruitfully—analyzed from a variety of perspectives. For d'Alembert and Euler, the vibrating string was interesting on account of the mathematical problems it posed.[38] Consequently, their research centered on general mathematical questions and methodology, and more specifically, over the definition of an "analytic function," and how restricted this definition ought to be. Bernoulli, on the other hand, was more interested in explaining the physical phenomena associated with the string itself (particularly its overtones), than in the abstract mathematical questions which engaged d'Alembert and Euler. Basing his work upon a careful empirical analysis of the vibrating string, Bernoulli concluded that its motion must be analyzed by a standing wave, and the only wave which conforms to this stipulation is one of Taylor's sines. In order to produce the irregular curvatures which Euler and d'Alembert attempted to represent by their various species of "functions," Bernoulli proposed a trigonometric expansion series.[30] In other words, the graph of any string undergoing periodic motion could be composed, as shown below, through the addition of infinitely many harmonically related sines (or, as he called them, "trochoids"), with suitably adjusted amplitudes:

$$y = \alpha \sin \frac{\pi x}{a} + \beta \sin \frac{2\pi x}{a} + \gamma \sin \frac{3\pi x}{a} \ldots$$

These sines were nothing less than the upper modes of vibration he had discovered while investigating the hanging chain. By this theory, he believed he could account for all the harmonic overtones heard in a vibrating string. Thus, Bernoulli was arguing that overtones and the formula for the vibrating string, two subjects which had up to that point been considered as unrelated, were indeed related in the most fundamental way. Unfortunately, while this theory is essentially correct, there was no known general principle upon which it could be proven. A rigorous mathematical proof would not be formally set down until the nineteenth century with the work of Fourier.[40] Thus, Bernoulli had recourse only to heuristic arguments of physical plausibility. For just these very reasons, however, Euler and d'Alembert rejected Bernoulli's solution.[41]

These difficulties notwithstanding, Bernoulli launched a scathing verbal attack upon Rameau. He argued that the *corps sonore,* as defined by Rameau, was a myth. Many of the elastic bodies he studied did not emit the complete and strictly harmonic series of partials Rameau claimed. The inharmonic modes of vibrating rods and bars could coexist just as easily as could harmonic modes. On this basis, he concluded that

> every sonorous body contains potentially an infinity of sounds and an infinity of corresponding ways of making its regular vibrations. Finally, in each different kind of vibration the bendings of the parts of the sonorous body occur differently.[42]

Parodying an experiment undertaken by Rameau, Bernoulli found:

> If you take an iron rod by the middle and strike it, you will hear at the same time a mixture of confused sounds which would be found by an experienced musician to be extremely unharmonious.[43]

Rameau, certainly an experienced musician, had of course come to quite a different conclusion, although one wonders whether his conclusion was based on real empirical evidence as much as wishful self-delusion. He stated in the sixth "experiment" of the *Génération harmonique* that if one struck a pair of tongs, the resultant clang would quickly settle down and produce a harmonious sound.

Hang up some tongs by a slender thread, each end of which you apply to an ear. Strike it; you will perceive at first only a confusion of sounds, which will prevent you from discerning any of them. But as the highest ones gradually abate, as the sound diminishes in strength, the lowest sound of the whole body begins to seize the ear . . . along with which is distinguished its 12th and major 17th.[44]

Bernoulli would have none of this. Inharmonic partials were as natural as the perfect twelfth and major seventeenth, "from which one sees that the harmony of sounds heard in a vibrating body at the same time is not essential to it and *ought not to serve as a principle for systems of music*" (emphasis mine).[45] Bernoulli would often repeat this point in his writings. The idea that under normal circumstances every sonorous body emits harmonic overtones, let alone harmonic overtones delimited by the fifth partial, seemed absolutely perposterous to Bernoulli.

Euler apparently agreed with Bernoulli. In a letter to Rameau in 1752, Euler gently suggested to the composer, "I admit also that many sounds of musical instruments actually contain their octave, 12th, 15th, and 17th, although it seems to me that this mixture is not the rule and that there are also pure sounds."[46] Elsewhere Euler was more categorical. In a letter to Lagrange in 1759, he wrote,

As for musical tone, I am in perfect agreement with you, Sir, that the consonant sounds M. Rameau claims to hear in a single string derive from other vibrating bodies. And I do not see why this phenomenon ought to be regarded as the principle of music more than the true proportions which are their foundations.[47]

Euler refers here to a passage in Lagrange's *Recherches sur la nature et la propagation du son* which appeared in 1759. There Lagrange had attributed the cause of overtones to sympathetic resonance:

But I confess that after much reflection, I have not been able to resolve this subject [overtones] satisfactorily. Having examined the oscillations of a stretched string with all the attention of which I am capable, I have found them always to be simple and singular in their motion throughout the length of the string, whence it appears to me impossible to conceive how different sounds could be generated at the same time. . . . I am thus inclined to believe that these sound are produced by other bodies which resonate to the sound of the principal, just as one sees with [several] strings. Giving some credence to this conjecture [is the fact that] this *mélange* of harmonic sounds is audible only in a harpsichord or other instruments possessing several strings.[48]

In order to verify his conjecture, Lagrange suggests that someone "with an extremely fine ear" and "well experienced in hearing music" listen to a single vibrating string with no surrounding strings which might resonate sympathetically.[49] Lagrange never seems to have undertaken this experiment himself.

Throughout the entire vibrating string controversy, the only scientist who accepted *verbatim* Rameau's description of the *crops sonore* was d'Alembert. We are led to wonder, then, how d'Alembert reconciled Rameau's *corps sonore* with his research into the vibrating string. Surprisingly, the answer is that he did not. D'Alembert believed his wave equation accounted for the shape and motion of a string set into vibration, but he never claimed that it also accounted for harmonic overtones. In fact, d'Alembert saw the phenomenon of overtones, much as did his seventeenth-century predecessors, as an entirely separate issue. He did not believe it was his business as a mathematician to explain something in the domain of the physicist. Writing in 1761, he admitted:

> One may object, perhaps, that it is impossible to explain by my theory why a string struck in various ways always renders much the same sound, since its vibrations, according to my theory, can be very irregular in many cases. I agree, but I am persuaded that the solution to this question does not pertain to analysis, which has accomplished all that could be expected of it. It is up to physics to handle the rest.[50]

The actual sounds produced by the *corps sonore* were clearly of no concern to d'Alembert in his calculations. It was not that d'Alembert refused to recognize the empirical evidence for overtones, rather, he simply did not believe it to be relevant to his particular concern which was mathematical. Thus, he could admit that "the real movement of the string given by experience is very different from that which one finds by calculation," yet nonetheless insist upon the verity of his mathematical equations.[51]

D'Alembert's faith that the true geometer need not, indeed, ought not, be overly concerned with accommodating all empirical evidence may strike the reader as somewhat paradoxical. Yet is was an attitude characteristic of d'Alembert's scientific epistemology, and indeed, much eighteenth-century science in general. One might object that d'Alembert unfairly simplifies his work precisely by excluding intractable variables from his equations. To an extent this objection is valid. However, d'Alembert's methodology is not really so unreasonable. In solving any problem, a scientist must invariably delimit the domain he investigates. This means knowing what to exclude from consideration as much as what to include. For eighteenth-century science, a certain amount of mathematical abstraction and disregard for physical evidence proved highly productive; by analyzing physical phenomena as a Cartesian problem of matter and impact, and quantifying such concepts as mass and force, the scientist may operate with a rigorous mathematical methodology without recourse to experimentation. It was with just such a methodology, Truesdell has pointed out, that the greatest strides were made in the eighteenth century in hydrodynamics, statics, astronomy, and optics, sciences for which he has coined the term "rational mechanics."[52] D'Alembert's research on the vibrating string is just one example of rational mechanics. Although the wave equation is not adequate by itself for describing the empirical phenomenon of the vibrating string, it proved to be a powerful mathematical tool with applications far beyond what d'Alembert could have envisioned.

Once d'Alembert had intellectually separated his mathematics from empirical phenomena, he could easily accept Rameau's description of the *corps sonore* as accurate. Indeed, what better witness is there for an acoustical phenomenon than the ear of a great musician?

> Moreover, M. Rameau, possessing an ear upon which we can rely in this matter, tells us in the *Génération harmonique,* p. 17, that if one strikes a tong, one will perceive first a confusion of sounds that cannot be distinguished, but as the highest sounds begin to die away insensibly as the resonance diminishes, a most pure sound of the entire body begins to seize the ear along with which is distinguished its 12th and its 17th.[53]

Empiricism and calculation each had an independently valid epistemological basis. If they seemed mutually inconsistent, that was only because our knowledge was so limited. D'Alembert had no doubt that there was some scientific explanation for overtones which would prove congruent with his calculations, but he did not pretend to offer one. In positivist fashion, he restricted himself to one clearly definable subject. He refused to erect ad hoc hypotheses—as he accused Bernoulli and Mairan of doing—to account for a poorly understood, even if well-confirmed, physical observation. D'Alembert believed it essential for a scientist to recognize the limits of his knowledge.[54] Thus it was that he chastised Bernoulli:

> Let us recognize, then, that all these facts [overtones] are an enigma inexplicable by us. In effect, can one flatter himself to explain them by regarding the movement of the points of the string as composed of many others, by supposing ficticious anti-nodes and mobile nodes? There would be nothing, it seems to me, which could not be explained by so arbitrary a method.[55]

D'Alembert's criticism of Bernoulli underscores the excesses to which d'Alembert's rationalist arrogance at times carried him. If d'Alembert's bias towards mathematical abstraction could lead him to brilliant insights, so, too, at other times could his disdain of experimental physics lead him astray. He accuses Bernoulli of proposing an unfounded hypothesis for which d'Alembert the geometer can find no mathematical justification. Bernoulli's justification, of course, was entirely empirical. Long before, Sauveur had shown that a vibrating string did indeed contain various nodal points that were for all practical purposes stationary. It was precisely the experimental evidence which was Bernoulli's strongest defense. D'Alembert's unwillingness to appreciate this kind of evidence was certainly in Bernoulli's mind when he wrote to Euler several years earlier complaining about d'Alembert's research:

> [d'Alembert] gave not the slightest attention to my experiments to verify how closely my physical hypothesis agrees with nature and whether my mathematical calculations satisfy the hypotheses of the physicist.[56]

What then of Bernoulli's conclusion that the *corps sonore* cannot serve as a foundation for harmony on account of its frequently inharmonic overtones? This conclusion, d'Alembert feels, is too "precipitous," since "in general, vibrating bodies generate very audibly the 12th and the 17th as M. Daniel Bernoulli himself has agreed."[57] If there are exceptions, d'Alembert insists, they are "extremely rare," and "without doubt stem from some structure peculiar to the body which prevents it from truly being regarded as a *corps sonore.*"[58] He follows up Bernoulli's example of the tongs: "The sound of tongs, for example, may contain many discordant sounds. But also the sound of tongs is scarcely a harmonious and musical sound. It is more a dumb noise than a tone."[59] D'Alembert then goes to say that in any case, Rameau has confirmed that a pair of tongs will indeed resonate its upper 12th and 17th after its other partials have died away.

It is ironic that Bernoulli's trigonometric expansion series offers one of the strongest scientific justifications for Rameau's principle conceived in the eighteenth century. Bernoulli showed that the only vibrations which a vibrating string can sustain without suffering decomposition are those harmonically related. Given, too, that the higher modes tend to decay more quickly than the lower modes, Rameau's claim that the vibrating string emits harmonic overtones, delimited by the fifth partial is not altogether unreasonable. It is only when one analyzes more complex elastic bodies capable of sustaining multiple transverse waves (such as bells and rods) that one encounters inharmonic modes of vibration. Had Rameau been satisfied to restrict his definition of the *corps sonore* to a vibrating string, then, perhaps he would have found in Bernoulli an ally instead of an antagonist.

The importance of d'Alembert's corroboration for Rameau's theory can scarcely be overestimated. Just when the very scientific foundation of Rameau's principle of the *corps sonore* was being attacked by Bernoulli and Euler, arguably the most influential scientist in France came to the composer's defense.[60] It is true, of course, that d'Alembert did not provide a mathematical explanation of his own for the *corps sonore,* the correct explanation ironically being available to Daniel Bernoulli. The essential point is that a respected scientist legitimized Rameau's empirical description of the *corps sonore* as well as its invocation as the principle of harmony. For Rameau, who above all else wished to set music theory upon a firm scientific footing, d'Alembert's support provided essential credibility.

It must not be thought that d'Alembert's role with respect to Rameau's theory was merely one of corroborator, though. He also served as an advisor and critic, helping the composer to clear his theory of unnecessary and obviously bogus acoustical and mathematical baggage, including Mairan's atomistic hypothesis. The cleansing effect of d'Alembert's criticism is manifest in Rameau's next major treatise written after the *Génération harmonique,* the *Démonstration du principe de l'harmonie* of 1750. Here some historical background is necessary.

In 1749, Rameau submitted to the *Académie royale des sciences* a *Mémoire où l'on expose les fondemens du système de musique théorique et pratique.*[61] As Albert Cohen's recent monograph has documented, it was long considered the proper domain of the Academy to review and pass judgment upon musical questions pertaining to tuning systems, instrument design, pedagogical methods, and harmonic theories.[62] As the most prestigious scientific institution in continental Europe, the official approbation of the Academy obviously was highly coveted. This was precisely Rameau's goal with his *Mémoire.*[63] As customary, a committee of Academy members was appointed to review the work in question. D'Alembert and Mairan were among those selected. This marked d'Alembert's first exposure to Rameau's theory. Indeed, it was probably his first exposure to any music theory of a sophisticated nature. Unlike his encyclopedist colleagues Diderot and Rousseau, d'Alembert had no musical training. Yet despite this handicap, d'Alembert was able to read and master Rameau's *Mémoire,* and, indeed, he became an enthusiastic supporter of the composer's ideas. To understand how Rameau's complex and recondite theory could so impress a scientist with so limited a background in music is to delve deeply into d'Alembert's peculiar rationalist epistemology, a subject obviously beyond the scope of this article.[64] Suffice it to say that d'Alembert saw in Rameau's theory a paradigm of logical systematization and methodology corresponding to his own work in rational mechanics. In other words, d'Alembert most likely was attracted to Rameau's system for epistemological—not musical—reasons. (Probably a similar motivation impelled d'Alembert in the drafting of his *Elémens de musique théorique* of 1752.)

As head of the reviewing committee, it was d'Alembert's duty to write a summarizing report for the Academy. He accomplished this task brilliantly. In a remarkable document, d'Alembert effectively presented the contents of Rameau's *Mémoire* in clear and precise language.[65] After some forty-five pages of quite technical prose, d'Alembert concluded with this flattering salute:

> M. Rameau successfully explains by means of this principle [the fundamental bass] the different facts of which we have spoken, and which no one before him had reduced to a system as consistent and extensive. . . Thus, harmony, commonly subjected to rather arbitrary laws or guided by blind experience, has become through the efforts of M. Rameau a more geometric

science, and one to which the principles of mathematics can be applied with usefulness more real and perceptible than had been until now.[66]

The length and tone of d'Alembert's report stands in marked contrast to the cool reception given the *Génération harmonique* some thirteen years earlier by the Academy.

Rameau soon thereafter published his *Mémoire*—proudly bound with the committee report—as the *Démonstration du principe de l'harmonie.* In the interim, though, Rameau had made a number of changes in the text, not the least significant being its new title. Erwin Jacobi has pointed out that the original manuscript of the *Mémoire* was significantly revised upon publication; numerous passages and whole pages were crossed out or covered with revisions pasted over the original manuscript.[67] He has suggested that it was Rameau who made these changes. According to Jacobi's reconstruction of events, the composer surreptitiously inserted these alterations after the original *Mémoire* had been read in order to introduce material—such as the title—to which the Academy would have undoubtedly objected. And indeed, there is much evidence for this; in latter years, when the relations between Rameau and d'Alembert had distinctly soured, d'Alembert would chastise Rameau for having presumed that his theory was "demonstrated," let alone that the Academy would have sanctioned such a claim.[68]

Yet not all of Rameau's changes were of this kind. It is just as reasonable to assume that many of the alterations were made at the request of the Academy. Indeed, based upon my own examination of the original *Mémoire*, it is evident that many of the changes actually clarify or correct material found in the original manuscript. A good example is Rameau's "resonance theory" of the minor triad which relies upon Mairan's atomistic hypothesis. By 1750, it was recognized by most informed scientists that Mairan's atomistic hypothesis was untenable. D'Alembert, for instance, was able to expose a number of fallacies in his important *Encyclopédie* article "Fondamental."[69]

It seems plausible, then, that it was d'Alembert who informed Rameau of the dubiousness of Mairan's theory soon after he had read the original *Mémoire*. At the point in the manuscript where Rameau speaks of the minor triad, a "resonance theory" similar to the one proposed in the *Génération harmonique* is crossed out. Pasted over this material is a new explanation of the minor triad which firmly renounces the "resonance theory."[70] In its place, Rameau offers a "modified resonance theory." Essentially, Rameau admits strings tuned a twelfth and seventeenth below a sounding string do not resonate sympathetically as a whole, rather, only in aliquot parts corresponding to the frequency of the sounding string. This would produce a series of unisons. Rameau then lamely claims that this must still be the source of the minor triad, even though the triad is never acoustically sounded. Recognizing the obvious weakness of this argument, though, Rameau finds it expedient to return to the question of the minor triad again later on in the *Démonstration* to offer a second, entirely independent derivation, one foreshadowing the "phonic" theory of reinforced resonance of upper partials proposed by Hermann Helmholtz in the nineteenth century.[17]

In abandoning Mairan's atomistic hypothesis, Rameau was not thereby weakening his principle of the *corps sonore.* He was in fact strengthening this principle. Now he could claim that overtones were intrinsic to the *corps sonore* itself, and not the result of some mechanical collision of air particles taking place outside the *corps sonore.* This was indeed a more attractive proposition for Rameau from a philosophical viewpoint; the *corps sonore* was the sole and unique source for all these harmonic partials. Every vibrating string thus contains in itself the germ of all music.

> The *corps sonore*—which I rightfully call the *fundamental sound*—this single source, generator, and controller of all music, this immediate cause of all its effects, the *corps sonore,* I say, does not resonate without producing at the same time all the continuous proportions from which are born harmony, melody, modes, and genres, and even the least rules necessary to practice.[72]

Ironically, this brought Rameau back to much the same ontological position he had articulated in the *Traité de l'harmonie* some twenty-eight years before.

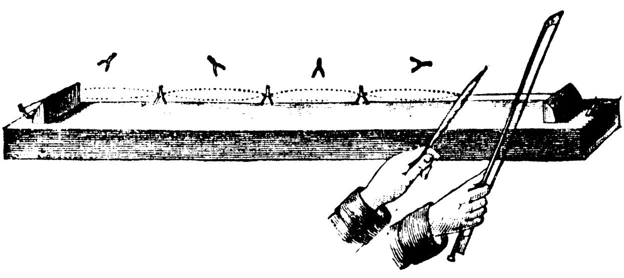
Rameau did not offer in any of his later writings a physical explanation of the *corps sonore.* As we have seen, bouyed by d'Alembert's corroboration, he obviously felt no need to offer one. Writing in 1752, he could confidentally assert:

> As soon as the *corps sonore* vibrates, it divides itself into aliquot parts and produces as a consequence different sounds. Can we know at present what is the principle of this division? It is one of the those first causes the understanding of which is above our faculties, and which the true philosophes today search.[73]

As d'Alembert had argued, it was enough for the scientist (and musician) simply to recognize and accept the *corps sonore* as an empirical reality without having to provide any kind of formal explanation. With the *corps sonore* thus established as a legitimate—even if ill-understood—scientific observation, Rameau now had a seemingly unshakable empirical foundation upon which to build his theory in the *Démonstration.* The *corps sonore* offered first of all the harmonic and arithmetic proportions, 1:3:5 and 1:1/3:1/5, respectively (the latter from the "modified resonance theory"). These proportions served as the source for the major and minor triads. The ratios found between the two proportions provide a geometric series of numbers with equally important musical implications; the "lower" and upper perfect twelfth of the fundamental (1/3:1:3) provides the triple proportion 1:3:9 which translates into the primary fundamental bass motion of music: the progression of fifths. The lower and upper seventeenth (1/5:1:5) provides the quintuple proportion (1:5:25) upon which is founded a subsidiary fundamental bass motion: the progression of thirds. The triple progression becomes the source of the diatonic scale, and hence the definition of mode, while the quintuple proportion introduces chromatic and enharmonic "genres." The double proportion (1:2:4, and so forth), which Rameau believed to be inaudible in the *corps sonore,* became his proof for the identity of octaves. By adding the first term of the triple progression to the harmony of the third term (for example, F to the G major triad in C major), Rameau was even able to find a source for the dominant seventh chord, and consequently all dissonance.

However much one may dispute the logic of Rameau's deductions from his *corps sonore,* there is an unarguable aesthetic unity here. From the first five aliquot divisions of a single vibrating string, Rameau was able to derive more or less successfully his entire theory of music. It is not surprising, then, that the *Démonstration* marks both the culmination and termination of Rameau's theoretical efforts. The elderly composer—by now sixty-seven years old—had seemingly achieved his life-long goal of a definitive systematization and "demonstration" of musical harmony with the principle of the *corps sonore.* So confident was the composer of his theory, that at this point in his life he undertook his most concerted efforts to secure its acceptance among the European scientific community.[74] While he would continue to produce additional writings over the next fourteen years, none of these were bona fide theoretical treatises containing any substantially new ideas; rather, they were textbooks on accompaniment, voice, and composition, polemical tracts, or essays on philosophical and aesthetic questions pertaining to the *corps sonore.* What original theoretical arguments were introduced in such works as the *Code de Musique* of 1760 were still firmly a part of the theoretical paradigm codified in the *Démonstration.* The essential principle of the *corps sonore* as the origin of all music remained unaltered. Indeed, one of the questions which would preoccupy Rameau to the end of his life was to what extent the other arts and sciences had *their origin* in the *corps sonore.* As he saw it, if the fine arts and geometry were based upon numerical proportions, surely the *corps sonore* must be accepted as their progenitor given that the very first sound uttered by primitive man—a kind of "*Ur-corps sonore*"—brought to his attention the notion of "rapport."

In his last years, Rameau's espousal of such metaphysical claims would errode the support and goodwill of those *philosophes* and scientists for which he had so long labored. D'Alembert was one of these casualties. For d'Alembert the geometer, the proposition that music could be epistemologically prior to geometry was difficult to swallow. Eventually the strain caused by their differences led to a complete break in their friendship. D'Alembert and Rameau became bitter enemies, engaging in a prolonged pamphlet war ending only with the composer's death.[75] Yet with all their profound differences, d'Alembert never ceased to accept the empirical validity of Rameau's *corps sonore,* nor—with all its difficulties—that it was the probable principle of harmony.[76] Thus, to the end of his life, Rameau remained secure in his belief that the *corps sonore* was indeed a scientifically credible principle, and consequently his system of harmony a truly scientific system.



NOTES

1. This is the conclusion reached, for example, by James Doolittle in his article "A Would-Be Philosophe: Jean Philippe Rameau," *Publication of the Modern Language Association of America* 74 (1959): 233–48; and Charles Paul, "Jean-Philippe Rameau (1683–1764), The Musician as Philosophe," *Proceedings of the American Philosophical Society* 114/2 (April, 1970): 140–54. Indeed, there is scarcely any contemporary reference to Rameau's theory without some kind of prefatory *apologia* for his scientific excursions.
2. There are a number of fine studies that trace the scientific understanding of overtones through the seventeenth and eighteenth centuries. Two of the most detailed are: Clifford Truesdell, "The Rational Mechanics of Flexible or Elastic Bodies 1638–1788," *Euleri Opera Omnia,* Series 2, Vol. 11/2 (Zurich: Orell Füssli, 1960); and Sigalia Dostrovsky, "Early Vibrational Theory: Physics and Music in the Seventeenth Century," *Archive for History of Exact Sciences* 14 (1975): 169–218. Two less technical studies with emphasis upon the musical implications of this scientific research are Burdette Green, "The Harmonic Series from Mersenne to Rameau: An Historical Study of Circumstances Leading to its Recognition and Application to Music" (Ph.D. diss., Ohio State University, 1969); and Claude V. Palisca, "Scientific Empiricism in Musical Though," *Seventeenth Century Science in the Arts,* ed. Hedley Howell Rhys (Princeton: Princeton University Press, 1961): 91–137.
3. Système général des intervalles des sons & son application à tous les systêmes & à tous les instrumens de musique," *Mémoires de l'Académie royale des sciences* 1701 (Amsterdam, 1707): 390–482.
4. "De Motu Nervi Tensi," *Philosophical Transactions* 28 (1713): 26–32.
5. *Mémoires pour l'histoire des sciences & des beaux arts [Journal de Trévoux],* (October, 1722): 1734. Reprinted in Jean-Philippe Rameau, *Complete Theoretical Writings,* ed. Erwin R. Jacobi, 6 vols (Rome: American Institute of Musicology, 1967–72) 1: xxxv. (Henceforth cited as "*CTW.*") "car non-seulement une corde peut faire en même tems deux sons à l'octave l'un de l'autre, mais encore trois & quatre, & sans doute les six UT, UT, SOL, UT, MI, SOL. C'est un fait attesté par M. Sauveur que lorsque la nuit on touche une grande corde, on entend la douziéme UT, UT, SOL, & même souvent dix-septiéme UT, UT, SOL, UT, MI, & que dans les trompettes on en entend encore davantage, de sorte que dans la Physique, la nature nous donne le même système que M. Rameau a découvert dans les nombres. . ."
6. Bernard de Fontenelle was the Academy's *sécretaire perpétuel.* It was his duty to summarize in short reports the work of the Academy's scientists. His report on Sauveur's research was entitled "Sur un nouveau système de musique," (*Histoire de l'Académie royale des sciences* 1701 [Amsterdam, 1707]: 155–75.) It should be noted that a "system" for Sauveur and Fontenelle meant dividing the octave for tuning purposes. Sauveur does not consider a system of music in any way related to a theory of harmony as does Rameau.
7. *Nouveau système de musique theorique* (Paris: Ballard, 1726), p. iii. "Il y a effectivement en nous un germe d'harmonie, dont apparament on ne s'est point encore apperçû. Il est cependant facile de s'en appercevoir dans une corde, dans un tuyau, etc. dont la resonance fait entendre trois sons differents à la fois. Puisqu'en supposant ce même effet dans tous les corps sonores, on doit part consequent le supposer dans un son de nôtre voix, quand même il n'y seroit pas sensible."

8. Ibid., p. 17.
9. Ibid. "Ainsi nous croyons pouvoir proposer cette experience comme un fait qui nous servira de principe pour établir toutes nos consequences."
10. The *Nouveau système* was in fact subtitled "pour servir d'introduction au Traité de l'harmonie" and bound with a reissue of the *Traité.*
11. Cuthbert Girdlestone, *Jean Philippe Rameau: His Life and Work* (New York: Dover, 1969), p. 522, no. 5.
12. Henry Guerlac, "The Newtonianism of Dortous de Mairan," *Essays and Papers in the History of Science* (Ithaca: Cornell University Press, 1981), pp. 479–90.
13. At least one person took the color-sound analogy seriously: Rameau's erstwhile admirer and reviewer for the *Traité*, Father Castel. Castel designed an *occulaire clavecin* based upon this idea. Each key of Castel's instrument was connected to a wheel covered by a colored ribbon which would rotate visibly when that particular key was depressed. Although not commercially successful, Castel's invention attracted much attention throughout the eighteenth century. For a full discussion of Castel's *occulaire clavecin,* see Donald S. Schier, *Louis-Bertrand Castel, Anti-Newtonian Scientist* (Cedar Rapids: Torch Press, 1941), pp. 135-96.
14. "Discours sur la propagation du son dans les différens tons qui le modifient," *Mémoires de l'Académie royale des sciences* 1737 (Amsterdam, 1740); 1–87.
15. *Discours sur la propagation du son,* p. 13. This is essentially a description of the modern physiological "resonance" theory.
16. Ibid., p. 15.
17. Ibid., pp. 18–19. "Voila donc une de ces expériences, à mon avis, inexplicables par tout autre système. On trouve ici en même tems les principales loix de l'harmonie dictées par la nature même, l'accord parfait fondé sur la correspondance que les particules harmoniques de l'air ont entre elles, & une source féconde de règles, que l'art & le calcul pourront étendre, & que la philosophie pourra avouer."
18. Ibid., p. 19. "Mais je puis d'autant plus me dispenser d'entrer là dessus dans le détail qu'un célèbre musicien de nos jours, à qui ces idées & mon hypothese ne sont pas inconnues, va donner incessamment au public un traité de musique qui tend à ce but, & qui porte sur ces mêmes principes."
19. *Génération harmonique* (Paris: Prault fils, 1737), p. 1. "L'Harmonie qui consiste dans un mélange argréable de plusieurs sons différens, est un effet naturel, dont la cause réside dans l'air agité par le choc de chaque corps sonore en particulier."
20. Ibid., pp. 4–5.
21. Ibid., p. 5. "les vibrations les plus lentes ont plus de puissance sur les plus promptes, que celles-ci sur celles-là, & que par conséquent les plus promptes n'agitant que foiblement les plus lentes, ne peuvent donner aux corps qui les reçoivent tout l'ébranlement nécessaire, pour que le son puisse en être transmis à l'oreille."
22. Ibid., p. 9. "vous verez non-seulement la grave frémir dans sa totalité, vous la verrez encore se diviser en trois parties égales, formant trois ventres de vibrations entre deux noeuds, ou points fixes."
23. I choose the label "resonance theory" instead of "undertone theory." The latter term erroneously suggests the Riemannian theory that the vibrating string itself emits lower frequencies analogous to its "overtones," whereas Rameau explicitly attributes these frequencies to causes occuring outside the string itself, namely, the collision of commensurable air particles.
24. Ibid., p. 32. "moins parfaite, moins naturelle que la premiere."

25. Ibid., p. 23. "le moyen artificiel dont on se sert pour faciliter l'apprétiation d'un son inapprétiable par lui-même."
26. Taylor, it is worth pointing out, never made any mention of overtones in his study of the vibrating string. Ironically, in later years he read and reported on Rameau's *Nouveau Système* for the British Royal Society. But even in his report—incidentally a quite favorable one—Taylor makes no mention of Rameau's new acoustical principle. (Leta E. Miller, "Rameau and the Royal Society of London: New Letters and Documents," *Music and Letters* 66/1 (1985): 25–26.)
27. "Theoremata de oscillationibus corporum filo flexili connexorum et catenae verticaliter suspensae," *Commentarii Academiae Scientiarum Imperialis Petropolitanae* 6 (1732–1733): 108–122. This article is reprinted with an accompanying English translation in the appendix to John T. Cannon and Sigalia Dostrovsky, *The Evolution of Dynamics: Vibration Theory From 1687 to 1742* (New York: Springer-Verlag, 1981).
28. Reprinted from Ibid., p. 141.
29. Canon and Dostrovsky cover the history of this research in detail in their excellent survey. Ibid., pp. 53–109.
30. "De sonis multifariis quos laminae elasticae diversimode edunt disquisitiones mechanico-geometricae experimentis acusticis illustratae et confirmatae," *Commentarii Academiae Scientiarum Imperialis Petropolitanae* 13 (1741–1743): 167–96.
31. Steven B. Engelsman, "D'Alembert et les équations aux dérivées partielles," *Dix-Huitième Siècle* 16 (1984): 27.
32. *Elémens de musique théorique et pratique, suivant les principes de M. Rameau* (Paris: David l'ainé, 1752).
33. *Réflexions sur la cause générale des vents,* (Paris: David l'ainé, 1747).
34. S. S. Demidov, "Création et développement de la théorie des équations différentielles aux dérivées partielles dans les travaux de J. d'Alembert," *Revue d'histoire des sciences* 35/1 (Jan. 1982): 15.
35. "Recherches sur la courbe que forme une corde tendue mise en vibration," *Histoire de l'Académie royale des sciences et belles lettres de Berlin* 3 (1747): 214–49; "Suite des recherches. . ." Ibid., pp. 220–49.
36. Thomas Hankins, *Jean d'Alembert: Science and the Enlightenment* (Oxford: Clarendon Press, 1970), p. 48.
37. Philip Davis and Reubin Hersh, *The Mathematical Experience* (Boston: Birkhaüser, 1981), p. 257.
38. A good discussion of the vibrating string controversy, especially in regards to the methodological arguments, is by J. R. Ravetz, "Vibrating Strings and Arbitrary Functions," *The Logic of Personal Knowledge: Essays Presented to Michael Polanyi* (London: Routledge, Kegan Paul, 1961): 71–88. Truesdell thoroughly covers the technical side of the controversy in *The Rational Mechanics,* pp. 237–50.
39. "Réflexions et éclaircissemens sur les nouvelles vibrations des cordes exposées dans les mémoires de 1747 & 1748," *Histoire de l'Académie royale des sciences et belles lettres de Berlin* 9 (1753): 147–72; "Sur le mêlange de plusieurs especes de vibrations simples isochrones, qui peuvent coexister dans une même système de corps," Ibid., pp. 173–95.
40. Fourier was to show that the solutions of Bernoulli and Euler were essentially in agreement once their respective terminologies and analytic perspectives were properly sorted out.
41. In the lengthy *Encyclopédie* article "Fondamental," d'Alembert rebutted Bernoulli's

theory with this objection: If vibrations truly could co-exist, there would be no way for the smaller vibrations (higher frequencies) to be harmonic. For a string to produce an octave, d'Alembert points out, the middle node must precisely bisect the string and be absolutely stationary. Yet this is an ideal which is never achieved under normal circumstances due to the distortions caused by the fundamental mode. *Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers,* ed. d'Alembert, Diderot, 17 vols. (Paris: Briasson, David, Le Breton, Durand, 1751-65).

42. *Réflexions et éclaircissemens,* p. 151. "tous les corps sonores renferment en puissance une infinité de sons, & une infinité de manieres correspondantes de faire leurs vibrations régulieres; enfin, que dans chaque differente espece de vibrations les infléxions des parties du corps sonore se font d'une maniere differente."
43. Ibid, pp. 152–53. "si l'on tient par le milieu une verge d'acier, & qu'on la frappe, on entend à la fois un mêlange confus de plusieurs tons, lesquels étant appréciés par un habile musicien se trouvent extrémement desharmonieux."
44. *Génération harmonique,* pp. 17–18. "Suspendez une pincette à un cordon un peu mince, dont vous appliquerez chaque bout à chaque oreille; frappez-la, vous n'y distinguerez d'abord qu'une confusion de sons, qui vous empêchera d'en pouvoir apprétier aucun. Mais les plus aigus venant à s'éteindre insensiblement, à mesure que la résonnance diminue de force; le plus grave, celui du corps total, commence à s'emparer de l'oreille . . . & avec lequel elle distingue encore sa douziéme & sa dix-septiéme majeure."
45. *Réflexions et éclaircissemens,* p. 153. "d'où l'on voit que l'harmonie des sons, qu'on entend dans une même corps sonore à la fois, n'est pas essentielle à cette matiere, & ne doit pas servir de principe pour les systèmes de musique."
46. Letter dated September 13, 1752. "Je conviens aussi que plusieurs sons d'instrumens renferment actuellement leur octave, 12me, 15me, 17me et quoiqu'il me semble que ce melange ne soit general, et qu'il y ait aussi des sons purs. . . ." *CTW* 5: 147.
47. Letter to Lagrange dated October 23, 1759; in *Correspondance de Leonhard Euler* in *Euleri Opera Omnia,* Series 4, vol. 5, p. 425. "Pour les sons de musique, je suis parfaitement de votre avis, Monsieur, que les sons consonnants que M. Rameau prétend entendre d'une même corde viennent des autres corps ébranlés; et je ne vois pas pourquoi ce phénomène doit être regardé comme le principe de la musique plutôt que les proportions véritables qui en sont le fondement."
48. *Oeuvres de Lagrange,* ed. J. A. Serret (Paris: Gauthier-Villars, 1867) 1: 146–47. "Mais j'avoue qu'après bien des réflexions, je ne suis pas encore parvenu à trouver sur ce sujet rien de satisfaisant. Ayant examiné avec toute l'attention dont je suis capable les oscillations des cordes tendues, je les ai toujours trouvées simples et uniques dans toutes leur étendue, d'où il me parait impossible de concevoir comment divers tons peuvent être engendrés à la fois. . . Je suis donc enclin à croire que ces sons peuvent être produits par d'autres corps qui résonnent au bruit du son principal, comme on vient de le voir dans les cordes; et ce qui peut donner quelque poids à cette conjecture, c'est que ce mélange de sons harmonieux n'est guère sensible que dans les clavecins ou dans les autres instruments montés de plusieurs cordes."
49. Ibid., p. 147.
50. "Recherches sur les vibrations des cordes sonores" in d'Alembert, *Opuscules Mathematique* (Paris: David, 1761) Vol. 1, p. 40. "On objectera peut-être, qu'il est impossible d'expliquer dans ma théorie, pourquoi la corde frappée d'une maniere quelconque, rend toujours à peu près le même son; puisque ses vibrations, selon moi, peuvent être

très-irrégulieres en plusiers cas. J'en conviens; mais je suis persuadé que la solution de cette question n'appartient point à l'Analyse: elle a fait tout ce qu'on étoit en droit d'attendre d'elle; c'est à la Physique à se charger du reste."

51. Ibid., p. 41. "le mouvement réel de la corde donné par l'expérience est très-différent de celui qu'on trouve par le calcul."

52. Clifford Truesdell, "A Program Toward Rediscovering the Rational Mechanics of the Age of Reason," *Archive for History of Exact Sciences* 1/1 (1960): 10.

Nor is this characteristic only of eighteenth-century science. Recent historians of science have recognized that a good deal of the methodology historically followed by scientists is non-empiric, and even anti-empiric, contrary to the picture traditionally drawn by positivist historians. See Larry Laudon, *Progress and Its Problems* (Berkeley: University of California Press, 1977), especially pp. 45–69. Also see Thomas Kuhn's thought-provoking essay, "Mathematical versus Experimental Traditions in the Development of Physical Science," reprinted in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), pp. 31–65.

53. *Encyclopédie,* Article, *Fondamental.* "D'ailleurs M. Rameau, à l'oreille duquel on peut bien s'en rapporter sur ce sujet, nous dit dans la Génération harmonique, page 17, que si l'on frappe une pincette, on n'y apperçoit d'abord qu'une confusion de sons qui empêche d'en distinguer aucun; mais que les plus aigus venant à s'éteindre insensiblement à mesure que la résonance diminue, alors le son le plus pur, celui du corps total, commence à s'emparer de l'oreille, qui distingue encore avec lui sa douzieme & sa dix-septieme."

54. For a more detailed discussion of d'Alembert's scientific epistemology, see Ronald Grimsley, *Jean d'Alembert* (Oxford: Clarendon Press, 1963), pp. 246–68.

55. "Recherches sur les vibrations des cordes sonores," p. 61. "Reconnoissons donc que tous ces faits sont une énigme inexplicable pour nous. En effet, peut-on se flatter de les expliquer, en regardant le mouvement des points de la corde comme composé de plusieurs autres, en supposant des ventres fictifs & des noeuds mobiles? Il n'y auroit rien, ce me semble, dont on ne pût rendre raison par une méthode si arbitraire."

56. Letter dated September 7, 1745. in Ph. H. Fuss, *Correspondance mathématiques et physique de quelques célèbres géomètres du XCIIIème siècle* (St. Petersburg, 1843), p. 584. "Hätte er billig auf meine Experimente sollen Achtung geben, um zu sehen, wie seit meine hypotheses physicae mit der natura sei übereinstimmen und ob meine calculi mathematici den hypothesibus physicus satisfacieren."

57. *Encyclopédie,* Article, *Fondamental.* ". . . en général les corps sonores rendent très-sensiblement la douzieme & la dix-septieme, comme M. Daniel Bernouilli en convient lui-même."

58. Ibid. "viennent sans doute de quelque structure particuliere des corps, qui les empêche de pouvoir être véritablement regardés comme des corps sonores."

59. Ibid., "Le son d'une pincette, par exemple, peut renfermer beaucoup de sons discordans: mais aussi le son d'une pincette n'est guere un son harmonique & musical; c'est plutôt un bruit sourd qu'un son."

60. D'Alembert was one of the most powerful figures in the *Académie royale des sciences* in addition to having influential connections among the aristocracy and philosophes. Inevitably his judgment carried with it a good deal of weight. Not the least significant manifestation of his power—and vanity—were his successful efforts to block dissemination of Bernoulli's and Euler's work on the vibrating string in the pages of the

Academy's *Mémoires* and the *Encyclopédie.* (Truesdell, *The Rational Mechanics,* p. 245).

61. Now housed in the "Dossier Rameau" in the archives of the *Académie royale des sciences.*
62. Albert Cohen, *Music in the French Royal Academy of Sciences: A Study in the Evolution of Musical Thought* (Princeton: Princeton University Press, 1981).
63. The *Mémoire* was not Rameau's first contact with the Academy; some 13 years earlier, he had dedicated and submitted his *Génération harmonique* to the Academy in order to secure similar approval. While that treatise was politely received, Rameau's hopes for an official endorsement went unfulfilled.
64. I have discussed this topic in detail in my dissertation, "Science and Music Theory in the Enlightenment: d'Alembert's Critique of Rameau," (Ph.D. diss., Yale University, 1985), especially in Chapters 2 and 3, pp. 54–137.
65. *Rapport sur un Mémoire où M. Rameau expose les fondemens de son système de musique theorique et pratique,* Dec. 10, 1749. Printed as the *Extrait des registres* and bound with the *Démonstration du principe de l'harmonie* (Paris: Durant, Pissot, 1750); *CTW* 3: 223–46.
66. Ibid., pp. xliv–xlvi; *CTW* 3: 244–45. "M. Rameau explique avec succès, par le moyen de ce principe les différens faits dont nous avons parlé, & que personne avant lui, n'avoit réduit en un systême aussi lié & aussi étendu . . . Ainsi l'harmonie assuyétie communément à des loix assez arbitraires, ou suggérées par une expérience aveugle, est devenue, par le travail de M. Rameau, une Science plus géometrique, & à laquelle les principes Mathématiques peuvent s'appliquer avec une utilité plus réelle & plus sensible, qu'ils ne l'ont été jusqu'ici."
67. *CTW* 3: xliii–xlv.
68. See, for instance, the revised edition to the *Elements: Elémens de musique théorique et pratique . . . éclarçis, developpés, et simplifies* (Lyon: J. M. Bruyset, 1762), p. xvi, and pp. 212–15.
69. For one thing, d'Alembert pointed out, if individual air particles behave by purely mechanistic interaction, as Mairan claimed, they must follow strictly defined laws of motion and impact. There is no reason, though, why the oscillations of one particle should activate only those particles whose natural frequency differ by integral divisions or multiples. And having recently written the most important study of dynamics since Newton—the *Traité de Dynamique* of 1744—d'Alembert was quite familiar with the behavior of colliding rigid bodies. There was no physical law d'Alembert knew that could possibly support Mairan's hypothesis. More damaging to Mairan's thesis, though, was d'Alembert's demonstration of the longitudinal propagation of sound with the wave equation. (Clifford Truesdell, "The Theory of Aerial Sound 1687–1788," in *Euleri Opera Omnia,* Series 2, Vol. 13, p. xxxvii.) Axiomatic to a longitudinal wave theory is the presumption that air is a homogeneous mass which responds uniformly to all displacement forces, contrary to Mairan's hypothesis that air is composed of heterogeneous particles.
70. The passage in question begins on p. 64 of the *Démonstration:* ". . . pour former un accord parfait où le genre mineur est lieu." One wonders, incidentally, how Mairan reacted to all this criticism considering he was on the very reviewing committee for a work which repudiated his own scientific theory. Mairan never got along well with d'Alembert, whom he believed to be an arrogant young upstart. The two had earlier engaged in a polemic concerning the utility of philosophical systems for science.

Nonetheless, by 1750, it was clear that an atomistic hypothesis of sound propagation offered to the physicist more problems than it solved, a conclusion Mairan himself was eventually forced to concede.

71. *Démonstration*, pp. 62–84. The phonic theory of the minor triad—one of Rameau's most important and original theories—may also have been a last minute addition to the manuscript. (How else can one account for two contradictory accounts of the minor triad in the same treatise?). As with the first discussion of the minor triad in the *Mémoire*, this second discussion is also heavily rewritten. Indeed, entire pages have been pasted over with new material. But exactly how much all of this is new or simply a rewording of the original manuscript is impossible to say without being able to remove the scraps of paper for a comparison of texts. Unfortunately, both Jacobi and I were refused permission by the authorities of the Academy to make such a comparison. (*CTW* 6: XVIII.)
72. *Démonstration*, pp. 19–20. "Le corps sonore, que j'appelle, à juste titre, *son fondamental*, ce principe unique, générateur & ordonnateur de toute la musique, cette cause immédiate de tous ses effets, le corps sonore, dis-je, ne résonne pas plutôt qu'il engendre en même tems toutes les proportions continues, d'où naissent l'harmonie, la mélodie, les modes, les genre, & jusqu'aux moindres regles nécessaires à la pratique."
73. *Nouvelles Réflexions de M. Rameau sur sa démonstration du principe de l'harmonie* (Paris: Durand, Pissot, 1752), p. 3; *CTW* 5: 100. "Dès que ce corps sonore résone, il se divise en ses parties aliquotes, & fait entendre, en consequence, différens sons. Savoir à présent quel est le principe de cette division? C'est une de ces causes premieres, dont la connoissance est au-dessus nos facultés, & que les vrais philosophes se dispensent aujourd'hui de chercher."
74. Copies of the *Démonstration* and his following work, the *Nouvelle Réflexions* were sent to the scientists Euler, Johann (II) Bernoulli (brother of Daniel), and Gabriel Cramer, as well as academies in London, St. Petersburg, and Bologna, inviting them to add their approbation to that of the Paris Academy.
75. For a good account of this polemic, consult Jonathan Bernard "The Principle and the Elements: Rameau's Controversy with d'Alembert," *Journal of Music Theory* 24 (1980): 37-62. Also see my dissertation, Chapters 6-8, pp. 231-329.
76. E.g. in d'Alembert's "Réflexions sur la Théorie de la musique," (1777), printed in *Oeuvres et correspondances inédites de d'Alembert*, ed. Charles Henry (Paris: Perren, 1887), p. 137.