Discussion.

Mr. W. H. PREECE, C.B., Vice-President, said all who were present must have recognized the extremely clear and able way in which the Authors had brought a most interesting subject before the Institution. Not only did the members attest by their number the interest of the Paper, but their applause had almost anticipated the proposition which it was his pleasure to make—that a hearty vote of thanks be accorded to the Authors. Mr. Preece.

Mr. J. I. THORNYCROFT, after referring to the Table (Appendix I) of the experiments made with the small model boiler by Prof. Capper, exhibited and described twenty-seven lantern-slides of a number of vessels illustrating the subject of the Paper. He afterwards showed the model boiler at work, in which the circulation of water could be seen, and demonstrated by aid of a model the effect of the distortion of an ordinary crank due to the turning-moment transmitted through it. Mr. Thornycroft.

Mr. A. F. YARROW thought the details of the "Daring" would be made more complete if the Authors would give the displacement at the commencement of the three hours' run. He did not think that was stated in the Paper, but it was essential in order to form a correct opinion as to the efficiency obtained. It might, perhaps, be interesting to the members to know the results obtained by his firm in the case of the "Havock" and the "Hornet" class of destroyers, so as to compare the power and speed obtained with the curves which the Authors had given. The progressive trials of the "Hornet" showed the following powers and speeds: 11·3 knots per hour with 207 HP.; 16·7 knots with 1,200 HP.; 23·4 knots with 2,700 HP.; and 27·6 knots with 3,884 HP., the last being the speed at the official trial. At that speed the mean air-pressure in the stokehold during three hours was 1·6 inch head of water, showing a large margin of boiler-power in excess of what was required. The "Hornet" at the time of the trial had a displacement of 220 tons. In comparing the weights of the boilers, the Authors gave the weight of the boilers in the "Daring" with water and mountings complete, including up-takes but without the funnels, at 48½ tons. In the "Hornet," which had 8,200 square feet of efficient heating-surface and 173 square feet of grate-surface, the weight of the boilers, including the same items, was 43 tons. As to the pro- Mr. Yarrow.

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Mr. Yarrow. portions of the propellers and the great gain shown by an increased area, to which the Authors had referred, it would add to the value of the Paper if it were stated what those areas were; because in the description it was simply stated that in the efficient screws the blade-area was increased by 45 per cent., but the areas or diameters of those formerly used were not given. He might say that in the destroyers built by his firm, propellers had been adopted of 6 feet 6 inches diameter and 8 feet uniform pitch, the three blades of each propeller having a total developed area of about 1,700 square inches. In the "Hornet" with 3,884 I.HP. a speed of 27·6 knots per hour was obtained, with a slip of 11 per cent. In the "Havock" at the speed of 26½ knots per hour the slip was 9 per cent. It might be interesting to note that one of the first boats built of about the same size as the present destroyers, was the "Kodaka," constructed by his firm for the Japanese Government in the year 1885. She was 170 feet in length by 19 feet 6 inches beam, and a peculiarity of the vessel lay in her being armour-plated with 1-inch plates throughout the boiler and engine compartment, both on deck, along the sides, and at the ends of the machinery space. The official reports of the performance of the vessel during the war were to the effect that the armour-plating had been a source of immense advantage, and it was this vessel which had led the successful torpedo attack at Port Arthur.[1] She also took a leading part in the attack at Wei-Hai-Wei, coming out with comparatively little injury, owing to the armour-plating. It might, perhaps, be a question for Governments to consider whether the increased protection secured by the armour-plating might not be well worth the reduction of speed which it involved.

Prof. Capper. Prof. D. S. Capper observed with reference to the series of trials which he had made with the model Thornycroft boiler exhibited, that it had at first been intended merely to determine the efficiency of the model so as to compare it with a full-sized boiler of the same type. The boiler had a heating-surface of 12·4 square feet, and when filled to working-level contained 4 lbs. of water. A complete report of the trials with a Table of the results was given in the Appendix to the Paper. One of the first trials made was that given in the third column of the Table. With a mean rate of transmission of 22·35 British thermal units per square foot of heating-surface per minute, the water evaporated per square foot of heating-surface per hour was 1·195 lbs., and the efficiency was 86·8 per cent. On comparing this with the trials made by

[1] *The Times*, November 24, 1894, p. 9.

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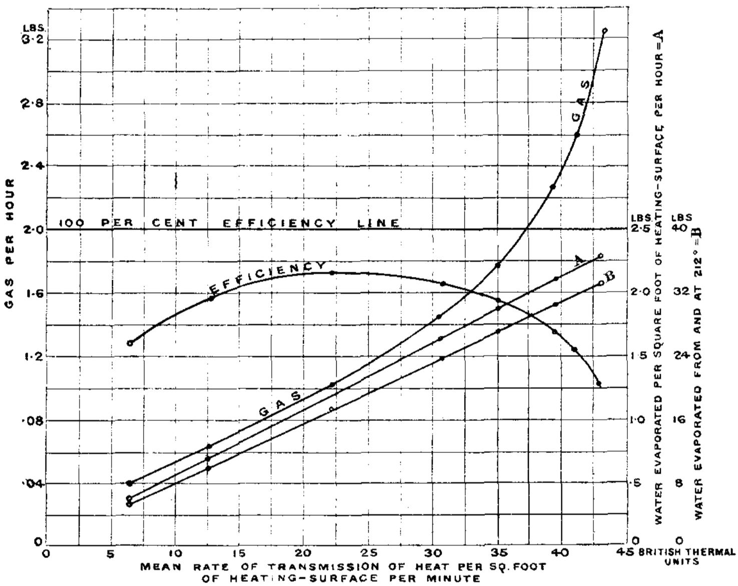
Dr. A. B. W. Kennedy on a Thornycroft boiler having 1,837 square feet of heating-surface, with a rate of transmission of 23·8 British thermal units per square foot heating-surface per minute, and water evaporated equal to 1·24 lbs. per square foot of heating-surface per hour, it appeared that the same efficiency had been obtained. This result was so striking that a series of trials at progressive rates of steaming had been carried out. The trials were all conducted at atmospheric pressure, the arrangements not being such as to permit of working at higher pressures. It might also be pointed out that there was no means at hand for artificially increas- Prof. Capper.

Fig. 24.



ing the air-supply, and consequently the rate of transmission of heat, *i.e.*, the rate of steaming, could only be increased by increasing the gas-supply. He thought the result of that would clearly be seen in the efficiency-curve, *Fig. 24.* The 100 per cent. efficiency-line was shown on the diagram, and the curve crossing it showed at each rate of steaming the efficiency as compared with perfect efficiency; starting with a rate of transmission of 6·4 British thermal units per square foot per minute, an efficiency of 64·7 per cent. was obtained, from which point the curve rose gradually up to its maximum value, 86·8 per cent. as already stated. After the maximum was reached, the efficiency decreased more and more

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Prof. Capper. rapidly until at a rate of transmission of 42·95 British thermal units per square foot per minute, it was 51·8 per cent. In Dr. Kennedy's trials that fall was not nearly so rapid, for at a rate of transmission of 158 British thermal units per square foot per minute he had obtained an efficiency of 66·6 per cent. This divergence was most probably due to the incomplete combustion of the gas in consequence of deficient air-supply in the experiments with the model after the maximum point had been reached. There were distinct signs that this was taking place during these trials. He thought it was highly probable that if experiments were carried out with a properly arranged artificial air-supply, the curve of model efficiencies would approach much more closely at its latter end to that of the full-sized boiler.

The first part of the curve was the most interesting and important, and clearly showed that in this case at least, experiments with a model having less than $12\frac{1}{2}$ square feet of heating-surface were comparable with the results obtained with a boiler having a heating-surface nearly 150 times as great. The thermal value of the gas used had been taken as 19,000 British thermal units per lb., this being the average of a number of analyses made by Mr. G. N. Huntly. These analyses differed little, although the samples were taken at long intervals—the last of them within a month of the trial. He thought that value could be adopted as accurate, although the gas was not analysed during the trials. Taking that figure, 1 lb. of gas was equivalent in heating-value to 1·32 lbs. of carbon; and at its maximum efficiency the model boiler therefore evaporated 13·02 lbs. of water from and at 212° F. per lb. of carbon value. That was a remarkable result in a boiler of the size referred to, and seemed to indicate that the type of boiler under consideration was as efficient on a small as on a large scale.

It had been pointed out by the Authors that they had invariably found when the speed of the engine exceeded a certain amount, the power seemed to be underlogged. It was a very interesting matter, which called in question the accuracy of the indicator. He would like to hear how far it was found that the power was underlogged, or that it was suspected of being underlogged. It was difficult to carry out experiments with an indicator in such a way as truly to compare its indications with any scientific standard when running at a high speed, and thus to determine its errors under working conditions. He had made a number of experiments bearing on that point on an apparatus he had devised for the purpose. A steam-cylinder was suitably connected with

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Prof. Capper.

a mercurial column so that pressures in the cylinder could be read directly in inches of mercury. He had found that if the pressure was suddenly applied by opening the cock of the indicator, the pressure recorded by the latter was invariably less than the true pressure as shown on the column. And that this difference was not due to any drop in the pressure of the steam-cylinder on opening the indicator-cock, was shown by the indications of a gauge attached to the cylinder. He was aware that gauge-readings were not to be relied on for absolute readings; but for checking differences of pressure they could be made to all practical requirement reliable. Those results seemed to point in the same direction as the underlogging of power recorded by the Authors, but his experiments, though extensive, were not sufficient in number and range to warrant him in asserting definitely that similar results would always be obtained. He thought the Authors were to be congratulated on having pointed to a limit being possibly obtained, in one direction at least, in the complicated problem of propellers; and the results which had been shown in connection with cavitation were extremely interesting and valuable.

Mr. Seaton.

Mr. A. E. SEATON asked if the float arrangement in the boiler was found to work fairly accurately, because he thought in a boiler of that kind, which was a pronouncedly priming boiler, there might be some difficulty in regulating the flow. He agreed in the advisability of providing such an apparatus. With respect to the screw-propellers, he prided himself in having predicted twelve months ago failure for the first of the type referred to in the Paper. He had told Mr. Thornycroft, Jun. (who had been good enough to give him the particulars of the propeller), nearly what would happen, not only as to failure of speed but as to the general results of its working. He had likewise indicated the area that he would recommend, which was, he thought, 45 per cent. greater than Messrs. Thornycroft had then fitted. He only mentioned these matters to show that the subject might be worked out without such an amount of scientific investigation as the Authors had favoured the Institution with. The Admiralty had been good enough to give his firm an order for some similar boats, and the first thing he considered was how he could strike out an original line; this he had done more in the construction of the engines than in the boilers, because at the last moment his firm was called upon to use one of three or four boiler-types that had been tried. Unfortunately there were not at Hull the sheltered harbours that existed on the Clyde, nor a long river as in the case of London. So far the weather had prevented

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Mr. Seaton. him from carrying out many experiments. One had, however, been successfully made. A boiler-tube had broken, and had not been discovered for half-an-hour, thus showing that that class of boiler was safer than had been anticipated at the outset. He would like to have the information which had been asked for by Mr. Yarrow, but at the same time he could not help thinking that it was trespassing on the Authors' generosity. A most interesting experiment, to his mind, was that shown with the crank-shaft. He had not seen such an experiment before, but it bore out an investigation he had made some years ago as to the action on the after-most bearing in an ordinary steamship. He was convinced that the breaking of crank-shafts was not due to the causes to which it was generally attributed, and the experiment shown by Mr. Thornycroft had confirmed that view. With the simplest apparatus, the Authors had shown that a great benefit could be obtained by an apparently slight alteration in the form of the shaft. The experiment shown with the model boiler was most interesting, as was also the statement made by Prof. Capper as to the comparative results of the model and a full-sized boiler. In the future manufacturers would be enabled to place models of their boilers in the hands of engineers and have them examined, without going to the expense of making full-sized boilers; and that, he thought, was most important—not only as saving in first cost, but as enabling experiments to be made which previously had been unthought of. He believed there was a great future for the water-tube boiler, in spite of the accidents that were heard of. Notwithstanding the attacks so freely made on the system, it was one which would survive, and a development would be seen in the future such as had not before been experienced in any branch of engineering. Experiments with water-tube boilers had been made long before he or any member present was born. In the early days of the present century, the pioneers of steam-engine engineering had made such experiments as Mr. Thornycroft had alluded to, and they were apparently quite as inventive as that gentleman or his coadjutors. That they had not succeeded was due to causes then beyond their control, and Mr. Seaton did not think much benefit could be derived from their experiments, inasmuch as the records had been so long buried in the past. In a Paper read before Section G of the British Association, in 1894,[1] Sir Frederick Bramwell had alluded to what had been done in his younger days with road-carriages;

[1] *The Engineer*, August 17, 1894, vol. lxxvii. p. 152.

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Mr. Seaton.

that was a subject which between 1825 and 1835 had occupied the engineering world very largely. In looking back upon the work of that period, it was interesting to find that engineers then were almost as advanced as those of the present day; inasmuch as the boiler which had been exhibited did not differ materially from that of Mr. Goldsworth Gurney, patented in 1827. The coil boiler, generally attributed to Herreshoff, was older still. A boiler worked in America in 1805—it was interesting to know that it had been suggested and introduced for marine purposes—did not differ largely from that of Mr. Yarrow's design; indeed, the tubes were of equal length in both boilers. They had been made of copper—he did not know whether that had been the cause of their failure—but they were eventually discarded.

Rear-Admiral FitzGerald.

Rear-Admiral C. C. P. FitzGerald, without entering into the technical part of the subject or appraising the differences between experts, thought, from a general view of the question, a class of vessel had been produced to meet the present requirements. It was felt throughout the Navy that most valuable vessels had been produced as the outcome of the highest ingenuity and engineering skill. A sketch of the development of the torpedo-boat destroyers, from the enlarged torpedo-boat to the "Halcyon," had been given by the Authors, who had modestly sandwiched the "Daring" into the series. He should have placed the last-mentioned vessel outside the series and considerably above any of those which had been referred to, because she was one with reduced tonnage and increased speed, which was what was wanted. The development had been somewhat curious. The enlarged torpedo-boat had been dropped, and the "Rattlesnake" resorted to, but she had been found not sufficiently fast or sea-worthy. The "Speedy" had then been built, with larger tonnage and more sea-going power, but no greater speed, and then the "Halcyon," 1,070 tons, with a larger armament and a slight reduction in speed. They had been called torpedo-boat destroyers or catchers, which was a misnomer, as they certainly never could have caught a torpedo-boat, and consequently could not have destroyed it. During that period of development, Mr. Thornycroft appeared to have retired from the scene; but, when he had been called upon to do so, he had gone back to 200 tons and produced an increase in speed of 7 knots, which was undoubtedly a marvellous development. He did not see why the horizontal screen, Figs. 6, Plate 1, should not act, but he did not like it from a nautical point of view, because it might come up just when it was wanted to stay down, and *vice versâ.* He should like to see a more solid and permanent fitting; for there

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Rear-Admiral FitzGerald. was no doubt that in the case of one vessel chasing another, the arrangement of the bow-gun was an important matter. If anything could be done to keep the spray out of the men's eyes, it would add greatly to the accuracy of fire. The question of colour, with regard to which the Authors had mentioned that birds of a variegated colour were less visible than those of one fixed colour, might, and he hoped would be, made the subject of experiment. The "Daring" appeared to be a marvellous production of engineering ingenuity and skill, and deserved the highest approbation. There was one point of technical detail with regard to the great rudder-heads of 7-inch steel shafting, to which he should like to refer. No doubt Mr. Thornycroft had considered such weight was necessary to obtain the speed and turning-power. The wonderful development in torpedo-boats was due, he thought, largely to the healthy rivalry which existed between the great torpedo-boat firms. Towards the conclusion of the Paper the Authors had stated that the limit of speed which could be efficiently obtained by the screw had been nearly reached. If higher speed could not be obtained with the screw, something else must be tried, for the Navy would never be satisfied with any finality in speed; engineers would have to find some other means of propulsion, and never tell seamen that any speed was the highest that could be attained.

Mr. Halpin. Mr. DRUITT HALPIN mentioned that in 1880 and 1881 he had tested a water-tube boiler designed by Mr. Fryor in 1874, *Figs. 25*, which had worked very successfully. The boiler had three tubes, a large one at the top and two at the bottom with large connecting down-comer tubes at each end and small tubes between the large top and bottom horizontal tubes. There were two furnaces, the gas passing between the tubes. It also had one of the first systematic arrangements for circulation, separate down-comer pipes 1 foot in diameter being supplied.[1] There were four such pipes, two at each end, quite outside the boiler, and the water could return downwards vertically through them, ascending through the smaller pipes, which were exposed to the fire. The boiler had 28 square feet of grate, and 573 square feet of heating-surface. It was a double-fired boiler with one furnace at each end, so that the smoke from one fire was more or less consumed by the other; the combustion was perfect, and was greatly assisted by a fire-arch. It was not used for marine work, but was simply

[1] An earlier design, Patent No. 12514 of 1849, by Messrs. Clarke and Motley, shows separate down-comer pipes to produce circulation.

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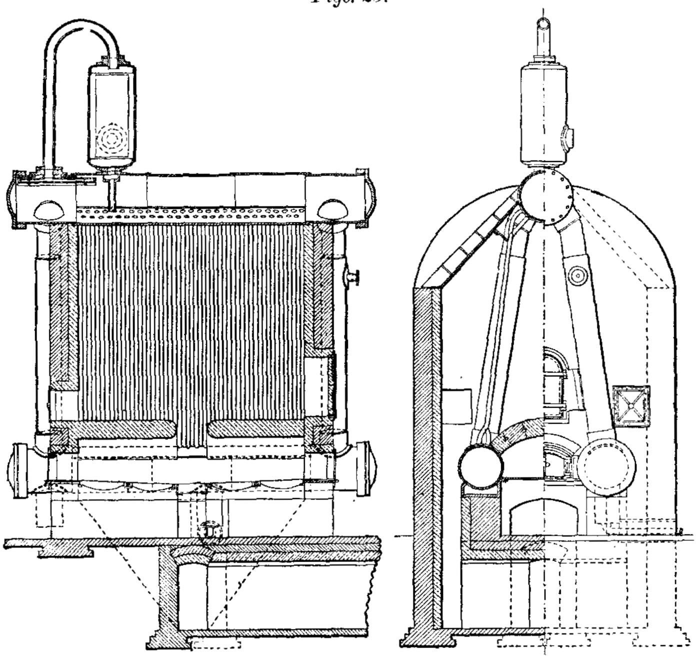
Mr. Halpin.

a boiler designed and built for use on land before torpedo-boats had been invented. He had made experiments at Mr. Yarrow's works some years ago upon a boiler of exactly similar design to those of torpedo-boats, but to work on land, so that the question of weight was not a matter for consideration. In spite of that, however, the boiler was very light, weighing only $2\frac{1}{2}$ tons. It had 7 feet of grate-area, and 364 superficial feet of heating-surface. The results showed that the boiler was evaporating 2,330 lbs. of water per hour at 200 lbs. per

Figs. 25.



Scale $\frac{1}{8}$ inch = 1 foot.

square inch. It was driven at the rate of 53 lbs. of coal per square foot per hour. There had been no calorimeter, and he could not speak with accuracy as to the dryness of the steam; but as far as it was possible to judge by the eye and ear, and by putting the hand in the steam, it was absolutely dry, and he had never had a trial run with greater comfort or with less trouble, even at that rate. The total length of the boiler was 8 feet and the width was 4 feet 8 inches. Steam was produced at

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Mr. Halpin. the rate of $63\frac{1}{2}$ lbs. of steam per hour per square foot of ground occupied. A Lancashire boiler 30 feet by 7 feet 6 inches, would occupy 350 square feet, and give between 5,000 lbs. and 6,000 lbs. of steam per hour, or only $15\frac{1}{2}$ lbs. of steam per square foot of surface occupied. Whilst worked at the rate of 53 lbs. of coal per square foot per hour, the boiler evaporated 7·57 lbs. of water per lb. of coal calculated from and at 212° F. He had not analysed the coal, but it was of the best Welsh quality. Assuming that its calorific value was 15,000, it would give about 48 per cent. thermal efficiency, which he considered exceedingly good under the conditions at which the boiler was driven. An interesting Table had been furnished by Prof. Capper, and if it was true that it was possible from model boilers to obtain figures which would be of use in large boilers, it was exceedingly valuable. In regard to the figures as compared with the test made by Dr. Kennedy, they were so near, and there were so many in parallel that it appeared as if the law indicated must be true, and as if it were not a mere series of coincidences. It appeared to him that the only flaw in the argument was that the model boiler was working at atmospheric pressure, whilst a full-sized boiler worked at 150 lbs. or 200 lbs. per square inch. The conditions were then very different, the specific gravity of the water varying 10 per cent., and the weight of the steam by hundreds per cent.; so that it was doubtful whether it would be absolutely safe under those circumstances to use models, giving steam of such different quality from the steam produced in the real boilers, and to draw conclusions from the results. He would be glad if Prof. Capper could make an addition to the Table (Appendix I) which would render it more valuable, by stating the temperature at the Bunsen burner; because it would then be possible from all the data to calculate what the real transmissions were. The temperatures of the escaping gases were given in the Table for all except the first two experiments; and in the case of the third experiment of the 25th February, there was the anomalous result that with the smallest difference of final temperature, which in that case was only 8° F., one of the highest transmissions of heat, viz., 35 B. T. U. per square foot per minute, was obtained. If it were possible to give the initial and the final temperatures of the hot gases, the mean temperature could then be deduced, the thermal head obtained, and the conditions be known more completely. The efficiencies taken from the Table were very good, averaging 71·4 per cent. A result as high as 86·8 per cent. had been obtained, and those who had achieved it should be heartily congratulated. The efficiencies

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obtained by Dr. Kennedy with the last large boiler were equally good. He understood that trial had been of a short duration of four or five hours, but he could mention four tests of 72 hours, 72 hours, 168 hours, and 168 hours' duration respectively made with a Lancashire boiler. The water and the coal had been carefully measured, and during the periods referred to—which of course meant clearing fires, night work, day work, Sunday work and the like—the efficiency had averaged 86·5 per cent. Mr. Halpin.

Mr. A. Mallock said, with reference to the work he had done in balancing the engines of the "Daring," the diagrams, Figs. 17, Plate 1, showed the effect on the engines during revolution. They consisted of a series of ellipses, the meaning being that the radius represented the force which acted on the engine at the crank when it was in a certain position defined by the radius of the circle. With every crank and eccentric, every moving part, there would be an ellipse of that kind formed, and when those ellipses were combined, taking the resultants of those forces in their order, they gave the total resultant force acting on the shaft. In the same way, by taking the moments of those forces about the centre of gravity of the moving parts, the resultant couple acting on the engine was obtained. All those forces and couples were of course reducible to a single force and a single couple; and if the necessary weights were applied in the shape of balances, the force acting on the engine would be balanced, in so far as it was simply a harmonic force acting once per revolution. But it had been brought out lately with some prominence that the shortness of the connecting-rods had an important effect on the balance of the engine. The force which acted on the engine was not a simple harmonic force, but had other terms in it. The chief effect of the shortness of the connecting-rod was to introduce a force acting on the engine, which had the same effect as $\frac{r}{4\,l}$ times the moving mass would have if revolving at twice the speed of the engine—r and l being the radius of the crank and the length of the connecting-rod respectively. That was a considerable force, and was the more important from the fact that it changed its sign so often. It had twice the frequency of the engine. The importance of small vibrations was only coming into prominence. Supposing a ship to be set in vibration by the engine, it would require an amplitude of vibration of $\frac{1}{2}$ inch when the engines were running at 250 revolutions per minute to produce an accelerating force equal to g. Taking 1,500 vibrations Mr. Mallock.

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Mr. Mallock. per minute, which he had often observed with an engine running at 250 revolutions per minute, $\frac{1}{100}$ inch in amplitude would give the same force. This amplitude was occasionally reached by the quick vibrations, and on the whole he thought that they caused more inconvenience than slower vibration with larger amplitude; because in the former case, when the upward acceleration was the greatest, the slightest sideways force would set in motion anything held in its place merely by gravity. In connection with the vibration of ships, it had lately been pointed out by several persons that it was important to know beforehand, if possible, where the nodes or stationary places of a ship would be when the vibrations were being executed, because it was possible to avoid vibration by disposing the moving masses in such a way as to be in the proper relation to those nodes. He had placed on the table an apparatus by which, with the aid of models, the position of the nodes in any ship could be determined beforehand. He might illustrate his remarks by a plank which he exhibited, which was the dynamical analogue of a ship as far as regarded oscillations in a vertical plane. It was a plank of uniform thickness, the profile of which was a curve with ordinates proportional to the bending-moments of the ship, the loads on it being proportional to the curve of loads. A plank of that kind was equivalent, in regard to vertical vibration, to the ship. Any disposition of load could be placed upon it. An abrupt variation in the strength of the ship would be represented by an abrupt difference of breadth in the plank, and an abrupt change of load would be represented by a discontinuous disposition of the load on the plank.

Mr. Terry. Mr. S. H. TERRY thought that the models exhibited by the Authors probably explained many a mysterious failure in a crank-shaft, and also illustrated the importance of dealing with any force due to pressure and momentum at the precise spot where the effects of that force were felt, instead of storing up these pressures to be imperfectly controlled at an adjacent bearing. In experiments which had been made as to the continuity of turning and immunity from vibration of different types of engines, viz., one-, two- and three-crank engines, in all cases a marked improvement had been found in the action of the last-mentioned type. At first sight it might be supposed that the uniformity of turning would be greater with the three-crank engine, but that there would be less vibration with a two-crank twin-engine with cranks at 180°. That, however, was not the case, and the three-crank engine showed not only greater uniformity of turning, but also superior freedom from vibration. The explanation of the matter had been recently very

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lucidly given by Mr. M. Robinson and Capt. M. H. P. R. Sankey.[1] Mr. Terry. Probably, with the types illustrated in Figs. 15 and 17, Plate 1, a very uniform turning-effort would be exerted. It might be learned from the Tables given by the Authors, and from the performance of certain boats with different types of water-tube boilers and locomotives, that where extremely high speeds were required, as in the case of vessels of great lightness in relation to the power expected from them, some form of water-tube boiler must be employed. He did not say that that was necessarily the case with mail-steamships and cargo-boats; but in the case of small fast craft it was certainly so.

Mr. A. Rigg remarked that the Paper dealt with a subject of which Mr. Rigg. few persons outside the firms which built torpedo-boat engines had sufficient experience to enable them to discuss it. With regard to the steering-gear and rudders, Figs. 13, Plate 1, it was an excellent arrangement, and that for several reasons which the Paper did not set forth. The boat prevented the air from entering at the top, and the side rudders not only guarded the screw but also protected it from air going in at the sides. The screw-propeller was single, with blades turned backwards for the purpose of overcoming the centrifugal force of the water, which at a high speed must be enormous. He had made many experiments with screw-propellers, and his conclusion was that, whatever else happened, the water left the blade vertically. There were many types of propellers made, but he thought that four blades were in some respects better than three; that, however, might be due to his having experimented with larger screws and lower velocities than those used by the Authors. It occurred to him that the horizontal vibration described in the diagram might be to some extent counteracted by the motion of the slide-valves. The cranks appeared to him to have too many sharp corners, because it was notorious that of whatever the material the shafts were made, such corners should be well rounded or fracture was inevitable; but probably the figure was only a diagram and was not accurate in regard to such details. There were two systems of running high-speed engines. One was the rigid system, in which case the shaft was made of great strength, and the other was the flexible system. The engines of a torpedo-boat ought to be on the elastic or flexible system. The bearings described were apparently rigid, but he should have thought it

[1] Transactions Inst. Naval Architects, 5 April, 1894. "On a Method of Extinguishing Vibrations in Marine Engines."

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Mr. Rigg. would have been an improvement to make them slightly movable. The shaft would bend and the bearings would be always free, and would be more durable than if they were kept too rigid. His opinion was that there was no such thing as slip of a screw, and that it was a misnomer in every way. It assumed that the screw-propeller should go through water as a solid body. It really acted as an oblique paddle. By regarding the propeller as an oblique paddle, it gave a good screw to work the ship, but taken as a true screw it was a bad propeller, because no one treated it in practice as if the theory was correct. By comparing the performances of a true screw and a flat board or blade, the latter would give the best result so long as it had sufficient "pitch." The experiments described showed much the same thing, and if the information given in the Paper were digested it would prove extremely valuable not only to builders of torpedo-boats, but to that far more extended class who used screw-propellers, and who sought in vain among writers on the subject to find anything better than "rule of thumb" to guide to a correct form suitable to any particular vessel.

Mr. Beaumont. Mr. W. W. BEAUMONT could not agree with the remarks of Mr. Rigg with regard to flexible bearings and securing the means for permitting crank-shafts to bend. He thought if there was any part that should be rigid, in an engine of the kind referred to in the Paper, it was that which carried the crank-bearings. Assuming that the crank and bearings and all such parts were perfectly true to begin with, they could not be too rigid, and the bearings could not be too near to their work or to the pressure that had to be brought to bear upon them. If flexibility was to be introduced anywhere, it was certainly not in the part where the bearings were necessarily close together. Anything like bendings, repeated in exactly opposite directions as they were, with so great a rate of revolution, must ultimately result in fracture of the crank-shaft. With reference to the balancing of the engines, it would be seen that calculations were made showing what must be the weight of bob-weights, or equivalent bob-weights, to meet and resist certain disturbing forces. He thought it might be shown with regard to vertical forces brought to bear in the engines, that they might be balanced within a small amount by means that were commonly known—including the question of obliquity on the connecting-rod and of the difference in the velocity at the different parts of the stroke. It was well known that Mr. Yarrow had done a great deal in that direction by means of bob-weights which had a vertical motion. It appeared to Mr. Beaumont, guided by what

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he had done in other directions in making use of the forces that were set up by unbalanced masses, that by using bob-weights, or weights that were moved by forces originating vibration, the excess of weight necessary to balance the vertical forces might be employed in a horizontal instead of a vertical direction; or in some cases they might be used, if not horizontally, at an angle that should be determined by the balance that had to be effected. If, instead of using vertical bob-weights, for instance, there were weights either suspended or allowed to slide, the forces that had to be dissipated, as otherwise setting up vibratory effects, might be used to move those weights either horizontal or nearly so. It would be found that the paths of the force, or curve representing the force, as set out by Mr. Mallock, and shown on the diagram, Figs. 17, Plate 1, might be converted into circular paths; and in that case, the unbalanced forces might be reduced to so small a value that it would not be a question so much of what the positions of the nodes of the vessel were, as one of the engine, so as to make the ship-builder independent of that consideration altogether. He thought it was quite impossible to arrange matters in such a way that the loads in a ship should always be the same. The cargo changing, and also the speed, would make the nodes vary in position; and it therefore seemed to him to be necessary that the engine should receive all the attention possible, and that the work which had been done by Mr. Yarrow and Mr. Mallock should be developed in such a way as to obliterate those forces tending to produce vibration, rather than that attempts should be made to cause ships to suit the vibratory tendency of those waste forces in unbalanced engines. Mr. Beaumont.

Mr. DARNTON HUTTON considered that the Paper contained many entirely novel points. He knew something of the "Daring," and had received a letter from one of her officers, stating that the engines and boilers, especially the latter, had given unqualified satisfaction. With reference to the spray-guard, it was perfectly true that it was a difficult thing, on account of the spray, to lay the guns at full speed; but there was a greater difficulty still, as to which he could testify from personal experience, namely, that the vibration made the sights jump so much that it was not possible to lay the guns with precision when going at full speed. The usual way was to stop the engines for a minute or so, take aim, and then to fire and go on again. It would be interesting to know whether some additional speed could not be obtained by cutting away the fore-foot of the ship, as was now done in the case of racing vessels. It would tend to reduce the surface-friction. The Authors might Mr. Hutton.

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Mr. Hutton. perhaps state the surface-resistance and the wave-resistance at different speeds, as the information would be exceedingly interesting.

Prof. Greenhill. Professor A. G. GREENHILL expressed surprise at hearing that the Authors were beginning to lose confidence in their old ally the screw-propeller, and to fear that it would not continue to respond to all the increasing demands made upon it. He did not share those misgivings, especially as he had read in the Paper that an increase of the blade-area in the propellers of the "Daring" was sufficient to overcome the difficulty; and he thought that in time the screw-propeller would approximate to the shape of the Archimedean screw of the early inventors. No difficulty had been felt in the use of the turbine and Pelton wheel under the highest water-pressure; and the screw-propeller ought also to work equally well under more serious and more arduous conditions, considering that the dynamical theory was the same for those two classes of engines. He hoped that the Authors would continue their valuable experiments with model screw-propellers, especially since those experiments might be made indoors in the laboratory. The experiments could be carried out in an artificially created current of water moving with high velocities to imitate the conditions attained in high-speed torpedo-boats—speeds of 30 knots or 40 knots per hour, or even higher. Such currents could easily be obtained from the mains of the Hydraulic Power Company.

Mr. McGregor. Mr. J. MCGREGOR could hardly agree that experiments with model boilers were likely to form a basis of calculation for the construction of large boilers. He did not think there had been any law deduced from the experiments referred to with the model boiler, by which the performance of large boilers could be ascertained. The conditions of the experiments were so different from those of actual practice that it was a difficult thing to apply them in any way. He did not follow the results stated; *e.g.*, the mean rate of transmission per square foot of heating-surface per minute in the first experiment was 6 units, in the second 12 units, then 22 units and 30 units. Those figures were so small as compared with the results obtained by the Research Committee of the Institution of Mechanical Engineers on Marine-Engine Trials, that he hardly understood how they could be applied in practice. The figures obtained by that Committee were between 51 units and 159 units. With reference to the temperature of the gases, he observed that they had been ejected at temperatures of 228° F., 238° F., and 220° F.—a few degrees above boiling-point—so that the amount of fuel burned in the boiler must have been small in relation to the heating-surface. With regard to the efficiencies—

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64 per cent., 78 per cent., 86 per cent., and 82 per cent.—he believed that the efficiencies of ordinary steam-boilers were seldom above 70 per cent. One result, indeed, of the Research Committee went as far as 84 per cent., but that was rejected as untrustworthy. [Mr. BARNABY asked if water-tube boilers were used.] Water-tube boilers had not been used. He was alluding to ordinary heating-surface. He would, therefore, like to know more about the trials where such high rates of efficiency were obtained. They were undoubtedly interesting, but he did not see that they could be profitably applied in the way suggested. Mr. McGregor.

Mr. MARK ROBINSON said that what might be called one of the by-subjects of the Paper, which had come into prominence during the discussion—viz., the question of vibration, was a subject to which his colleague, Captain Sankey, and he had lately given a great deal of attention. They had learned from the Author that the "Daring's" four-crank engines had given good results in respect to freedom from vibration. Various views had been expressed on the subject, which he would be sorry to attempt to discuss without more preparation; but he thought the influence of the obliquity of the connecting-rod was now recognised, and that soon a useful and practical theory of the vibration of marine-engines would be arrived at, and would be the means of stopping it. Reference had been made to the position of vibratory nodes in the ship, and to the importance of so placing the engines with reference to the nodal points that the vibration set up should not be communicated to the ship. No doubt that was a very important subject of investigation, but the line which Captain Sankey and he had taken up was that it was better to get engines altogether free from vibration. Ordinary engines gave rise to forces due to inertia which might of course be resolved into a couple and into a force; but in an engine which had three cranks equally spaced, or four cranks equally spaced, and all its parts of equal weight, there was nothing but the couple to deal with, and no other inertia force. The "Daring" had four-crank engines, but the moving parts were not of equal weight. He did not know that they differed largely from equality, because the low-pressure cylinder was divided into two; in any case a good result was given. If an engine was not produced which had neither couple nor vertical force to deal with, the next best thing was to have an engine which had a couple, but which eliminated, or nearly eliminated, the vertical force. Mr. Robinson.

Mr. C. H. WINGFIELD said there were no sharp corners where the section of the crank-shafts shown in Fig. 15, Plate 1, changed; Mr. Wingfield.

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Mr. Wingfield. the corners between the flat and cylindrical parts were rounded to a large radius. The small loads that were imposed on the main journals conduced to cool bearings. The two L-P. pistons were of the same size, and when one was pushing the crank-shaft down the other was pulling it up, and a couple was thus formed tending to tilt the shaft. The resistance of the bearings, which were about three times as far apart as the two crank-pins, formed an opposing couple. The pressure on each bearing would then be about one-third of the load on one piston. With the more usual arrangement of two bearings for each crank, the pressure on each was about one-half the load on the piston. The direct result of the design therefore was that the pressure on the main bearings and the tendency to heat was two-thirds of what it would be with similar cylinders having twice the number of bearings and a crank-shaft of the usual design. The indicator-springs had been referred to by Professor Capper. He should like to know how the proper temperature at which to test indicator-springs was usually determined. If the strength varied at different temperatures of the springs, a spring which gave a correct reading for the initial pressure on an indicator-card would not be correct when indicating the back-pressure line, since the spring was then exposed to cooler steam and its readings would be to a different scale. Probably the best temperature at which to test them would be a mean between the maximum and minimum to which they would be exposed when in use. He had tested a safety-valve spring while it was surrounded by water kept boiling by a gas-jet, and there had been no sensible difference between the load required to produce a given deflection under these conditions and when cold. The arrangement of cranks shown in Figs. 14 and 15, Plate 1, led to a more regular turning-moment than was at first apparent. Since two adjacent pistons reached the ends of their strokes simultaneously, the resulting twisting-moment was identical with what was found in engines having cranks opposite and cylinders parallel. At first sight such a pair of cranks might be supposed to have a turning-moment as irregular as a single crank and single cylinder, so that the dotted line c in *Fig. 26* would represent two superimposed crank-effort diagrams (one for each cylinder) which when combined gave the full curve d. On referring to *Fig. 27* it would be seen that if one piston P_1 were put in the centre of its stroke, the other one P_2 would be some distance past it, and the crank-effort diagrams $c\,c^1$ (*Fig. 28*) being thus relatively displaced, the combined curve d was flatter than that in *Fig. 26*, showing that the turning-moment was more uniform. When this was combined with the curves for the

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other two cylinders, the cranks of which were at right-angles to the first pair, it was found that the maximum twist was only 1·2 of the mean turning-moment; a result which compared favourably with engines having their cranks 120° apart. Mr. Wingfield.

It had been assumed by the Authors, p. 61, that the indicated thrust at zero speed was a measure of the initial friction of the engines. He thought the initial thrust multiplied by the pitch of the propeller would be found to give a more accurate measure

Fig. 26.

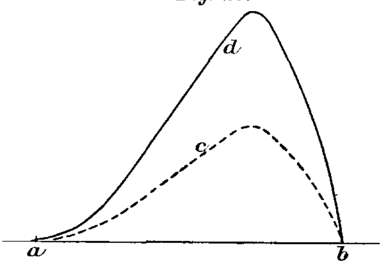


Fig. 28.

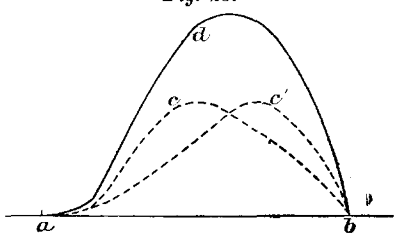
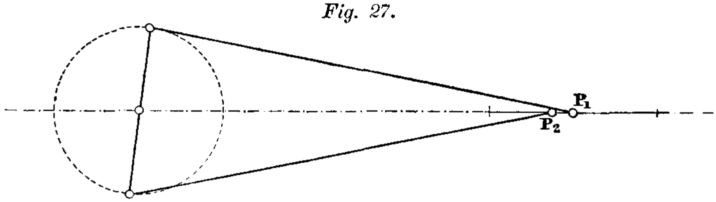


Fig. 27.



of the power wasted per revolution in friction.[1] Measured in this way, the friction per revolution of the "Speedy's" engines and shafting was 1·84 of that of the "Daring," although the latter had four cylinders and valves against three in the case of the "Speedy." If the thrust alone were considered to

[1] It has been experimentally proved by Prof. R. H. Thurston (*Engineering*, vol. xlvii. p. 22, &c.) that the I.HP. wasted per revolution in overcoming friction is nearly constant at all speeds and loads, and that, contrary to what was at one time accepted, it is also nearly independent of the steam-pressure. That conclusion has since been confirmed by other experimenters, and the late Mr. Peter Willans has proved its correctness.

If the I.HP. wasted per revolution is thus constant, it can be represented by $\frac{t \times \text{pitch of propeller in feet}}{33{,}000}$, where t is a constant thrust corresponding with that given by Mr. Froude's diagram at zero speed. This is deduced from the equation for the indicated thrust-curve, which is

$$T = \frac{33{,}000 \times \text{I.HP.}}{\text{pitch in feet} \times \text{revolutions per minute}}.$$

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Mr. Wingfield. represent friction, the ratio was $\frac{2800}{1750}$ or 1·6 only. He thought the thrust *per se*, which was a purely static pressure, could not be a measure of power wasted in friction, which was expressed in dynamical units. The adoption of horizontal bob-weights had been suggested by Mr. Beaumont. The use of such bob-weights had been, in the form of pendulums, fully considered when the engines had been designed. It had been decided, however, that they were unnecessary, and the engines had proved to be very free from vibration — so much so that vibration was scarcely perceptible in the engine-room, and the men in the boiler-rooms often did not know whether the engines were running or not. That was largely due to the causes given in the Paper, but partly, no doubt, to those which had been mentioned by Mr. Mark Robinson.

Prof. Boys. Prof. C. V. Boys expressed some hesitation in speaking before so many engineers on an engineering subject, and would not have ventured to do so had he not been one of the fortunate few who had been able to be present on the occasion when the "Daring" beat all previous records. It so happened that that took place on one of three consecutive days in which he was experiencing some modern methods of passing through the air at a high speed. One had been spent on a new racing bicycle; the second was that to which he had referred, and the third, or part of it, was spent in runs on Mr. Maxim's flying-machine. He would like to ask a question as to the limit of speed which was physically possible. If the skill of the mechanical engineer had been almost exhausted in producing a maximum of power with a minimum of weight, and that of the naval architect in producing a form of ship in which the resistance to passage through the water should be as small as possible; if further, as seemed to be the case, judging from the wave which was experienced, the amount of energy spent in producing waves in a vessel such as the "Daring" was not, after all, enormous compared with that occupied in overcoming skin-friction—anything that would reduce the skin-friction by even a very moderate amount might be a matter of great importance. An idea suggested was to pump air down the stem of the vessel and along the keel—not too far along the keel, as it was important that the air should not get into the propellors, but so far that the rising air should be entirely free from entering the rush of water which passed by them; then the question was, would the skin-friction be greater or less in rushing through the foam? Certainly *a priori* considerations, which he knew were of very little consequence, would have led him to believe that less skin-friction

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would be experienced in driving a vessel through foam; and since Prof. Boys. only 1 HP. or so would be needed (taking a vessel of the size of the "Daring," and travelling at its speed) to send down enough air to coat the vessel with a layer, which, if continuous, would be $\frac{1}{4}$ inch in thickness; surely, if any advantage was to be gained by the use of that species of ball-bearing, then the power and the trouble involved would be slight in comparison with the gain, if that gain were any moderate percentage of the 4,000 HP. or 5,000 HP. which was developed in so small a ship.

Mr. BARNABY, in reply, said, in reference to Mr. Hutton's state- Mr. Barnaby. ment that it was impossible to lay the guns of the "Daring" with precision when going at full speed, and that it was necessary to stop the engines in order to take aim, he presumed that Mr. Hutton referred to the 12-pr. gun which was mounted upon the top of the conning-tower. By an error of judgment the top plate of the tower to which this somewhat heavy gun was attached had been made flat; and as the diameter of the base of the cone mounting was considerably less than that of the tower, the plate proved to be insufficiently rigid, and allowed the gun to shake with the motion of the ship. In the sister vessel, the "Decoy," and in all the later destroyers, the top of the tower was domed, and this simple alteration had remedied the defect. Mr. Barnaby had attended the gun-trials of the "Decoy" for the purpose of observing the behaviour of that gun, and he had seen all the guns laid and fired at speed, and so far as he knew no further trouble from the cause referred to had been experienced. Further particulars had been asked for by Mr. Yarrow as to the displacement of the "Daring" and the dimensions of her propellers; but he was sorry not to be able to give that information. He might state, however, that from the figures Mr. Yarrow had supplied, it was found that the performance of the "Hornet," as measured by the Admiralty displacement coefficient at 27·6 knots per hour, was between that of the "Daring" and the "Decoy," but that the later boats, the "Boxer" and the "Bruiser," which ran at a considerably higher speed, had given much higher coefficients. Particulars of the screws of the "Hornet" had been given by Mr. Yarrow, and Mr. Barnaby had found from those particulars that the negative pressure amounted to 6·15 lbs. per square inch. It was said in the Paper that cavitation appeared to become detrimental if a negative pressure of $6\frac{1}{4}$ lbs. per square inch was exceeded, so that the "Hornet's" screws gave results singularly near that figure, and showed that they were large enough for the power and speed obtained. It had been stated by

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Mr. Barnaby. Mr. Seaton that he prided himself for having predicted the failure of the first screws of the "Daring," not only as to failure of speed, but as to the general result of working. He thought that before a prophet could take credit for a prediction, he must show that the prediction was previous to the event professed to be foretold. It so happened that the date of the conversation referred to was March 15th, 1894. The failure of the first screws of the "Daring," which Mr. Seaton said he foretold, took place in the months of January and February of that year, so that his statement was not quite a prediction. It was a matter of common knowledge in March among their competitors that there was a failure to obtain the speed with the screws of the "Daring." Mr. Seaton had stated that he had "mentioned these matters to show that the subject might be worked out without such an amount of scientific investigation as the Authors had favoured the Institution with." He could only say that the remarkable failure of the narrow-bladed screws came upon them as a surprise; and as the cause appeared to be novel and attributable solely to the high speed sought to be attained, they thought the results of the investigation they were led to make into the subject, and the conclusion they drew from them, might be of interest. Admiral FitzGerald did not see why the horizontal screen should not act, but would prefer the object to be attained by more flaring bows. It had been proposed to do that to some extent in the new destroyers, but it should be remembered that in long ships of that kind, flaring above water increased the bending-moments when in the trough of the wave, and he did not think it would suffice to keep the spray down. It would probably run up the flare of the bow until it arrived at the deck edge, and would then be blown on board, which was not the case if it met a horizontal screen. It was a most important point, as affecting the possibility of getting good practice with bow-guns at a high speed. If the chase made off to windward, it would put the pursuer at a great disadvantage in having to fire through the spray. It had also been said very properly by Admiral FitzGerald that the Navy would never be satisfied with any finality in speed. He did not say that the limit of speed had been reached, but expected a considerable increase over any that had yet been obtained. All he contended was that that increase of speed would probably be attained by some large sacrifice of power, because reduced efficiency of screw-propulsion seemed likely to attend any increase over what had been already effected.

Mr. Thornycroft. Mr. THORNYCROFT, in reply, said it was a mistake to suppose

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Mr. Thornycroft.

that the boiler exhibited did not differ materially from that patented by Mr. Goldsworthy Gurney in 1827. One important feature which enabled the boiler to be put into a torpedo-boat with success, was that, instead of being surrounded by a fire-brick chamber, it consisted largely of walls of tubes. That formed a feature which protected the coal-bunkers from heat and enabled a large fire to be put near a considerable amount of combustible coal without danger. When fire-bricks were used, unless special means could be arranged to pass a current of air around the bricks, they became heated throughout, and the result was a temperature which was in danger of setting fire to the ship. He was glad to be able to say that the float-gear had worked exceedingly well. The feeding on the trial of the "Bruiser" quite recently was so perfect that for about three hours at full speed the feed was not touched, being regulated entirely automatically. It seemed merely to require that a man should watch, and that the pump in the engine-room should continue to supply the water which was distributed as required. The boiler represented in *Figs. 25*, consisted almost entirely of two rows of tubes having very narrow openings between them for the gases to pass through, which would not be suitable for the purpose in question. He might be permitted to say that it appeared to him that that boiler did not in any way forestall that described in the Paper. The questions raised by Mr. Mallock were of great interest, that of balancing engines being one which had recently occupied much attention. The disturbance which took place twice during the revolution, due to the obliquity of the connecting-rod, and consequently the greater acceleration during the upper part compared with the lower part of the revolution, due to the crank, was a matter which required attention. The point raised by Mr. Wingfield, the equality of the turning-moment, from what was equivalent to right-angled engines due to that obliquity, was an illustration of what might be a disadvantage in one case and a help in another. If there was a disadvantage due to obliquity, to some extent there was a gain in another respect; because really the stresses on the crank-shaft were reduced by the uniformity of the turning-moment. He would direct particular attention to the model exhibited by Mr. Mallock showing how the position of the nodes might be estimated, and also the period of vibration of a steamer. It might be true that, owing to the change of load, the nodes might vary somewhat in position, but in warships the distribution of the weights would vary little in any particular ship; and it was certainly a great point if the period of vibration

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Mr. Thornycroft.

could only be known before building a vessel. Great expense was often incurred in making new screw-propellers, because, on trying a vessel, it sometimes happened that the particular period at which the vessel vibrated coincided nearly with that of the period of revolution of the screw-propellers. He might be permitted to mention that costly modifications had been necessary in some of the largest and most expensive vessels running across the Atlantic. With regard to the rapid vibrations which took place in vessels, Mr. Mallock had stated that he had measured 1,500 in a minute. That appeared to be the fourth phase in a particular ship measured, where a vibration so small as $\frac{1}{100}$ inch was equivalent in acceleration to the force of gravity, so that objects would travel on a table and only touch it occasionally. There was one thing that unfortunately happened in ships and required special attention—that, although all the nodes of a ship might be known, and provision be made against vibration, still individual objects, such as binnacles and things standing, as it were, on levers, would synchronise with the engines. He thanked the members for the manner in which the Paper had been received, and acknowledged his indebtedness to the energetic assistance rendered by his staff in its preparation.

Correspondence.

Mr. Durley.

Mr. R. J. DURLEY observed that, in the case of the "Daring," the indicated thrust at zero speed amounted to 1,750 lbs., the corresponding quantity for the "Speedy" being 2,800 lbs. These numbers were proportional to the frictional resistance of the main engines in the two cases; and the conclusion was drawn that the smaller loss from friction in the engines of the "Daring" was due to the fact that they were of the four-crank inclined type, as contrasted with the three-crank engines of the "Speedy." The two cases did not seem comparable, inasmuch as the engines of the "Speedy," although of practically the same I.HP. as those of the "Daring," were much larger and heavier, and were driven at a lower speed. It might therefore be expected that the total loss from friction would be less in a smaller and lighter engine, although the mechanical efficiency of the engine would not necessarily be higher. This view seemed to be corroborated by the value of the ratio $\frac{\text{indicated thrust at zero speed}}{\text{indicated thrust at full speed}}$ in the two

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