THE INSTITUTION OF NAVAL ARCHITECTS.

THE annual spring meeting of the Institution of Naval Architects was held on Wednesday, Thursday, and Friday of last week, in the theatre of the Society of Arts. There was a long list of papers set down for reading and discussion as follows:—

"Notes on further Experience with first-class Battle-ships," by Sir William White, Director of Naval Construction.

"The Elements of Force in a Warship," by Vice-Admiral P. H. Colomb.

"On Steam Pipes," by J. T. Milton, Chief Engineer Surveyor, Lloyds' Registry of Shipping.

"Light Draught River Steamers," by George Rickard.

"On solid Stream Forms and the Depth of Water necessary to avoid abnormal Resistance of Ships," by D. W. Taylor, Senior Assistant to Chief Constructor of U.S. Navy.

"On the Method of initial Condensation and Heat Waste in Steam Engine Cylinders," by Prof. R. H. Thurston, Sibley College, Cornell University, New York.

"Description of an Aluminium Torpedo Boat built for the French Government," by A. F. Yarrow.

"On Vibrations of Higher Order in Steamers, and on Torsional Vibrations," by Otto Schlick.

"On the Vibrations of Ships and Engines," by W. Mallock.

"On a Method of Extinguishing Vibrations in Marine Engines," by Mark Robinson and H. Riall Sankey, Captain (retired) R.E.

"On the Transverse Stability of Floating Vessels containing Liquids, with special Reference to Ships carrying Oil in Bulk," by W. Hök.

"Induced Draught as a Means for developing the Power of Marine Boilers," by W. A. Martin.

After the usual formal proceedings had been transacted, and the President, Lord Brassey, had given a brief address, Sir William White's paper was read by his colleague at the Admiralty, Mr. W. E. Smith, the author being absent owing to serious illness; from which, every one will be glad to hear, he appears in a fair way of recovery. This contribution dealt chiefly, indeed almost wholly, with the question of steadiness in the large battle-ships of the *Royal Sovereign* class, and the effect of fitting them with bilge keels. This paper may be said to be the complement of another memoir by the same author, read before the Institution last year, in which the experience gained with the big line of battle-ships was recorded. When the *Royal Sovereign* was designed, it was thought that the period of oscillation would approximate to that of the *Inconstant*, *Hercules*, and *Sultan*, which were all remarkably steady ships. The *Hercules* and *Sultan* had only shallow side keels from 9 to 10 inches deep. It was thought that the great inertia due to the large dimensions of the new ships would render them so steady that these small bilge keels might be omitted. That the expectations formed in this respect have not been fulfilled, shows that even the best-informed naval architects, even when supported by the best scientific evidence at their command, may be wrong at times. The performance of the ships of the *Royal Sovereign* class at sea led to the fitting of bilge keels to one of them—the *Repulse*. These keels are each about 200 feet long and 3 feet deep. Four of the class were at sea off the west coast of Scotland in company. There was a long low swell, 300 to 400 feet from crest to crest, and with a period of about ten to twelve seconds. The *Resolution*, without bilge keels, rolled 23, and the *Repulse* 10. The sea was on the quarter, and the speed such as to produce heavy rolling. The records of angle of roll were obtained by the pendulum, an instrument notoriously untrustworthy, but in the present case the results quoted were probably fairly accurate. At any rate, there was no doubt that the behaviour of the ship was immensely improved by the bilge keels, and it was decided to fit all other ships of the class in a similar manner to the *Repulse*. The consensus of opinion of naval officers is that rolling has been greatly reduced. Sir William White very frankly admits that the steadying effect of bilge keels has greatly exceeded that which was anticipated; and, indeed, the opinion of naval architects at large will now have to be readjusted, and the data upon which ship designers work will require to be modified. For scientific purposes more exact information was required than could be obtained by trials at sea, on which, as is well known, it is generally impossible to obtain accurate records. It was therefore arranged that still-water rolling experiments should be made with the sister ships *Revenge* and *Resolution*. Mr. R. E. Froude, whose father's work is so well known in this field, had charge of these trials. One series of experiments was made before bilge keels were fitted, and another after the keels had been attached. Each series was divided into two sections, the ships having a metacentric height of 3¼ feet in one case, and a little under 4 feet in the other. The conditions approximate to those of maximum and minimum stability on service. The oscillation of the ships was produced by training the heavy guns and running men across the deck in the usual way. The results of these trials were shown by means of diagrams hung on the walls of the theatre. Curves for the older ships *Sultan* and *Inconstant* were also placed on the diagram. Starting from an angle of inclination of 12° from the vertical, it was seen that in order to reduce the corresponding inclination of 6°, the *Revenge*, without bilge keels, required to make 18 to 20 swings, and the *Sultan* about 17; there being a remarkable similarity between the curves of declining angles of the two ships. The *Sultan*, as already stated, has always been considered a satisfactory vessel in regard to steadiness. Starting from an angle of inclination of 6° from the vertical, it required 45 swings in the *Revenge* without bilge keels to reduce the corresponding angle of inclination to 2°. After bilge keels were fitted, an equal reduction was obtained by only 8 swings. For the *Sultan* and *Inconstant*, 32 to 20 swings respectively would be required to produce the same reduction. The *Revenge*, before the bilge keels were fitted, could be rolled up to an angle of inclination of 13° to the vertical by moving her barbette guns. After the keels were in place it was difficult to exceed an inclination to the vertical of 6° to 8°, even with 300 to 400 men running across the deck, and acting in conjunction with the movement of the guns.

The variation in the periods of swing (from out to out) brought to light by these trials are instructive. Without bilge keels, the author continues, the rolling was practically isochronous at large as well as small angles. The period for a single swing was 7·6 seconds for maximum stiffness, and 8 seconds for minimum stiffness, for large as well as small arcs of oscillation. With bilge keels, within the range of experiment up to a swing of about 12° (6° each side), the period of a single swing decreased as the angle of inclination became smaller; the reduction being about 2½ per cent. in going from a mean inclination to the vertical of 5° to one of 1°. When the metacentric heights and radii of gyration of the ship were appreciably unchanged, there was an increase of about 5 per cent. in the period due to the action of the bilge keels.

A study of the action of bilge keels on a ship's performance not only involves questions of the greatest practical importance to the ship designer, but also problems of high scientific interest. From whichever point of view we consider Sir William White's paper, it is one of exceptional value. It is seldom that the naval architect has the opportunity of making trials or experiments on so grand a scale as that afforded by the big battle-ships of the *Royal Sovereign* class, and for this reason we are giving unusual space to this paper. Nevertheless, we are unable to follow the author in his comparison of the experiments under consideration with those previously made both by French and English engineers. For these we must refer our readers to the original memoir, confining ourselves to the main results, from which a comprehensive view of the scope of the paper may be formed. One of the most important of these results is the increased extinctive effect shown by the experiments to be produced by bilge keels when the ship has headway, compared to the effect when she is not progressing. The experiments under way were limited in number, and, as the author points out, are perhaps not so definite as those with the ship not having headway. "Still they are very suggestive," to once more quote the author's words, "and confirmatory of the conclusion from previous experience that the rate of extinction is sensibly increased by headway, the ship entering undisturbed water while oscillating, and the inertia of that undisturbed water having to be overcome." We can only quote the broad results. Starting at 5° from the vertical the angles of inclination reached, after a certain number of swings, were as follows:—After 4 swings, no headway 2·95°, at 10 knots 2·35°, at 12 knots 2·2°. After 16 swings, no headway 1·15°, 10 knots ·20°, 12 knots ·25°.

It will be remembered by students of this subject that the late Mr. Froude assigned a coefficient of resistance of 1·6 lbs.

per square foot for deadwood and keels moving at 1 foot per second, and this was confirmed by the behaviour of the *Sultan*. Accepting Mr. Froude's formula, the extinctive effect due to bilge keels, such as have been added to the *Revenge*, was calculated, with the result that, in an extreme case, supposing the ship to be rolling to 20° on each side of the vertical, the extinction value due to the bilge keels would appear to be not quite 2°. This, it will be seen, is far short of the observed results obtained from the vessels themselves. Working on the data that have been obtained in this way, new coefficients have been calculated for bilge keel resistance, it being assumed that the whole increase of work were credited to the bilge keel area. For an angle of swing of about 10° instead of the coefficient being 1·6 lbs., it would be about 11 lbs. for a swing of 4° the coefficient would reach as high a value as 15 or 16. It must be remembered that these coefficients are not put forward as truly representative, they only hold good if the assumptions stated are accurate.

In any case the difference is very great. Mr. R. E. Froude, who contributed a most interesting speech to the discussion, confessed that the results took him, when he first had them put before him, entirely by surprise, and, indeed, he did not credit the statements made as to the improved behaviour of the ships; or, rather, he could not attribute this improvement to the presence of the bilge keels. We judged, however, from his remarks that he now accepts the observed data and the truth of the recorded experimental conditions, but still considers the phenomenon one for which he can offer no adequate explanation. He himself had made tank experiments which agreed fairly well with the results obtained by his father, and was quite at a loss to account for the great difference between these experiments and the results of the trials now recorded. The only explanation he could suggest was that bilge keels, on a rolling ship, meet on the return roll with water set in motion by the previous roll; but this, he thought, was quite insufficient to account for an increase in resistance of as much as, say, ten times that which would be calculated on the 1·6 coefficient. Naval architects will be glad to hear that the whole question is to be madethe subject of exhaustive inquiry at Haslar. The principal reasons that bilge keels are not fitted—putting aside expense and difficulties as to docking—are that they add to the immersed surface, and are thus likely to decrease speed. It is, therefore, satisfactory to learn that "the practical tests of actual service prove there is no sensible reduction in speed for power." As it is also stated that the keels have not sensibly reduced diameter of circles made by the vessels, and, further, that additional steadiness in steering has been obtained, it is not hazarding much to say that in future ships of this class in the Royal Navy will all be fitted with bilge keels, unless exigencies of docking forbid their application.

The space which we have devoted to Sir William White's paper will compel us to dismiss most of the other contributions briefly. Mr. Milton's paper on steam-pipes was an excellent practical contribution, and was followed by a no less excellent discussion. The general conclusion arrived at appeared to be that, with high pressure, steel steam-pipes are likely to take the place of those of copper. Mr. Taylor's paper was read in brief abstract, and as it was not in the hands of members until a few minutes before the meeting, we must pass it by. The same thing may be said of Prof. Thurston's paper. It is very gratifying to the members of an English institution to receive papers from foreign members of such eminence as the two American gentlemen just mentioned. We regret we have not yet been able to devote the time to their contributions which their merits doubtless demand. Mr. Yarrow's paper on the aluminium torpedo boat he had built for the French Government, was a very interesting contribution. The boat appears to have been thoroughly successful, so much so that she is to be the prototype of a class. The discussion turned largely on the form of test pieces for copper alloys, it being generally conceded that there is a want of standard conditions for tests. The microsections of various alloys thrown on the screen were also very interesting.

The last day of the meeting (Friday) was devoted chiefly to the vibration question, the sitting proving one of the most instructive of the series. As will be seen, three papers were contributed on this important and interesting subject.

These three papers on vibration of steamers formed, with Sir William White's paper on steadiness, the two distinctive features of the meeting. It is hardly necessary to insist on the importance of these two features in steamship performance, both of which affect alike the comfort of the passenger in mercantile vessels, and fighting efficiency in a war vessel. Since engine power has increased so greatly and speeds have been raised, the vibration question has become one of extreme importance in passenger steamers. Two of the most recent largest and costliest of our ocean liners were almost unfitted for carrying passengers—at any rate, they were fast acquiring an uneviable reputation—on account of excessive vibration. By the application of scientific principles the cause of this defect was traced, and the evil cured; a circumstance, if measured by money value, now worth many thousands of pounds to the owners. The extremely interesting paper and series of experiments performed two or three years ago by Mr. Yarrow, at a meeting of this Institution, will be remembered by our readers; and since then Herr Schlick has read two papers on the subject. Records of these will be found in previous volumes of NATURE. The seriousness of vibration in steam vessels is largely dependent upon the period of the hull as a structure synchronising with the beats of the engines, and thus it is that a vessel may vibrate excessively at speeds less than the highest speed she can attain. That is the elementary fact upon which a study of the problem is based. Herr Schlick, in his previous papers, has already considered the case of vibrations of the first order—that is to say, such oscillations of the longitudinal axis of a ship in a vertical direction as have two nodular points. Vibrations of this order claim most attention because they are most common, and are more violent than those of higher orders. It is in vessels with engines running at high speeds that vibrations of a higher order are sometimes observed. It would, as the author of the paper points out, be very advantageous if the naval architect could ascertain beforehand the position of the nodular points of a ship in getting out the design; but this, he is of opinion, cannot be done directly in a satisfactory manner. Mr. Mallock also enters into this question, as will be seen when we deal with his paper later on. As the question cannot be treated directly in a satisfactory manner in the case of a ship, Herr Schlick has recourse to the mathematical investigations of the vibrations of an elastic, prismatical rod. Such investigations have been made by several authorities, and the author quotes at some length the formulæ that have been constructed for vibrations of the first and higher orders. These it is not necessary to repeat.

It is evident that in a complex structure like the hull of a vessel, the vibrations will be of a very different nature to those of a prismatical rod. Treating only of vibrations of the first order—for the author has not yet succeeded in correctly ascertaining coefficients for the second order—Herr Schlick finds that in a ship they are at a greater distance from the ends than in a vibrating prismatic rod; a circumstance which is explained by the fact that a ship is less weighted in the ends than in the middle. For ships of very fine lines, the only class investigated, for vibrations of the first order the distance of the after nodular point from the after perpendicular is 0·231 to 0·253 times the length of the ship. The distance of the fore modular point from the fore perpendicular varies from 0·310 to 0·365 times the length. The author had already shown that an ordinary engine with three cylinders cannot produce vibrations of the first order when the moving weights (pistons, &c.) of each cylinder are in such proportions to each other that the products obtained by multiplying these weights by the distance between the axis of the cylinder and the next nodular point are the same for all three cylinders. The same engine, therefore, will produce vibrations of the second order when the number of the revolutions increases accordingly. The new nodular point, moving away from the engine, causes the moments of the moving weights to be no longer equal to each other. The author considers, therefore, that as the nodular points can only be determined after the ship is completed, it is necessary to alter the moving weights of the engines in such a manner that their moments respecting the nodular point are made equal to each other. The vibrations will thus be considerably reduced, if not entirely avoided. The influence of the screw in producing vibration, owing to the impulses it imparts at the extreme end, is also discussed in this part of the paper; and the author then proceeds to deal with the so-called "horizontal vibrations," which he considers really consist of a twisting action on the ship's axis, due to the turning-moment of the engines, acting on a screw, in the case of a single propeller. An apparatus, constructed for the purpose, illustrated this fact by showing that the horizontal oscillations at the deck and bottom of the ship respectively are in opposite directions at the same instant of time. At a certain height above the keel there is no horizontal oscillation, this being therefore the *locus* of the axis of torsion. Maxima and minima of the turning moment at each revolution depend on the number of cranks, the amplitude of the oscillations being mostly dependent on steam distribution. These oscillations are periodic, and likewise have their nodular points. The author next proceeds to treat the points mentioned mathematically in the case of a prismatic rod. He shows that the number of vibrations is proportional to the speed of progress of vibration. Substituting a ship's body, he finds that this speed of progress remains constant for similar ships, and also that the number of torsional vibrations varies in an indirect proportion with the length of the ship. For a better understanding of these points, we must refer our readers to the original paper and the diagrams by which it is illustrated. That engines of special construction will cause no vibration if placed just above the nodular point, is also true for torsional vibrations.

Mr. Mallock, who, it may be stated, has done much excellent work for the Admiralty in connection with this subject, dealt in his paper with "the determination of the direction and magnitude of the forces and couples which arise from the unbalanced moving parts of marine engines." Something may be done, the author said, towards balancing an engine by the proper disposition of the pistons, connecting-rods, and cranks; but it does not seem practicable to produce a complete balance in any ordinary engine without having recourse to counterbalance weights. In order to determine the weights required, the author has produced a geometrical construction showing, by the aid of arithmetic only, the resultant force and couple due to the unbalanced moving parts of any engine. Without the aid of the diagrams shown at the meeting, it would be impossible to make the explanation clear, even if space permitted us to give the details in full. It will be sufficient to say that the engine is divided up into its component parts, to each of which a value is given, and in this way the resultant force is found and the resultant couple determined. Having got all the information necessary to assign the magnitude and direction of the force and couple which will completely balance the engine, if the force could be applied at the centre of gravity of the moving parts, it would merely remain to decide what weights should be used to produce the required effect. In general the construction of the engine makes this inconvenient, if not impossible, and other positions for the counterbalance weights must be found. This aspect of the problem is then considered in detail by aid of the figures.

The second part of the paper was devoted to showing how the frequency of vibration of any ship, loaded in any manner, can be found by models, and that all the data for shaping these models can be readily obtained from curves which would be in the hands of the ship designer. An example of the apparatus used was shown, the author giving a practical illustration of its working. The course pursued is to make an exact copy of the ship on a very small scale, exactly proportional in all dimensions and identical in material. It is known by theory that the frequency of vibration of the model and ship will be inversely proportional to their lengths. The model is replaced by an exact copy on the same scale, made of some other material—wood—the frequency of the new model differing from that of the former in the ratio

$$\sqrt{\frac{q_s \rho_m}{q_m \rho_s}},$$

where q_m, q_s, and ρ_s are the respective elasticities (Young's Modulus) and densities of the wood and the material of the ship. Next the wooden model is replaced by a plank of the same wood of uniform thickness but variable breadth, the breadth being such that the stiffness of the plank against bending at every cross section is proportional to the stiffness of the model at the corresponding position. Weights are fixed to the plank in such a manner that the weight at any cross section is proportional to the weight at the corresponding section of the model. Then the frequency of the plank, compared with that of the model, can be ascertained by a formula.

In the apparatus shown the plank was supported by two rollers slung from two similar rollers, the latter resting on an overhead railway. The plank was kept vibrating by a magnetic apparatus, and a recording device was added. The rollers supporting the plank gave the position of nodes. It is only when the rollers are at the positions where the nodes would be, if the plank was free from all constraint, that the frequency of the plank will be related to that of the ship as given by the author's formula. The natural nodes are found by varying the position of the rollers until the frequency is a maximum for the type of vibrations under consideration.

The method here introduced by Mr. Mallock is interesting and ingenious, but how far it is applicable to the needs of the naval architect, or whether the average ship builder, if he wish to reduce vibration, will prefer the former method of adjusting the engine to the known conditions of the ship after she is built, are questions which experience alone can decide.

The paper of Mr. Robinson and Captain Sankey dealt largely with the question of vibration in connection with electric light engines, the problem of vibration in the hull of a vessel being thus eliminated. Here again a number of diagrams were used which we cannot now reproduce, and our abstract of this paper must be therefore brief. Investigation showed that in the case of an electric light station the high speed vertical engines, each 200 indicated horse-power, with two cranks set opposite each other, and run at 350 revolutions per minute, were mounted on a large slab of concrete. The engines being vertical, the moving parts had to travel through a greater distance during the upper half of the revolution of the crank pin, than during the lower half. Calculation showed that each line of parts singly tended to lift the engine at up-stroke by about 3·5 tons, and it tended to depress it 2·3 tons. Therefore twice in a revolution a net lifting power of one ton acted upon the engine, and changed an equal number of times into a depressing power of about 1·2 tons. The result was a "pumping action" on the water-soaked soil beneath the concrete slab, and in this way vibration was conveyed to surrounding buildings. The action, it will be seen, was due to the angular movement of the connecting rods, a feature which Herr Schlick said might be neglected; a point in which the authors, naturally, and also Mr. Mallock, by no means agreed with him.

An arrangement of two engines with their framings rigidly connected, and having three cylinders each, was proposed, the object being to neutralise the endways rocking or tilting tendency, and also to give freedom from tendency to vary the downward pressure.

A discussion followed the reading of these papers.

Mr. Hök's paper described a new way of carrying out a known investigation. Whether the new way is better than the old way, is a point which may be decided by experience. The last paper of the meeting, that by Mr. Martin, was of a disappointing nature. Marine engineers have long been asking for an explanation of the hitherto unexplained fact—if fact it be—that "induced draught" is so much better for boilers than "forced draught." Mr. Martin's experiments were quite beside the mark.

The summer meeting of the Institution will be held in Paris, commencing on June 11.

QUESTIONS BEARING ON SPECIFIC STABILITY.[1]

At the suggestion of your President, I beg to submit three questions to the notice of this Society. They bear on a theoretical problem of much importance, namely, the part played in evolution by "organic stability."

The questions are especially addressed to those who have had experience in breeding, but by no means to breeders only; nor are they addressed only to entomologists, being equally appropriate to the followers of every other branch of natural history. I should be grateful for replies relating to any species of animal or plant, whether based on personal observation or referring to such observations of others as are still scattered through the wide range of periodical literature, not having yet found a place in standard works. The questions are for information on:—

(1) Instances of such strongly marked peculiarities, whether in form, in colour, or in habit, as have occasionally appeared in a single or in a few individuals among a brood; but no record is wanted of monstrosities, or of such other characteristics as are clearly inconsistent with health and vigour.

[1] A paper read at the Entomological Society, April 3, 1895, by Francis Galton, F.R.S.