In regular trochoidal waves the particles move in vertical circles with a constant velocity and are always subject to the same pressure. Of the energy of disturbance half goes to give motion to the particles and half to raise them from their initial position to the mean height which they occupy during the passage of the wave.

Now the mean horizontal positions of the particles remain unaltered by the waves, hence, since their velocities are constant, none of their energy of motion is transmitted; nor since the pressure on each particle is constant can any energy be transmitted by pressure. The only energy therefore which remains to be transmitted is the energy due to elevation, and that this is transmitted is obvious since the particles are moving forward when above their mean position, and backward when below it. This energy constitutes half the energy of disturbance, and this is therefore the amount transmitted.

For a definite mathematical proof that—

In waves on deep water the rate at which the energy is carried forward is ½ the energy of disturbance per unit of length × by the rate of propagation.

Let h_0 be the initial height occupied by a particle supposed to be of unit weight, h_1 the height of the centre of the circle in which it moves as the wave passes, r the radius of the orbit, and θ the angle the radius vector makes with the horizontal diameter, then the height of the particle above its initial position is $h_1 - h_0 + r \sin \theta$, adding to this the height due to its velocity we have the whole energy of disturbance—

$$= 2(h_1 - h_0) + r \sin \theta.$$

The velocity of the particle is—

$$= \sqrt{2g(h_1 - h_0)},$$

and the horizontal component of this is—

$$= \sqrt{2g(h_1 - h_0)}.\sin \theta.$$

Therefore the rate at which energy is being transmitted by the particle—

$$= \left\{2(h_1 - h_0) + r \sin \theta\right\} \sqrt{2g(h_1 - h_0)}.\sin \theta.$$

and the mean of this—

$$= \frac{1}{2\pi}\int_0^{2\pi} \left\{2(h_1 - h_0) + r \sin \theta\right\} \sqrt{2g(h_1 - h_0)} \sin \theta \, d\theta$$

$$= \tfrac{1}{2} r \sqrt{2g(h_1 - h_0)},$$

and if λ be the length of the wave, and $n\lambda$ the rate of propagation—

$$h_1 - h_0 = \frac{\pi r^2}{\lambda} \text{ and } \frac{2g}{\lambda} = 4\pi r^2,$$

∴ the mean rate at which energy is transmitted by this particle

$$= n\lambda(h_1 - h_0),$$

or the rate of propagation multiplied by half the energy of disturbance. Q.E.D.

It now remains to come back to the speed of the groups of waves and to show that *if the rate at which energy is transmitted is equal to the rate of propagation multiplied by half the energy of disturbance, then the velocity of a group of waves will be ½ that of the individual waves.*

Let P_1, P_2, P_3, P_4 be points similarly situated in a series of waves which gradually diminish in size and energy of disturbance from P_3 to P_1, in which direction they are moving. Let E be the energy of disturbance between P_1 and P_2 at time t, $E + a$ the energy between P_2 and P_3, $E + 2a$ between P_3 and P_4, and so on.

Then at the time $t + n$ after the wave has moved through one wave-length it follows that the energy between P_1 and P_2 will be—

$$= \frac{E + E + a}{2} = E + \frac{a}{2},$$

and between P_2 and P_3 will

$$= \frac{E + a + E + 2a}{2} = E + \frac{3a}{2},$$

and again after another interval, n, the energies between P_1 and P_2, P_2 and P_3 will be respectively—

$$= \frac{E + \frac{a}{2} + E + \frac{3a}{2}}{2} = E + a,$$

$$= \frac{E + \frac{3a}{2} + E + \frac{5a}{2}}{2} = E + 2a.$$

So that after the waves have advanced through two wave-lengths the distribution of the energy will have advanced one, or the speed of the groups is ½ that of the waves. Q.E.D.

Of course this reasoning applies equally to the waves on the suspended balls, when connected by an elastic string, as to water; and in this case the conclusions may be verified for, as on water, the groups of waves travel at a slower rate than the waves. This experiment tends to throw light on the manner in which the result is brought about. When a ball is disturbed, the disturbance is partly communicated to the adjacent ball by the connecting string, and part retained in the form of pendulous oscillation; that part which is propagated forward is constantly reduced in imparting oscillations to the successive balls and soon dies out, while the motion retained by the swinging pendulum constantly gives rise to succeeding waves until it is all absorbed. If the tightness of the cord be adjusted to the length of the suspending threads, waves may be made to travel along in a manner closely resembling the way in which they travel on water, the speed of the group being ½ the speed of the individual waves.

Although the progression of a group has hitherto been spoken of as if the form of the group was unaltered, this is by no means the case as a rule.

In the mathematical investigation it was assumed that the motion of the particles is circular; this, however, cannot be the case when the succeeding waves differ in size by a sensible quantity, and hence in this case the form of the group cannot be permanent. And it may be further shown that as a small group proceeds, the number of waves which compose it will continually increase, until the gradation becomes indefinitely small; and this is exactly what is observed, whether on water or on the strings.

So far as we have considered deep water, when the water is shallow compared with the length of the waves, the results are modified, but in this case the results as observed are strictly in accordance with the theory.

According to this, as waves enter shallow water the motion of the particles becomes elliptical, the eccentricity depending on the shallowness of the water; and it may be shown that under these circumstances the rate at which energy is transmitted is increased, until when the elliptic paths approach to straight lines the whole energy is transmitted, and consequently it follows that the rates of the speed of the groups to the speed of the waves will increase as the water becomes shallower, until they are sensibly the same. In which case only the groups of waves are permanent, and Mr. Scott Russell's solitary wave is possible. Besides the explanation thus given of these various phenomena, it appears that we have here a means of making some important verifications of the assumptions on which the wave theory is based; for the relative speed of the groups and the waves which compose them affords a criterion as to whether or not the particles move in circles.

SECTION D.—BIOLOGY.

Department of Anthropology.

ADDRESS BY FRANCIS GALTON, F.R.S.

PERMIT me to say a few words of personal explanation to account for the form of the address I am about to offer. It has been the custom of my predecessors to give an account of recent proceedings in anthropology, and to touch on many branches of that wide subject. But I am at this moment unprepared to follow their example with the completeness I should desire and you have a right to expect, owing to the suddenness with which I have been called upon to occupy this chair. I had indeed the honour of being nominated to the post last spring, but circumstances arising which made it highly probable that I should be prevented from attending this meeting, I was compelled to ask to be superseded. New arrangements were then made by the Council, and I thought no more about the matter. However, at the last moment, the accomplished ethnologist who otherwise would have presided over you was himself debarred by illness from attending, and the original plan had to be reverted to.

Under these circumstances I thought it best to depart somewhat from the usual form of addresses, and to confine myself to certain topics with which I happen to have been recently engaged, even at the risk of incurring the charge of submitting to you a memoir rather than an address.

I propose to speak of the study of those groups of men who are sufficiently similar in their mental characters or in their physiognomy, or in both, to admit of classification; and I especially desire to show that many methods exist of pursuing the inquiry in a strictly scientific manner, although it has hitherto been too often conducted with extreme laxity.

The types of character of which I speak are such as those described by Theophrastus, La Bruyére, and others, or such as may be read of in ordinary literature and are universally recognised as being exceedingly true to nature. There are no worthier professors of this branch of anthropology than the writers of the higher works of fiction, who are ever on the watch to discriminate varieties of character, and who have the art of describing them. It would, I think, be a valuable service to anthropology if some person well versed in literature were to compile a volume of extracts from novels and plays that should illustrate the prevalent types of human character and temperament. What, however, I especially wish to point out is, that it has of late years become possible to pursue an inquiry into certain fundamental qualities of the mind by the aid of exact measurements. Most of you are aware of the recent progress of what has been termed psycho-physics, or the science of subjecting mental processes to physical measurements and to physical laws. I do not now propose to speak of the laws that have been deduced, such as that which is known by the name of Fechner, and its numerous offshoots, including the law of fatigue, but I will briefly allude to a few instances of measurement of mental processes, merely to recall them to your memory. They will show what I desire to lay stress upon, that the very foundations of the differences between the mental qualities of man and man admit of being gauged by a scale of inches and a clock.

Take, for example, the rate at which a sensation or a volition travels along the nerves, which has been the subject of numerous beautiful experiments. We now know that it is far from instantaneous, having indeed no higher velocity than that of a railway express train. This slowness of pace, speaking relatively to the requirements that the nerves have to fulfil, is quite sufficient to account for the fact that very small animals are quicker than very large ones in evading rapid blows, and for the other fact that the eye and the ear are situated in almost all animals in the head, in order that as little time as possible should be lost on the road, in transmitting their impressions to the brain. Now the velocity of the complete process of to and fro nerve transmission in persons of different temperaments has not been yet ascertained with the desired precision. Such difference as there may be is obviously a fundamental characteristic and one that well deserves careful examination. I may take this opportunity of suggesting a simple inquiry that would throw much light on the degree in which its velocity varies in different persons, and how far it is correlated with temperament and external physical characteristics. Before I describe the inquiry I suggest, and towards which I have already collected a few data, it is necessary that I should explain the meaning of a term in common use among astronomers, namely, "personal equation." It is a well known fact that different observers make different estimates of the exact moment of the occurrence of any event. There is a common astronomical observation, in which the moment has to be recorded at which a star that is travelling athwart the field of view of a fixed telescope, crosses the fine vertical wire by which that field of view is intersected. In making this observation it is found that some observers are over sanguine and anticipate the event, while others are sluggish and allow the event to pass by before they succeed in noting it. This is by no means the effect of inexperience or maladroitness, but it is a persistent characteristic of each individual, however practised in the art of making observations or however attentive he may be. The difference between the time of a man's noting the event and that of its actual occurrence is called his personal equation. It remains curiously constant in every case for successive years, it is carefully ascertained for every assistant in every observatory, it is published along with his observations, and is applied to them just as a correction would be applied to measurements made by a foot-rule, that was known to be too long or too short by some definite amount. Therefore the magnitude of a man's personal equation indicates a very fundamental peculiarity of his constitution; and the inquiry I would suggest, is to make a comparison of the age, height, weight, colour of hair and eyes, and temperament (so far as it may admit of definition) in each observer in the various observatories at home and abroad, with the amount of his personal equation. We should thus learn how far the more obvious physical characteristics may be correlated with certain mental ones, and we should perhaps obtain a more precise scale of temperaments than we have at present.

Another subject of exact measurement is the time occupied in forming an elementary judgment. If a simple signal be suddenly shown, and if the observer presses a stop as quickly as he can when he sees it, some little time will certainly be lost, owing to delay in nerve transmission and to the sluggishness of the mechanical apparatus. In making experiments on the rate of judgment, the amount of this interval is first ascertained. Then the observer prepares himself for the exhibition of a signal that may be either black or white, but he is left ignorant which of the two it will be. He is to press a stop with his right hand in the first event, and another stop with his left hand in the second one. The trial is then made, and a much longer interval is found to have elapsed between the exhibition of the alternative signal, and the record of it, than had elapsed when a simple signal was used. There has been hesitation and delay: in short, the simplest act of judgment is found to consume a definite time. It is obvious that here, again, we have means of ascertaining differences in the rapidity of forming elementary judgments and of classifying individuals accordingly.

It would be easy to pursue the subject of the measurement of mental qualities to considerable length, by describing other kinds of experiment, for they are numerous and varied. Among these is the plan of Prof. Jevons, of suddenly exhibiting an unknown number of beans in a box, and requiring an estimate of their number to be immediately called out. A comparison of the estimate with the fact, in a large number of trials, brought out a very interesting scale of the accuracy of such estimates, which would of course vary in different individuals, and might be used as a means of classification. I can imagine few greater services to anthropology than the collection of the various experiments that have been imagined to reduce the faculties of the mind to exact measurement. They have engaged the attention of the highest philosophers, but have never, so far as I am aware, been brought compendiously together, and have certainly not been introduced, as they deserve, to general notice.

Wherever we are able to perceive differences by inter-comparison, we may reasonably hope that we may at some future time succeed in submitting those differences to measurement. The history of science is the history of such triumphs. I will ask your attention to a very notable instance of this, namely, that of the establishment of the scale of the thermometer. You are aware that the possibility of making a standard thermometric scale wholly depends upon that of determining two fixed points of temperature, the interval between them being graduated into a scale of equal parts. These points are, I need hardly say, the temperatures of freezing and of boiling water respectively. On this basis we are able to record temperature with minute accuracy, and the power of doing so has been one of the most important aids to physics and chemistry as well as to other branches of investigation. We have been so accustomed, from our childhood, to hear of degrees of temperature, and our scientific knowledge is so largely based upon exact thermometric measurement, that we cannot easily realise the state of science when the thermometer, as we now use it, was unknown. Yet such was the condition of affairs so recently as two hundred years ago, or thereabouts. The invention of the thermometer, in its present complete form, was largely due to Boyle, and I find in his "Memoirs" (London, 1772, vol. vi. p. 403) a letter that cannot fail to interest us, since it well expresses the need of exact measurement that was then felt in a particular case, where it was soon eminently well supplied, and therefore encourages hope that our present needs as anthropologists may hereafter, in some way or other, be equally well satisfied. The letter is from Dr. John Beale, a great friend and correspondent of Boyle, and is dated February, 1663. He says in it:—

"I see by several of my own thermometers that the glass-men are by you so well instructed to make the stems in equal proportions, that if we could name some degrees, ... we might by the proportions of the glass make our discourses intelligible in mentioning what degrees of cold our greatest frosts do produce. . . . . If we can discourse of heat and cold in their several degrees, so as we may signify the same intelligibly, ... it is more than our forefathers have taught us to do hitherto."

The principal experiments by which the mental faculties may be measured require, unfortunately for us, rather costly and delicate apparatus, and until physiological laboratories are more numerous than at present, we can hardly expect that they will be pursued by many persons.

Let us now suppose that, by one or more of the methods I have described or alluded to, we have succeeded in obtaining a group of persons resembling one another in some mental quality, and that we desire to determine the external physical characteristics and features most commonly associated with it. I have nothing new to say as regards the usual anthropometric measurements, but I wish to speak of the great convenience of photographs in conveying those subtle but clearly visible peculiarities of outline which almost elude measurement. It is strange that no use is made of photography to obtain careful studies of the head and features. No single view can possibly exhibit the whole of a solid, but we require for that purpose views to be taken from three points at right angles to one another. Just as the architect requires to know the elevation, side view, and plan of a house, so the anthropologist ought to have the full face, profile, and view of the head from above of the individual whose features he is studying.

It might be a great convenience, when numerous portraits have to be rapidly and inexpensively taken for the purpose of anthropological studies, to arrange a solid framework supporting three mirrors, that shall afford the views of which I have been speaking, by reflection, at the same moment that the direct picture of the sitter is taken. He would present a three-quarter face to the camera for the direct picture, one adjacent mirror would reflect his profile towards it, another on the opposite side would reflect his full face, and a third sloping over him would reflect the head as seen from above. All the reflected images would lie at the same optical distance from the camera, and would, therefore, be on the same scale, but they would be on a somewhat smaller scale than the picture taken directly. The result would be an ordinary photographic picture of the sitter surrounded by three different views of his head. Scales of inches attached to the framework would appear in the picture and give the means of exact measurement.

Having obtained drawings or photographs of several persons alike in most respects, but differing in minor details, what sure method is there of extracting the typical characteristics from them? I may mention a plan which had occurred both to Mr. Herbert Spencer and myself, the principle of which is to superimpose optically the various drawings and to accept the aggregate result. Mr. Spencer suggested to me in conversation that the drawings reduced to the same scale might be traced on separate pieces of transparent paper and secured one upon another, and then held between the eye and the light. I have attempted this with some success. My own idea was to throw faint images of the several portraits, in succession, upon the same sensitised photographic plate. I may add that it is perfectly easy to superimpose optically two portraits by means of a stereoscope, and that a person who is used to handle instruments will find a common double eye-glass fitted with stereoscopic lenses to be almost as effectual and far handier than the boxes sold in shops.

In illustration of what I have said about photographic portraits, I will allude to some recent experiences of my own in a subject that I have still under consideration. In previous publications I have treated of men who have been the glory of mankind, I would now call your attention to those who are its disgrace. The particular group of men I have in view are the criminals of England, who have been condemned to long terms of penal servitude for various heinous offences.

It is needless to enlarge on the obvious fact that many persons have become convicts who, if they had been afforded the average chances of doing well, would have lived up to a fair standard of virtue. Neither need I enlarge on the other equally obvious fact, that a very large number of men escape criminal punishment, who in reality deserve it quite as much as an average convict. Making every allowance for these two elements of uncertainty, no reasonable man can entertain a doubt that the convict class includes a large proportion of consummate scoundrels, and that we are entitled to expect to find in any large body of convicts a prevalence of the truly criminal charactistics, whatever these may be.

Criminality, though not very various in its development, is extremely complex in its origin: nevertheless, certain general conclusions are arrived at by the best writers on the subject, among whom I would certainly rank Prosper Despine. The ideal criminal has three peculiarities of character; his conscience is almost deficient, his instincts are vicious, and his power of self-control is very weak. As a consequence of all this, he usually detests continuous labour. This statement applies to the criminal classes generally, the special conditions that determine the description of crime being the character of the instincts; and the fact of the absence of self-control being due to ungovernable temper, or to passion, or to mere imbecility.

The deficiency of conscience in criminals, as shown by the absence of genuine remorse for their guilt, appears to astonish all who first become familiar with the details of prison life. Scenes of heartrending despair are hardly ever witnessed among prisoners; their sleep is broken by no uneasy dreams—on the contrary, it is easy and sound; they have also excellent appetites. But hypocrisy is a very common vice; and all my information agrees in one particular, as to the utter untruthfulness of criminals, however plausible their statements may appear to be.

The subject of vicious instincts is a very large one; we must guard ourselves against looking upon them as perversions, inasmuch as they may be strictly in accordance with the healthy nature of the man, and, being transmissible by inheritance, may become the normal characteristics of a healthy race, just as the sheep-dog, the retriever, the pointer, and the bull-dog have their several instincts. There can be no greater popular error than the supposition that natural instinct is a perfectly trustworthy guide, for there are striking contradictions to such an opinion in individuals of every description of animal. All that we are entitled to say is, that the prevalent instincts of each race are trustworthy, not those of every individual. A man who is counted as an atrocious criminal by society, and is punished as such by the law, may nevertheless have acted in strict accordance with his instincts. The ideal criminal is deficient in qualities that oppose his vicious instincts; he has neither the natural regard for others which lies at the base of conscience, nor has he sufficient self-control to enable him to consider his own selfish interests in the long run. He cannot be preserved from criminal misadventure, either by altruistic or by intelligently egoistic sentiments.

It becomes an interesting question to know how far these peculiarities may be correlated with physical characteristics and features. Through the cordial and ready assistance of Sir Edmund Du Cane, the Surveyor-General of Prisons, who has himself contributed a valuable memoir to the Social Science Congress on the subject, I was enabled to examine the many thousand photographs of criminals that are preserved for purposes of identification at the Home Office, to visit prisons and confer with the authorities, and lastly to procure for my own private statistical inquiries a large number of copies of photographs of heinous criminals. I may as well say, that I begged that the photographs should be furnished me without any names attached to them, but simply classified in three groups according to the nature of the crime. The first group included murder, manslaughter, and burglary; the second group included felony and forgery; and the third group referred to sexual crimes. The photographs were of criminals who had been sentenced to long terms of penal servitude.

By familiarising myself with the collection, and continually sorting the photographs in tentative ways, certain natural classes began to appear, some of which are exceedingly well marked. It was also very evident that the three groups of criminals contributed in very different proportions to the different physiognomic classes.

This is not the place to go further into details: indeed my inquiry is far from complete. I merely quote my experiences in order to show the way in which questions of character, physiognomy, and temperament admit of being scientifically approached, and to give an instance of the helpfulness of photography. If I had had the profiles and the shape of the head as seen from above, my results would have been much more instructive. Thus, to take a single instance, I have seen many pencil studies in outline of selected criminal faces drawn by Dr. Clarke, the accomplished and zealous medical officer of Pentonville Prison; and in these sketches a certain very characteristic profile seemed to me conspicuously prevalent. I should have been very glad of photographs to corroborate this. So, again, if I had had photographic views of the head taken from above, I could have tested, among other matters, the truth of Prof. Benedict's assertion about the abnormally small size of the back of the head in criminals.

I have thus far spoken of the characters and physiognomy of well-marked varieties of men: the anthropologist has next to consider the life history of those varieties, and especially their tendency to perpetuate themselves, whether to displace other varieties and to spread, or else to die out. In illustration of this, I will proceed with what appears to be the history of the criminal class. Its perpetuation by heredity is a question that deserves more careful investigation than it has received, but it is on many accounts more difficult to grapple with than it may at first sight appear to be. The vagrant habits of the criminal classes, their illegitimate unions and extreme untruthfulness, are among the difficulties. It is, however, easy to show that the criminal nature tends to be inherited while, on the other hand, it is impossible that women who spend a large portion of the best years of their lives in prison can contribute many children to the population. The true state of the case appears to be that the criminal population receives steady accessions from classes who, without having strongly marked criminal natures, do nevertheless belong to a type of humanity that is exceedingly ill-suited to play a respectable part in our modern civilisation, though they are well-suited to flourish under half-savage conditions, being naturally both healthy and prolific. These persons are apt to go to the bad; their daughters consort with criminals and become the parents of criminals. An extraordinary example of this is given by the history of the infamous Jukes family in America, whose pedigree has been made out with extraordinary care, during no less than seven generations, and is the subject of an elaborate memoir printed in the thirty-first annual report of the Prison Association of New York, 1876. It includes no less than 540 individuals of Jukes blood, among whom the number of persons who degraded into criminality, pauperism, or disease, is frightful to contemplate.

It is difficult to summarise the results in a few plain figures, but I will state those respecting the fifth generation, through the eldest of the five prolific daughters of the man who is the common ancestor of the race. The total number of these was 103, of whom thirty-eight came through an illegitimate granddaughter, and eighty-five through legitimate grandchildren. Out of the thirty-eight, sixteen have been in gaol, six of them for heinous offences, one of these having been commited no less than nine times; eleven others were paupers or led openly disreputable lives; four were notoriously intemperate; the history of three had not been traced, and only four were known to have done well. The great majority of the women consorted with criminals. As to the 85 legitimate descendants, they were less flagrantly bad, for only five of them had been in gaol and only thirteen others had been paupers. Now the ancestor of all this mischief, who was born about the year 1730, is described as having been a hunter and a fisher, a jolly companionable man, averse to steady labour, working hard and idling by turns, and who had numerous illegitimate children, whose issue has not been traced. He was, in fact, a somewhat good specimen of a half-savage, without any seriously criminal instincts. The girls were apparently attractive, marrying early and sometimes not badly; but the gipsy-like character of the race was unsuited to success in a civilised country. So the descendants went to the bad, and the hereditary moral weaknesses they may have had rose to the surface and worked their mischief without a check. Cohabiting with criminals and being extremely prolific, the result was the production of a stock exceeding 500 in number, of a prevalent criminal type. Through disease and intemperance the breed is now rapidly diminishing; the infant mortality has of late been horrible among them, but fortunately the women of the present generation bear usually but few children, and many of them are altogether childless.

This is not the place to go further into details. I have alluded to the Jukes family in order to show what extremely important topics lie open to inquiry in a single branch of anthropological research and to stimulate others to follow it out. There can be no more interesting subject to us than the quality of the stock of our countrymen and of the human race generally, and there can be no more worthy inquiry than that which leads to an explanation of the conditions under which it deteriorates or improves.

SECTION G.—MECHANICAL SCIENCE.

THE following is an abstract of the address of the president, Mr. E. Woods, C.E:—The president selected the question of railway brakes as his topic. He said that the provision of adequate brake power to control trains was a subject which had latterly much engaged the attention of railway companies and of the Government. In the summer of 1874 a Royal Commission was appointed to inquire into the causes of accidents on railways, and the possibility of removing them by further legislation. One branch of the inquiry naturally led to the consideration of accidents caused by collision; and it appeared from the evidence taken before the Commissioners that trains were generally provided with insufficient controlling power, and that the distance within which, when running at high speed, they could be stopped by the brake ordinarily in use had not been ascertained with any approach to accuracy. It was under these circumstances that the Commissioners applied to the railway companies to institute a definite series of experiments to test the value of hand-brakes, and the effect of various systems of continuous brakes. In conjunction with Col. Inglis, R.E., he was intrusted by the Commissioners with the supervision of the experiments, to the satisfactory conduct of which the railway companies contributed in the most liberal manner. With few exceptions, and up to a comparatively recent period, the companies had remained content with the brake appliances which were common forty years ago. These, no doubt, were sufficient to control the trains in those early days, few as they were in number, and limited in weight and speed. The brakes were applied separately, and by hand-power, always to the tender, and usually to some few of the carriages and to the guard's van or vans, if such accompanied the train. As long ago as 1858 the Board of Trade called the special attention of the railway companies to the fact that the amount of brake power then habitually applied was insufficient to prevent frequent accidents occurring from collisions, many of which they considered might be averted. Particular reference was made to two systems which had come into daily use on the East Lancashire and the Lancashire and Yorkshire railways, namely, the brakes of Newall and of Fay, by means of which trains of ninety to 100 tons weight, running fifty miles an hour, could be effectually controlled by driver or guard, even when proceeding down steep inclines, and brought up within a moderate distance. It was certainly matter for surprise, seeing the advantage of continuous brakes, that the railway companies should have so long tolerated the old system, and been so slow to adopt a method which, instead of being dependent for its due action on the attention of several persons, was effectually placed under the control of one. This lethargy prevailed, too, throughout a period when increased speed had come to be demanded, when augmenting traffic required heavier trains, and when, consequently, more ponderous and powerful engines had to be used—circumstances which ought to have induced the companies to effect simultaneously a readjustment of their brake appliances. After the year 1850 many attempts were made to supersede the ordinary type of brake, some of the brakes introduced being self-acting and put into operation by the momentum of the train, while others acted as sledges or shoes. None, however, proved successful. The continuous breaks of Newall and Fay simply involved a wider distribution of power over the different vehicles of the train, and gave the means of applying that power by one, or, at most, two attendants. It was in that direction that the ingenuity of inventors had recently been turned, and there were now several systems of continuous brakes in successful working on the leading railways, each claiming some special advantages over its rivals, whether as more simple in construction, less expensive in application, or effecting more complete control of the train. The Royal Commissioners desired that attention should be primarily directed to the following points:—1. The distances within which trains running at various speeds could be stopped by the system of brakes in ordinary use on the different lines of the United Kingdom; (2) what results could be obtained by the additional application of brake power; and (3) how far a very large amount of brake power could be suddenly resorted to with safety in heavy trains running at high speeds. For the purpose of the experiments a portion of the Nottingham and Lincoln branch of the Midland Railway was selected as offering a piece of line comparatively level and free from any sharp curves. Six companies furnished eight complete trains, which represented as many systems of continuous brakes comprehended in four classes, namely, (1) Clarke's and Webb's and Fay's brakes, applied by ordinary mechanical gear; (2) Smith's and Westinghouse's vacuum brakes, actuated by atmospheric pressure produced by exhaustion of air; (3) Westinghouse's and Steel McInnes's air brakes; and (4) Clarke's and Barker's hydraulic brakes. The experiments extended over a week, and comprised several series. It was demonstrated that the friction of a complete train, in which the weight of the engine and tender constituted, say one-fourth of the gross weight, inclusive of the atmospheric resistance it encountered in its course, was 42-100ths per cent., or about 9½ lbs. per ton. This result confirmed what long experience had led them to anticipate. It was discovered further that, on a level line, a train running at the rate of forty-five miles an hour could be stopped by hand brakes within 1,000 yards, or, if at the rate of sixty miles, within 1,700 yards. The