Discussion.

Sir William Thomson.

Sir WILLIAM THOMSON commenced with an explanation of the models and drawings. He showed a model constructed to aid in the design for No. 2 Clyde tide gauge, in which was a pencil-marker on the second of the two plans described in the Paper and represented in Plate 1, Fig. 1. The cylinder carrying the paper was inclined to the vertical, and looked like the "leaning tower of Pisa." The cylinder could be taken off and re-applied in a moment. The bearing plate was slightly cupped, so that the weight of the cylinder pressed entirely on its rim. The spike in the centre bore no part of the weight; it was merely a guide to keep the cylinder in the middle. The cylinder was adapted either for a long roll of paper or for marking a week's or a fortnight's curve on one paper. It had been almost determined in the case of the Clyde tide gauges that instead of a very long slip of paper for a year's curves, the curve of a week or perhaps a fortnight should be traced on one piece of paper. There would be little trouble and no difficulty in the affair. The person who wound up the clock would carry the cylinder away and substitute another with paper properly placed on it. Or if he could be trusted to put on fresh paper, there would be a table in the tide-gauge house with papers ready ruled with the time and the datum lines. All that would be necessary was to lay the cylinder in the proper position and roll it over the paper so as, by aid of the hour-pins on the cylinder, to wind the paper upon it, and then fix it by two little adhesive overlaps. Referring to the geometrical slides of the two plans, and to the general principle of a geometrical slide, Sir W. Thomson remarked that good workmanship was too often put in requisition to overcome evils of a bad design. A good design in many cases required no fitting; and where it was possible it was better to manage with no fitting; for the finest fitting might be undone by a little warp in the material or by a piece of grit. In the pen- or pencil-markers exhibited there might be an inequality in the paper projecting as much as $\frac{1}{8}$ inch, but, if not too steep, it would not disturb the marker, which would be pressed out, and simply slip over it. He would test the instrument by shaking it roughly, and it would be seen that there was no error in the marking. With regard to the floater, he had a greenheart one shown in action. It weighed 50 grammes, and was a little heavier than water. Greenheart took kindly to the water, but india-

Sir William Thomson.

rubber or gutta-percha, slightly weighted with metal, would do very well. Something was wanted which would just sink—not with too much force. If the motion of the wheelwork became arrested—if the pencil broke down or was caught in the paper —when the tide rose, the wire would become slack and therefore be liable to kink if the floater floated without any upward pull from the wire. The great safeguard was to beware of kinks. In fact the sinking floater might be called an anti-kink arrangement. In case of any arrested action of the wheelwork, the floater would sink, and keep the wire tight. The early specimen alluded to in the Paper, made by Mr. Légé for the Author, was exhibited. In all the tide gauges since made for the Author a very light flat-rimmed wheel was substituted for that in the specimen, which was too heavy, and spirally grooved. There was no occasion for the grooves, because the height of riding of the thin platinum wire in the groove was infinitesimal in its effect on the reckoning of the water-level; so that the riding of the wire was not a thing to be avoided. A tide gauge should always when possible be placed vertically over the vertical sea-tube. In cases in which this was not judged practicable, as in the tide gauge recently placed on the Admiralty pier, at Dover, a stouter wire, a larger floater, and a counterpoise besides the ink-bottle were necessary. Otherwise the simple arrangement was one shaft for the main wheel on which the wire was wound, and a second shaft carrying a drum on which the wire bearing the pencil was wound; then, when the tide rose, the pencil went down, and when the tide fell, the pencil went up. Thus in a tide-gauge curve on its drum in the instrument the high water was down, and the lower water up; the hours must therefore be marked from right to left, and the turning must be in the direction of the hands of a watch, so that when the paper was taken off and turned to make high water up and low water down, the part corresponding to past time would be to the left, and future to the right. In all the tide gauges he had made hitherto, he had used a long slip of paper. The long slip extending round the room contained a year's curve for Mauritius, drawn by Mr. Roberts, by means of the South Kensington Tide Calculating Machine. At the rate of a foot a day the length of paper would be 365 feet. With seven curves on one paper the sum of the lengths would only be 52 feet. Fourteen curves would probably be a more convenient number to put upon one paper. He held in his hand a paper with thirty curves, drawn by a self-recording tide-gauge at Loanda (West Coast of Africa), and it would be seen

Sir William Thomson. that the appearance was very confusing. That number was too great, but he did not think that a fortnight's curves on one paper of twenty-four hours' length would be too much. He was certainly a convert to putting several curves on one paper. The Dover tide-curves, which he was able to exhibit through the kindness of Mr. Druce, M. Inst. C.E., the harbour-engineer, were very interesting. It would be seen that the line was much thickened. There were oscillations of 2 or 3 feet in the tide gauge which had been recently put up there under the direction of Mr. Druce, who had informed him that in all other respects its working was satisfactory. Mr. Légé was now making a copper guard tube, in order to remedy that evil. It was to extend 6 feet below low-water mark, ending in a small hole about ¼ inch in diameter, so that it would be impossible for any pumping up and down to take place. If in two or three minutes the water rose by several inches, or even half a foot, the floater would show it. A rising and falling by wave-disturbance in five or ten seconds would not affect the floater to any sensible degree. Thus the quick oscillation up and down would be annulled, and he had no doubt that the tide gauge would be in a perfectly satisfactory state.

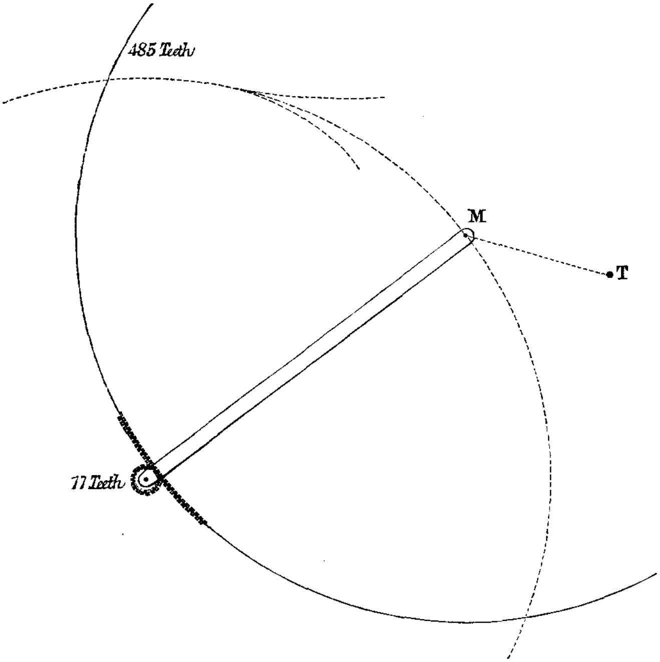
With regard to the Tidal Harmonic Analyser, he had a rough model to exhibit. One of the twenty-four hours' tidal curves, or a paper containing seven or fourteen, was placed on the cylinder, which went round once in twelve hours. The circumference of the cylinder was half the twenty-four hours' length of the paper. The two ends of the paper were united, and it ran round (like an endless towel on a roller) when the cylinder was turned. The instrument had been explained in the paper, and he would only show the management of it. To move the pointer along to the right and left in tracing the curve he had to turn a little crank with his right hand alternately to right and to left. The left hand was always kept turning one way; the right hand alternately one way and the other. The manipulation might appear to be very puzzling, but he was informed that at the Meteorological Office the instrument worked satisfactorily, and that the manipulation became easy after a little practice. It would be seen that the disk oscillated alternately in one direction and the other, and caused the counter to turn alternately forwards and backwards if the centre of the globe remained in one position. But if, while that change of direction of motion of the disk took place, the globe was turned over to the other side by the motion of the fork bar, then the counter kept turning in one direction, and thus each of the eleven counters counted out

Sir William Thomson.

the amount of one particular harmonic constituent of the complex variation represented by the curve presented for analysis.

With regard to the Tide Predicter he wished to explain an epicyclic mechanism for the combining of two simple harmonic motions, which he had described at a meeting of the British Association at Brighton in 1872, and was the simplest possible way of producing and of combining two harmonic constituents, though essentially inapplicable to more than two. Fig. 12 represented a pinion fixed on the end of a stud. The large circle represented a wheel about twenty-eight and a half times the diameter of the pinion. There were seventeen teeth in the pinion, and four hundred and eighty-five in the wheel. Imagine first a fixed framework, the pinion rotating in one direction, and the wheel rotating in the opposite direction. The angular velocities of the wheel and pinion would be as 485 to 17. Take now the whole machine and turn it round the axis of the pinion with an angular velocity equal to the pinion's but in the contrary direction; that annulled the angular velocity of the pinion, and added the amount to the first-supposed angular velocity of the wheel;

Fig. 12.



Sir William Thomson.

making the angular velocity of the wheel 485 + 17, or 502, if 485 be called the angular velocity with which the bearing of the wheel was carried round. Now the "speeds" of the mean solar and mean lunar semi-diurnal tides were as 502 to 485, very exactly. (See M_2 in the Table of Speeds in Part III. of the Paper.) Thus, by a crank-pin T, carried by the wheel in this mechanism, a solar tide was superimposed on a lunar tide. The point T (Fig. 12) changed its level according to the resultant effect of the mean lunar semi-diurnal tide and the mean solar semi-diurnal tide. The semi-range of the first of these tides was the radius of the circle described by the wheel; the semi-range of the second was the distance of the point T from the centre of the wheel.[1] This epicyclic method was a mechanical realisation of the construction in kinematics corresponding to the "polygon of forces" well known in elementary statics, for the case of two constituents; and it was a very useful as well as simple method when there were only two to be combined.

With regard to the particular mode of combining the motion which he adopted, by a hair-spring passing under and over pulleys:—as stated in the Paper, Mr. Tower had made the suggestion to him in a railway journey from Portsmouth to Brighton, at the commencement of the meeting of the British Association in 1872. Before the end of the meeting he wrote to Mr. White, and gave him instructions, and before the end of the month he wrote to Mr. Roberts, and told him that he had given instructions, for the construction of the wooden model now exhibited. Mr. Roberts had been with him, before the meeting of the British Association, for a few days, assisting to complete the report of the Tidal Committee for the meeting of the Association. During that time he continued discussions that he had had with Mr. Tower on the subject of his projected tide-predicting machine: and in the course of these discussions Mr. Tower made several suggestions, and amongst them a suggestion as to floaters in tubes, according to which, on hydrostatic principle, the effect could be summed up. Pushing in one piston caused water to rise in a tube; pushing in another added its effect; pulling out another caused a corresponding subtraction from the whole; and so on, with any number of pistons. With that beautiful idea it could be seen how the combination might be realised without mechanism. That was a very interesting suggestion of Mr. Tower's inventive mind; but he did not himself regard it as a convenient

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[1] A working model was exhibited at the meeting of the 15th of March. See conclusion of the Discussion.—W. T.

Sir William Thomson.

practical solution of the problem, nor did Sir W. Thomson. Mr. Roberts wrote to him: "I like Mr. Tower's idea of floaters as well as any of the different things that have been contemplated; but at present I cannot see a good method of combination." In reply Sir W. Thomson commented thus, in a letter to Mr. Roberts despatched before the end of August 1872: "The floats would not work well. He (Mr. Tower) suggested also a plan with pulleys and a cord or chain which led me to a very simple plan with a long hair spring round pulleys centered on cranks. I have given White instructions to commence a partial model for trial." The result of those instructions was the wooden eight-component model (Fig. 8) now before the meeting. It had only yesterday come to his knowledge that in the report in the *Athenæum* of the meeting of the British Association at Bradford in 1873, the Tide Predicter had been described as "Mr. Roberts' instrument." The origin of that misapprehension had been explained by Mr. Roberts, in a letter to the Author of date October 23, 1873, informing him that a label describing the instrument as of his (Mr. Roberts') design had been affixed to the instrument by mistake during his absence. No doubt the reporter had taken his information from the false label. When the machine was exhibited at Paris in 1878, in charge of Mr. Légé, under direction of Mr. Roberts, it was designated as Sir William Thomson's Tide-calculating Machine, and was accompanied with a description, extracted from the catalogue of the loan collection of South Kensington, which contained the following: "The general plan of the screw gearing for the motions of the different shafts is due to Mr. Légé, the maker of the machine. The construction has been superintended throughout by Mr. Roberts, and to him is due the whole arithmetical design of the gearing to give with sufficient approximation the proper periods to the several shafts." Mr. Roberts' and Mr. Légé's translation of that passage (giving less credit to Mr. Roberts) was, "Tous les nombres ont été donnés par Mr. Roberts." He should not have troubled the members with such a statement, but that he wished to make it clear that he had dealt in a perfectly fair manner with those who had worked for him on the tide-predicting machine.

Mr. Roberts.

Mr. E. ROBERTS, of the Nautical Almanac Office, remarked, with regard to the Author's tide gauge, that in the "Philosophical Transactions" for 1838,[1] there was a description of Mr. T. G. Bunt's tide gauge, erected in 1837 in front of the

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[1] *Vide* p. 249.

Mr. Roberts. Hotwell House at Bristol. An abridged description of it was also given in the article on Tides and Waves in the "Encyclopædia Metropolitana," by the present Astronomer Royal.[1] The instrument had a recording pencil with vertical guides, a vertical recording barrel divided round its upper and lower edges to time, float wire and wood float. Great care was bestowed on the pencil suspension and guides. The instrument recorded accurately, and was at work from the time of its erection until dismounted in the course of the improvements in the river Avon about 1872. The tide gauge was, he believed, still preserved in the dock-engineer's office. Various materials were successively tried for the float line, including wire, silk, and fish-line. The material that answered the purpose best was silk saturated with a mackintosh varnish. The platinum wire of the Author's tide gauge had been found to be acted upon prejudicially, and to break into small pieces after being some time in use. Fine gilt copper wire also, as used on the tide gauge at Dover, required renewal every few weeks.

With respect to the Tidal Harmonic Analyser, the outcome of Professor James Thomson's Disc-Globe-and-Cylinder Integrator, he could not agree with the Author's statement that the whole series of declinational, parallactic and other perturbational lunar and solar semi-diurnal tides could be estimated with all needful accuracy for practical purposes. The total omission also of all long-period tides, both in the completed machine and also in that projected, was a serious defect, as it was impossible to estimate those tides, depending as they did so much on local and meteorological circumstances. The accuracy of the results obtained by the machine would largely depend on the precision with which the mechanism had been made. The globes should be perfect spheres, the disk a true plane, and the recording cylinder truly turned, balanced, and accurately centred. The homogeneity of the metal spheres would also enter as an element in the accuracy of its working. If the metal was not homogeneous and free from air cells, the sphere would fail to respond to the motion of its fork-guide, and the proper effect would not be communicated to the brass cylinder.

The manipulator also must very carefully follow the course of the curve with the tracing style, which must be kept true to time, otherwise inaccuracies would be introduced in the results which could not be eliminated. He thought the slow motion screws of the record-barrel and the tracing style could be omitted with advantage. A hand-rest would be sufficient for the tracing style,

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[1] *Vide* vol. v., p. 364 (473).

Mr. Roberts.

and a fair speed could be given to the record-barrel by a pulley and weight. Sufficient resistance to the record-barrel could be applied by the finger should it turn too fast for those portions of the curve not easily followed by the tracing style. A stop pin would arrest the motion when desired. This arrangement would allow greater freedom to the manipulator.

An instrument made to include the twenty to thirty tidal constituents usually evaluated, if not arranged with parallel bars or in other ways, and on the same scale as the Author's, would be of the very undesirable length of at least 100 feet.

In order, however, to test the accuracy of the working of the complete analyser, he should be pleased if, after the Author had passed the series of Spanish and African tides through the machine, he might be permitted to analyse the same curves numerically. A comparison of the results would afford a practical test of the efficacy of the instrument.

It was necessary, with regard to the Tide Predicter, that Mr. Roberts should enter somewhat into detail, as he considered the Author's version of his share in the production conveyed a very inaccurate idea of the real facts of the case. He was present on board the Author's yacht in August, 1872, when Mr. Beauchamp Tower suggested, among possible methods of combination, that of the chain and pulleys. He was also present a few days after in Section A of the British Association at Brighton, when the Author gave a description of a projected Tide Predicter. Sir W. Thomson was, however, he believed, in error in stating that it was Mr. Tower's chain-and-pulley method of combination that he there described. Mr. Roberts well recollected the plan which the Author sketched out and illustrated by a drawing on the black board, and which was an extension of an epicyclic method of combination, already carried out to a limited extent in a tidal clock then just completed, and afterwards shown before the close of the Brighton meeting. Mr. Tower, who was also present, afterwards told Mr. Roberts how much simpler he considered his own plan than the one described by the Author of the present Paper. The following account, extracted from the *Athenæum* of the 31st of August, 1872, bore on this point:—

"Sir W. Thomson went on to describe a tidal watch which was being constructed, and which he hoped to exhibit in a few days. It would indicate the height of the water, so far as dependent on the sum of the principal lunar tide and the principal solar tide, and would be adjustable for the amplitudes of these two tides at different places. There was an additional provision for

Mr. Roberts. showing the time of high water. Very similar in principle to this watch is a calculating machine, which Sir William is planning for tidal computations. Each elementary tide gives a height of water proportional to the height of the end of a clock hand, which makes one revolution with uniform speed during the period of the elementary tide. If the end of this hand carries another clock, the hand of this clock will be at a height corresponding to the sum of two elementary tides. It is proposed in this way to produce a movement which shall represent the sum of all the sensible constituents of the total tide, and to make the machine self-recording by a curve which it will trace on paper."

A still further confirmation of this was given later on. The Author stated that "Before the end of the meeting he wrote from Brighton to Mr. White at Glasgow, ordering the construction of a model to help in the designing of the finished mechanism for the projected machine," Fig. 8. Mr. Tower's plan, however, if taken up, appeared to have been quickly abandoned by the Author, for at the end of the following November, Mr. Roberts received a description of a new machine which the Author intended having made by Mr. White. It was intended for two purposes, "one to serve as a cheap tide indicator, and another to serve as a constituent in the tide-calculating machine to be made," and was thus described: "a new machine . . . . will have the solar hand on a fixed axle, and the lunar hand connected with it by a fixed pinion, working on a movable wheel and pinion, and a movable pinion working on a second wheel, which is to carry a second hand, and give it a slow retrograde motion relatively to the solar hand." Thinking from this description that the Author had abandoned Mr. Tower's idea, Mr. Roberts, unknown to the Author, gave Mr. Légé (a mechanician specially recommended for the purpose), in February 1873, directions to construct the two-component model now exhibited, on Mr. Tower's suggested plan, and fitted on the rigorous method of combining two simple harmonic motions by means of parallel slides. This model was completed, as now shown, about May 1873, and it was then seen for the first time by the Author, who accompanied Mr. Roberts to Mr. Légé's workshop to inspect it. It was also seen shortly afterwards, amongst others, by Professor Guthrie, Mr. Bottomley, and by the late Mr. Froude. It was upon this model that the British Association ten-component machine was planned, with certain alterations by the Author, who was chairman of the Tide Committee. The chief alterations were the substitution of an ink-recorder in place of the pencil, the change of plane of the instrument to one nearly vertical, the omission of the slides, Mr. Roberts. the centering of the pulleys on the cranks, and the consequent reduction of the length of the recording barrel to lessen the geometrical error introduced by the non-parallelism of the chain. The machine thus sketched in idea in June 1873, was completed during the Author's absence from England, under Mr. Roberts' direction, and exhibited in September 1873, at the Bradford Meeting of the British Association. The following extract was from the *Athenæum* of the 4th of October, 1873:—

"Mr. Roberts' machine, which was exhibited in connexion with the Report of the Tidal Committee, traces a curve which is the resultant of no less than ten harmonic motions in parallel directions. It has been constructed out of the funds of the Tidal Committee, for the purpose of tracing a perpetual succession of tidal curves for any particular port. The period-ratios of the ten motions are put into the machine once for all by permanent gearing, and correspond to the period-ratios of certain elements of the sun and moon with respect to the earth. The ten amplitudes are adjustable to suit any particular place, but are permanent for that place. The pen which traces the curve hangs at one end of a fine steel chain, which passes alternately above and below ten pulleys corresponding to the ten harmonic motions, the other end of the chain being fixed, and the centre of each pulley describes a circle whose radius represents amplitude. The machine is driven by turning a handle, and the amount of chain hauled in or paid out during any part of the motion is equal to the sum of the rises and falls or falls and rises of the upper and lower pulleys respectively. The machine is an improvement upon a very different plan, which was suggested by Sir William Thomson at last year's meeting." Some additions and alterations were subsequently made. An improved suspension was made to the recording pen, which allowed the instrument to be strictly vertical, a disk with projecting cams was fitted to the solar component, to give time indications by producing a short horizontal movement of the pen, and on the suggestion of Mr. Légé, continuous paper was adopted for the record.

With regard to the Tide Predicter which Mr. Roberts had made for the Indian Government, it would only be necessary to refer to his Paper upon it in the Proceedings of the Royal Society, read on the 19th of June, 1879. He might here, however, state that he submitted that Paper, before being read, to Sir W. Thomson, by whom it was absolutely approved. In it he had given credit to the Author for the improved parallel slide, and also to Mr. Légé for the plan of the wheel gearing. No progress, Mr. Roberts. however, had been made with the machine when Mr. Roberts received the drawing of the slide from the Author in March, 1878, as he did not receive the official authorization from the Indian Government to plan and construct the machine until the following May. Mr. Roberts alone was responsible for the construction of the instrument, and was free to employ whomever he pleased to make the mechanism. A full and illustrated description of it would be found in the *Engineer* of the 19th December, 1879. The Author's objection to the limit of speed of working of the India Office predicter was not a valid one; the machine, being automatic, could be set over night, the traced curves being ready for manipulation the next morning. He had found, however, that even with this machine it could be driven at a greater speed than would allow a true and unbroken curve to be traced on the recording barrel, and he doubted much if the Author's projected machine could be run to trace a whole year's curves in twenty-four minutes, unless a very short abscissa was given to the curves, and a short barrel again used for the delineations. He could not regard the choice of less accurate ratios in the Author's contemplated machine as an improvement in design, seeing that the machine was one to produce as accurately as possible the best attainable results. Even allowing this derogation from possible accuracy, the numbers of the teeth in some of the wheels were very large. For instance, one of the wheels for the N component had eight hundred and two teeth. Allowing only a pitch of 1½ millimetre for the pitch of the teeth of the bevel wheels, this number would give a diameter of 15 inches, a size extremely difficult to make with the lightness and hardiness desired by the Author. It was a curious fact with regard not only to Mr. Roberts' two-component model, but also to both the completed Tide Predicters, that they were all not actually seen by the Author before completion and in thorough working order.

In conclusion, he was exceedingly pleased that in the Author's projected predicter, he had again reverted to Mr. Roberts' original plan of slides, which were mounted on the first model and in the more finished predicter which he had made for the Indian Government.

Mr. Roberts took this opportunity to announce that the Government of India were most anxious that as much use as possible might be made of their Tide Predicter, and he was authorized to use it on payment of a nominal fee for the predictions of the tides for any place, whether for the purposes of navigation or the manifold wants of harbour engineers.

Mr. Deacon.

Mr. G. F. Deacon said the points upon which he desired to offer a few remarks were very minor ones. As more than five hundred recording instruments of different kinds had been constructed according to his designs, he had had some experience in the matter of the pens and pencils employed for the automatic registration. He had been delighted to hear that this small but most important subject had received attention from the Author of the Paper. The principles which Sir William Thomson advocated had been, in a modified degree, applied in many of the instruments to which he had referred. In the instrument before him, for example, the construction of the pencil-carriage was such that the point of the pencil was almost exactly in the vertical line passing through the centre of gravity of the carriage; and the wire upon which the carriage hung was exactly in that vertical line. The pencil was a metallic one, and the pressure was produced by a constant weight acting upon it through the agency of a lever. Two forces tended to press the carriage out of the vertical line of its supporting wire; first the normal pressure of paper against the pencil, second, the friction of the paper against the pencil tending to move it in the direction of rotation of the drum. The first was balanced, as already stated, by a weight at the end of a short lever, the reaction of which was balanced by two small rollers bearing against vertical guides; its amount was about 20 grammes or 1 oz., showing that the metallic pencil and prepared paper were in this respect inferior to the plumbago and common paper, which, according to the Author, only required a pressure of 10 grammes. The plumbago pencil, however, was much less hardy, and required more frequent renewal than the so-called metallic pencil. The second force was inappreciable in its effect where the vertical pull upon the pencil-carriage was counterbalanced by such a weight as was necessary in most instruments. Where, however, the vertical pull of gravity was insufficient to prevent appreciable deviation from this cause, it was well to allow a portion of the carriage to lie between the two vertical guides with a play of only about $\frac{1}{50}$ inch.

For the comparatively large pressure required to make metallic pencils mark satisfactorily, he believed that this arrangement involved the least possible friction in the case of a vertical drum, or with a drum in any other position, except perhaps that slight inclination from the vertical, by means of which the Author caused gravity to balance the pressure of the paper against the pencil. This was the perfection of simplicity, and was no doubt practicable in tide and river gauges, but not in many other

Mr. Deacon. instruments where the moving wire could not conveniently be turned from the vertical line.

As the Author had said, an ink marker had been tried for tide gauges, but had hitherto been found unsuccessful on account of the slowness of motion, and the long time through which the action had to be continued. This, however, was not the case in the fresh water analogue of the tide gauge, viz., the river or reservoir gauge. For some years he had had such gauges constructed with ink markers, and had found them perfectly successful for the slowest motions required in this instrument, and for the quickest motions required in other instruments; while he had not yet discovered the limiting time during which the same pen would continue to mark without attention.

In all respects, therefore, he had found the ink marker greatly superior to any form of pencil, but he had not been able to attain this result with any of the pens ordinarily used for the purpose. He had tried a glass pen, and an admirable device, originated by the late Mr. Froude, a metallic pen with an exceedingly fine hole in it, and which at moderately uniform rates made a fine and perfect line. The pen he had used was devised by his assistant Mr. William Davies. The point was a solid cone of brass, fixed in the end of a short tube, conveying the liquid from the under side of the ink reservoir. In the upper side of the cone was a vertical hole $\frac{1}{20}$ inch in diameter, communicating at its lower end with the supply tube, and at its upper end with a little trough passing down the upper side of the cone to its point. The hole was stuffed up with cotton wool, and when the cotton wool was pressed in rightly, the liquid, one-quarter glycerine, and three-quarters aniline dye, passed through it and ran down the little groove to the paper. If the pen moved slowly, and required little ink, the groove conveyed to the hole by capillary action just the necessary quantity; if it went quickly, and required more ink, the groove conveyed a larger quantity. In that respect he had found the pen to answer admirably, and, what was even more satisfactory, the pen never became choked, and so far as he knew its proper action without attention was only limited by the size of the reservoir and by the actual wearing down of the point, which however need only just touch the paper in order to make a good line.

It was often desirable to place such gauges in places rarely visited by skilled persons, and in such cases it was imperative that they should go for a long period without attention. The pen which he exhibited contained sufficient ink to go for two years without being touched. Its twin brother had already made a

Mr. Deacon.

diagram eighteen months long, and he was sure he could trust it for another month. The diagram paper he had been in the habit of using was sufficiently long for three months, but it was generally cut off at one month; the clock also went for three months without being rewound. Instead of ruling the diagrams, he had found that the best way was to allow them to prick their own hours and their own zeros, exactly in the same manner as was done in the Author's tide gauge.

No one could avoid admiring the perfection to which the Author had brought every part of his instruments, but perhaps a little extra motive power would be an advantage. It might not be necessary when there were skilled instrument makers to attend to the instruments, but in isolated places, where men were employed who had not been accustomed to such apparatus, he thought it would be desirable to have the instrument a little stronger, the wires thicker, and the motive power greater. In his own practice he had made the floats from 6 inches to 9 inches in diameter. Possibly that might be excessive, but there was no difficulty about it, and he had found no disadvantage attending it. It occurred to him that there was a possible disadvantage in connection with small floats. In sea water a coating covered the float after a short time, and inasmuch as the motive power was as the square of the diameter, and the effect of the coating would be only directly as the diameter, he thought that a large float would have some advantage in that respect over a small one. He approved of the Author's advocacy of vertical or nearly vertical diagram drums. It was impossible to construct a pen or pencil carriage working in a horizontal line, the power required to drive which would not be a large multiple of the power necessary to move the pen or pencil in any of the arrangements described of vertical or nearly vertical action.

Those who had had occasion to employ recording instruments largely would not fail to appreciate the importance of these details upon which the correctness of their action largely depended.

The Astronomer Royal.

Sir G. B. Airy, Astronomer Royal, said, before he entered upon the actual subject under discussion he would advert to a point incidental to it—the formation of fractions whose combination would produce any desired proportion. It occurred on the present occasion in reference to the combination of toothed wheels which would be used to establish the proportion between one argument of an inequality in the tides and another argument. A want of that kind had occurred to him long since, and several years ago he had constructed, with the aid of his excellent assistant, Mr. Ellis, a table

The Astronomer Royal. containing the values of all the vulgar fractions whose numerators did not exceed 100, and whose denominators did not exceed 100, and their logarithms; and those were arranged, not in the order of numerators or in the order of denominators, but in the order of the ultimate value of the fractions; and that gave extraordinary facility for fixing upon any numbers which should represent those values. For instance, if there were any proportion upon which approximate numbers were wanted, taking the logarithm of that proportion and looking over the table, all the fractions whose values approximated to it would be found brought close together. And curious differences would be found amongst them. Two successive fractions seemed to have no relation whatever in the numbers of their numerators and denominators; and the value of the fractions approximated more and more nearly on one side towards that which was sought, and on the other side they receded from it more and more. When the numbers were below 100 (which he should think would always be the case in any practical application), they were taken at sight from the table; when the numbers were greater than 100, and a combination of two sets of toothed wheels was required to establish the desired proportion of results, it would be necessary to go through an operation of addition or subtraction, and the work would be a little longer.[1]

The Paper by Sir William Thomson consisted of three parts. The first was with reference to mechanical improvements in the tide gauge, of which it was sufficient to say that they were made by the Author, who was an excellent mechanic. He was quite sure, from what he had seen, that beautiful instruments would be produced, and that, considering the improvement in general theory in such matters, they would repay the expense incurred. Altogether, he looked upon them as a valuable addition to the mechanics of the science. The third part of the Paper was on the composition of inequalities of various classes for the purposes of prediction, in which it was supposed that each of the various inequalities followed a law like the sines and cosines of angles for different periodic times, and that their coefficients had different values, all of which were to be assigned as the subject of a previous research. There was no doubt that that could be done with the utmost accuracy, and he should think with reasonable safety, although the machine was extremely complex; that was

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[1] The Astronomer Royal has consented to allow the Table of Vulgar Fractions to be printed in the Minutes of Proceedings of the Inst. C. E.—SEC. INST. C. E.

The Astronomer Royal.

in a great measure a matter of mechanical skill. He might say that subjects of that class were not new to him; for when he was giving a series of lectures on the disturbance of the compass in iron ships, first at South Kensington and secondly at the Naval College at Greenwich, he used a model, which still existed at the Royal Naval College, showing by toothed-wheel working, the combination of two inequalities, one depending upon the permanent magnetism of the ship, which had its two terms of sines and cosines, and the other depending upon the induced magnetism of the ship, which also had its two terms of sines and cosines, but repeated twice as often as the others. In fact it was a machine corresponding, *mutatis mutandis*, exactly with that which was used for predicting the diurnal tides. He might also say that he once drafted, but did not complete, a machine for exhibiting the result of the ordinary powers of numbers (decimally expressed) with arbitrary coefficients, and of uniting them so as to form the expression $a + bx + cx^2 + \&c.$, exhibiting them in such a way that it would be possible, by sliding a particular part of the machine which received the delineation, to find when the sum vanished, and therefore when the equation $a + bx + cx^2 + \&c. = 0$ was solved.

He had alluded briefly to the first and the third sections of the Paper; he would now speak of the second, which was really the important one, and the one on which he felt great difficulties; namely, the extraction (from a series of delineated tide heights) of the coefficients of inequalities of pre-arranged form. That was a subject which really alarmed one to enter upon, it was so difficult practically and so troublesome. Employing the symbols θ, ϕ, ψ, for different angles, increasing uniformly as multiples of the time, the form assumed for the expression which was to represent the height of the tide was of this kind: $A + B, \sin\theta + C, \cos\theta + D, \sin\phi + E, \cos\phi + \&c.$; and the question was, whether, assuming that form and assuming also the multiples of time which represented successively the angles θ, ϕ, ψ, &c., one could, after that assumption and with the tidal traces before one, extract those multiples A, B, C, D, and so on numerically. That was what occurred in almost everything in connection with physical astronomy and physical research of many kinds, and it was a most appalling process. It was mastered theoretically by a very celebrated theorem, Fourier's theorem, in which the whole series of results taken numerically had to be broken up in parts corresponding to the positive and negative values of every individual of those terms. Sine θ had to be broken up with

The Astronomer Royal.

reference to the positive and negative values of sine θ; cosine θ had to be broken up with reference to the positive and negative values of cosine θ; in like manner with sine ϕ and cosine ϕ, and in like manner with sine ψ and cosine ψ. There was no other way in which that could be done with accuracy than by so breaking them up, and by adopting successively each one of those functions as the one to whose coefficient importance was wanted especially to be given at the moment, and to multiply separately every term of the equation by the coefficient of that one. For instance, he had a long series of terms, including sine θ, cosine θ, sine ϕ, cosine ϕ, sine ψ, &c. The first process must be to multiply every line of that equation by sine θ which belonged to it, and to take the sum of those, and then he should get an equation in which the coefficient of sine θ was the conspicuous term. So for every one of the others. Conceive what had to be done in such a case! There was a series of records extending over a long time, and it had to be broken up individually for the changes of sign. That had to be done even where they seemed most closely combined. For instance, there were terms well known to the Author of the Paper, of which the period was twenty-four hours, and there were other terms of which the period was nearly twenty-five hours. It might be thought that those two would go together, and on any day they could not be separated; but going twelve days it would be found that one was opposed to the other, and there would be a state of things totally different. There was no way of mastering that except by going through the strict process of separating every one at the proper time; and so for all the other terms of the formula in question. Therefore the whole series must be broken up into a great number of parts, of which it was necessary to have the exact numerical value as deduced from the tidal gauge, or in any other way, and to add those together with the different signs plus, minus, according as the sine of the angle was plus or minus; and to do it strictly it would be necessary to multiply every one by a different number, according as the numerical value of the sine or cosine changed. There were some ways in which that immense labour might be alleviated. One was by assuming that wherever the cosine was plus it might be called plus unity, and wherever it was minus it might be called minus unity. That amounted to the same thing as saying that wherever the difference of the numbers was to be taken it might be conceived that all that had one tendency had equal values, and all that had the other tendency had equal values, but their difference must be taken; and that was one alleviation to the great labour.

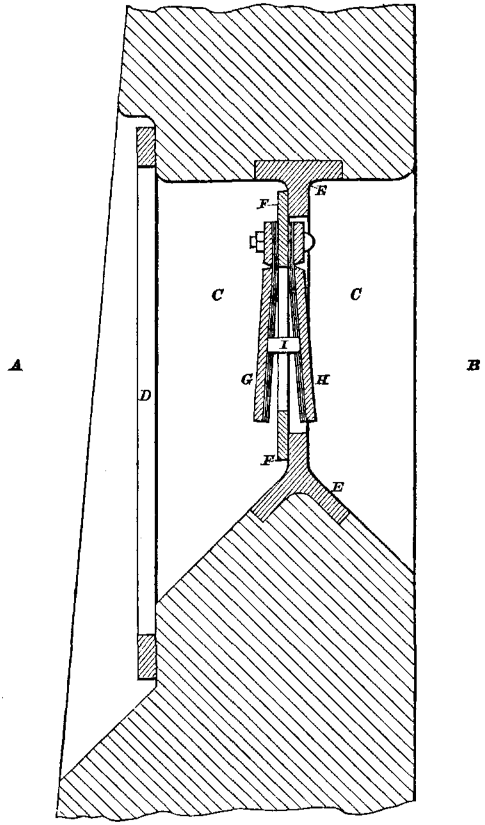
Another alleviation was, that in taking that state of difference all the terms might be abandoned except that which was to be the conspicuous one; and in that case there would be a set of terms for sine θ without the combinations of ϕ's or ψ's; and in like manner another for cosine θ, and in like manner another for sine ϕ, and so on. Still there was this which could not be diminished—that for every one of those terms it was necessary to divide into separate quantities all the records which were to be taken, plus and minus. And all would have their divisions at different times: one would have its division at twenty-four hours, another at twenty-five hours, another at a quarter of a year, and so on; and they could not be combined or separated in any way, but each of them must be actually grouped, and the sums or differences taken in that form. That was the lowest state to which the labour could be reduced. In that lowest state if the machine which was the subject of the communication could make the labour easier it would be a good thing; but he conceived that it was impossible that it could master it. He did not think it possible that it could manage the breakings up to which he had referred; he did not think any mechanism could do it. The mechanism was not sufficiently described in the Paper in order to enable anything specific to be pointed out as to the possibility or impossibility; but speaking in general terms, he did not think that it was possible that mechanism could do it with greater ease than it could be done by the use of figures upon paper. In point of fact, if mechanism were used for the purpose, records must be extracted from it at every one of the breaks of which he had spoken, and transferred to paper for further treatment. He had given his general impressions on the subject. For want of information he could not do it completely; but he had no doubt as to the accuracy of his general conclusion. The Astronomer Royal.

Mr. Law. Mr. HENRY LAW thought the profession was much indebted to the Author for the perfection to which he had brought these beautiful machines, and for the prominent manner in which he had introduced them to the notice of the members. He hoped that the practical result would be to make the use of automatic tide gauges more general than it had been, because there could be no comparison between the advantages of continuous automatic observations and those which were taken even by the most careful observers. In the case of these latter observations there was not only the liability to error, but, what was much worse, there was the danger that the irregularities to which the tide was particularly subject would be either overlooked

Mr. Law. altogether, because they came between the periods of observation, or, if observed, would be put down as errors. For a long time, indeed until the Rev. Dr. Whewell, Hon. M. Inst. C.E., showed that it was a constant phenomenon, the diurnal variation was regarded in many ports as an error of observation. Again, the automatic gauge gave continuous observations which were of the utmost value, because they showed those peculiar phenomena which occasionally occurred, such as repeated tides, irregularities of tides, and also the special form of curve. Again, they were recorded without labour, and presented the results to the eye in a graphic form, rendering it very easy to draw general conclusions; moreover they were in a form exceedingly convenient for comparison, and for analysis. He therefore thought that such an improvement upon tide gauges, and any such general advertisement (if he might use the word) of tide gauges, as the discussion would lead to, would certainly be a benefit to the profession. On one point his experience agreed with that of Mr. Deacon, namely, that when the tide gauge was applied to the ocean, being subject to considerable oscillation in that case, to use the expression of the Author, a more hardy instrument was desirable, and a larger float He thought there could be no reason for limiting the size of the float. In tropical seas, for instance, where weed and insects grew rapidly, and attached themselves to the float, and would choke and interfere with the appliances which were essential to prevent the oscillations of the water, a less delicate instrument would be desirable. There was a mode which he had used for preventing the oscillations of the sea affecting the records, which had proved perfectly effectual. The Author had stated that at Dover, where, of course, there was considerable motion, a difficulty had been experienced and a suggestion had been made to allow the water to enter the tube through a very small opening; that, however, would soon become choked by seaweed and other obstructions. He had found the method shown in Fig. 13 answer perfectly. Within the line of piles or wall a well should be constructed to contain the float, of considerable diameter, having a channel of communication, the upper side of which should be level; in the centre there should be a diaphragm, and the invert should rapidly slope down in both directions. The diaphragm should have a plate with an opening of 6 inches square, attached to it by two thumb screws, and should have a double valve like Dr. Arnott's ventilating valve, kept at a fixed distance apart. The effect of such an arrangement was that, when a wave rose above its normal height, the tendency of the water to enter immediately closed the valve on that side, and when the wave receded the fall of the water below that in the well, closed the valve on the other side; and consequently there was no disturbance at all in the well. He had found the method effectual, even with waves 5 feet high. Of course seaweed, &c., grew upon the valve, but at low water, by means of the thumb screws, it was easily detached, cleaned and replaced, without interfering with the action of the tide gauge.

Mr. Law.

FIG. 13.



A. Tidal estuary or river.
B. Well or chamber for float of tide gauge.
C. Channel of communication.
D. Gun-metal grating opening on hinges.
E. Metal frame built into masonry.
F. Movable plate secured in place by two thumbscrews.
G. and H. Hinged valves kept at a constant distance apart by the stud or link I.

With reference to the machine for predicting tides, he remarked

Mr. Law. that the circumstances which caused the varying height of the tides might be classified under three heads. First, astronomical, such as the varying declination of the sun and moon, the varying interval between the passage of the moon over the meridian, and that of the sun, and the varying distance of the sun and the moon from the earth. These had a cycle of about nineteen years, and if they were the only circumstances, the tides might be easily predicted. Secondly, the local circumstances, such as difference of latitude, which increased the effect of gravity by diminishing centrifugal force, and increased the attractive force of the particles of water towards the centre of the earth as the latitude increased. Those were constant, and easily calculated. Then there were the influences produced by the peculiar configuration of the bed of the sea, or tidal channels leading to the place of observation. These were not permanent and constant, especially in the case of an estuary or tidal river, because, either from slow natural causes or from engineering operations, the circumstances changed, and altered the heights and intervals of the tides. But there was the third class which might be called purely accidental; he referred to the influences produced by meteorological and seismographic causes,—the effects of earthquakes and volcanic influences, which, although rare, sometimes produced great disturbances in the tides. It was evident, he thought, that the most perfect tide predicter would be that which calculated the tide as due to the first two series of causes, astronomical and local, eliminating altogether the influences of the weather, which were purely accidental as regarded time, and which were not so inconsiderable as many persons might imagine. The point to which he referred was illustrated by Fig. 14. That was a record of the tides from the 25th of January to the 29th of February, 1836. The dotted line showed the height to which it was predicted, in the tables of the late Sir John Lubbock, that the tide would rise, and the black columns showed the height to which the tide actually did rise at the Shadwell entrance of the London docks. On the 4th of February, for instance, the tide rose above the height predicted, on the next day a little below, on the next, still more below. In one case there was a remarkable tide, 4 feet 4 inches above the predicted height, and on inquiry into the circumstances it appeared that there was on that day a gale blowing from the north-west. The arrows at the bottom of the diagram showed the direction in which the wind blew, and the figures denoted the force of the wind according to the Admiralty table. It would be observed that the tides which rose

Mr. Law.

FIG. 14.

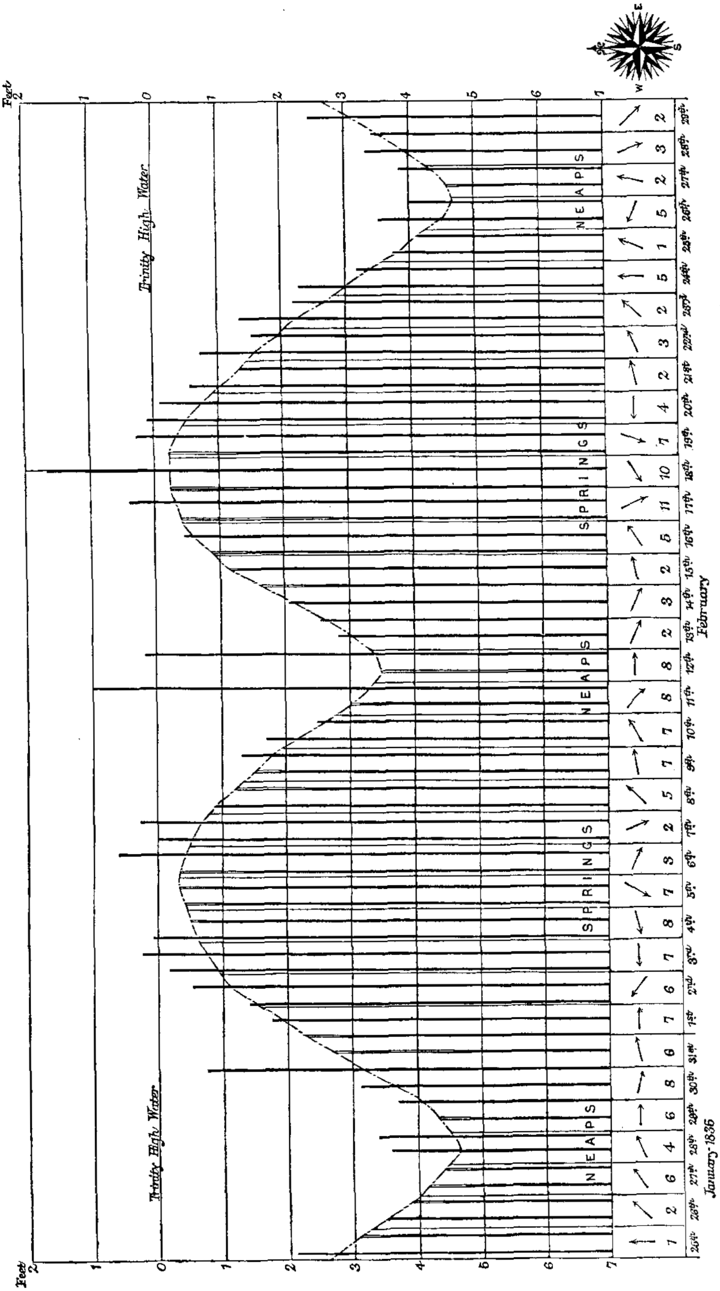


DIAGRAM SHOWING THE INFLUENCE OF THE WIND UPON THE HEIGHT OF THE TIDE AT THE LONDON DOCKS.

Force of the wind.—1. Light airs. 2. Light breeze. 3. Gentle breeze. 4. Moderate breeze. 5. Fresh breeze. 6. Strong breeze. 7. Moderate gale. 8. Fresh gale. 9. Strong gale. 10. Whole gale. 11. Storm.

Mr. Law. above the predicted heights were in every case preceded by southerly and westerly gales, changing rapidly to northerly and westerly gales. On the 17th and 18th of February, for instance, there was the greatest gale that had been known, short of a hurricane. The wind blew with the force of 11, almost northerly, and the tide rose 2 feet 4 inches above Trinity high water. There was a curious phenomenon which might have been overlooked (fortunately it was not) by human observation, but which would have been recorded with all its incidents by the tide-recording machine. It was a case of a double high water in one tide, both in the morning and afternoon. The tide rose to a height of 9 inches below Trinity, then fell (he did not know how much), and then rose again, and the same thing occurred at the subsequent tide. This was occasioned by the storm from the N.N.W., which by accelerating and increasing the North Sea tidal wave, caused it to arrive so much earlier than the English Channel tidal wave as to produce its own high water, independent of, and previous to, the second high water, produced when the English Channel tidal wave (which had been retarded by the gale) subsequently arrived. It would also be observed that after a high tide it frequently occurred that the succeeding high water was lower than usual. That was when the disturbance had been of such a nature as to affect the general level of the sea when the low water had been raised as well, and therefore the reaction of the wave carried it down again. The only mode at present of eliminating the effect of these accidental causes was by discussing an enormous number of observations. Sir John Lubbock had founded the coefficients of his formulæ upon twenty-four thousand observations taken continuously at the London docks—a series of observations which had been continued to the present day. It was by the discussion of these—a work of gigantic labour—that he was enabled to arrive at the means of calculating the tides with the great precision with which they were now predicted. If, therefore, mechanicians could eliminate by mechanical means the accidental effects of the weather, the harmonic analyser would become of the greatest possible value. He quite believed that it would be within the range of possibility, and he had no doubt that Sir William Thomson would see his way to effecting it. In ports where there had been continuous observations of the wind, the rain, and the barometer, the three meteorological phenomena which affected tides to the greatest extent, if the tidal observations were analysed by some modification of the machine, a corrected curve might be obtained, which

Mr. Law.

being analysed for the tides would afford the elements of the coefficients to enable the tides to be predicted with great certainty. He also thought that the analyser was of peculiar value, because, as had been shown by Prof. James Thomson in his Paper in the Proceedings of the Royal Society, cones might be substituted for disks. He knew that there was a mechanical difficulty in getting the ball to remain at zero with the cones and not to record at that point, but cones might be used instead of toothed wheels for giving motion to the disks, by which means a variation in their angular velocity might be produced. If computers required other functions than those of harmonic analysis, by the introduction of cams of variable form, in such manner that the cone would vary the angular velocity, and the cam would be substituted for the crank, they might be enabled to integrate functions of a much more complicated character, and he had no doubt that the genius of Sir William Thomson would enable him to effect this object.

Mr. Redman.

Mr. J. B. REDMAN said, if any argument were necessary to show the expediency of adopting self-acting tidal gauges at the harbours and great tidal rivers of a maritime country like England, it might be found in the remarkable divergence of opinion expressed after the recurrence of any abnormal high tide in the river Thames. In illustration of that he would shortly describe the conditions of the river on the 18th of January last as compared with the 15th of November, 1875. There were on the Thames four tidal gauges: one at Sheerness, which had been worked for some years, one which had been established a few years at Gravesend, one at Greenwich, and one at the head of the tidal influence at Teddington. There was, he believed, a fifth, the property of the Metropolitan Board of Works, at Crossness. At Sheerness he had frequently, in searching for exceedingly low tides to compare them with the tides in the Port of London, met with a very disappointing and aggravating blank in the record, with a short note stating that the mud had accumulated, and that there was no record of the tide. On the 18th of January last, or a few days afterwards, he was desirous of ascertaining the precise height of the tide at Gravesend, and found from the Conservancy authorities that the tide gauge had been rendered unworkable by the frost; the same thing had occurred at Teddington, and he supposed also at Greenwich. Some remarkable figures had been quoted in reference to the tide of January 18th, and considering the character of the day, the river being covered with floes of ice, and an easterly gale of extraordinary velocity blowing, it was quite intelligible that observations, however carefully made, Mr. Redman. might deviate several inches, as in fact they did. At Sheerness dockyard zero of the gauge was the mean level of the tide. It had been generally assumed as 11 feet 3 inches below the level of Trinity standard in the Thames. That, he believed, was connected by the late Mr. Page; but some years back he had had some correspondence with the authorities at Southampton, and Colonel Bailey took great trouble in looking through the levels, and he found that the observations of the tides at Sheerness, which were contained in the large book of levels of the Ordnance Survey, were made outside the dockyard; and he believed up to the present time had never been connected by direct levelling. Colonel Bailey estimated it at 11 feet 1 inch. On the 18th of January high water at Sheerness was 1 foot 9 inches below Trinity. At Gravesend the tide was 2 feet 9 inches above Trinity, a difference of 54 inches in a length of 21 miles, or 2½ inches per mile. He thought the difference might be explained by the fact that the estuary of the Thames at Sheerness was 5 miles in width; the width of the river at Gravesend was ⅞ mile, and the rapid diminution of the width over the 21 miles of course heaped up the water more rapidly than in the more gradual diminution from Gravesend to Westminster bridge (a length of 29 miles) from ⅞ mile to 800 or 1,000 feet in the Port of London. At Limehouse, the next height to which he could refer with any degree of certainty, at the West India dock entrance, and at the Lower Commercial dock entrance, nearly opposite, the height was 4 feet 5 inches above Trinity, or a rise of 1 inch per mile, 20 inches in some 22 to 23 miles. At the Surrey Commercial dock entrance, and at the London dock entrance opposite, the heights recorded were 4 feet 8 inches above Trinity; the length was 1½ mile, and the rise 3 inches, giving a gradient of about 2 inches per mile. That might be accounted for by the rapid decrease in the width of the river above the Shadwell entrance at Wapping. At London bridge the height recorded was 4 feet 10 inches; 2 miles higher up a difference of 2 inches, or 1 inch per mile. At Westminster bridge the height was 5 feet, a difference of 2 inches in 2 miles, or 1 inch per mile. At Teddington, 17 miles higher up, the height was 5 feet 5 inches. The absolute difference in level of the water surface at Sheerness as compared with Teddington was 7 feet 2 inches. That difference was remarkable, and it would be hardly credited by those who had not looked into the question. The tide of the 15th November, 1875, had this remarkable difference: that it was higher at sea. At Sheerness it was 3 inches below Trinity, and it was correspondingly high at

Mr. Redman. Gravesend, 3 feet 3 inches above Trinity, and yet it was 3 inches lower in the Port of London. This doubtless arose from the great gale of the 18th of January last affecting the water in the narrower metropolitan reaches more than in the estuary and wider lower reaches. Neither of those tides were equinoctial, but ordinary springs. That of 1875 after a great west gale, changing to north, was 3 feet 3 inches above the forecast, but that of the 18th of January was absolutely 5 feet above the estimated height, due to the great east gale, and in all probability the greatest excess on record. The phenomena at low water showed also the necessity of automatic gauges on the river. The tide in the Port of London ebbed considerably below that of Sheerness. He knew that it was discredited by many, but it was an absolute fact: those who disputed it forgot that in addition to the difference in level of the respective low waters that at the moment of low water in the Port of London the tide had risen 2½ hours at Sheerness—⅓ of the tidal flow, and that at the same instant the level of the water at Sheerness was 6 feet higher than it was in the Port of London. In fact the momentum of the ebbing water continued until it was overcome by the head of water from the sea. There was one point to which he desired to refer in reference to the fixing of the gauges—the great importance of having shafts removed away from the tide margin, which would allow the float to oscillate vertically through the entire passage of the tide. There was now an ebb of more than 23 feet below Trinity in the Port of London, and a maximum flow of 5 feet above Trinity. Shafts were wanted at least 30 feet in vertical height. Then the connection between the shaft and the river was of great importance. Sir William Thomson had referred to that fact in reference to Dover. The only practical difficulty was the checking of the undulatory motion, caused by passing steamers, and the question of frosting up the float. An arrangement of diaphragms in the connecting tunnel or culverts with the vertical shafts, or perforated diaphragms, was necessary. He thought that the connecting culverts in the Thames at least should be in duplicate, so that they might be pumped out and cleansed ultimately without interfering with the work of the gauge. Last and not least in importance, was the question of zero of indices sufficiently low to avoid minus readings? At present there were no less than four data in the Port of London, viz., Trinity standard, Ordnance datum, and two Admiralty low-water levels.

Mr. Cushing. Mr. T. Cushing remarked that the Astronomer Royal had observed that to the ingenuity of the Author was due the improved

Mr. Cushing. mechanical appliances now used in connection with tide gauges, and in this remark he to a considerable extent agreed; but the fact must not be overlooked that much credit was also due to others who had laboured in the same field. The Author, in describing his tide gauge, remarked that "for the present at all events, a pencil is by general consent the marker of an automatic tide gauge." Now, in the Loan Collection of Scientific Apparatus at South Kensington, in 1876, there was exhibited, by the Royal Prussian Geodetic Institute of Berlin, a tide gauge in which the marking point was a diamond. In that case the recording cylinder was covered with blackened chalk paper, and the diamond point, in obedience to the motions of the float, cut away the black surface, leaving a beautifully fine white line on a black ground. Again, in the year 1873, Professor van Rysselberghe, now of the Royal Observatory, Brussels, designed an automatic tide gauge for the Port of Ostend, in which the marker was a diamond point; but in this case the record was not made on paper, but on a sheet of ordinary zinc. The zinc was by a very neat arrangement fixed round the recording cylinder after having been coated on one side with etching varnish, and on this side the diamond point drew the tidal curves for seven days; the plate was then removed and dipped in etching fluid, and after that any number of prints could be taken from it. He possessed prints for the whole of the year 1878, taken from the plates automatically engraved by the Ostend instrument, an inspection of which would show that not only could the state of the tide be accurately determined at any given time, but that the time and the height of each individual wave was recorded, and he ventured to think that no less refined marking point would give such a result. In the next place the Author said that in his present tide gauges "the motion of the marker is vertical, the axis of the paper cylinder being vertical, instead of both being horizontal, as had been the case in nearly all previous tide gauges." This statement hardly agreed with the present state of knowledge on the subject, for many previous tidal machines had their recording cylinders placed in a vertical position. In Bunt's tide gauge, at Bristol, one of the first ever constructed on the automatic principle, and described, with illustrations, in the Philosophical Transactions in the year 1838, the recording cylinder was vertical. The instrument of Professor van Rysselberghe, just referred to, and erected on the eastern pier at the entrance to the Port of Ostend, and which engraved the plates from which the beautiful curves he exhibited were printed, had its cylinder vertical. Professor van Rysselberghe had also designed similar tide gauges, which were erected at Antwerp, Dendermonde, Tamise, Mr. Cushing. and other places in Belgium, all of which had their cylinders placed in the same position. Then there were several tidal machines at present in this country recording the tides on vertical cylinders. He might also mention that Colonel Sankey, R.E., designed in 1870 what he then called a flood gauge, intended for automatic registration in Indian rivers and tideways, with a vertical registering barrel. One other point might be mentioned with reference to tide gauges, and that was with regard to the scale of the instrument. It was a common thing, as the Author said, to make the motion of the pencil from one-tenth to one-hundredth of the motion of the floater; but in the twelve large tide gauges constructed by Mr. Adie for the Government of India in 1878–79, and now employed on the tidal operations of the Great Trigonometrical Survey in that country, the scale of each instrument could be adjusted at pleasure, so that the motion of the pencil was either equal to the rise and fall of the float itself, or was equal to a scale of $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{6}$, or $\frac{1}{8}$; in the latter case the instrument was capable of registering a tide of 40 feet, the counterpoising of the pencil carriage, so as to keep the friction as nearly as possible constant under all circumstances, being most effective, and he had reason to believe that the instruments had given great satisfaction to the officers of the department in which they were employed.

With regard to the Tide Predicter constructed for the India Office by Messrs. Légé and Co. he had had many opportunities of seeing it both in and out of action, and, whatever theoretical opinions might be held regarding it, he was sure there could not be two opinions as to the way in which it performed its work. It was a masterpiece of mechanism, reflecting the highest credit both on the designer and the constructor. The instrument took nearly five hours to work off the tidal curves of a given port for one year, including winding up the driving weight, &c. The Author considered this too slow a rate of working, but even with this speed the whole of a year's tides for five hundred different ports could be predicted in less than a year. It was true that some loss of time occurred in winding up the driving weight of this machine, which was at present 560 lbs; but this weight was distributed over a quadruple line to suit the exigencies of the building in which it was placed, which also necessitated a number of pulleys to lead off the weight to a convenient place, each of which meant more friction and additional weight to overcome it. He had no doubt that the machine could be driven at full speed

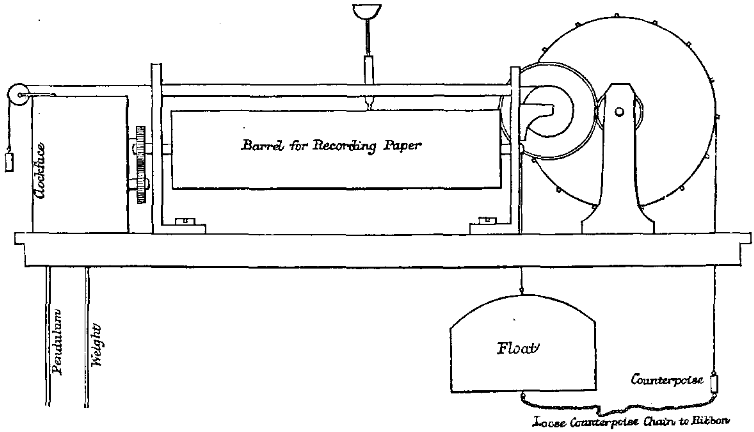
Mr. Cushing. with less than 100 lbs. weight on a single line if there was sufficient fall provided for it, and he thought this did not indicate much friction in a complex machine having something like two hundred and fifty moving parts, some moving with high velocities. It ought perhaps to be mentioned that it was originally intended to send this predicter to India, hence the necessity for providing a driving clock of some sort. But there were two ways of getting rid of the loss of time occasioned by winding up; first, by adopting Mr. Légé's idea of driving it by a small turbine, which was quite practicable where the machine was now placed, as there was a fall of water in the building of something like 80 feet. The second plan was to adopt Mr. Cooke's method of maintaining power, which had this advantage over all other maintaining powers with which he was acquainted, that the driving power of the machine was continued, however long the winding up might take. Either of these methods could now be adapted to this machine at a small cost, and would dispose of what he believed to be the only ground of objection which could be raised against it. A few specimen curves were drawn with this machine in the room in which the time scale was printed across the paper; this was done at the same time that the curves were drawn, and greatly facilitated the reading off. The arrangement for doing this was not included in the original design, but was added before the completion of the machine, and he had found it work well, as the specimens which he had produced clearly showed. The curves were drawn on a continuous band of paper to a particular scale; that scale was fixed by the present tide gauges in use in India, so that the paper on which the predicted curves were drawn might be sent out and compared with the curves drawn by the actual tides. His own opinion was that in future some arrangement, similar to that of Professor van Rysselberghe, would be adopted for recording the predicted curves by means of a diamond point upon copper or zinc plates, which would allow a fortnight's or a month's curves to be drawn on a single plate. The lines being very fine, and at the same time well defined, would enable a smaller scale, if necessary, to be adopted.

Mr. Adie. Mr. P. Adie desired to say a few words on the subject, because he felt that he was, to some extent, practically involved in the matter. Ten or twelve years ago, Colonel (now General) Walker applied to him to recommend the best form of gauge for the observations which he was desirous of taking in India in connection with supposed changes in the relative heights of sea and land in different parts of the Peninsula, and for which purpose

Mr. Adie.

he had now altogether made about twenty gauges (Fig. 15). He explained to him that the observations were to extend over a period of from ten to twenty, fifty, or, it might be, one hundred years. Of course, to meet such a case, it was his business to find out the most durable instrument that could be suggested, and, with that view, he adhered very much to the old style. He did not know who was the inventor, but the first maker of the pattern that he was aware of was the late Mr. Newman of Regent Street. As to the question of vertical and horizontal cylinders, he had tried it experimentally. Twenty-five years ago he had put up a vertical one for the Metropolitan Board of Works. It was rather too large in diameter and for comfortable manipulation, and he never liked it, especially as it did not answer for large heavy cylinders. As to the question in itself, he really thought that there was nothing in it. He looked upon a horizontal cylinder as being preferable, simply because it was more easily driven by the clock, and because the friction was more easily reduced. As to the question of friction, in the matter of registering with accuracy, he did not think there was much in it because the ocean afforded unlimited power, and therefore the float might be of any size. He saw no reason for circumscribing the power of the machine or diminishing the float. As he would give preference to a machine over a model, so he would make a tide gauge practically more of a machine than a model. He was guided by those principles in recommending the old form. He had brought a portion of a small

Fig. 15.



Tide gauge $\frac{1}{10}$ real size.

Mr. Adie. tide gauge to illustrate what he meant with regard to friction. If the float were pressed gently into the water and allowed to return of its own accord, and then by reverse process pulled gently out, and the difference observed on the full scale, as shown by the index against the wheel carrying the float, the greatest magnified error possible would be under $\frac{1}{10}$ inch, while the motion of the water corrected for this, and that on the diminished scale, was quite inappreciable. Another point might be added, that this pattern of gauge, while it was strong and durable, was much less costly than the other specimens shown. He did not like the results of diamond marking when he saw them.

Mr. Symons. Mr. G. J. Symons did not propose to allude to the purely tidal question, his acquaintance with tides being very slight, but he had had a little to do with self-recording machinery, and, with profound respect for the Author, he thought he had gone in the wrong direction in throwing away the motive power of the ocean. He could not help thinking that it was a mistake to try and work with so small a float. He had had considerable experience in trying to get a record of the measurement of evaporation. He wanted a record of the changes of water level to the hundredth of an inch, which of course was not a very easy thing to accomplish. The only mode was to use the largest float that could be got. The smaller the float obviously the less the motive power, and the less the motive power the more the element of friction came to counterbalance it. It was not as if one could always have the apparatus in one's laboratory. The Author had spoken of locking it up for twelve months. In most places, however, spiders' webs and such things would probably come in the way and form a disturbing element. A proof of that might be found in the statement of Mr. Redman as to what had happened with respect to the tides in January; for out of four or five gauges three had broken down. With regard to pencils, they were no doubt a source of great trouble. If they broke just when the machine was going to be wound up it would not matter, but they were sure to break at some other time. A great deal had to be learned from our continental friends. The method of engraving on plates or marking on blackened paper cylinders, as explained by Mr. Cushing, gave a very delicate line, which could be read with ease to the $\frac{3}{1000}$ or $\frac{4}{1000}$ of an inch. It might be thought that such cylinders were awkward to manage, but they were not so, and the result was superior to anything that could be obtained from photography or by pencils. One point with regard to tides had not been mentioned. Hitherto the suggestion had been that

Mr. Symons.

the main disturbing element in tides produced by meteorological causes was due to the wind. As a rule that was no doubt the case, and almost always in regard to rivers, except that some confusion arose from the coming down of the land-water. There had, however, been some most remarkable tidal disturbances in connection with thunderstorms. In the case of certain thunderstorms, there had been obtained most peculiar curves from self-recording barographs, showing a most marked variation in atmospheric pressure; and the same thing occasionally occurred in regard to tides. Mr. Cushing had just handed him an illustration of that in the Ostend records for 1878, but it was nothing like the extent of the variations to which he had alluded. In Pegwell Bay in 1858, during a sharp thunderstorm at nine o'clock in the morning, the water went out about 200 yards with a sudden rush, and in a short time came back again.[1] The same thing had been observed at Penzance, and had been the subject of Papers written by a gentleman residing in that locality.[2] There were also earthquake-waves, which afforded a strong argument for the erection of self-recording tidal gauges to trace their progress. It might be remembered that on the occasion of one of the violent earthquakes in South America not long ago the shock produced such a disturbance of the ocean that it travelled across to Australia, and was recorded on the tide gauge in Sydney harbour. He was afraid that the element of cost had not been sufficiently taken into consideration in regard to self-recording instruments. Many cases had occurred in his experience in which an automatic record of what had happened, as to the flow through sewers, or the amount of rainfall, or the changes in river levels, would have prevented expensive law suits; and if such records could be made simply and cheaply, a great advantage would be obtained. He could see no necessity for the great expense hitherto attaching to such apparatus. There was a simple apparatus used in many parts of London for measuring the pressure of gas in the mains, and he should imagine that an instrument of that kind could be turned out for a fourth or a fifth of what was paid for tide gauges. With regard to the communication between the shaft in which the floats were to work and the open sea, he was afraid that barnacles would get in the way of the flap-valve suggested by Mr. Law. If there

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[1] *Vide* "Symons's Monthly Meteorological Magazine," vol. iii. (1868), p. 81.

[2] *Vide* "On the two great Thunderstorms, with extraordinary Agitations of the Sea, in 1846," by R. Edmonds; and "On Earthquakes and extraordinary Agitations of the Sea."—"Phil. Mag." Jan. 1866, p. 45.

Mr. Symons. were a considerable number of them they might trust to the majority of them working all right, but he could not help thinking that the arrangement was a risky one, and he should prefer trusting to a multiplicity of small straight tubes.

Mr. Cowper. Mr. E. A. Cowper said it should be borne in mind with respect to the question of friction in the case of floats, that there was a constant slight motion produced by the waves, both above and below the average line, he therefore could not conceive of there being any error due to friction. It was just like tapping the barometer, which practically did away with friction.

Sir William Thomson. Sir William Thomson, observed, in reply, that Mr. Roberts had criticised the platinum wire used in tide gauges put up by himself, or under his advice. He had chosen platinum wire because it appeared to him to be more durable, and less likely to be disturbed by the moisture and the action of sea-water, and of sulphurous emanations in the case of tide gauges for harbours or rivers, than any other available substance; and he saw no reason to recede from the opinion which had primarily led him to its adoption. The only other substance comparable with platinum wire, in regard to those qualities, was gold. Platinum, however, was much stronger than gold, and he had no hesitation in giving it preference. Mr. Roberts, in speaking of the harmonic analyser, had referred to the total omission of all long-period tides both in the completed machine (of which the drawing was exhibited) and of a suggested second machine which he had sketched. Means for evaluating the long-period tides had not been omitted. The disk, which revolved uniformly, gave the true average of the height, and by reading it off once every month twelve data were obtained, from which it was perfectly easy to work out, in a few minutes, the annual and semi-annual tides. In the work done by him from year to year for the Tidal Committee of the British Association, they were found to be of great importance, and, as Mr. Roberts had said, it was necessary to take them into account in tidal predictions. He was glad to listen to all remarks on the scientific questions that had been brought before the Institution, but he did not think that anyone could be pleased to have such subjects brought forward as Mr. Roberts had introduced in the latter part of the statement which he had made. Mr. Roberts had stated that he was present on board the Author's yacht in August 1872, when Mr. Tower suggested, among other possible methods of combination, that of the chain and pulleys. It was not until after Mr. Roberts had left the yacht, and Mr. Tower and himself were making a journey in a railway carriage

Sir William Thomson.

from Portsmouth to Brighton, that Mr. Tower suggested that method of combination. With regard to that exceedingly good suggestion, it had been pointed out to him within the last few days that it had been realised long before by Mr. Bashforth, in a machine which he described to the British Association at Cambridge in 1845. An account of a machine involving the same plan for the construction of simple harmonic motions had been communicated to the Royal Society on the 17th June, 1869, by Mr. W. H. L. Russell, in a Paper "On the Mechanical Description of Curves." The "Philosophical Magazine" for 1870 (vol. xxxix., p. 304) contained an abstract of that Paper, with a drawing of the very instrument, of the existence of which Mr. Tower and himself were ignorant. Mr. Russell went back to Mr. Bashforth as having described the machine in 1845, and referred to him as the originator of it. Only a short abstract of the description was published in the volume of the British Association for the Cambridge (1845) meeting (Trans. of Sections, p. 3). They had Mr. Russell's authority for the statement that the machine was the same as that which he (Mr. Russell) described, and of which he gave a diagram. Mr. Roberts had quoted from the *Athenæum* report of the Brighton meeting of the British Association in 1872, a statement obviously fragmentary. "Each elementary tide gives a height of water proportional to the height of the end of a clock hand, which makes one revolution with uniform speed during the period of the elementary tide." That was, in fact, a description of the first essential structure or foundation of the tide-predicting machine. What was wanted to complete it was something to combine the motions of bodies, each moving, so as to be on a level with the end of a clock-hand moving in the manner described. Mr. Roberts stated that he (Sir W. Thomson) did not describe to the British Association at Brighton Mr. Tower's method of combination of pulleys and chain. What, however, was more to the purpose, in respect to Mr. Roberts' allegations, he wrote to Mr. White, before the end of the meeting, ordering a model to show the combination by hair-spring and pulleys; and he informed Mr. Roberts before the end of August 1872 that he had done so. Sir W. Thomson described to the British Association the multiple revolving clock-hands, each separately showing one tide, with a crosshead to give the simple harmonic motion, and he described to Mr. Roberts the method of combining by chain or hair-spring and pulleys. Mr. Roberts, believing Sir W. Thomson had abandoned the sliding crosshead, and believing that he had abandoned the hair-spring and pulleys, unknown to the Author, gave Mr. Légé, in February 1873, Sir William Thomson. directions to construct the two-component model with crossheads and chain and pulleys. Mr. Roberts was in the employment of the British Association Tidal Committee, being paid to make calculations for that committee. He was working under Sir W. Thomson's direction as chairman of the committee. Taking two methods which he believed Sir W. Thomson had discarded, one of which was all the world's, and the other was Mr. Tower's, Mr. Russell's, and Mr. Bashforth's, he stole a march upon him, as he supposed, in constructing the machine. He omitted to say that it was constructed at Sir W. Thomson's expense. The Author had in his hand Messrs. Légé's account for the completed instrument, headed, "Makers of Sir William Thomson's Tide Calculating Machine," and certified by Mr. Roberts, which was presented to him by Mr. Roberts for payment. Mr. Roberts had explained that a false label, by mistake, had been attached at Bradford to the machine in 1873, and he had apologised to the Author for the mistake; but now, in 1881, he adopted the effect of the false label in the description "Mr. Roberts' machine." Mr. Roberts had referred to his communication to the Royal Society of the 19th June, 1879. It was necessary to explain that, wishing to do every possible justice to Mr. Roberts, to give him an opportunity of coming before the Royal Society as having had the superintendence of the construction of the tide-predicting machine for the India Office, he had asked him to write an account of it, and said that he would himself communicate it. He had gone so far as to read the Paper to the Royal Society. After doing so he pointed out that a change of title was absolutely necessary to give a true account of the origin of the instrument, and that he thought the title which appeared was an inadvertence. He was exceedingly surprised to find that Mr. Roberts adhered to his title, which seemed to imply that the instrument was a new one, invented by himself. He then withdrew from being the communicator of the Paper, and he arranged, in consultation with Professor Stokes that he, as Secretary, should communicate it. Sir W. Thomson had not taken any notice of the matter for two years, and one result was that, in the *Engineer*, the machine was described as Mr. Roberts' Tide Predicting Machine. In fact, no detail of the India Office Tide Predicter, any more than of the two-component model and the South Kensington Tide Predicter, was due to Mr. Roberts, except the arithmetical design of the wheel gearing. Mr. Roberts said that "he was exceedingly pleased that in the Author's projected predicter, he had again reverted to Mr. Roberts' original plan of

Sir William Thomson.

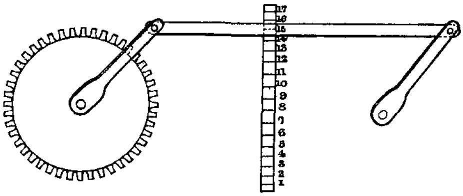
slides." Mr. Roberts' idea in 1872 of a parallel slide, or of the best mechanism to serve for one, was illustrated by the following extract from a letter to Sir William Thomson:—

"MR. EDWARD ROBERTS TO SIR WILLIAM THOMSON.

"85, HIGH STREET, HASTINGS.
"*Aug.* 26, 1872.

"I have been thinking of the Calculating Machine, and I think that the chief (and apparently the only) difficulty, will be in the combination; and I think that the simplest mechanism will be the best—a movement similar to the connecting-rod of the driving wheels of a locomotive, giving the range of each tide component.

FIG. 16.



This movement will give great power, and will not readily get out of order."

The India Office Tide Predicter, notwithstanding the serious faults referred to in the Paper, was a beautiful and grand piece of mechanism, and its construction did the greatest credit to its maker, Mr. Légé. Its usefulness was such that the Indian Government might be congratulated on the results of the expenditure, though heavy, which it had involved. The great fault of the mechanism,—the endless screws and the high speeds of their shafts,—was objectionable, not only because of the limitation of the speed of working to the four hours for running off a year's curves, but also because it necessitated an amount of driving power which was excessive, considering the work done. Attempting to make the fault seem of no importance, Mr. Roberts said, "The Author's objection to the limit of speed of working of the India Office predicter was not a valid one; the machine, being automatic, could be set over night, the traced curves being ready for manipulation the next morning." He had omitted to say that it had to be wound up (a process taking twenty minutes) three times in the course of the work for one year, stopping the work each time during

Sir William Thomson. the twenty minutes' winding. Thus, if it was to do a year's curves in the course of a night, the person in charge would have to get up twice in the night to wind it, and spend twenty minutes each time to do so. It was worked by a weight of 5 cwt., descending 26 feet for four months' work. That weight had to be wound up three times to a height of 26 feet to get through a year's work, and the whole time of winding-up was an hour for a year's work of tracing the tidal curve. Mr. Roberts was unwilling to admit that a machine which completed a year's work in twenty-four minutes was an improvement; but he (Sir Wm. Thomson) thought that to do the same work with less heavy and less costly and simpler mechanism, and tenfold greater speed, would generally be considered a great improvement. Mr. Roberts objected to No. 3 tide predicter in respect to accuracy of the speeds. He could scarcely have examined into the figures, or with his known skill in arithmetic, he would have made no such remark. The Table of speeds in Part III. of the Paper showed as the greatest amount of error 1°·95 in half a year; and this on the (MS.); a tidal constituent, which was of small amount, depending on shallow water, under the combined influence of the sun and moon. When it was remembered that the instrument was essentially re-set in respect to epochs for each year's work, it would be seen that the error arising from want of accuracy in the speeds was absolutely nil—that, in fact, it was impossible practically to perceive any effect of the difference between perfect accuracy and the degree of accuracy given by the numbers in the Table, in the results of the work of the machine.

As to Mr. Roberts' objection to the 802 teeth in one of the wheels of the No. 3 tide predicter, the Author had found the primary and secondary N wheels with their 802 and 423 teeth to work perfectly well. He had no difficulty in keeping them geared together. The diameter of the wheel of 802 teeth was not 15 inches, as supposed by Mr. Roberts to be the smallest practicable: it was 6 inches, as shown in the full-size drawing before the meeting.

Referring to the Astronomer Royal's remarks on the problem to be solved by the Harmonic Analyser, let

$$y = A_o + A\cos nt + B\sin nt + A'\cos n't + B'\sin n't \\ + A''\cos n''t + B''\sin n''t,$$

be the theoretical equation of the curve given by the self-registering instrument, whatever it be (tide-gauge in the special case before the meeting), or given by plotting properly the observations

Sir William Thomson.

(as in astronomical applications, pointed to by Sir George Airy). The nt, $n't$, $n''t$ corresponded to the θ, ϕ, ψ of Sir George Airy's verbal statement. The several counters of the machine, after it had been worked through a space corresponding to a whole time, T, gave the values of the integrals $\int_o^T y\,dt$, $\int_o^T \cos nt\, y\,d\,t$, $\int_o^T \sin nt\, y\, d\, t$, &c. Thus if C_o, C, D, C′, D′, C″, D″ denoted the readings of the several counters, there would be the following seven equations to determine the seven unknown quantities A_o, A, B, A′, B′, A″, B″:—

$$\mathrm{T\,A}_o + \frac{1}{n}\sin n\,\mathrm{T}.\mathrm{A} + \frac{1}{n}(1 - \cos n\,\mathrm{T})\,\mathrm{B} + \&\mathrm{c.} = \mathrm{C}_o,$$

$$\frac{1}{n}\sin n\,\mathrm{T}\,.\,\mathrm{A}_o + \frac{1}{2}\left(\mathrm{T} + \frac{1}{2\,n}\sin 2\,n\,\mathrm{T}\right)\mathrm{A} + \frac{1}{2\,n}(1 - \cos 2\,n\,\mathrm{T})\,\mathrm{B}$$
$$+\frac{1}{2}\left.\frac{\sin(n-n')\,\mathrm{T}}{n-n'} + \frac{\sin(n+n')\,\mathrm{T}}{n+n'}\right\}\mathrm{A}' + \&\mathrm{c.} = \mathrm{C},\ \&\mathrm{c.}$$

Here, provided T was large enough, the coefficient of A_o was relatively very large, and all the others small, in the first equation; that of A was relatively large, and all the others small, in the second equation, and so on. Hence approximately (more and more approximately the larger was T),

$$\mathrm{A}_o = \frac{\mathrm{C}_o}{\mathrm{T}},\ \mathrm{A} = \frac{2\,\mathrm{C}}{\mathrm{T} + \frac{1}{2\,n}\sin 2\,n\,\mathrm{T}},\ \&\mathrm{c.}$$

If T was not large enough to make these approximations sufficiently close, the first approximate values of A, B, A′, B′, A″, B″, were to be used in the equation for C_0 above, and a second approximate value for A_o was then instantly calculated by it. Similarly the first approximate values of A_o, B, A_1, &c., were to be used in the equation for C, and a second approximate value for A calculated from it, and so on for B, A′, B′, A″, B″. These second approximate values were in most practical cases as accurate, practically speaking, as the available observations could give. Thus the Harmonic Analyser really did the work which seemed to the Astronomer Royal so complicated and difficult that no machine "could master it."

An interesting point had been brought forward by Mr. Cushing in connection with engraving curves, and he had shown a beau-

Sir William Thomson. tiful specimen of the Ostend tide curves. He did not quite agree with him in his objection to the pencil. He had not the slightest difficulty in getting a sufficiently fine line with a pencil, and he therefore objected to introducing anything less simple. The diamond was valuable when they wished to engrave the result; but when that was not required it was an unnecessary expense. He knew of no case in which the pencil had been broken. With regard to the floater, he did not neglect the great power of the ocean, but he would not use a windmill 30 feet in diameter when a windmill 30 inches would suffice to do the work; and on that principle he preferred the smaller floater and a finer line connecting the floater with the wheel to the enormous floater shown by Mr. Adie, involving large expenditure to provide a large enough tube under water to guard it. Another reason for not making the floater too heavy was that in case of accident it should not be able to break the wire. He liked to have the wire strong enough to bear the floater. It would not do to have it too strong, or it would not be flexible enough to go round the pulley; and the strain must not be too great or the ink-marker would have to be too heavily weighted. He desired to call attention to a model made since the last meeting, and placed before the present meeting, showing the epicyclic method of combination, which he had explained at the commencement of the discussion on the present Paper, and which he had first described more than nine years ago to the British Association at Brighton. It was the very simplest of all ways of combining two constituents. It was essentially limited to two, and for some time he contemplated introducing it for combining the two chief tidal constituents, the lunar semi-diurnal, and the solar semi-diurnal in the predicting machine. He had given Mr. Légé instructions for making the model only last Tuesday, and it was now exhibited to the members; having been made according to the drawing (Fig. 12), which they had seen at the last meeting. This plan had an interesting feature—it showed to the eye the priming and the lagging of the tides. Thus it showed that the times of tides were unaffected by the disturbing influence of the sun at spring and at neap tides, but that the amplitudes were affected. It showed exactly how at the quarters intermediate between neap and spring tides there was a lagging or a priming.