AN INTRODUCTION TO THE PHYSICS OF VIBRATIONS AND WAVES

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PREFACE

This book is intended as a sequel to *An Introduction to the Physics of Mass, Length and Time* (Edinburgh University Press, 1959). Much of its content could indeed have been included under that general title, for much of it deals essentially with matter in motion. A knowledge of the conditions determining the simple harmonic motions of particles is basic for many of the discussions, and the various theorems relating to the composition and resolution of such motions of necessity find wide application. The general concept of inertia is required, and, of the specific properties of bulk matter, those of elasticity (of solids, liquids and gases) and surface tension (of liquids) are all-important at various stages of the argument. All these topics were treated in the former book.

The decision not to include any systematic discussion of vibrations and waves was taken, when that book was written, after full consideration. Three reasons may be given for it. In the first place, to do justice to the new topics it was considered that they should be accorded wider scope than the previous title allowed. Secondly, whereas it was possible, and I still believe that it was desirable, to present the subject-matter of *Mass, Length and Time* without formal use of the calculus, to accept the same limitation in relation to *Vibrations and Waves* did not appear to be equally profitable—or equally possible. The third reason is of a different order. In modern sub-atomic physics the classical concepts of particle and wave, derived from our accumulated experience of physical phenomena at the macroscopic level, are seen to be related in a fundamental aspect of complementarity. In *Mass, Length and Time*, the classical concept of particles was formulated and elaborated. It was considered appropriate that the sequel, *Vibrations and Waves*, should be devoted to the formulation and elaboration of the classical concept of waves. It is clearly important that the beginning specialist in physics should have every opportunity of familiarising himself with these two concepts, in all their ramifications, as early as possible in his undergraduate career.

PREFACE

Mass, Length and Time was written largely as a record of lectures given, for many years, to first-year students of the University of Edinburgh, in the first term of their course in physics. The present book has no such pedigree. It was written without the prior experience of presenting the subject, in the flesh, to a living class. True, a course of lectures has emerged from the writing of it, but that is not the same thing as the other, in terms of practical experience. Indeed, I am acutely conscious of the presumption of offering, in print, a text which has not been fully tested in the lecture room. Only the first half of the course has in fact been delivered—for the first time, in the first term of the current session. The lectures were given to would-be honours students in the second year of their four-year curriculum. These students were the surviving members of a class which, twelve months previously, took its first steps in physics at the university with *Mass, Length and Time* as its guide. I feel that I owe the class both gratitude and an apology: an apology that I should have asked them, this year, to follow my unorthodox approach to the subject without the background security that a printed text automatically affords—and gratitude that they did so, and so fortified my own conviction that in this case unorthodoxy is not without its rewards.

Now, when the book has been written, when it is soon to be in print, I have other debts to acknowledge. Many friends and colleagues have read parts of it, in its formative stages. Their interest and criticism has been of the greatest value. Dr. M. A. S. Ross, Mr. R. M. Sillitto, and Mr. A. G. A. Rae are my chief creditors in this respect: I offer them my best thanks. I must also thank Miss D. E. Brewster. Over this volume, as over its predecessor, she has spent very many laborious hours, producing a fair typescript from a heavily over-written holograph.

NORMAN FEATHER

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CHAPTER 1

INTRODUCTION

In chapter 1 of *An Introduction to the Physics of Mass, Length and Time* (hereinafter referred to as *M L & T*) the attempt was made 'to set down some of the rules of the game' as they apply to the physicist. Here it is necessary to repeat only the first of these rules: 'Accepting the regularity of happenings in the inanimate external world, the physicist is prepared to find that every event in that world has some relation with—is partially determined by, or partially determines—every other event.' The common-sense experience of early man was the ground on which this expectation was originally based, and the more systematic investigations of professional physicists during the last four centuries have provided its overall justification. Countless situations have been recognised in which the physical behaviour of one piece of inanimate matter may be correlated with the behaviour of another piece of matter, allowing for a suitable lapse of time—short or long according to the circumstances of the case. Over the ages the notion of 'cause' or 'influence' naturally arose from these correlations, and the physicist has refined this notion, diminishing its philosophic content, perhaps, whenever he has been able to give it quantitative expression.

Nowadays, he uses the word 'wave' when he has in mind one particular type of process by which an influence reaches out or is transmitted from one body to another. This book is concerned with the physics of wave processes in general, and with the vibrations in which these processes originate, by which they are sustained, or ultimately dissipated. For the present we shall not define these terms more closely; it will be instructive to examine the origins of the concept of waves first, later to consider particular cases in detail, only then to come to generalities.

Writing in 1931, C. G. Darwin, at that time Tait professor of natural philosophy in the University of Edinburgh, pointed the distinction as follows: 'The most elementary way in which I can attract anyone's attention is to throw a stone at him. . . . Another way is to poke him with a stick and this has quite a different character, because there is no transfer of matter from me to him—a small motion that I produce in the wood at my end turns into a small motion at his end' (*New Conceptions of Matter*, p. 29). In the fourth century BC, Aristotle had maintained that force cannot be communicated from one body to another except by impact or pressure. In a sense, then, Darwin's dichotomy, although it was not put forward as rigorously all-inclusive, was Aristotelian. But it is Aristotelian with this difference—that the modern view, that the 'small motion' takes time to travel along the stick from one end to the other, has complicated the picture. The final result of the poking may be a 'pressure' exerted through the stick, but the initial phase involves the propagation of a 'pulse' or 'wave' of displacement along its length. For many purposes even the modern physicist finds it convenient to ignore this phase and to use idealised concepts, thus in certain circumstances he treats his real bodies as 'rigid' bodies (see *M L & T*, pp. 7, 154): in this connection, however, he cannot afford that approximation—his real bodies are 'elastic' (see *M L & T*, chap. 16); if he did not recognise that fact he would be left with only the transmitted pressure, as the Greeks supposed.

The notion of wave processes transmitted through solid bodies did not, as we have seen, occur to the scientists of ancient Greece, but the ordinary Athenian—and the artisans of the Pharaohs, a thousand years earlier, in the valley of the Nile—were accustomed to observe waves on the surface of water ruffled by the passing of a boat, the dropping of a stone, or by the wind. By the agency of water waves force is communicated from one body to another without transfer of matter. So firmly has this attribute of an expanse of water in bulk become a feature of man's awareness that the familiar ripple-lines communicate their meaning unambiguously whether, as in an Egyptian papyrus, they stand as a 'determinative' modifying another character in the text, or whether, as in the Bayeux tapestry of the eleventh century, they constitute a directly pictorial element in the design. An expanse of water perfectly still, without any aspect of motion, is a formal abstraction; all around us the waters are in perpetual agitation, the most quiescent of them dappled with patterns of waves, ever breaking out afresh, spreading and dying through a long summer day.

The air, too, is full of sound: a 'still small voice' in the wilderness, or a cacophony of noise in the city—sound born, echoing, dying away in the distance. A Roman architect, a decade or two before the birth of Christ, likened this process to the spreading of ripples on the surface of water. His inspired guess made no impact on his contemporaries: for a millennium and a half it remained a dead letter in a monks' library (see p. 112).

Through these many centuries Aristotle's physics dominated the schoolroom, becoming ever more ineffectual and distorted, circumscribed by the dogmatic metaphysics of the schoolmen. The beginnings of revolt were slow in breaking, and at first made little headway; then, with Galileo, they burst in full flood (see *M L & T*, p. 120). Galileo was an experimenter of genius, and the phenomena of sound occupied him over many years. In the end he was in no doubt: sound, he said, is propagated through the air as a wave process. The analogy with ripples on the surface of water was implicit in his use of language.

With Galileo the thraldom of science to scholasticism was broken: Descartes (1596-1650), thirty-two years his junior, attempted single-handed to construct a speculative philosophy which should replace its metaphysic. History has judged the system which he devised to be a failure, but this was a later verdict: it remained a potent influence on thought for more than a century. It was conceived out of acute dissent from the philosophy of the ancients, but it retained at least one feature of Aristotle's world: force could be communicated only by impact or by pressure. So, empty space became a 'plenum', the locus of mechanical process of great complexity. In so grand a cosmology, light, our only messenger from the heavens, necessarily held a central place; sound, a purely mundane manifestation, raised problems which were too trivial for detailed consideration. Light was conceived as propagated by pressure, instantaneously: the symbol of the stick recurred—the sighted are able to appreciate the pressure which is light, the blind have to feel their way with a stick, by pressure also. The notion of waves does not occur throughout the system as a whole.

In the Cartesian philosophy force is communicated only by impact or by pressure, but the emphasis is heavily on the latter mode. When space is completely filled with matter, albeit of different degrees of tenuity, the simple notion of impact loses its clarity. A contemporary of Descartes, Pierre Gassendi (1592-1655), equally anti-scholastic in his outlook, took the opposite view: for him space was entirely empty except for the unchangeable atoms of matter—there were impacts, but there was no sustained pressure. We recognise in this view the beginnings of the modern approach, but Gassendi could say nothing about light: his was but a partial cosmology, which Newton accepted as a working hypothesis and built upon, but the philosophers largely disregarded it for its obvious limitations. Its ultimate champion was Roger Joseph Boscovich (1711-1787), more than a century later. In Boscovich's world even the unchangeable atoms were de-materialised to become mere centres of force. It is a strange commentary on the fate of fundamental ideas in science that so downright an experimenter as Michael Faraday (1791-1867) should have been sympathetic to this extreme viewpoint, and should have commended it to his readers, in 1844, as still worthy of serious attention.

In his old age Galileo had set down on paper his considered views on the science which had been his life's work. In 1638 his book was published under the title *Dialogues on the New Sciences*. There, for the first time, the propagation of sound was recognised, by a physicist of genius, as involving a wave process in a medium. We have referred already to the break with tradition that Galileo made absolute. After him there was no turning back. The seventeenth century of our era has often been called the century of genius. We may single out four men, two physicists and two philosophers, all of them born within the one decade before Galileo's book was published, who carried his views on wave motion forward, ahead of their time. The physicists were Christiaan Huygens (1629-1695) and Robert Hooke (1635-1703), the philosophers Nicolas de Malebranche (1638-1715) and Ignace Gaston Pardies (1636-1673). The philosophers were Cartesians by discipline and by inclination, but in this matter they renounced the doctrine of their master. To Pardies, the older of the two, the credit of priority probably belongs of maintaining that light is not transmitted instantaneously, but that rather its velocity is finite, and that it is a wave process propagated in a medium. Pardies did not need to renounce Cartesianism to postulate the existence of an all-pervasive medium, or ether: Descartes's plenum provided precedent enough for that.

Of the views of the physicists, Huygens and Hooke, we shall have much to say in the proper place (see, in particular, p. 179). Here we take note of them merely as, first among their scientific fellows, joint proponents of the wave picture in relation to light. Of the philosopher de Malebranche, who was admitted an honorary member of the Académie des Sciences in Paris at the age of sixty-one, it is sufficient to note that in the same year he was the first to point out clearly that in a wave process the amplitude of particle displacement is the physical quantity determining the intensity of the effect produced when the wave is incident on a 'receiver'.

In the seventeenth century Isaac Newton (1642-1727) was, by common consent, the towering genius among men of science in Britain. Newton was the first to obtain, though by intuitive rather than by rigorous methods, an expression for the velocity of propagation of surface waves on shallow water, when the effect of gravity determines the motion (see p. 165). He was likewise the first to obtain an expression for the velocity of elastic waves in a material medium (see p. 114). In view of his contributions to the theory of wave motion in these two particulars, it is at first sight surprising that he should have been so resistant to the view that light, which exhibits so many features in common with sound, is essentially and simply a wave process of some kind, also. It is surprising, at first sight—but the question is a subtle one, and two long chapters of this book (chapters 8 and 9) have been found necessary to treat of its subtleties. In the end, though we cannot justifiably read into Newton's caution any valid foresight of what was to come, we can hardly fail to recognise that his ultimate position in respect of the nature of light was one of dualism, very much as that of the twentieth-century physicist is dualist.

Furnished as they were, through the labours of Newton and Leibnitz (1646-1716), with the tool of the calculus, the mathematicians of the eighteenth century made great advances in the theory of vibrations, but they were less successful with the theory of waves. For the most part, the phenomena of light were described and interpreted in terms of an emission hypothesis, theorists and experimenters alike adopting a point of view more narrowly corpuscular than that which Newton had formulated. Even in relation to the phenomena of sound, doubts arose concerning the adequacy of an explanation in terms of waves. A quotation from a standard text of the mid-century, *Leçons de physique expérimentale* written by l'Abbé Nollet (1700-1770), member of the Académie des Sciences and fellow of the Royal Society, will serve to illustrate these doubts. Nollet wrote (2nd edn., vol. 3, 1750) 'No one has any difficulty in understanding how it is that two bodies, acting as sources of sound, execute their vibrations independently. . . . But how is it that two different notes can be present at the same time in the same air, if the notes themselves are not, in the air, vibrations of a definite frequency, as they are in the sounding bodies; how can the same mass of air reproduce faithfully, at one and the same time, the notes of two strings, one the octave of the other? . . . [It has been thought valid to answer this question] by comparing the motion of the air which transmits these sounds to the circular waves which are produced in still water when stones are thrown in. It is said that the air accepts the different notes together, and transmits them without confusion to the hearer, just as the expanding waves cross one another without loss of identity and spread outwards to the water's edge. But . . . even this comparison is defective, and almost every point of similarity disappears when the character of the respective motions is analysed in detail.' Next follows a passage drawing the distinction between 'gravity' waves on water and elastic waves in a fluid, then Nollet continues, 'Besides, when the water waves cross one another, it cannot be denied that, where the waves meet, the momentum is compounded of the masses and the velocities of the parts that meet, and that a small body placed at this point of intersection necessarily receives this resultant momentum. It is not the same with two sounds . . . each is effective as if it were acting alone.'

In order to explain this imagined difference, Nollet commends, with great power of persuasion, a then recent suggestion of J. J. de Mairan, himself a physicist of considerable renown. 'M. de Mairan', he says, '. . . puts forward a system, so simple but at the same time so happily conceived, that one soon forgets that it is merely an hypothesis, when one applies it to phenomena . . . since the molecules of air are chance assemblages of smaller units, which coalesce and dissociate as the result of innumerable causes, is not one led to believe that they differ from one another in size over an infinite range, rather than to suppose gratuitously that each resembles every other in every particular? This idea, on which the whole system of M. de Mairan is founded, is the only feature of it which is merely plausible; all the other features are such necessary consequences of this assumption (once it is accepted) that they are quite irrefutable. If the molecules of air are different in size, they must differ also in their degree of resilience. . . . Consequently, wherever a sounding body may be placed, it will find in its surroundings some molecules of air whose elasticity is similar to its own—some molecules capable of receiving, sustaining and transmitting its vibrations. In this way two strings of different frequencies may be heard through the same mass of air. . . .'

If M. de Mairan's ingenious 'system' had still been seriously entertained by physicists at the turn of the century—which it was not—the one 'merely plausible' feature of it, the notion of a wide variation in molecular size, would certainly have been discredited, and with it the whole system brought into disrepute, by the experiments of Gay-Lussac, and the hypotheses of Dalton and Avogadro, which provided their interpretation and laid the foundation of the new physics of atoms, in the first years of the century that followed. But the development of physics in the first years of the nineteenth century did more than provide a refutation of a speculative hypothesis which never received universal support; in the contributions of Thomas Young (1773-1829) there was furnished a solution to the problem to which de Mairan's speculations had been directed—and, incidentally, a complete exposure of the misconceptions implicit in the posing of the problem as Nollet posed it.

Young's principle of superposition is described in its own right in chapter 8 of this book, but it is tacitly assumed in much that goes before. Indeed, the principle is basic for, and of universal relevance to, the whole class of wave processes in which isochronous vibrations play a part. Its formulation, as we shall eventually describe (p. 178), was almost entirely underivative from the notions that earlier workers had proposed. Daniel Bernoulli (1700-1782), it is true, had formulated a principle of superposition in mathematical language, in respect of the vibrations of strings under tension, in 1755, but he had undeservedly failed to convince his fellow mathematicians (Euler and Lagrange, in particular) of the validity of his ideas, and he had certainly not extended them to the wider realm of wave processes in general.

Young was led to the principle of superposition directly, from common experience, by the intuition of genius.

Having brought our history to this point, it is unnecessary to follow it farther. With the contributions of Young the physicist's ideas of vibrations and waves took on their modern aspect. We are better advised to turn now from the general to the particular, meeting history on the way, but treating each topic systematically, in its relation to the whole. Perhaps, when he has traversed the book, the reader may return with added profit to this introduction, finding in it, also, something of a summary—and a background against which to evaluate the significance of what he has read.

CHAPTER 2

STRETCHED STRINGS

2.1. HISTORICAL

The view has been held, and it is difficult to disprove it, that the origin of the stringed instruments of music is to be traced back to the hunter's bow. The Assyrians and Egyptians of ancient history raised their armies of bowmen, and the earliest musical instruments of which we have direct knowledge have been found in the royal tombs of the valley of the Nile. For the most primitive of these instruments, the nanga, there is abundant evidence of fragments over the period 2000 to 1500 BC, and complete specimens dating from about 1500 BC are to be seen in the British Museum. The frame of the instrument was bow-shaped; it had three strings, or sometimes four; the strings were twanged by the performer using his fingers. The nanga was thus at least as close, in form and function, to the warrior's bow as it is to the modern harp, which, by slow stages of evolution, has developed out of it.

Let us consider, very briefly, this evolutionary history, hypothetical though it may be in its earlier phases. In the use of the hunter's, or the warrior's, bow, the staff is bent as the bow-string is drawn back. At the extremity of this motion, when the barbed head of the shaft is close to the bowman's outstretched hand, there is momentary equilibrium: the tensions in the bow-string at its ends balance the restoring forces of elastic deformation of the staff, and, at the centre of the string, the tensions balance the force of drawing. For a given bow-staff, and a given shaft drawn to the full, the equilibrium situation is well defined. When the shaft is released, the tensions in the string subside after a regular pattern. There is a faint and characteristic sound peculiar to the event. The hunter and the warrior cannot have been insensitive to it. For a bow of another size, or perhaps with a heavier or a lighter string, the sound is similar, but in one respect it is different. The hunter who had known the varied sounds of the forest, with their differences, must eventually have recognised this possibility of difference in the sound of the bow-string—this difference in general 'pitch'. When he and his companions sought to make music for themselves—as the spirit surely led them—one bow with three strings or four provided them with at least a narrow compass of possible 'notes', and a small range of possibility in respect of their combination. In the nanga the strings were of different lengths: this alone opened the compass, and differences of tension could open it wider. In the musical instrument, the bow, its frame, was almost inflexible, the tensions of the strings were not materially altered in the process of playing: the sound of a string was that of a string under effectively constant tension, it was different in primitive 'quality' from the sound of the bow-string.

This is not the place to embark on further technicalities, or to undertake a systematic history of the development of the stringed instruments of the modern age. Suffice to say that in every case we have to do with the vibrations of strings under constant tension, with strings set in vibration by various means—by plucking, or twanging with the fingers or with a plectrum, by bowing, or by being struck with a hammer. It is appropriate, rather, that we should now consider, in the light of our previous knowledge, how such strings may possibly vibrate, leaving on one side, for later consideration, the mechanism of their excitation and the differences which may thereby arise.

2.2. A GENERAL RESULT

Imagine a uniform, flexible string, of total length l and total mass ml, forming a closed loop. In this connection 'uniform' implies constancy of cross-sectional area and homogeneity of material. Suppose this loop to be slightly stretched so as to fit closely around the outer cylindrical surface of a cylinder of radius r ($2\pi r$, the circumference of the right section of the cylinder, being infinitesimally greater than l). In this state let the tension in the string be T. It is required to find the magnitude of the outwards force on a small length Δl of the string in contact with the cylinder ($\Delta l \ll r$). The situation is as represented in fig. 1. Here $\Delta\theta$, the angle subtended at the centre of the circular section of the cylinder by the element of length of string under consideration, is given by $\Delta\theta = \Delta l/r$, and this small portion of the string is assumed to be held in equilibrium by forces T at its extremities and a resultant