HEINRICH HERTZ

THE

PRINCIPLES OF MECHANICS

PRESENTED IN A NEW FORM

Preface by H. VON HELMHOLTZ; *authorized English translation by* D. E. JONES *and* J. T. WALLEY; *with a new Introduction by* ROBERT S. COHEN, *Assistant Professor of Physics and Philosophy, Wesleyan University*

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them are fuller than the manuscript as finally prepared for the press. With regard to two paragraphs of the work I have found it impossible to satisfy myself, the author's intention as to the final form having remained doubtful to me. I have marked these two paragraphs and have thought it best to leave them entirely unaltered.

After sending off the manuscript the author had noted certain corrections in a second copy; all these have been included before printing off. I have completed the references to earlier paragraphs of the book (of which few were given in the second part, and scarcely any in the last chapter), and have drawn up an index to the definitions and notation.

P. LENARD.

TRANSLATORS' NOTE

HERTZ'S *Principles of Mechanics* forms the third (and last) volume of his collected works, as edited by Dr. Philipp Lenard. English translations of the first and second volumes (*Miscellaneous Papers* and *Electric Waves*) have already been published.

The translation of the first two volumes was comparatively easy; the third has proved to be a more difficult undertaking. If it has been brought to a satisfactory conclusion this will be largely due to Professor Lenard, through whose hands the proof-sheets have passed. He has again, notwithstanding the pressure of other work, been good enough to advise and assist us from time to time, and we tender to him our warmest thanks.

We also desire to thank the publishers and printers for the extreme consideration shown by them while the book was being prepared for the press.

D. E. J.
J. T. W.

September 1899.

PREFACE BY H. VON HELMHOLTZ

ON the 1st of January 1894 Heinrich Hertz died. All who regard human progress as consisting in the broadest possible development of the intellectual faculties, and in the victory of the intellect over natural passions as well as over the forces of nature, must have heard with the deepest sorrow of the death of this highly favoured genius. Endowed with the rarest gifts of intellect and of character, he reaped during his lifetime (alas, so short!) a bounteous harvest which many of the most gifted investigators of the present century have tried in vain to gather. In old classical times it would have been said that he had fallen a victim to the envy of the gods. Here nature and fate appeared to have favoured in an exceptional manner the development of a human intellect embracing all that was requisite for the solution of the most difficult problems of science,—an intellect capable of the greatest acuteness and clearness in logical thought, as well as of the closest attention in observing apparently insignificant phenomena. The uninitiated readily pass these by without heeding them; but to the practised eye they point the way by which we can penetrate into the secrets of nature.

Heinrich Hertz seemed to be predestined to open up to mankind many of the secrets which nature has hitherto concealed from us; but all these hopes were frustrated by the malignant disease which, creeping slowly but surely on, robbed us of this precious life and of the achievements which it promised.

To me this has been a deep sorrow; for amongst all my pupils I have ever regarded Hertz as the one who had penetrated furthest into my own circle of scientific thought, and it was to him that I looked with the greatest confidence for the further development and extension of my work.

Heinrich Rudolf Hertz was born on 22nd February 1857, in Hamburg, and was the eldest son of Dr. Hertz, who was then a barrister and subsequently became senator. Up to the time of his confirmation he was a pupil in one of the municipal primary schools (*Bürgerschulen*). After a year's preparation at home he entered the High School of his native town, the *Johanneum;* here he remained until 1875, when he received his certificate of matriculation. As a boy he won the appreciation of his parents and teachers by his high moral character. Already his pursuits showed his natural inclinations. While still attending school he worked of his own accord at the bench and lathe, on Sundays he attended the Trade School to practise geometrical drawing, and with the simplest appliances he constructed serviceable optical and mechanical instruments.

At the end of his school course he had to decide on his career, and chose that of an engineer. The modesty which in later years was such a characteristic feature of his nature, seems to have made him doubtful of his talent for theoretical science. He liked mechanical work, and felt surer of success in connection with it, because he already knew well enough what it meant and what it required. Perhaps, too, he was influenced by the tone prevailing in his native town and tending towards a practical life. It is in young men of unusual capacity that one most frequently observes this sort of timid modesty. They have a clear conception of the difficulties which have to be overcome before attaining the high ideal set before their minds; their strength must be tried by some practical test before they can secure the self-reliance requisite for their difficult task. And even in later years men of great ability are the less content with their own achievements the higher their capacity and ideals. The most gifted attain the highest and truest

success because they are most keenly alive to the presence of imperfection and most unwearied in removing it.

For fully two years Heinrich Hertz remained in this state of doubt. Then, in the autumn of 1877, he decided upon an academic career; for as he grew in knowledge he grew in the conviction that only in scientific work could he find enduring satisfaction. In the autumn of 1878 he came to Berlin, and it was as an university student there, in the physical laboratory under my control, that I first made his acquaintance. Even while he was going through the elementary course of practical work, I saw that I had here to deal with a pupil of quite unusual talent; and when, towards the end of the summer semester, it fell to me to propound to the students a subject of physical research for a prize, I chose one in electromagnetics, in the belief that Hertz would feel an interest in it, and would attack it, as he did, with success.

In Germany at that time the laws of electromagnetics were deduced by most physicists from the hypothesis of W. Weber, who sought to trace back electric and magnetic phenomena to a modification of Newton's assumption of direct forces acting at a distance and in a straight line. With increasing distance these forces diminish in accordance with the same laws as those assigned by Newton to the force of gravitation, and held by Coulomb to apply to the action between pairs of electrified particles. The force was directly proportional to the product of the two quantities of electricity, and inversely proportional to the square of their distance apart; like quantities produced repulsion, unlike quantities attraction. Furthermore, in Weber's hypothesis it was assumed that this force was propagated through infinite space instantaneously, and with infinite velocity. The only difference between the views of W. Weber and of Coulomb consisted in this—that Weber assumed that the magnitude of the force between the two quantities of electricity might be affected by the velocity with which the two quantities approached towards or receded from one another, and also by the acceleration of such velocity.

Side by side with Weber's theory there existed a number of others, all of which had this in common—that they regarded the magnitude of the force expressed by Coulomb's law as being modified by the influence of some component of the velocity of the electrical quantities in motion. Such theories were advanced by F. E. Neumann, by his son C. Neumann, by Riemann, Grassmann, and subsequently by Clausius. Magnetised molecules were regarded as the axes of circular electric currents, in accordance with an analogy between their external effects previously discovered by Ampère.

This plentiful crop of hypotheses had become very unmanageable, and in dealing with them it was necessary to go through complicated calculations, resolutions of forces into their components in various directions, and so on. So at that time the domain of electromagnetics had become a pathless wilderness. Observed facts and deductions from exceedingly doubtful theories were inextricably mixed up together. With the object of clearing up this confusion I had set myself the task of surveying the region of electromagnetics, and of working out the distinctive consequences of the various theories, in order, wherever that was possible, to decide between them by suitable experiments.

I arrived at the following general result. The phenomena which completely closed currents produce by their circulation through continuous and closed metallic circuits, and which have this common property, that while they flow there is no considerable variation in the electric charges accumulated upon the various parts of the conductor,—all these phenomena can be equally well deduced from any of the above-mentioned hypotheses. The deductions which follow from them agree with Ampère's laws of electromagnetic action, with the laws discovered by Faraday and Lenz, and also with the laws of induced electric currents as generalised by F. E. Neumann. On the other hand, the deductions which follow from them in the case of conducting circuits which are not completely closed are essentially different. The accordance between the various

theories and the facts which have been observed in the case of completely closed circuits is easily intelligible when we consider that closed currents of any desired strength can be maintained as long as we please—at any rate long enough to allow the forces exerted by them to exhibit plainly their effects; and that on this account the actual effects of such currents and their laws are well known and have been carefully investigated. Thus any divergence between any newly-advanced theory and any one of the known facts in this well-trodden region would soon attract attention and be used to disprove the theory.

But at the open ends of unclosed conductors between which insulating masses are interposed, every motion of electricity along the length of the conductor immediately causes an accumulation of electric charges; these are due to the surging of the electricity, which cannot force its way through the insulator, against the ends of the conductor. Between the electricity accumulated at the end and the electricity of the same kind which surges against it there is a force of repulsion; and an exceedingly short time suffices for this force to attain such magnitude that it completely checks the flow of the electricity. The surging then ceases; and after an instant of rest there follows a resurging of the accumulated electricity in the opposite direction.

To every one who was initiated into these matters it was then apparent that a complete understanding of the theory of electromagnetic phenomena could only be attained by a thorough investigation of the processes which occur during these very rapid surgings of unclosed currents. W. Weber had endeavoured to remove or lessen certain difficulties in his electromagnetic hypothesis by suggesting that electricity might possess a certain degree of inertia, such as ponderable matter exhibits. In the opening and closing of every electric current effects are produced which simulate the appearance of such electric inertia. These, however, arise from what is called electromagnetic induction, *i.e.* from a mutual action of neighbouring conductors upon each other, according to laws which have been well known since Faraday's time. True inertia should be proportional only to the mass of the electricity in motion, and independent of the position of the conductor. If anything of the kind existed we ought to be able to detect it by a retardation in electric oscillations, such as are produced by the sudden break of an electric current in metallic wires. In this manner it should be possible to find an upper limit to the magnitude of this electric inertia; and so I was led to propound the problem of carrying out experiments on the magnitude of extra-currents. Extra-currents in double-wound spirals, the currents traversing the branches in opposite directions, were suggested in the statement of the problem as being apparently best adapted for these experiments. Heinrich Hertz's first research of importance consisted in solving this problem. In it he gives a definite answer to the question propounded, and shows that of the extra-current in a double-wound spiral $\frac{1}{30}$ to $\frac{1}{20}$ at most could be ascribed to the effect of an inertia of electricity. The prize was awarded to him for this investigation.

But Hertz did not confine himself to the experiments which had been suggested. For he recognised that although the effects of induction are very much weaker in wires which are stretched out straight, they can be much more accurately calculated than in spirals of many turns; for in the latter he could not measure with accuracy the geometrical relations. Hence he used for further experiments a conductor consisting of two rectangles of straight wire; he now found that the extra-current due to inertia could at most not exceed $\frac{1}{250}$ of the magnitude of the induction current.

Investigations on the effect of centrifugal force in a rapidly rotating plate upon the motion of electricity passing through it, led him to find a still lower value to the upper limit of the inertia of electricity.

These experiments clearly impressed upon his mind the exceeding mobility of electricity, and pointed out to him the way towards his most important discoveries.

Meanwhile in England the ideas introduced by Faraday as

to the nature of electricity were extending. These ideas, expressed as they were in abstract language difficult of comprehension, made but slow progress until they found in Clerk Maxwell a fitting interpreter. In explaining electrical phenomena Faraday was bent upon excluding all preconceived notions involving assumptions as to the existence of phenomena or substances which could not be directly perceived. Especially did he reject, as did Newton at the beginning of his career, the hypothesis of the existence of action-at-a-distance. What the older theories assumed seemed to him inconceivable—that direct actions could go on between bodies separated in space without any change taking place in the intervening medium. So he first sought for indications of changes in media lying between electrified bodies or between magnetic bodies. He succeeded in detecting magnetism or diamagnetism in nearly all the bodies which up to that time had been regarded as non-magnetic. He also showed that good insulators undergo a change when exposed to the action of electric force; this he denoted as the "dielectric polarisation of insulators."

It could not be denied that the attraction between two electrically charged bodies or between two magnet poles in the direction of their lines of force was considerably increased by introducing between them dielectrically or magnetically polarised media. On the other hand there was a repulsion across the lines of force. After these discoveries men were bound to recognise that a part of the magnetic and electric action was produced by the polarisation of the intervening medium; another part might still remain, and this might be due to action-at-a-distance.

Faraday and Maxwell inclined towards the simpler view that there was no action-at-a-distance; this hypothesis, which involved a complete upsetting of the conceptions hitherto current, was thrown into mathematical form and developed by Maxwell. According to it the seat of the changes which produce electrical phenomena must be sought only in the insulators; the polarisation and depolarisation of these are the real causes of the electrical disturbances which apparently take place in conductors. There were no longer any closed currents; for the accumulation of electric charges at the ends of a conductor, and the simultaneous dielectric polarisation of the medium between them, represented an equivalent electric motion in the intervening dielectric, thus completing the gap in the circuit.

Faraday had a very sure and profound insight into geometrical and mechanical questions; and he had already recognised that the distribution of electric action in space according to these new views must exactly agree with that found according to the older theory.

By the aid of mathematical analysis Maxwell confirmed this, and extended it into a complete theory of electromagnetics. For my own part, I fully recognised the force of the facts discovered by Faraday, and began to investigate the question whether actions-at-a-distance did really exist, and whether they must be taken into account. For I felt that scientific prudence required one to keep an open mind at first in such a complicated matter, and that the doubt might point the way to decisive experiments.

This was the state of the question at the time when Heinrich Hertz attacked it after completing the investigation which we have described.

It was an essential postulate of Maxwell's theory that the polarisation and depolarisation of an insulator should produce in its neighbourhood the same electromagnetic effects as a galvanic current in a conductor. It seemed to me that this should be capable of demonstration, and that it would constitute a problem of sufficient importance for one of the great prizes of the Berlin Academy.

In the Introduction to his interesting book, *Untersuchungen über die Austreitung der elektrischen Kraft*,[1] Hertz has described how his own discoveries grew out of the seeds thus

[1] [*Electric Waves.* London, Macmillan, 1893.]

sown by his contemporaries, and has done this in such an admirably clear manner that it is impossible for any one else to improve upon it or add anything of importance. His Introduction is of exceeding value as a perfectly frank and full account of one of the most important and suggestive discoveries. It is a pity that we do not possess more documents of this kind on the inner psychological history of science. We owe the author a debt of gratitude for allowing us to penetrate into the inmost working of his thoughts, and for recording even his temporary mistakes.

Something may, however, be added as to the consequences which follow from his discoveries.

The views which Hertz subsequently proved to be correct had been propounded, as we have already said, by Faraday and Maxwell before him as being possible, and even highly probable; but as yet they had not been actually verified. Hertz supplied the demonstration. The phenomena which guided him into the path of success were exceedingly insignificant, and could only have attracted the attention of an observer who was unusually acute, and able to see immediately the full importance of an unexpected phenomenon which others had passed by. It would have been a hopeless task to render visible by means of a galvanometer, or by any other experimental method in use at that time, the rapid oscillations of currents having a period as short as one ten-thousandth or even only a millionth of a second. For all finite forces require a certain time to produce finite velocities and to displace bodies of any weight, even when they are as light as the magnetic needles of our galvanometers usually are. But electric sparks can become visible between the ends of a conductor even when the potential at its ends only rises for a millionth of a second high enough to cause sparking across a minute air-gap. Through his earlier investigations Hertz was thoroughly familiar with the regularity and enormous velocity of these rapid electric oscillations; and when he essayed in this way to discover and render visible the most transient electric disturbances, success was not long in coming. He very soon discovered what were the conditions under which he could produce in unclosed conductors oscillations of sufficient regularity. He proceeded to examine their behaviour under the most varied circumstances, and thus determined the laws of their development. He next succeeded in measuring their wave-length in air and their velocity. In the whole investigation one scarcely knows which to admire most, his experimental skill or the acuteness of his reasoning, so happily are the two combined.

By these investigations Hertz has enriched physics with new and most interesting views respecting natural phenomena. There can no longer be any doubt that light-waves consist of electric vibrations in the all-pervading ether, and that the latter possesses the properties of an insulator and a magnetic medium. Electric oscillations in the ether occupy an intermediate position between the exceedingly rapid oscillations of light and the comparatively slow disturbances which are produced by a tuning-fork when thrown into vibration; but as regards their rate of propagation, the transverse nature of their vibrations, the consequent possibility of polarising them, their refraction and reflection, it can be shown that in all these respects they correspond completely to light and to heat-rays. The electric waves only lack the power of affecting the eye, as do also the dark heat-rays, whose frequency of oscillation is not high enough for this.

Here we have two great natural agencies—on the one hand light, which is so full of mystery and affects us in so many ways, and on the other hand electricity, which is equally mysterious, and perhaps even more varied in its manifestations: to have furnished a complete demonstration that these two are most closely connected together is to have achieved a great feat. From the standpoint of theoretical science it is perhaps even more important to be able to understand how apparent actions-at-a-distance really consist in a propagation of an action from one layer of an intervening medium to the next. Gravitation still remains an unsolved puzzle; as

yet a satisfactory explanation of it has not been forthcoming, and we are still compelled to treat it as a pure action-at-a-distance.

Amongst scientific men Heinrich Hertz has secured enduring fame by his researches. But not through his work alone will his memory live; none of those who knew him can ever forget his uniform modesty, his warm recognition of the labours of others, or his genuine gratitude towards his teachers. To him it was enough to seek after truth; and this he did with all zeal and devotion, and without the slightest trace of self-seeking. Even when he had some right to claim discoveries as his own he preferred to remain quietly in the background. But although naturally quiet, he could be merry enough amongst his friends, and could enliven social intercourse by many an apt remark. He never made an enemy, although he knew how to judge slovenly work, and to appraise at its true value any pretentious claim to scientific recognition.

His career may be briefly sketched as follows. In the year 1880 he was appointed Demonstrator in the Physical Laboratory of the Berlin University. In 1883 he was induced by the Prussian Education Department (*Kultusministerium*) to go to Kiel with a view to his promotion to the office of Privat-docent there. In Easter of 1885 he was called to Karlsruhe as ordinary Professor of Physics at the Technical School. Here he made his most important discoveries, and it was during his stay at Karlsruhe that he married Miss Elizabeth Doll, the daughter of one of his colleagues. Two years later he received a call to the University of Bonn as ordinary Professor of Physics, and removed thither in Easter 1889.

Few as the remaining years of his life unfortunately were, they brought him ample proof that his work was recognised and honoured by his contemporaries. In the year 1888 he was awarded the Matteucci Medal of the Italian Scientific Society, in 1889 the La Caze Prize of the Paris Academy of Sciences and the Baumgartner Prize of the Imperial Academy of Vienna, in 1890 the Rumford Medal of the Royal Society, and in 1891 the Bressa Prize of the Turin Royal Academy. He was elected a corresponding member of the Academies of Berlin, Munich, Vienna, Göttingen, Rome, Turin, and Bologna, and of many other learned societies; and the Prussian Government awarded him the Order of the Crown.

He was not long spared to enjoy these honours. A painful abscess began to develop, and in November 1892 the disease became threatening. An operation performed at that time appeared to relieve the pain for a while. Hertz was able to carry on his lectures, but only with great effort, up to the 7th of December 1893. On New Year's day of 1894 death released him from his sufferings.

In the present treatise on the Principles of Mechanics, the last memorial of his labours here below, we again see how strong was his inclination to view scientific principles from the most general standpoint. In it he has endeavoured to give a consistent representation of a complete and connected system of mechanics, and to deduce all the separate special laws of this science from a single fundamental law which, logically considered, can, of course, only be regarded as a plausible hypothesis. In doing this he has reverted to the oldest theoretical conceptions, which may also be regarded as the simplest and most natural; and he propounds the question whether these do not suffice to enable us to deduce, by consistent and rigid methods of proof, all the recently discovered general principles of mechanics, even such as have only made their appearance as inductive generalisations.

The first scientific development of mechanics arose out of investigations on the equilibrium and motion of solid bodies which were directly connected with one another; we have examples of these in the simple mechanics, the lever, pulleys, inclined planes, etc. The law of virtual velocities is the earliest general solution of all the problems which thus arise. Later on Galileo developed the conception of inertia and of the accelerating action of force, although he represented this as

consisting of a series of impulses. Newton first conceived the idea of action-at-a-distance, and showed how to determine it by the principle of equal action and reaction. It is well known that Newton, as well as his contemporaries, at first only accepted the idea of direct action-at-a-distance with the greatest reluctance.

From that time onwards Newton's idea and definition of force served as a basis for the further development of mechanics. Gradually men learned how to handle problems in which conservative forces were combined with fixed connections; of these the most general solution is given by d'Alembert's Principle. The chief general propositions in mechanics (such as the law of the motion of the centre of gravity, the law of areas for rotating systems, the principle of the conservation of *vis viva*, the principle of least action) have all been developed from the assumption of Newton's attributes of constant, and therefore conservative, forces of attraction between material points, and of the existence of fixed connections between them. They were originally discovered and proved only under these assumptions. Subsequently it was discovered by observation that the propositions thus deduced could claim a much more general validity in nature than that which followed from the mode in which they were demonstrated. Hence it was concluded that certain general characteristics of Newton's conservative forces of attraction were common to all the forces of nature; but no proof was forthcoming that this generalisation could be deduced from any common basis. Hertz has now endeavoured to furnish mechanics with such a fundamental conception from which all the laws of mechanics which have been recognised as of general validity can be deduced in a perfectly logical manner. He has done this with great acuteness, making use in an admirable manner of new and peculiar generalised kinematical ideas. He has chosen as his starting-point that of the oldest mechanical theories, namely, the conception that all mechanical processes go on as if the connections between the various parts which act upon each other were fixed. Of course he is obliged to make the further hypothesis that there are a large number of imperceptible masses with invisible motions, in order to explain the existence of forces between bodies which are not in direct contact with each other. Unfortunately he has not given examples illustrating the manner in which he supposed such hypothetical mechanism to act; to explain even the simplest cases of physical forces on these lines will clearly require much scientific insight and imaginative power. In this direction Hertz seems to have relied chiefly on the introduction of cyclical systems with invisible motions.

English physicists—*e.g.* Lord Kelvin, in his theory of vortex-atoms, and Maxwell, in his hypothesis of systems of cells with rotating contents, on which he bases his attempt at a mechanical explanation of electromagnetic processes—have evidently derived a fuller satisfaction from such explanations than from the simple representation of physical facts and laws in the most general form, as given in systems of differential equations. For my own part, I must admit that I have adhered to the latter mode of representation and have felt safer in so doing; yet I have no essential objections to raise against a method which has been adopted by three physicists of such eminence.

It is true that great difficulties have yet to be overcome before we can succeed in explaining the varied phenomena of physics in accordance with the system developed by Hertz. But in every respect his presentation of the *Principles of Mechanics* is a book which must be of the greatest interest to every reader who can appreciate a logical system of dynamics developed with the greatest ingenuity and in the most perfect mathematical form. In the future this book may prove of great heuristic value as a guide to the discovery of new and general characteristics of natural forces.

AUTHOR'S PREFACE

All physicists agree that the problem of physics consists in tracing the phenomena of nature back to the simple laws of mechanics. But there is not the same agreement as to what these simple laws are. To most physicists they are simply Newton's laws of motion. But in reality these latter laws only obtain their inner significance and their physical meaning through the tacit assumption that the forces of which they speak are of a simple nature and possess simple properties. But we have here no certainty as to what is simple and permissible, and what is not: it is just here that we no longer find any general agreement. Hence there arise actual differences of opinion as to whether this or that assumption is in accordance with the usual system of mechanics, or not. It is in the treatment of new problems that we recognise the existence of such open questions as a real bar to progress. So, for example, it is premature to attempt to base the equations of motion of the ether upon the laws of mechanics until we have obtained a perfect agreement as to what is understood by this name.

The problem which I have endeavoured to solve in the present investigation is the following:—To fill up the existing gaps and to give a complete and definite presentation of the laws of mechanics which shall be consistent with the state of our present knowledge, being neither too restricted nor too extensive in relation to the scope of this knowledge. The presentation must not be too restricted: there must be no natural motion which it does not embrace. On the other