SUMMER MEETINGS OF THE SEVENTY-FIFTH SESSION
AND INTERNATIONAL CONFERENCE ON EXPERIMENT TANK WORK

OF THE

INSTITUTION OF NAVAL ARCHITECTS,

LONDON, JULY 10 TO 13, 1934.

SUMMER MEETINGS IN LONDON.

PROGRAMME.

TUESDAY, JULY 10.

OPENING of the Meetings by the Right Hon. LORD STONEHAVEN, P.C., G.C.M.G., D.S.O., LL.D., in the Lecture Hall of the Royal Society of Arts, John Street, Adelphi, London.

Presentation of William Froude portrait (bronze plaque) to the Representatives of British and Foreign Experiment Tanks.

PAPERS.—1. *William Froude*, by Sir WESTCOTT S. ABELL, K.B.E., M.Eng., Vice-President (Durham University).

2. *Influence of Form on Frictional Resistance*, by General G. ROTA, R.I.N., Member (Rome National Tank).

3. *Some Notes on the Nomenclature Suitable for the Presentation of Model Data*, by Professor T. B. ABELL, O.B.E., M.Eng., Vice-President (Liverpool University).

Afternoon.—Trip on the Lower Thames to visit the Port of London Docks.

Evening.—Reception given by H.M. Government at Lancaster House.

WEDNESDAY, JULY 11.

PAPERS.—4. *Experimental Investigations on the Resistance of Long Planks and Ships*, by Vice-Admiral Y. HIRAGA, I.J.N. (ret.) (Tokyo University).

5. *The Influence of Viscosity on Thrust and Torque of a Propeller Working near the Surface*, by Dr. G. KEMPF, Member (Hamburg Tank).

6. *Skin Friction Correction*, by Professor L. BAIRSTOW, C.B.E., F.R.S. (Imperial College of Science and Technology, London).

7. *The Design of Screw Propellers, with Special Reference to the Single-screw Ship*, by G. S. BAKER, Esq., O.B.E. (Member of Council) (William Froude Laboratory).

8. *Propeller Cavitation Studies*, by Lieut.-Comm. C. O. KELL (C.C.), U.S.N. (Experimental Model Basin, Washington, D.C.).

9. *The Effect of Inclination, Immersion, and Scale on Propellers in Open Water*, by Dr. Ing. RENATO DE SANTIS, Associate (Rome National Tank).

Afternoon.—Visit to the Royal Naval College, Greenwich.

Evening.—Banquet at the Grosvenor House Hotel, Park Lane.

THURSDAY, JULY 12.

PAPERS.—10. *Model Experiments of the Combined Effect of Aft-body Forms and Propeller Revolutions upon the Propulsive Economy of Single-screw Ships*, by M. YAMAGATA, Esq. (Teishinsho Tank, Tokyo).

11. *Ship Performance in Relation to Tank Results*, by M. P. PAYNE, Esq., R.C.N.C., Member (Admiralty Experiment Tank, Haslar).

12. *On the Theory of Double Systems of Rolling of Ships Among Waves*, by Monsieur E. G. BARRILLON (Ingénieur Général du Génie Maritime, Paris).

13. *Wave Patterns and Wave Resistance*, by Professor T. H. HAVELOCK, M.A., D.Sc., F.R.S. (Durham University).

14. *Trials of the Training Ship "Cristoforo Colombo" with Two Co-axial Contrary-turning Screws*, by Colonel F. ROTUNDI, R.I.N. (del Genio Navale).

Afternoon.—Visit to the National Physical Laboratory (Teddington), William Froude Experiment Tank, etc.

Evening.—Supper Dance and Cabaret at the Dorchester Hotel, Park Lane.

FRIDAY, JULY 13.

All-day excursion to Southampton and Cowes.

Visit to Messrs. J. Samuel White & Co.'s Works, Cowes.

NAMES OF DELEGATES AND REPRESENTATIVES OF EXPERIMENT TANKS.

Austria.

Representative of the Vienna Tank:
Dr. Ing. F. Gebers.

France.

Delegates:
Professor E. G. Barrillon.
Monsieur H. J. Guntzberger.

Representatives of the Association Technique Maritime et Aeronautique:
Monsieur Emmanuel Rousseau (President).
Monsieur G. Bourges (Secretary).

Germany.

Representatives of the Hamburg Tank:
Dr. G. Kempf.
Herr H. Lerbs.

Representative of the Berlin Tank:
Dr. Ing. H. M. Weitbreckt.

Other Representatives:
Dr. Ing. E. Foerster.
Dr. Ing. Fr. Horn.

Holland.

Delegate:
Ing. L. Troost (Wageningen Tank).

Other Representative:
Baron van Haersolte.

Italy.

Delegates:
General G. Rota, R.I.N. (Rome National Tank).
Lieut.-Col. G. N. Figari (Royal Italian Navy).

Other Representatives:
Dr. Ing. Renato de Santis (Rome National Tank).
Dr. Ing. E. Castagneto (Rome National Tank).

JAPAN.

Delegates:

Professor Vice-Admiral Y. HIRAGA, I.J.N. (ret.) (Tokyo University and the Mitsubishi Tank).

Constructor Lieut.-Comm. A. IIKAWA, I.J.N. (Assistant Naval Attaché).

M. YAMAGATA (Department of Communications and Teishinsho Tank, Tokyo).

NORWAY.

Representative of the Trondhjem Tank:

Professor H. R. MØRCH.

SPAIN.

Delegates:

Capt. Constr. D. N. DE OCHOA (Madrid Tank).

Lieut. Constr. CARLOS LAGO (Madrid Tank).

SWEDEN.

Representative of Stockholm Tank:

Dr. H. F. NORDSTRÖM.

UNITED STATES OF AMERICA.

Delegate:

Lieut.-Comm. C. O. KELL (C.C.), U.S.N.

Other Representative:

Captain H. S. HOWARD (C.C.), U.S.N.

BRITISH TANK REPRESENTATIVES.

Admiralty Tank, Haslar:

Mr. M. P. PAYNE, R.C.N.C.

William Froude Laboratory of the National Physical Laboratory:

Mr. G. S. BAKER, O.B.E.

Mr. J. L. KENT.

Messrs. John Brown & Co. (Clydebank):

Mr. J. M. MCNEILL, B.Sc.

Messrs. William Denny & Brothers (Dumbarton):

Mr. J. F. ALLAN, B.Sc.

Messrs. Vickers-Armstrongs (St. Albans):

Mr. H. J. MUNDEY.

INTRODUCTORY PROCEEDINGS, TUESDAY, JULY 10, 1934.

The following gentlemen, having been duly recommended by the Council, were unanimously elected:—

Members (23).

AMOUR, JAMES CHARLES, Manager of the New Engineering & Shipbuilding Works, *Shanghai.*
*BURNETT, ALEXANDER, Naval Architect, P. Smit, Jr., Shipbuilding & Engineering Works, *Rotterdam.*
*GOULD, ALFRED JOHN, M.Sc., Ph.D., Professor of Engineering, *University of Rangoon.*
HANLON, MICHAEL JOSEPH, Vice-President of the Black Diamond S.S. Co., *New York.*
*HAYWARD, CHARLES HEMBRY, Superintendent Engineer, British West Indies, and Honorary Corresponding Member, I.N.A.
IIKAWA, A., Constructor Lieut.-Comm., I.J.N., Imperial Japanese Navy, Broadway, *Westminster.*
*KARI, ALEXANDER, M.Sc., Naval Architect, Managing Director of the Unislip Propeller Co., *Newcastle-on-Tyne.*
*LAING, WILLIAM PATRICK, Ship Surveyor to the British Corporation Register of Shipping, *Glasgow.*
LAJOUS, RAOUL E., D.Sc., Chief Constructor, Argentine Navy Department, *Buenos Aires.*
LÉVY, ANDRÉ, Managing Director, Chantiers et Ateliers de Saint-Nazaire (Penhoët), *France.*
MANERA, EDMUNDO, D.Sc., Inspector of Works, Navy Department, *Buenos Aires.*
MAZZOLI, JULIO, D.Sc., Chief of Hull Department & Dry Docks, Arsenal Puerto, *Belgrano. Argentina.*
NAVARRO, HECTOR, D.Sc., Director of Naval Works, Arsenal Puerto, *Belgrano, Argentina.*
PIERROTTET, ERNESTO, Professor, Royal School of Naval Architecture, *Genoa.*
PLUYMERT, NICOLAAS JACOBUS, Director, Standard Vacuum Transportation, *New York.*
*RICHARDS, WILLIAM MILDON, Consulting Engineer, *Càrdiff.*
ROMANO, PAUL, Engineer-in Chief, Compagnie Générale Transatlantique, *Paris.*
ROSSELL, HENRY EASTIN, M.Sc., Professor of Naval Construction, Massachusetts Institute of Technology, *Boston, Mass., U.S.A.*
*SHEPHERD, REGINALD JOHN, Ship Surveyor to the Board of Trade, *Newcastle-on-Tyne.*
*SMART, WILLIAM, Partner in the firm of Messrs. G. L. Watson & Co., *Glasgow.*
*SMITH, EBENEZER, B.Sc., Naval Architect, Hooghly Docking & Eng. Co., *Calcutta.*
TAYLOR, ANDREW ALLAN MACK, Naval Architect, Union Steam Ship Co., *Wellington, New Zealand.*
*YARROW, NORMAN ALFRED, Managing Director of Messrs. Yarrows, *Victoria, B.C.*

Associate-Members (2).

HARDING, RICHARD, B.Sc., Messrs. Swan, Hunter & Wigham Richardson, *Walker-on-Tyne.*
†DE SANTIS, RENATO, Dr. Ing., The National Experiment Tank, *Rome.*

* Transferred from Associate-Member Class. † Transferred from Associate Class.

Associates (12).

BARAKOVSKY, VALENTIN, Ir.
DE BLASI, FERNANDO, Lieut.-Col., R.I.N.
BUNSTER, MANUEL LARRAIN
ELENBAAS, M. J.
FERDINANDS, CHARLES WALTER VANDEN DRIESEN
HANSEN, WILLIAM
JAMES, FRANK
JOHNSTON, WILLIAM DAVIDSON
MALGLAIVE, PIERRE DE, B.A.
OATES, JOHN GERALD BEVERIDGE
ROBBINS, SYDNEY
WILSON, JAMES MUDIE

Students (4).

ABELL, THOMAS WESTCOTT DAVENPORT
COOK, JOHN
LIVINGSTONE, MURDOCH
SPANNER, WILLIAM FRANK

PRESIDENT'S ADDRESS.

The PRESIDENT: Ladies and Gentlemen, I do not propose, in opening these Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, to give you anything in the nature of a Presidential Address, for you will have, I feel sure, a sufficiently crowded time in which to read and discuss the important papers on ship resistance and propulsion which are to be presented. But I would like in the first place to offer on behalf of our Institution a most hearty welcome to all those delegates from abroad who have done us the honour to attend this International Conference. Many of them have come long distances—from Japan and the United States; from Scandinavia and the European capitals—to represent their country's activities in the special field of naval architecture which we are assembled to explore and investigate on this occasion.

Many men of many nationalities have helped to elucidate those intriguing and often baffling problems which confront and sometimes perplex the ship designer, but there is one name which stands out above all others in this connection—a name which should be associated for all time with this kind of research. I refer to the late William Froude, originator and pioneer of the Experiment Tank method of research. Froude by his genius and perseverance was able to establish, in the face of the sceptics and unbelievers, the eternal truth that science must lead where engineering is to follow. He was able to convince the Board of Admiralty of his day that the little model hulls he was making and testing in his small Tank or Basin were not mere toys, and could in the hands of a competent staff be trusted to give results which might be relied on, and thus save the large expenditure which experiments on full-sized ships would entail. He was one of those modest and self-effacing men who manage to achieve by brilliance of intellect and strength of character what others less gifted would fail to accomplish. That so much of his work was done without expectation of profit or reward—indeed, the cost of his early experiments made deep inroads into his private means—must remain as a memorial to his greatness of character. Sir Westcott Abell will tell you in the opening paper this morning more of Froude's life and work, and of its influence on naval architecture, not merely in his own time but for the future, and you will, I feel sure, wish me to place on record at the very outset of these meetings an expression of our deep regard and admiration for William Froude's life and work.

With so many countries represented here to-day, it would be invidious to make comparisons, but all have made their contributions to this branch of naval architecture. Long before we in England had attempted to reduce the shipbuilding problem to an exact science, French ships were noted for the excellence of their design and construction, due to advanced theoretical studies and their application to research.

The early efforts of those who founded our Institution in 1860 were largely directed to laying down an educational system based on what had long been current in France, and the School of Naval Architecture at South Kensington—subsequently transferred to Greenwich—was the result. But the testing of ship models had not been one of the subjects of research, and the Government Model Basin near Paris did not materialize until many years had elapsed after Froude first convinced the Admiralty authorities of the soundness of his methods.

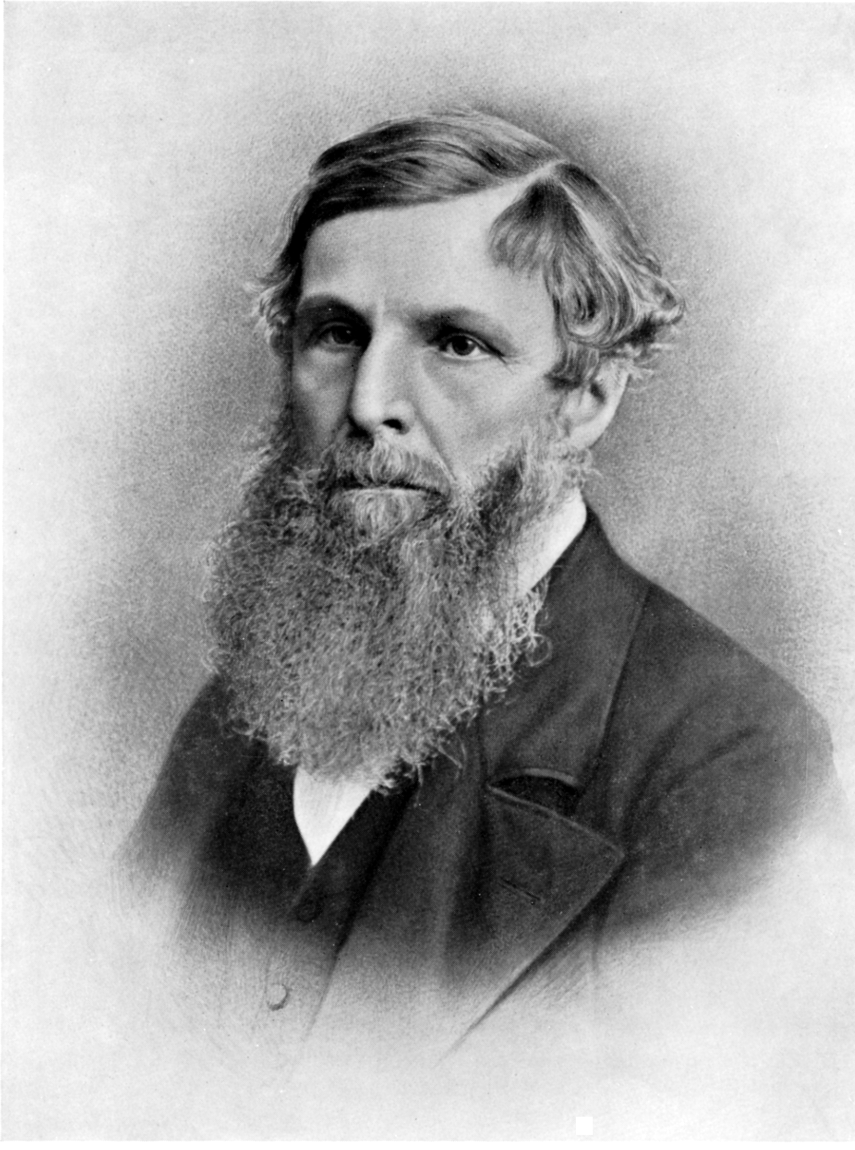
William Froude's early Tank at Torquay was opened in 1871, and was the only one in existence for some years. But the late William Denny had realized the possibilities attaching to the work, and he felt the importance of the results to be obtained by this method. His firm was the first of the private firms to follow Froude's example, and they built a Tank at Dumbarton, which was opened in 1883. So far as we know, they have had no cause to regret their enterprise. The Naval Tank at Haslar, commenced in 1884, came next, and was placed under the supervision of his son, R. E. Froude. This was followed by experiment Tanks in Italy (Spezia, 1889), St. Petersburg, Ubigau (Dresden), Washington (1898). At the latter establishment wooden models instead of paraffin ones were used on account of the higher summer temperature. In its day this Tank was the largest in existence, but has now been surpassed in size by others.

The new century saw a fresh outburst of Tank activity. Germany built one at Bremerhaven in 1900, and one at their Technical High School, Charlottenburg, in 1902. The French Government established one near Paris in 1905, and in the same year Michigan University, U.S.A., followed suit with a large Tank at Ann Arbor. In 1908 the Clydebank Tank (Messrs. John Brown & Co.) was built, where models of our largest and fastest Atlantic liners have been made and tested. The Japanese established one at Nagasaki in 1908, and another at Tokyo two years later. Then came our William Froude Tank, presented by Sir Alfred Yarrow to the National Physical Laboratory. This was opened, appropriately enough, in the Jubilee year of our Institution (1911), and was the last Tank to be completed before the Great War. The Norddeutscher Lloyd laid down a very large Tank at Hamburg in 1913 and this was completed in 1915. The Vienna Tank, which has produced much valuable material under the able guidance of Dr. Gebers, was completed in 1916, but after the war was over there came a lull in new Tank construction. Eight years later, however, the Italian Government decided that their Spezia Tank was insufficient for their requirements, and they laid down a large Tank on the outskirts of Rome, which our members had the opportunity of visiting just before its formal opening, when the Institution paid a memorable visit to Italy (1929). General Rota, one of our most distinguished members, of nearly forty years' standing, is in charge of this Tank. The latest additions are the new large Admiralty Tank at Haslar, the Wageningen Tank in Holland, opened last year, and the Spanish Government Tank in Madrid, which is shortly to be completed. This, briefly, is a list of the principal Tanks of the world, in their chronological order, and it speaks for itself as showing the immense importance attached by all maritime nations to the real value of ship-model experiments when carried out under scientific direction.

Addressing you, as I do, as a layman, I would like to urge that the spirit of friendly intercourse should prevail at these Meetings now as in the past, and I would add: let no one refrain from addressing his or her neighbour for want of a formal introduction!

I will now proceed to present to the representatives of these Tanks the copies of William Froude's portrait in bronze plaques which have been prepared for this occasion, and which we hope will serve to keep ever fresh in the minds of shipbuilders the debt they owe to that truly great man.

The bronze plaques were then presented to the Representatives of the various Tanks.



WILLIAM FROUDE, F.R.S., LL.D.

PIONEER OF SHIP MODEL RESEARCH

1810–1879

WILLIAM FROUDE.

By Sir Westcott Abell, K.B.E., M.Eng., Vice-President.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 10, 1934, the Right Hon. Lord Stonehaven, P.C., G.C.M.G., D.S.O., President, in the Chair.]

William Froude came from an old Devonshire family. His father, the Venerable Robert Hurrell Froude, was Rector of Dartington, on the river Dart, when William, the sixth child, was born in 1810.

Three of the five sons made their mark on history. The eldest, Richard Hurrell, was one of the leaders of the Oxford Movement, of which Dean Church said: "Keble had given the inspiration, Froude the impetus; then Newman (afterwards Cardinal Newman) took up the work."

The youngest, James Anthony, was the famous historian, and his writings contain references to the family life. He describes their father as a High Churchman who regarded the Church as part of the Constitution and the Prayer Book as an Act of Parliament. He was Archdeacon of Totnes and a Justice of the Peace: most of the magistrates' work of the neighbourhood passed through his hands. His advice was most sought after, and he was once asked to lay a troublesome ghost. His children knew him as a continually busy, useful man of the world, a learned and cultivated antiquary, and an accomplished artist.

His mother, Margaret Spedding, belonged to a Cumberland family the members of which had been distinguished for their scholarship and had had for some generations a strong turn for science, especially mechanical science.

After receiving his early education in Devonshire, William Froude went to Westminster School from which, in 1828, he proceeded to Oriel College where his eldest brother, Richard Hurrell, was a tutor. He was still at Oriel in 1835 and is described as being the chemist as well as the mechanist of the college. He began his professional career as a Civil Engineer, and about 1838 was engaged on the construction of the Bristol and Exeter Railway under Isambard Kingdom Brunel, who was responsible largely for building the steamships *Great Western* (1835), *Great Britain* (1838), and *Great Eastern* (begun in 1851). This association with Brunel encouraged Froude to proceed with his studies in Naval Architecture and led to his investigations into the rolling of ships.

William Froude was always making experiments with models of screw propellers, certainly from 1850 if not before. He came to live at Paignton in 1859, where the idea of an experimental Tank first occurred to him. He bought some land at Cockington, Torquay, on which he built a house known as Chelston Cross, completed in 1867, and there the first Tank was constructed.

His first paper on the rolling of ships, read before the Institution of Naval Architects in 1861, brought him into touch with the leading scientists of the time and the Chief Constructor of the Navy, Sir Edward Reed. His contributions to the Institution are voluminous, beginning, as already stated, in 1861 and lasting right up to the time of his death in 1879.

Apart from his experimental skill Froude had a wide knowledge of mathematics, as his study of the rolling and oscillation of ships indicated. He had a clear knowledge of wave mechanism, and his original explanation of the performance of a propeller is as satisfactory as anything subsequently devised.

His greatest achievement was the application of model experiments to determine the resistance of ships. At Sir Edward Reed's suggestion he sent him an account of certain of his model experiments in April 1868, which was followed in December of that year by a full account of his proposals.

The Admiralty gave their first qualified approval of his suggested experiments in 1870, but it was not until 1872, when Froude was already 62 years old, that his first Tank was finished. It was during the last seven years of his life that he established the truth of his search, although not enjoying the best of health. For that reason he went on a voyage to the Cape, where he became ill, and died at Simons Town, where he was buried in 1879.

Rear-Admiral D. W. Taylor, of the United States Navy, his distinguished disciple, has remarked that "Froude was far ahead of his time, not only as a pioneer of the rolling of ships and propulsion and resistance but as a genius, who with a model Tank which was very crude compared with those of to-day, established methods and quantitative coefficients which served the naval architects for some 50 years." Taylor continues: "We know now that Froude's coefficients can be improved upon, but for practical purposes improvements have been astonishingly small."

Sir Arthur Johns, last year, found in the records of the Admiralty the official letter of February 1870, giving approval to Froude's suggested experimental Tank, as well as Froude's earlier observations and suggestions on the method of experiments which he had submitted to the Admiralty in December 1868. This reasoned statement of Froude's is so excellent in its conception of the problem, and such a beautiful example of exposition, that it still forms the best introduction to the study of ship resistance. It is given in the Appendix. It may be mentioned that the reports of the British Association for 1869 contain a further statement of Froude's conclusions.

The text of the Admiralty letter is interesting as indicating that much was expected for the sum of £2,000, which was actually spent before the principal experiments had been properly begun:—

Admiralty,
1st February, 1870.

Sir,

I am commanded by my Lords Commissioners of the Admiralty to inform you that after a full consideration of your proposals respecting *Experiments upon the resistances of Ships*, my Lords have been pleased to approve of the same. It is the desire of my Lords that these Experiments should be carried out in the manner proposed by yourself, and in accordance with a detailed Scheme which they have directed Dr. Woolley and the Chief Constructor of the Navy to draw up in concert with yourself and to submit for the approval of my Lords. They will require that the Experiments shall be at all times during their progress open to the examination of either or both of those Officers, who have been directed to report upon them from time to time as may seem to them requisite or as my Lords may direct.

My Lords are also desirous of having carried out by yourself and during the progress of the Experiments upon resistance a few Experiments *on the rolling of floating bodies*, and especially upon *the rolling bodies of ellipsoidal form* which have been suggested by the Reverend Canon Moseley.

My Lords desire that you should distinctly understand that the total outlay which they can sanction upon these Experiments is a sum of £2,000, of which an expenditure of £1,000 only will be provided for during the financial year next ensuing ending with March 1871 and the remainder in the following financial year.

It is the particular desire of My Lords that you should so provide the means of making the proposed experiments and so conduct them, that no claim or application of any kind may be made to them hereafter for any sums in excess of the above named amounts, observing that they will not be responsible in any degree for any such excess under any circumstances whatever.

You will understand that the experiments on rolling are only to be carried out on the understanding that their cost will be included in the sum stated above.

You are requested to place yourself in early communication with Doctor Woolley and the Chief Constructor, but no actual Expenses should be incurred until the Vote for Experiments which my Lords have caused to be inserted in the Navy Estimates has received the sanction of Parliament.

W. FROUDE ESQRE.

Apart from his scientific contributions to naval architecture he took a great interest in the general advancement of science. His wide outlook and outstanding character are shown by the evidence which he gave to the Royal Commission on Scientific Instruction in 1872. Two brief extracts from that evidence may be given as illustrating Froude's views on both the general question of the application of science to industry and his firm belief in the value of model experiment and research.

"One of the functions of the Government is to assist in promoting the advancement of science. It seems to me an object of national credit and national importance that scientific knowledge in this country should be advanced in both those departments to the highest degree, and this would be of immense pecuniary value."

"I think that any experiment almost on the sailing or rolling properties of a big ship, when tried in a big ship to begin with, is a waste of money. The cost of construction of a big ship as an instrument of investigation is enormous; and if it is tried with a view to the application of the new principle, there must be the risk that the experiment will be to some extent wasted. Being an experiment, the very fact that it is an experiment implies that it may not turn out as it is expected, and a failure in so costly a piece of apparatus as a new complete ship is inevitably a very costly failure. So far as it is possible to arrive at a proper understanding of such subjects by small scale trials, it is of the utmost importance, economically, that that method should be adopted, and I think that that has not been sufficiently adopted."

If it be possible to specify some of the qualities of the genius which laid the scientific foundations of the motion of ships at sea some fifty years ago, it may perhaps be said that Froude combined in just proportions character, science, and practice. His hands were as skilful as his brain was active, and the simple means by which he produced the most delicate results call for the highest admiration. In himself, he showed a rare degree of modesty and disregard of self, while his sympathetic heart was reflected by an almost pathetic tone in his speech. He possessed a determination to see things as they were and a certain positive tendency which made his illuminations of the problem strikingly clear.

Perhaps, however, one of the best tributes to the man himself is that given by his brother, James Anthony Froude, in a letter to Lady Derby, which runs as follows:—

"My brother, though his name was little before the public, was well known to the Admiralty and, indeed, in every dockyard in Europe. He has contributed more than any man of his time to the scientific understanding of ships and shipbuilding. His inner life was still more remarkable. He resisted the influence of Newman when all the rest of his family gave way, refusing to become a Catholic when they went over, and keeping steadily to his own honest convictions. To me he was ever the most affectionate of friends. The earliest recollections of my life are bound up with him, and his death takes away a large part of the little interest which remained to me in this most uninteresting world. The loss to the Admiralty for the special work in which he was engaged will be almost irreparable."

APPENDIX.

Observations and Suggestions on the Subject of Determining by Experiment the Resistance of Ships.

(W. Froude, December 1868).

My communication addressed to the Chief Constructor of the Navy dated 24th of April 1868 contained a lengthened report of a series of experiments made by me on the resistance at various velocities of certain models.

The results of those experiments, independently of any inference that might be founded on them favourable to the adoption of any particular forms in Naval Architecture may I think be regarded as having established the following propositions.

(i) Experiments duly conducted on a small scale will give results truly indicative of the performance of full size ships.

(ii) There are very important defects and inaccuracies in the received views concerning the resistances of different forms.

The second of these propositions shows the existence of a state of things the ill effects of which considering the magnitude of the interests involved, it is not easy to overrate; while the first indicates the applicability of what appears to me to be the only remedy, namely a system of small scale experiments; though such experiments have hitherto been considered barren and misleading.

Object of this communication.

It is my purpose in this communication to illustrate these considerations more fully, as well as to carry them to a practical result.

Outline of Argument.

In pursuance of this object.

(I.) Firstly, I shall place the above mentioned propositions and the grounds on which they rest in a more precise and apposite form.

(II.) Secondly, I shall point out the defects of full size trials as a means of investigation, contrasted with the effectiveness for that purpose of small scale experiments;

(III.) Thirdly, I shall describe that method of making such experiments which my experience recommends as the best;

(IV.) and Fourthly, I shall suggest a practical scheme by which such a system may be put into effect.

I. (Propositions established by former experiments.)

In accordance with the above design, I begin by drawing out afresh the two propositions above mentioned.

(i) Experiments duly conducted on a small scale will give results truly indicative of the performance of full size ships.

In the former communication this proposition was shewn to be probably correct; but it may be well to recur to it here with especial reference to two objections which are commonly made to it.

The large waves seen to be made by models commonly urged as an objection to the validity of small scale experiments.

(a) It is observed that models, when towed through the water make proportionately much larger waves than full size ships are ever seen to do, and under the impressions derived from this circumstance it is asserted that any deductions based on such experiments are misleading since it is certain that wave-genesis must form an important element of the whole resistance.

In answer to this objection I venture to assert that there is prima facie reason to suppose that full size ships would create waves of quite as formidable appearance if driven at corresponding velocities.

What is meant by "corresponding velocities" may be better understood by referring to my former communication,* where it was argued from the principles of wave motion that similar models of whatever dimensions, or what is more pertinent here, a model and a ship similar to it, would make similarly dimensioned waves when their relative velocities were as the square roots of their respective dimensions. And it was further suggested that under this condition the proportion between the resistance due to wave-genesis, and that due to other sources would be the same for both ship and model.

* form: comm: VII3 p. 20.

Principle of comparison of the performances of a model and a similar ship enunciated.

To express the conclusion from this in a simple form, the diagram which exhibits to scale the resistance of a model at various successive velocities, will express equally the resistance of a ship similar to it, but of (n) times the dimension, at various successive velocities, if in applying the diagram to the case of the ship we interpret all the velocities as $(\sqrt{n})$ times, and the corresponding resistances as (n^3) times as great as on the diagram.*

This principle is borne out by my experiments.

The results obtained in my experiments, with the variously dimensioned, similar models when severally reduced to diagrams on this hypothesis exhibit such precise resemblance † as practically to justify its adoption.

The hypothesis admits, however of definite proof, and this shall be given, but as the proof is somewhat lengthy and may be regarded as superfluous it is placed in an Appendix.‡

The "rigidity" of water commonly urged as an objection to the validity of small scale experiments.

(b) The second objection is founded upon what Professor Rankine has termed the "rigidity" of water.§

Observing that this property in any substance (such as tar or semi-fluid earth) which possesses it largely exhibits more highly marked effects when the substance is dealt with in small volumes than when in large, it is inferred that in such degree as it may exist in water, it will form an element in the resistance of a model which has no commensurable counterpart in that of a full size ship.

The degree of weight to be attached to this objection depends on whether the dimension of the model dealt with exceeds the limits of dimension within which the operation of the property, as it exists in water is practically felt.

This may be pretty fairly estimated by observing within what limits of dimension the behaviour of waves is affected by it.

In fact it is noticeable that while capillary waves are obliterated almost immediately that the originating impulse ceases to act, exhibiting in its greatest intensity the action of the property we are considering, yet waves of a few inches in length though they die out in a short distance, travel so far as to indicate that the relative intensity of its action is materially less, and waves of two or three feet in length run to such considerable distances without any apparent diminution as to indicate that they are practically exempt from it. It will be fair then to conclude that models of, say, 6 feet long and upwards will be to all intents and purposes unimpeded by such "rigidity" as exists in water.

Models 6 feet long large enough not to be within range of the operation of the property of "rigidity" of water.

Moreover while it was found in my experiments that on comparing the performances of a 3 foot and 6 foot similar model, the former exhibited a slight excess of proportionate resistance possibly attributable to the above cause; between the 6 foot and 12 foot models no such difference was observable.

Lastly this is worth notice. Calculations based on a large number of indicator diagrams taken from the largest well-proportioned vessels of the wave-line type, at such speeds as not to create notable waves, have satisfied me that we may regard from 23 lb. to 24 lb. per square foot of midship section as the resistance of such vessels at 10 knots, this figure, reduced by the rule of the squares of the velocities, which my experiments shew to be fairly correct below such speeds as create notable waves, would give ·74 lb. to ·78 lb. per foot as the resistance due to 1·8 knots. Now, at that speed, the 6 foot wave-line model makes, according to my experiments a resistance of just ·75 lb per square foot. Whereas if a 6 foot model were considerably affected by the action referred to, its resistance should be markedly greater.

It appears then that so far from the foregoing objections having any real foundation, we are able, in fact, to frame from the results of small scale experiments by a precise and simple method an accurate prediction of the performance of full size ships.

(ii) The existing state of knowledge as to the merits of particular forms in ships is very unsatisfactory.

Erroneous opinion concerning the resistance of forms of the "wave-line" type.

(a) It is generally held that Vessels of what is called the "wave-line" type offer the least resistance, and there is nothing either in the reasonings or the statements of its advocates to limit its superiority to the condition of comparatively moderate velocity.

Now, if it is allowed as I have contended that the performance of such models as were used in my experiments is precisely similar to that of similar ships, it is clear from those experiments,||

* form: comm: p. 21.
† Diagrams B, C & D form: comm:
‡ Not reproduced here.
§ form: comm: p. 27.
|| form: comm: Diagrams A, B, C & D.

that at high speeds the waveline form is markedly inferior to a form* totally dissimilar to it in appearance and utterly opposed to it in principle.

Misconception of the modus operandi of Surface friction.

(b) An opinion is beginning to gain ground and with good reason, that surface friction forms a large share of the resistance of a ship, yet concerning the nature of this force much misconception plainly exists; for by the best Authorities who have treated of the subject it has been held as the basis of calculation that a long plane surface of say one foot in width moving endways through the water encounters the same resistance of friction for each square foot throughout its length. Now it is quite clear that the first square foot in its length in experiencing frictional resistance from the particles passing it, and thus impressing force on them in return must communicate to them some velocity in the direction of its motion. The particles in contact with the second square foot having thus already received forward velocity must exert less frictional resistance upon it, unless the degree of that frictional resistance be independent of the degree of velocity with which the particles traverse the surface; a supposition manifestly erroneous, and quite as subversive of received opinions as the alternative conclusion, I advance that surface friction is not uniform for every part of the surface.

Defective state of knowledge concerning due ratio of length to breadth and to depth.

(c) In the existing state of knowledge there is no guide for determining the influence upon a ship's resistance of the ratio of her length to her breadth and to her depth; a question of the most vital importance in the construction of ships of war, where by increasing the length though we no doubt increase the carrying power, we increase also in a greater degree the weightiest and costliest parts of the structure, adding at the same time to the difficulty of handling the ship; whereas by increasing the breadth we can add almost as largely as we please to the carrying power with an increase only to the less weighty and less costly parts of the structure, leaving the handiness unimpaired.

Defective state of knowledge concerning limiting speeds for different forms.

(d) Though there are current very precise rules by which it is said we can determine the limit of speed beyond which a vessel of given length cannot economically be driven, these rules have been fixed by an (as it appears to me) arbitrary and not strictly relevant application of the theory of wave-motion. Moreover, the rule implies that the limit will be at the same speed for all ships of the same length, but this according to my experiments is obviously not the case, for instance in the experiments on the two 6 foot models† the same in length, breadth and displacement, one has the limit at a speed fully 8 per cent. higher than the other, making at that speed 20 per cent. the less resistance of the two though at the lower speeds it had made the greater resistance.

I have here enumerated certain deficiencies and inaccuracies in the received views on the subject. These alone are manifestly of very high importance, moreover we may reasonably expect that an extension of the inquiry will discover many other deficiencies and errors equally serious.

II.
(Full size trials compared with small scale experiments.)

It needs no argument to show that correct and complete knowledge on these points is of even national importance.

Nevertheless, though with this view careful trials of ships have for some years been vigorously conducted yet our knowledge appears to be still in the unsatisfactory state I have been describing.

Full size trials imperfect.

Nor can we wonder at this, if we consider the defects inherent in any system of full size trials regarded as a mode of experimental inquiry.

Full size trials limited in number

In the first place the information gained is sparse, because to construct mere experimental forms of the size of large ships (ship sized models in fact) would be so costly that we are restricted to experimenting on those ships which from time to time are built for actual service, the costliness, too, of each individual experiment hinders us from so varying the trials as to make the most of even this scanty material. At best so disjointed a series of results as can be thus obtained furnishes a most imperfect basis on which to construct any kind of law.

and limited in range,

It is limited in range, because the risk of signal failure incurred in deviating widely from the beaten track is too serious to be hazarded in the design of a ship intended for actual service.

also indecisive

It is moreover indecisive, because the information furnished by each individual experiment includes in one result with the resistance of the ship, the efficiency of the engines, and the efficiency of the mode of propulsion, and its analysis is complicated by the uncertainties due to the state of the weather, the trim and other indeterminate conditions incidental to such trials.

and slow.

Lastly, it is tardy, because not only is considerable time required for the preparation and

* form: comm: p. 15.

† form: comm: diagram C.

completion of any one experiment on so large a scale, but before it is possible to take a single step in advance along any new line of thought which may have been suggested by the experience already acquired, it is necessary to wait during the time occupied in the design and construction of a new ship.

It has already been shewn that the results of small scale experiments are capable of a direct and accurate application to the case of full size ships and I will now shew that a system of such experiments is a mode of inquiry at once exhaustive, rapid and economical thus furnishing a marked contrast to what is in fact the only alternative, a system of full size trials.

Results of small scale experiments complete.

Not only does the facility of manipulation inherent in mere smallness of dimension allow of the experiments being many, but the models being cheap of construction and specially adapted to the purpose may be multiplied and varied to an almost indefinite extent, so as to direct the inquiry to any particular question that may be opened up, and to complete all the links in each chain of results from which any individual law we are in search of is to be deduced.

Results of small scale experiments definite and precise.

Again we are not perplexed by a multitude of collateral functions such as in full size trials as already has been observed must intrude themselves into the result; which result in small scale trials on the contrary is an exact and unmixed record of the resistance experienced by the form, disembarrassed from the questions of efficiency of Engine power or of propulsive agency.* The resistance moreover may be in itself freed from the adventitious influences of wind and rough waters etc., as the whole experiment may be carried on under cover.

Process of investigation by small scale experiments rapid.

The experiments being under cover may be carried on in all weathers, the advantages of the rapidity thus insured to this system are too obvious to need pointing out.

III.

(Description of best method of making small scale experiments.)

Having thus demonstrated on general grounds the superiority of small scale experiments I will next explain what sort of arrangement my experience leads me to recommend as the best.

The water space must be devoted to the special purpose.

The water space in which the experiments are to be tried should be devoted to the special purpose, so that it may be secured free from currents, as well as from the hindrances which may sometimes occur from any other uses to which the water might be put and that it may be maintained at a uniform level, and free from scum, weed etc. Further it is highly important that the water space should be housed in that the experiments may not be stopped by the weather.

Necessary dimensions of waterway.

The waterway must be deep enough and broad enough not to affect the resistance of the model passing along it by constriction of the fluid and must be long enough to give the model length of run sufficient for it to attain gradually the highest speed required leaving still length enough for the experiment at a steady speed, and though I have above shewn good grounds for concluding that a model 6 feet in length (it being about half a square foot in section) is sufficiently large, yet it would be well to have arrangements capable of trying models of twice that dimension. For this, a minimum radius of 10 feet in section of waterway and a length of 250 feet † would I think be quite sufficient.

Drawing Office and Workshop should be attached.

A Drawing Office and a Workshop suitable to the model making should be attached.

As to the mode of constructing the models which, if a sufficient number are made to try all the necessary variations, will form a considerable item in the expenditure it is clear that if we can by any means rapidly alter the shape of any model it is not necessary that a model of each form should survive, as long as there is a record of its shape upon paper. For purposes of alteration then, the Material for some exterior thickness should be such as can be cleanly and rapidly cut, and should be fusible for the purpose of "melting on" additions. Stearine is perhaps the best material as it possesses sufficient firmness and is at the same time capable of being rapidly and accurately cut or pared. There must of course be an internal shell of wood which will carry the fittings for ballast, towing and handling, round this the stearine will be roughly "melted on" in a clay or other mould to a sufficient thickness to allow for subsequent shaping.

Material best for models.

* Not, of course, that these other questions are unimportant but neither can they be investigated to good purpose unless in their turn disembarrassed from the question of the resistance of the Vessel.

† Should this seem an unnecessary distance, it must be remembered that though such a model might be started to its full speed in say 10 feet, yet the fluid motions proper to the speed take some time to become established in their normal shape, and the nature of these motions has an important effect on the resistance. For this reason it is desirable to start the model gradually.

Method of shaping models.

The models may be cut to their primary shape, and afterwards altered to others by a revolving cutter mounted on a traversing frame (something similar to Taylor and Jordan's wood carving Machinery) save that here the tracer which regulates the path of the cutter instead of following the surface of a solid pattern will follow on a drawing the successive water-lines, the cutter being set successively at the levels corresponding to the water-lines. The intermediate excrescences left by the cutter will afterwards be pared off by hand.

Nature of experimental apparatus.

Next as to the experimental apparatus itself by which we are to ascertain the relations between velocity and resistance in any given model.

Common method.

The prima facie simplest way of doing this is that which has been generally adopted, namely that of applying a definite suspended weight acting through a delicate tow line as the tractive force, which force is thus in each experiment regarded as the measure of the resistance, and noting the time taken by the model to traverse certain distances.

Objections to common method.

Now at the outset it is essential for the sake of avoiding "personal error" that the results should be self-recorded, but under the above mentioned arrangement it will be found that owing to various difficulties such as the elasticity of the tow line, the danger of causing adventitious friction etc., it is not easy to satisfy this requirement.

A still graver difficulty is that of properly guiding the model, for no model can be trusted to follow exactly the direction of the tow line, indeed some forms, in which the tendency to sheer would be least suspected, I have found to possess it to an astonishing degree. Now any guiding arrangement, such as a stretched wire, introduces a considerable yet irregular and unascertainable element of frictional resistance thus misapplying an unknown amount of the tractive force which cannot therefore be regarded as a correct measure of the resistance of the water to the model.

Improvement that I suggest on common method.

If however, we substitute for the guiding wire a light railway with a truck travelling on it we may attach the model to the truck through the medium of a dynamometer, affixing the tow line to the truck instead of to the model, and the dynamometer while it guides the model inexorably will indicate purely and solely the resistance experienced by the model in traversing the water. The rotation of the truck wheels will afford an exact measure of the distance as it is travelled by the model; and a "time apparatus" such as I used in my former experiments, being added, we have all the necessary elements, which when duly combined in one apparatus will record by lines traced on a travelling sheet of paper the velocity and resistance of the model at each point of its course.

Since, under this arrangement, we have an exact record of the resistance, we may do away with the definite weight and delicate tow line; substituting for it an inexorable mechanical force, regulated by a chronometric governor which will ensure a uniform speed.

No doubt all this entails an apparatus of somewhat costly construction, but after giving the subject much trial and consideration I am confirmed in the opinion that it is the only sure way of obtaining a suitably accurate result.

What experiments should be tried and in what order.

As to the experiments to be tried, it will as a preliminary be important to make a set of experiments on surface friction, chiefly in order to ascertain to what nicety it will be necessary in the construction of the models to bring them to one standard quality of surface, and these same experiments will furnish data from which the correct laws of surface friction may be determined.

Nextly before commending any methodical investigation of leading principles it will probably be well to make some experiments with existing forms adopted in the Navy, to determine whether any excessive resistance is experienced by any peculiar forms, such for instance as the double stern of some of the twin screw ships, etc., etc., and whether great length possesses that merit which has been attributed to it; inquiries of immediate practical value.

Proceeding to the methodical investigation it will perhaps be best to begin with successive variations in the forms of entrance and run, carried through every reasonable variety of form and proportion; then to examine the question of the best position for the greatest transverse section; and then to examine the effect of varying the proportion of length to breadth and to depth. It is however obvious that all these questions react on one another and that the course of investigation must consequently be in a great measure left to be directed according to the discoveries made as the inquiry proceeds.

Assuming a prima facie case to have been made out in favour of establishing such appliances and apparatus and instituting such a course of experiments as I have recommended, it will become a question *where* and by *whom* the inquiry is to be conducted.

IV. (Scheme for carrying out experiments.)

And I believe it is no undue self assertion which leads me to suggest that on the score alike of efficiency and economy the work should be entrusted to myself at my residence here.

I propose to undertake them myself.

I will at the outset state that I am prepared gratuitously to devote my services to the investigation if my proposals are approved of, and if I am provided with the means of meeting the expenditure necessary for its pursuit.

With respect to the essential requirement of fitness for the work, though there are probably many men of science whose qualifications would be found in various respects superior to my own, yet, unless their services could be obtained gratuitously to place them on a footing of professional remuneration on a scale proportioned to their qualification would of itself introduce a very serious *additional* element into the cost of the inquiry.

But also it would be false delicacy on my part were I to have any scruple in asserting my entire and I may add my special competence; and I will explain more fully the grounds of the assertion.

It is a simple fact, not only that the subject is one to which I have devoted great study for very many years, but that I have devoted to it several years of almost exclusive attention, coupled with extensive experiments, carried on at a considerable expense to myself.

Facilities arising from having made previous similar experiments.

I have thus acquired a large stock of apposite knowledge and matured habits of experimental inquiry. Indeed I may fairly express a doubt whether any other person has the advantage of me in this respect, or has produced as instructive a series of experimental results as that which I have already presented to the Chief Constructor.

I am besides, what is termed a good mechanic and I have a good workshop in my own house; I am now providing steam power which will be of material service in the operations I suggest.

Much of the apparatus which I have used in previous experiments is capable of being efficiently and easily adapted to the arrangement which I recommend and which I have already described, and I could manufacture at home the greater part of the additional machinery which will be required at a considerably less cost than if it were supplied in any other manner.

It will be extremely gratifying to me if the proposal I have made should be approved of but in offering my services thus gratuitously I may fairly make the following stipulations.

(i) If I carry out the experiments the establishment should be close to my own residence. This is necessary, not merely because in that way alone could I arrange to give my constant attention to the work, but also because nowhere else could I approach it with the same mechanical advantages.

That I must conduct the experiments at my own residence.

(ii) As I have already pointed out there will at the outset probably be several important experiments to be tried with the various forms of ships at present in use in the Navy and in course of construction, and what is of more importance, with forms of vessels which are being designed or proposed for construction and indeed any series of experiments which may be in progress, can at any time without detriment thereto be suspended in order to try any special form of vessel, the properties of which it may be desired to determine. I should however wish to be allowed myself to have the responsible direction of the course of experiments forming the "methodical investigation" since I could not pursue it effectually along a track divergent from my own natural course of thought.

Responsible direction to be in my own hands.

(iii) During the construction of the necessary covered waterway and its accessories, which should be completed by contract under my direction, I should be provided with funds to make the requisite payments to the Contractor as well as for the tools and appliances to be used in model making and for the adaptation or completion of the experimental apparatus, and during the experiments, I should be repaid all incidental expenses including those required for the employment of an adequate staff. All such expenditure being of course properly accounted for.

Funds for original outlay and for working expenses to be provided.

To place this proposal, as far as possible in a practical and definite shape, I have carefully endeavoured to ascertain the outlay which it involves.

In the first place, as the land attached to my residence does not afford adequate level space for the formation of the waterway, I have provisionally obtained the consent of the owner of the surrounding land for the use of a sufficient portion of contiguous field on the condition that when

Rental of proposed site.

the inquiry is concluded, I fill in the excavation and make good the land to its former surface, paying meanwhile in consideration of its use and its inevitable deterioration the sum of £12.10.0 per annum. The materials of the roof when removed will more than pay for making good the land.

Provisional tender for execution of work.

In the next place I have prepared a complete set of drawings and a specification of the work to be executed comprising the waterway 250 feet long (having a depth of 10 feet, a width of 6 feet at the bottom, and of 26 feet at the top) roofed throughout, and having a drawing office and workshop attached and I have obtained from a firm on whom I can rely and for whom I would be responsible a tender for the execution of the work amounting to £584.

To fill and to keep filled, so large a water space, should the supply from my own well prove inadequate, it will be requisite to lay temporarily a three inch iron pipe for about $\frac{3}{4}$ of a mile in connection with the Torquay water service, and the cost of this will be £105.

Probable total cost.

To add the necessary machinery for the model making and the completion of the experimental apparatus would probably bring the cost of the Establishment up to nearly £1,000. I will however engage if so required that that sum shall not be exceeded.

Probable working expenses.

With regard to working expenses, I should require to employ probably in permanence, two competent workmen, one of them a skilled mechanic. I have two such men now in my employment who have long been engaged for me in similar work, and on whom I can rely, their united wages are 51/– per week, or if fully employed £133 per annum.

I shall also require the services of a competent draughtsman and assistant, for which purpose I should propose to employ my son, who is commencing his profession as a civil engineer and who has already served an apprenticeship in this description of work, having prepared the drawings for the models and the apparatus used in my former experiments, throughout which he was the principal operator in the experimental manipulations, and who, I may add, more thoroughly understands the work than any one else whose services I could similarly secure.

I should propose that he should be paid at a rate of say £3.3.0 per week for his time, which would probably be fully employed in the work, but which as well as that of the workmen would of course not be charged except when so employed.

These expenses in salary and wages alone would therefore amount to £289 per annum, but as other incidental expenses would inevitably be incurred, and indeed as with a view to more rapid progress it might be requisite to employ additional hands, it might be prudent to regard the working expenses as limited to, say £500 per annum, and indeed here, as in reference to the first cost of the establishment I would guarantee that the sum named should not be exceeded.

Probable duration of experiments.

Though the experiments which I have contemplated would probably yield some useful results at a very early period it might occupy two years to complete the series exhaustively, so that the whole cost involved in them would be about £2,000.

Should this sum appear large certain considerations may at once be suggested to shew that the expenditure can hardly be regarded as unremunerative.

Assuming the enquiry to result in even nothing better than the saving of a permanent expenditure of Ten Horse Power in all, in the employed engine power of Her Majesty's Navy, the annual reduction in the national coal bill even thus would pay the interest of the outlay by which the saving had been effected, were it to render unnecessary half a dozen measured mile trials in the course of each year the same result in fuel saved would follow. Were it to result in beneficially taking 10 feet off the length of a single ironclad the whole outlay would probably be recouped at once.

But I think it is scarcely necessary to have recourse to such modest hypotheses for the justification of an outlay which when duly considered, promises to contribute materially to the speed and the general economy of the Navy.

(Signed) W. FROUDE.
DECEMBER, 1868.

CHELSTON CROSS,
NEAR TORQUAY.

[*This was followed by an Appendix containing a proof of Froude's law of comparison.*]

DISCUSSION.

General G. Rota, R.I.N. (Member): *My Lord, Ladies and Gentlemen, Our Conference could not begin its work under better auspices than those which result from a consideration of the valuable work so well described by Sir Westcott Abell. Being an old student of Froude's work from a scientific point of view, I had the privilege of starting the first experiment Tank which was erected on the European Continent in 1889. After the advice given by Colonel Soliani to the Director of Naval Construction of the Italian Navy, it was possible to obtain from our Admiralty in the Royal Arsenal of Spezia designs for a Tank exactly similar to the Haslar Tank.

For many years I have used the results of Froude's valuable work, and that of his son, Edmund Froude, and I have been able to realize the importance of the method for practical purposes. Indeed, all the Italian warships built since 1890 have profited by the Tank experiments, and also the more recently built ships, which presented very difficult problems because of their very high speeds in relation to their displacement. I refer to the *Condottieri* class, which has offered a very clear proof of the real advantage of Froude's methods in the solving of this difficult problem. I am very glad of this opportunity, as an old contemporary of the Froudes, to pay a tribute to the memory of that great naval architect whom we honour to-day.

I should like to add that ever since the publication of the work of the National Tank in Rome in 1928 I have had the portrait of Froude in my office in token of our constant recollection and admiration of his great achievement.

Professor Vice-Admiral Y. Hiraga, I.J.N.: The pioneer work which William Froude carried out on resistance and propulsion has been studied carefully in my country. We have accepted it as sound. I am ready to affirm that, on the principles he established, many Japanese warships have been built which have satisfied completely the estimates of their designers.

Froude's classical experiment on the *Greyhound* has stood for many years as a clear demonstration of the correctness of his methods, but as pointed out by this Institution's Skin Friction Committee in 1924, this experiment has required corroboration. A few years ago we carried out in Japan towing experiments on ships similar to those made by Froude on the *Greyhound*. I shall have the honour of describing those experiments in my paper to-morrow, so I will only say now that while they indicate that skin friction correction may be different in degree to that used by Froude, they do show that Froude's system is satisfactory.

Professor E. G. Barrillon: *The homage we are paying to-day to the memory of William Froude seems to be particularly appropriate. We recognize in him a great experimenter, guided by an unfailing instinct for penetrating the innermost nature of the phenomena which he investigated. Two points merit attention: First, the idea of separating skin friction. This idea was opposed at first on account of its having no theoretical basis. Yet it has held good until our time and has not been supplanted by any better theory. Secondly, the rules for determining the amount of skin friction. After numerous attempts to improve on Froude's rules these are now used in the same form as he left them. In France our experiment Tank has remained faithful to Froude's methods, for we have not considered that the novelties introduced at different times were based on solid grounds. This constancy has had the advantage that our records are now continuous and are in accord with the most recent recommendations of Tank Superintendents. We must not, however, lose sight of the fact that this state of things may be the result of our lack of experience on full-sized ships.

* Remarks as translated.

Mr. M. P. Payne (Member): *The paper presented by Sir Westcott Abell calls not so much for discussion as for thanks. The published accounts of the life of William Froude are practically confined to the obituary notice in the Transactions of this Institution—that was in 1879—and the address given by Sir Westcott to the Devonshire Association last year. He could have chosen no happier method of illustrating the character, vision, and scientific discernment of William Froude than by quoting in full the text of his thesis to the Admiralty regarding the project for an experimental Tank for examining the resistance of ships by trials with models.

It is unnecessary to draw attention to the completeness of Froude's conception of the problem and the means of solution. Sixty-five years have seen improvements in detail, but the essentials, the covered waterway, the towing carriage, and, perhaps the most distinctive feature, the automatic records, are all unchanged.

One aspect of Froude's work is infrequently mentioned and should perhaps be placed on record—I refer to the system of analysis of the automatic records which he initiated. Quick to realize the importance of this process, he inaugurated a routine whereby the diagrams could be rapidly and accurately translated into terms of resistance and speed with small labour. The instincts of method and accuracy which he initiated, and which were so ably developed later by his son, R. E. Froude, have through him influenced the whole body of experiment Tank workers.

There is one further matter to which I should like to refer which illustrates the character of William Froude as Sir Westcott Abell has described it; that is, the isolation in which Froude stood for some time as a protagonist of model experiments. The report of the Committee on the stability, propulsion, and sea-going qualities of ships to the British Association, in 1869, shows him advocating these in a minority of one, and in opposition to such men as Rankine and Merrifield. The Transactions of this Institution show that former members at first were strongly opposed to the idea. We find Mr. Merrifield, a forerunner in office of Mr. Dana, adhering to his preference for experiments on full scale, and Scott Russell, who had himself considerable experience with ship model experiments, was convinced that Froude's proposals would not lead to fruitful results. Mr. Froude noted that the feeling of the meeting was very much against experiments with models. Froude, however, remained true to his convictions, and he had already attracted the support of Mr. (later Sir) E. J. Reed, Chief Constructor of the Navy, who, in the face of professional opposition, secured a fair trial for the ideas which Froude had advanced, although the Government of the time is still remembered in Service circles as one passionately devoted to naval economy. To their credit it may be recorded that the members of the Institution were then speedily convinced. Further, although the opposition was so emphatic, it is of interest to recall the words of Scott Russell in 1870: "Let us congratulate ourselves that Mr. Froude, who is so capable of making beautiful scientific experiments with great accuracy, and with great ingenuity, has been authorized and empowered, as I understand, by the Admiralty to make a series of experiments upon small models."

Finally, may I quote the letter sent by the Board of Admiralty to R. E. Froude after the death of his father? In the perspective which time gives, the sentiment appears to be, if anything, an under-statement, in that his influence and therefore his loss were international even more than national:—

Admiralty, S.W.
27th May, 1879.

Sir,

1. I am commanded by My Lords Commissioners of the Admiralty to inform you that they have received, with a deep sense of sorrow, the intimation contained in your letter of the 26th instant of your father's death.

2. They feel that Mr. Froude rendered great services to the Navy, and the Country, in making

* Remarks read by Mr. R. W. L. Gawn.

his great abilities, knowledge, and powers of observation available for the improvement of the design of Ships, without reward or any other acknowledgment than the grateful thanks of successive Boards of Admiralty.

3. My Lords desire to convey to you, and other members of the family, the expression of their most sincere sympathy at the irreparable loss which you have sustained—a loss which cannot be looked upon as other than a national one.

I am,
Sir,
Your obedient Servant,
(Sd.) Robert Hall.

Professor Dr. Ing. F. Horn (Member): There seems to be on the whole—in spite of our being aware of the spirit of compromise in the Froude method—agreement that the chief idea of Froude, at least for the practical purposes of Tank practice, should remain unaltered—namely, that the frictional and the residuary resistance must be separated from each other, and that the former must be related to the basis of the frictional resistance of planks, and the latter translated from model to ship according to the Froude law of similitude.

There are two points which have been widely and rather intensively discussed by the Committee for Resistance and Propulsion set up by the Schiffbautechnische Gesellschaft in Germany, and they seem not only to need revision but also to be rather capable of revision.

The first is the influence of the frictional form effect. This matter will be dealt with later on by General Rota. It seems necessary that tests for one and the same ship form ought to be made with models of different scales in order to get as accurately as possible, for the same Froude numbers, the variation with Reynolds number of the specific total resistance, and by that means also for the specific frictional resistance. In order to overcome the well-known difficulties that have been experienced in such tests due to the laminar and eddy-making disturbances with the smaller models of the family, we have decided to carry out the tests with very large models up to 12 metres. Fortunately, the big Hamburg Tank gives us the chance of making such tests.

For three different types—a freighter, a quick passenger ship, and a cruiser—the tests are at present prepared and already partially made. In this sense we have already fulfilled, though in another direction, the proposal of General Rota to test the influence of form on frictional resistance with different types of ship forms.

The tests with most of the models of the freighter type have been already carried out in the Wageningen Tank—a remarkable example of international co-operation which was inaugurated at the Conference last year in Holland, and for which we are very much indebted to the Director, Dr. Troost, and the Council of the Wageningen Tank. Each set comprises five to six models.

By these means we hope to ascertain the suitable basis for the influence of form on frictional resistance, and a possible comparison of the frictional curves of the ship forms with those of smooth planks, for which the question of the right equation and extrapolation has been satisfactorily settled in recent years. This curve is, we know, different from the Froude curve, as it is first of all in accordance with the Reynolds law and secondly it is free from influence of roughness by which the course of the Froude curve in the range of ships' lengths is partially governed.

The influence of roughness—and this is the second point why an alteration of the Froude method is needed in detail—should be treated separately, and the augment of resistance due to roughness added to the smooth curve. According to the well-known recent systematic tests carried out in Göttingen with different degrees of roughness, this influence seems to be very important. At present the Institute of Göttingen is about to carry out systematic tests for surfaces of that kind of roughness with which we have to do in shipbuilding.

As I mentioned before, all this does not touch upon nor alter the chief idea of Froude, but it may perhaps be considered as a tribute of thanks which shipbuilders in Germany tender to the genius of William Froude.

The PRESIDENT: I think Dr. Horn has rather anticipated the discussion which we hope will take place on the next paper which will be read by General Rota.

Lieut.-Comm. C. O. KELL, C.C. (U.S.N.): Before the discussion on Sir Westcott Abell's paper closes I should like, on behalf of Admiral Taylor, to add his respects for William Froude. I regret very much that this paper did not reach me before leaving the United States. Had it done so, I feel certain that Admiral Taylor would have had some personal remarks to make which he would have delegated to me to read.

Sir WESTCOTT ABELL, K.B.E., M.Eng. (Vice-President): I esteem it an honour to have paid this small tribute to the memory of William Froude. I would say that I have derived much benefit during the last two years from the study of his life and work, but most particularly from the study of the man himself.

I do not feel competent to assess what he has done for naval architecture, and throughout the little memoir that I have read to you I have striven consistently to adopt the words of those who lived and worked with him and who could form a far better estimate of his achievements than I could possibly hope to do.

The PRESIDENT: Nothing could be more flattering than the tributes which have been paid to our fellow-countryman on all sides from all over the world. I would like to offer the thanks of the Institution of Naval Architects to the distinguished representatives from every other country for all they have said on behalf of our great fellow-countryman.

INFLUENCE OF FORM ON FRICTIONAL RESISTANCE.

By General G. Rota, R.I.N. (Rome Experiment Tank), Member.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 10, 1934, the Right Hon. Lord Stonehaven, P.C., G.C.M.G., D.S.O., President, in the Chair.]

The method adopted in the various experiment Tanks, to apply the results obtained from towing models to full-size ships, is well known—division of the resistance into two parts: first, the frictional resistance calculated according to definite rules; and second, the residuary resistance to which for corresponding speed one applies the law of similitude. The ship's resistance is completed by adding to the residuary the corresponding frictional resistance, calculated as usual. This method of Froude's has required special experiments with planks to obtain some data on the parameters to introduce into the formulæ, which express the value of frictional resistance in the case either of models or of ships.

Froude's experiments are classic, and afterwards those of Gebers, based on that conception; various formulæ are used: Froude, Tideman, Gebers, Le Besnerais, Kempf. In all these it is admitted that the frictional resistance is the same for a ship-shape body as for a plane of the same length and area at the same speed.

This method has not been accepted without criticism, because some do not admit the separation of the resistance into its elements, as above stated, and they consider it better to treat the resistance as an indivisible whole, and therefore the method is classed as an empirical one. It was even suggested recently that it would be nearer to the truth to transfer the resistance of the model to the ship at "corresponding speed," applying directly the law of similitude, and thus E.H.P. $= e.h.p. \times \alpha^{3 \cdot 5}$. But up to now there has been no demonstration which would entitle us to apply this method.

Nevertheless, for over sixty years Froude's method has stood the test of time, and the results obtained thereby satisfy us closely enough for all practical purposes.

However, naval architects now believe that this method could be advantageously revised, or at least they wish to adopt a uniform method for these calculations in order to permit a more exact comparison of results obtained.

This question is included in the programme of our Conference, and we all wish to reach satisfactory conclusions. There will certainly be some objections; one can say that any change from the usual and particular method of calculation may bring forth difficulties in the use of the valuable collections of data belonging to every Tank establishment, which would be obliged to change their methods. This is perhaps a justifiable objection, in view of the conservative spirit.

The chief doubt which arises on the validity of the formula used for the determination of frictional resistance is that no parameter is introduced in it to take into account the effect of the curvature of the wetted surface, the formula being the same both for a plane surface and for a ship-shape body.

The interesting researches made by Mr. Perring in 1925 * for the determination of the

* W. G. A. Perring: "Form Effects and Form Resistance of Ships," Trans. I.N.A., Vol. LXVII., p. 95.

form resistance of supposed ship-shape bodies of various forms and proportions, but of equal length, equal area of wetted surface and displacement, have added a valuable contribution to the solution of this question, and one might conclude that some differences in the calculated values of the frictional resistance within unimportant limits does not involve a serious error in the final results for the full-sized ship.

New researches on this question may, however, appear to be justified, and this brief paper is written in the hope of arriving at some data on the influence of the curvature of the frictional surface on the amount of the resistance. With this object in view, we considered five models of ships of the same length, derived from a given one, changing the scale of the breadths, and in a reciprocal one the scale for the heights, settling the draught afterwards in order to leave the same area of wetted surface. These five models are similar to those I described in 1905 in a brief paper presented to the I.N.A.*

The following table contains the principal dimensions of the five models as used for the experiment:—

Dimensions of the Models.	C 93.	C 94.	C 95.	C 96.	C 97.
Length on the water-line, metres.. ..	5·64	5·64	5·64	5·64	5·64
Length between perpendiculars, metres ..	5·40	5·40	5·40	5·40	5·40
Breadth, metres	1·08	0·945	0·810	0·675	0·540
Draught, metres	0·123	0·1614	0·2028	0·2469	0·291
Trim, millimetres	15·3	17·5	20·4	24·5	30·6
Wetted surface (A), square metres ..	3·997	3·997	3·997	3·997	3·997
Area of maximum cross-section, sq. metres	0·1084	0·1282	0·1396	0·1422	0·1326
Displacement in kilogrammes	307·125	374·625	416·137	425·165	393·187
Height of the model, metres	0·30	0·375	0·450	0·525	0·600

Fig. 1 (Plate XXV.) represents the outlines of the transverse sections of the medium model. Towing experiments with the above models were made in the National Tank in Rome. The results have been collected and presented in the usual form of specific resistance $\frac{R}{\delta A V^2}$, and on Fig. 2 the corresponding curves of the five models are brought together on the abscissæ values $\frac{V L}{\nu}$. Curves representing the specific frictional resistance calculated by the usual method are also shown on Fig. 2; evidently they are the same for all five models.

At the point where wave-making resistance appears noticeable, so to speak, in the field corresponding to small speeds, the values of the specific resistance are practically the same for the five models, and show a notable analogy with the corresponding specific frictional resistance, deduced from the usual calculations.

We could therefore infer that, at least in the case of the models now considered, the different curvature of the wetted surface has no influence on the value of the frictional resistance.

These experiments cannot be considered as definitive because the conclusions to which they lead relate, as we have said, only to the particular case considered; but the continuation of these experiments should enable us to examine other differently shaped models, but of same length and wetted surface. The carrying out of experiments on towing a ship

* G. Rota: "Experiments with Models of Constant Length and Form of Cross-section, but with Varying Breadths and Draughts," Trans. I.N.A., Vol. XLVII. (Part II.), p. 334.

under perfect conditions of similitude with a corresponding model in a Tank is to-day greatly to be desired.

If we admit that the curvature of the wetted surface has little or no influence on the value of the frictional resistance, full-scale experiments on any ship would also have considerable importance, and the comparison would yield precious data.

Ever since 1924 full-scale towing experiments were suggested by the Advisory Committee of the Institution of Naval Architects, and on other occasions their usefulness was demonstrated. Only Government departments could afford to carry out such experiments, which are no doubt very expensive. But so far the question has not yet been resolved.

On our side we have considered some time ago the possibility of making in the Bay of La Spezia trials of this kind; many are the difficulties to be overcome—chiefly to ensure a straight run for the towed ship—but we rely, in consideration of the financial help always generously given by the Italian Minister of Marine to every research directed to the progress of Naval Science, that the suggested and much to be desired experiments may at last be made.

DISCUSSION.

Dr. Ing. F. Gebers: I only wish to say a few words on this excellent paper which has been read to us by General Rota. I think it is very interesting that these different forms of ships have given no indication of the frictional resistance. The formula which I have given is mentioned here in the beginning of the paper. It is not intended for hulls of ships, but only for planks. I stated last year that we cannot reckon from the resistance of a rectangular thin plate* the resistance of a plate of another shape but of the same length. We are not able to reckon from a rectangular plate the resistance of a triangular or other shape. I think it would be very difficult to use the resistance which we have found from Tank tests with long planks directly for ship bodies: we must have more information on this point. The effect will also be quite different if we have propellers abaft the ship, because the streamlines round the ship body will be different from those for a naked ship without propellers. I think we must have the greatest respect for our old master, William Froude, whose pioneer work was so remarkable, and we must consider very carefully before we make any change in his methods.

Mr. G. S. Baker, O.B.E. (Member of Council): A large amount of literature is accumulating on the skin friction of different shaped bodies, and we owe our thanks to General Rota for this last contribution to the subject. The results which he has produced are rather of a negative character, since a large change in proportions produced very little alteration in resistance. But I rather doubt whether he expected to find a large change when he started those experiments, because if you look at the conditions which he laid down they were of a kind which almost prohibited it. In all the changes which he made in his models, the longitudinal distribution of the skin remains the same. That is one of the most important factors in the specific resistance of a body.

Mr. Perring showed in the experiments which he carried out in the National Tank that if one made a plank in which the longitudinal resistance was the same as that of an ordinary shaped hull, the total resistance obtained was the same. From that point of view, therefore, no change in resistance was to be expected.

A second item which influences the effect of form is the radius of curvature of the transverse sections. Dr. Telfer suggested it should be taken into account in the formula for resistance. I do not think many of us do it, but most of us recognize that it is there.

* *Schiffbau, Schiffahrt und Hafenbau,* 1933, Heft 2 und Heft 14, Sonderdruck: Flächenform und Flächenwiderstand einige weitere Versuche.

The radius of curvature was so large in this form that its influence was reduced to an almost negligible quantity, and it only showed up in the extremely flat broad forms like General Rota's C 93.

The only other word that I would like to say is that I notice at the extreme end of the paper he has been able to persuade his Government to support him in making experiments with very long bodies. He is to be congratulated on making progress with work which at one time we hoped might be done in this country.

Dr. G. Kempf (Member): The attempt which General Rota has made to elucidate the problem of form effect is very interesting, and we are very thankful to him. It is not the only method for elucidating this problem, and Professor Horn has told you already, in the discussion on Sir Westcott Abell's paper, that attempts are to be made by the Dutch and German Tanks with different families of ships, and we hope to get some useful results. I agree entirely with Mr. Baker that the negative result of these experiments shows no evidence that there is any form effect at all.

I would ask General Rota if it would not be possible to calculate wave resistance from the formula of Havelock, to get one part of the difference between the total resistance and the frictional resistance, so that there might remain perhaps 2 or 3 per cent., which could then be added to form effect. Perhaps it may be possible to calculate it by the same means which we have now for wave resistance. I agree with General Rota that the carrying out of towing experiments on the hull of a ship under perfect conditions of similitude with a corresponding model in a Tank is to-day greatly to be desired. In Germany, and perhaps also here in England, we are not so lucky as our friends in Italy, who have beautiful conditions for their experiments. We all hope that they may succeed in getting accurate results from towing experiments, as General Rota has suggested in his paper.

Mr. M. P. Payne (Member): * The subject is one that cannot too frequently be brought to our notice. The author modestly states that the results described by him are particular rather than general. They are, none the less, a most useful contribution to our knowledge of a subject of great interest from a theoretical point of view, and in a quantitative sense they may, according to available information, be of appreciable importance in full ships, if not so much in fine ships. As the author remarks, his experiments cannot be regarded as definitive, and an extension of the investigation is desirable. It would appear that the definite conclusion from the experiments is that at low speeds the differences in curvature and shape of the surface have produced no measurable difference in specific total resistance, nor in the excess of that resistance above the calculated skin friction resistance. This does not, however, necessarily imply that curvature has a negligible effect on the frictional resistance.

An examination of the shape of the sections in the body-plans given by General Rota in the 1905 paper referred to by him suggests that the change in shape of the sections of the models considered is in the direction of a transfer of sharpness from the water-line to the keel in passing from model C 93 to model C 97. Assuming, for the sake of argument, that the greatest section is a semi-ellipse, it is seen that the radius of curvature at the water-line changes from 0·03 metre to 0·3 metre, while at the keel it changes from 2·4 metres to 0·25 metre. The combined effect on mean curvature of the change of dimensions may tend to cancel out and may account, at least in part, for the absence of any appreciable effect on form resistance in the present case.

Further, the difference in resistance due to divergent wave-making which may be present even at the very low speeds, and the effect of trim, may be contributory factors leading to a cancellation of difference in resistance. It would be interesting if General Rota could give us the records of sinkage and trim of the model at various speeds during the experiment,

* Remarks read by Mr. Gawn.

as a difference in sinkage may be taken as some indication of a difference in distribution of velocity, and accordingly of surface skin friction.

General G. Rota, R.I.N. (Member): It has been suggested that the same longitudinal distribution of the wetted surface, as assumed in the five ship models considered, is the reason for the frictional resistance being the same, which at low speeds is the main part of the total resistance. This is, I think, only an apparent reason. The five models considered in my experiments represent only a part of the infinite series of transformations of the model C 95. This series of models, which all have the same wetted surface and the same length, and also are alike in the extreme shape of ideal hull, like a sort of plank with an extreme breadth or extreme depth value (as it is developed in horizontal or vertical dimensions), can benefit by similar results in the matter of frictional resistance, although being of quite different shapes. The shapes of these ideal series of hulls are very different, and so, even admitting, for instance, that the distribution of the wetted surface bore some resemblance in each, I can rightly deduce that the equal frictional resistance which results at low speeds is, in the case examined, independent of the shape of hull of the entire series.

One of the characteristics of these types which derive from the model C 95, all of equal wetted surface, is the value of the displacement which has a maximum in model C 96, decreasing in the others, and distinguished for its great breadth or depth. We observe that the block coefficient values, i.e. about 0·45 for model C 95, decrease more and more for all the transformed models in order to assume a horizontal or vertical development. From these results, therefore, I think that the method used up till now, due to our great master, W. Froude, to calculate the frictional resistance function of the length and the extension of the wetted surface, whatever may be the hull's shape, is still very suitable.

It has been observed that the effective trim assumed by the five models experimented with in the towing researches may have contributed to give the same resistance. I can add that for small values of $\frac{V L}{\nu}$ no marked difference has been observed in the trim value of the model as compared with the same one at rest.

The results of the new experiments which Dr. Kempf says are to be made by the Dutch and German Tanks with the same object as mine but by a different method will be awaited with great interest by everyone. It is evident that even at low speed, if the wave-making could be eliminated from towing researches, or if we were able to experiment on ship-shaped bodies without having any surface disturbance, it would be a great advantage. Without doubt this may be done by some practical method and I, too, shall continue such researches.

I learned with great pleasure that everybody agrees to the utility of towing experiments being made with different full-sized ships, and it is to be hoped that they will be carried on with success in a short time.

The President: Gentlemen, We need no stimulus to pass a very hearty vote of thanks to General Rota for this most interesting paper with which our Session has been opened. I put it to you that we are all very grateful to him for having prepared this paper, which has given rise to so stimulating a discussion, and I ask you to pass a very hearty vote of thanks to the General.

To Illustrate General G. Rota's Paper on "Influence of Form on Frictional Resistance."

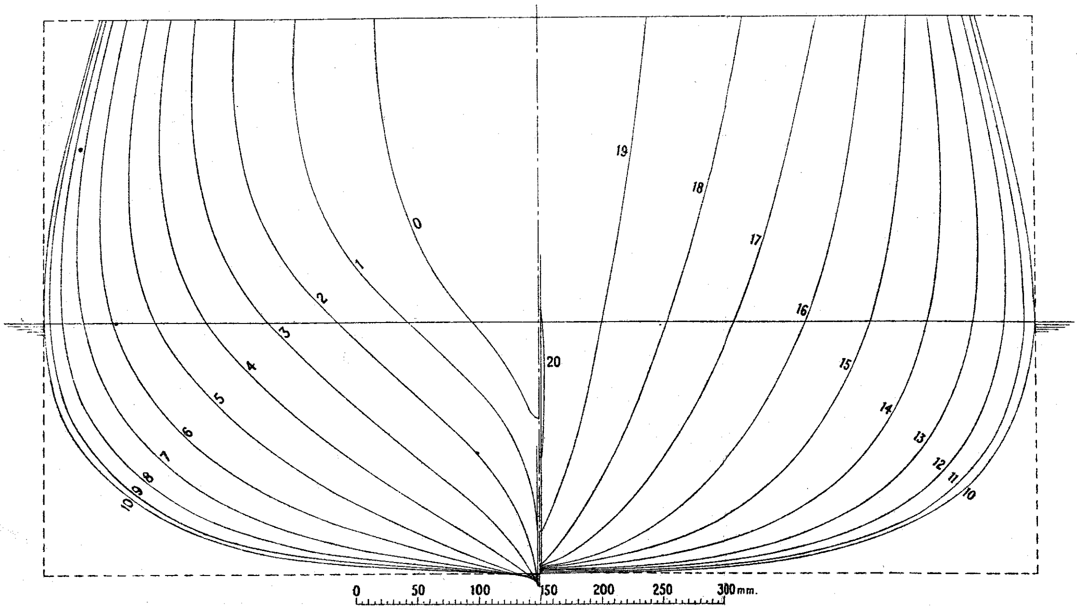


FIG. 1.—BODY-PLAN OF THE MODEL C 95 (MEDIUM).
Distance between the sections of the body-plan, 270 mm.

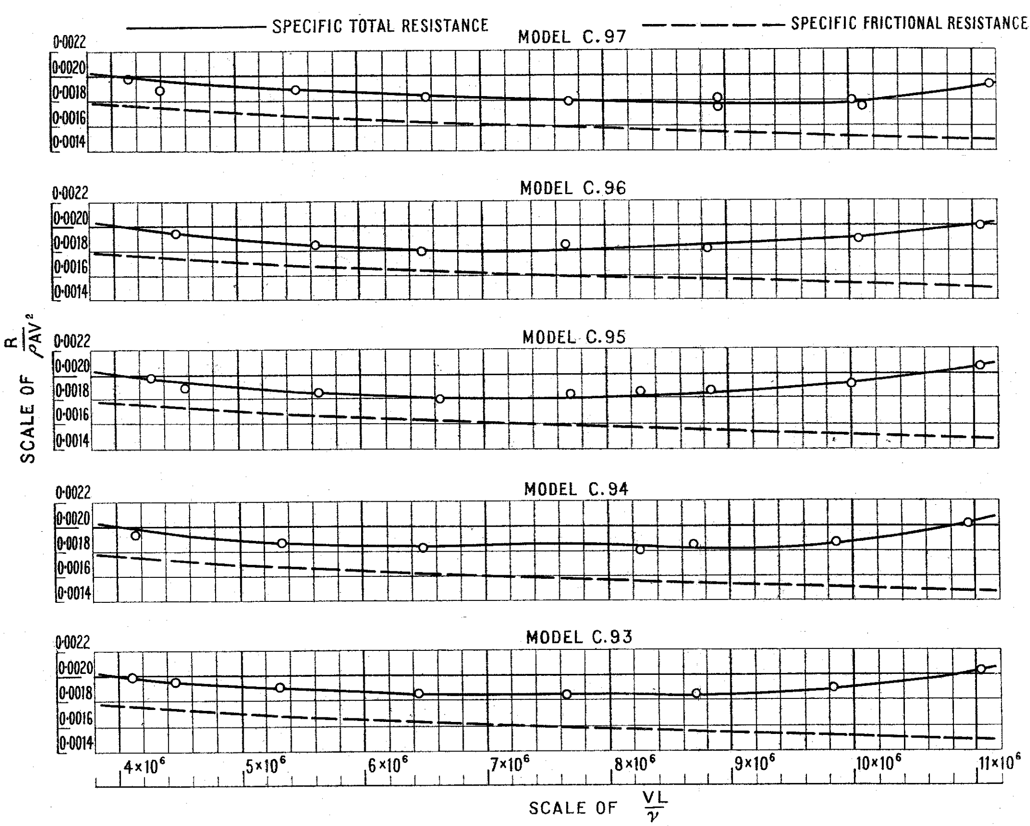


FIG. 2.

SOME NOTES ON THE NOMENCLATURE SUITABLE FOR THE PRESENTATION OF MODEL DATA.

By Professor T. B. Abell, O.B.E., M.Eng. (of Liverpool University), Vice-President.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 10, 1934, the Right Hon. Lord Stonehaven, P.C., G.C.M.G., D.S.O., President, in the Chair.]

In replying to the discussion following the presentation of his paper "On the Constant System of Notation of Results," read before this Institution in 1888, Mr. R. Edmund Froude said:—

> "My principal object in bringing it before the meeting was to put this method on record in order that other persons who are occupied in treating experimental results, if they find occasion to use a method of notation of this character, should, as far as possible, adopt this system of notation which we have been in the habit of using, instead of other equivalent methods, which in themselves may be quite as good, simply in order that one method of notation may as far as possible become universal."

At that time only two Tanks, other than the one at Haslar, were in existence. One of these was at Dumbarton, the oldest existing establishment, which had four years' work to its credit. The other was at St. Petersburg, now Leningrad, and was probably then not in full working order. Edmund Froude's suggested notation was thus put forward at an opportune time, for there could have been no large accumulations of model data expressed in any other form.

Froude in his paper stated:—

> ". . . the results of experiments on models when obtained are communicated to the Admiralty in the form of curves of E.H.P. calculated from the model resistance . . . and this form of presentation of the results is of course the most serviceable for the purposes for which, in the first instance, the experiments have been made. But the utility of such experiments is not, of course, limited to their application to the particular ships represented by the models experimented . . . the results obtained in the past, with all miscellaneous models, form a storehouse of information for the future . . . the information must be so presented that the performance and also the proportions (and as far as may be the principal characteristics of shape) of the several forms may be directly comparable; so that it may be determined at sight (1) what selection of forms previously tried are qualified by their general proportions and shape to be brought into comparison with any new design, (2) how the individual 'forms' so qualified compare with each other in performance."

I think it desirable to give these quotations because they state the case (*a*) for the necessity for general co-operation in the use of a uniform international notation, and (*b*) the requirements which any system of notation should endeavour to satisfy. They were made nearly fifty years ago, and the questions we may well ask ourselves at this moment are: "Have the suggestions of Edmund Froude been realized?" and "Have conditions or requirements so changed, or has our present knowledge of the general laws of fluid resistance been so extended, as to prevent their realization?" I think it can be said with fairness that the answer to both questions is in the negative.

For it seems to me that the main reason for the failure of R. E. Froude's objects is psychological; on the one hand, a natural dislike to express oneself or to present one's work in any language other than one's own, and on the other hand, our different valuations of what is immediately utilitarian and what is permanently utilitarian. His plan was to present the results immediately needed in engineer's units, but for permanent use to present them in an abstract form which would bring out the resistance characteristics of a form in a manner which would leap to the eye.

During recent years there has been a more general readiness to publish results of investigations resulting in large accumulations of very useful data in many different languages and notations. The desire for a universal notation has become more evident, but the difficulties have become greater. The psychology has changed in a marked degree, for anyone who attended the Conference of Tank Superintendents, convened in July 1933 at The Hague by the Council of the Nederlands Scheepsbouwkundig Proefstation of Wageningen, must have been impressed by the desire on all sides to find some common basis of agreement, and by the very substantial progress made towards a uniform method of calculating resistances and of their presentation, eventually agreed upon (see Appendix I.). In the methods now used by different experimenters there will be found differences in form, in units, and in terminology. Terminology is governed by language; units are governed by national systems of weights and measures, and by conventions not specially associated with ship-model experiments. Both these are, internationally, comparatively unimportant; for differences in terminology may be overcome by a key table, or a glossary of terms and symbols; differences in units involve simply differences in the values of numerical coefficients or factors. On the other hand, differences in form are most difficult to appreciate and assimilate. With a uniform scheme of presentation of results, differences of terminology do not matter at all, and differences in the units, whether cm. gm. sec., or lb. ft. sec., do not affect the recognition of characteristic changes in performance or proportions and shape. My remarks will therefore be devoted almost exclusively to the form of presentation, and I shall take first the resistance experiments with the naked hull.

I am prompted to make some observations as to the desirability of confining resistance performance experiments selected for publication to the naked hull, exclusive of all appendages. Appendages are necessary evils; they may be well designed, they may be suitably placed, but it is the main hull which contributes the greater part of the resistance to propulsion, and is the element on which the greatest economies or losses can be experienced; on the other hand, appendages may be badly designed, and unsuitably placed in a form which is otherwise good. I think we may be losing valuable information if we confine ourselves to self-propelled models, for we are there measuring too many things at one time.

Method of Presenting Performance.—The performance of a particular shape or form of hull is measured by its resistance at different speeds. The resistance of different models is dependent on shape of model, on the relative speed, and also on scale or size. Edmund Froude stated that "the factor of variation which we desire to eliminate is that of size and not those of shape and speed, to determine which is the very purpose of the experiments whose results are to be dealt with." The total water resistance is made up of three elements: surface friction, wave-making, and eddy-making resistance (resistance due to form).

The effect of size alone for any given shape can be expressed in general terms by Rayleigh's law of dynamic similitude or dimensional theory by the equation

$$\frac{R}{\rho\, v^2\, l^2} = \phi_1\left(\frac{v\, l}{\nu}\right) + \phi_2\left(\frac{v^2}{g\, l}\right)$$

in which the first function represents skin friction and eddy-making resistance, the second wave-making resistance. This law applies to similar forms, provided the conditions of flow are the same for ship and model, i.e. provided the flow is turbulent, and provided the eddy flow is established in the model. In these circumstances the first term varies with speed in a quite continuous and regular way; the second varies in quite an irregular way, having maximum and minimum rates of change of resistance depending upon the value of the argument $\frac{v^2}{l}$. As it is this term which is particularly sensitive to shape of hull, it is the one which must govern the selection of the expression to give the best indication of relative performance of different shapes.

The problem, then, is twofold.

(*a*) Whether to plot the two terms separately, as was done by William Froude when presenting in 1877 his work on the effect of the introduction of varying lengths of parallel middle body, and by D. W. Taylor at the Washington Tank from 1898 onwards, or to plot both together as is the practice at all other Tanks.

(*b*) How to express the term $\frac{R\ (\text{total})}{\rho\, v^2\, l^2}$ and the argument $\frac{v^2}{l}$ in the most suitable form.

As shape of form is the most important element affecting performance, we should consider the second problem first. The term on the left-hand side of the equation can be expressed in a variety of forms without any loss of generality, as follows:—

$$\frac{R}{\rho\, v^2\, l^2};\quad \frac{R \times v}{\rho\, v^3\, l^2};\quad \frac{\text{E.H.P.}}{\rho\, v^3\, l^2};\quad \frac{R}{\rho\, l^3};\quad \frac{\text{E.H.P.}}{\rho\, l^{\frac{7}{2}}};\quad \frac{R}{\Delta};\quad \frac{R}{\rho \nabla};\quad \frac{R}{\rho\, S\, v^2};\quad \frac{R}{\rho\, v^2\, (\Delta^{\frac{1}{3}})^2}$$

In these expressions four criteria of size occur: length l, surface S, volume of displacement ∇, and weight displacement Δ.

Again quoting Edmund Froude:—

"The choice of the particular characteristic of size that should be employed depends not on any considerations of resistance theory (as, for instance, the question what size characteristic mainly influences resistance), but on the question what size characteristic enters into the best criterion of performance from a purely practical point of view. And it appeared to me unquestionable that the most serviceable criterion *available* (though not of course a perfect one for all cases) lay in the degree of resistance at given speed for given displacement."

Then $\frac{v^2}{g\, l}$ may without loss of generality as between ship and model be put in the alternative forms:—

$$\frac{v}{\sqrt{l}};\quad \frac{v}{\nabla^{\frac{1}{6}}};\quad \frac{v}{\Delta^{\frac{1}{6}}}$$

since g for our purposes is constant at all the experiment Tanks (see Appendix II). And in selecting the precise form to be used we must try to select that which will not obscure any of the wave-making characteristics of the shape. In estimating the horse-power of a ship from its model, it is quite immaterial which alternative we select, but for comparing the performances of different shapes we must select the most suitable form.

William Froude showed that where length is increased by the introduction of parallel middle body in which the form of the fore-body and of the after-body remain the same, length may be taken as a criterion of wave interference. Baker showed, in the same circumstances, in fact from Froude's experiments, that the product of length and the prismatic coefficient, P·L, was also a criterion. Edmund Froude showed, in his methodical series of 1904, that increase of length by adding to the fore-end of the curve of areas (actually he "snubbed" the parent form, and so reduced the length) is not so good a criterion. Baker also showed for models having the same displacement and practically the same prismatic coefficients that if the lengths of the entrance and run were altered, keeping the sections of the same character, P·L was not then a satisfactory criterion. Edmund Froude by his methodical series also showed that for the same length and similar curves of areas, but increased displacement, i.e. increased M value by expanding the sections in breadth and draught proportionally, again length is not a satisfactory criterion. There is therefore considerable difficulty in deciding whether length or displacement should be used for this purpose. I think the position may be summed up in this way; there is, in fact, a large range of length of vessels of about the same block coefficient, and for vessels of this fulness length may be taken as a criterion, but equally so may displacement. One of Edmund Froude's guides to performance was the vessel's M value or ratio of length to the cube root of the volume of displacement. This, taken with the prismatic coefficient and with the actual length in relation to speed, or with Δ in relation to speed, may be assumed to give equally satisfactory criteria. Edmund Froude selected the latter, as on the whole being the best available.

The Hague Conference agreed that the total model resistance should be expressed in one of two forms, and plotted with

$$(1) \quad \frac{R}{\frac{\rho}{2}\nabla^{\frac{2}{3}} V^2 \times 10^2} \quad \text{or} \quad \frac{R}{\frac{\rho}{2} S V^2 \times 10^3} \quad \text{as ordinates,}$$

to

$$(2) \quad \frac{V}{\sqrt{g L}} \quad \text{or} \quad \frac{V}{\sqrt{g \nabla^{\frac{1}{3}}}} \quad \text{as abscissæ.}$$

The length of the model is to be stated and represented by L. R is the total resistance; ρ is the absolute density of fresh water; V is the speed in knots; ∇ is the displacement; S is the wetted surface calculated by multiplying the mean girth of the sections, without correction for curvature, by the length of the curve of areas; the temperature of the water to be stated, but all model results to be corrected to a standard temperature of 15° C. = 59° F. In this country the practice at Haslar and at the William Froude Tank has been to correct model data to a standard temperature of 55° F. The changed standard will thus reduce the C value slightly as compared with previous published data.

No particulars of units are given here except where the unit used is international. Values of the density, viz. mass per unit volume, are given in Appendix II. for fresh water and for sea-water at different temperatures. In this country the value of ρ used in the presentation of model results in fresh water is 1·938. This is correct at 68° F. For sea-water the density at the same temperature is 1·986, with a salinity of 35 and 1,025 oz./cub. ft. As these figures have been so much used, it is perhaps advisable to adhere to them for general use, although they do not correspond to the standard temperature to which the skin friction correction is made, viz. 59° F. The ratio of the densities is then very nearly in the ratio of 64 lb. to 62·4 lb., or of 1,024 oz. to 998·4 oz.

The relative density of fresh water at 64° F. is 0·9984. There are thus slight discrepancies in these figures, but for general practical purposes it will be satisfactory if figures which have already been used are adhered to.

Users of published data may with some confidence put forward their requirements somewhat on these lines: "We would like data put in a common form which, while it shall not in any way conceal any resistance characteristics of the form, shall be expressed in symbols representing those elements of the ship which are most vital in the early stages of preparing a design." These are the principal dimensions, L, B, D, displacement, block coefficient, or prismatic coefficient. The wetted surface, S, as a rule, will not appear in any of the preliminary design calculations; if it does, it will be calculated by approximate rules, and then it will not be suitable for use in deducing R from the model curves of performance. This leaves us with L and Δ or ∇. Either will be suitable for the proposed alternative notations, but it is to be hoped that Tank Superintendents will be able to agree to the use of an internationally uniform system. It may be noted that whereas in one system there are two symbols used, viz. V and ∇, in the other there are three, viz. V, S, and L.

Returning to the first problem as to whether the wave-making term should be separated from the skin friction term, any method of applying the correction for skin friction in passing from model to ship implies the separation of the total resistance into its two elements. No attempt is made, as a rule, to obtain the resistance due to form. It is included with the skin friction, except that, in very full forms, an addition is sometimes made for the increased eddy-making then experienced. It thus becomes very important that the method of estimating the skin friction for the model and ship should be uniform for all published data if the wave-making term, following Froude's law of comparison, as obtained at different Tanks for the same shape is to be strictly comparable. Such uniformity involves the use of the same coefficients of resistance for the range of model and ship lengths, and also the same index of speed. Moreover, it requires the same method of estimating wetted surface.

Different experiment stations adopt different methods of estimating both. William Froude's data with Edmund Froude's extension of them in the form of his O_M and O_S values is perhaps most widely used, but Tideman's data and his analysis of Froude's experiments, the formula of Le Besnerais, Gebers' data, and, in some instances, Telfer's method of extension, are also used. The Conference agreed to use Edmund Froude's formula for skin friction for publication of results, but the actual coefficients are to be determined by a small committee of Superintendents. As already stated, the wetted surface is to be the product of the length of the curve of areas (this will include the cruiser stern in the length) and the mean girth. If, therefore, the length, the wetted surface and displacement, and the geometric particulars of the form are given, all the data requisite for estimating resistance and power will be supplied under the agreements reached at the Conference.

Although the Tanks have agreed to give full information on the performance of the models tested, each user will still be left with the task of putting the results in the most convenient form for his own limited applications, but he will have the satisfaction of knowing that all new data will be presented on the same basis. He will himself have to decide whether he should convert these data to a 100-ft. or a 400-ft. ship, or a model of 1 cub. ft., 1 cub. metre, 1 metric ton, or 1 ton displacement. It will probably be agreed that this is the right stage to which Tanks should bring their work; for they cannot hope to meet everybody's requirements. It will give rise to many difficulties, for the full skin friction correction will have to be applied from model to ship, instead of, in the case of the William Froude Tank data, from a 400-ft. ship to the actual ship length.

Skin Friction.—This is not the place to discuss skin friction. It is being dealt with in other papers to be read at these Meetings, but some reference should be made to some of the problems arising in testing models. A large amount of data is available on the resistance of planks within the range of model lengths, and more is being obtained. The chief problem affecting testing is the condition of the surface of the model and the state of flow at the lower speeds. The Conference decided that in order to obtain a uniform state of the surface of models, all paraffin models should be trimmed to have a reasonably smooth surface, and that any pores formed while the model is cooling should be filled with the wax and then rubbed down. In regard to flow conditions, the models should be large enough to establish the turbulent layer conditions without the aid of artificial means, which in themselves may give rise to uncertain states of flow. Roughening the fore-end of the model introduces conditions which cannot be standardized, and the use of a trip wire appears to be by no means constant in its operation.

Edmund Froude always took great pains to get standard conditions at the lower speeds in two ways: First, by having long intervals between successive runs, and by using current meters to eliminate Tank currents. Some published results show evidence of the intervals being insufficient, as evidenced by the different results obtained at the beginning and end of a day's experiments.

The Conference proposed to enquire into the region of uncertainty of the experiment results at low speed, and then to decide at what Reynolds' * numbers the results obtained should be used with caution. To indicate this on the published data the resistance constant curves will be shown by a dotted line.

Values of the O_M and O_S coefficients used at the Admiralty Experiment Tank for lengths beyond those published by Edmund Froude in 1888 are given by kind permission of Sir Arthur Johns, K.C.B., Director of Naval Construction, in Appendix III. These values are extended from 600- to 1,200-ft. lengths. This extension was made at the time experiments were being carried out by Edmund Froude for the Cunard liner models *Lusitania* and *Mauretania* at Haslar. For convenience, the values for model lengths are also given. It will be noted that the values for lengths below 600 ft. differ slightly from the values given in the 1888 paper.

Screw Propellers.—The forces experienced by a propeller working in any fluid will depend upon

ρ the density of the fluid;
V the axial speed of advance;
n the speed of rotation;
D the diameter of the propeller, or some other linear dimension—for example, the pitch P;
ν the absolute viscosity of the fluid;
p the absolute pressure of the fluid in contact with the propeller;
E the modulus of elasticity of the fluid;

and if the propeller is working near the surface of the fluid, although it may not actually cleave the surface, upon

g the gravitational force.

* Osborne Reynolds seems to have been the first to formulate the law of skin friction in a form similar in character to Rayleigh's law deduced from the theory of dynamic similarity. He showed that, for circular pipes, the specific resistance for turbulent as well as for viscous laminar flow was a function of $\frac{v\,D}{P}$, where v was the average flow velocity, D was the diameter of the pipe, and P was proportional to the coefficient of viscosity. In modern nomenclature this is expressed in the form $\frac{v\,l}{\nu}$, where v is the relative velocity of the body and the undisturbed surrounding fluid, l is a linear dimension of the body, and ν the absolute viscosity or the coefficient of kinematic viscosity. This quantity is known as Reynolds' number. It is non-dimensional.

The thrust T may then be expressed in the most general form by the equation

$$T = \phi\,(\rho, V, n, D, \nu, p, E, g) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(i)}$$

The principal condition to be satisfied in this equation when obtaining a law of thrust for similar propellers is that, whatever precise form the expansion of the function may take, each and every term of the expansion must have the same dimensions as the thrust T. Satisfying this condition, we find that the function can be expressed by

$$\frac{T}{\rho\, D^2\, V^2} = \phi\left(\frac{n\,D}{V}, \ \frac{V\,D}{\nu}, \ \frac{p}{\rho\,V^2}, \ \frac{g\,D}{V^2}, \ \frac{E}{\rho\,V^2}\right) \quad . \quad . \quad . \quad . \quad \text{(ii)}$$

in which each of the arguments $\frac{n\,D}{V}$, $\frac{V\,D}{\nu}$, etc., are non-dimensional.

For working within the limits of ordinary practice we can eliminate several of these arguments. For example, the change of density of the fluid due to compression will not arise until velocities, whether of rotation, circumferential speed, or advance, approach the velocity of sound in the fluid. The argument $\frac{V\,D}{\nu}$ is believed to be negligible* under ordinary conditions, so that we can rule out scale effect, for it will only enter into the problem at very slow speeds, or with very small propellers. This does not mean that scale effect can be ignored, for in certain types of ship it may be necessary to use propellers of 5 or 6 in. diameter, at which experimenters (Miss Keary in this country) have found that some correction for scale is needed. But if the diameter of model propeller used is 9 in. or more, the scale effect is believed to be negligible. $\frac{p}{\rho\,V^2}$ regulates the liberation of air in solution, and the formation of water vapour cavities. $\frac{g\,D}{V^2}$ can be neglected if the propeller is not near the surface, and since g is the same at all experiment stations we need not concern ourselves with it. We are thus left with

$$\frac{T}{\rho\, D^2\, V^2} = \phi\left(\frac{n\,D}{V}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(iii)}$$

for ordinary conditions, and

$$\frac{T}{\rho\, D^2\, V^2} = \phi\left(\frac{n\,D}{V}, \ \frac{p}{\rho\,V^2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(iv)}$$

if we have to consider a propeller working near the cavitation condition.

Equation (iii) may be put in another form:—

$$\frac{T}{\rho\,.\,n^2\,.\,D^4} = \phi\left(\frac{n\,.\,D}{V}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(v)}$$

or

$$\frac{T}{\rho\,.\,n^2\,.\,D^4} = \phi\left(\frac{V}{n\,.\,D}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(vi)}$$

The density of water is practically constant (see table of density in Appendix II.), but since air screws and marine screws are directly comparable, it is advisable to retain ρ.

Equation (iii) or (vi) would appear to be the more suitable for presenting the results of model screw experiments. Equation (vi) is the form recommended by the Hague

* See Jefferies, *Phil. Mag.*, Vol. 2, 1925, p. 815.

Conference. The same equation is adopted for air-screw work. The results may therefore be presented in tabular form, or by plotting the thrust coefficient $T_c = \frac{T}{\rho\, n^2\, D^4}$, to a base of $J = \frac{V}{n\,.\,D}$.

By a similar reasoning the torque Q may be expressed by

$$Q_c = \frac{Q}{\rho\,.\,n^2\,.\,D^5} = \phi\left(\frac{V}{n\,.\,D}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(vii)}$$

$\frac{V}{n\,.\,D}$ is proportional to the ratio of the speed of advance to the circumferential speed of the propeller tip. It thus has a convenient physical meaning, although it will not have any important significance until conditions require the use of equation (iv).

Equation (vi) may be expressed in words by, "for geometrically similar propellers under conditions in which there is no surface disturbance or incipient cavitation, T_c is constant if J is constant."

For similar propellers of pitch-diameter ratio r,

$$\frac{V}{n\,.\,r\,.\,D} = \mathrm{I} - s \quad \text{and} \quad \frac{V}{n\,.\,D} = r\,(\mathrm{I} - s) = J$$

where s is the slip ratio. The value of J decreases as slip ratio increases, and when $s = 1 \cdot 0$ then $V = 0$. In that case equation (vi) reduces to

$$\frac{T}{\rho\,.\,n^2\,.\,D^4} = C \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(viii)}$$

where C is a number which is constant for a given propeller and for similar propellers. The value of this constant is obtained by a static test. It serves the useful purpose of terminating the curves of T_c for $J = 0$.

It is worth noting Edmund Froude's expression for thrust which took the form

$$\frac{T}{n^2\,.\,D^4} = a\,.\,s$$

where a is a coefficient depending on density of the fluid, the type of screw, pitch ratio, and blade-width or disc-area ratio for a propeller of unit diameter; s is the slip ratio from the pitch for zero thrust. Froude systematized his methodical series of screw experiments by tabulating the values of a for different pitch ratios and disc-area ratios.

From the thrust and torque constants T_c and Q_c the expression for efficiency at once follows, for

$$\text{Efficiency} = \eta = \frac{T_c \times V}{Q_c \times 2\,\pi\, n\, D} = \frac{1}{2\,\pi} \cdot \frac{T_c}{Q_c} \cdot J \quad . \quad . \quad . \quad . \qquad \text{(ix)}$$

The Hague Conference decided to provide corresponding values of T_c and Q_c for different values of J, in the form either of tables or curves. Coupled with a complete drawing of the propeller, this will give the designer all the information about that propeller. It will enable propellers to be readily compared. The method * will probably meet with general approval, but it will not help the designer very much in selecting the propeller for a particular design. He will himself have to do the work of assimilating all the propeller data published by the Tanks if he desires to design propellers rather than ask the Tanks to select them for him.

* For a pictorial representation of the method, see Baker, Trans. I.N.A., 1934.

And here the main problem for the user of these data will arise, for it has become almost as difficult to classify propellers as it is to classify ships' forms, owing to the varying blade sections and pitch lines adopted with recent propellers. We can no longer make an estimate of the pitch with sufficient accuracy if it has aerofoil sections over one portion of the blade and plano-convex or double-convex sections over another. Published data, so far as I have seen, do not seem to include a comprehensive series of model propellers, such as Froude's, or Taylor's, or Schaffran's, covering a wide range of pitch ratio with the same type of section. There is much data on isolated propellers with different sections. It is now becoming more and more the practice to design the screw, and then test it in the model for revolutions and efficiency. In the proposed method neither can the face pitch be defined (except for screws with a helical face), nor is the effective pitch (for zero thrust) measured. It is thus not the simple matter that it was, when screws with helical faces were used, to select the propeller to give the revolutions in the ship suitable for the design of engines. It does seem desirable that the effective pitch should be measured or estimated by the Tanks, and this value given with the other particulars.

Self-propelled Model Tests.—A discussion of the technique, which has been built up at the different Tanks for the conduct of self-propelled model tests, hardly comes within the scope of this paper on nomenclature. Nor, indeed, have I the necessary acquaintance with the problem, or experience, to put the problem before this gathering in a satisfactory manner. At the same time I think it is permissible for me to urge that just as there is a necessity for the Tanks to present the results of their experiments in a common form, so is there a necessity for a great measure of uniformity of technique. For example, a shipping company for an important design outside the field of "full size" experience may think it necessary to have tests made in two Tanks. If the techniques differ considerably, the results submitted by the two Tanks may prove very puzzling to a shipowner. Uniformity of technique does not involve uniformity of apparatus, for if the apparatus is properly calibrated, and sufficient measurements of friction in the apparatus taken, the results should be consistent and comparable.

The data which Tanks propose to supply to the general public from time to time, and the form in which they are to be supplied, as described here, show the extent to which we can reasonably expect co-operation for the time being. It is a very creditable attempt to simplify matters for the Tanks themselves (the most important aspect of the problem, for they are the people who "provide the goods") without hampering in any way those who are willing to make a serious attempt to understand a universal language. My personal view is that individuals, particularly those who are not subscribers to the upkeep of research stations, cannot expect more, nor can they expect to get the fullest information from those Tanks, which rely for their maintenance upon voluntary contributions and earnings, or where they are not maintained fully by the State. At the same time, subscribers would have very good claims for preferential treatment in the matter of supply of data.

It may not be out of place on an occasion like this to make some comments on the general aspect of the nomenclature. I believe that the system which Edmund Froude initiated for ships' forms will in its general character be the one likely to prove of greatest use to the industry of shipping and shipbuilding, because it is the one of greatest service to the Tanks which supply the information. In detail it may vary, but when the Superintendents of Tanks agree that it is best for general use to codify the results of their experiments in some "constant" form, and to present them in that form, together with full particulars of the models used, I suggest that users of published data should learn the language of the Tanks. A large and increasing number of people in this country are appreciating the language of Edmund Froude, and are experiencing its advantages. They, at any rate, will have no difficulty in assimilating the small changes which are now proposed by the Hague Conference. In relation to screw propeller data, so much has been published about "air screws" in a "constant" notation which has a direct bearing on marine screws that for this reason, if for no other, it is advisable to present marine data in a similar manner. Here, again, it seems to me that assimilation of all this data is a matter for the user alone, because of the particular applications with which they themselves are concerned. And I cannot do better than end as I began, by referring to the quotation of Edmund Froude cited at the beginning of my paper.

At the Hague Conference it was agreed that the publication of model data should follow certain lines for one year. That year is now past, and it is sincerely to be hoped that since this initial step has been taken, and data have already been published on the lines proposed, the work commenced at the Hague Conference should be continued, and should be sealed by the appointment of a small committee of Tank Superintendents to complete their work. The completion involves the publication of a pamphlet setting out the form in which all data are to be supplied for general publication, together with tables of symbols, coefficients, physical constants, and conversion tables for different units of measurement. No doubt it would be necessary to invite subscriptions to provide money for such a publication, but it would be a fitting memorial of these conferences if money could be found for the purpose at once.

APPENDIX I.

The Hague Conference of Tank Superintendents, 1933.

Extracts from a letter addressed to the Superintendents of experiment Tanks by the Council of the Nederlandsch Scheepsbouwkundig Proefstation, Wageningen, Holland, convening the Conference.

"During the Hydro-Mechanical Congress held in Hamburg in May 1932, Mr. John De Meo pleaded . . . for international co-operation in the field of ship propulsion.

. . . . .

"The Council of the Dutch Tank, in agreement with the above, have the honour to invite you to a Conference of experimental tank superintendents to be held in The Hague on the 13th and 14th July, 1933.

. . . . .

"That these discussions will be continued is assured by the fact that the I.N.A. of London intend during their Summer Meeting in 1934 to devote the greater part of their programme to subjects dealing with Tank research work. . . .

"The meeting in Holland is to be regarded as a first step to reaching a standard system of publication of tank results.

. . . . .

"(Sd.) Ir. L. Troost,
"Directr. Nederl. Scheepsbouwkundig Proefstation,
"Wageningen, Holland."

Representatives of the following Tanks attended the Conference:—Dumbarton, Washington, Berlin, Paris, Vienna, Charlottenburg, Tokyo, Teddington, St. Albans, Hamburg, Rome, Wageningen.

Extracts from the DECISIONS *reached at the Conference.*

The decisions are to remain in force till the next meeting in 1934.

On the dimensions and finish of models.

(*a*) The dimensions of the model should be sufficiently large to ensure that the boundary layer is formed over the major portion of the hull, so that the fitting of a trip wire, or the use of a local roughening on certain portions of the entrance, can be dispensed with. In this connection a committee consisting of Mr. Baker, Professor Barillon, Dr. Kempf, and Mr. Troost will try to fix minimum Reynolds' numbers for different cases.

(*b*) All paraffin models should have any pores filled so as to produce a reasonably smooth surface all over in all cases.

On the determination of length and wetted surface.

(*a*) For vessels with cruiser stern the length on the water-line should be used. For vessels with raised stern the length is to be taken to the after-side of the rudder-post.

(*b*) The wetted surface is to be obtained by multiplying the mean girth by the length.

Physical constants.

The table of Lyle and Hosking is accepted for the kinematic viscocity of fresh water.

Skin friction.

It is agreed that Froude's method of calculation shall be used with R. Edmund Froude's friction formula. The exact friction coefficients will be fixed by the committee after replotting the original coefficients.

It is agreed that all model results shall be corrected to a standard temperature of 15° C = 59° F. by a correction of 0·43 per cent. of the frictional resistance per 1° C., or 0·24 per cent. per 1·0° F. (No correction will be made for varying temperature of the sea on trial.)

On the presentation of model results.

Particulars of the presentation of model particulars as proposed by the Wageningen Tank were approved.

The results of resistance experiments are to be given in one or more of the "constant" systems.

The curves of the "constant" are to be drawn dotted below values of Reynolds' numbers to be decided by the committee.

Screw propeller tests.

To be presented in the manner described in the paper.

Instead of the developed area, it is agreed to use the expanded area taken outside the boss line.

APPENDIX II.

Physical Properties of Fresh and of Sea Water.

(a) Relative Density (Specific Gravity) and Absolute Density.

g at Kew = 32·191 ft./sec.^{2}

Fresh Water.

Temperature, °C.	Relative Density.	Density.	Weight in Oz./Cub. Ft.
4	1·000	1·9415	1,000
10	0·99973	1·941	999·73
12	0·99953		
14	0·99927		
15	0·99913	1·9399	999·13
16	0·99897		
18	0·99862		
20	0·99823	1·9381	998·23
22	0·99780		
24	0·99732		
26	0·99681		
28	0·99626		
30	0·99567	1·9331	995·67

Value of g in cm./sec.^{2}

Kew	981·2	Berlin	981·29
Glasgow	981·56	Rome	980·32
Portsmouth ..	981·14	Vienna	980·91
Washington ..	980·097	Petrograd ..	981·91
Paris	980·95	Leyden	981·36
Potsdam ..	981·25		

Sea-water.

The density of sea-water depends upon its salinity. The salinity is defined as the number of grammes of mineral salts per kilogramme.

Table of relative density referred to fresh water at 4° C.

It equals the ounces per cubic foot ÷ 1,000.

Temperature in° C.	Salinity.		
	30.	35.	40.
0	1·02411	1·02813	1·03217
5	1·02375	1·02770	1·03167
10	1·02308	1·02697	1·03088
15	1·02215	1·02599	1·02985
20	1·02099	1·02478	1·02860
25	1·01960	1·02337	1·02715
30	1·01801	1·02175	1·02551

The salinity of ocean water varies from about 34 in the Polar seas to 37·5 near the Tropics. It reaches a maximum in the southern portion of the North Atlantic at 37·5. The salinity in the Pacific is appreciably less than in the Atlantic.

(*b*) *Absolute or Kinematic Coefficients of Viscosities.*

Hosking and Lyle measured the absolute viscosity of fresh water free from air. Their results are given in *Phil. Mag.*, 1902, Vol. 1, p. 493. Subsequently Hosking made a number of control experiments, and determined the viscosity at three temperatures, 0·05° C., 25° C., and 50° C. Knibbs (Roy. Soc. of New South Wales, Vol. XXXI.) determined the viscosity at 10° C. These results are given in *Phil. Mag.*, 1909, Vol. 2, p. 260, in c.g.s. units. They are as follows:—

0·05° C. = 0·017897; 10° C. = 0·013107; 25° C. = 0·008926; 50° C. = 0·005500.

From these four results the values at other temperatures were computed and compared with all Hosking's experiments.

The values for sea-water given by Lees in the Transactions of I.N.A. (1916) appear to be Hosking and Lyle's results for solutions of common salt. These figures are given below, but, in addition, further values quoted by Dr. Otto Krümmel in his *Handbuch der Oceanographie* for sea-water of different salinities are also given.

Temperature, ° C.	Fresh Water.		Sea-water.			
			Salinity.			Lees, Trans. I.N.A. Specific Gravity, 1·026 at 0°.
	Hosking.	Lees, 1916.	30.	35.	40.	
0·05	0·017897					
0·0	0·017928	0·01794	0·018735	0·01886	0·018988	0·01780
5	0·01522		0·015974	0·016100	0·016225	
10	0·013105	0·01309	0·013841	0·013946	0·014073	0·01318
15	0·01142	0·01144	0·012101	0·012227	0·012334	0·01158
18		0·01063				0.01074
20	0·01006	0·01011	0·010729	0·010846	0·010954	0·01025
25	0·008926		0·009556	0·009663	0·009771	
30	0·00800	0·00806	0·008623	0·008713	0·008803	0·00825

APPENDIX III.

Note on the Skin Friction Coefficients of R. Edmund Froude.

The values of the skin friction coefficients in the constant form O as deduced by R. Edmund Froude from the results of the plank experiments conducted by William Froude are given in Trans. I.N.A., Vol. XXIX. (1888), p. 313. The values of O which were given for lengths of model and ship from 8 ft. to 600 ft. were in regular use at Haslar until 1904. In 1904 it was found necessary to extend the data to lengths greater than 600 ft., and in the course of the extrapolation R. Edmund Froude found that an exponential formula approximately meaned the then accepted values of the skin friction coefficient for lengths of 250 ft. and upwards. This formula gives values agreeing exactly with the original values at lengths of 275 ft. and 535 ft. respectively, and did not anywhere differ from the original values by an amount of importance for lengths above 250 ft. Values of O for lengths above 250 ft. were worked out by this formula and have been in use since. The values are given in the table below in comparison with the original values.

Table of Values of O for Various Lengths.

Length in Feet.	Value of O.	Length in Feet.	Value of O.	
8	0·14090	120	0·08511	
9	0·13734	140	0·08351	
10	0·13409	160	0·08219	
12	0·12858	180	0·08108	
14	0·12406	200	0·08012	
16	0·12035	250	0·07814	
18	0·11727	300	0·07655	
20	0·11470	350	0·07525	(0·07523)
25	0·10976	400	0·07412	(0·07406)
30	0·10590	450	0·07312	(0·07305)
35	0·10282	500	0·07219	(0·07217)
40	0·10043	550	0·07132	(0·07136)
45	0·09839	600	0·07051	(0·07062)
50	0·09664	700	—	(0·06931)
60	0·09380	800	—	(0·06818)
70	0·09164	900	—	(0·06724)
80	0·08987	1,000	—	(0·06636)
90	0·08840	1,100	—	(0·06561)
100	0·08716	1,200	—	(0·06493)

Note.—The values of O are (with the exception of those in brackets) the same as those published by R. Edmuud Froude in the Trans. I.N.A., Vol. XXIX., p. 313.

The values of O in use at Haslar since 1904 are as given in the above table for lengths of 300 ft. and below. For lengths above 300 ft., the values used are as in brackets.

DISCUSSION.

Mr. G. S. Baker, O.B.E. (Member of Council): At the Hague meeting of Tank Superintendents in 1933 certain broad agreements were reached and many matters connected with Tank technique were discussed, as summarized in Professor Abell's paper.

A meeting of the Special Committee set up to settle details and to discuss the future was held at Teddington yesterday. The values of kinematic viscosity for fresh and sea water have been agreed to. The temporary acceptance of Froude's method of correction for skin friction and his O values for this purpose has been extended until further information is at hand. The frictional coefficients to be used in Froude's formula $R = f\,A\,V^{1 \cdot 825}$ have been worked out from the O values as recorded in this paper, and will be adopted. The agreement on the temperature correction will be continued, and it is proposed that the particulars of any hull shall be given in an agreed form. These data will be circulated to all Tanks with a request that they adhere to them in their work and it is proposed to print them as soon as we are in agreement on all the points involved.

There was general agreement in the Committee that model results below a certain Reynolds' number are unreliable, but our data are not sufficient to fix this number and its dependence on certain variables such as length, fullness, etc. Its lowest value is believed to be about 3×10^6, but other higher values have been suggested—Professor Bairstow in his paper gives 8×10^6. If for any reason a sufficiently high Reynolds' number cannot be obtained, it is recommended that when the speed of a model is below that corresponding to this minimum Reynolds' number the resistance curve and data based on it be marked in dotted lines.

It has also been agreed that the Hamburg, Wageningen, Paris, Teishinsho, and Froude Tanks will try from five to ten models during the next year, first with the usual surface and then with a specified serrated band in the fore-body to ensure setting up turbulent flow. Dr. Kempf has agreed to make and to supply any Tank with the necessary tool for this purpose to ensure that these shall all be alike, and the serrations shall be the same. The results will be compared and analysed, ready for next year. It is hoped that other Tanks represented here to-day will take part in this effort to settle this matter.

A method of giving the particulars of a screw propeller was generally agreed to last year and is contained in Mr. Troost's Memoranda on those Meetings. It is suggested that for the sake of completeness this shall also be reproduced in the proposed publication. Where a very large number of screws are described, as in my paper, the case is not so simple, and although my method of characterizing blade sections has obvious weak points, no better one was suggested, and it is proposed to continue it in the National Tank. The feeling in some cases was that a reference to a publication of one of the aeronautical establishments, containing particulars of a blade of any particular shape, would be better—for example, Göttingen A 6, or R.A.F. 4—but no decision was reached. In my opinion this would involve each shipbuilding and propeller-making establishment keeping all the publications of all the different aeronautical research establishments. In England I should imagine that not more than 2 per cent. of such firms would be in possession of such data, and it seemed rather useless to adopt this method.

Mr. L. Troost (Member): I would like to make a few remarks upon the history of co-ordination and co-operation in Tank work. In 1932 pioneer work was done by Dr. Förster and Dr. Kempf in Hamburg. There was a great Congress on hydrodynamics, and at a dinner of the Congress Mr. John de Meo, a well-known member of our Institution, made a speech in favour of international technical co-ordination. It was in answer to that speech that the Wageningen Tank invited those Superintendents who were interested in collaboration and co-ordination to come to Holland for a meeting at The Hague which was held last year.

The more science tries to clarify the problems relating to ship resistance and propulsion, the more it is generally felt that collaboration amongst the experiment Tanks is essential to the advancement of the art and improvement of working methods.

Since William Froude carried out his pioneer work, the various Tanks have worked almost independently in developing their mode of experiments and methods of deducing ship resistance from that of models. I remember Dr. Tideman running his models in the Amsterdam Tank two years later than William Froude. Dr. Tideman had already developed his own constants which are used by some Tanks up to the present time. So that you will see that when there was no collection of data there were different views on the subject. William Froude was the originator and Tideman was one of his first imitators, and since then each Tank has collected a large amount of data which have been filed in records for the general information of Tank authorities.

For comparative purposes it is necessary to bring model resistance into some standard form, or subdivide it into skin friction resistance and residuary resistance by the application of a temperature correction and a skin friction formula. The publications of the various Tanks demonstrate that both of these vary from Tank to Tank, so that it is evident that the results depend on the calculation made, thus showing that direct comparison is only possible if exactly the same methods of calculation and presentation are adopted. In any case, the original measurements, which have a lasting value, are lost in presenting the results in a corrected form. It is a matter of importance that the original experiment data should be presented in the same form. That is the reason why the Superintendents of various Tanks came together in order to take, in one form or another, decisions regarding the presentation of model data.

We have to thank Professor Abell for the very admirable way in which he has shown us the outcome of last year's meeting—which was a domestic one—to the general public here. I am sure that I can speak in the name of my colleagues when I suggest that Tank users should give us comments and express their wishes to the Superintendents of Tanks regarding the co-ordination of Tank work; for the Tanks are working primarily for those who have models tested, and what we do must be in accordance with the wishes of the practical men who have to make use of the data.

Sir Archibald Denny, Bart., LL.D. (Hon. Vice-President): I have taken no very active part for some years in Tank work, but I suppose I am the oldest man present who has had anything to do with Tanks—a sort of father to you all.

My brother, William Denny, persuaded his partners to construct a Tank in 1881, and the first experiment was made in February 1883. I had just finished my course at the Royal Naval College the year before, and owing to my brother's illness I was thrown into the work as a young and largely inexperienced man to take over what he had been doing. Since then our Tank has been altered in length. It was originally 300 ft., but 50 ft. was used up in two small docks, one at each end. A fire occurred in 1924, and we took advantage of the fire to increase the running length by 55 ft. Altogether we have made 211,000 experiments from single observations, and the number of models is 1,200. So a very considerable amount of work has been done there.

My brother read a paper at the British Association Meeting in 1875, in which he proposed, I believe for the first time, that instead of simply one trial showing what the ship could do at her maximum speed, there should be several speeds—progressive trials in fact. Wm. Froude in 1876, in papers read here, used the data with which my brother supplied him of a ship called the *Merkara* and of two other ships built by Dr. Inglis. He used the data to show how Tank work was dependent upon observations made on the measured mile, and demonstrated the value of progressive trials.

Our firm has sometimes been blamed for not publishing more Tank data. But I think that is hardly fair, because if you look at our Transactions you will see that whenever such work has been described we have generally made our contribution to the discussion.

In the year 1888 when we tried the *Princess Henrietta* and the *Josephine*, the first two outstanding fast paddle vessels, nearly 300 ft. long, of over 21 knots speed, Dr. John Inglis, who was a little cynical, remarked: "Now I know what a very fast paddle boat I can build." So if we did not publish all our data we at least published the results obtained from the Tank, and thus other shipbuilders knew what was possible and what was probable.

Wm. Froude adopted a small truck with a narrow gauge, and we naturally followed him. Dr. Purvis, whose health will not permit of his being here—I suppose he is the oldest man who was a pupil of Wm. Froude—told me that Wm. Froude's ingenuity and mechanical ability in the use of his hands was perfectly marvellous. He practically constructed the truck himself, and it was made largely of wood in box-section girders. We adopted the narrow gauge, and, so far as I am aware, if we had to build another Tank we would use the same gauge; the advantage being that we have a light truck, acceleration is very rapid, and we have not found any disadvantages. All the modern Tanks have got carriages completely spanning the Tank. Our Tank is 22 ft. wide. It is similar to most of them, and the depth of water is 8 ft. 9 in.

In regard to Professor T. B. Abell's paper, I think it is a very valuable one, and I would ask the Tank Superintendents not to make any more changes in constants and nomenclature than they can help.

I did not quite understand why you have turned Δ upside down. You have called it volume. When I looked at the paper first I could not make out what this sign was. The symbol Δ for displacement has been sanctified by years of use. After all, it is only the presentation of data in public that we are discussing. Each Tank will naturally keep its data, as we do, in its own original form. In fact, all our data are based on ships 100 ft. long. I have a sample sheet showing how we keep our results, if any of you would care to see it. We do not propose to depart from that, but if we present the data we would naturally like to do so in a form generally agreed upon.

Professor H. R. MØRCH (Member): I must admit that I was rather disappointed on hearing from Mr. Baker that the four Tank Superintendents had not agreed on the minimum values of Reynolds' number for testing different types of ship models, because smaller minimum Reynolds' numbers mean smaller models and consequently smaller testing channels, and therefore less expensive Tanks. We hope in Norway next year to start building a large model Tank which for the last twenty years has only existed on paper. In order to settle the final dimensions of the Tank, the knowledge of minimum Reynolds' numbers would be of great value.

Some twelve years ago I had to look into the question of the size of such a model Tank. The transverse dimensions were fixed at 36 ft. breadth and 19 ft. depth of water. These dimensions were fixed after laying down the three following conditions which had to be fulfilled, viz.:—

(1) The product of length in feet of ship model and the speed of same in feet per sec. should be at least 50.

(2) The influence of the walls and bottom of the channel on the total resistance of the ship model should not exceed 2 per cent.

(3) The model propellers to be used in connection with the ship models should in no case be less than 6 in. diameter.

With these three conditions as a basis, I worked out the necessary dimensions of ship models and propellers for the various types of ships we have in Norway. The result showed that the three conditions were fulfilled except in regard to our small fishing boats and tug-boats.

For my purpose I would like to have heard from these four experts what minimum Reynolds' numbers could be safely used for calculating the dimensions of our model Tank.

Dr. Ing. Renato de Santis (Associate-Member): I entirely agree with all the proposals to secure international co-operation on the publication of the results of Tank work. I should like to add that since 1931 the Rome National Tank has issued Annual Reports in which is given full information about its Tank experiment results. Our next Annual Report will follow the preliminary decisions reached at the Hague Conference.

Mr. H. G. Williams, O.B.E. (Member): It is gratifying to those of us who are interested in the use of model experiment results for practical purposes to know that the experiment Tank Superintendents have agreed on a uniform system of presenting their results. This uniformity will greatly facilitate the comparison of data from different sources and make their use much more convenient.

In spite of the author's disclaimer of expert status in experimental Tank research, I am sure that no one is more capable than he of interpreting the scientist to the practical man, and, in particular, of making clear to us, as he has done in this paper, the decisions of the Tank Superintendents with the reasons for and the implications of those decisions.

There are a number of points in the paper which appear to me to invite comment after more careful consideration than can be given at a first hearing of the paper; but one in particular seems to me to be so important that I must venture on comment without perhaps the full consideration which should be given.

I notice on page 265 after a statement of the forms in which total model resistance will be expressed, that "V is the speed in knots," and in the next paragraph, "No particulars of units are given here except when the unit used is international."

The forms referred to are evidently intended to be non-dimensional, i.e. to have the same numerical value when their various complex factors are expressed all in the same fundamental units of length, time, and mass. If, therefore, V is the speed in knots, L must be the length of the model in nautical miles, g the acceleration of gravity in nautical miles per hour per hour, and ρ the number of units of mass in a cubic nautical mile. As it seems extremely inconvenient to use such units of length and time for the dimensions of the model, the acceleration of gravity, etc., the wording of the sentences I have quoted from the paper rather suggests that the intention is to express the speed V in knots, but the dimensions of the model, the acceleration of gravity, etc., in the customary metre per second or foot per second units. Should this be the case, it will destroy the non-dimensional character of the forms, and perpetuate the practice which, I believe, has prevented the universal adoption of Mr. R. E. Froude's "Constant System of Notation."

It is a long time since I have seen Mr. R. E. Froude's paper; but I think I can trust my recollection to say that he stated therein the correct principle on which the "constants" should be constructed, namely, by the use, for all measurements entering into them, of any consistent system of fundamental units, and that he gave a sound method of applying that principle. Nevertheless, in view of the fact that the practical man always thinks of the speed of a ship in knots, Froude introduced the knot into his "constants," thereby making his fundamental units inconsistent and destroying the non-dimensional character and therefore the universality of the "constants." I hope history is not going to repeat itself. Scientists seem to have altogether too low an opinion of the capabilities of practical men. I think none of us, even the most junior apprentice draughtsman, has much difficulty in converting feet per second into knots or *vice versa*. At any rate, if the scientists think otherwise it would be better that they should give us a comprehensive conversion table of knots into metres per second and feet per second than that they should destroy the universality of the forms in which they express their results by using inconsistent units in deference to our supposed limitations. I note with a certain satisfaction the forms (equations (vi) and (vii) on pages 268 and 269) in which the Tank Superintendents have decided to present the results of model propeller experiments, as these are the forms to which I have converted propeller data from the time of the publication of Admiral Taylor's first experiments onward, at the cost of very considerable arithmetical labour. These forms or, better still, their equivalents $\frac{(\text{T.H.P.})}{\rho\, n^3\, \text{D}^5}$ and $\frac{(\text{D.H.P.})}{\rho\, n^3\, \text{D}^5}$, are those required for the analysis of ship trial results, in which the propeller diameter and form are known and the speed of the propeller through the water is the quantity to be found.

As the author remarks, they are not forms which are appropriate to the process of propeller design, in which the speed of the propeller through the water is given and the propeller diameter is the unknown quantity. I suggest that the experiment Tanks in publishing their propeller results should add to the "constants," which the Conference of Tank Superintendents has decided to adopt, two others not containing the propeller diameter as a factor. These might be of Admiral Taylor's form, namely $\frac{n\,.\,(\text{T.H.P.})^{0\cdot 5}}{\rho^{0\cdot 5}\,.\,\text{V}^{2\cdot 5}}$ and $\frac{n\,.\,(\text{D.H.P.})^{0\cdot 5}}{\rho^{0\cdot 5}\,.\,\text{V}^{2\cdot 5}}$, or some equivalent, plotted on $\frac{\text{V}}{n\,.\,\text{D}}$ as abscissa.

With the experimental thrusts, torques, speeds, and all other data in their possession, it should be a comparatively easy matter for the Tanks to produce these "constants," whereas production of them by a person only in possession of other "constants" given on a small-scale diagram is laborious and probably inaccurate.

The paper is very interesting from both its subject-matter and the skill with which the latter is presented, and I hope more at leisure to give to it the study it deserves.

Monsieur E. G. Barrillon: I would like to offer a few remarks based more upon what previous speakers have said than upon the paper itself. The Hague Conference certainly did not say that V was to be expressed in knots, nor that g was a force, and we need not discuss these details.

To Mr. Baker's explanations I would add that the understanding arrived at regarding a standard of skin friction adopted by some Tanks must not be understood to restrict this investigation to the Tanks represented at the preliminary meeting held yesterday. On the contrary, it is very desirable that as many Tanks as possible should carry out tests after agreeing on a uniform method of procedure.

As to the fears expressed by Professor Mørch regarding the limitation to a minimum Reynolds' number, I would only say that we never suggested that lower Reynolds' numbers should not be used. We merely pointed out that those portions of the curves relating to Reynolds' numbers lower than the minimum should be indicated by dotted lines.

On the subject of non-dimensional units used in propeller tests, I would observe that one dimension of length is necessary, and this can only be either diameter or pitch. There are a whole series of pitches in a propeller: several geometrical pitches, four dynamical pitches (no-thrust either forwards or backwards, and zero couple), whereas there is but one diameter. The retention of the diameter as a unit is therefore fully justified.

Mr. E. Wilding, C.B.E. (Member): As one of the senior assistants to the late R. E. Froude who are taking part in the meeting I felt, when reading the paper, that he would certainly have approved of it. It carries out his ideas much more elaborately and much more completely than was possible in the year 1888 when he wrote the original paper, and the general character of the presentation which is now agreed to by the International Tank Superintendents is very much on the lines which R. E. Froude originally tried.

Professor Abell makes a comment regarding the skin friction coefficient curves, in particular the extension of the curves for lengths over 600 ft. He is quite correct in saying it was caused by the preliminary investigations of models that eventually developed into the *Mauretania* and *Lusitania*. At that time the skin friction curve did not extend beyond 600 ft., and the longest ship we had then used it for at Haslar was 500 ft. b.p., and the problem of passing to a 750-ft. ship was one which we had to take into account.

The curve O_S has always been based on the original William Froude plank experiments to 50 ft. length made somewhere about 1872, published soon after that time. Then the attempt was made to extend them, checked by the results of the towing of the *Greyhound*, to full-sized ships up to about 400 ft. The extension, so far as I could learn, was made largely by eye—that is, by the spring of a batten through the original spots to get a fair curve. This certainly was the method used in 1886 when the Tank was transferred from Torquay to Haslar, when the curve was extended to 600 ft. In 1902, as mentioned, the question of extending to greater lengths came up. Froude discussed with me the possibility of doing this for a short length of 150 ft. in the same way. But I thought it might be possible to find a definite equation for the curve, and it proved eventually possible to get an exponential expression to fit the curve from 200 ft. to 600 ft. I have here the original plotting discussed with Froude, which proved that it was possible with some very small modification of the existing values to fit a very simple formula. It was found that the value of log $(O_S - 0{\cdot}065)$ plotted on a base of length was a straight line; and it is much easier to produce a straight line accurately than a curve. Froude approved it, and that was adopted. It has no other authority than that it proved to fit, from 200 ft. to 600 ft., the previous extensions very closely.

I want to make the point clear, as I note that Froude's data are now being accepted internationally. We are still based, so far as I know, on the original experiments of William Froude on planks which did not extend beyond 50 ft. in length, and all this extension is purely empirical. The only steadying point that William Froude or R. E. Froude had for their further data was such analysis of trials as were available and the results of the *Greyhound* towing experiments. If further evidence comes forward from any suitable towing or other data showing that a different curve is needed for the greater lengths, I feel sure that Mr. R. E. Froude would have been the last person to raise any objection to it, because he was perfectly aware of the fact that the logarithmic curve, which I found fitted the original extension by eye and enabled him to extend it further, was a purely empirical affair and had no experimental or mathematical basis. But after all, it is more important that the data of all the Tanks should be consistent than that they should be minutely accurate.

There are two other points in the paper that I think might be mentioned, rather in connection with Professor Abell's comments than Froude's original selection of constants. It is suggested that the published results of experiments should be for the naked hull, exclusive of any appendages. I would like to add my personal belief that that is correct, because any such appendages in large ships become almost unmanagably "small" in models.

I think also that Professor Abell does not exaggerate when he says (page 263): "I think we may be losing valuable information if we confine ourselves to self-propelled models, for we are there measuring too many things at one time." I do not suggest that it is wrong to publish such results, or to confine ourselves to self-propelled models, but it is a weakening of our data, because with the self-propelled model we are dealing not only with several things at once but with the complicated interaction of several influences without any attempt to disentangle them.

Mr. M. P. Payne (Member): *Professor Abell is to be congratulated on his eloquent and lucid plea for the universal adoption of a consistent nomenclature for the presentation of model data. While it is true that R. E. Froude's notation in its complete form has not commanded that general adoption he had hoped, the system is, nevertheless, a logical, simple, and useful one, and, with minor additions, has stood the test of fifty years' experience on Admiralty work. It is clearly a great tribute to the nomenclature initiated by Froude

* Remarks read by Mr. R. W. L. Gawn.

that the alternative now proposed by the Congress of Tank Superintendents for universal acceptance is, like Froude's, generally non-dimensional and differs from Froude's in certain constants only by the arithmetical multiplier.

The geometrical and form constants are the same as Froude's and the length and speed constants differ in that Froude related the speed to that of a hypothetical wave, whereas in the proposed alternative the corresponding coefficients have more simple multipliers and are accordingly to be preferred, as long as they are non-dimensional. Similarly the nomenclature proposed to indicate propeller performance is comparable to that used by Froude.

Turning to matters of detail, I should like thoroughly to endorse Professor Abell's remarks on page 267 as to the necessity of providing adequately long intervals between successive runs, and on the utility of current meters. We have always used the logs despite the amount of care and maintenance required for accurate readings, and consider that we have been well served by them. In many resistance experiments the current is often negligible, but in screw propeller experiments we find the current correction to speed is invariably important. Another routine initiated by R. E. Froude is the correction of the results by reference to a standard model, a procedure for which we have frequently had occasion to be grateful.

Then with regard to Professor Abell's comment on effective pitch, it is very difficult to measure it accurately, but in these days, when propeller pitch changes from the root to the tip, it seems that the only reliable measurement of its power-producing capacity is the relation of effective pitch to the diameter.

Mr. E. V. Telfer, D.Sc., Ph.D. (Member): On page 265 Professor Abell summarizes the resistance and speed constants authorized by the Hague Conference. But the first coefficient is a displacement coefficient and must be associated with the second speed coefficient, which also involves displacement. The first speed coefficient cannot be used with the first resistance coefficient. That leads to an interminable misunderstanding in the selection of suitable model forms from systematic data. The point arose, for example, in Dr. Todd's paper of two years ago where Ⓒ values were plotted to a base of Ⓟ. That particular formula destroys the relative value of the various models in the family. I pointed it out, and Mr. Baker submitted a diagram showing cross-contours of Ⓚ which could then be used to determine the order of superiority of that family. I hope, therefore, when the Hague Conference results are accepted by experimenters, that the Hague resistance constant will always be used with the speed displacement constant, and when we are discussing the specific resistance we would then use the Froude number as used now in Germany—that is to say, the length speed constant with the surface speed constant.

I am sorry no reference has been made to the plotting of the specific resistance to the basis of Reynolds' number. I am sure it is very much more important that we should have it put down in a form in which the professional man can examine its accuracy. That is the first thing he must satisfy himself upon, and there is no doubt that going round the experimental Tanks the accuracy in the past has left a lot to be desired. We therefore should have the resistance numbers available with the Reynolds' numbers. The subsequent building of them on a resistance basis will come afterwards.

Professor T. B. Abell, O.B.E., M.Eng. (Vice-President): Personally I am very pleased I made the effort to write this paper, because it has given the Institution an opportunity of getting valuable information.

I really think the conclusions of the Tank Superintendents and the results of their Conference should be published, if possible, in a pamphlet form for general use, so that anyone who desires to have this information can use it to their own satisfaction.

Mr. Troost added some historical particulars for the calling of the Hague Conference. I should have liked very much to have published the whole of the letter convening the

Hague Conference in my Appendix II., but I contented myself with just giving the principal facts and the origin of the actual Conference. I did not state the matters which were discussed there because I gave the conclusions of the Conference, and I thought that would draw attention, in an indirect way, of course, to the actual business of the Conference.

I am somewhat alarmed at Professor Barrillon's statement that the speed in knots has been incorrectly quoted. I must have read the conclusions of the Hague Conference wrongly. Although I well knew that the intention was to take non-dimensional constants, I still thought it was the decision of the Conference to use the term V in knots.

I am very glad that Mr. Wilding has been able to give the origin of Edmund Froude's extension of William Froude's plank experiments and the *Greyhound* experiments to the figures and lengths of 1,200 ft., which I have been permitted to give in Appendix III.

Dr. Telfer raised a point about plotting the resistance constant to the base of Reynolds' numbers. The way I looked upon it was that the Tanks are publishing information with regard to actual models in one of two particular forms, but by plotting those particulars on a base of Reynolds' numbers this would give all the information necessary to enable one to explore the data more thoroughly.

The President: It is quite obvious from the interest shown in the discussion how grateful everyone is to Professor Abell for having taken the trouble to write this paper, and for the very interesting discussion it has given rise to. I propose a hearty vote of thanks to Professor Abell for his paper.

EXPERIMENTAL INVESTIGATIONS ON THE RESISTANCE OF LONG PLANKS AND SHIPS.

By Professor YUZURU HIRAGA, Constructor Vice-Admiral, I.J.N. (Ret.), Member.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 11, 1934, Sir EUSTACE D'EYNCOURT, Bart., K.C.B., D.Sc., F.R.S., Vice-President, in the Chair.]

INTRODUCTION.

AN investigation on the frictional resistance and the total resistance of ships is deemed to require further development under the present status, and towing experiments with long planks and ships are necessary as suggested by the Skin Friction Committee's Report.* The author has deduced a new formula for the frictional resistance of planks and ship models in fresh water and sea-water from his experimental results in the small Tank of the Naval Technical Research Department, which will be published at the Autumn Meetings of the Japanese Society of Naval Architects this year.

The new formula is

$$R_f = K_1 S \left\{ 1 + \frac{(L' + C\,L)\,x}{6\,S} \right\} V_K^{1 \cdot 90} \rho\, \nu^{0 \cdot 165}$$

or

$$(R_f)_{55^\circ \text{F.}} = K_2 S \left\{ 1 + \frac{(L' + C\,L)\,x}{6\,S} \right\} V_K^{1 \cdot 90},$$

where R_f = frictional resistance in lb.,
L = length on water-line in ft.,
L' = L − (length of aft cut-up of keel) in ft.,
S = wetted surface in ft.^{2},
V_K = speed in knots,
ρ = density of fresh water or sea-water in lb. × ft.$^{-4}$ × sec.^{2},
ν = kinematic viscosity of fresh water or sea-water in ft.^{2} × sec.$^{-1}$,
K_1 = resistance coefficient (0·0331 for a painted surface in fresh water and sea-water),
K_2 = resistance coefficient (0·01004 for a painted surface in fresh water, and 0·01033 in sea-water);

x is the edge effect, which diminishes with the increase of the length and practically vanishes when $L = 26$ ft., and C is the coefficient of scale effect for thick planks and ship models and is zero for thin planks. Therefore, if it were possible to extend this formula to long planks and ships, then for planks and ships exceeding 26 ft. in length the expressions will be

$$R_f = K_1 S V_K^{1 \cdot 90} \rho\, \nu^{0 \cdot 165},$$

and

$$(R_f)_{55^\circ \text{F.}} = K_2 S V_K^{1 \cdot 90}.$$

From the necessity of investigating the resistance of actual ships, and also of verifying

* Trans. I.N.A., 1925.

this formula when sufficient evidence of the truth of this formula was established in 1928, towing experiments with longer planks and actual ships were planned, and various preparations were made to carry them out. In the operation of towing experiments, as there would be a great many difficulties encountered as described in the Skin Friction Committee's Report,* preparations to overcome them would be necessary, but at the same time the principal important points should be aimed at without being hampered by trying to overcome minor defects. Further, minute detailed measurements, or measurements not strictly connected with the main purpose of the experiment, would only mean greater difficulties; therefore speedy and essential measurements are more practical than measurements of a complicated nature; hence the following procedure should be followed:—

(1) A ship should be towed in such a way that it shall follow directly behind the towing ship in a straight line, which would cause it to run in the wake and race, and also in the wave system of the towing ship. Therefore these effects should be previously investigated by Tank experiments and open-water experiments, and the distance between the two ships should be such that the towed ship shall be practically free from the above effects. At the same time, if the towing hawser is too long, at low speeds it might touch the bow of the towed ship, or might break in touching the sea bottom. Further, a very large space would be necessary in turning round the mile-post ends, which would, of course, make the manœuvring of the ship exceedingly difficult; therefore, in consideration of these problems, a wake and race correction must be previously investigated.

(2) The ship's hull should be made a flush smooth surface as far as practicable, all appendages except the rudder should be removed, and the hull coated with anti-corrosive composition. The experiments would be carried out directly after being docked, and in the shortest possible time. A fouling correction might be needed, and provision for this correction should be previously made.

(3) Dynamometers would be of the spring-balance type, and fitted on the forecastle head of the towed ship, so arranged as to record only the horizontal component of the towing force, statical and dynamical methods being used for their calibration.

(4) The measured-mile speed should be the mean of means of three successive careful mile-post runs, and a suitable wind correction made on the recorded resistance when necessary.

(5) A calm sea is one of the important elements in carrying out the experiments, and is also one of the important conditions in completing the experiments in the shortest possible time; therefore the months of July and August in the Bay of Tokyo should be chosen for the *Yudachi* experiment.

The towing experiment with a plank ship, length 77 ft. and breadth only 6·3 in., the wave-making resistance of which was negligibly small, was undertaken by the Naval Technical Research Department. At the same time, for a contribution towards the advancement of science, a special Committee was appointed by the Admiralty, and the towing experiments with the ex-destroyer *Yudachi* and a tug-boat were carried out during three years from 1928 to 1930. Reynolds' number obtained at the highest speed in the plank-ship experiment, being 227×10^6, exceeded four times those obtained in Froude's and Gebers' plank experiments, and was somewhat larger than that of Kempf's pipe experiment, and in the case of the *Yudachi*, it being $1{,}009 \times 10^6$, was far above that of the *Greyhound* experiment (about 270×10^6). Finally, these results did not prove that the frictional resistance is a function of Reynolds' number, but they have pointed out a possibility of expressing it by the author's new formula. Further, this new formula was found to be applicable to the *Greyhound* experiments.

The author's purpose in writing this paper is, by describing these towing experiments and the analysis of the results, to show that the frictional resistance and the total resistance of ships are greater than those hitherto considered, and also to show the applicability of

* *Loc. cit.*

the new formula for frictional resistance, from short planks to long planks, and to substantiate the truth of Froude's method of deducing the total resistance of ships from model experiments by applying the new formula to models and ships. For convenience, the paper is divided into four parts:—

Part I. Towing Experiment with a Plank Ship.
Part II. Towing Experiment with the ex-Destroyer *Yudachi.*
Part III. Towing Experiment with a Tug-boat.
Part IV. Frictional Resistance and Total Resistance of Long Planks and Ships.

Of the five items of the aforementioned procedure, (1) the effect of wake, race, and waves, (2) the effect of fouling, and (3) the towing arrangement and dynamometers, will be described in Appendix I. (with Figs. 1 to 6, Plate XXVI.), Appendix II. (with Figs. 7A and 7B), and Appendix III. respectively.

Part I. Towing Experiment with a Plank Ship.

Particulars of the Plank Ship.

§ 1. For the purpose stated in the Introduction, the design of a plank ship was commenced in the beginning of 1928. As soon as the design was completed, the Naval Technical Research Department placed the order for construction with the Yokohama Dock Co., and after completion she was safely towed round to the Yokosuka Naval Station in May of the same year.

The principal dimensions and lines, etc., are shown in Fig. 9, Plate XXVII. The displacement completed was 3·356 tons, and length on W.L. (L) 77·319 ft., and the greatest breadth on W.L. (B) 0·525 ft., and B/L being 1/147 and exceeding the limiting thickness $\frac{1}{200}$L for the frictional resistance only, the existence of the residuary resistance was inevitable. However, as its value would be very small, it was decided that this should be deduced from the tank experiment on a similar model.

Result of the Experiment.

§ 2. The preparation for the experiment was at once made at Yokosuka, and, after docking the plank ship, a series of experiments took place (June 15th to November 20th). During this period, choosing days of calm and smooth sea, the ship put out to sea nine times, performing the experiments at forty-two different speeds with one hundred and twenty-one measured-mile runs, and at the final experiment on November 20th, after finishing the one hundred and twenty-second run with 20-knot speed, and in turning round to enter the next run she broke and sank. However, the entire programme of the experiments was completed, and the purpose was attained, which is entirely due to the efforts of Captain S. Ohta, the then commanding officer of H.I.M. Training Ships *Kasuga* and *Fuji*, who gave his consent to the request of the Naval Technical Research Department, and conducted the experiment. Assistance was also given by the same officers and men in carrying out the screw-race experiment on actual ships as described in Appendix I.

The experiments may be classified into (I.) the low-speed experiment in the harbour and (II.) the high-speed experiment outside the harbour.

The low-speed experiment was carried out on the specially erected mile posts in the Nagaura Harbour with a 56-ft. vedete boat of H.I.M.S. *Fuji* as a towing ship. The towing-speeds ranged between maximum and minimum speeds controllable by a proper adjustment of R.P.M. of the towing ship. The high-speed experiment was carried out on the mile posts for the small type of warship outside the Nagaura Harbour with the second-class destroyers *Katsura* and *Kayede*, and the first-class destroyer *Shimakaze* as towing ships. The towing-speed range was between the highest speed in experiment (I.) and 16 knots. The arrangement of the towing truck and dynamometer is shown in Fig. 8, Plate XXVII. The dynamometer reading was entirely done by using a telescope from the stern of the towing ship. In experiment (I.), a steersman was stationed on the plank ship, but in experiment (II.), finding that the plank ship was following the towing ship so well, and partly owing to signs of weakness appearing in the ship, no one was placed on the plank ship. These anticipations proved to be correct, for in the first place the plank ship had followed the towing ship beautifully without a steersman, and in the second place the plank ship broke and sank in the final experiment at 20 knots' speed.

Experiment (I.) may be divided into six series of experiments (1) to (6) in order of operations. In (1), (2), and (3), resistance-speed trials at measured-mile speeds of 2·45 to 9·07 knots were carried out, and in (4) the variation of resistance of the towed ship with the change of the distance between the two ships was tested with five different values of 5, 6, 7, 8, and 9 for $(D + L_1)/L_1$ (where L_1 is the length on W.L. of the towing ship) at the constant speed of 7·6 knots. Therefore (1) to (4) may be looked upon as one group. The plan, then, was to go on with experiment (II.), but finding that the shell plating started to leak from the time of series (4), the plank ship was docked for repairs. However, owing to the extreme thinness of the plating, it was not possible to make a satisfactory repair, therefore as a last resort the surface was covered with Japanese paper and coated with *Veneziani* anti-fouling composition instead of the *Takata* anti-fouling composition which was originally used. The difference in the nature of surface between these compositions was recognized as being almost nil, but there being a difference in the number of days out of dock compared with the previous experiments, for the combined effect of these differences, series (5) and (6) were carried out as a part of experiment (I.) to compare with series (1), (2), (3), and (4) already carried out, and at the same time to represent the low-speed experiment of experiment (II.) in this new condition.

Experiment (II.) was performed with speeds ranging from 7·54 to 16·22 knots, and was completed with series (7), (8), and (9).

Thus experiments with measured-mile speeds ranging from 2·45 to 16·22 knots and with measured resistances ranging from 30·4 lb. to 1,333 lb., with as many as forty-two spots for resistance against speed and with Reynolds' numbers ranging from 29×10^6 to 227×10^6 were completed.

Throughout the experiments the distance between the two ships was well maintained in conformity with the scheme. It was measured constantly by a range-finder, but the difference was never more than a few feet. Also the plank ship had followed the towing-ship nicely, always running within the screw race, and in looking from the stern of the towing ship the rope and the plank ship were almost always in one straight line. This will also be seen from the record of tiller angles, etc., of the plank ship which will be described hereafter.

The desire to carry out experiments soon after being docked was not always possible on account of the weather and other reasons. Therefore, for some of the experiments, it was necessary to make a suitable fouling correction. As investigation for the latter was made by Tank experiment simultaneously with the towing experiment, it is given in Appendix II.

§ 3. All the measurements hereafter described are, except those of speeds, the results of measurements taken at four equal time intervals in every run, in which generally its mean was taken as a value for that run, and the mean of means of three successive runs (seldom two successive runs) was taken as the result of one trial. All the results are shown in the Table, Plate XXVIII.

(*a*) Measured-mile trial. In forty-two trials, one set of the trials is formed of three successive runs, but in five of the trials one set is formed of two successive runs.

(*b*) Measured-mile speed. For those in which one set of trials is formed of three successive runs, their mean of means, and for those in which one set of the trials is formed of two successive runs, their mean was taken as the final value. In each case, according to Appendix I., (iii), and Fig. 6, Plate XXVI., a screw-race correction was made, but $(D + L_1)/L_1$ being generally 6, the factor of screw-race was no more than 0·65 per cent. The speed of the current in the harbour ranged from 0 to 0·5 knot, and outside from 0·1 to 0·8 knot.

(*c*) Measured resistance. The mean of the four readings, as described in Appendix III., was taken as the measured resistance of one run. In these experiments no automatic recorder was used; however, in the light of the *Yudachi* experiment, which will be described in Part II., the mean value of four readings may be confidently accepted as the correct value. Owing to the narrow beam, small free board, and practically no deck erection of the plank ship, the air resistance is clearly very small, and still-air resistance at the highest speed of 16·22 knots was only a few pounds, i.e. no more than 0·1 or 0·2 per cent. of the measured resistance, and no correction for the air resistance was made.

A fouling correction on the measured resistance was made by using the correction factors described in Appendix II.

(*d*) Tiller angle. All tiller angles in series (1) to (6) were recorded. Of the total number of two hundred and ninety-two records, seventy-four recorded zero, and the remainder were generally 1° to 3° with 5° as the maximum. This maximum 5° was mostly recorded in series (1) of the first day of the experiment due to the unfamiliarity of the steersman, and in series (2) to (6) there were only a few cases. In series (7), (8), and (9), the rudder was fixed at 0°. In each trial of series (1) to (6), the arithmetical mean value of mean tiller angles of three successive runs is less than 2·7°, the mean tiller angle for one run being the algebraical sum (taking + sign for port, and − sign for starboard) divided by the number of records. Supposing the ship is going ahead with a tiller angle set to 2°, the drag would not exceed about 5 lb., even at the highest speed, and 0·4 per cent. of the measured resistance at any speed. Therefore no correction was made for the drag.

(*e*) Horizontal angle between the tow-rope and the middle line plane of the towing ship. This angle was measured in series (1) and (2). Out of seventy-six records twelve were zero, and others ranged from 1° to 3° with a few records of 4° to 5°. It was not possible to measure this angle from the plank ship; however, since this angle does not always indicate the position of the towed ship, which is far away from the towing ship, and since the plank ship had followed the towing ship exceedingly well, and was always inside of the screw race as already described, an error in the measured resistance due to this effect may be taken as quite negligible.

(*f*) Vertical angle between the tow-rope and the horizontal plane at the aft-end of the towing ship. This angle was measured in series (1) to (4). The maximum angle recorded was 11° when the speed was 2·45 knots, and the minimum angle was about 0·7° when the speed was 7·62 knots, and $(D + L_1)/L_1 = 5$. At higher speeds above 7 knots the angle was less than 3°, and the whole length of the tow-rope was entirely clear of the water. It was not possible to measure this angle from the plank ship, but it may be taken to be practically the same, or slightly larger. In series (1) to (6), the total weight of the tow-rope was only 26 lb. to 41 lb., and the friction due to the vertical component of the tow-rope tension at the towing truck of the plank ship would be quite negligible.

(*g*) Additional change of trim in the plank ship during the run. The wave-making resistance being almost negligible, the change of trim due to the wave would be very small if there was any. The change of trim by the bow due to the couple formed by the horizontal force on the towing truck and the ship's resistance was calculated and found to be only about 0·3 ft. even at the highest speed. In short, though the actual additional change of trim was not measured the angle formed by the towing-truck plane with the horizontal due to the change of trim would be merely a fraction of 1°, and the measured resistance needs no correction.

(*h*) Wetted surface area. In the experiments after series (5), the draught was gradually increased due to a leakage, and in series (8), the wetted surface was increased by 7 per cent. compared with that of series (1), and in series (9) rapidly increased to 16 per cent. to 23 per cent., therefore a comparative study per unit area was made for the frictional resistance.

(*i*) Temperature, density, and viscosity of sea-water. These were measured from the water taken from the sea where the experiments were carried out.

(*j*) Residuary resistance of the plank ship deduced from a model experiment. An 8-ft. similar model was made, and was tested in the small Tank. The frictional resistance obtained by the new formula was deducted from the measured total resistance, and the remainder was taken as the residuary resistance of the model. This was corrected to the sea-water of the plank-ship experiments, and by the law of comparison the residuary resistance $(R_w)_t$ of the plank ship was determined. It will be seen from the Table, Plate XXVIII., that the residuary resistance is quite negligible, and the measured total resistance is practically the frictional resistance. The air resistance was not deducted in either the ship or the model, as referred to in (*c*).

(*k*) Frictional resistance of the plank ship. The frictional resistance $(R_f)_t'$ was obtained by deducting $(R_w)_t$ from the measured resistance $(R)_t$, and the frictional resistance per unit area corrected to 55° F. and to the standard painted surface $(R_f)_{55^\circ F.}/S$ was obtained from

$$(R_f)_{55^\circ F.}/S = \{(\text{Factor of fouling correction}) \times (R_f)_t' \times (\rho\, \nu^{0\cdot165})_{55^\circ F.}/(\rho\, \nu^{0\cdot165})_t\}/S,$$

with the measured $(\rho)_t$ and $(\nu)_t$, and the deduced $(\rho)_{55^\circ F.}$ and $(\nu)_{55^\circ F.}$ in the Table, Plate XXVIII., $(R_f)_{55^\circ F.}/S$ to base V curve, and $\log\{(R_f)_{55^\circ F.}/S\}$ to base log V curve are shown in Figs. 11A and 12 (Plate XXVIII.) respectively, and it may be observed from these curves that the result of the experiment as a whole is uniform and satisfactory, considering the difficulty of conducting this kind of experiment in the open sea. The latter of the above two curves clearly shows that the experimental result may be expressed by $(R_f)_{55^\circ F.}/S \propto V^n$, and $n = 1\cdot90$. Therefore,

$$(R_f)_{55^\circ F.} = K_2\, S\, V_K^{\,1\cdot90}$$

and values of K_2 were worked out. The values of K_2 are shown in the Table, Plate XXVIII., and also in K_2 to base V curve (Fig. 13, Plate XXVIII.); they are independent of the speed and the final mean of 42 values is $0\cdot01040$. Therefore,

$$(R_f)_{55^\circ F.} = 0\cdot01040\, S\, V_K^{\,1\cdot90},$$

in which form the two curves above mentioned will naturally be expressed, and at any temperature either in fresh water or in sea-water,

$$R_f = (R_f)_{55^\circ F.}\, (\rho\, \nu^{0\cdot165})/(\rho\, \nu^{0\cdot165})_{55^\circ F.} = 0\cdot0334\, S\, V_K^{\,1\cdot90} \rho\, \nu^{0\cdot165}.$$

Also the mean values of K_2 for every series, every nature of surface, and each of the experiments inside and outside of the harbour are shown in Table I., from which again the uniformity of the experiment will be observed.

(*l*) The result of series (4), in which $(D + L_1)/L_1$ was changed for five variations from 5 to 9, and for which the experiments were carried out at the same place with the same 56-ft. vedette boat as a towing ship and with a uniform R.P.M. maintained, is clearly shown in the Table, Plate XXVIII. It will be perceived that when $V/\sqrt{L_1}$ is approximately $1\cdot02$, for these varied distances between the two ships the speed is practically constant with a uniform R.P.M., and the measured resistance is almost constant for practically constant speeds. Further, as may be seen from K_2 to base $(D + L_1)/L_1$ curve and K_2 to base $(D + L_1)/\lambda$ curve (Figs 11B and 11C, Plate XXVIII.), when $(D + L_1)$ is smallest, i.e. $5\, L_1$ or $8\cdot7\,\lambda$, the resistance as well as K_2 is somewhat large, and $K_2 = 0\cdot01084$, but in the other four cases, when $(D + L_1)$ are 6, 7, 8, and $9\, L_1$, or $10\cdot2$, $12\cdot1$, $14\cdot0$, and $15\cdot7\,\lambda$, the values of K_2 are 0·01027, 0·01027, 0·01013, and 0·01037 respectively, and with the mean value 0·01026 the difference is no more than 1·3 per cent. at the greatest, therefore the resistance may be taken as independent of these four distances. There is neither a trace of wave interference such as producing maximum and minimum values alternately for each $\frac{1}{2}\lambda$, nor a trace of unsuitability of the screw-race correction, therefore when $V/\sqrt{L_1}$ is less than 1·02 and $(D + L_1)$ is above $6 L_1$ or $10 \cdot 2 \lambda$, the application of the established screw-race correction will be safe.

Of all the forty-two experiments, only four were not within the scope above mentioned, one in each of series (1), (2), (3), and (5); however, they presented no results particularly noticeable compared with other results. On the other hand, since the plank ship produces practically no wave, there is no wave interference, hence what was described above may probably be a natural result, also whether the somewhat large value of the resistance and

TABLE I.

MEAN VALUES OF K_2.

Series.		Nature of Surface.	Measured-mile Speeds, Knots.	Number of Different Speeds.	Weighted Mean of K_2.					
Inside Harbour	1	*Takata*	3·43–9·07	4	0·01037 (series 1–4)	0·01031	0·01037 (series 1–4)	0·01036 (series 1–6)	0·01040 (series 1–9)
	2	*Takata*	4·72–8·34	3		0·01010			
	3	*Takata*	2·45–8·94	8		0·01051			
	4	*Takata*	7·60–7·72	5		0·01038			
	5	*Veneziani*	2·50–8·87	6	0·01043 (series 5–9)	0·01059	0·01033 (series 5–6)		
	6	*Veneziani*	3·69–7·66	6		0·01008			
Outside Harbour	7	*Veneziani*	7·54–16·11	4		0·01035		0·01055 (series 7–9)	
	8	*Veneziani*	10·41–16·22	4		0·01089			
	9	*Veneziani*	9·85–14·60	2		0·01030			

K_2, when $D + L_1 = 5 L_1$ or $8 \cdot 7 \lambda$, is really due to screw race or wave interference, or is an experimental error, is uncertain.

Comparison of the Experimental Result with the New Formula, and the Formulæ of Several Authorities.

§ 4. It was stated in the Introduction to the present paper that the new formula when extended to long thin planks with the standard painted surface in sea-water is

$$R_f = 0 \cdot 0331 \, S \, V_K^{1 \cdot 90} \rho \, \nu^{0 \cdot 165},$$

and

$$(R_f)_{55^\circ F.} = 0 \cdot 01033 \, S \, V_K^{1 \cdot 90};$$

whereas the experimental result of the plank ship gives

$$R_f = 0 \cdot 0334 \, S \, V_K^{1 \cdot 90} \rho \, \nu^{0 \cdot 165},$$

and

$$(R_f)_{55^\circ F.} = 0 \cdot 01040 \, S \, V_K^{1 \cdot 90}.$$

Therefore the new formula may be said to be in conformity with the experimental result of the plank ship, giving a resistance less than the actual resistance by 1 per cent.

In Fig. 11A (Plate XXVIII.), $(R_f)_{55^\circ F.}/S$ to base V curves for a 77·319-ft. plank obtained from the formulæ of Froude, Gebers,* Kempf,† and the curve of Baker ‡ are shown, together with the experimental result of the plank ship. It will be seen that for speeds between 5 and 17 knots the plank ship experiment results exceed by 21 to 42 per cent. those given by several authorities, the ratios of the former to the latter being as follows:—

Speed.	to Froude's.	to Gebers'.	to Kempf's.	to Baker's.
17 knots	1·36	1·42	1·21	1·30
15 ,, ...	1·36	1·42	1·22	1·30
10 ,, ...	1·32	1·40	1·24	1·28
5 ,, ...	1·25	1·38	1·26	1·26

Part. II. Towing Experiment with the ex-Destroyer "Yudachi."

Executive Committee of the Experiment.

§ 5. A cherished desire of the author and the Naval Technical Research Department to aid the study of the resistance of ships by performing a towing experiment with the ex-destroyer *Yudachi* was recognized by the Admiralty. The Minister of the Navy stated that the Navy was willing to undertake it if it contributed in any way to the advancement of science. He issued an order to the Commander-in-Chief of the Yokosuka Naval Station, and an Executive Committee of the Towing Experiment with the *Yudachi* was formed on July 14, 1928. The members of the Committee were Captain S. Ohta, the then commanding officer of H.I.M. Training Ships *Kasuga* and *Fuji*, as chairman of the committee, executive officers and engineers of the same ships, dockyard officers, and Tank staff. The committee decided to carry out the experiment in July and August, the months likely to have the most quiet sea of the year.

The experiment was conducted in August 1928, and the speeds experimented with were sixteen different speeds up to 20·97 knots. It so happened that at this time the plank-ship experiment (described in Part I.), which was commenced in June, was in full swing. On the other hand, as the result of the experiment, the Committee saw the possibility of towing up to a speed of about 26 knots with a first-class destroyer as a towing ship, and decided to carry out the second experiment at higher speeds in 1929. Under the leadership of Captain Y. Ono, the new commander of the *Kasuga* and the *Fuji*, the experiments at five different speeds up to 26·33 knots were completed in July 1929.

Thus the experiments were carried out during two years in spite of many difficulties, and their purpose satisfactorily fulfilled. The following descriptions are those of the Committee's work, and of the analysis of the experiments by the author and the Tank Staff.

Three photographs showing the *Yudachi* being towed at 7·3-, 17·4-, and 21-knot speeds are given in Plate XXXIII.

Preparation of the Experiment.

§ 6. The *Yudachi* was completed in 1906 as a third-class destroyer, and in 1928 she was removed from the warship list. Her hull being comparatively well preserved, and in a sufficiently good condition to be used for experiments, she was chosen as the subject ship of the experiment. Her sheer draught and particulars are shown in Fig. 15A, Plate XXIX. The following preparations were made for the experiment:—

(*a*) Actual offsets of the hull were measured in the dock and were compared with the sheer draught, but practically no change was observed, the maximum difference with the offsets being less than ½ in. The middle line of the hull was found to be in a particularly good condition.

* *Schiffbau*, April 20, 1920.
† *Werft Reederei Hafen*, October 22, 1924
‡ *Trans. North-East Coast Inst.*, 1915–16.

(*b*) Undulating surfaces of the shell were made even. The joints of the shell were butt joints and lapped edges, the thickness of the shell being 7·5 to 10 lb. per sq. ft. amidships and 5 lb. to 6 lb. at ends as shown in Fig. 15B, Plate XXIX., the mean thickness was only about 0·15 per cent. of the half-breadth of the ship. The bottom was coated as usual with the *Takata* anti-corrosive and anti-fouling compositions.

(*c*) All appendages, such as bilge keels, shaft brackets, bossing, shafting, and propellers, etc., were removed, and the shell was faired off after their removal. In fact, the hull was turned to a perfectly naked body with the rudder only intact. The rudder area was 1/31 of the ship's middle line area.

(*d*) All the masts, funnels, vents, deck-houses, and miscellaneous fittings on the upper deck and outboard, except those which were useful for the experiment, were removed to reduce the air resistance. Suitable ballast was shipped in order to adjust the draught, displacement, and metacentric height.

(*e*) A first-class destroyer was to be used as a towing ship, and the best possible towing arrangement was made to allow the *Yudachi* to follow the towing ship in a straight line. The dynamometers used were of 5-, 20-, and 60-ton capacity, as described in Appendix III. The arrangement of the towing truck and dynamometer is shown in Fig. 14A (Plate XXIX.).

(*f*) The distance between the two ships or $(D + L_1)$ was fixed at $6\frac{1}{2}$ L_1 for speeds below $V/\sqrt{L_1} = 1$ of the towing ship, i.e. for speeds of 18 knots and below, and $8\frac{1}{2}$ L_1 above 18 knots. But with low speeds such as 7 knots and below the fore-part of the structure in the *Yudachi* might interfere with the movement of the hawser, therefore it was reduced to $4\frac{1}{2}$ lengths, and for each of the speeds of 13·5 knots and 15·5 knots comparative experiments at the constant speed with varying $(D + L_1)/L_1$ were arranged.

(*g*) The Tateyama mile-posts for large warships in the Bay of Tokyo were used, and each trial consisted of three successive runs.

(*h*) Suitable arrangements were provided to measure the height of wave produced by the ship herself along the ship's side during the run, to measure the change of trim during run, and to measure the relative wind velocity and direction, etc.

Result of the Experiment.

§ 7. The experiment in 1928 or the 1928 series was commenced first with the independent measured-mile trial of the towing ship *Yukaze* on August 17th, and from the following day the experiment proper was commenced and finished on the 22nd, completing the measured-mile speeds of 5·61 to 20·97 knots. During the experiment the sea was generally quiet, with very little wind and very little swell. On the second day, the 20th, there was a small swell, and its effect was observed to a certain degree, but it had no important bearing on the whole.

The 1929 series was also commenced with the independent measured-mile trial of the towing ship *Shiokaze*, sister ship to the *Yukaze*, and the experiment of measured-mile speeds 18·29 to 26·33 knots was completed on July 19th and 20th. During the experiment the sea was exceptionally quiet.

The lowest towing speed of the experiment was fixed by a uniform minimum R.P.M. obtainable in the *Yukaze*, and the highest speed from a consideration of the towing capacity and the safety of towing, etc., of the *Shiokaze*. Thus experiments at speeds from 5·61 to 26·33 knots with Reynolds' numbers 218×10^6 to $1{,}009 \times 10^6$ and measured resistances of 0·53 to 16·95 tons were completed.

Throughout the experiment the distance between the two ships was constantly measured by a range-finder. The *Yudachi* followed the towing ship very well, and in looking at the general situation from the towing ship, and also from the *Yudachi*, it was observed that both the ships were always in a straight line, with the *Yudachi* following in the screw race produced by the towing ship. These facts may be deduced from the records of the horizontal angles formed by the two ships and the tow-rope, or of the tiller angles of the *Yudachi*, which will be described later; above all, this may be seen from the uniformity of the whole results.

All the measurements hereafter described are, except those of speed, results of four readings at equal time intervals for every run, and generally their mean is taken as the value of that run, and the mean of means of three successive runs or four successive runs is taken as the result of one trial. The results of all these trials are shown in the Table, Plate XXX.

(*a*) Measured-mile trial. In the 1928 series, sixteen trials (forty-nine runs) in four days, and in the 1929 series, five trials (sixteen runs) in two days were carried out. Each trial in each series consisted of three successive runs, except one trial in each series, which consisted of four successive runs.

(*b*) Measured-mile speed. The mean of means was taken as the final value, and in each case with the screw-race correction in accordance with Appendix I., (iii), and Fig. 6 (Plate XXVI.).

(*c*) Measured resistance. In every run, the mean of four measures taken from the direct reading of the dynamometer dial, and the mean of the continuous record of the automatic recorder attached to the dynamometer were found to be always in perfect conformity with each other. A few examples of the automatic records of directly measured resistances and measured tiller angles are shown side by side in Figs. 16A and 16B, Plate XXX. From these it will be seen that the resistance oscillates between about one ton above and below the mean value in the same manner as in the dynamical testing of the dynamometer described in Appendix III. In all the runs automatic records were taken, but for the reason given in Appendix III. the mean of four direct readings was taken as the measured resistance of a run, and the mean of means of one series of runs as the measured resistance of one trial.

(*d*) Air resistance. In the 1928 series, relative wind velocities and directions were measured by means of a hot-wire anemometer fitted at the bow of the *Yudachi*. By the application of Hughes' experiment results for a cargo steamer,* the head or axial resistance of the air against the *Yudachi* was estimated, and was deducted from the measured resistance in each run, i.e.,

$$\text{Axial resistance in lb.} = \text{C} \times (\text{Equivalent transverse projected area B in ft.}^2) \times (\text{Relative air speed V in kts.})^2,$$

where C varies from $0 \cdot 0041$ when $\theta = 0°$, to $0 \cdot 0055$ when $\theta = 30°$, etc., θ being the angle of relative wind to the axis of the ship. But, according to Constructor Commander Izubuchi's experiments on destroyer models in a wind tunnel in the Naval Technical Research Department, when $\theta = 0°$,

$$\text{Axial resistance in lb.} = 0 \cdot 0018 \times (\text{Transverse projected area A inclusive of deck erections in ft.}^2) \times \text{V}^2,$$

which will give $\text{C} = 0 \cdot 0030$ when $\theta = 0°$, in Hughes' formula. Hence for the *Yudachi*, Hughes' result was applied by taking 73 per cent. of his coefficient. Further, the axial air resistance estimated in the 1928 series by the above method corresponds on the average to $1 \cdot 11$ times the air resistance estimated by $0 \cdot 0030\, \text{B}\, \text{V}_1^2$, where V_1 is the component of the relative wind velocity along the ship's axis. In the 1929 series, for relative wind velocities, only the ship's axial components were measured, and it was necessary to calculate the air resistance due to that velocity by using the $0 \cdot 0030\, \text{B}\, \text{V}_1^2$ formula. The result multiplied by $1 \cdot 11$ was assumed to be the axial air resistance, and deducted from the measured resistance.

* G. Hughes, "Model Experiments on the Wind Resistance of Ships," Trans. I.N.A., 1930.

Axial air resistances thus calculated correspond to 0·9 per cent. of the measured resistance at the highest speed of the 1928 series and 1·0 per cent. of that of the 1929 series.

(*e*) Fouling effect. In the 1928 series, the experiment was commenced after fifteen days out of dock, and in the 1929 series after eight days out of dock, but even on the first day of the experiment in either series the ship ran a distance more than twenty nautical miles before the commencement of the experiment. After the investigation described in Appendix II., it was admitted that the bottom was clean and smooth, requiring no fouling correction. Therefore, in each series, when the frictional resistance is expressed by the new formula, the values will be, as explained in the Introduction to this paper, $K_1 = 0 \cdot 0331$ in both fresh water and sea-water; or $K_2 = 0 \cdot 01033$ in sea-water, taking $(\rho\, \nu^{0 \cdot 165})_{55^\circ \text{F.}}$ for sea-water to be 0·312.

(*f*) Tiller angle. Originally it was set to 5° as the maximum. In the 1928 series, fifty-one records out of one hundred and ninety-six records were 0°, and the rest were mostly 0·5° to 2°; only ten records were 3° to 5°. In the 1929 series, ten records out of sixty-four records were 0°, and the rest were 0·5° to 2·5°, with only a few records of 3° as the maximum. In every trial, the arithmetical mean value of mean tiller angles of three successive runs was 1·1° or less. If a ship were supposed to be running ahead with a tiller angle set to 2°, the drag would not exceed 0·3 per cent. of the measured resistance, and no correction was made for that effect.

(*g*) Horizontal angles between the tow-rope and the middle line planes of the towing ship and the *Yudachi*. In the 1928 series, these angles in the towing ship were generally 0° to 2° with only a few records of 4° or 5°. In the *Yudachi*, one-third of one hundred and ninety-six records of the actual measurement were 0°, and the rest were mostly 1° to 3°, except those of the (5), (6), (7), and (8) trials on August 20th, when there was a certain amount of swell and wind, in which 5° to 6° was frequently recorded. Of the final arithmetical mean of these angles for each trial, those of the *Yudachi*, excepting 4·8° to 6·1° on August 20th, were 0° to 2·5°, and the error of the measured resistance with 2·5° was only 0·1 per cent., even with 6° it would be about 0·5 per cent. since the *Yudachi* was proceeding straight on her proper course, keeping the above angle with the tow-rope. Therefore, no correction for that effect was made to the measured resistance. In the 1929 series no measurement was taken.

(*h*) Vertical angles between the tow-rope and the horizontal plane at the aft-end of the towing ship and the fore-end of the *Yudachi*. In the 1928 series, this angle at the towing ship was 5° to 19°, and at the *Yudachi* was 5° to 34°. In the trials of the 1928 series the maximum value of the vertical force on the towing truck due to this angle was about 1 ton, and the friction produced thereby was quite negligible when compared with the measured resistance. In the 1929 series no records were taken.

(*i*) Additional change of trim in the *Yudachi* during the run. By means of pendulums of a very small period fitted at three places in *Yudachi* forward, amidships, and aft, the change of longitudinal inclination of the hull was measured. A change in the deflection of the ship during the run was taken into consideration, and, though very approximate, an additional change of trim was calculated from the mean value obtained. Trim-speed curve, together with that deduced from the 13-ft. model, is shown in Fig. 17, Plate XXX., from which it will be seen that the actual trims are always more trim by the bow than those deduced from the model; this appears to be principally due to the couple formed by the horizontal force on the towing truck and the ship's resistance. For example, if a calculation was made under this assumption at speeds of 17·41 and 20·97 knots, it would be about 0·1 ft. and 0·2 ft. by the bow respectively. The difference between the two curves at these speeds in Fig. 17 are 0·2 ft. and 0·5 ft. respectively; whatever it may be, it was evident from the result of the model experiment that a change of trim of this degree would not affect the resistance, and no correction for the measured resistance would be necessary.

Also, at 26·33 knots, the highest speed of the whole experiment, the actual trim was 3·7 ft. by the stern, including the initial trim of 0·51 ft. This was the maximum value of trim by the stern throughout the experiment, but the angle formed by the towing truck plane and the horizontal was not quite 1°, and a correction on the measured resistance to this effect would not be necessary.

(*j*) Height of surface of waves produced by the *Yudachi* herself. In the 1928 series the heights of wave surface above L.W.L. along the ship's side were measured. They were quite similar to the wave profile deduced from a 6-ft. model in form and height above undisturbed water surface at various speeds, for instance, the actual heights of the wave crest at about 10 ft. abaft the bow at 10·1- and 21-knot speeds were 0·9 ft. and 2·6 ft. against about 1·1 ft. and 2·8 ft. from the model respectively.

(*k*) Variation of the measured resistances with the change of $(D + L_1)/L_1$. In the

TABLE II.

THE VARIATION OF THE MEASURED RESISTANCES, ETC., WITH THE CHANGE OF $(D + L_1)/L_1$.

Number of Experiments.	Measured Mile Speed V_0, Knots.	$\frac{V_0}{\sqrt{L_1}}$	$\frac{D+L_1}{L_1}$	$\frac{D+L_1}{\lambda}$	(Measured Resistance) − (Air Resistance), Tons.	Towing Ship.		Corrected to a Constant Speed.			
						S.H.P.	R.P.M.	Speed, Knots.	R, Tons.	S.H.P.	R.P.M.
(4)	13·75	0·76	6·52	20·3	3·21	1,837	137·1	13·74	3·21	1,840	137·1
(8)	13·74	0·76	8·36	26·0	3·14	1,858	137·7		3·14	1,860	137·7
(13)	13·64	0·76	4·88	15·4	3·04	1,787	135·2		3·09	1,840	136·3
								Mean	3·15	1,850	137·0
(5)	15·24	0·84	6·52	16·5	3·99	3,081	158·2	15·41	4·09	3,230	160·1
(7)	15·47	0·86	8·36	20·5	4·25	2,916	156·5		4·21	2,870	155·8
(14)	15·41	0·85	4·88	12·1	4·08	2,935	154·0		4·08	2,935	154·0
								Mean	4·13	3,010	156·6

1928 series the resistance was compared when $(D + L_1)/L_1$ had three different values, viz. 4·88, 6·52, and 8·36 in each of the trials at approximately 13·7- and 15·4-knot speeds. The results are shown in Table II. It will be seen that in spite of these differences in the distance between the two ships, the difference between the mean value of (Measured resistance − Air resistance) for three distances and an individual value is only 1·9 per cent. in 13·74-knot speed, and 1·9 per cent. in 15·41-knot speed, and also the resistance in the shortest distance is observed to be the least at both 13·74-knot speed and 15·41-knot speed. Further, judging from the relation of S.H.P. and R.P.M. of the towing ship, it will be proper to say that the resistance is constant within the range of $V/\sqrt{L_1} = 0{\cdot}76$ to $0{\cdot}85$ of the towing ship and $0{\cdot}91$ to $1{\cdot}02$ of the towed ship, and if $(D + L_1)/L_1 = 4{\cdot}88$ or $(D + L_1)/\lambda = 12{\cdot}1$, the resistance may be taken as constant with no effect from the waves due to the towing ship.

From the above results, from the fact that the plank ship is free from any interference when $(D + L_1)$ is greater than $6\,L_1$ or $10{\cdot}2\,\lambda$ at $V/\sqrt{L_1} = 1{\cdot}02$ of the towing ship, and also from Appendix I., (iv), it would not be unreasonable to conclude that there is practically no effect of waves when $(D + L_1)/L_1 = 8 \cdot 43$ or $(D + L_1)/\lambda = 7 \cdot 1$ at the highest speed 26·33 knots, i.e. $V/\sqrt{L_1}$ of the towing ship 1·46 and that of the towed ship 1·73.

(1) The residuary resistance of the *Yudachi* deduced from model experiments. Similar models, 4, 5, and 6 ft. in length, were tested at the small Tank, and 13- and 20-ft. models at the Mitsubishi Tank. Of these models, those which were not affected by the wall or bottom of the Tank at the speed corresponding to the highest speed of 27 knots of the actual ship were the 4- and 13-ft. models. In each of the 5-, 6-, and 20-ft. models, recognizing that the effects start to act at the speeds corresponding to about 24, 20, and 19 knots respectively, or 350, 320, and 540 ft. per min. of the respective model, speeds lower than the above model speeds were only taken for these models. Also in the 4-ft model, as there was a second critical speed of about 300 ft. per min. corresponding to 23 knots for the actual ship, only speeds higher than this were observed.

To start with, the scale effect was calculated for the 20-, 13-, 6-, 5-, and 4-ft. models in the range of speeds taken up as above, and C in

$$R_f = K_1 S \{1 + (L' + C L) x/(6 S)\} V_K^{1 \cdot 90} \rho \nu^{0 \cdot 165}$$

was determined and 0·36 was taken (C value from the C to base $\Delta/(\frac{1}{100} L)^3$ curve corresponding to $\Delta/(\frac{1}{100} L)^3 = 29$ was 0·33). The results from each of the 4-, 5-, and 6-ft. model experiments suitably applied to represent results for the 13-ft. model are shown in Fig. 19, Plate XXX., and the 20-ft. model result in Fig. 20. All these results are in conformity with one another.

Of these, the 13-ft. model being free from the wall and bottom effect at the speed corresponding to the highest speed of the *Yudachi*, the frictional resistance having the above scale effect was deducted from the measured total resistance of that model; further, the air resistance of the model above water obtained by "$0 \cdot 0018 A V^2$" was deducted, the result was taken as the residuary resistance without the air resistance of the model, and by the law of comparison was further reduced to the residuary resistance without the air resistance of the actual *Yudachi*. Also, the 20-ft. model was worked out similarly, and the residuary resistance without the air resistance of the actual *Yudachi* was obtained. Of these two values of the residuary resistances, the mean value was taken for speeds of 19 knots and under, but that deduced only from the 13-ft. model was taken for speeds over 19 knots. All of these processes for each displacement in the 1928 and 1929 series are shown in detail in the Table, Plate XXX.

§ 8. In accordance with the analysis as described in the previous paragraph, the actual result of the *Yudachi* at a constant displacement of 368·3 tons in sea-water at 76·9° F. was examined. To the residuary resistance $(R_w)_t$ deduced from the model adding the frictional resistance $(R_f)_t'$ calculated from the new formula $(R_f)_t' = 0 \cdot 0331 S V_K^{1 \cdot 90} (\rho \nu^{0 \cdot 165})_t$ with the measured ρ and ν, the total resistance $(R)_t'$ was obtained, and $(R)_t'$ to base V curve was plotted as shown in Fig. 21A, Plate XXX. Taking V as the real speed corrected for screw race in the measured-mile speed of the *Yudachi*, the net measured total resistance $(R)_t$ was plotted upon the above curve, being corrected for the displacement 368·3 tons of the 1928 series from 362·1 tons of the 1929 series, after deducting the air resistance from the measured resistance. As is seen clearly from the graph, the measured spots lie on the above curve almost perfectly. Further, in the Table, Plate XXX., the ratio of the net measured total resistance to the calculated total resistance, or $(R)_t/(R)_t'$, at each speed was 0·94 to 1·08 for twenty-one different speeds, and their mean value was exactly 1·00.

With the correction of temperature to 55° F. ($\rho = 1 \cdot 986$, $\nu = 1 \cdot 376 \times 10^{-5}$ in ft. lb. sec. unit as deduced from the measured ρ and ν in this series of experiments) for $(R_f)_t$, which was obtained by subtracting the deduced residuary resistance $(R_w)_t$ from $(R)_t$, $(R_f)_{55° F.}$ was obtained, and K_2 was worked out from $(R_f)_{55° F.} = K_2 S V_K^{1 \cdot 90}$. The results show that K_2 has a value of between 0·0094 and 0·0113 (see Table, Plate XXX.), and that the mean value of twenty-one K_2's is 0·0102; this practically coincides with 0·01030, which is the corrected value of the standard 0·01033 of the painted surface for the above deduced ρ and ν. From Fig. 21A, and also from K_2 to base V curve (Fig. 21B, Plate XXX.), it will be recognized that K_2 is independent of speed, and the mean value 0·0102 is the correct mean value. Therefore Froude's method of finding the total resistance of the actual ship (in which the residuary resistance of the actual ship is deduced from the residuary resistance of the model by means of the law of comparison, and the computed frictional resistance is added to the deduced residuary resistance) has actually been demonstrated by the experiment on the *Yudachi*, bearing in mind that a new formula has been used for the frictional resistance of the model and the ship.

A Comparison of the Measured Total Resistance of the "Yudachi" with those deduced from the Model by the New Formula and with Other Formulæ.

§ 9. The total resistance of the *Yudachi* deduced from the model, using the new formula, Froude's and Gebers' formulæ for evaluating frictional resistance, is given and compared in Table III.

In the 13-ft. model the calculated frictional resistance r_f is the greatest by the new formula, the next in magnitude is by Froude's, and the smallest by Gebers'; consequently the residuary resistances r_w and $(R_w)_t$ of the model and the actual ship are the smallest according to the new formula, the next smallest according to Froude's and the greatest according to Gebers'.

The frictional resistance $(R_f)_t'$ of the actual ship as given by the new formula is greater than as given by Froude's formula, and greater still than as given by Gebers'. At speeds from 8 to 28 knots it is 1·36 to 1·49 times that by Froude's, and 1·58 to 1·63 times that by Gebers' formula. Despite such remarkable differences, the total resistance deduced does not show so large a difference, as the residuary resistance difference offsets the frictional resistance difference as stated above, and the new formula gives a value for total resistance from 1·31 to 1·15 times that by Froude's, and 1·33 to 1·16 times that by Gebers' formula. In the case of the *Yudachi*, with a length of 232 ft., the new formula gives good coincidence with the real resistance at low and high speeds, whilst in both Froude's and Gebers' results an increase of 31 to 33 per cent. to the total resistance at low speeds and 15 to 16 per cent. at higher speeds is necessary for complete agreement with the real total resistance. If the so-called "roughness factor" is introduced in order to increase the calculated frictional resistance of the actual ship, so as to bring the total resistance estimated into agreement with the total resistance (although whether such convention is reasonable or not is a different matter), it is necessary at all speeds to multiply by such a large factor as about 1·33 if using Froude's formula for friction, and about 1·38 if using Gebers'.

In the *Yudachi*, at 8 to 28 knots, the ratio of the frictional resistance calculated by the new formula to that calculated by Kempf's * and Baker's † formulæ is 1·20 to 1·12 for Kempf's and 1·36 to 1·43 for Baker's.

Further, in the actual results of the *Yudachi* at speeds of 9 knots and below, the measured total resistance and the calculated total resistance have the same value as the calculated frictional resistance (see Fig. 21A). It follows that at this speed the total resistance is entirely frictional. Also in the 20- and 13-ft. models at corresponding speeds the same complete conformity is established. It is to be noted that in the change of trim to base V curves in Fig. 17, Plate XXX., the trim by the stern of the *Yudachi* at speeds of 9 knots and below is constant (i.e. about 0·1 ft.), whilst at speeds above 9 knots a state of trim by the bow is gradually attained. The speed showing no change of trim is 9 knots, at which the curve crosses the base line. The photograph of the ship at 7·3 knots (Plate XXXIII.)

* *Loc. cit.* † Baker, "Ship Design, Resistance, and Screw Propulsion," Vol. I.

TABLE III.

Comparison of the Measured Total Resistance of the *Yudachi* with the Total Resistance deduced from the Model by the New Formula and Froude's and Gebers' Formulæ.

Speed V, Knots.	$V/\sqrt{L}$.	13-Ft. Model at 59·3° F. and in Fresh Water, at Corresponding Speeds.			*Yudachi*, 368·3 tons at 76·9° F. and in Sea-water.								
		Calculated Frictional Resistance / Measured Total Resistance			Deduced Total Resistance, Tons.			Measured Total Resistance / Deduced Total Resistance $(R)_t/(R)_t'$.			Factor to be Applied to the Calculated Frictional Resistance to make $(R)_t/(R)_t' = 1$.		
		Author.	Froude.	Gebers.	Author.	Froude.	Gebers.	Author.	Froude.	Gebers.	Author.	Froude.	Gebers.
8	0·52	1·00	0·98	0·90	1·06	0·81	0·80		1·31	1·33		1·32	1·39
10	0·66	0·975	0·95	0·86	1·66	1·27	1·28		1·31	1·30		1·33	1·37
16	1·05	0·90	0·86	0·80	4·46	3·55	3·51	Average	1·25	1·27	Average	1·33	1·38
22	1·44	0·70	0·65	0·61	11·14	9·48	9·45	1·00	1·18	1·18	1·00	1·33	1·38
26	1·71	0·66	0·61	0·58	16·34	14·16	14·03		1·15	1·17		1·32	1·38
28	1·84	0·66	0·60	0·58	18·70	16·19	16·06		1·15	1·16		1·36	1·37

TABLE IV.

Comparison of the Measured Total Resistance of the Tug-boat with the Total Resistance deduced from the Model by the New Formula and Froude's and Gebers' Formulæ.

Speed V, Knots.	$V/\sqrt{L}$.	13-Ft. Model at 58·5° F. and in Fresh Water, at Corresponding Speeds.			Tug-boat at 61·0° F. and in Sea-water.								
		Calculated Frictional Resistance / Measured Total Resistance			Deduced Total Resistance, Tons.			Measured Total Resistance / Calculated Total Resistance $(R)_t/(R)_t'$.			Factor to be Applied to the Calculated Frictional Resistance to make $(R)_t/(R)_t' = 1$.		
		Author.	Froude.	Gebers.	Author.	Froude.	Gebers.	Author.	Froude.	Gebers.	Author.	Froude.	Gebers.
4	0·37	0·67	0·69	0·60	0·328	0·280	0·295	1·04	1·21	1·16	1·06	1·36	1·30
6	0·56	0·67	0·66	0·59	0·714	0·616	0·646	1·04	1·20	1·15	1·06	1·36	1·30
7	0·65	0·64	0·63	0·57	0·994	0·865	0·903	1·03	1·20	1·13	1·05	1·34	1·28

shows that there could be hardly any waves at such speeds. These facts assist in forming a conclusion that the new formula gives the adequate frictional resistance.

PART III. TOWING EXPERIMENT WITH A TUG-BOAT.

§ 10. After the towing experiment with the *Yudachi* in 1929, a towing experiment with a tug-boat of a length of 114·83 ft. and a displacement 296·93 tons was carried out in 1930 by order of the Admiralty. The towing ship was the tug-boat *Suzuura Maru.* Both of these tugs were the property of the Uraga Dock Co. The sheer draught and particulars of the boat are shown in Fig. 23A, Plate XXXI. The thickness of shell plating (Fig. 23B) was 7 to 10 lb. per sq. ft., i.e. the mean thickness was about 0·16 per cent. of the half-breadth of the boat.

The propeller was removed, but the rudder, bilge keels, shafting, and other appendages were left intact. The experiment was carried out directly after un-docking, therefore the bottom was a clean painted surface. The towing arrangement and dynamometer used were those used in the *Yudachi* experiment, and with $(D + L_1)/L_1 = 8 \cdot 22$ (Fig. 22, Plate XXXI.), the experiment was completed without any hitch.

The speeds experimented with were from 2·39 to 6·85 knots, Reynolds' numbers from 38×10^6 to 109×10^6, and the measured resistances from 0·134 to 0·980 tons. The whole result is shown in the Table and Fig. 25, Plate XXXI.

A 4-ft. similar model with a varnished surface and a 13·12-ft. similar model made of paraffin wax were tested at the small Tank, and the new Tank respectively, both the models being fitted with all the appendages similar to the actual ship. The scale effect C of these models with appendages was obtained by the same method as for naked models referred to in § 7 above, on the assumption that the method is equally applicable to models with appendages. A critical speed did not exist in the case of the 4-ft. model. Wall and bottom effect were not present in either model. The surface area, including all appendages, were taken for S in the new formula.

The residuary resistance of the model was obtained as usual, and the residuary resistance of the actual ship was deduced by the law of comparison as shown in Fig. 24 and Table, Plate XXXI.

For the calculation of R_f of the actual ship, the new formula was taken

$$R_f = K_1 \, S \, V_K{}^{1 \cdot 90} \, \rho \, \nu^{0 \cdot 165},$$

where S = total surface area of the main body and all appendages. The above method of deduction would be admissible for appendages of model and ship with the low value of $V/\sqrt{L}$ as in this experiment. No correction for air resistance was made for either model or actual ship in this case.

§ 11. To the residuary resistance $(R_w)_t$ deduced from the model adding the frictional resistance $(R_f)_t'$ calculated from the new formula with the measured ρ and ν, the total resistance $(R)_t'$ was obtained, and $(R)_t'$ to base V curve was plotted as shown in Fig. 25. When the measured total resistance $(R)_t$ was plotted upon this curve, it was found to lie a little above the $(R)_t'$ curve as shown in the Figure. Further, as can be seen from the Table in the same Plate, the ratios of the measured total resistance to the calculated total resistance $(R)_t/(R)_t'$ are 1·028 to 1·175 at five different speeds. The mean value for the five different speeds is 1·087, and for the two highest speeds 1·039. The actual resistance is always greater than the deduced resistance.

From $(R_f)_{55^\circ F.}$, which is the frictional resistance $(R_f)_t$ deduced from the measured total resistance by subtracting the deduced residuary resistance, and corrected to 55° F. ($\rho = 1 \cdot 981$ and $\nu = 1 \cdot 331 \times 10^{-5}$ as deduced from the measured ρ and ν in this series of experiment), K_2 values calculated are 0·0108 to 0·0128. The mean value of five K_2's being 0·0116, and that of the two highest K_2's $0\cdot0110$, they are greater by 13 and 7 per cent. respectively, compared with the value corrected for the above ρ and ν in the standard painted surface of $0\cdot01033$.

A comparison of these results with those obtained from other formulæ is given in Table IV (page 298).

In this Table a considerable difference observable in comparison with the case of the *Yudachi* is that the total resistance calculated by the new formula is somewhat smaller than the actual resistance by about 4 per cent. But as usual it is greater than that deduced by Froude's or Gebers' formula. At speeds of 4 to 7 knots the ratios of the frictional resistance calculated by the new formula to those calculated by other formulæ are $1\cdot25$ to $1\cdot30$ in Froude's, $1\cdot39$ to $1\cdot41$ in Gebers'.

In this ship the total resistances of the model as well as the ship at low speeds are much bigger than the frictional resistance determined by the new formula, unlike the case of the *Yudachi*. It may be considered as rather a rare case that the total resistance becomes the frictional resistance at a speed up to 9 knots as in the case of the *Yudachi*.

PART IV. FRICTIONAL RESISTANCE AND TOTAL RESISTANCE OF LONG PLANKS AND SHIPS.

Extension of the New Formula to the "Greyhound" Experiment.

§ 12. In the foregoing three Parts the measurement of the frictional and total resistances of a long plank or a plank ship as well as of two ships was described. Before discussing the general consequence deduced from the above results, we will examine how the new formula is applicable to Froude's classical experiment with H.M.S. *Greyhound*.

In the towing experiment with the *Greyhound*, the maximum speed $12\cdot8$ knots corresponds to a speed-length ratio $0\cdot8$ of the towing ship (that of the *Greyhound* being $1\cdot0$). Although the clear distance between the towing ship *Active* and the *Greyhound* was only about one-quarter the length of the *Active*, there would have been no effect of the wake and screw race, and also practically no effect of the wave system, as the *Greyhound* was towed from the end of a 45-ft. spar projected outboard of the *Active*, and the speed was low. The temperature is assumed to be 55° F. in the model as well as in the ship in the following calculation.

In the first place the frictional resistance of Froude's paraffin model of 1/16 full size of the *Greyhound* will be found by the new formula

$$(R_f)_{55^\circ \text{F.}} = 0\cdot01008\,S\left\{1 + (L' + C\,L)\,x/(6\,S)\right\}V_K^{1\cdot90}$$

or

$$(R_f)_{55^\circ \text{F.}} = 0\cdot0332\,S\left\{1 + (L' + C\,L)\,x/(6\,S)\right\}V_K^{1\cdot90}\,(\rho\,\nu^{0\cdot165})_{55^\circ \text{F.}},$$

where $x = 0\cdot26$ in., $C = 3\cdot35$ for the condition of normal displacement, and $C = 2\cdot70$ for the condition of light-load displacement from C to base $\Delta/(\frac{1}{100}\,L)^3$ curve. That is

$$(R_f)_{55^\circ \text{F.}} = 0\cdot01008 \times 1\cdot069\,S\,V_K^{1\cdot90} \text{ for normal displacement,}$$

and

$$(R_f)_{55^\circ \text{F.}} = 0\cdot01008 \times 1\cdot064\,S\,V_K^{1\cdot90} \text{ for light displacement.}$$

By the law of comparison, the residuary resistance of the actual *Greyhound* is found. The frictional resistance of the actual *Greyhound* is obtained from the new formula,

$$(R_f)_{55^\circ \text{F.}} = 0\cdot01033\,S\,V_K^{1\cdot90} \quad \text{or} \quad 0\cdot0331\,S\,V_K^{1\cdot90}\,(\rho\,\nu^{0\cdot165})_{55^\circ \text{F.}}.$$

The sum of this resistance and the residuary resistance deduced from the model is considered as the calculated total resistance. The original figures in Froude's paper of

1874 are reproduced in Figs. 27, 28A, and 28B, Plate XXXII. The curves of frictional resistance worked out by the author (Fig. 27) have been added to the original "curves of resistance of the *Greyhound* model." The curve of frictional resistance and the spots showing the values of the total resistance worked out by the author (Fig. 28A) have been added to "curves of actual resistance of H.M.S. *Greyhound* compared with that above deduced from model." Although in Froude's paper the deduction at light displacement was not shown, the author worked this out similarly and the result is given in Fig. 28B.

It will be seen that the calculated result is in agreement with the actual result in both the normal and light conditions. As shown in Table V., in either the normal or light condition the ratio of the actual resistance to that worked out by the new formula is approximately independent of the speed except in cases at the low speeds of 480 to 640 ft. per min. The mean value of this ratio at the five speeds of 800 to 1,200 ft. per min. is 1·00 and 0·99 respectively. Therefore, if the bottom copper surface of the *Greyhound* is assumed to be equivalent to a clean painted surface, the new formula well represents the *Greyhound's* actual resistance. If it is assumed as a clean brass bottom, the above ratio 1·00 becomes 1·05 by using $K_2 = 0 \cdot 00986$.

TABLE V.

COMPARISON OF THE ACTUAL RESISTANCE OF THE *GREYHOUND* WITH THE TOTAL RESISTANCE DEDUCED FROM THE MODEL BY THE NEW FORMULA.

Speed V, Ft. per Min.	$V/\sqrt{L}$.	Normal Displacement.			Light Displacement.		
		Actual Resistance,* R.	Deduced Resistance by the New Formula, R′.	Actual Resistance / Deduced Resistance R/R′.	Actual Resistance, R.	Deduced Resistance by the New Formula, R′.	Actual Resistance / Deduced Resistance R/R′.
		lb.	lb.		lb.	lb.	
480	0·36	1,850	1,760	1·05	1,700	1,550	1·10
640	0·48	3,360	3,190	1·05	2,960	2,820	1·05
800	0·60	5,360	5,230	1·02	4,900	4,850	1·01
960	0·72	8,650	8,710	0·99	8,000	8,200	0·98
1,040	0·78	11,240	11,330	0·99	10,280	10,480	0·98
1,120	0·84	14,850	14,840	1·00	13,200	13,330	0·99
1,200	0·90	19,110	19,050	1·00	16,590	17,000	0·98

* Measured from the original curves.

Froude attributed the excess of the actual resistance of the *Greyhound* in the normal condition over the resistance deduced from the model to the deterioration of her bottom copper by age, and obtained a new deduced resistance curve practically coinciding with the actual resistance curve by correcting her surface to two-thirds varnish and one-third calico, that is, by increasing the frictional resistance of the *Greyhound* by about 30 per cent. he obtained agreement between the actual and deduced total resistances. If the deduction is made from a clean smooth surface without making such a correction, the ratios of the actual total resistance to that deduced from the model at the speeds of 640 and 1,200 ft. per min. are about 1·30 and 1·11 respectively.

In the case of the *Yudachi*, an increase of about 32 per cent. to the ship's frictional resistance as deduced by Froude's formula will secure agreement between the actual and deduced total resistances. If the frictional resistance be not so increased, the above ratio of the total resistances is 1·31 at the lowest speed and 1·15 at the highest speed, thus producing very close similarity with the case of the *Greyhound*.

Frictional Resistance and the Total Resistance of Long Planks and Ships.

§ 13. The towing experiments with the plank ship (length, 77·3 ft.), the ex-destroyer *Yudachi* (length, 232 ft.), and the tug-boat (length, 114·8 ft.) cover a wide range of Reynolds' numbers from 29×10^6 to $1{,}009 \times 10^6$, and as the greatest value of V L is 6,100 in knot-foot unit, which corresponds to 10·2-knot speed of a 600-ft. ship, or 15·3-knot speed of a 400-ft. ship, it will be seen that the experiments cover a wide range of actual ships.

The values of $(R_f)_t/(\rho\, S\, V^2)$ are found from $(R_f)_t$ deduced from the measured total resistance of actual ships and those of thin painted planks, and these values are plotted to curves with $V\,L/\nu$ as the base as shown in Fig. 29, Plate XXXII. Also similar values obtained by correcting $(R_f)_t$ of 1- and 2-ft. lacquered planks to a painted surface are shown in the same Fig. These ships and planks, as inferred from the new formula, are represented by curves which are practically parallel to one another, and are nearly straight lines, and the frictional resistance cannot be represented as a function of Reynold's number.

§ 14. The total resistance of various ships at present being generally considered to be deduced from Froude's formula, and sometimes from Gebers', the ratio of the total resistance deduced from the new formula to that deduced from Froude's or Gebers' formula for various ships will be now surveyed. From the results of the towing experiments with the plank ship and the two actual ships, the author believes that the total resistance deduced from his new formula may represent the actual resistance of ships in the majority of cases. If so, the above ratio in most cases may be considered as the ratio of the actual resistance to that deduced from Froude's or Gebers' formula.

In general, the new formula in comparison with other formulæ gives the following results regarding the frictional resistances r_f and R_f, and the residuary resistances r_w and R_w of the model and the ship. For models, calculated r_f is greater, and therefore r_w is smaller; and for ships, R_w deduced by the law of comparison is smaller, and R_f calculated is much greater. And as a final result the ship's total resistance $R_w + R_f$ is greater than that deduced from other formulæ. However, there may be some special cases where the model is considered to have a particularly large frictional resistance owing to the scale effect which is taken into account in the new formula, but not in the other formulæ. The case of the tug-boat, for which a small model such as 4 ft. in length was used, is one of these special cases, and as a result other formulæ give a very large R_w for the ship, and despite that R_f for the ship is small the total resistance may be even somewhat greater. Such small models not being generally used in ordinary experimental tanks, the case such as that of the tug-boat may be considered as a rare one.

Therefore, generally the new formula gives a greater total resistance. But the fact that it does not give so great a resistance as is imagined from a big frictional resistance is very clear in the examples of the *Yudachi* and the *Greyhound*, in which the ratio of the total resistance at the highest speed deduced by the new formula to that deduced by Froude's formula is 1·15 (1·16 by Gebers') in the *Yudachi*, and is about 1·11 in the *Greyhound*, i.e. in the two ships at the highest speeds it is necessary to add 11 to 15 or 16 per cent. to the total resistances deduced by Froude's or Gebers' formula for coincidence with the total resistance deduced by the new formula, or the actual total resistance. This percentage increases, however, with decrease of speed, owing to the fact that the frictional resistance occupies a more important part in the total resistance at low speeds. The factor to be applied to the ship's frictional resistance in order to make the total resistance deduced by Froude's formula coincide with that deduced by the new formula is about 1·32 to 1·30 for all speeds in both the cases of the *Yudachi* and the *Greyhound*, the factor for that deduced by Gebers' formula is about 1·38 in the *Yudachi*.

When a similar ship with four times the dimensions of the *Yudachi* is taken for a trial, the corresponding figures being 928 ft. in length, 23,600 tons in displacement, and 16 to 56 knots in speeds, the above-mentioned ratio of the total resistances becomes 1·43 to 1·19 when compared with Froude's formula, and 1·50 to 1·22 with Gebers'.

The author expects that the total resistance deduced by the new formula is not affected by the size of the model, and is almost definite in its value, as due consideration is paid to the scale effect of models as shown in the case of the analysis in the *Yudachi*. In other formulæ, however, the total resistance deduced might differ in accordance with the size of the model, and consequently the ratio of the total resistances and the factor for the frictional resistance above referred to would be altered according to the size of the model.

For further examination of the above relations, the ratios as stated above were computed for warships of various categories, models of which have been run in the Mitsubishi Tank for the Imperial Japanese Navy, and for merchant ships tested in the experimental Tank of the Ministry of Communications: these results were kindly placed at the author's disposal by the staff. The computed results for the ratios are given in Table VI.

It will be seen that the actual total resistances obtained from the towing experiments and the total resistances of the actual ships as deduced by the author's formula from ordinary models in the ordinary experimental Tanks as shown in Table VI. are 1·11 to 1·25 times the total resistance deduced by Froude's formula, or 1·16 to 1·33 times the total resistance deduced by Gebers' at the highest speed in each ship. In all the cases this ratio increases as the speed decreases.

The factor to be applied to the frictional resistance of the ship calculated by Froude's or Gebers' formula in order to make their deduced total resistance conform with the actual total resistance obtained by the towing experiments, or the total resistance deduced by the author's formula, is almost independent of the speed, and is 1·25 to 1·42 for various ships to Froude's, and 1·32 to 1·68 to Gebers', as shown in Table VI.

§ 15. Recent measurements of thrust, carried out in actual steam trials of ships, show that the resistance of the ship or E.H.P. predicted by the methods hitherto adopted is too small. After discussing the measured thrusts of the s.s. *Sheffield*, etc., Baker * stated that "where thrusts have been measured, these are usually higher than would be predicted from model resistance data." The case of the s.s. *Clairton* † is the same. In the case of the U.S. destroyer *Hamilton* ‡ a roughness factor of 1·25 was applied to the ship's frictional resistance calculated by Gebers' formula, and by this the E.H.P. was made to coincide with the E.H.P. deduced from the measured thrusts.

Conclusion.

§ 16. From the experiments with the plank ship, the ex-destroyer *Yudachi* and the tug-boat, the frictional resistance and the total resistance of ships are confirmed to be greater than those hitherto considered. Contrary to the original aim of obtaining a correct form for the generally accepted modern term $R_f = \rho S V^2 f(VL/\nu)$ by a faithful analysis of the experimental results of resistance, temperature effect and sea-water effect in the small experimental Tank of the Imperial Japanese Navy, the investigations have resulted in the formation of one inductive formula. As is described in the present paper, the total resistances deduced by this new formula not only coincide with the actual resistances in the plank ship, the ex-destroyer *Yudachi*, and approximately in the tug-boat, but are also in conformity with the result of the *Greyhound* experiments if her bottom is assumed to be equal to a clean painted surface. The author believes that the truth of Froude's method

* *Trans. North-East Coast Inst.*, 1925–26.

† Adams, *Trans. Soc. N.A. Mar. Engrs.*, 1930.

‡ Saunders and Pitre, *ibid.*, 1933.

TABLE VI.

COMPARISON OF THE E.H.P. DEDUCED BY THE NEW FORMULA WITH THAT DEDUCED BY FROUDE'S OR GEBERS' FORMULA.

Type of Ship	Battleship.		Battle Cruiser.		Cruiser.		Destroyer.		Mine Layer.		Merchant Ship (13,600 Tons).		Merchant Ship (5,300 Tons).		Merchant Ship (446 Tons).	
Length of model, ft.	14		15		16		12		21		20		20		18	
Speed, knots	12	22·5	20	26	26	33	26	34	12	19	14	19	11	14	10	14
E.H.P. ratio to Froude's	1·31	1·23	1·28	1·25	1·25	1·22	1·19	1·14	1·19	1·14	1·23	1·18	1·23	1·20	1·19	1·16
E.H.P. ratio to Gebers'	1·39	1·31	1·38	1·31	1·41	1·33	1·22	1·19	1·30	1·20	1·28	1·22	1·28	1·25	1·21	1·17
Factor * to Froude's	1·42	1·39	1·38	1·41	1·37	1·41	1·39	1·35	1·23	1·26	1·34	1·42	1·34	1·33	1·27	1·28
Factor * to Gebers'	1·63	1·62	1·60	1·60	1·65	1·68	1·51	1·51	1·41	1·41	1·62	1·60	1·48	1·48	1·33	1·33

* Factor to be applied to R calculated to make the deduced total resistance equal to that by the new formula.

TABLE VII.

RATIOS OF R.P.M. OF LEADING SHIPS IN SCREW-RACE EXPERIMENTS AND TOWING EXPERIMENTS TO R.P.M. OF THE SAME SHIPS WHEN THEY ARE RUNNING ALONE WITHOUT TOWING.

Leading Ship—Following Ship.	Where Experimented.	Length on W.L.		Measured-mile Speed V, Knots.	$V/\sqrt{L_1}$.	$\frac{D + L_1}{L_1}$	$\frac{D + L_1}{\lambda}$	Ratios.	Year.
		Leading Ship. L_1, Feet.	Following Ship. Feet.						
Experiment of wake and screw race—									
(1) *Shimakaze—Yudachi*, models	Mitsubishi Tank	5·708	4	1·48–3·00	0·62–1·26	1·5–7·5	34–1·8	1·00	1928
(2) A destroyer—a ship, models	A pond	26	15	1·53–3·94	0·30–0·77	2–8·66	119–7·3	1·45–1·37	1928
(3) Vedette boat—*Hashike* ..	Nagaura Harbour	56	26	2·81–8·70	0·38–1·16	3–8·5	82·5–3·9	1·33–1·13	1928
(4) *Katsura*—vedette boat ..	Outside Nagaura Harbour	270	56	8·15–9·45	0·50–0·58	6	43·9–32·7	1·17–1·18	1928
(5) *Shimakaze*—vedette boat ..	Outside Nagaura Harbour	326·25	56	7·99–10·19	0·44–0·56	3–6	49–21	1·07–1·04	1928
Towing experiment—									
Vedette boat—plank ship ..	Outside Nagaura Harbour	56	77·319	2·45–9·07	0·33–1·21	5–9	101–7·4	1·21–1·10	1928
Katsura—plank ship	Outside Nagaura Harbour	270	77·319	7·54–16·11	0·46–0·98	6	51·3–11·2	1·13–1·08	1928
Kayede—plank ship	Outside Nagaura Harbour	270	77·319	10·41–16·22	0·63–0·99	6	26·9–11·1	1·15–1·13	1928
Shimakaze—plank ship ..	Outside Nagaura Harbour	326·25	77·319	9·85–14·60	0·55–0·81	6	36·3–16·5	1·04–1·01	1928
Yukaze—Yudachi	Tateyama, Bay of Tokyo	326·25	232	5·61–20·97	0·31–1·16	4·68–8·56	87·4–11·4	1·09–1·05	1928
Shiokaze—Yudachi	Tateyama, Bay of Tokyo	326·25	232	18·29–26·33	1·01–1·46	8·43	14·8– 7·1	1·07–1·06	1929

of deducing the total resistance of ships from models is reasserted with greater accuracy by means of the new formula.

Further, the results of the application of this formula to various ships correspond with the modern generally accepted idea that the total resistance predicted from the model experiment is too small.

As it is, the frictional resistance of ships is greater than that hitherto considered, and it comprises a more important part of the total resistance. More serious consideration will have to be given to the dimensions and form of the ship as well as to the cleanliness of the bottom surface. Further, the total resistance being confirmed as being greater than that hitherto predicted from models, the propulsive coefficient determined or adopted will be increased, entailing a fresh consideration for the power required. The author hopes that the actual resistance of ships will be more widely and deeply investigated in future.

In concluding this paper the author expresses his deep gratitude to all members of the experimental Tank staff in the Naval Technical Research Department for their sincere and earnest co-operation in carrying out the investigations, which succeeded beyond the author's expectation.

APPENDIX I.

Effects of Wake, Race, and Waves produced by a Towing Ship on a Towed Ship.

(i) Behind the towing ship there would be produced a resultant stream of the wake in the same direction of the ship's progress, and of the screw race in the opposite direction, and with these the effect of the transverse wave system produced by the towing ship would be combined. Generally the screw race being the most prominent of the two, the direction of the resultant stream is astern, hence the resultant stream may be simply termed as a screw race. Needless to say, the greater the distance from the towing ship, the less will be the velocity of the screw race, and the more will the wave system have died out.

Regarding the effect of the screw race, if a current-meter were fitted below the water-line, directly in front of the bow of the towed ship, and a reading taken, then by subtracting the measured-mile speed of the ship from the current-meter record the speed of the screw race would be obtainable. However, at the front of a moving ship there would be a bow wake produced by the ship herself, therefore a correction for this is necessary. To eliminate the effect of the current in the sea due to tides, the mean of means of successive runs is taken for the ship's measured-mile speeds and the current-meter records.

In the small Tank the bow wake was measured by a current-meter fitted in front of an 8-ft. destroyer model. The distance D_1 between the bow of the model and the centre of the current-meter was changed to seven grades, and D_1/L_1, the ratio D_1 to the length of the model L_1 was ranged from $0 \cdot 06$ to $1 \cdot 00$. The result indicated the existence of a stream or wake in front of the model in the same direction as the model's progress, and the smaller D_1/L_1, and the greater $V/\sqrt{L_1}$, the greater was the ratio (Wake)/(Speed of model) or the wake factor. The result is shown in Fig. 2, Plate XXVI.

Also a measured-mile trial of a 56-ft. vedette boat was carried out in the calm water of the Nagaura Harbour, and the wake was measured from the mean of means of three successive runs with $D_1/L_1 = 0 \cdot 06$, L_1 being the ship's length. The measured current-meter speeds are shown in Fig. 1 (Plate XXVI.). It will be seen that the general condition is very similar to that of the 8-ft. destroyer model, but the values are greater in the vedette boat, thus with $D_1/L_1 = 0 \cdot 06$,

$V/\sqrt{L_1}$	$0 \cdot 6$	$1 \cdot 0$	$1 \cdot 2$	$1 \cdot 5$
8-ft. destroyer model ($C_p = 0 \cdot 64$)	0	$0 \cdot 9$	$1 \cdot 4$	$2 \cdot 2$
56-ft. vedette boat ($C_p = 0 \cdot 62$) ..	$1 \cdot 7$	$2 \cdot 7$	$3 \cdot 2$	$3 \cdot 4$

The wake so measured would change according to the form. However, C_p, the prismatic coefficient of ships which have a close relation to the present investigation of screw race, being generally about $0 \cdot 6$, the result of the vedette boat was taken as the standard value, and for change of D_1/L_1, the standard value was interpolated from the result of the 8-ft. destroyer-model experiment.

(ii) Again, a current-meter was fitted at a distance D behind the same 8-ft. destroyer model, and the wake behind the model at a point in the extended middle line of the model was measured. The result obtained was that the wake factor at the same place was practically constant for a change in speed of model, but at different places showed a remarkable decrease with the increase of distance. The general conditions are shown in (Wake factor behind ship) against D/L_1 curves (Fig. 3, Plate XXVI.).

Also a 26-ft. self-propelled destroyer model with twin screws was subjected to a basin trial, by fixing the hull and revolving propellers in a rolling tank (60 ft. $\times$ 20 ft. $\times$ 10 ft.), and the screw-race speed behind in the extended middle line of the model was measured. In this case the screw-race speed at the same place was found to be practically proportional to R.P.M., and if the available speed of the model with the same R.P.M. was surmised, the screw-race speed would be also proportional to that speed, i.e. the supposed screw-race factor, or (Screw-race speed)/(Supposed speed of model) is independent of R.P.M. or the supposed speed of the model. As shown in (Screw-race speed) against D/L_1 curves (Fig. 4, Plate XXVI.), the screw-race speed diminishes conspicuously with the increase of the distance, and this will apply also to the supposed screw-race factor. For instance, the factors for R.P.M. 475 and 950 are 78 and 80 per cent. respectively when $D/L_1 = 0 \cdot 077$ (or 67 and 65 per cent. of the pitch of the propeller multiplied by R.P.M.), and 12 and 16 per cent. respectively when $D/L_1 = 1 \cdot 5$. Further, the screw races at various distances from the middle line on the transverse plane at $D/L_1 = 0 \cdot 62$ were measured. As a result the race directly behind the propeller was found to be the greatest, corresponding to about $1 \cdot 1$ times that at the middle line. The race at a distance three times the half-breadth of the model was half that at the middle line.

Therefore the wake factor and the supposed screw-race factor when the model is stationary are practically independent of the speed of model, and diminish conspicuously with increase of distance, hence it may be considered more than probable that the screw-race factor of an actual ship would be also independent of the ship's speed, and diminish greatly with the increase of the distance.

(iii) As the next step, a real screw race was measured with two ship models and three ships. For leading ships, the would-be towing ships of the plank ship and of the *Yudachi* or their models were employed, and for following ships, suitable boats or ship models were employed.

Of the five cases, case (1) was entrusted to the Mitsubishi Tank. In this case both leading and following models were fixed to the same carriage, and the leading ship was run at the corresponding number of revolutions to the R.P.M. of the actual ship running alone. Therefore the race would be too small, when compared with that of the towing condition in which R.P.M. must be increased in order that the ship may have the same speed as if running alone. In case (2), a ship model was towed by a 26-ft. self-propelled destroyer model in a pond. In cases (3), (4), and (5), suitable boats were towed by a 56-ft. vedette boat and a 270-ft. destroyer, both of which were to be the towing ships of the plank ship, also by a 326-ft. destroyer, which was to be the towing ship of the plank ship and the *Yudachi*. In each case a current-meter was fitted in front of the following ship. The amount of the screw race was determined by the relation, (Current-meter record) — (Measured-mile speed) + (Bow wake) = (Screw race), and in cases (2) to (5) the current-meter record and the measured-mile speed were determined from the mean of means of three successive runs, and the bow wake from the conclusion of (i) in this Appendix.

Then if the speed of screw race obtained in cases (2) to (5) was taken as a fraction of the measured-mile speed, and called a factor of screw race, and the distance between F.P. of the leading ship (length on W.L. $= L_1$) to F.P. of the following ship was represented by $D + L_1$, for each different $(D + L_1)/L_1$ the factor was plotted on the $V/\sqrt{L_1}$-base. Observing the general tendency (Figs. 5A to 5F, Plate XXVI.), it will be seen that the value of the factor for a ship at a constant $(D + L_1)/L_1$ is independent of the speed, as deduced in (ii), and between the different ships it is independent of $V/\sqrt{L_1}$, and throughout the four cases values may be taken as almost similar to one another, that is to say, it may be taken that there is a constant factor of screw race at a constant $(D + L_1)/L_1$ throughout the four cases. Plotting the factor of screw race obtained for each of $(D + L_1)/L_1$ on $(D + L_1)/L_1$-base, a curve almost asymptotic to the co-ordinates was obtained as shown in Fig. 6 (Plate XXVI.). Further, the result of case (1) independently dealt with is also shown in Fig. 6, from which it will be seen that though the values of the factor are somewhat low, the nature of the curve is exactly the same. In short, as the results of cases (2) to (5), the factor of screw race is 1 per cent. or less, when $(D + L_1)/L_1 = 5 \cdot 5$, and may be considered almost negligible when $(D + L_1)/L_1 = 7$ to 8. Also no relation between the factor and $V/\sqrt{L_1}$ would also mean no relation between the factor and the wave system. Though there may be a certain influence of the latter when $(D + L_1)/L_1$ is small, it was not specially noticed.

Now, in towing, R.P.M. and screw race of the towing ship are greater than when the ship is running alone at the same speed, but the wake is the same in either case, therefore the resultant screw race is greater in the case of the towing ship, and the greater the resistance of the towed ship is, the greater the resultant screw race would be, and when the wake is negligibly small, it would increase almost proportional to R.P.M. Hence, to arrive at such a simple result as shown in Fig. 6 from the four cases mentioned above is theoretically unreasonable. But at the same time the deduction indicates the insignificance of the difference due to these reasons in the actual experiments, and explains the general similarity of the results in these cases. Ratios of R.P.M. of leading ships in screw-race experiments to R.P.M. of the same ships when they are running alone, and also those in the towing experiments with the plank ship and the *Yudachi*, are shown in Table VII (page 304).

If $(D + L_1)/L_1 = 6$, i.e. the distance clear between two ships be five times the length of the towing ship, the screw race would be $0 \cdot 65$ per cent. of the measured-mile speed; this was taken as the standard distance between the two ships in the towing experiment.

Though the breadth and depth of screw race were not clearly known, from the fact that the towing ship was broader and deeper than the towed ship, and from the result of screw race in a basin trial described in (ii), and since the towed ship was nicely following in the towing ship's course, the whole of the immersed body of the towed ship was assumed to be running in the uniform current of screw race. And $(1 + w) \times$ (Measured-mile speed) was taken as a real relative speed of ship to water, w being the factor of screw race.

(iv) Next to be considered was the effect on the wave-making resistance of the following ship due to the transverse wave system produced by the leading ship. No special effect produced by the wave system on the speed of screw race was observed with the experiments in (iii). However, when $(D + L_1)/L_1$ is small, a great influence on the resistance of the following ship is quite evident from the instances of fleet manœuvres. Also from Motora's investigation on two similar destroyer models in the Mitsubishi Tank,* it was seen that when $V/\sqrt{L_1} < 1$, the effect on the residuary resistance of the following ship is very small, but with the increase of $V/\sqrt{L_1}$, the effect is gradually increased, and for every $\frac{1}{2}\lambda$ distance where $\lambda = 1/1 \cdot 8\, V^2$ the maximum and minimum values are produced alternately. As the distance increases, the fluctuation gradually diminishes, and finally at a certain distance it assumes a normal value, i.e. no effect. For instance, when $V/\sqrt{L_1} = 1 \cdot 3$, if the distance is small, there will be a considerable effect, but if the distance is increased to, say, $(D + L_1)/L_1 = 4 \cdot 7$ or $(D + L_1)/\lambda = 5$, the effect would be very small.

Also, from an investigation on destroyers of similar type in the small Tank by Constructor Captain Viscount Tokugawa, it was seen that with $V/\sqrt{L_1}$ up to $1 \cdot 5$ at $(D + L_1)/L_1 = 6$, the wave profile is almost a still-water level, and the resistance is also practically the normal value.

In Tank experiments of this kind, the influence of wall effect is quite evident when the distance of two ships is large, and it is exceedingly difficult to establish a general rule from the Tank experiment. Needless to say, the effect is due to the interference of the transverse wave system produced by the leading ship and of that produced by the following ship, therefore it has no bearing of importance on the towing of the plank ship which produces practically no waves. But it would be an important problem in the case of the *Yudachi*. In short, the best solution was to bring the distance of two ships to such an amount as to be practically free from the effect.

In the case of the plank ship the maximum $V/\sqrt{L_1}$ experimented with of the towing ship was $1 \cdot 21$, and $(D + L_1)/L_1 = 6$ and $(D + L_1)/\lambda = 7 \cdot 4$. Therefore, supposing there was a considerable residuary resistance, there would be no possibility of any effect. In the case of the *Yudachi* $(D + L_1)/L_1 = 8 \cdot 5$ was taken when $V/\sqrt{L_1}$ of the towing ship > 1 and the maximum $V/\sqrt{L_1}$ being $1 \cdot 46$, and $(D + L_1)/\lambda$ being $7 \cdot 1$, there would be also practically no effect of waves on resistance.

* *Jour. Soc. N.A., Japan,* Vol. 28, 1921, and Vol. 41, 1927.

Further, in each case of the plank ship and the *Yudachi*, a measure was taken to check the effect on the resistance by changing $(D + L_1)/L_1$ into several grades at certain constant speeds, i.e. from 5 to 9 at $V/\sqrt{L_1} = 1 \cdot 02$ for the former case and from $4 \cdot 9$ to $8 \cdot 4$ at both $V/\sqrt{L_1} = 0 \cdot 76$ and $= 0 \cdot 85$ for the latter. These are described in the respective experiments, and the deviation of each value of the resistance from their mean value when the distance was thus changed being $1 \cdot 3$ per cent. in the plank ship and about $1 \cdot 9$ per cent. in the *Yudachi*, it proved a successful evasion of the effect of screw race as well as that of waves.

APPENDIX II.

Fouling Effect.

When the plank ship was docked, five 5- and 8-ft. thin brass plates (thickness $0 \cdot 2$ in., with sharp ends and edge), coated exactly the same as the plank ship, were immersed in the sea at the same place where the plank ship was moored after undocking, and the effect of fouling was tested at the small Tank.

Each of the towing experiments of the nine series was commenced after 8 to 41 days out of dock, but this number of days did not necessarily represent the fouling of bottom at the time of the experiment, because in such instances as series (1) to (4) which was carried out after a long time out of dock of 28 to 41 days, divers were sent down before starting for the experiment to clean the bottom thoroughly, and therefore the bottom may be taken practically as a clean surface. In series (5) to (9), which were carried out after 8 to 24 days out of dock, the bottom cleaning was not made for fear of interfering with the watertightness by touching the bottom, but as the ship was run a few miles before the commencement of the experiment, the bottom cleaning may be considered to be effected in that way as will be described later.

Tests in the Tank were carried out to correspond with the actual condition of the nature of the surface of the plank ship. The results are shown in Figs. 7A and 7B (Plate XXVI.). On the first day out of the water, each of the five plates was run about 5,000 ft. in the fouled condition, and it was observed that in all the plates, except plate (V.), the resistance gradually diminished as the distance of run was increased. On the second day, some of the plates were cleaned and some were not, and were subjected to a test of four days, and the result obtained was generally as follows:—

Plates (II.) and (III.). These were coated with the *Takata* composition immersed 33 and 63 days respectively and were to represent series (1) to (4) of 28- to 41-day immersion. On the second day their surfaces were lightly cleaned and were subjected to tests. It was found that the resistance was practically constant throughout four days, with the mean value of $K_2 = 0 \cdot 0100$, and was equal to a new clean *Takata* surface separately tested. Therefore a surface treated in such a manner may be recognized as a clean condition, and the correction factor to bring the measured resistance in series (1) to (4) to those of the clean surface coated with the same paint may be taken as unity.

Plates (I.) and (IV.). These were coated with the *Takata* and the *Veneziani* compositions respectively and immersed 17 and 10 days respectively. In the same condition after the first day's test they were subjected to the tests of the second and the following days. It was found that in each of the plates the resistance was continually diminished though the rate of reduction due to the increase of the distance run became gradually small, yet it continued diminishing until finally it became almost the same as that of a clean surface. Within the scope of the experiment, in plate (I.) the reduction was from $K_2 = 0 \cdot 01056$ of the first day to $K_2 = 0 \cdot 01018$ of the fifth day, i.e. from $5 \cdot 6$ to $1 \cdot 8$ per cent. more than a clean *Takata* surface separately tested, and in plate (IV.) the reduction was from $K_2 = 0 \cdot 01119$ to $K_2 = 0 \cdot 01048$, i.e. from $7 \cdot 0$ to $0 \cdot 2$ per cent. more than a clean *Veneziani* surface of $K_2 = 0 \cdot 01046$ separately tested. Therefore it may be admitted that these plates, when they were run a sufficiently long distance, were cleaned to a degree equal to that of a clean surface coated with the same kind of paint. Hence with an actual ship after 10 to 17 days out of dock, provided she ran a sufficiently long distance before the trial, the correction factor may be taken as unity for the measured resistance; the series (5) to (7) of the plank experiments may be taken as corresponding to the case of the *Veneziai* surface of 10-day immersion.

Plate (V.). This was coated with the *Veneziani* composition and immersed 24 days and corresponds to series (9). Quite unlike the other plates, barnacles grew on the surface of this plate as observed on the bottom of the plank ship. Probably owing to the above reason, the resistance of the plate was not practically reduced throughout the experiments of five days in the Tank, though a small reduction was effected on the first day. K_2 from the second day to the end of the experiment was almost constant in value, viz. 0·01262, i.e. 20·6 per cent. increase over $K_2 = 0 \cdot 01046$ of the clean surface with the same composition. Therefore, the correction factor may be taken as 1/1·206 or 0·829 for series (9). For series (8), from a curve titled *Veneziani* becoming a constant resistance after a long run" in K_2 to base (days of immersion) curves (Fig. 7B, Plate XXVI.), $K_2 = 0 \cdot 01142$ was obtained for a 19-day immersion, i.e. 9·2 per cent. increase over the clean surface. Consequently the correction factor may be taken as 0·916.

For all the plates except (III.), n's in V^n were generally less than 1·90 in the beginning of experiments and approaching 1·90 at the end of experiments. For plate (III.), immersed 63 days, n was 2·02 in the middle of the first day and 1·90 after cleaning on the second day. In all cases K_2's were obtained by assuming $n = 1 \cdot 90$.

All the correction factors above referred to are those to correct each fouled surface to the clean surface of the same nature. Therefore to correct each fouled surface to the standard painted surface of $K_2 = 0 \cdot 01004$ the *Takata* surface in series (1) to (4) has to be once more multiplied by 1·004 and the *Veneziani* surface in series (5) to (9) divided by 1·041. However, it was decided that both the *Veneziani* and the standard painted surfaces are equal in smoothness when they are actually used for coating ships (as is usually recognized in the dockyard), and it was deduced that the greater K_2 observed for the test plate with the *Veneziani* surface is due to the fact that the surface, after being coated, was not even and smooth enough owing to the smallness of the area of the plate for this kind of paint. Correction factors finally adopted were 1·004 for series (1) to (4), 1·000 for series (5) to (7), 0·916 for series (8), and 0·829 for series (9). Series (5) and (6) were conducted, as a matter of fact, to obtain the relative correction factor against series (1) to (4) as stated in § 2, the plank ship being towed by the same boat with the equal $(D + L_1)/L_1$ and the equal range of speeds at the same mile-posts throughout the experiments in these series. The results are shown in Table I., § 3; the mean value of K_2's for 12 different speeds in series (5) and (6) is 0·01033, whereas that of K_2's for 20 different speeds in series (1) to (4) is 0·01037 and the final mean value for series (1) to (6) is 0·01036; therefore the correction factors for these series and other series connected therewith seem to be adequate.

An important deduction from the above fouling experiments in the Tank is that for such a fouled plate as plate (V.), which was immersed in the sea for 24 days and on which barnacles had grown, the resistance is not reduced by a continuous motion, but for such fouled surfaces as plates (I.) and (IV.), which were immersed in the sea for 10 to 17 days and on which no barnacles had grown, the resistance is gradually reduced by moving and finally becomes equal to that of a clean surface after a long run.

APPENDIX III.

Towing Arrangement and Dynamometers.

The towing arrangement and dynamometer for the plank ship are shown in Fig. 8 (Plate XXVII.) and those for the *Yudachi* in Fig. 14A (Plate XXIX.). In each case the end of the towing hawser was connected to the towing truck, resting upon four hard steel rollers on the forecastle deck. The truck was made to have a horizontal movement due to the towing force and to transmit only the horizontal component of the towing force or the resistance of the towed ship to a dynamometer fixed to the hull at a small distance abaft the truck and connected to the truck by a wire rope or a rod. The friction due to this arrangement was very small, and after the completion of the whole arrangement it was measured and found to be quite negligible.

Dynamometers: Small ones were of a type having two parallel spiral springs, whose aggregate linear extension was indicated on the dial by means of a rack and pinion. These were used in the plank-ship experiments and were of 0·6-, 0·8-, and 2·4-ton capacity. The large ones used in the *Yudachi* experiment were a flat-spring type and were of 5-, 20-, and 60-ton capacity. Of these, the 5- and 20-ton capacity ones were made by the Schäffer and Budenberg Co., Germany, the others were all of Japanese make. In addition to usual statical calibrations, a dynamical calibration to suit the towing purpose was made for some of them. As an example, the 20-ton dynamometer will be explained:—

(*a*) It was tested, starting from no load and by adding 2 tons each time very quietly up to 20 tons, and again starting from 20 tons and taking off 2 tons each time to no load. By plotting the readings on the load-base, a line obtained for the gradual increase of the load was practically a straight line, and if it was called the standard line, then a line representing the gradual decrease of load from the top load down to no load was found to lie slightly above this standard line, and a loop having the standard line as its lower edge was formed. The maximum width of the loop was observed near the 10-ton load and indicated almost as much as 0·7 ton. This showed that 20-ton actual load was too much for the 20-ton dynamometer. Again, it was tested through the range 0 to 15 tons, 0 to 12 tons, 0 to 9 tons, and 0 to 5 tons. It was found that all upward lines conformed with the standard line, but the downward lines formed loops having widths gradually diminished, the maximum widths were 0·36 ton at 7 tons, 0·25 ton at 6 tons, 0·12 ton at 4 tons, and 0·01 ton at 3 tons respectively.

(*b*) Further, tests for no load to 6 tons, 6 tons to 3 tons, 3 to 7, 7 to 4 . . . 12 to 9, 9 to 13 tons were made, and the back journey from 13 tons to no load was made in the same manner. Repeating this process for a few times, the upward line was always found to lie on the standard line, but the downward line was found to lie on the standard line only in the first three processes, and from the fourth process it formed a very narrow loop.

(*c*) Each of 2, 4, 6, 8, 10, and 12 tons was loaded separately and quietly, and the readings were taken, and in each case it was temporarily released from the load by means of a cam mechanism, then suddenly was subjected to an instantaneous loading of the same load. In these cases the indicator oscillated between 0·8 to 3·2 tons in the 2-ton load and 10·70 to 13·40 tons in the 12-ton load, and the mean value of the maximum and minimum readings was taken, also the final reading when settled was taken. In comparing the results, with 6-ton load and under, three readings of initial, mean, and final were exactly the same, but with 8 tons and above, while the mean and final readings were exactly the same, each was about 0·05 ton greater than the initial reading. Hence it may be admitted that the initial reading being in conformity with the standard line, if the mean value of oscillation was taken, a correct record would be obtained for the load of 6 tons and under even with an instantaneous loading, and about 0·6 per cent. error for the load of 8 tons and 0·4 per cent. for 12 tons.

The 5-ton dynamometer was tested exactly in the same manner, and similar results were obtained. Thus the nature of spring dynamometers being ascertained, only statical tests were made for 60-ton and other dynamometers, and generally results of a very similar nature were observed.

But in an actual towing, the nature of the load would be perhaps one intermediate between (*b*) and (*c*), and the smaller the load for the dynamometer capacity, the less would be the error. Hence by using comparatively large dynamometers for the anticipated loads on the basis that the greatest force to be measured is to be about $\frac{1}{3}$ the dynamometer capacity, and taking the mean readings of oscillations, sufficient accuracy for practical purpose would be obtained. The greatest force to be measured in the case of the plank ship being about 0·6 ton, small-capacity dynamometers were used. In the case of the *Yudachi* the greatest force being about 18 tons, large-capacity dynamometers were used.

Naturally, with spring dynamometers there would be a temperature effect, but it was found to be very slight with those of 20-ton capacity and under. Further, as the calibration before and after the towing experiment was found to be exactly the same, no account of this effect was taken. A noticeable temperature effect was observed with the 60-ton dynamometer, and temperature corrections were made in the readings.

In the case of the plank ship the time required for one measured-mile run was assumed. This assumed time was divided into four equal parts and the mean dial reading during each fourth part was taken, and the sum of the four mean readings divided by four was taken as the measured resistance of that run. In the case of the *Yudachi*, dynamometers of 5-ton capacity and above were used. As each of them was fitted with an automatic recorder which registers time and load the mean of continuous records was obtained from this recorder. The mean value thus obtained agreed very well with the mean of four readings obtained by the same method as taken in the plank ship. However, on more detailed examination it was found that the direct dial reading was more accurate, therefore the mean of four readings was taken as the correct record, and the automatic records were taken only as aids.

DISCUSSION.

Sir James B. Henderson, D.Sc. (Associate): Admiral Hiraga has asked me specially to take part in this discussion, otherwise I would not venture to do so. In pre-war days I was Professor of Applied Mechanics at the Royal Naval College at Greenwich, and at that time we always had Japanese students with us: one a constructor and the other an engineer. Although our own students were specially selected for their brilliance, the Japanese were also, and the international competition was healthy for both, and was very keen. One of the best students I ever had in my time was Admiral Hiraga—he was really brilliant. I have followed his career from a distance, and have been pleased to see that he has been in turn Chief Constructor, Head of the Research Laboratory in Japan, and is now, in his retirement, starting as Professor in Tokyo University, where he joins a brilliant group of Professors:—Tanakadate, who is the greatest physicist of Japan,—I worked with him as a student in Kelvin's Laboratory,—and Professor Nagaoka, whom I worked with in Berlin; Professor Yamamoto, who has just died, was a fellow-student of mine in Glasgow. The record of Tokyo University I am certain will not suffer by Admiral Hiraga's appointment there.

On the technical side the subject is not one that I can deal with adequately, because I have not taught it since the war, and so much research work has been done since then. Due to the exigencies of war I gave up my Professorship at the request of the Admiralty to devote my attention to other matters, so that I cannot profess now to be an expert in the matter, and I will leave the technical side to those who are better able to express an opinion.

The teacher's work is something like the gardener's: he plants the seed, he may see the seed sprout in the look of intelligence on the student's face, but he rarely sees the full-grown tree. I would like to think that the tree represented by the colossal piece of work described in the paper and covering many years, was inspired to some extent by the seeds which Admiral Hiraga gathered at Greenwich College.

Dr. G. Kempf (Member): We should be extremely grateful to Admiral Hiraga and to the Imperial Japanese Navy for having brought before our Institution this thorough investigation of friction for long planks and for real ships. I think it is the first time since Froude made his experiments with the *Greyhound* that we have had such an investigation which helps us to calculate the skin friction for real ships.

It is certainly impossible without study to fully appreciate such a paper as this, which has involved a work of two or more years, or to discuss that paper now with any approach to finality. I am sure that Admiral Hiraga will not expect us to do that here.

There are, however, some questions which I would like to put to the author.

We see that the following ship was in the wake of the towing ship, though there certainly was a very large distance between the two, but does not the resistance depend on the wake and on the turbulence set up by the first ship in a rather uncertain way, as may be seen from Figs. 5 and 6 (Plate XXVI.)? I remember that Froude towed the ship from a line at the end of a spar. I am not quite sure if you tried that also, Admiral Hiraga?

Professor Hiraga: No spar; simply with a tow-rope from the stern of the towing ship.

Dr. G. Kempf: Now I come to the "Conclusion" given at the end of the paper. It is both interesting and satisfactory that Admiral Hiraga also found that the frictional resistance must be greater than that calculated by the normal formula of Froude, which we have used up to now. I have heard that in Washington they use the formula of Gebers plus 15 per cent., and it seems to be quite reasonable, as Admiral Hiraga gives a similar addition.

The only point upon which I am not quite clear is at the foot of page 303. Admiral Hiraga says, "but are also in conformity with the result of the *Greyhound* experiments if her bottom is assumed to be equal to a clean, painted surface." I did not quite get what the surface of the *Greyhound* really was. Perhaps Mr. Payne can tell us.

Mr. M. P. PAYNE (Member): Actually, she was a wooden ship and covered with copper. I shall make a remark concerning that myself later.

Dr. G. KEMPF: I am not so sure that the bottom of the *Greyhound* can be assumed to be equal to a clean, painted surface. It seems that we must have much further research —and Admiral Hiraga wishes it also—about the kind of roughness which the ship really had. Therefore, I would be glad if Admiral Hiraga could tell us what kind of roughness his destroyer had: how the plates and their butts were, and how the shell looked.

Sir WESTCOTT ABELL, K.B.E., M.Eng. (Vice-President): I am somewhat in the position of Sir James Henderson, because I have had to deal with, or instruct, students in the principles that underlie flow resistance. Admiral Hiraga was a student of mine at Greenwich. He was one of the best students that I have had through my hands, and while he was with me he carried out two pieces of research, one of which was very complicated. As to his aptitude for research and his capacity to carry out work of this kind, I am quite satisfied that his analysis and measurements are as good as anybody can make them. Therefore, if that is the case, and if Admiral Hiraga finds that there is a distinct difference between the results of his experiments and the usually accepted method of calculations, we are driven in argument to this question: as to whether, for example, Admiral Hiraga and William Froude are measuring the same thing. That seems to me to be the point at issue. As Dr. Kempf observed, William Froude towed at the end of a spar; Admiral Hiraga towed in the remains of the wake of the ship. The point we have first of all to satisfy ourselves about is: does the wake of a ship as he found it increase the so-called frictional resistance by some 30 per cent., or is there a wave factor as well which complicates the issue?

When I found such a striking coincidence between the author's comparison of William Froude's and his own results, I was beginning to doubt whether they were measuring the same thing or not. It would be a remarkable coincidence if by adding 30 per cent. to Froude's calculation we got the same result as Admiral Hiraga.

Then, of course, we have the other point, that the surface as Froude used it—I am only speaking generally—was a surface of a particular kind, and the probabilities are that the surface was more resistent than the one used by the author.

I cannot deal as a Tank expert with this measurement of friction resistance, but I do feel that there is an enormous amount of work to be done in this direction. Nothing I have read as yet satisfies me as to how frictional resistance arises: there is no sensible or logical explanation of the mechanism of the frictional wake, although we have measured it in all sorts of ways. You, Sir, as Chairman of the Tank Committee, know that we have longed to try a ship with a Reynolds' number as high as Admiral Hiraga has used, and now we are face to face with the fact that there is this 30 per cent. increase of resistance.

I hope some of the Tank Superintendents, who must have studied the wake resistance, will give us some idea as to what is the difference in condition between Admiral Hiraga's experiment and those that we usually contemplate.

Professor VON KÁRMAN: I am very doubtful whether power formulæ are suitable for extrapolation of values of the friction resistance. In fact, modern research on the boundary layer theory shows that the relationship of the various factors is more complicated than had been supposed. The exponent of the exponential formula varies from 1·7 to 1·9 if the range of Reynolds' number is extended. The correct connection between Reynolds' number and the frictional coefficient is probably given by a logarithmic formula which I have deduced theoretically and compared with the experiments by K. E. Schoenherr.* As

* *Trans. Soc. Nav. Arch and Marine Eng.*, 1932, pp. 279–313.

far as smooth surfaces are concerned, theory and experiment seem to agree very well, but with rough surfaces the correspondence is not, as yet, nearly so close.

Mr. M. P. PAYNE (Member): This excellent paper by Admiral Hiraga may well make these Meetings memorable as a landmark in the development of our knowledge of the subject of resistance. It is exactly sixty years ago since William Froude read his paper describing to this Institution the results of his experiments on the *Greyhound*, and this with the results of his well-known plank experiments has served so far as the one important direct test of the truth of the laws of comparison.

It is clear that Froude's outstanding achievement of sixty years ago has now been surpassed, in both length of plank and length of ship under test, and it has been outstandingly surpassed as regards the speeds to which the experiments have been carried.

The paper is so long and thorough that in the time in which it has been in my hands I cannot pretend to have thoroughly digested its contents. It is clearly a paper that will be much studied and quoted in the future.

Without venturing to criticize either the method of experiment or analysis—everything has been so thoroughly thought out and completed—I must confess to considerable scepticism as regards the magnitude of the skin friction as finally deduced by Admiral Hiraga. As will be inferred from the paper that I am reading to-morrow, I am quite prepared for the eventual proof that Froude's data understate the skin friction for a plated and painted ship. The value of the skin friction deduced by the author can only be substantiated, so far as the analysis of warship trial results go, by the assumption that we have unduly underestimated the efficiency of our propellers, or that we have seriously overestimated the resistance of the appendages.

As regards the comparison that Admiral Hiraga makes between the resistance deduced from William Froude's *Greyhound* experiments and the skin friction, I think regard must be paid to the fact that the copper surface of the *Greyhound* had deteriorated with age.

An important point, too, is what is shown in one of the sketches accompanying the paper, that the total resistance of the ex-destroyer *Yudachi* agrees exactly with the skin friction as computed by Admiral Hiraga's formula for all speeds up to 9 knots. Well, I should have thought that although 9 knots is a low speed for a destroyer, it corresponds to $V\sqrt{L}$ of $0 \cdot 6$; there would have been some surface disturbance at that speed, and consequent wave making and residuary resistance. As a matter of fact, as Admiral Hiraga states in the paper—at 10·1 knots, I think it is—a wave was generated of a height of 0·9 ft. compared with 1·1 ft. deduced from model experiments.

Another point is the index of the velocity in the skin resistance equation. A cursory examination of the diagram (Fig. 12, Plate XXVIII.) shows that a straight line representing an index of 1·825 will very nearly average those spots as well as that representing the 1.9 index as drawn.

The logarithmic plotting naturally assists in bringing scattered spots more closely to a mean curve, and it might well happen that the alternative straight line suggested—that is, the 1·825 line—when transposed to Figs. 11A and 13, would be an equally fair average of the scattered experiment spots as the curve drawn. I am tempted to ask the author, therefore, if he has endeavoured to obtain a curve to such an exponent, the more so as the agreement between Froude and the author is closer at the lower speeds and particularly in the model range. At the higher speeds the "idle" items in the resistance of the towed ship, if any, due to the method of towing may be of greater importance than at the low speeds. Dr. Kempf referred to that really being the wave and wake-making incidental to the towing ship.

With such a comprehensive paper, time will not permit discussion of many interesting points, but the author can rest assured that we at Haslar will have frequent occasion to refer to and study it with interest and profit.

In conclusion, I should like to extend my hearty congratulations to the author and to the Japanese authorities on having bravely faced the many difficulties with which such an investigation is beset, and on having, by such splendid foresight and skill, pursued the research to such a successful issue. They have the satisfaction of having forged the most important link in our chain of evidence on the subject since William Froude's day, and well merit our very best thanks for bringing the results to our notice.

Mr. S. V. Goodall, O.B.E. (Member of Council): Professor Hiraga is to be congratulated on his work, and, if I may say so, this Institution is honoured by receiving such an important paper. To do it justice by a few remarks is not possible. It requires considerable careful analysis, and perhaps even some experiments, before one is satisfied that the results which the author has obtained can be accepted without demur.

Professor Hiraga was a fellow-student with me at Greenwich. We have kept in touch for a number of years, and consequently I am fairly well acquainted with his investigations on skin friction resistance. He was good enough to show me his earlier paper, which—I understand—will probably be published by the Society of Naval Architects in Japan this year. Considering this and the present paper together, one is doubly impressed with the amount of work which Professor Hiraga has done, and I would advise those who are apt to be critical of his results to wait for this autumn publication, and then study the two together.

Professor Hiraga, in the summary which he read, went at once to the formula given at the outset of his paper. Naturally the thought arises, "Why has he jumped to this high index figure (1·90) of the velocity?" Mr. Payne has mentioned the point, and is going to investigate it. But a study of Professor Hiraga's earlier work will show that he has consulted all the authorities, made his own experiments, lasting about ten years and culminating with ship experiments, before he felt justified in announcing this formula. Now that he has published to the world his results, at least we must treat his formula with very great respect.

A careful study of Appendix I. will have to be made. This deals with the screw race correction. As Sir Westcott Abell has pointed out, many will say: "Why did Professor Hiraga adopt this method of direct towing, and not follow Froude's?" Perhaps he will tell us whether he made any preliminary experiments before he gave up the idea of towing from a boom, so that the towed ship would not be in the screw race.

Professor Hiraga says that in the case of the tug-boat the results do not agree so well with his formula. I am not very much impressed with the discrepancy, because for the tug-boat experiment the appendages were not removed as they were from the ex-destroyer *Yudachi*, and everyone knows that there is considerable difficulty in estimating appendage resistance from model experiments.

On the whole, while my attitude is one of suspended judgment, I have no hesitation in expressing my deep admiration for Professor Hiraga's work. Those who have to carry out model experiments know that they have their troubles, but those who have had to make experiments with full-size ships know that such troubles are magnified. There is a very real scale effect. For example, it is not possible for the experimenter to be in the towing ship and the towed ship at the same time. I have no doubt that Professor Hiraga could tell us of many tribulations and trials which he passed through before he achieved these fine results. He does mention that the plank ship finally broke and sank, but happily not till after it had fulfilled its purpose. He has overcome these difficulties, and given us the result of his labours. He deserves our very sincere thanks and congratulations.

Mr. F. H. Todd, B.Sc., Ph.D. (Associate-Member): This paper is one of the most important we have had on the subject of frictional resistance of ships. Our previous knowledge, derived directly from experiments on ships themselves, has hitherto been confined to the classic experiments with the *Greyhound*, and those with single plates let into the hull on the *Hamburg* carried out by Dr. Kempf. The amount of time, labour, and money expended upon the constructional, experimental, and analytical work must have been very great, and Professor Hiraga must be cordially congratulated on having had such opportunities presented to him, and of having then made such very excellent use of them.

The general result of the work is to suggest that the frictional resistance, as calculated by the standard Froude method used in this country, is too small, and that the total resistance so found may be some 16 to 23 per cent. lower than that obtained from the present experiments. There are a certain amount of data suggesting that this is the case, but the difference is probably only about half the amount stated above. Evidence of this occurs in Dr. Kempf's work at Hamburg, and at Teddington we have a number of cases where the thrust as measured on the ship was some 10 to 12 per cent. higher than would be expected from the effective horse-power predicted from the model results, after allowing for appendage, air, and other forms of resistance for ideal trial conditions. Dr. Kempf's work gave a figure of some 10 per cent. for ship surfaces having rivet points and laps exposed.

On Plate XXVIII. the author gives the data relative to the plank results. Fig. 12 gives the plotting from which the index $1\cdot 90$ is obtained. Now the value of this index n is very sensitive indeed to the slope of the mean line as drawn, and it is quite easy to draw a line having a slope corresponding to an index of $1\cdot 83$ without departing materially from the experimental spots.

The values of the author's coefficient K_2 are plotted in Fig. 13, and whilst he has taken a constant mean value for K_2—the variation of the experimental values is some 30 per cent.—it is moreover quite possible to give the line in Fig. 13 a slight slope and still retain as good a mean line as that drawn. It appears, therefore, that the conclusions drawn from these diagrams—that K_2 is constant, and that resistance varies as $V^{1\cdot 90}$—are not the only possible ones.

I have taken the resistance found at 800 ft./min. and calculated the resistance at other speeds, on the assumption that it varies as $V^{1\cdot 83}$. The resultant curve does not fall below the lowest experimental spots, and the difference between the measured resistance and that calculated by Froude's or other data seems to be more a question of the coefficient of friction of the surface than of the rate of variation of resistance with speed. The fact that the curve of $V^{1\cdot 83}$ lies more or less through the minimum spots suggests another possibility—that whilst every endeavour has been made to obviate errors due to yaw, tow-rope pull, propeller race, wake and air resistance, yet these are all tending to increase the resistance above the ideal condition, and are cumulative. Do the lowest spots perhaps represent the true results rather than a mean through them all?

In Fig. 29 (Plate XXXII.) are plotted the results for the long plank and the two ship hulls, together with those for a number of short planks. The spots fall into groups, through each of which a line has been drawn having a slope corresponding to an index $n = 1\cdot 90$. The value of n alters very rapidly for slight changes in these mean lines, and again I think this could be altered appreciably. A mean curve drawn through the whole diagram leads to an average value of n of approximately $1\cdot 96$, and at the higher Reynolds' numbers is obviously tending to the value $2\cdot 0$. This is in accordance with other experimental work, which suggests that for higher Reynolds' numbers the specific resistance $R/\rho\, A\, V^2$ is constant.

The conclusion reached from these data, therefore, as exemplified by the equations of page 284, does not seem at the moment to be justified. The formula in itself is not dimensionally correct if K_2 is a constant for all Reynolds' numbers—to make the formula correct in this respect, K_2 should have dimensions involving V and L, and should decrease as length increases, very much in the manner of the Froude coefficients. The formula must, therefore, be looked upon at the moment as having been derived from the experimental results, and as having no theoretical justification.

When we have Professor Hiraga's work published in its complete form, it will be necessary to reconsider these points.

There is one other point I should like to mention—it is a request for more information. At Teddington we have results of several thrust-measuring experiments on ships, and we should very much like to use Professor Hiraga's data to compare them with those to be expected from the model in the light of this new information.

Now in determining the effective horse-power he introduces a "scale coefficient" C and a coefficient of "edge effect" x, and without further information on these two items we cannot calculate the ship effective horse-power by his method. For the *Yudachi* the author had five models, from which a curve of C was presumably obtained. But one suspects, from his reference to the *Greyhound*, that he has a good deal more knowledge on these points, and we should be very grateful for anything further he can tell us.

Mr. H. G. Williams, O.B.E. (Member): I do not wish to enter unprepared on a discussion of this important paper, but I should be glad if the author would confirm or correct the dimensions given for ρ, the density of water, in the list of symbols on the first page of the paper.

The dimensions given are not those of what is usually described as density, and it will remove a possible stumbling-block in the study of the mathematics of the paper if this point is cleared up.

The Chairman (Sir Eustace d'Eyncourt, K.C.B., D.Sc., F.R.S., LL.D., Vice-President): There is one little point I would like to ask Admiral Hiraga. In certain sister destroyers I found that one ship apparently had a great deal more resistance than the sister ships. I had this examined when I was at the Admiralty, and as far as I could make out it was partly due to the proudness of the rivets over the ship's bottom. Of course, with the smaller destroyers in the early days we used rather to insist on the rivets being pretty proud in order to make an effective construction; I cannot help thinking that this has certainly a noticeable effect on the frictional resistance of the bottoms of destroyers, and it would be interesting if he could tell us what was the actual condition in this respect of the destroyer which they towed. We used to find that the centre and the head of the rivets stuck out sometimes as much as $\frac{1}{8}$ in. from the line of the hull, and were large and rough in proportion.

Professor Hiraga: I thank Sir James Henderson and Sir Westcott Abell for remembering my young days when I was a student at Greenwich.

A question was asked, "Why was the ship towed directly behind the towing ship, unlike the *Greyhound*, which was towed from a boom projecting from the side?" Waves are made by the towing ship. In the case of the *Greyhound* the speed was low and no appreciable wave was produced, so it is all right if only wake and race effect are avoided; but in the case of the *Yudachi* at high speeds there is a system of waves produced by the towing ship. If we had towed from spars like the *Greyhound* we could not escape from the effect of the waves, but with the direct towing system, as I told you in my paper, we can estimate the effect of wake; also we could get rid of the effect of the waves by allowing a sufficient distance between the two ships, and so I thought it better to tow the destroyer in this way.

With regard to the effect of the surface plating and rivet heads, about which Dr. Kempf and Sir Eustace d'Eyncourt asked me, the thickness of plating in the *Yudachi* was about 10 lb. amidships and 5 or 6 lb. at the ends. It is quite thin plating, and it is my opinion that the surface may be considered a smooth surface. The boat being old, the space between the rivets was filled in with paint, so that on the whole the surface of the *Yudachi* was flush and quite smooth.

As to the dimensional effect of this formula, I should like to mention one matter to which Mr. Goodall kindly referred. This paper is really the conclusion of my investigations, and the first part is in another paper which will be published in the near future, I expect in November, so that it is difficult to explain briefly now. I am well aware of the difficulties of the formula. It is a real and inductive one, and you may call it empirical. But to obtain it I investigated the temperature effect by testing planks and models, the water in the Tank being artificially warmed; the temperatures used varied from 38° F. to 138° F. We obtained an index for the kinematic viscosity ν of $0 \cdot 165$. These are the true experimental results. If we had not carried out that experiment, the index $0 \cdot 10$ for ν might have been adopted, because we obtained by other experiments $1 \cdot 90$ for the index of the velocity V; or we would have had an index $1 \cdot 835$ for V according to the dimensional homogeneity, because we obtained an index of $0 \cdot 165$ for ν.

With our present knowledge of science I think it is better to express the formula as obtained from our experiment. That is why the formula is not right from the principle of similitude or of the dimensions. I think you will understand that when you look at my other paper.

As to the smoothness of the *Greyhound*, I thank Mr. Payne for his information. Really we do not know how smooth or rough the *Greyhound* was, but my calculations from the formula gave me the results I obtained, and I think nobody can say what the extent of the roughness was.

As to the index of V, as I said just now, we obtained $1 \cdot 90$ from the experimental results on planks with knife-edged ends and edge, after a great many experiments (the number of curves, from each of which one value of the index was determined, was over 100), and with the plank ships I think $1 \cdot 90$ is the right figure.

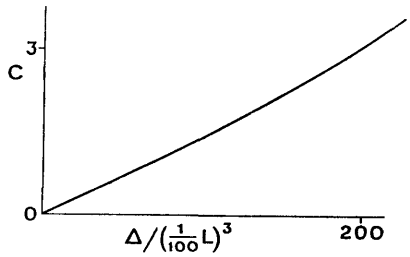


FIG. A.

At the low speeds in all the models which we tested the curve of frictional resistance calculated from the new formula always lies below the total resistance curve obtained from the test, the case of the *Yudachi* (in which these curves coincided) being the exceptional case, as I have stated on page 300. On the other hand, with such a low value of the index as $1 \cdot 83$, the former curve often lies *above* the latter, as far as I know. This is one reason why I consider the value $1 \cdot 90$ adequate.

One thing more: As to C, I made investigations on the value of C from a great number of models with many different types of ships—warships and merchant ships—and each one was tried at both light draught and deep draught. I should say there were more than one hundred in the series of models. The conclusion was that it is best to obtain C from the result of experiments on similar models. But at the same time it can approximately be obtained from a curve such as in Fig. A.

Of course the curve is quite empirical. The way it is obtained will be described in my other paper, but really it is best to obtain C from a series of experiments on three similar models, or at least two. I have used five for the *Yudachi*, and have obtained the C value given in the paper. In the case of the *Greyhound*, we had no similar models, and I obtained C from the curve C to base $\Delta \Big/ \left(\frac{1}{100} \mathrm{L}\right)^3$. This curve and the value of x, etc., will be fully given in my other paper.

I will reply in writing to the remaining points. Again I thank you very much for the interest you have taken in my paper.

The CHAIRMAN: Gentlemen, I think everybody who has spoken on Admiral Hiraga's paper has already paid a tribute to the wonderful research work and ingenuity and care which he has shown in this investigation. The paper will, I am sure, be regarded as one of the most, if not the most, valuable contribution to the literature on this subject which we have had for a long time. It is really a great tribute to the education that Admiral Hiraga received here, and to the position of this Institution, that he has come from Japan to give us this outstanding contribution to our Transactions. Finally, it is a great tribute to the genius of Froude that the paper really pursues his methods, and agrees in principle with the conclusions at which he arrived.

I should like to move a very hearty vote of thanks to Admiral Hiraga for contributing his paper to this Institution.

Written Contributions to the Discussion.

Mr. J. F. C. Conn, B.Sc. (Associate-Member): Admiral Hiraga has accomplished most excellent work and his experiments must take their place with those of William Froude on the *Greyhound* as classic examples of their kind.

The author must regard it as a compliment that we seek fuller information regarding his results. Analysis of existing data cannot be attempted, if we lack knowledge of his C and x values. Perhaps the author will be so good as to include some account of these in his reply to the discussion, if only to make the paper complete in itself. It is also unfortunate that he has not supplied the test data for his destroyer models of different scales, as this information would greatly increase the value of the paper.

The author's most important finding is that the resistances of ships are higher than previous knowledge had led us to anticipate. Against this, however, it must be remembered that all his experimental errors, although small in themselves, are cumulative. In the absence of basic information we cannot profitably dispute the author's analysis of his results. There are many obstacles to progress towards more accurate knowledge of ship resistance. One outstanding requirement appears to be further information regarding the frictional resistance of long surfaces and the relevant form effects. Certainly more full-scale tests on special craft are urgently needed.

Dr. G. Kempf (Member): Admiral Hiraga has given the results of his experiments, but he has discussed them only regarding his new formula, and he has compared them neither with the theory established by Prandtl and von Kármân, nor with existing experiments. It will be instructive to show how the results of Admiral Hiraga agree with these former experiments and theories. In order to do so, I have prepared the figure on page 319 from Fig. 29, Plate XXXII., of Admiral Hiraga's paper and from Fig. 6, on page 80, of my own paper on "frictional resistance" in the *Transactions of the Hamburg Congress* (1932), "Hydrodynamische Problème des Schiffsantriebs."

In the figure all results are given in the form of $\left(\frac{R_f}{\rho\, S\, V^2}\right)$ against $\left(\frac{V\, L}{\nu}\right)$, where R_f means the total frictional resistance of a surface S of a length L at a speed V.

The results of Admiral Hiraga's experiments for the plank ship and the *Yudachi* are plotted as isolated spots, the theoretical values for a smooth surface are drawn as a plain curve, and three other curves are drawn from my experiments with three different kinds of roughnesses, viz.:—

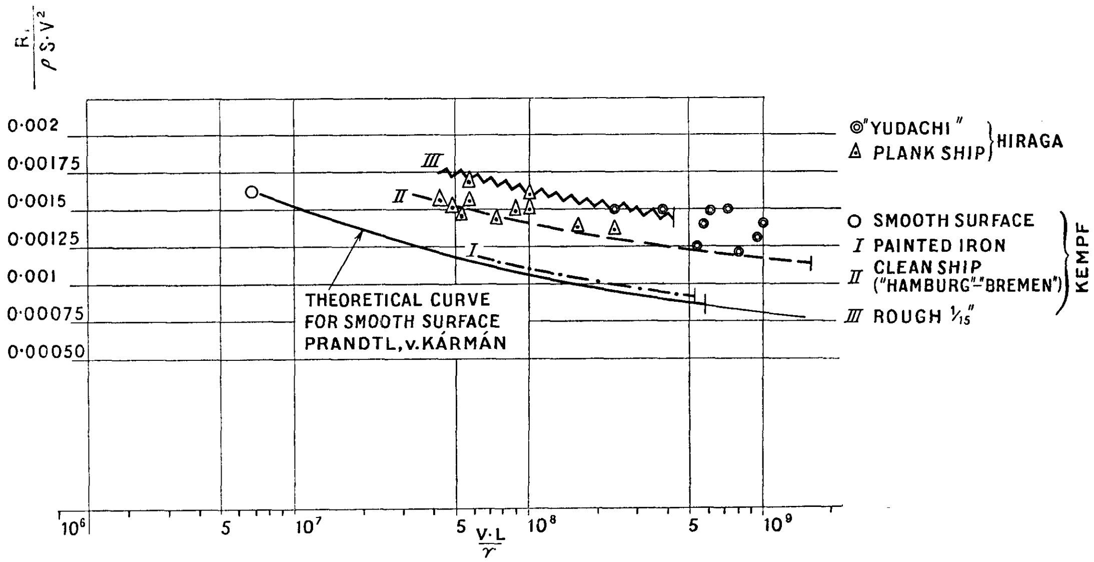
(1) for a painted smooth surface,
(2) for a clean, painted, ship's surface of passenger liners, such as *Bremen* and *Hamburg*,
(3) for a surface roughened by grains of sand $\frac{1}{15}$ in. diameter each.

It will be seen that the lowest spots of Admiral Hiraga's are in fair agreement with the curve II. of a clean, painted, ship's surface, which was measured by myself in three different sets of experiments in 1926, 1927, and 1929 on the liners *Hamburg* and *Bremen.*

It is my personal opinion that the spots measured by Admiral Hiraga which indicate higher resistances are due to the enormous difficulties of towing a ship in a straight line without any accelerations and deviations, i.e. without any additional resistances. The *Yudachi* experiments, with their maximum Reynolds' number of 10^9, lay well within the limits of my own experiments, which were extended to a Reynolds' number of $1 \cdot 33 \times 10^9$.

Concerning the new formula proposed by Admiral Hiraga, I do not quite understand why one should leave the certain and accurate basis of the theory of a smooth surface, which has been thoroughly ascertained by experiments conducted under much more favourable conditions than those on a towed ship ever can be.

For the calculation of the resistance of a definite roughness, as, for example, a ship's surface, it seems to me more reliable to start from this very certain basis of smoothness than from any other experimental results with surfaces the roughness of which is not sufficiently known, and which by the method of towing must to a certain extent be unsteady.



The results of the Japanese experiments show again that the enormous difficulties arising from this method of towing a ship cannot sufficiently be overcome even if they are carried out with such an admirable thoroughness as Admiral Hiraga's. Everybody who is concerned with model-work and its transference to real ships must be grateful to him for his new endeavour to push forward the famous work inaugurated by William Froude when he carried out the *Greyhound* towing experiments.

Professor Hiraga: After reading the printed discussion and written contributions, I will add a few lines to my remarks in answer to some of the questions put to me.

As to the wake and wave effects of the towing ship, not only did we make preliminary investigations with models and ships, but the plank ship and the *Yudachi* were towed at certain constant speeds, and resistances were compared when the distance between the towing ship and the towed ship had three or four different values, and they always gave a very good agreement, thus proving that there were no appreciable wake and wave effects. The investigations and the experiments are fully given in the paper.

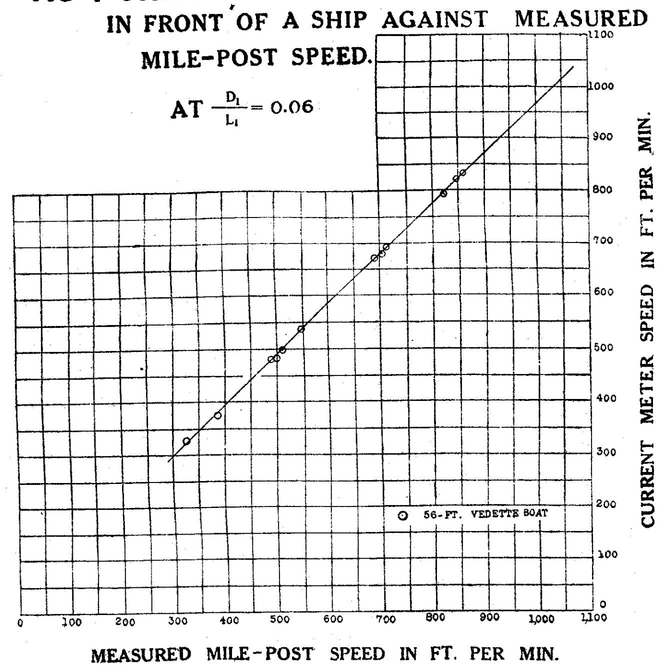
Mr. Goodall rightly refers to a discrepancy in the case of the tugboat. In her case the actual resistance was greater by a small percentage than that deduced from the model. The maximum speed-length ratio experimented with was only $0 \cdot 64$; still I cannot help thinking that an error has been introduced to some extent in my calculation by assuming all the appendages as if they were a part of the main hull. This I shall leave to a future investigation.

As to the remarks of Dr. Todd and Mr. Conn that the error is cumulative towards increasing resistance, it is true, but it is small, and does not give a material change in the resistance coefficient experimented with, as I explained in the paper. As a whole, I believe the error is not any greater, if not actually smaller, than in the case of the most carefully conducted steam trials of actual ships.

I thank Dr. Kempf for the comparison of his experiment and theory with our experiment. The present theory assumes that frictional resistance is governed only by the surface area and the linear dimensions of a body, the density and the kinematic viscosity of the fluid, and the speed. Even so, the nature of the surfaces should be similar for similar bodies. I do not yet know any theory which could be extended from short to long planks, or from smaller to larger bodies, when the nature of the surfaces is not similar, or is practically a constant. When we know that edge effect decreases when the length increases, the problem becomes more complicated. Experimentally, when the range of the Reynolds' numbers is great, and it covers many experimental spots from short to long planks or ships, we found it impossible to represent them by a single curve, as can be seen, for some of them, from Fig. 29 in my paper. For similar planks shorter than 26 ft., it was also impossible to represent them by a single curve when the planks had different values at similar draughts, or when the fluid had different temperatures. But with the new formula they may be correctly represented, and this is the reason why I adopted the new formula. My other paper will show this in detail.

Finally, I thank Professor von Kármán for his general remarks on the present state of the theory.

FIG. 1 CURVE OF MEASURED CURRENT VELOCITY IN FRONT OF A SHIP AGAINST MEASURED MILE-POST SPEED.



PRINCIPAL DIMENSIONS OF SHIPS AND MODELS.

	56-FT. VEDETTE	8-FT. T.B.D. MODEL	26-FT. T.B.D. MODEL	T.B.D. KATSURA	T.B.D. SHIMAKAZE	5.71-FT. SHIMAKAZE MODEL	15-FT. SHIP MODEL	26-FT. HASHIKE	4-FT. YUDACHI MODEL
DISPLACEMENT	16.00 TONS	43.67 LB.	1400 LB.	596 TONS	1300 TONS	15.45 LB.	650 LB.	1.1 TONS	4.23 LB.
LENGTH ON WATER LINE IN FT.	56.00	8.00	26.00	270.00	326.25	5.71	15.00	26.00	4.00
BREADTH IN FT.	10.80	0.74	2.33	24.00	29.3	0.52	1.60	5.45	0.37
DRAUGHT IN FT.	3.60	0.23	0.72	7.9	9.5	0.17	0.60	1.31	0.10
PRISMATIC COEFFICIENT	0.62	0.64	0.64	0.58	0.64	0.64	0.59	0.49	0.57
(THE DEPTH OF THE CENTRE OF THE CURRENT METER) (DRAUGHT)	0.28	0.63	0.69				0.76	0.76	0.8
DIAMETER OF CURRENT METER IN INCHES.		3.15							1.2
D_1/L_1	0.06	0.06-1.0	0.077				0.2	0.18	0.01

PLAN OF 56-FT VEDDETE TOWING HASHIKE.

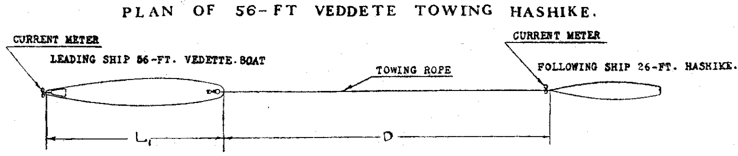


FIG. 3 CURVES OF WAKE FACTOR BEHIND SHIP AGAINST D/L_1.

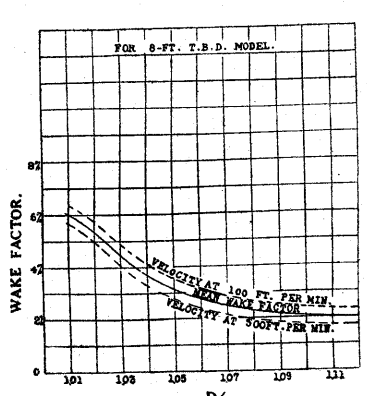


FIG. 4 CURVES OF SCREW-RACE AGAINST D/L_1 IN THE BASIN TRIAL.

(THE SHIP STANDS STILL.)

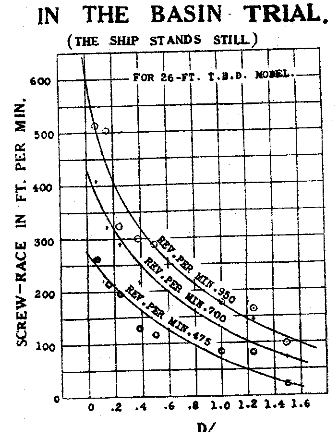


FIG. 2 CURVES OF FACTOR OF WAKE VELOCITY IN FRONT OF A SHIP AGAINST D/L_1 AT SEVERAL CONSTANT $V/\sqrt{L_1}$.

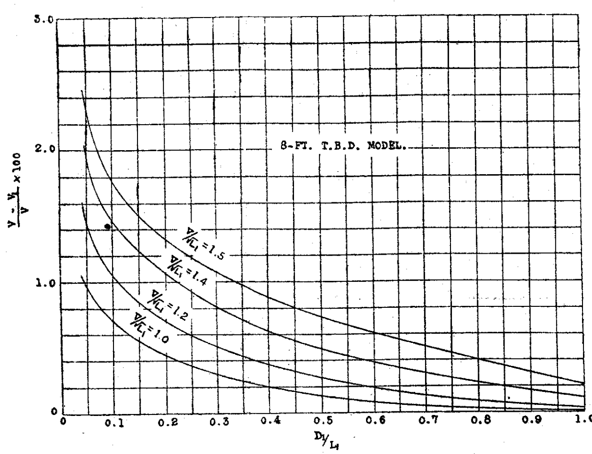


FIG. 5 CURVES OF FACTOR OF SCREW-RACE BEHIND SHIP AGAINST $V/\sqrt{L_1}$.

FIG. 5_a

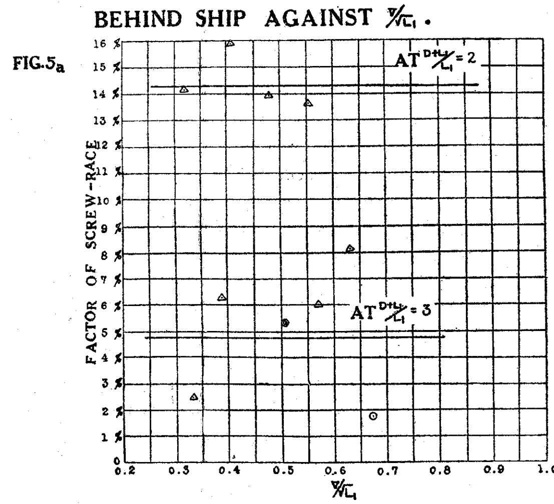
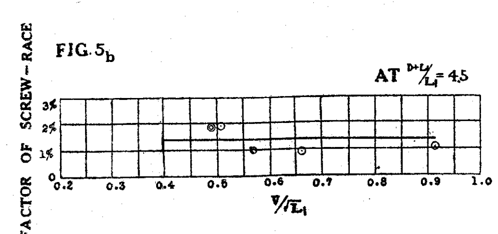


FIG. 5_b



LEADING SHIP ... FOLLOWING SHIP

△ 26-FT. T.B.D. MODEL ...... 15-FT. SHIP MODEL.
⊙ 56-FT. VEDETTE BOAT ...... 26-FT. HASHIKE
⊕ T.B.D. "KATSURA" } ...... 56-FT. VEDETTE BOAT
○ T.B.D. "SHIMAKAZE" }

N.B. V .... SPEED OF A MODEL OR SHIP
V_1 .... SPEED RECORDED BY CURRENT METER.
L_1 .... THE LENGTH ON WATER LINE OF A LEADING MODEL OR SHIP
D .... DISTANCE BETWEEN LEADING AND FOLLOWING MODELS OR SHIPS
D_1 .... DISTANCE BETWEEN F.P. AND CURRENT METER IN THE FRONT OF A MODEL OR SHIP.

FIG. 5_c

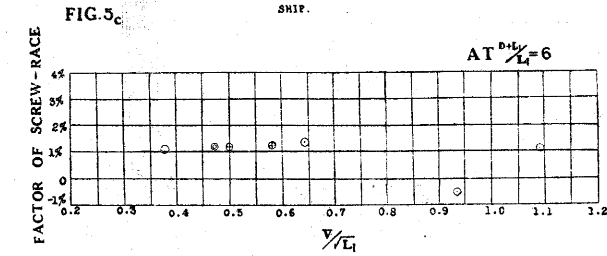


FIG. 5_d

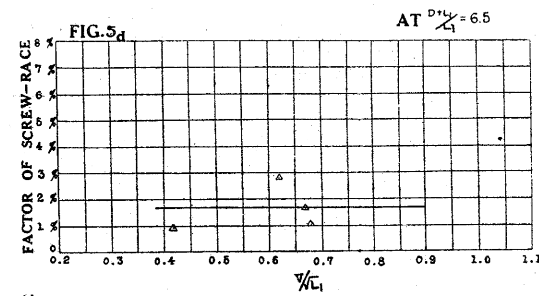


FIG. 5_e

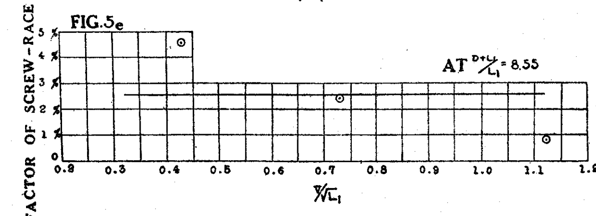


FIG. 5_f

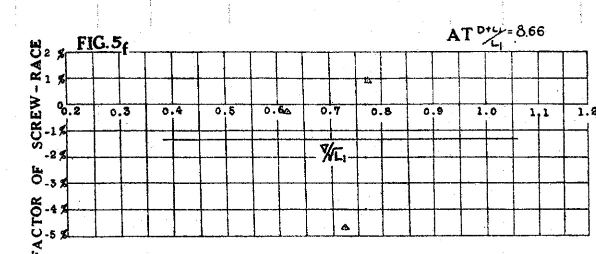


FIG. 6 CURVES OF FACTOR OF SCREW-RACE BEHIND SHIP AGAINST $\frac{D+L}{L_1}$.

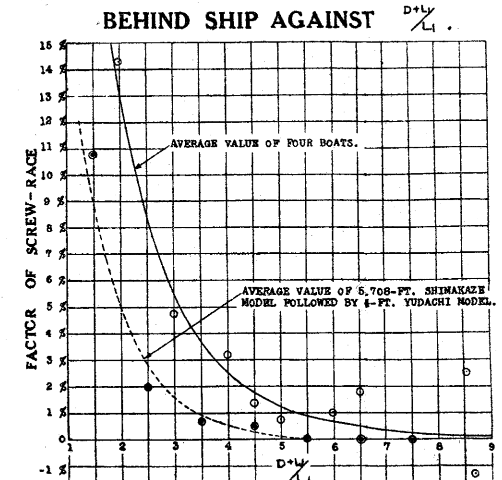


FIG. 7_a THE EFFECT OF FOULING ON THE PLATE RESISTANCE AGAINST RUNNING DISTANCE.

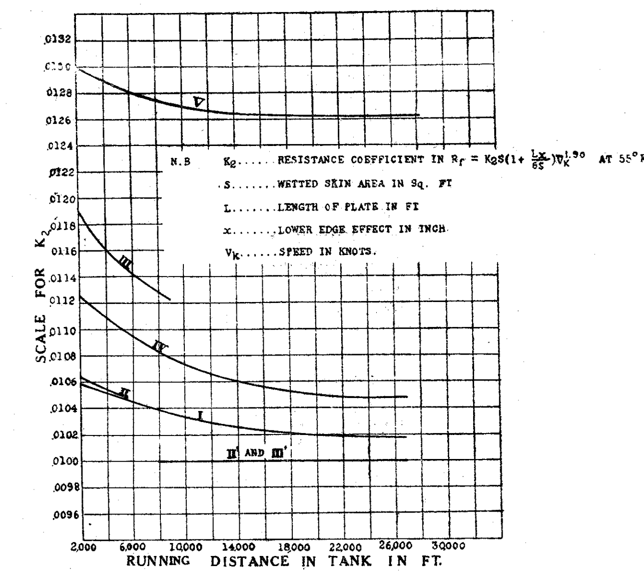
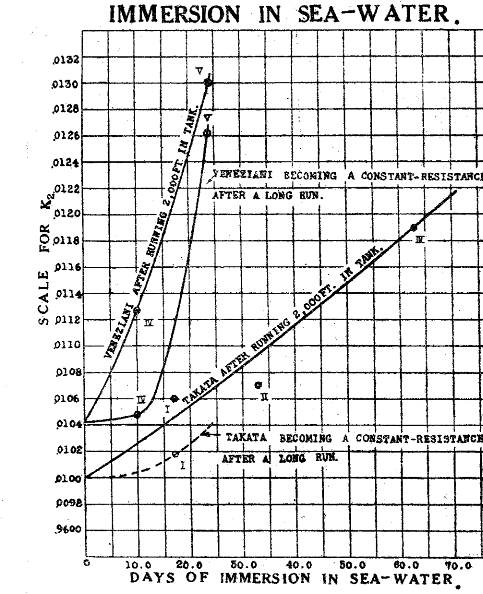


FIG. 7_b CURVE OF K_2 AGAINST DAYS OF IMMERSION IN SEA-WATER.



	DATES OF IMMERSION	DURATION OF IMMERSION DAYS	PAINT	LENGTH OF PLATES FT.	MEAN TEMPERATURE °F.
I.	AUG. 10-27, 1928	17	TAKATA	6	75.8
II	" 10-SEP.12 "	35	"	"	77.6
II'			"	"	"
III	" 10-OCT.11 "	63	"	5	65.6
III'			"	"	64.8
IV.	SEP. 7-17, "	10.	VENEZIANI	6	75.2
V.	OCT. 28-NOV. 21 "	24	"	"	52.7

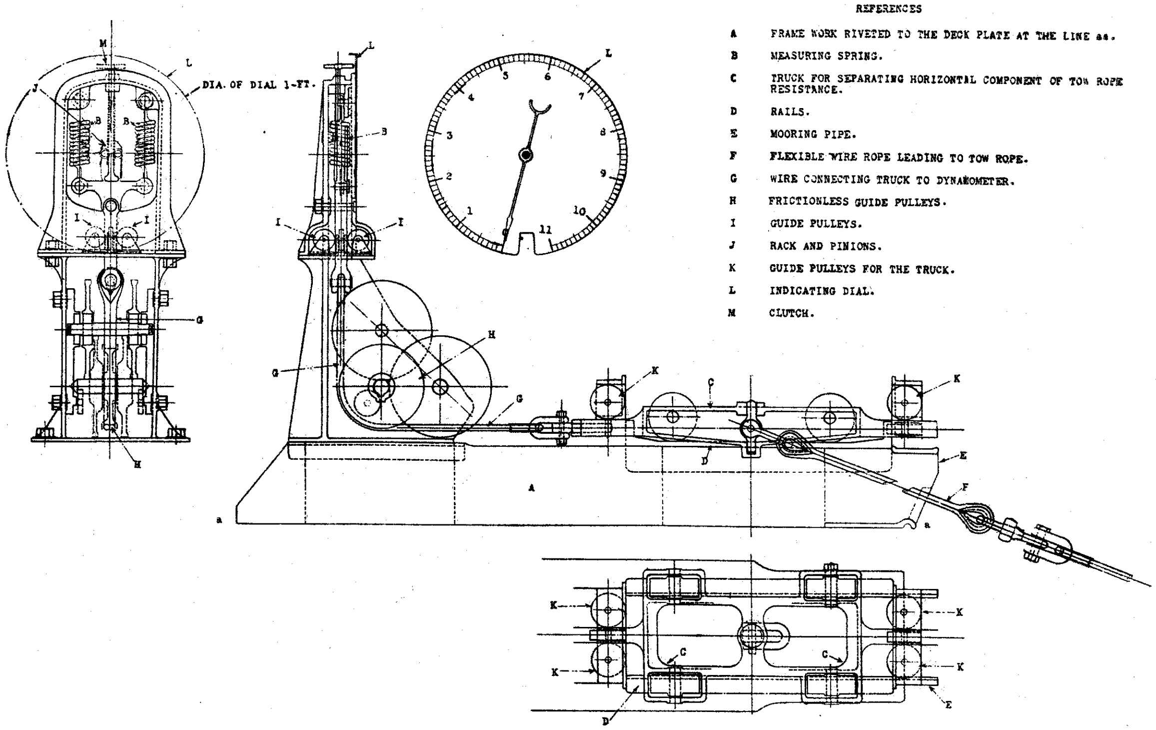
EXPERIMENTS ON WAKE, SCREW RACE, AND FOULING.

To Illustrate Professor Yuzuru Hiraga's Paper on "Experimental Investigations on the Resistance of Long Planks and Ships."

FIG. 8 DYNAMOMETER

PRINCIPAL DIMENSIONS OF THE PLANK-SHIP

LENGTH ON WATER LINE (L) IN FT.	77.319
MAXIMUM BREADTH ON WATER LINE (B) IN FT.	0.525(6.30 INCHES)
DEPTH (D) IN FT.	4.364
DRAUGHT (d) IN FT.	3.340
DISPLACEMENT (INCLUSIVE RUDDER) IN TONS	3.356
AREA OF THE MIDSHIP SECTION IN SQ. FT.	1.580
GIRTH OF THE MIDSHIP SECTION IN FT.	6.796
AREA OF WETTED SURFACE (INCLUSIVE RUDDER) IN SQ. FT.	524.800
PRISMATIC COEFFICIENT	0.962
MIDSHIP SECTION COEFFICIENT	0.901
B/L	1/147
L/b	17.7
d/L	0.0432
RUDDER AREA (ONE SIDE) IN SQ. FT.	4.886
G.M. IN FT.	0.397
CENTRE OF GRAVITY OF THE PLANK SHIP ABOVE THE BOTTOM OF BAR KEEL IN FT.	1.480



REFERENCES

- A FRAME WORK RIVETED TO THE DECK PLATE AT THE LINE aa.
- B MEASURING SPRING.
- C TRUCK FOR SEPARATING HORIZONTAL COMPONENT OF TOW ROPE RESISTANCE.
- D RAILS.
- E MOORING PIPE.
- F FLEXIBLE WIRE ROPE LEADING TO TOW ROPE.
- G WIRE CONNECTING TRUCK TO DYNAMOMETER.
- H FRICTIONLESS GUIDE PULLEYS.
- I GUIDE PULLEYS.
- J RACK AND PINIONS.
- K GUIDE PULLEYS FOR THE TRUCK.
- L INDICATING DIAL.
- M CLUTCH.

SECTION AT "bb"

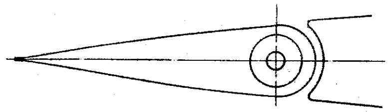
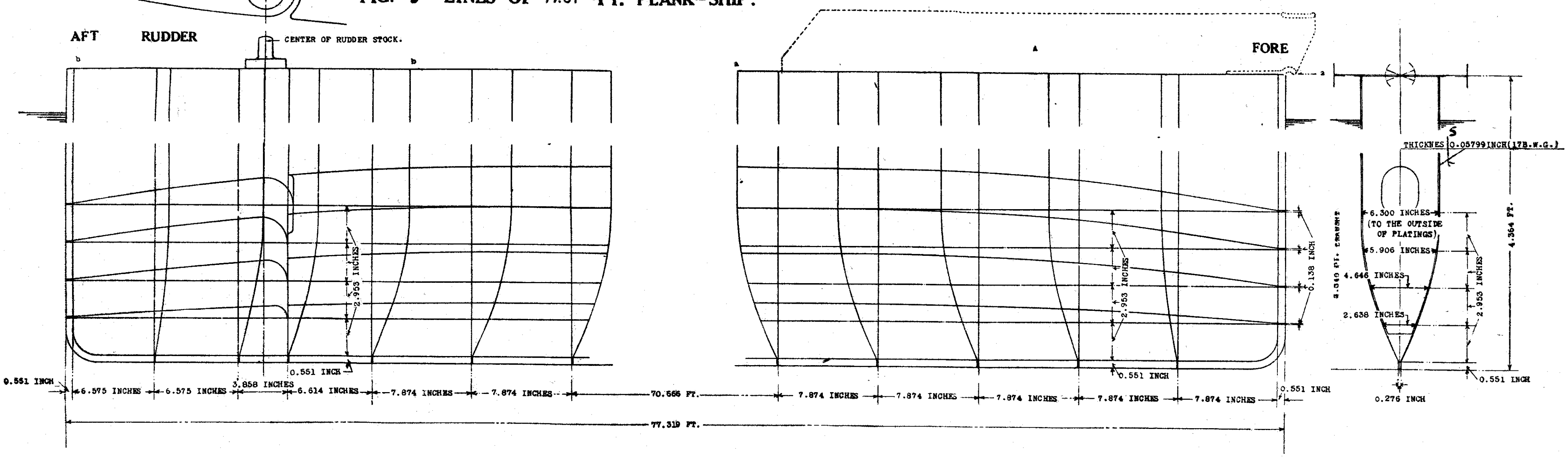


FIG. 9 LINES OF 77.319-FT. PLANK-SHIP.



SCALE OF LINES

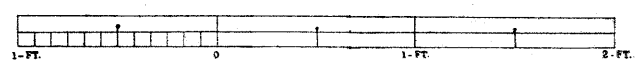
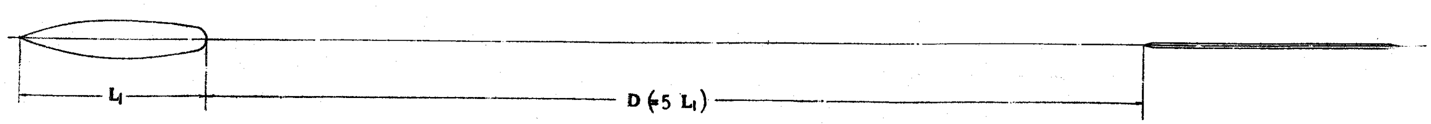


FIG. 10 PLAN OF 56-FT. VEDETTE BOAT TOWING THE 77.319-FT. PLANK-SHIP.

56-FT. VEDETTE BOAT. **77.319-FT. PLANK-SHIP.**



DYNAMOMETER FOR MEASURING THE RESISTANCE OF 77·319-FT. PLANK SHIP, ETC.

RESULTS OF THE TOWING EXPERIMENTS WITH THE PLANK-SHIP.

DEPTH OF WATER IN FATHOMS. 7.7 FOR SERIES (1) TO (6); 17 FOR SERIES (7) TO (9)

DISTANCE BETWEEN MILE-POSTS IN FT. 1247 FOR SERIES (1) TO (6); 3383 FOR SERIES (7) TO (9)

DENSITY OF SEA-WATER ρ IN (LB.)×(SEC.)^2×(FT.)$^{-4}$ AT 55°F. 1.981

KINEMATIC VISCOSITY OF SEA-WATER ν IN (FT.)^2×(SEC.)$^{-1}$ AT 55°F. 1.331×10^{-5}

SERIES OF EXPERIMENTS.	DATES OF EXPERIMENTS.	DAYS OUT OF DOCK.	CONDITIONS OF SEA.	MEAN DRAUGHT IN FT.	TRIM BY STERN IN FT.	DISPLACEMENT IN TONS.	NAME OF ANTIFOULING COMPOSITION.	TEMPERATURE AT °F. (t)	DENSITY OF SEA WATER ρ IN (LB.)×(SEC.)^2×(FT.)$^{-4}$	KINEMATIC VISCOSITY ν IN (FT.)^2×(SEC.)$^{-1}$	NAME OF TOWING SHIP.	L.W.L. OF TOWING SHIP IN FT. (L)
1	JUNE 15,1928	28	CALM	3.41	0.57	3.43	TAKATA	67.6	1.979		56-FT. VEDETTE BOAT	56
2	JUNE 16,1928	29	CALM	3.43	0.53	3.46	TAKATA	67.3	1.979	1.117×10^{-5}	56-FT. VEDETTE BOAT	56
3	JUNE 25,1928	39	CALM	3.44	0.56	3.47	TAKATA	68.6	1.979	1.124×10^{-5}	56-FT. VEDETTE BOAT	56
4	JUNE 28,1928	41	A LITTLE WAVY	3.46	0.56	3.49	TAKATA	70.1	1.978	1.075×10^{-5}	56-FT. VEDETTE BOAT	56
5	SEPT. 11,1928	8	CALM	3.56	0.05	3.59	VENEZIANI	80.5	1.971	0.943×10^{-5}	56-FT. VEDETTE BOAT	56
6	SEPT. 12,1928	9	CALM	3.59	0.06	3.60	VENEZIANI	80.4	1.971	0.943×10^{-5}	56-FT. VEDETTE BOAT	56
7	SEPT. 13,1928	10	CALM	3.59	0.03	3.60	VENEZIANI	81.1	1.970	0.933×10^{-5}	T.B.D. KATSURA	270
8	NOV. 15,1928	19	CALM	3.64	0.03	3.70	VENEZIANI	61.6	1.982	1.211×10^{-5}	T.B.D. KAYEDE	270
9	NOV. 20 1928	24	CALM	4.12	0.06	4.25	VENEZIANI	61.0	1.982	1.222×10^{-5}	T.B.D. SHIMA KAZE	326.25

SERIES	AREA OF WETTED SURFACE, S IN SQ. FT.	DISTANCE BETWEEN BOWS OF TOWING AND TOWED SHIP IN FT. $(D+L_1)$	$\frac{D+L_1}{L_1}$	R.P.M. OF TOWING SHIP.	MEASURED-MILE SPEED IN FT. PER MIN. (V_0)	MEASURED-MILE SPEED IN KNOTS (V_0)	$\frac{V_0}{\sqrt{L}}$	$\frac{D+L_1}{\lambda}$	FACTOR OF SCREW RACE (w) IN PERCENT OF V_0	REAL SPEED IN FT PER MIN. $v=V_0(1+w)$	REAL SPEED IN KNOTS $V=(1+w)V_0$	$\frac{V}{\sqrt{L}}=\frac{V}{\sqrt{77.319}}$	MEASURED RESISTANCE IN LB. (R_t)	RESIDUARY RESISTANCE DEDUCED FROM MODEL IN LB. (R_w)	FRICTIONAL RESISTANCE IN LB. $(R_f)_t=R_t-R_w$	FACTOR OF FOULING CORRECTION (f)	$(R_f)_t$ CORRECTED TO STANDARD PAINTED SURFACE IN LB.	$(R_f)_t$ CORRECTED TO 55°F IN LB.	K_2	$\left(\frac{vL}{\nu}\right)$ IN 10^6	$\left(\frac{(R_f)_t}{\rho S v^2}\right)$	θ_1	θ_2	TILLER ANGLES IN DEGREES	$\left(\frac{R_f}{S}\right)$ 55°F IN LB.
1	536	336	6	140	347	3.43	0.46	51.4	0.65	349	3.45	0.39	69.9	2.5	67.4	1.004	67.7	69.8	0.01238	40.4	0.00189	2.1	8	1.6	0.130
				220	529	5.22	0.70	22.2		532	5.25	0.60	141.0	3.7	137.3		137.9	142.2	0.01136	61.6	0.00165	0.3	5	2.2	0.265
				299	736	7.26	0.97	11.4		741	7.32	0.83	213.0	4.0	209.0		209.8	216.3	0.00919	85.8	0.00130	1.5	3	2.7	0.404
				378	919	9.07	1.21	7.4		925	9.13	1.04	294.0	6.2	287.8		289.0	298.0	0.00832	107.1	0.00115	0.3	3	2.5	0.556
2	538	336	6	261	634	6.26	0.84	15.4	0.65	638	6.30	0.72	182.0	3.8	178.2	1.004	178.9	184.3	0.01038	73.6	0.00149	0.4	4	0.9	0.343
				350	845	8.34	1.11	8.7		851	8.40	0.96	287.0	5.0	282.2		283.1	291.7	0.00951	98.2	0.00132	0.3	3	0	0.542
				192	478	4.72	0.63	27.2		481	4.75	0.54	108.0	3.6	104.4		104.8	108.0	0.01040	55.5	0.00153	1.4	7	0	0.201
3	540	336	6	100	248	2.45	0.33	100.8	0.65	250	2.47	0.28	30.4	1.7	28.7	1.004	28.8	29.6	0.00984	28.7	0.00155		11	1.5	0.055
				130	318	3.14	0.42	61.3		320	3.16	0.36	59.5	2.2	57.3		57.5	59.2	0.01232	36.7	0.00189		7	0.9	0.110
				160	398	3.93	0.53	39.2		401	3.96	0.45	74.3	3.0	71.3		71.6	73.7	0.00999	46.0	0.00150		4	0.8	0.136
				190	475	4.69	0.63	27.5		478	4.72	0.54	106.0	3.6	102.4		102.8	105.8	0.01027	54.8	0.00152		3	1.5	0.196
				240	578	5.70	0.76	18.6		582	5.74	0.65	156.0	3.8	152.2		152.8	157.3	0.01049	66.7	0.00152		1.5	1.5	0.291
				251	615	6.07	0.81	16.4		619	6.11	0.69	170.0	3.8	166.2		166.9	171.8	0.01021	71.0	0.00147		1.2	0.7	0.318
				280	671	6.62	0.88	13.8		675	6.66	0.76	208.0	3.8	204.2		205.0	211.0	0.01065	77.4	0.00152		1.2	0.7	0.391
				380	906	8.94	1.19	7.6		912	9.00	1.02	355.0	6.0	349.0		350.4	360.7	0.01027	104.6	0.00142		0.7	0.8	0.668
4	545	280	5	330	772	7.62	1.02	8.7	1.25	782	7.72	0.88	279.0	4.5	274.5	1.004	275.6	285.9	0.01084	93.7	0.00151		0.7	1.4	0.527
		336	6	330	782	7.72	1.03	10.2	0.65	787	7.77	0.88	268.0	4.5	263.5		264.5	274.4	0.01024	94.3	0.00143		1.2	1.8	0.505
		392	7	330	775	7.65	1.02	12.1	0.35	778	7.68	0.87	262.0	4.4	257.6		258.6	268.3	0.01027	93.3	0.00143		1.0	1.0	0.494
		448	8	330	770	7.60	1.02	14.0	0.15	771	7.61	0.87	254.0	4.4	249.6		250.6	260.0	0.01013	92.4	0.00141		1.5	0.6	0.479
		504	9	330	771	7.61	1.02	15.7	0.06	771	7.61	0.87	260.0	4.4	255.6		256.6	266.2	0.01037	92.4	0.00145		2.3	1.2	0.490
5	558	336	6	100	253	2.50	0.33	96.8		255	2.52	0.29	34.8	1.8	33.0	1.000	33.0	35.1	0.01086	34.9	0.00166			—	0.063
				170	398	3.93	0.53	39.2	0.65	401	3.96	0.45	81.8	3.1	78.7		78.7	83.7	0.01098	54.8	0.00160			1.2	0.150
				240	560	5.53	0.74	19.8		564	5.57	0.63	145.0	3.9	141.1		141.1	150.1	0.01029	77.1	0.00145			1.0	0.269
				280	665	6.56	0.88	14.1		669	6.60	0.75	198.0	3.9	194.1		194.1	206.5	0.01026	91.4	0.00142			0.4	0.370
				310	728	7.19	0.96	11.7		733	7.24	0.82	254.0	4.2	249.8		249.8	265.8	0.01108	100.2	0.00152			0.6	0.476
				380	899	8.87	1.19	7.7		905	8.93	1.01	344.0	6.1	337.9		337.9	359.5	0.01006	123.7	0.00135			1.3	0.644
6	558	336	6	160	374	3.69	0.49	44.4	0.65	376	3.71	0.42	65.6	2.9	62.7	1.000	62.7	66.7	0.00990	51.4	0.00145			1.0	0.120
	559			205	472	4.66	0.62	27.8		475	4.69	0.53	109.0	3.7	105.3		105.3	112.0	0.01063	64.9	0.00153			0.2	0.200
	560			225	538	5.31	0.71	21.5		542	5.35	0.61	128.0	3.9	124.1		124.1	132.0	0.00974	74.1	0.00138			0.8	0.236
	561			290	692	6.83	0.91	13.0		696	6.87	0.78	212.0	4.0	208.0		208.0	221.3	0.01013	95.1	0.00140			0.7	0.394
	562			330	776	7.66	1.02	10.3		781	7.71	0.88	254.0	4.6	249.4		249.4	265.3	0.00974	106.7	0.00133			0.3	0.472
	563			320	756	7.46	1.00	10.9		761	7.51	0.85	256.0	4.4	251.6		251.6	267.7	0.01031	104.0	0.00141			0.8	0.475
7	557	1620	6	107	764	7.54	0.46	51.3	0.65	769	7.59	0.86	284.0	4.4	279.6	1.000	279.6	298.1	0.01138	106.2	0.00155				0.535
	561			137	1006	9.93	0.60	29.6		1013	9.99	1.14	457.0	7.4	449.6		449.6	479.4	0.01078	139.9	0.00143				0.855
	563			177	1282	12.65	0.77	18.2		1290	12.73	1.45	657.0	4.7	652.3		652.3	695.5	0.00983	178.2	0.00127				1.235
	565			223	1632	16.11	0.98	11.2		1643	16.22	1.84	990.0	0	990.0		990.0	1055.6	0.00938	226.9	0.00119				1.868
8	569	1620	6	172	1218	12.02	0.73	20.2	0.65	1226	12.10	1.38	809.0	5.7	803.3	0.916	735.8	747.0	0.01151	130.5	0.00156				1.313
	571			181	1467	14.48	0.88	13.9		1477	14.58	1.66	1005.0	2.0	1003.0		918.6	932.6	0.01004	157.2	0.00134				1.633
	573			234	1643	16.21	0.99	11.1		1654	16.32	1.86	1331.0	0	1331.0		1219.1	1237.6	0.01071	176.0	0.00141				2.160
	575			150	1055	10.41	0.63	26.9		1062	10.48	1.19	615.0	7.6	607.4		556.4	564.9	0.01151	113.0	0.00156				0.982
9	622	1957.5	6	91	998	9.85	0.55	36.3	0.65	1004	9.91	1.13	605.0	8.1	596.9	0.829	494.8	501.6	0.01033	105.9	0.00143				0.806
	659			136	1479	14.60	0.81	16.5		1489	14.70	1.67	1333.0	2.1	1330.9		1103.1	1118.2	0.01027	157.0	0.00137				1.697

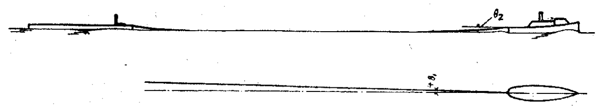


FIG. 11a CURVES OF RESISTANCE AGAINST SPEED.

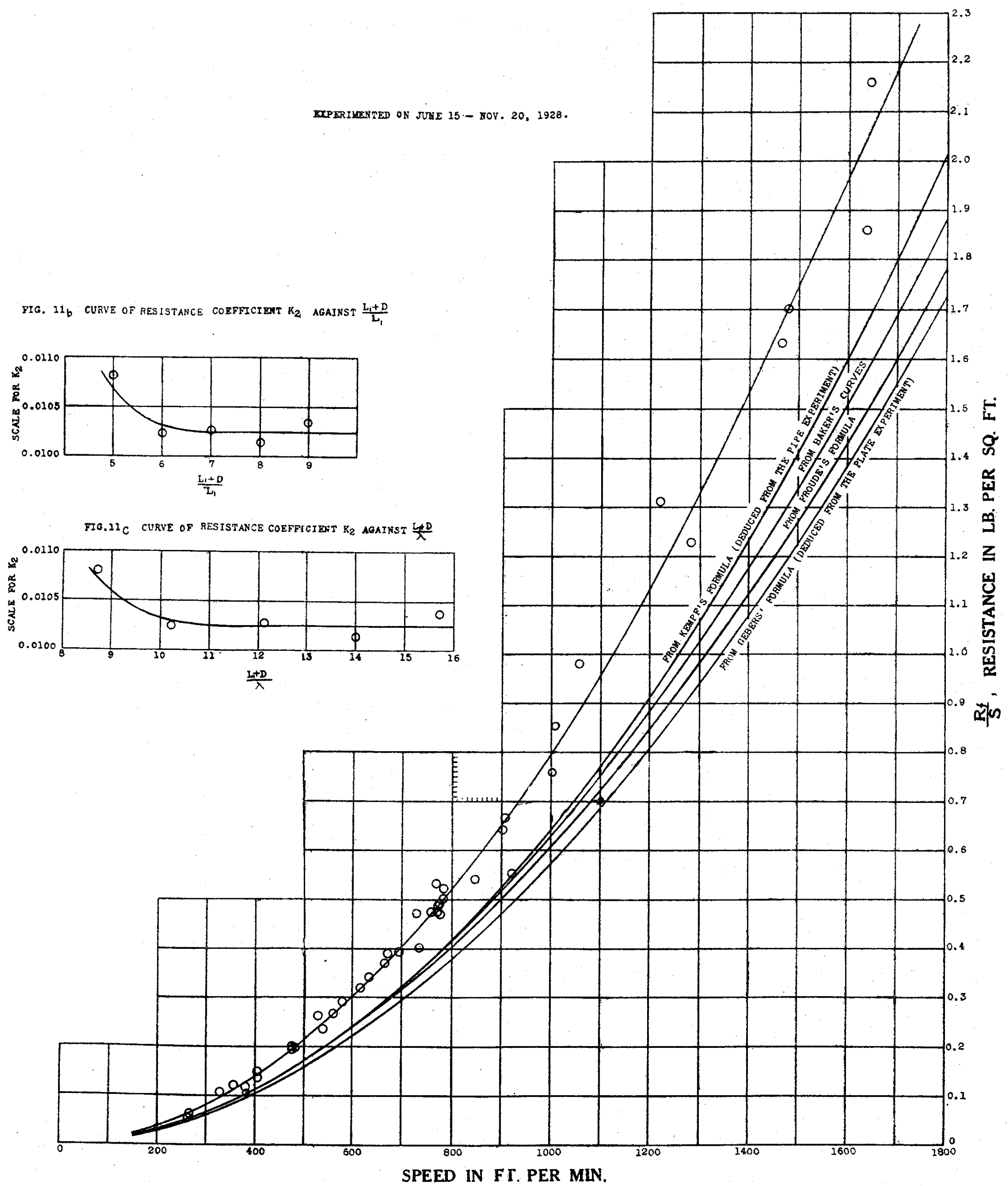


FIG. 11b CURVE OF RESISTANCE COEFFICIENT K_2 AGAINST $\frac{L_1+D}{L_1}$

FIG. 11c CURVE OF RESISTANCE COEFFICIENT K_2 AGAINST $\frac{L_1+D}{\lambda}$

FIG. 12 CURVE OF LOG R_f/S AGAINST LOG V.

N.B. v ...... SPEED IN FT PER MIN.
R_f/S .... RESISTANCE, LB. PER SQ. FT.
V_k ..... SPEED IN KNOTS.

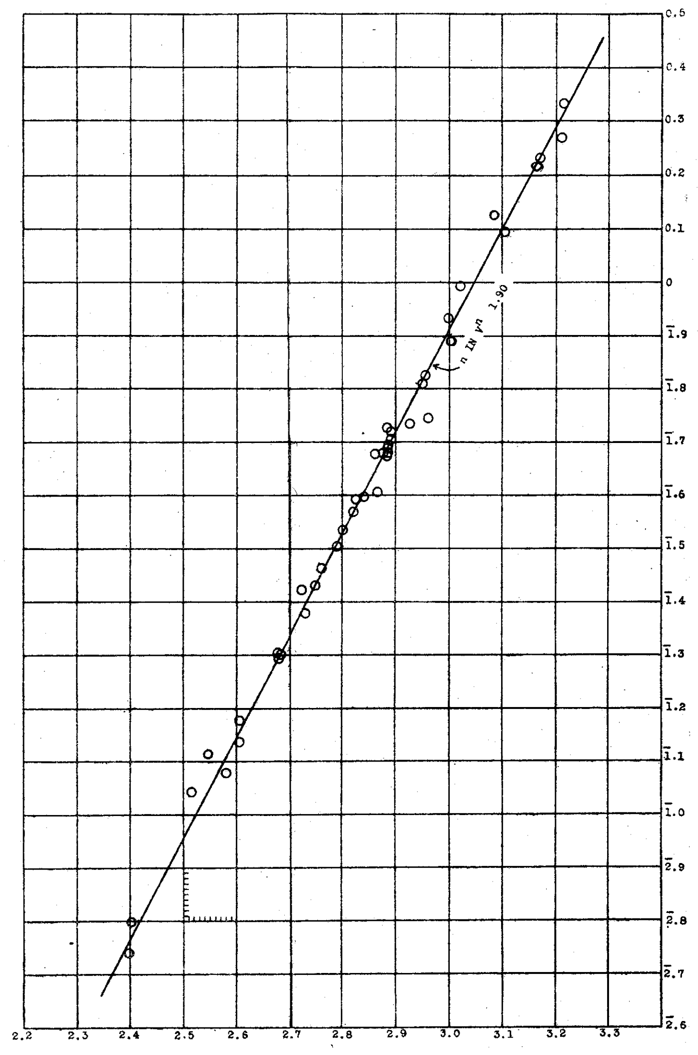
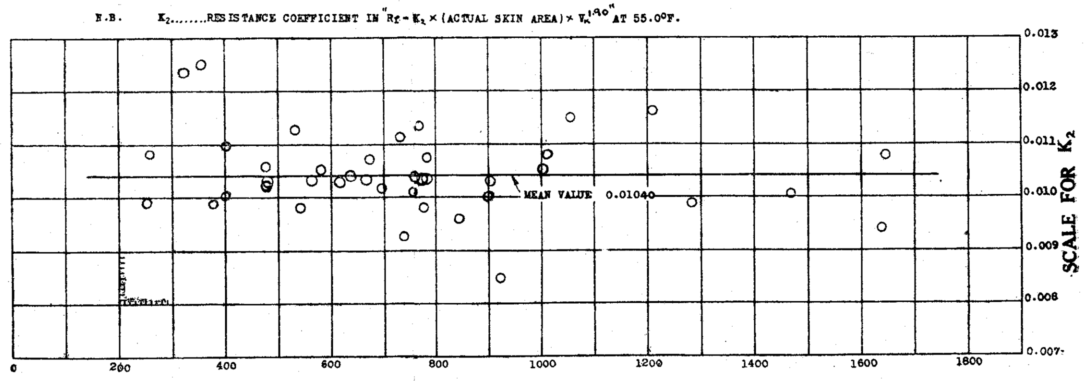


FIG. 13 CURVE OF K_2 AGAINST SPEED.

N.B. K_2 ...... RESISTANCE COEFFICIENT IN "$R_f = K_2 \times$ (ACTUAL SKIN AREA) $\times V_K^{1.90}$" AT 55.0°F.



FRICTIONAL RESISTANCE OF 77·319-FT. PLANK SHIP, SURFACE COATED WITH ANTI-FOULING COMPOSITION IN SEA-WATER AT 55·0° F.

FIG. 14_a DYNAMOMETER FOR TESTING THE RESISTANCE.

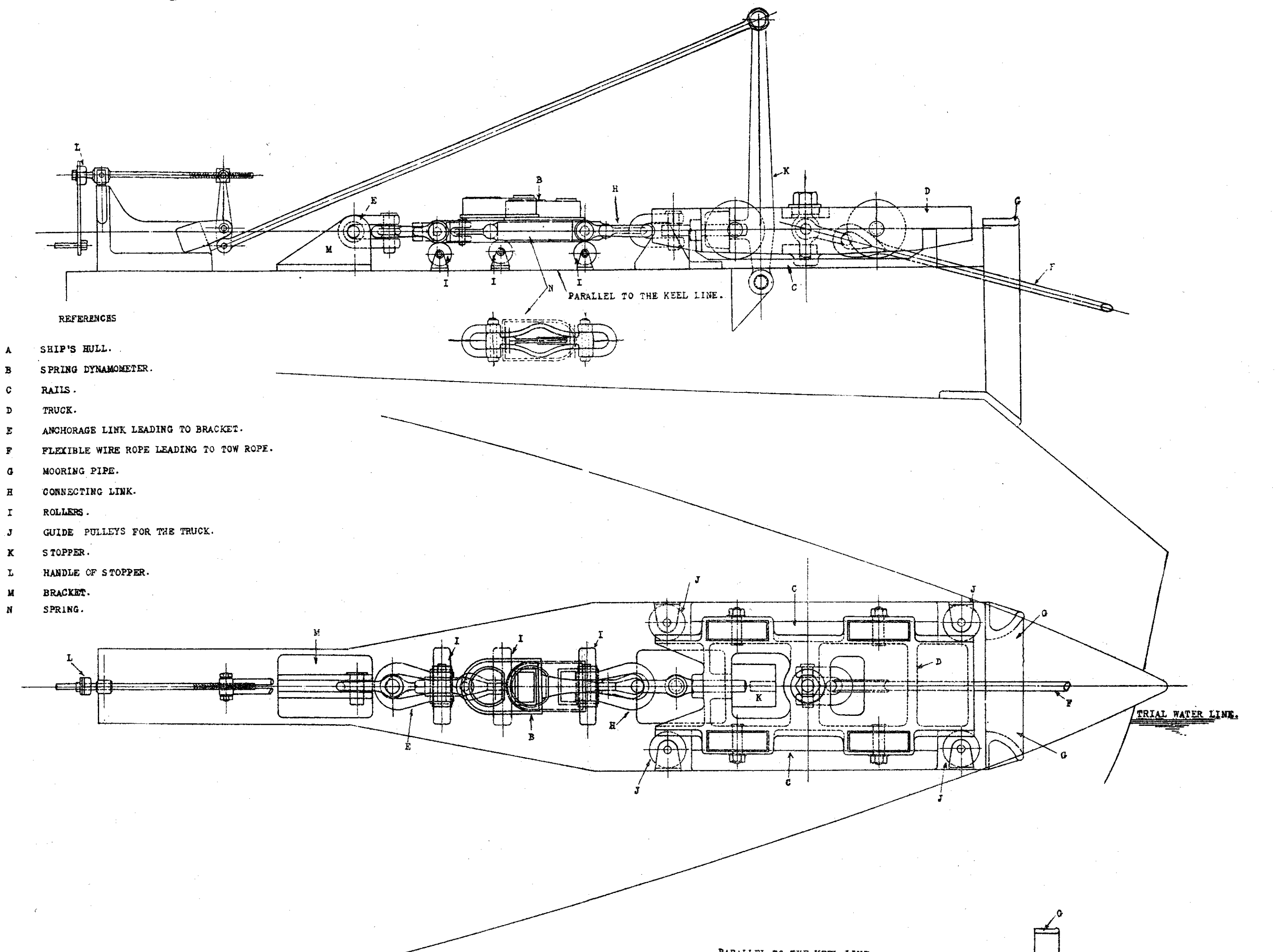
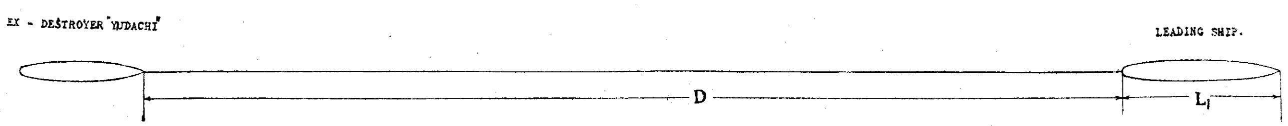


FIG. 14_b PLAN OF TOWING THE EX-DESTROYER "YUDACHI".



PRINCIPAL DIMENSIONS OF EX-DESTROYER "YUDACHI"

LENGTH ON WATER LINE IN FT.		232.00
BREADTH ON WATER LINE IN FT.		21.49
DEPTH TO THE UPPER DECK AT SIDE FROM THE BOTTOM OF KEEL IN FT.		14.24
DRAUGHT IN FT.	FORE	5.63
	AFT	6.19
	MEAN	5.91
DISPLACEMENT (INCLUSIVE RUDDER) IN TONS		368.3
MIDSHIP AREA IN SQ. FT.		96.209
MIDSHIP GIRTH IN FT.		26.634
WETTED SURFACE AREA (INCLUSIVE RUDDER) IN SQ.FT.		4,674
PRISMATIC COEFFICIENT		0.577
MIDSHIP COEFFICIENT		0.758
B/L		0.0926
d/L		0.0255
RUDDER AREA IN SQ. FT. (ONE SIDE ONLY)		34.14
ORDINATES APART IN FT.		11.35
WATER LINES APART IN FT.		1.5

FIG. 15_b.

SCANTLING OF SHELL PLATINGS.

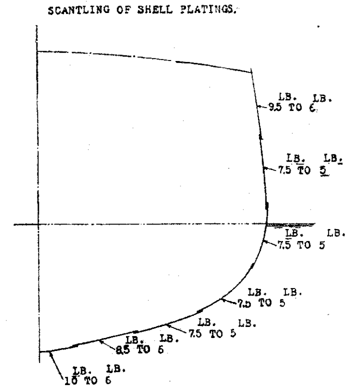
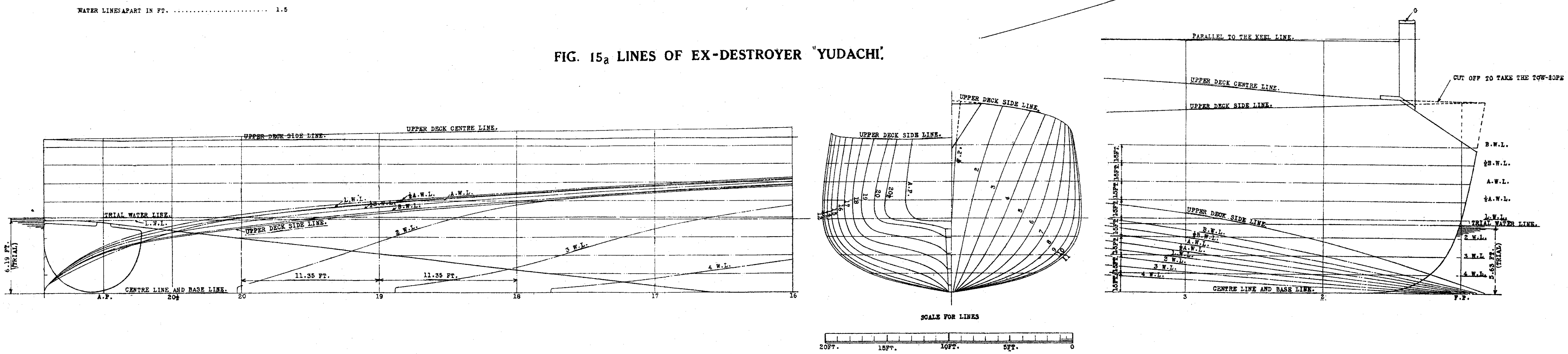


FIG. 15_a LINES OF EX-DESTROYER "YUDACHI".



DYNAMOMETER FOR MEASURING THE RESISTANCE OF EX-DESTROYER *YUDACHI*, ETC.

FIG. 16a

RECORDING DIAGRAM OF THE MEASURED RESISTANCE.

AT SPEED. 19.28 KT

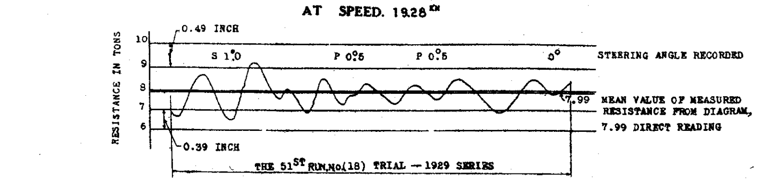


FIG. 16b

RECORDING DIAGRAM OF THE MEASURED RESISTANCE.

AT SPEED 26.432 KT

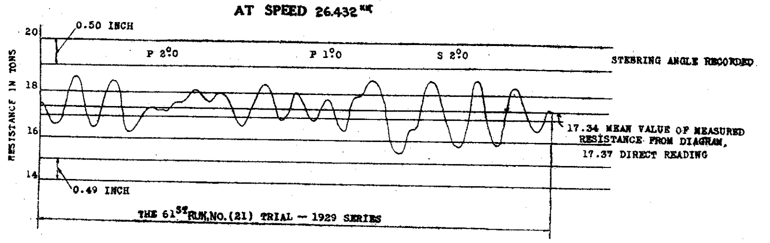


FIG. 17

MEASURED TRIM AGAINST SPEED OF SHIP.

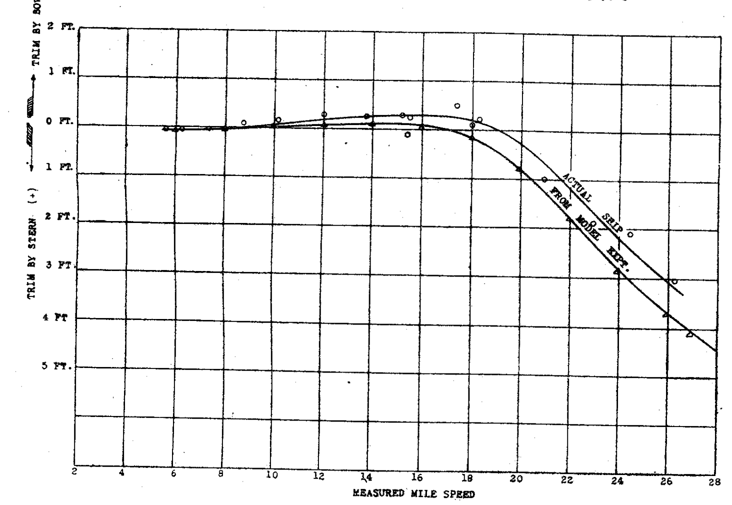
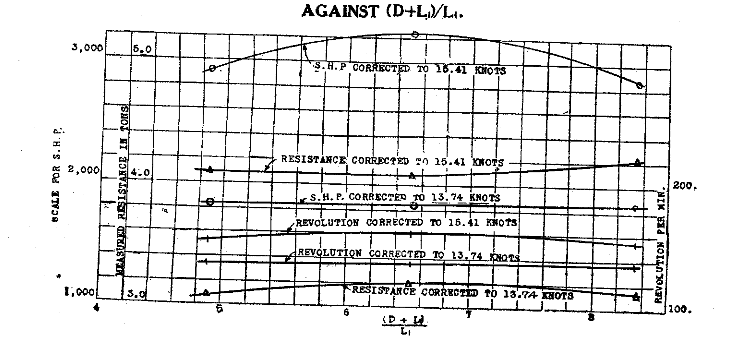


FIG. 18

RESISTANCE OF TOWED SHIP, AND S.H.P. AND REVOLUTION PER MIN. OF TOWING SHIP AGAINST (D+L1)/L1.



CALCULATION OF RESISTANCE OF EX-DESTROYER "YUDACHI" FROM 13-FT. SHIP-MODEL.

TABLE A (FOR DISPLACEMENT 368.3 TONS)

SPEED IN FT. PER MIN.	MEASURED RESISTANCE (λ) AT 59.5°F. IN lb.	FRICTIONAL RESISTANCE (λ_f) AT 59.5°F. IN lb.	$\lambda-\lambda_f=(\lambda_w)'$ AT 59.5°F. IN lb.	AIR RESISTANCE (λ_a) IN Lb.	$(\lambda_w)'-(\lambda_a)=\lambda_w$	CORRESPONDING SPEED OF "YUDACHI", V IN KNOTS	$\left(\frac{\rho_{sw}}{\rho_{fw}}\right)\lambda_w\times(\lambda)^3=(R_w)_t$ AT 76.9°F. (SEA-WATER) IN TONS	DO. FROM 20-FT. MODEL AT 76.9°F (SEA-WATER) IN TONS	* Do. THE FINAL RESULTS
95.9	0.14	0.14	0	0	0	4	0	0	0
119.9	0.21	0.21	0	0	0	5	0	0	0
143.9	0.29	0.29	0	0	0	6	0	0	0
167.9	0.39	0.39	0	0	0	7	0	0	0
191.8	0.51	0.51	0	0	0	8	0	0	0
215.9	0.63	0.63	0	0	0	9	0	0	0
239.8	0.80	0.78	0.02	0.01	0.01	10	0.03	0.02	0.03
263.8	0.95	0.93	0.02	0.01	0.01	11	0.03	0.04	0.04
287.8	1.14	1.10	0.04	0.01	0.03	12	0.08	0.06	0.07
311.8	1.36	1.28	0.08	0.01	0.07	13	0.18	0.11	0.15
335.8	1.59	1.47	0.12	0.01	0.11	14	0.29	0.18	0.24
359.7	1.84	1.68	0.16	0.01	0.15	15	0.39	0.29	0.34
383.7	2.10	1.90	0.20	0.02	0.18	16	0.47	0.41	0.44
407.7	2.39	2.13	0.26	0.02	0.24	17	0.62	0.53	0.58
431.7	2.73	2.37	0.36	0.02	0.34	18	0.88	0.87	0.88
455.7	3.20	2.63	0.57	0.02	0.55	19	1.43	1.45	1.44
479.7	3.77	2.90	0.87	0.02	0.85	20	2.21		2.21
503.7	4.37	3.18	1.19	0.03	1.16	21	3.01		3.01
527.6	4.99	3.47	1.52	0.03	1.49	22	3.87		3.87
551.6	5.61	3.78	1.83	0.03	1.80	23	4.67		4.67
575.6	6.17	4.10	2.07	0.03	2.04	24	5.30		5.30
599.6	6.73	4.43	2.30	0.04	2.26	25	5.87		5.87
623.5	7.26	4.77	2.49	0.04	2.44	26	6.34		6.34
647.5	7.78	5.13	2.65	0.04	2.61	27	6.78		6.78
671.5	8.31	5.49	2.82	0.05	2.77	28	7.19		7.19

TABLE B (FOR DISPLACEMENT 362.1 TONS)

SPEED IN FT. PER MIN.	MEASURED RESISTANCE (λ) AT 59.5°F. IN lb.	FRICTIONAL RESISTANCE (λ_f) AT 59.5°F. IN lb.	$\lambda-\lambda_f=(\lambda_w)'$ AT 59.5°F. IN lb.	AIR RESISTANCE(λ_a) IN lb.	$(\lambda_w)'-(\lambda_a)=\lambda_w$	CORRESPONDING SPEED OF "YUDACHI", V IN KNOTS	$\left(\frac{\rho_{sw}}{\rho_{fw}}\right)\lambda_w\times(\lambda)^3=(R_w)_t$ AT 76.9°F. (SEA-WATER) IN TONS	DO. FROM 20-FT. MODEL AT 76.9°F. (SEA-WATER) IN TONS	Do. THE FINAL RESULTS *
431.7	2.70	2.35	0.35	0.02	0.33	18	0.86	0.86	0.86
455.7	3.16	2.60	0.56	0.02	0.54	19	1.40	1.43	1.42
479.7	3.72	2.87	0.85	0.02	0.83	20	2.16		2.16
503.7	4.32	3.15	1.17	0.03	1.14	21	2.96		2.96
527.6	4.94	3.44	1.50	0.03	1.47	22	3.82		3.82
551.6	5.55	3.74	1.81	0.03	1.78	23	4.62		4.62
575.6	6.09	4.06	2.03	0.03	2.00	24	5.19		5.19
599.6	6.65	4.39	2.26	0.04	2.22	25	5.77		5.77
623.5	7.17	4.73	2.44	0.04	2.41	26	6.26		6.26
647.5	7.69	5.08	2.61	0.04	2.57	27	6.68		6.68
671.5	8.21	5.44	2.77	0.05	2.72	28	7.06		7.06

DIMENSIONS OF THE "YUDACHI" MODELS

ITEM	FOR TABLE A. 13-FT. MODEL	FOR TABLE A. 20-FT. MODEL	FOR TABLE B. 13-FT. MODEL	FOR TABLE B. 20-FT. MODEL
DISPLACEMENT IN LB.	141.45	515.32	139.05	506.65
L, LENGTH ON WATER LINE IN FT.	13.00	20.00	13.00	20.00
L' (LENGTH ON WATER LINE —AFTER CUT UP PORTION OF KEEL) IN FT.	10.19	15.68	10.19	15.68
BREADTH ON WATER LINE IN FT.	1.203	1.85	1.203	1.85
MEAN DRAUGHT IN FT.	0.331	0.509	0.328	0.504
WATTED SKIN AREA IN SQ. FT.	14.67	34.74	14.52	34.39
C, COEFFICIENT OF SCALE EFFECT	0.36	0.36	0.36	0.36
X LOWER EDGE EFFECT IN INCH	0.197	0.04	0.197	0.04
λ, DIMENTION RATIO.	17.85	11.60	17.85	11.60

* FOR 19 KNOTS AND UNDER, THE MEAN VALUES OF $(R_w)_t$ OF TWO MODELS ARE TAKEN; OVER 19 KNOTS, $(R_w)_t$ OF THE 13-FT. MODEL ONLY ARE TAKEN.

FIG. 19 CURVES OF RESISTANCE OF 13-FT. MODEL UNDER CONDITIONS OF DRAUGHT AND TRIM SIMILAR TO THOSE OF EX-DESTROYER "YUDACHI."

EXPERIMENTED IN THE MITSUBISHI TANK.

MEAN TEMPERATURE 59.5°F.

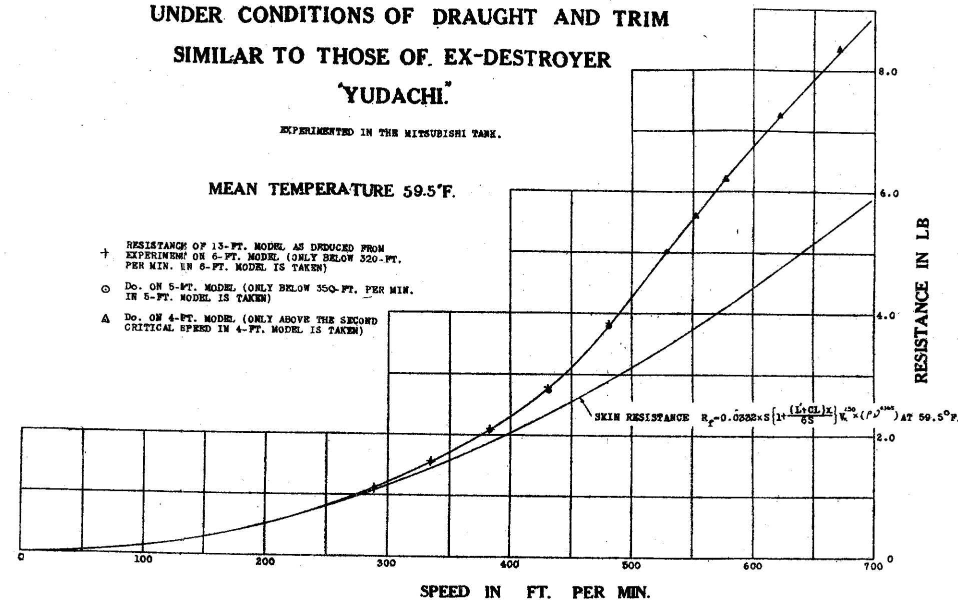
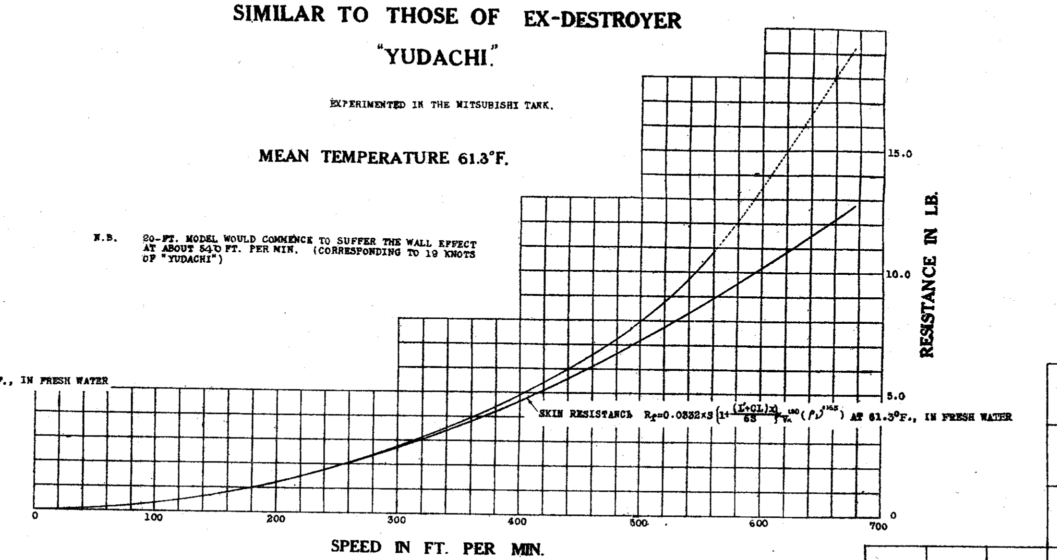


FIG. 20 CURVES OF RESISTANCE OF 20-FT. MODEL UNDER CONDITIONS OF DRAUGHT AND TRIM SIMILAR TO THOSE OF EX-DESTROYER "YUDACHI."

EXPERIMENTED IN THE MITSUBISHI TANK.

MEAN TEMPERATURE 61.3°F.



RESULTS OF TOWING EXPERIMENT WITH EX-DESTROYER "YUDACHI."

SERIES	NO. OF EXPERIMENTS	DATE OF EXPERIMENTS	DAYS OUT OF DOCK	CONDITION OF SEA	MEAN DRAUGUT IN FT.	TRIM BY STERN IN FT.	DISPLACEMENT IN TONS	WETTED SURFACE (S) IN SQ. FT.	DISTANCE BETWEEN BOWS OF TOWING SHIP AND "YUDACHI" (D+L) IN FT.	$\frac{D+L_1}{L_1}$	MEASURED-MILE SPEED, MEAN OF MEANS (V_0) IN KNOTS	$\frac{V_0}{\sqrt{L_1}}$	$\frac{D+L_1}{\lambda}=\frac{D+L_1}{\frac{1}{1.8}V_0^2}$	FACTOR OF SCREW RACE (W) IN %	REAL SPEED(V)=V_0(1+w)	SPEED-LENGTH RATIO OF "YUDACHI"	TEMPERATURE (t)°F.	DENCITY (ρ) IN LB. SEC²/FT⁴	KINEMATIC VISCOSITY (ν) IN FT²/SEC.	MEASURED-RESISTANCE, MEAN OF MEANS, (R_0) IN TONS	AIR RESISTANCE, MEAN OF MEANS, (R_a) IN TONS	$R_0-R_a=(R)_t$ IN TONS	CORRECTED TO CONSTANT DISPLACEMENT OF 368.3 TONS AND CONSTANT WETTED SURFACE OF 4674 SQ. FT.: RESIDUARY RESISTANCE DEDUCED FROM MODEL $(R_w)_t$ (362.1 tons)	FRICTIONAL RESISTANCE $=(R)_t-(R_w)_t$ (FOR 362.1 TONS)	FRICTIONAL RESISTANCE $\{(R)_t-(R_w)_t\}\times\frac{4674}{4626}=(1)$ (FOR 368.3 TONS)	RESIDUARY RESISTANCE R_w(368.3 TONS)=(2) DEDUCED FROM MODEL	TOTAL RESISTANCE $R_t=(1)+(2)$ (FOR 368.3 TONS)	$(R_w)_t$ FROM MODEL, TONS	$(R_f)'_t=K_1 S V_k^{1.90}\rho_r^{0.165}$, TONS	$(R_w)_t+(R_f)'_t=(R)'_t$ TONS	$\frac{(R)_t}{(R)'_t}$	$(R)_t-(R_w)_t=(R_f)_t$, TONS	$(R_f)_t$ CORRECTED TO 55°F., TONS.	K_2 FROM $(R_f)_t$, 55° F. †	$\left(\frac{VL}{\nu}\right)$ IN 10^6	$\left(\frac{(R_f)_t}{\rho S V^2}\right)$	θ_1 IN DEGREES	θ_2 IN DEGREES	θ_3 IN DEGREES	θ_4 IN DEGREES	MEAN TILLER ANGLES IN DEGREES	ADDITIONAL TRIM BY STERN PRODUCED DURING RUN IN FT.	R.P.M. OF TOWING SHIP
	1		15		5.909	0.558	368.3	4674	2127	6.52	8.66	0.48	51.1	0.55	8.71	0.57	76.8	1.981	1.023	1.27	0.04	1.23						0	1.23	1.23	1.00	1.23	1.30	0.0102	333	0.00138	31.2	18.2	-1.6	1.0	0.6	-0.1	85
	2	AUG.-18 1928		CALM					2127	6.52	10.08	0.56	37.7	0.55	10.14	0.67			$\times 10^{-5}$	1.77	0.02	1.75						0.03	1.67	1.70	1.03	1.72	1.81	0.0106	368	0.00142	22.0	15.0	-2.8	0.4	0.7	-0.2	101
	3								2127	6.52	12.04	0.67	26.4	0.55	12.11	0.80				2.47	0.04	2.43						0.08	2.33	2.41	1.01	2.35	2.47	0.0104	464	0.00136	13.2	11.8	-0.6	0.7	0.2	-0.3	119
	4								2127	6.52	13.75	0.76	20.3	0.55	13.83	0.91				3.26	0.05	3.21						0.22	3.00	3.22	1.00	2.99	3.15	0.0103	550	0.00133	11.6	9.3	-0.6	0.8	0.7	-0.3	137
	5		17		5.909	0.558	368.3	4674	2127	6.52	15.24	0.84	16.5	0.55	15.32	1.01	76.8	1.981	1.023	4.01	0.02	3.99						0.36	3.65	4.01	1.00	3.65	3.82	0.0102	587	0.00131	7.7	6.5	-5.8	-1.5	0.1	-0.3	158
1928 SERIES	6	AUG.-20 1928		A LITTLE WAVY					2127	6.52	17.41	0.96	12.6	0.55	17.51	1.15			"	5.32	0.04	5.28						0.68	4.71	5.39	0.98	4.60	4.84	0.0101	670	0.00127	5.4	6.7	-5.8	-1.0	0.7	-0.6	177
	7								2728	8.36	15.47	0.86	20.5	0.16	15.49	1.02				4.26	0.01	4.25						0.37	3.73	4.10	1.04	3.88	4.09	0.0107	593	0.00137	16.8	9.0	-6.1	0.6	1.1	-0.2	157
	8								2728	8.36	13.74	0.76	26.0	0.16	13.76	0.90				3.19	0.05	3.14						0.22	2.98	3.20	0.98	2.92	3.07	0.0101	527	0.00131	22.5	10.0	-4.8	-0.4	0	-0.2	138
	9		18		5.909	0.558	368.3	4674	2127	6.52	7.29	0.40	72.0	0.55	7.33	0.48	76.8	1.981	1.023	0.87	0.01	0.86						0	0.90	0.90	0.96	0.86	0.91	0.0099	281	0.00136	33.8	13.7	-1.4	-0.3	0.8	0	72
	10	AUG.-21 1928		CALM					2127	6.52	7.91	0.44	61.2	0.55	7.95	0.52			"	1.02	0.01	1.01						0	1.03	1.03	0.98	1.01	1.06	0.0099	304	0.00136	26.5	12.8	-2.0	0	1.1	0	78
	11								1527	4.68	6.23	0.35	70.8	1.53	6.33	0.42				0.70	0.01	0.69						0	0.67	0.67	1.03	0.69	0.73	0.0105	242	0.00146	25.6	14.0	-2.1	-0.1	0.2	0	62
	12								1527	4.68	5.61	0.31	87.3	1.53	5.70	0.37				0.53	0	0.53						0	0.55	0.55	0.96	0.53	0.56	0.0098	218	0.00138	33.0	17.4	-2.2	-0.1	0	0	54
	13		19						1592	4.88	13.64	0.76	15.4	1.34	13.82	0.91	76.8	1.981	1.023	3.14	0.10	3.04						0.22	3.00	3.22	0.94	2.82	2.97	0.0097	529	0.00125	10.9	16.6	-0.2	1.0	0.2	-0.3	135
	14	AUG.-22 1928		CALM	5.909	0.558	368.3	4674	1592	4.88	15.41	0.85	12.1	1.34	15.62	1.03			"	4.13	0.05	4.08						0.39	3.79	4.18	0.98	3.69	3.89	0.0101	598	0.00128	6.8	5.5	0	-0.1	0.7	+0.1	154
	15								2793	8.56	18.04	1.00	15.4	0.12	18.06	1.19				6.06	0.02	6.04						0.90	5.00	5.90	1.02	5.14	5.41	0.0106	691	0.00134	9.5	6.0	-1.1	0.1	0.1	-0.2	186
	16								2793	8.56	20.97	1.16	11.4	0.12	21.00	1.38				9.18	0.08	9.10						3.01	6.67	9.69	0.94	6.09	6.41	0.0094	804	0.00117	6.2	5.0	-0.2	1.2	0.4	+0.6	218
	17	AUG.-19 1929	8	CALM	5.850	0.505	362.1	4626	2749	8.43	18.29	1.01	14.8	0.13	18.31	1.20	77.5	1.982	1.023	6.62	0.06	6.56	1.01	5.55	5.61	1.08	6.63	1.02	5.13	6.15	1.08	5.61	5.96	0.0113	701	0.00142					0.3	-0.1	189
1929 SERIES	18								2749	8.43	19.74	1.09	12.7	0.13	19.77	1.30			"	7.83	0.07	7.76	1.96	5.80	5.86	2.01	7.87	2.01	5.94	7.95	0.99	5.86	6.16	0.0102	757	0.00127					0.4	+0.2	206
	19		9						2749	8.43	22.70	1.26	9.6	0.13	22.73	1.49	77.5	1.982	1.023	12.11	0.11	12.00	4.39	7.61	7.63	4.46	12.15	4.46	7.73	12.19	1.00	7.69	8.09	0.0103	870	0.00126					0.2	+1.5	239
	20	AUG.-20 1929		CALM	5.850	0.505	362.1	4626	2749	8.43	24.46	1.35	8.3	0.13	24.49	1.61			"	14.21	0.16	14.05	5.48	8.57	8.66	5.59	14.25	5.59	8.92	14.51	0.98	8.66	9.11	0.0100	938	0.00122					0.8	+2.4	260
	21								2749	8.43	26.33	1.46	7.1	0.13	26.36	1.73				16.95	0.16	16.79	6.41	10.38	10.43	6.51	17.00	6.51	10.25	16.76	1.01	10.49	11.04	0.0106	1,009	0.00128					0.8	+3.2	285
																														MEAN VALUE.	0.996 OR 1.00	MEAN VALUE		0.0102									

DEPTH OF SEA IN FATHOMS 164

DISTANCE BETWEEN MILE-POSTS IN FT. .. 6080

TOWING SHIP { 1928 SERIES T.B.D. "YUKAZE." / 1929 SERIES T.B.D. "SHIMAKAZE."

LENGTH ON WATER LINE IN FT. 326.25, L_1
BREADTH ON WATER LINE IN FT. 29.25
MEAN DRAUGHT IN FT. 9.46
DISPLACEMENT IN TONS. 1,300

DENSITY OF SEA WATER ρ IN (LB.) $\times$ (SEC.)^2 $\times$ (FT.)$^{-4}$ AT 55°F. 1.986

KINEMATIC VISCOSITY OF SEA-WATER ν IN (FT.)^2 $\times$ (SEC.)$^{-1}$ AT 55°F. 1.376×10^{-5}

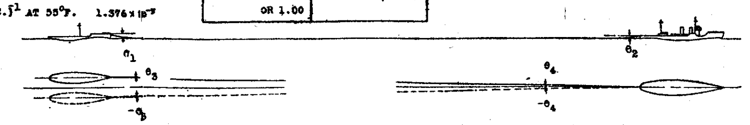
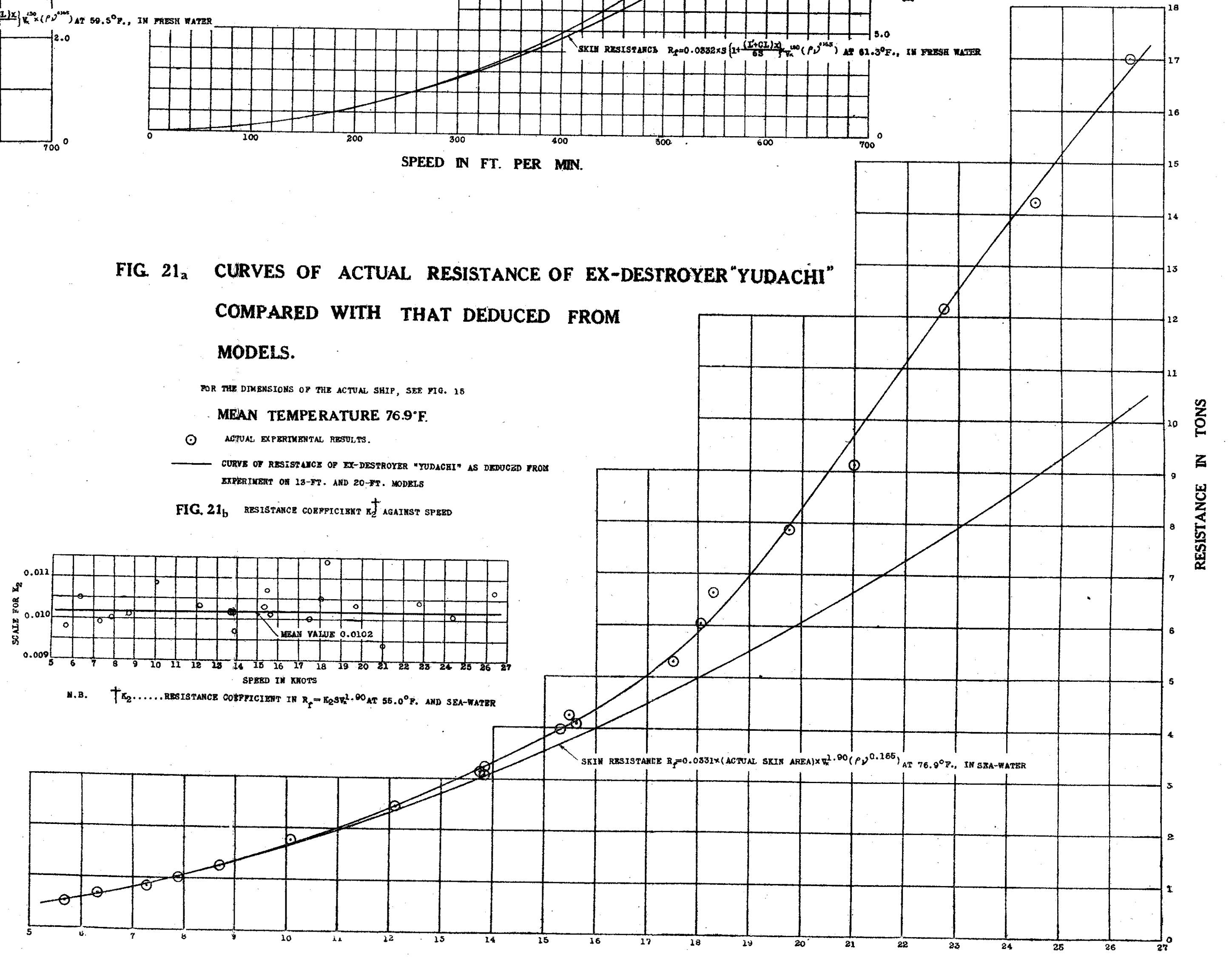


FIG. 21a CURVES OF ACTUAL RESISTANCE OF EX-DESTROYER "YUDACHI" COMPARED WITH THAT DEDUCED FROM MODELS.

FOR THE DIMENSIONS OF THE ACTUAL SHIP, SEE FIG. 18

MEAN TEMPERATURE 76.9°F.



CULCULATION OF RESISTANCE OF 300-TON TUG-BOAT FROM 13-FT. SHIP-MODEL.

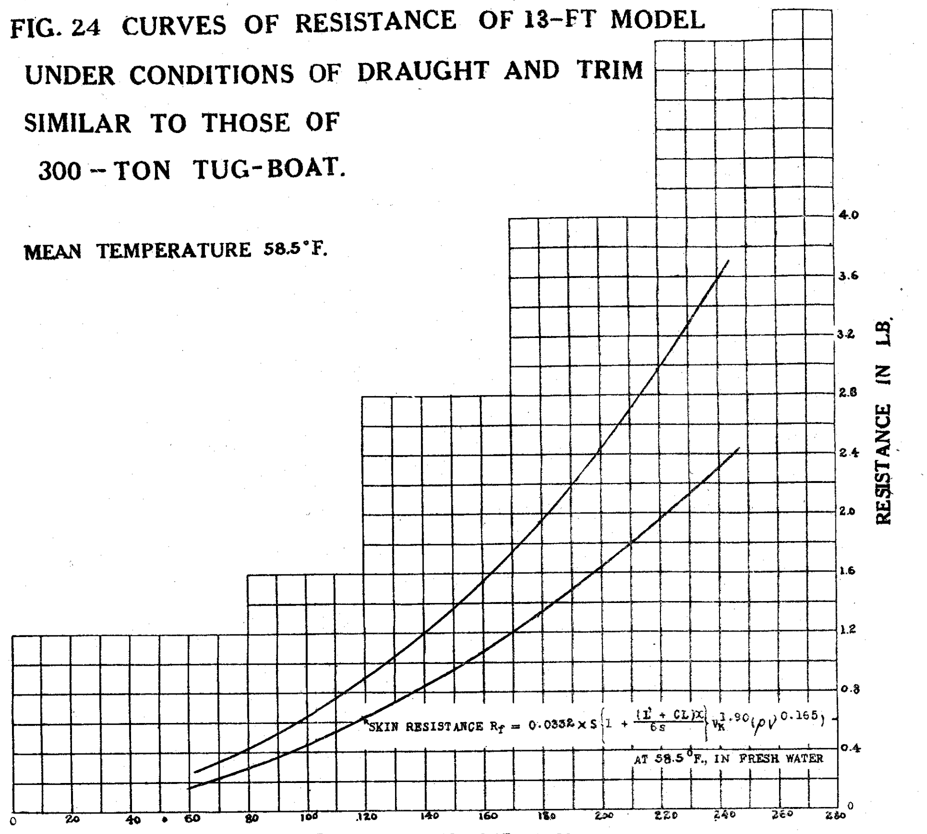
(WITH ALL APPENDAGES)

SPEED (v) IN FT PER MIN	MEASURED RESISTANCE (r) AT 58.5°F. IN LB.	FRICTIONAL RESISTANCE (r_f) AT 58.5°F. IN LB.	$r - r_f = (r_w)'$ AT 58.5°F. IN LB.	CORRESPONDING SPEED OF TUG BOAT (V) IN KNOTS.	$\frac{\rho_{s.w.}}{\rho_{f.w.}} \times (r_w)' \times \lambda^3 = (R_w)_t$ AT 61.0°F SEA-WATER IN TONS.
68.5	0.292	0.212	0.080	2.0	0.024
85.6	0.452	0.324	0.128	2.5	0.039
102.7	0.652	0.457	0.195	3.0	0.060
119.9	0.894	0.614	0.280	3.5	0.086
137.0	1.175	0.790	0.385	4.0	0.118
154.1	1.466	0.988	0.478	4.5	0.146
171.2	1.762	1.207	0.555	5.0	0.170
188.3	2.116	1.447	0.669	5.5	0.205
205.5	2.561	1.709	0.882	6.0	0.261
222.6	3.049	1.987	1.062	6.5	0.325
239.7	3.554	2.288	1.266	7.0	0.387

DIMENSIONS OF 300-TON TUG-BOAT MODEL

ITEM	
L, LENGTH ON WATER LINE IN FT.	13.12
L, (LENGTH ON WATER LINE-AFTER CUT UP PORTION IN FT.)	13.12
BREADTH ON WATER LINE IN FT.	2.63
MEAN DRAUGHT IN FT.	0.94
WETTED SKIN AREA IN SQ. FT.	43.42
C, COEFFICIENT OF SCALE EFFECT	1.83
x, LOWER EDGE EFFECT IN INCH	0.19
λ, DIMENSION RATIO	8.75

FIG. 24 CURVES OF RESISTANCE OF 13-FT MODEL UNDER CONDITIONS OF DRAUGHT AND TRIM SIMILAR TO THOSE OF 300-TON TUG-BOAT.



RESULTS OF TOWING EXPERIMENT WITH 300-TON TUG-BOAT.

DENSITY OF SEA-WATER ρ IN (LB.)×(SEC.)^2×(FT.)$^{-4}$ AT 55°F. 1.981, KINEMATIC VISCOSITY OF SEA-WATER ν IN (FT.)^2×(SEC.)$^{-1}$ AT 55°F. 1.331×10^{-5}

NO OF EXPT.	DATE OF EXPTS	DAYS OUT OF DOCK	CONDITION OF SEA	MEAN DRAUGHT IN FT	TRIM BY STERN IN FT	DISPLACEMENT IN TONS	WETTED SKIN AREA IN SQ. FT.	$\frac{D+L_1}{L_1}$	MEASURED-MILE SPEED MEAN OF MEANS. (Vo) IN KNOTS	$\frac{V_0}{\sqrt{89.0}}$	$\frac{D+L_1}{\lambda} = \frac{D+L_1}{\frac{1}{1.8}V_0^2}$	FACTOR OF SCREW RACE (w) IN %	REAL SPEED (V) = $V_0(1+w)$	$\frac{V}{\sqrt{114.83}}$	TEMPERATURE IN °F	DENSITY (ρ) IN LB×SEC^2×FT^{-4}	KINEMATIC VISCOSITY ν IN FT^2·SEC^{-1}	MEASURED RESISTANCE, MEAN OF MEANS (R)$_t$ IN TONS	RESIDUARY RESISTANCE FROM MODEL $(R_w)_t$ IN TONS	FRICTIONAL RESISTANCE $(R_f)'_t = K_1 S V_k^{1.9} \rho \nu^{.165}$ IN TONS	$(R_w)_t + (R_f)'_t = (R)'_t$ IN TONS	$\frac{(R)_t}{(R)'_t}$	$(R)_t - (R_w)_t = (R_f)_t$ IN TONS	$(R_f)_t$ CORRECTED TO 55°F $(R_f)_{55°F}$, IN TONS	K_2 FROM $(R_f)_{55°F} = K_2 S V_k^{1.9}$	$\left\{\frac{VL}{\nu}\right\}$ IN 10^6	$\left\{\frac{(R_f)_t}{\rho S V^2}\right\}$
1	1930 APR. 24	1	CALM	8.24	2.87	296.9	3324	8.22	6.85	.726	28.1	0.20	6.86	0.640	61.0	1.981	1.220 ×10^{-5}	0.980	0.369	0.584	0.953	1.028	0.611	0.620	0.0108	109.1	0.00155
2									5.72	.607	40.3	0.20	5.75	0.535				0.674	0.227	0.415	0.642	1.050	0.44	0.453	0.0111	91.1	0.00162
3									3.85	.408	88.8	0.20	3.86	0.360				0.340	0.109	0.196	0.305	1.115	0.231	0.234	0.0121	61.4	0.00135
4									2.39	.253	231	0.20	2.40	0.224				0.134	0.035	0.079	0.114	1.175	0.099	0.100	0.0128	38.2	0.00206
5									3.01	.319	146	0.20	3.02	0.282				0.196	0.061	0.123	0.184	1.065	0.135	0.137	0.0113	48.0	0.00177
																					MEAN VALUE	1.087	MEAN VALUE		0.0116		

DEPTH OF SEA IN FATHOMS 17

DISTANCE OF THE MILE POSTS IN FT. 3,383

TOWING SHIP "SUZUURAMARU"
- LENGTH ON WATER LINE IN FT, L_1 .. .. .. 89.00
- BREADTH ON WATER LINE IN FT. 18.75
- DRAUGHT IN FT. 9.25
- DISPLACEMENT IN TONS 143.00

FIG. 25 CURVES OF ACTUAL RESISTANCE OF 300-TON TUG-BOAT COMPARED WITH THAT DEDUCED FROM MODEL.

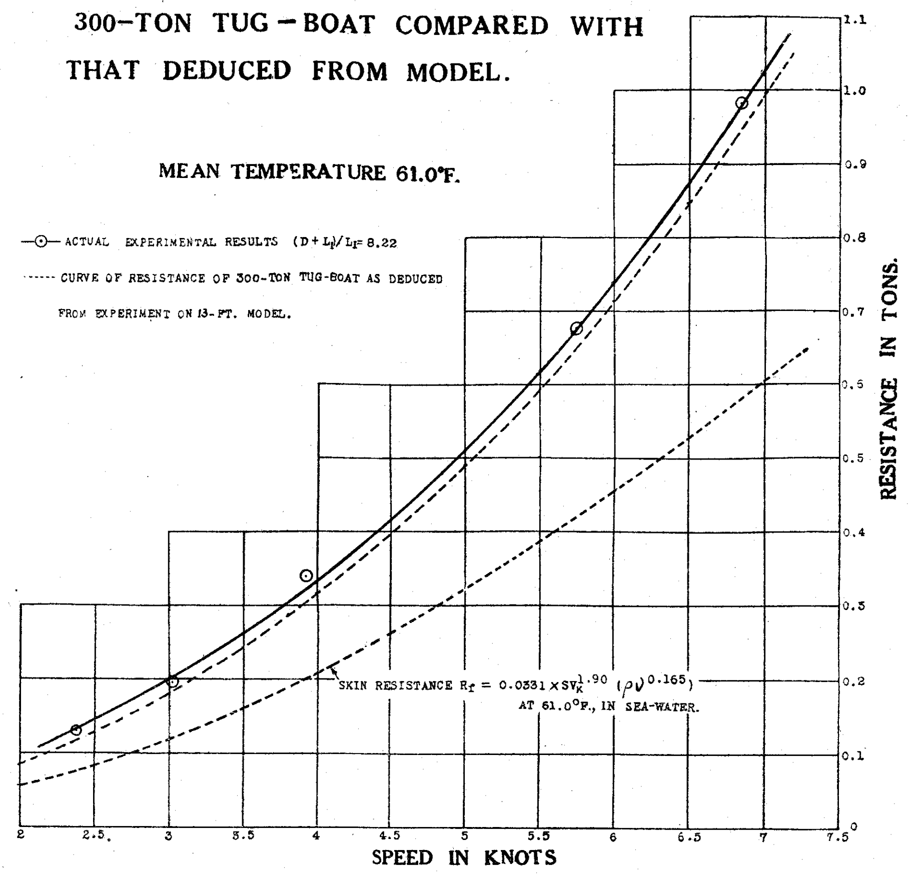


FIG. 23a LINES OF 300-TON TUG-BOAT.

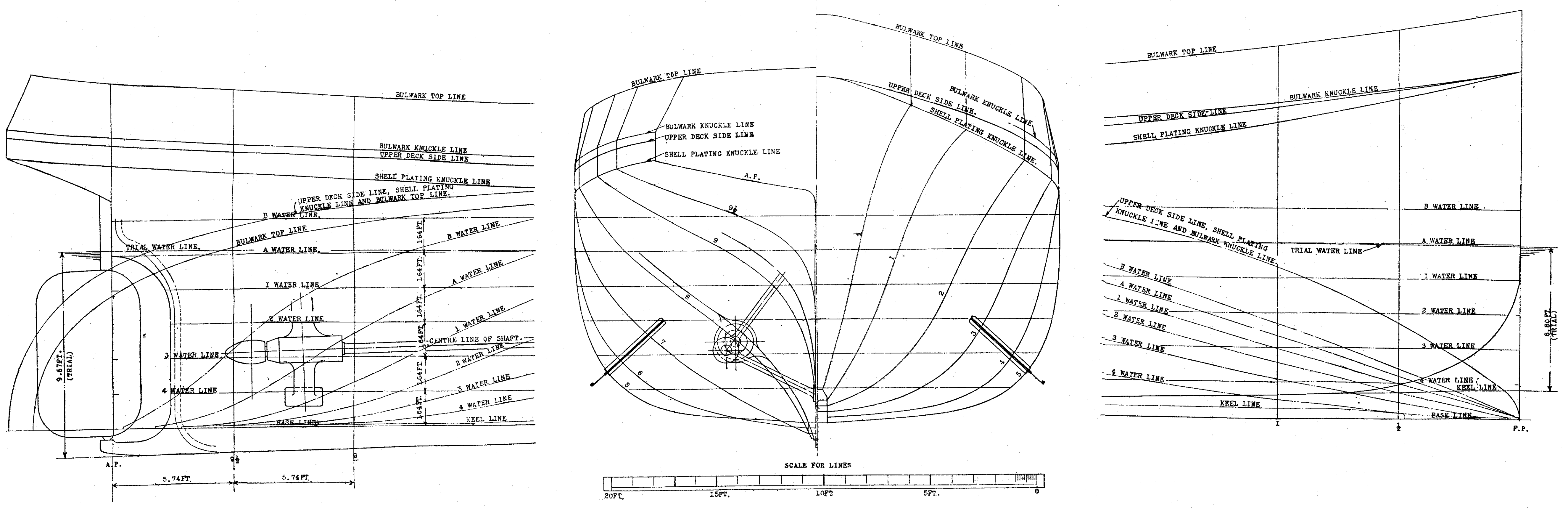
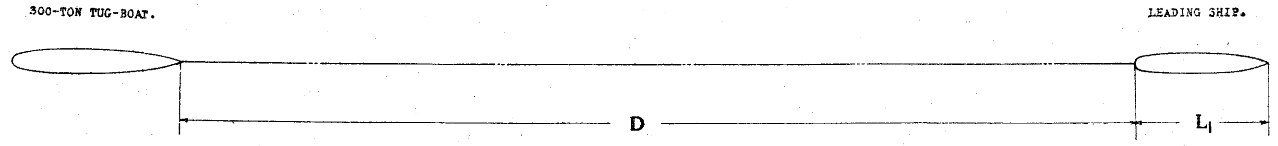


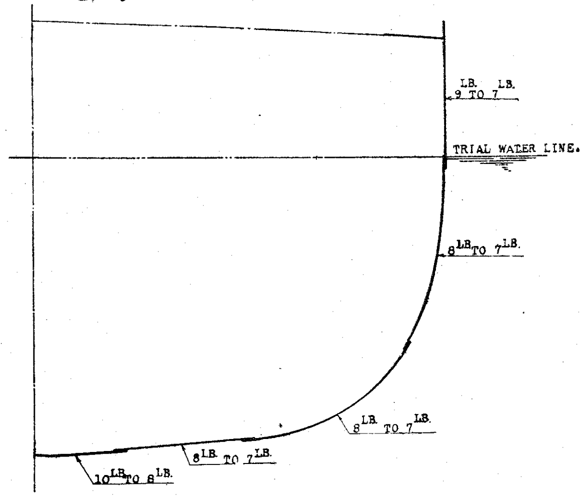
FIG. 22 PLAN OF TOWING THE 300-TON TUG-BOAT.



PRINCIPAL DIMENSIONS OF 300-TON TUG-BOAT.
(WITHOUT THE PROPELLER)

LENGTH ON WATER LINE IN FT.		114.83
BREADTH ON WATER LINE IN FT.		22.97
DRAUGHT IN FT.	FORE	6.80
	AFT	9.67
	MEAN	8.24
DISPLACEMENT (INCLUSIVE ALL APPENDAGES) IN TONS.		296.93
MIDSHIP AREA IN SQ. FT.		164.75
MIDSHIP GIRTH IN FT.		33.14
WETTED SKIN AREA (INCLUSIVE ALL APPENDAGES) IN SQ. FT.		3324
PRISMATIC COEFFICIENT		0.548
MIDSHIP COEFFICIENT		0.870
B/L		0.200
d/L		0.0717
RUDDER AREA IN SQ. FT. (ONE SIDE ONLY)		36.80
ORDINATES APART IN FT.		11.48
WATER LINES APART IN FT.		1.64
WETTED SKIN AREA FOR APPENDAGES ONLY IN SQ. FT.		415.00

FIG. 23b SCANTLING OF SHELL PLATINGS.



RESISTANCE OF ACTUAL SHIP AND SHIP MODEL, ETC.

To Illustrate Professor Yuzuru Hiraga's Paper on "Experimental Investigations on the Resistance of Long Planks and Ships."

FIG. 27 CURVES OF RESISTANCE OF "GREYHOUND" MODEL.

SCALE OF MODEL 1/16.

REPRODUCED FROM W. FROUDE'S 1874 PAPER.

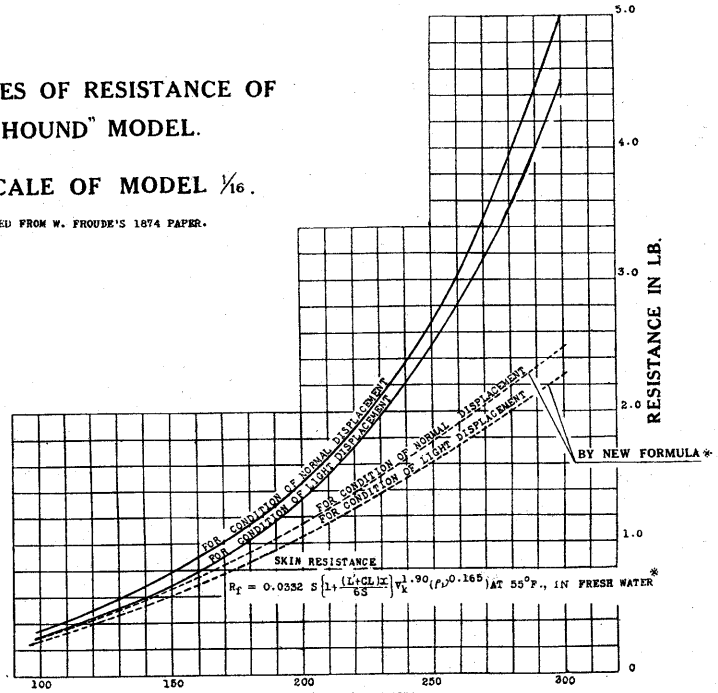
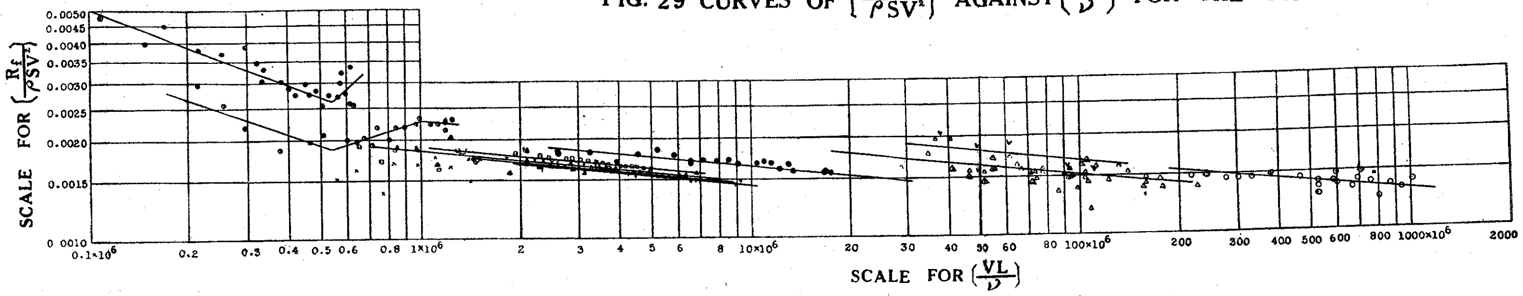


FIG. 29 CURVES OF $\left(\frac{R_f}{\rho S V^2}\right)$ AGAINST $\left(\frac{VL}{\nu}\right)$ FOR THE PAINTED SURFACE.



MARKS	SHIPS AND PLANKS	$\frac{d}{L}$	TEMPERATURE IN °F
○	EX-DESTOROYER "YUDACHI"	0.0255	76.9
▾	300-TON TUG-BOAT	0.0717	61.0
△	77.319-FT. PLANK SHIP	0.0432	61 – 81
●	25.774-FT. PLANK	0.0455	54.0
+	8-FT. PLATE	0.0625	72.8
▲	6-FT. PLATE	0.0313	78.5
▫	5-FT. PLATE	0.0313	72.5
×	4-FT. PLATE	0.0313	78.0
◐	2-FT. PLATE (LACQURED PLATE)*	0.0833	38.9
•	1-FT. PLATE (LACQURED PLATE)*	0.0833	43.9

* RESISTANCE WAS MULTIPLIED BY $\frac{0.0331}{0.0321}$ TO CORRECT TO THE PAINTED SURFACE.

FIG. 28 CURVES OF ACTUAL RESISTANCE OF H. M. S. "GREYHOUND" COMPARED WITH THAT ABOVE DEDUCED FROM MODEL.

REPRODUCED FROM W. FROUDE'S 1874 PAPER.

FIG. 28a CONDITION OF NORMAL DISPLACEMENT.

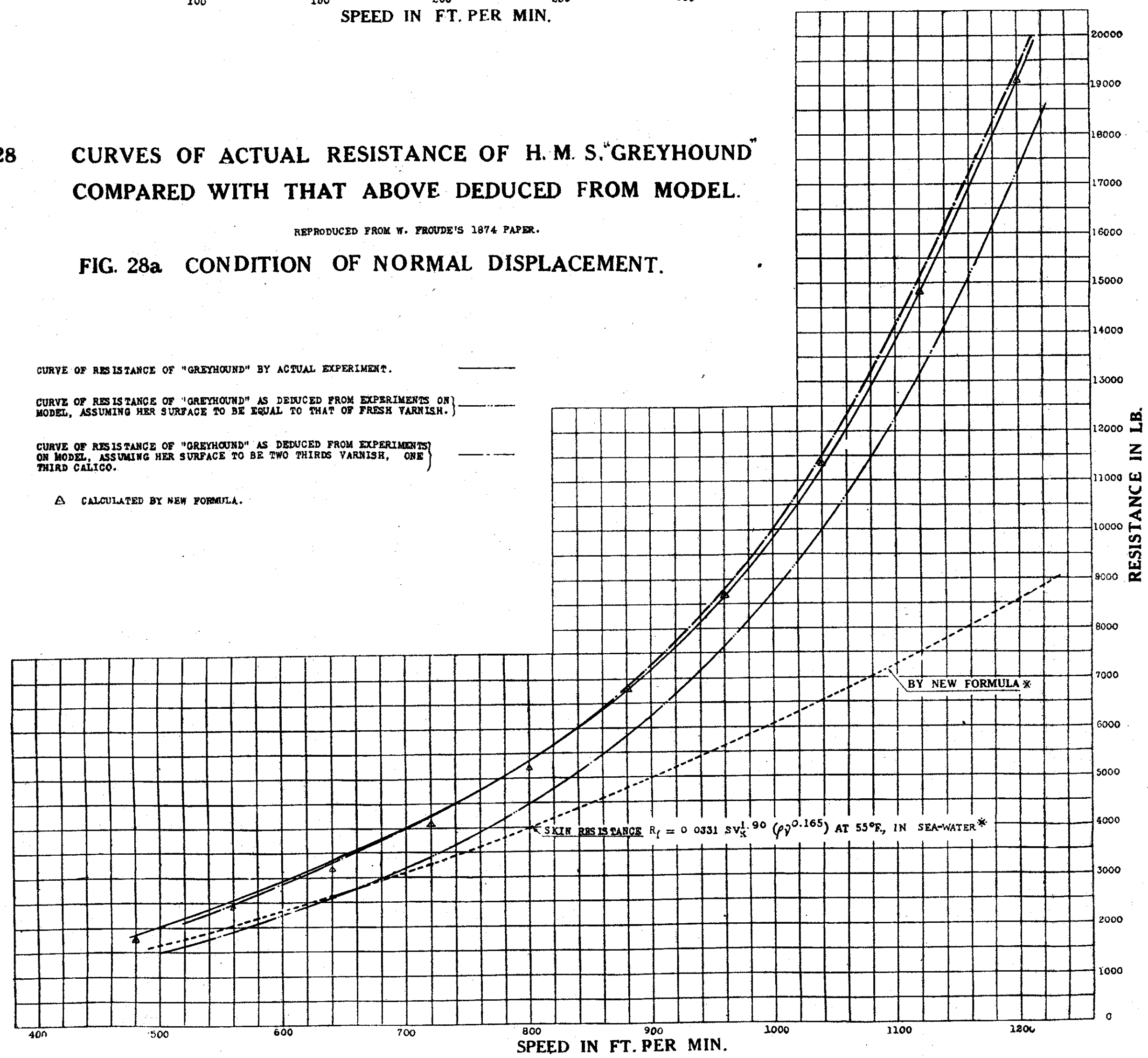
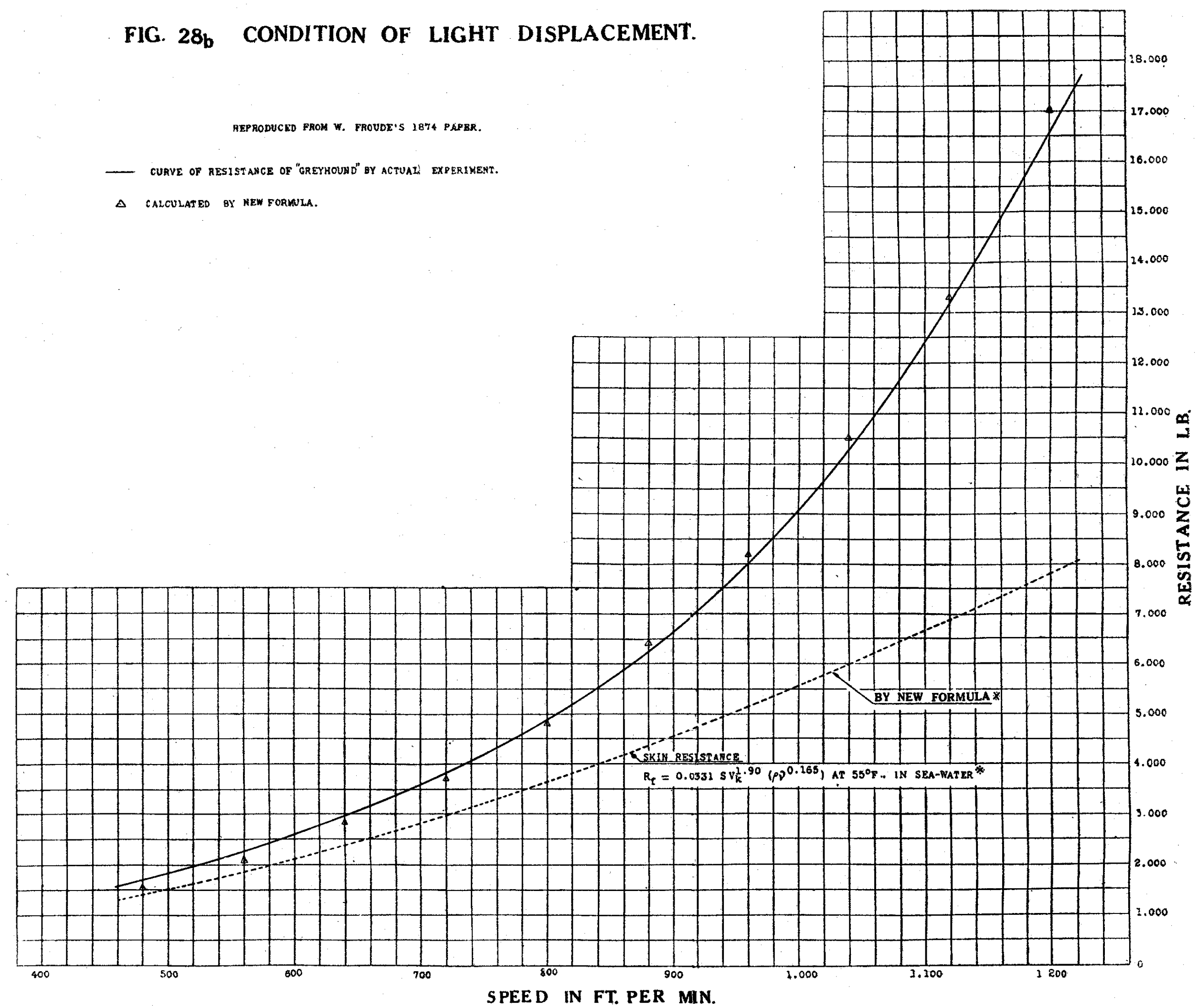


FIG. 28b CONDITION OF LIGHT DISPLACEMENT.

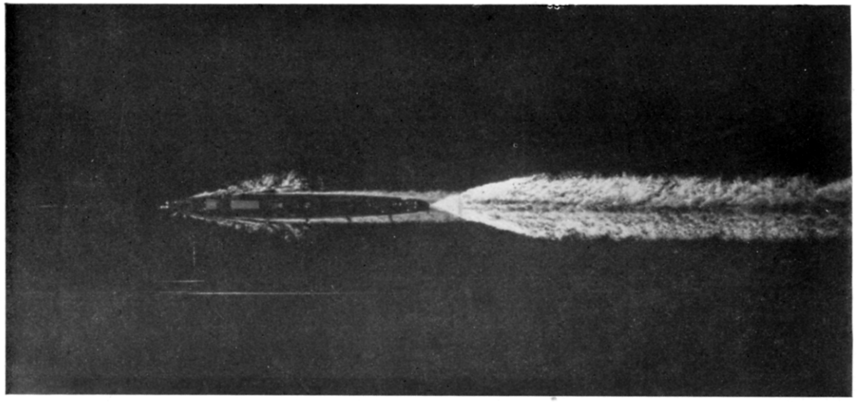
REPRODUCED FROM W. FROUDE'S 1874 PAPER.



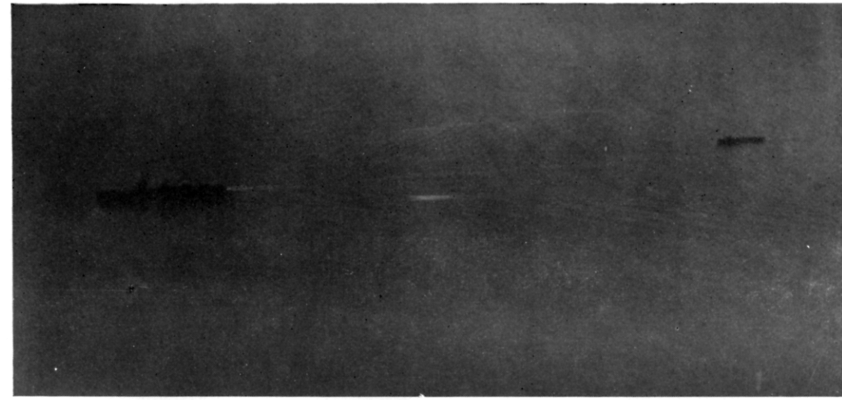
RESISTANCE OF ACTUAL SHIP AND SHIP MODEL.

Trans. Inst. Naval Architects, Vol. LXXVI., 1934. *Plate XXXIII.*

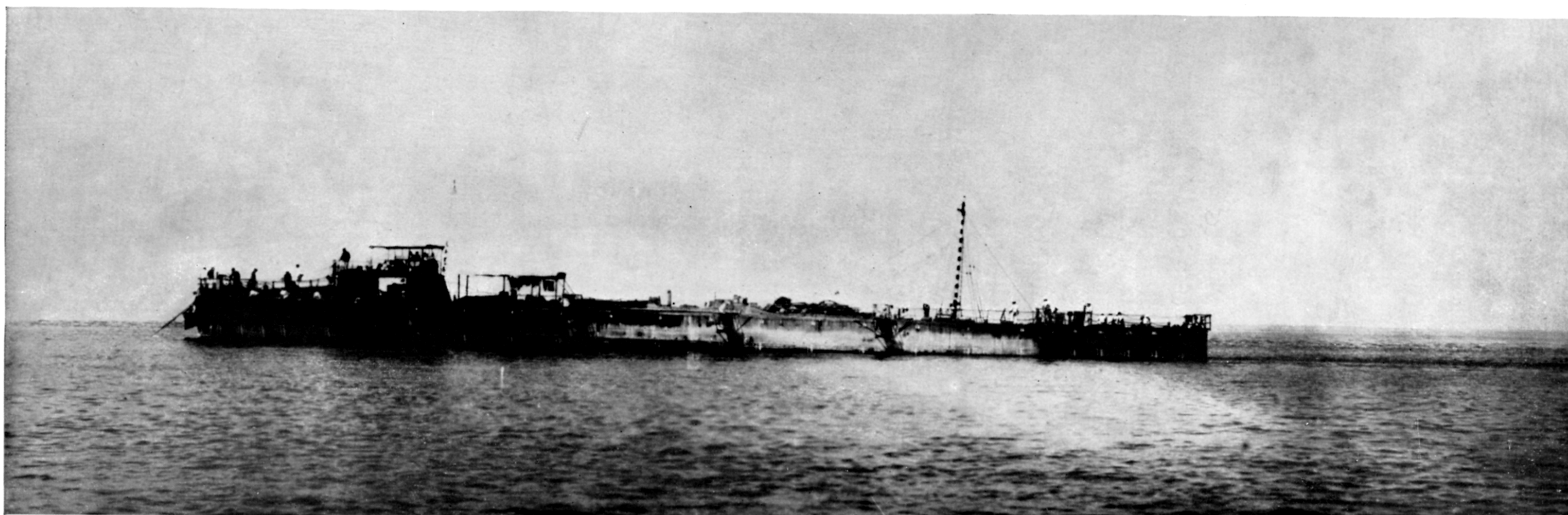
To Illustrate Professor Yuzuru Hiraga's Paper on "Experimental Investigations on the Resistance of Long Planks and Ships."



Ex-destroyer *Yudachi* being towed at 21 knots.



Ex-destroyer *Yudachi* being towed by the first-class destroyer *Yukaze* at 17·4 knots. (Length of tow-rope = 1,800 ft.)



Ex-destroyer *Yudachi* being towed at 7·3 knots.

TOWING EXPERIMENTS WITH EX-DESTROYER *YUDACHI*.

THE INFLUENCE OF VISCOSITY ON THRUST AND TORQUE OF A PROPELLER WORKING NEAR THE SURFACE.

By Dr. G. KEMPF, of Hamburg, Member.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, Julv 11. 1934, Sir EUSTACE D'EYNCOURT, Bart., K.C.B., D.Sc., F.R.S., Vice-President, in the Chair.]

1. *Introduction.*—The influence of viscosity on thrust and torque of a propeller has been studied up till now only for deeply immersed screw propellers. Recently the research work was carried on for this problem by Dr. Gütsche and Mr. J. F. Allan. They tested the lift- and drag-coefficients for blades of various sections at different Reynolds' numbers. From their results it can be stated that a minimum Reynolds' number of $\frac{V \,.\, b}{\nu}$ (with $V =$ actual speed, $b =$ breadth of the blade section at $0 \cdot 7$ of the radius, $\nu =$ viscosity) of $2 \cdot 5 \times 10^5$ can be admitted for testing model propellers without harming seriously the law of similarity when deriving the thrust and torque of the ship's propeller. The influence of viscosity for deeply immersed propellers may be neglected in practice above a Reynolds' number of $2 \cdot 5 \times 10^5$.

When testing self-propelled ship models in light condition it was found that thrust and torque diminished in a higher degree than should be expected from the immersed area of the propeller only, and experience showed that in certain cases the diminution of thrust of the ship's propeller was greater than that of the model propeller, which led to the conclusion that viscosity may interfere when the propeller tips are working near the surface so that air can be sucked down into the propeller.

2. The influence of immersion on a four-bladed model propeller of $0 \cdot 3$ m. diameter was measured at constant revolution, i.e. at constant Reynolds' number for equal slip. The tests were carried out for every immersion with constant revolutions at different speeds of advance. Even the curves of thrust and torque for deeply immersed condition of the propeller show a diminution beginning with a certain angle of slip in the neighbourhood of 55 per cent.

This may be the point where stalling begins at the inner sections of the blades. When the immersion of the tips gets less, air is infiltrating through the tip-vortex along the back of the blade into the deadwater region behind the inner sections, and into the central vortex behind the hub.

It is found, even on propellers working deeply immersed in clear water, that the air retained in the water from the bow wave is concentrated in small strings of air bubbles in the centre of the tip- and hub-vortices of the propeller.

With the tips coming nearer to the surface and breaking it the amount of air sucked down by the propeller is constantly growing, and consequently the density of the fluid is diminishing—and hence the thrust and torque of the propeller—without affecting the efficiency very much.

3. Experiments with a series of three-bladed propellers have given similar results. Three sizes of geometrically similar propellers were tested, viz:—

No. 134 with a diameter of 0·2 m.
No. 152 with a diameter of 0·3 m.
No. 136 with a diameter of 0·6 m.

The pitch ratio was 1·0. The blades had circular sections and elliptical form.

The propellers were similar to propeller No. 13 of D. W. Taylor's systematical set.

Propeller No. 136 was tested on a special dynamometer for big screws. The other propellers were tested on the normal dynamometer. The smaller propellers were tested at several speeds of revolution up to the highest possible. Thus a range of Reynolds' numbers within $1\cdot14 \times 10^5$ to $9\cdot70 \times 10^5$ was covered. In Figs. 1, 2 and 3 (Plate XXXIV.) the results are given for different immersions, and in Fig. 4 the results are given only for that immersion where the tips touch the surface in all three propellers. $I/R = 1\cdot0$.

4. It may be seen that at the point where stalling begins the thrust and torque break down rather suddenly by sucking air. For higher slips, when the point of satiation with

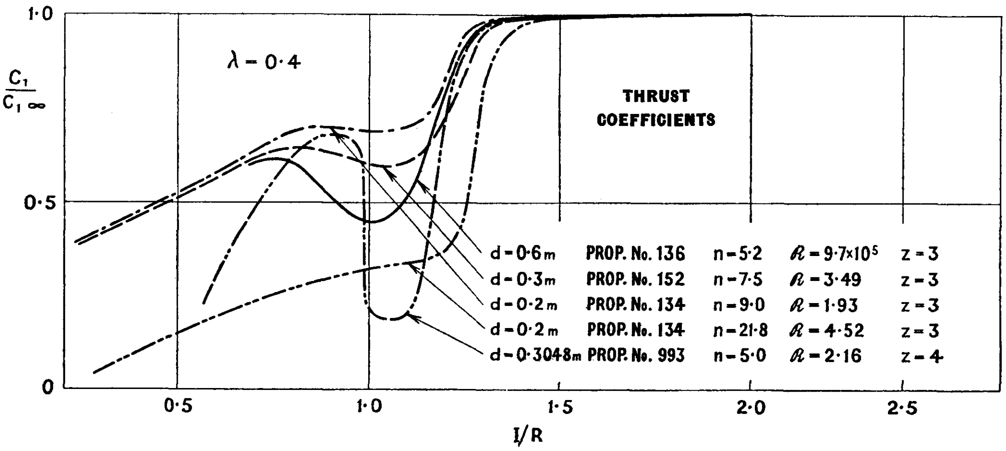


FIG. 5.

air has been reached a slight improvement of thrust and torque is to be noticed from the curves.

At higher revolutions, i.e. with higher Reynolds' number, more air is sucked down, the density (and hence the thrust and torque) diminishes still more, and the point of stalling on the blade sections is reached already at lower slip ratios. The same effect happens when the tips emerge still more out of the surface. And as the thrust recovers with increasing slip after having reached the lowest point when the vortices are satiated with air, it is quite explicable that at a certain slip ratio the thrust can increase even when the blade tips are more out of the surface. This result has been measured with all the propellers, and may be seen especially from Figs. 1 and 2 (Plate XXXIV.) and from Fig. 5. Some further experiments have also shown that with broader tips, i.e. when the area of the blades is shifted to the tips, air is sucked down already at a lower slip.

5. The amount of diminution of thrust is also to be seen from Fig. 5 above, where the ratio of $\frac{C_1}{C_{100}}$, i.e. thrust coefficient for the measured immersion C_1 to the thrust coefficient for deep immersion C_{100}, shows clearly the influence of viscosity.

6. The curve of the thrust coefficient for the lowest Reynolds' number $1\cdot14 \times 10^5$ for the propeller of 0·2 m. diameter on Fig. 4 shows at a sufficient deep immersion a marked

difference from the other curves at normal slip ratios. This is the influence measured by Gütsche and Allan, which may be attributed to there being more deadwater behind the blade section at lower Reynolds' number as it is also known from experiments with balls. More deadwater means reduced circulation.

7. *Conclusion.*—For propellers working near the surface the influence of viscosity is essential for still higher Reynolds' numbers than for propellers working deeply immersed. This influence begins to be noticeable when the tips approach the surface nearer than one-third radius of the propeller, and when the slip ratio and hence the angle of incidence for the blade sections becomes great enough for stalling, i.e. approximately at a slip ratio higher than 50 per cent. The limit for decreasing of thrust and torque has not yet been reached by the experiments, but it seems that with Reynolds' numbers over 5×10^5 the influence becomes gradually less. For model tests in light condition, when the propeller is expected to work at high slip ratio, caution must be taken. To avoid air being sucked in, with consequent diminution of thrust of propellers working near the surface, it is known that performance is improved when the surface over the propeller is covered by a plate.

APPENDIX.

The denominations and symbols are:—

D = diameter in metres;

R = radius;

I = immersion;

n = revolutions per second;

z = number of blades;

v_e = speed of advance;

$\mathfrak{R} = \dfrac{\mathrm{V} \,.\, b}{\nu}$ = Reynolds' number;

$\mathrm{V} = \sqrt{v_e^2 + u^2}$ = actual speed of the blade;

$u = 2\,\pi \,.\, n \,.\, 0{\cdot}7\,\mathrm{R}$ = peripheral speed;

b = breadth of the blade section at $0{\cdot}7$ of the radius;

$\nu = \dfrac{\mu}{\rho}$ = kinematic viscosity;

$\lambda = \dfrac{v_e}{n \,.\, \mathrm{D}}$ = rate of advance;

$\mathrm{C}_1 = \dfrac{\mathrm{T}}{\rho \,.\, \mathrm{D}^4 \,.\, n^2}$ = thrust coefficient;

$\mathrm{C}_2 = \dfrac{\mathrm{Q}}{\rho \,.\, \mathrm{D}^5 \,.\, n^2}$ = torque coefficient;

$\mathrm{C}_{100} = \mathrm{C}_1$ for $\mathrm{I/R} > 1{\cdot}5$;

$\eta_p = \dfrac{\mathrm{C}_1 \,.\, \lambda}{\mathrm{C}_2 \,.\, 2 \,.\, \pi}$ propeller efficiency.

DISCUSSION.

Dr. Ing. F. Gebers (Member): Dr. Kempf's paper gives me the opportunity of directing your attention to another type of propellers for which we must reckon very much more on the viscosity of the water; I refer to semi-submerged propellers. In this case, Tank tests will often not be in agreement with full-sized propeller tests. The reason is clearly the same as in the case of deeply submerged propellers, such as those of tug-boats where we have a very high pressure on the blades. Then very often large propellers scatter and disperse the water in millions of drops—whereas in Tank tests we have a very clean cut of the water from the single blade of the propeller. We made such tests in Vienna, and as we realized this effect we used a very large scale, one-quarter full size. Afterwards, with a large ship we found that we could only get 65 per cent. of the thrust obtained from the Tank experiments. We then found the reason to be quite simple, namely, the viscosity of the water. We took the three-blade propellers and changed them into two-blade propellers so as to get a single cut of water 50 per cent. larger than for the three-blade propeller. This gave us for the full-sized propeller the same thrust as had been given in experiments with two-blade propellers. In many ways Tank Superintendents and observers must have very great foresight in reckoning their results for full-sized ships.

The Chairman (Sir Eustace d'Eyncourt, Bart., K.C.B., D.Sc., F.R.S., Vice-President): You will, I am sure, wish to thank Dr. Kempf for his very interesting contribution to our papers this morning. We are greatly indebted to him, and I would like to move a hearty vote of thanks to Dr. Kempf.

Written Contributions to the Discussion.

Mr. M. P. Payne (Member): It is interesting to note from Dr. Kempf's results in the present paper that although viscosity or scale effect—it is much the same thing as here treated—influences the result for propellers when the tips are breaking surface or working near the surface, its importance is negligible at an immersion (to the centre of the propeller) of 80 per cent. of the diameter. This immersion was established as sufficiently deep many years ago, and it is gratifying to find that the many researches on surface effect of screw propeller models in recent years broadly agree in endorsing this early criterion.

It was with great interest that I read that Dr. Kempf had found that, with broader tips to the propeller, air is more readily sucked down. This agrees with the results of some special experiments carried out at Haslar recently. It is noted that the effect of different speeds at small immersions is very variable and changes in a more or less haphazard manner. This is not surprising as the fluid dealt with by a given blade during the revolution changes from air to water with a mixture of both fluids as the intermediate medium.

Mr. J. F. C. Conn, B.Sc. (Associate-Member): In the opening paragraph of his paper Dr. Kempf appears to have omitted an important qualification to his statement that the influence of viscosity may be neglected for Reynolds' numbers exceeding $2 \cdot 5 \times 10^5$. The influence of camber ratio must be considered also and Mr. Allan's results show that the number quoted above applies only to sections having camber ratios greater than $0 \cdot 10$.

The author's findings might be restricted as being only applicable to the effect of viscosity when the latter is influenced by change of density. For propellers working near the surface, the fluid acted upon must decrease in density as air is drawn down into the slipstream. The author points out that such model propellers must be tested at Reynolds' numbers higher than those necessary for deep immersions. This is undoubtedly the case, but is not the explanation to be found in the reduced density? If the actual density could be estimated and used in the calculation, the Reynolds' number would necessarily be higher. There is no evidence supplied regarding the effect of viscosity only.

Mr. J. F. ALLAN, B.Sc. (Associate-Member): This paper of Dr. Kempf's is interesting in that it indicates a danger zone rather than defines it for each particular case. From a practical point of view it is important to remember that the experiments were carried out in open water, and there is no doubt the presence of a ship form in front of the screw would influence the results favourably, but how much it is impossible to say. A case in point may be of interest. A full-form twin-screw river steamer was tested in the Tank at Dumbarton, corresponding to 7 ft. 6 in. draught and 11 ft. draught, the propeller tips being flush in the former condition and the propulsive coefficient obtained was somewhat higher than in the loaded condition. In predicting the ship result a percentage was added to the power for the light condition to allow for the propellers being flush, but the actual trials showed efficiencies for the deep and light conditions in approximately the same relation to each other as was indicated by the Tank tests.

Dr. Kempf calls this a viscosity effect, but the major part must be a density effect due to the presence of air in the water acted on by the propeller. Viscosity may conceivably influence the commencement and extent of air drawing, but a study of the results lends weight to the conclusion that absolute size of propeller model is also an important matter. This is in agreement with our general experience.

It is noted with particular interest that Dr. Kempf finds that broader blade tips aggravate the troubles of air drawing due to insufficient immersion; this is in line with our own opinion but directly the reverse of that held by some shipowners to-day, who maintain that the modern type of screw with broad root definitely falls off badly in ballast conditions.

Dr. Ing. G. KEMPF (Member): Mr. Payne's remarks give a further confirmation from his Haslar experience of what we have found. Mr. Conn in his remarks mentions the influence of camber ratio which must be considered in fixing a Reynolds' number. The number $2 \cdot 5 \times 10^5$ given in my paper was found by considering the blade sections at the root, which may exceed even the camber ratio of $0 \cdot 10$.

It may be possible that the change of density will be mainly responsible for the effect which has been measured, but I venture to suggest that the influence of capillarity may also enter and will be responsible for the fact that Reynolds' law is not entirely sufficient to account for the results.

Mr. Allan in his remarks points out that the results given in my paper were carried out in open water and that the screw behind a ship would be less influenced. This is certainly true so far as the immersion of the screw is mostly improved by the stern wave of the ship.

When I spoke of the influence of viscosity I meant the influence of the kinematic viscosity ν in Reynolds' number $\frac{v\,l}{\nu}$ wherein density is included. But I agree that the forces will be influenced also directly by any change of density in the slipstream.

The commencement and extent of air drawing may be influenced not only by viscosity but also by capillary attraction, so we have probably three different laws of similarity governing the phenomena of a screw drawing air:—

$$\text{Froude's law } \frac{v}{\sqrt{g\,.\,l}} = c$$

$$\text{Reynolds' law } \frac{v\,.\,l}{\nu} = c$$

$$\text{Weber's law } \frac{v\,.\,\sqrt{l}}{\sqrt{k}} = c$$

where k is the coefficient of kinematic capillarity.

As one can follow only one law at a time, it looks as if absolute size has an influence, but this is only the technical and not the physical aspect.

It is quite intelligible that both observations on the influence of blade shape should be correct. A broad tip will disturb the surface to a greater extent than a narrow one, and a propeller blade with a broad root has a more rapid change of thrust and hence a higher circulation at the tip. Therefore air will be more readily drawn in by both these shapes than by a normal ogival form of blade.

To Illustrate Dr. G. Kempf's Paper on "The Influence of Viscosity on Thrust and Torque of a Propeller Working Near the Surface."

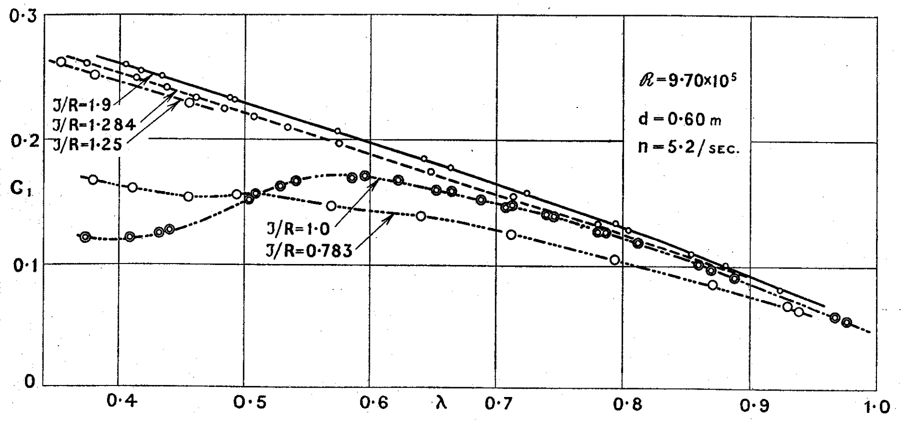


FIG. 1.—THRUST COEFFICIENT, PROPELLER No. 136.

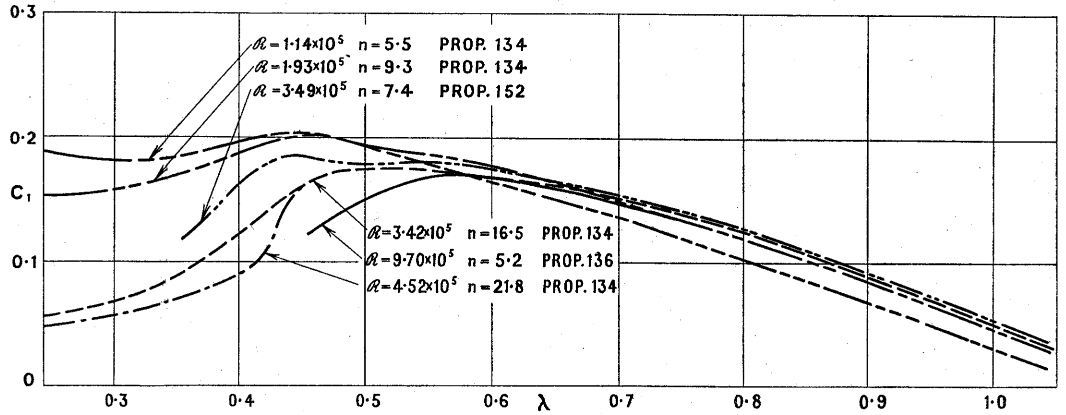


FIG. 4.—THRUST COEFFICIENTS.

Propeller No. 134: $d = 0{\cdot}2$ m., $z = 3$.
Propeller No. 136: $d = 0{\cdot}6$ m., $z = 3$.
Propeller No. 152: $d = 0{\cdot}3$ m., $z = 3$.

$\frac{\mathrm{I}}{\mathrm{R}} = 1{\cdot}0$

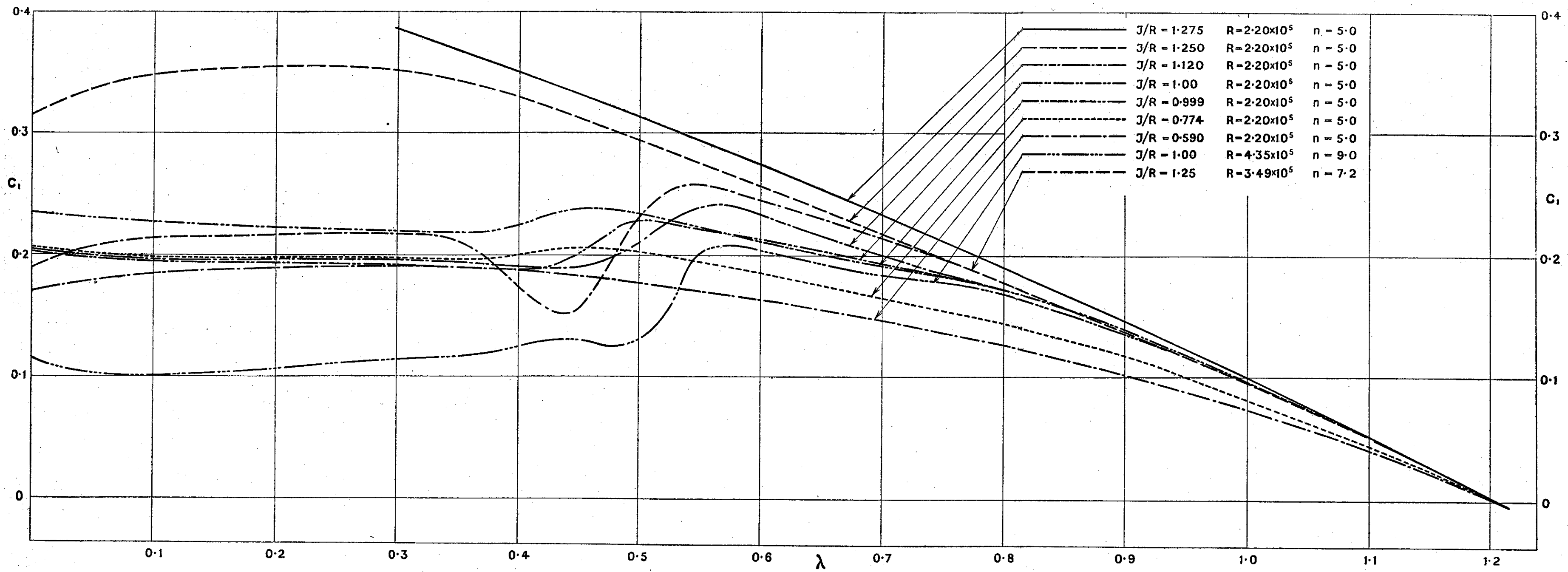


FIG. 2.—THRUST COEFFICIENT.

Propeller No. 993: $d = 0{\cdot}3048$ m., $z = 4$.

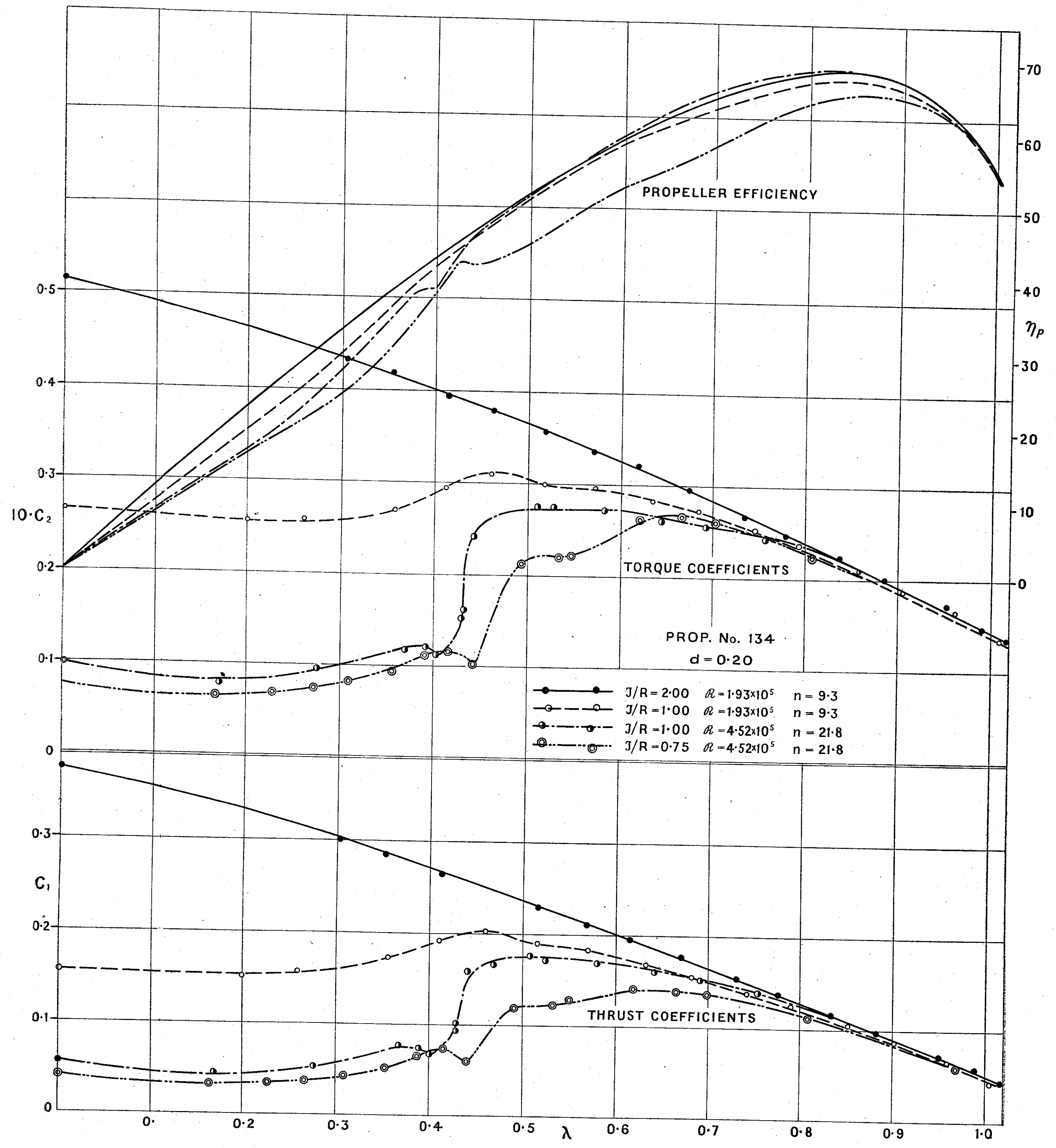


FIG. 3.

SKIN FRICTION CORRECTION.

By Professor L. BAIRSTOW, C.B.E., F.R.S.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 11, 1934, Sir EUSTACE D'EYNCOURT, Bart., K.C.B., D.Sc., F.R.S., Vice-President, in the Chair.]

A CONSIDERABLE literature about skin friction or boundary-layer theory exists both in English and in German and there is general agreement as to the best practicable method of dealing with a change from model to full scale. In the résumê now to be given no new ideas or facts are introduced.

The theorems of importance are generally developed in forms applicable to any fluid and in addition to experimental data from ship-model testing Tanks much information has been derived in aeronautical laboratories. This material, if suitably analysed, can be simply expressed; references may with advantage be made to a paper by Professor B. M. Jones in the Reports and Memoranda of the Aeronautical Research Committee entitled "Skin Friction and the Drag of Streamline Bodies" (December 1928) and to an article, "Turbulence and Skin Friction," by Th. von. Kármán, in the *Journal of the Aeronautical Sciences* (January 1934). Many other references are given at the end of this note.

No attempt will here be made to deal with details; instead, the endeavour will be to indicate salient considerations; for example: one of the main conclusions is that models tested in accordance with Froude's law of corresponding speeds should not be too small. A rough criterion, to be explained later, would state that the product of the speed of towing in feet per second and the length of the model in feet should not be appreciably less than 100 for tests in water. If this limit be provided for it is possible to use a formula connecting skin friction with wetted surface and speed which will give a reliable correction to model tests, whether on flat surfaces or on bodies usually referred to as "streamline." Neither of the above statements represents a theoretically established result, but together they reproduce empirically determined conclusions; theory is a powerful guide and help but is not sufficient.

In applications to naval architecture it is assumed that the resistances due to wave-making and skin friction are independent of each other; this is obviously an approximation, but one which need not be seriously questioned so long as the formulæ are not applied to cases in which the wetted surface is greatly changed by the waves. The problem is thus reduced to consideration of the resistance of a wholly submerged body, a condition which naturally arises in experiments in wind tunnels.

The typical experiment is the measurement of resistance as the relative speed of model and fluid is changed. The effective range can be increased by the use of a series of similar models. In all cases of ship and airship models, pipes, and for some other bodies not at present under consideration, the succession of events follows a well-recognized sequence. At very low speeds the resistance is calculable; at some speed often referred to as "critical" a change occurs to a type of flow which is described as "turbulent" as contrasted with the "laminar" calculable flow at slow speeds. The turbulence develops regularly and a

" transition " range of speed is required to pass from laminar to turbulent flow. Above the transition range a relatively high degree of certainty of experiment is achieved, but the transition stage cannot be trusted to occur in the same region in different tests.

Although the above statement is true it is not in its most general practical form. The first observation of the above sequence of changes of flow was made by Osborne Reynolds, who deduced a theorem of dynamical similarity for viscous fluids and verified it by experiments with water flowing through glass tubes. The theorem was generalized by the late Lord Rayleigh in a form now internationally accepted; it is that the resistance of an immersed body can be expressed in the form

$$F = \rho V^2 S f\left(\frac{V l}{\nu}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where F is the force, V the relative velocity of body and fluid, S the wetted surface, l the length of the body, and ν the kinematic coefficient of viscosity. Equation (1) is incomplete until the function f is known, and for practical purposes the final resort is to experiment; it is with a discussion of f that this résumé is concerned.

The value of equation (1) is in its suggestiveness; it is usually rewritten as

$$k_F \equiv \frac{F}{\rho V^2 S} = f\left(\frac{V l}{\nu}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and it then clearly implies that a single curve can be drawn connecting k_F and $\frac{V l}{\nu}$; the implication is not confined to a single body but extends to an unlimited series of geometrically similar bodies. The quantity $\frac{V l}{\nu}$ is so important that it has received an international name, " Reynolds' number," and is denoted by R (sometimes R N).

k_F and R are non-dimensional quantities and have the same numerical value no matter what the system of units so long as the system is dynamically self-consistent. There is an arbitrary difference of 2 : 1 between the American and German values of k_F and the English one because those countries write equation (1) as $F = \frac{1}{2}\rho V^2 S f\left(\frac{V l}{\nu}\right)$.

The units in England are the lb. for force, foot for length, and second for time. In these units, ν is equal to $1 \cdot 2 \times 10^{-5}$ for water at ordinary room temperatures; for air, ν is roughly 13 times as great.

The form of f for smooth flat plates is given approximately in the table on opposite page.

Column 2 of Table I. is prepared from a theoretical formula by Blasius for laminar flow; the equation solved is an approximate one due to Prandtl which is applicable if the Reynolds' number is large and if turbulence is absent. The final column has less solid theoretical foundation and it is perhaps sounder to regard column 4 as empirical. The transition figures of column 3 represent Gebers' results approximately; other experiments give different transition values. Column 2 is calculated from the formula $k_F = 0 \cdot 67\, R^{-\frac{1}{2}}$ and represents the resistance on small models, or, as we shall conclude later, on the front part of a model or ship. The last column contains figures in accordance with the formula $k_F = 0 \cdot 019\, R^{-0 \cdot 15}$; variations in the type of formula occur in the different papers referred to, some of them with a partial theoretical foundation. In effect, column 4 is a statement of experimental results and as such is to be regarded as subject to experimental error. The tests refer to flat plates or streamline bodies with smooth surfaces—not necessarily polished, but such as would correspond with a dull paint or a drawn tube.

Table I. shows the beginning of transitional flow at $R = 10^5$, where the resistance coefficient k_F is $0 \cdot 00212$ and equal to that in laminar flow and much less than the value of $0 \cdot 00338$ associated with this Reynolds' number in turbulent flow. At $R = 10^6$ the resistance coefficient for the transitional flow is above that for laminar flow but below that for fully developed turbulence. At $R = 10^8$ comparison of columns 3 and 4 shows that full turbulence has been developed.

Evidence shows that the starting and ending of the transition stage is very variable with experimental conditions; they vary with the roughness of the wetted surface and with the steadiness of the air or water. Such excrescences as the edge of a lapped joint produce early transition. On the other hand, the conditions defined as turbulent are not sensitive to surface conditions or steadiness of the stream, and one of the experimental methods of attack is to encourage turbulence by making small protuberances in the forward part of a model.

In making a test of a ship model it is clearly desirable to avoid this uncertain transition

TABLE I.

Column 1. Reynolds' Number, R.	k_F		
	Column 2. Laminar Flow.	Column 3. Transitional Flow.	Column 4. Turbulent Flow.
10^4	0·00670	—	—
$10^{4\cdot5}$	0·00377	—	—
10^5	0·00212	0·00212	0·00338
$10^{5\cdot5}$	0·00119	0·00119	0·00284
10^6	0·00067	0·00150	0·00239
$10^{6\cdot5}$	—	0·00155	0·00201
10^7	—	0·00145	0·00169
$10^{7\cdot5}$	—	0·00130	0·00142
10^8	—	0·00120	0·00120
$10^{8\cdot5}$	—	—	0·00101

stage and to choose R so great that flow is mainly turbulent. To convert this statement into more appreciable terms, consider experiments in water at 15° C. where $\nu = 1\cdot2 \times 10^{-5}$; for $R = 10^6$ we then have $V\,l = 12$ ft.^{2}/sec. and for $R = 10^7$ we have $V\,l = 120$ ft.^{2}/sec. If the towing speed is 10 ft. per sec. the lower value of R gives a length of 1·2 ft. and the higher a length of 12 ft.

Some explanation has now been given of the meaning of the statement that too small a model should not be used if full accuracy of prediction on the full scale is to be obtained. It would appear that for the standard surface used on the boards tested in the Froude tank of the National Physical Laboratory $R = 10^6$ is sufficient, but that with the smoother surface of wax models a higher value of $R = 10^{6\cdot5}$ or 10^7 is advisable. When this condition has been satisfied, correction from model to full scale can be made by using the formula $k_F = 0\cdot019\,R^{-1\cdot5}$. On the other hand, if the model be too small, errors of the order of 50 per cent. of the total frictional resistance are not improbable.

Streamline Forms.

For smooth surfaces the rather surprising result of a number of tests of streamline bodies in air has been to show that Table I. applies without modification; the principle of dynamical similarity leaves no doubt that the same is true for motion through water.

It has already been noted that different formulæ are used for resistance in turbulent flow; it is worth mention that the absolute values of k_F need not be in complete agreement whilst giving a good estimate of the difference between model and full scale. Both the absolute value and the difference are used in separating wave resistance from skin friction and changing the scale, but the effective correction is more dependent on the difference than on the absolute value.

Roughness.

When the roughness of a surface has appreciable effects on resistance the appropriate procedure is far less clear. Perhaps the chief difficulty is the absence of a satisfactory definition of roughness, but a further complication arises from the expectation that roughness in the middle and rear sections of a streamline body is less detrimental than in the fore-part.

For surfaces of uniform roughness, the curves of measured resistance are of the same type as for smooth surfaces, i.e. there is a stage of laminar flow followed by a transition stage and later by turbulence. The rougher the surface, the smaller the value of R at which transition occurs. In addition, the type of formula for turbulent flow changes; instead of $k_F = 0 \cdot 019\, R^{-0 \cdot 15}$ for smooth surface an expression with a higher coefficient and a lower index is required. In other words, the frictional resistance coefficient increases with roughness and tends to become independent of Reynolds' number.

To carry out an investigation as fully as that for smooth surfaces it would be necessary to reproduce the roughness of a ship to scale and to test a submerged model over a large range of Reynolds' number. As this would involve a series of measurements of almost impossible complexity, efforts have been made to analyse simpler experiments in helpful form. It cannot be said that the final conclusions have great precision.

A striking series of tests has been made by Nikuradse on rough pipes. Different roughnesses were produced by varnishing the inside surface of circular pipes and whilst the varnish was still sticky covering it with sand; a final coat of varnish held the grains fast against erosion. For the finest sand the grain diameter was about $\frac{1}{1000}$th of the pipes' diameter; the coarsest sand was $\frac{1}{30}$th of the pipes' diameter. The resistance coefficient at large values of R was increased by rather more than 50 per cent. The results are not easy to interpret in terms of ship models, for the reason that new fluid was not being continually encountered and a new type of argument is introduced. Skin friction is produced by the retardation of fluid near the moving surface; it is found by experiment and by calculation that all the retardation occurs in a comparatively thin layer called the "boundary layer." If δ is the thickness of this layer and x the distance from the entering edge of the body then

$$\delta \simeq x \frac{5 \cdot 5}{\sqrt{R}}$$

for laminar flow according to the Blasius formula. The limit of the layer is not very definite, and perhaps it would be better to say that at $\Delta = x \dfrac{1 \cdot 5}{\sqrt{R}}$ the velocity is half that in the free stream. For turbulent flow von Kármán gives the formula

$$\delta \simeq 0 \cdot 54\, x \sqrt{k_F}$$

Taking the latter as of more immediate interest we find that 10 ft. from the bow of a ship moving at 30 ft./sec. the value of δ is $0 \cdot 21$ ft. or about $2\frac{1}{2}$ in. At half an inch from the surface the speed would still be nearly 20 ft./sec., and so it would be expected that excrescences half an inch high would have a marked effect on the ship resistance. Further forward smaller roughness would matter, whilst towards the stern it is probable that greater protuberances could be permitted without serious addition to the resistance.

Finally, a series of experiments in air will be used to illustrate the effects discussed above. A streamline model of circular cross-section of fineness ratio 3 : 1 was tested at speeds ranging from 30 ft./sec. to 90 ft./sec. The overall length was 70 in. and the maximum diameter 23·2 in.; the surface was smooth with a good beeswax polish. As part of the experiment, threads were placed in rings at axial distances from the nose of 3 in. and 6 in.; the diameters of the rings were 10 in. and 14 in. respectively, and the threads 0·025 in. diameter. The following table gives some of the results:—

TABLE II.

Column 1. Wind Speed, Ft./Sec.	k_F			
	Column 2. Bare Body.	Column 3. With Thread at 3 in.	Column 4. With Thread at 6 in.	Column 5. With Threads at 3 in. and 6 in.
30	0·00195	0·00276	0·00299	0·00315
40	0·00193	0·00291	0·00290	0·00288
50	0·00200	0·00279	0·00279	0·00282
60	0·00202	0·00271	0·00271	0·00274
70	0·00202	0·00264	0·00263	0·00265
80	0·00205	0·00258	0·00257	0·00260
90	0·00207	0·00252	0·00250	0·00254

It will be seen from column 2 that the bare body is giving resistance in the transitional stage over the whole range of test speeds. At the lower speed of 30 ft./sec. a thread at 3 in. from the nose increased the resistance coefficient from 0·00195 to 0·00276 and even so small an excrescence introduced marked turbulence; the addition of a second excrescence brought k_F to 0·00315 and the flow to full turbulence. The thread 0·025 in. diameter therefore projects into the moving stream at 6 in. from the nose at low values of Reynolds' number.

At the other end of the table the conditions are rather different; strings at either 3 in. or 6 in. produced full turbulence.

The results suggest a possible method of test in small tanks where it is difficult to attain a sufficiently high value of Reynolds' number by direct means.

I have found it difficult to compress into this note the important considerations relating to skin friction and am fully aware of its incompleteness. For those interested a list of papers is attached and reference to those papers will itself extend the references.

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L. BAIRSTOW: Journ. Roy. Aero. Soc., Vol. XXIX., January 1925.
E. OWER and C. T. HUTTON: Investigation of the boundary layers and the drags of two streamline bodies R. & M., No. 1271, Adv. Comm. Aero.
L. F. G. SIMMONS: Exp. relating to the flow in the boundary layer of an airship model, R. & M., No. 1268.
Photographic records of flow in the boundary layer, R. & M., No. 1335.
A. FAGE: An experimental determination of the intensity of friction on the surface of an aerofoil, R. & M., No. 1315.

DISCUSSION.

Professor VON KÁRMÁN: It was very kind of Professor Bairstow to refer to my article on "Turbulence and Skin Friction," published in the *Journal of Aeronautical Sciences*. Dr. A. F. Zahm, whose name, I think, is well known in this connection, compiled a rather full bibliography on the subject containing, I think, something like six hundred items on skin friction and boundary layers, comprising both theoretical and experimental papers. Dr. Zahm is now in charge of the aeronautical library at the Congress in Washington, and I think he is going to send the bibliography, a booklet of about fifty pages, to everybody who is interested in this matter.

Dr. G. KEMPF (Member): Professor Bairstow has given a comprehensive statement of the skin friction theory up to date, and we have now reached the position that we have points measured up to $\log \frac{VL}{\nu} \sim 6$, and we have seen on the diagram he has shown us that aeroplanes are running at $\log \frac{VL}{\nu} \sim 7 \cdot 5$, whilst ships are running on yet higher Reynolds' numbers, say $\log \frac{VL}{\nu} \sim 9$. One might suggest that as the aeronautical people have had some help from tests made by Froude, so now we shipbuilders may look for some help from our aeronautical friends. They might test aeroplanes with certain degrees of roughness and certain distributions of rivets under their planes, and find out the resistance for these roughnesses on big aeroplanes. It would not involve very great expense to make a horizontal bottom under an aeroplane, and they could help us by testing that at very high speed, because even in our Hamburg Tank with our long pontoon we can only test up to a speed of 16 knots.

But there may be another suggestion; as something has been said about the importance of rivets on ships, one might perhaps, on the fore-end of the ship only, ensure a certain roughness by a distribution of rivets of different diameters in order to see what influence this roughness will have. The shipowner could remove them afterwards. It is only an idea which occurred to me during the reading of the paper.

Mr. M. P. PAYNE (Member): The "salient considerations," to quote the author's words, on the subject of skin friction, which he has brought to our notice, are presented in an interesting and concise manner and can clearly claim to be of the greatest importance. On the practical side, I was struck by the criterion suggested by the author to govern the minimum size and speed of a model for accurate measurement of its resistance in water. The author describes it as a rough criterion, but from the method of derivation it is possible that he intends it to be a counsel of perfection for those to attain who can.

In the paper it is suggested that the product of the speed of towing in feet per second and the length of the model in feet should not be appreciably less than 100 for tests in water. In our work at Haslar we find that a minimum figure of less than one-half that quoted above is adequate for reliable and consistent results, for both surface and submerged models. Actually we find that a model 16 ft. long can be towed at a minimum speed of $2\frac{1}{2}$ ft. per sec. and will then give satisfactory results. The maximum speed of towing, corresponding to the full speed of the ship, would naturally be in excess of this figure, and at such speeds the product of the length and speed is generally appreciably greater than the criterion suggested by the author.

The importance of an inferior limit to the size of models to avoid abnormal viscous effects has, of course, been appreciated by all model experimenters since William Froude's early experiments on the river Dart in 1867. In these experiments Froude measured the resistance of three similar models of lengths 3 ft., 6 ft., and 12 ft. respectively. I might say that those larger models are in the near future to be placed in the Maritime Museum at Greenwich. They have been stored at Haslar in the meantime.

Sir ARCHIBALD DENNY: Were they wooden models?

Mr. M. P. PAYNE: There were two of each size, and they were of different shapes, but the small ones have disappeared. The intermediate ones are of metal, and the large ones are of wood, painted of course.

Sir ARCHIBALD DENNY: The small ones were probably made of paper.

Mr. M. P. PAYNE: Probably they may have been. We have lost all trace of them. When William Froude compared the resistances of these models in accordance with the Law of Comparison he found that the results for the 3-ft. model were in general in excess of those for the 6-ft. model. He concludes—and I am quoting his actual words—"I think this is probably attributable to their being small enough to be within the range of viscosity." This point has been brought forward here as it was thought it might interest members as a remarkable example of the foresight of William Froude that a result of such fundamental importance was discovered at so early a date.

With reference to the author's investigation into the skin friction of rough surfaces on page 330, it would be of interest to know if he has considered Froude's experiments on roughened planks in this respect. Particulars are given of experiments in which the resistance coefficient of pipes at large Reynolds' numbers was increased by more than 50 per cent. by roughening the surface with sand. As the author points out, it is not easy to interpret such results for ship models, owing to the character of the flow in pipes. The plank experiments of Froude can, however, be used for a closer interpretation, and Froude found that the resistance when the planks were covered with sand was some 75 to 125 per cent. greater than when varnished; the proportion varies according to the degree of coarseness of the sand applied. In recent years the effect of roughening the surface of a ship's model with fine sawdust applied on wet varnish has been tried at Haslar. Incidentally, I might say I make some reference to this in the paper that I am reading to-morrow.

The results showed that the skin friction was more than doubled by this means. Such results well illustrate that large increases in resistance will be experienced in ships if the surface is not reasonably smooth. Trials that have been made on ships for different periods of time out of dock show that, due to the fouling of the ship's bottom, the skin friction increases at the rate of about $\frac{1}{4}$ per cent. per day in temperate waters, and at about twice that rate in tropical zones.

As Sir Archibald Denny is Chairman to-day I might mention that I believe at the Skin Friction Committee meeting the late Mr. Mumford, of your staff, said that your experience gave a higher figure.

Sir ARCHIBALD DENNY: Yes, $\frac{1}{2}$ per cent.

Mr. M. P. PAYNE: My figure applies to Service ships in home waters.

Professor Dr. Ing. F. HORN (Member): I made some remarks yesterday about the tests which, following a proposal of the Committee for Resistance and Propulsion of the Schiffbautechnische Gesellschaft in Germany, we are about to make in order to arrive at some conclusions relating to the frictional form effect of a ship body and, secondly, about the influence of roughness. Perhaps it may be of some interest to you if I enlarge on what I said yesterday by adding a few remarks that relate very closely to the paper of Professor Bairstow.

In the diagram (Fig. A, page 335) the abscissæ values represent Reynolds' numbers and the ordinates the resistance coefficients $\zeta_R = \frac{R}{\rho v^2/2\,S}$, R being the total resistance and S the wetted surface. Suppose a test of a ship model is made and you get the point P by the test for a special model speed corresponding to the speed of the ship in question. Which point P of the ship corresponds to the point P[1] of the model and along which curve must this latter point be transferred in order to reach the ship's point?

At first I put down the curve (1) of a smooth plank the running of which had been satisfactorily determined up to very high Reynolds' numbers in recent years, especially by the work of von Kårmån and the Göttingen school. This curve represents pure frictional resistance. Curve (2) of the pure frictional resistance of the smooth ship body will run somewhat higher because of the well-known circumstances which cause differences in the water velocities along the body as compared with those along the plank and therefore alter the frictional resistance. It seems possible that this curve can be rather closely calculated by means of a rotative body with the same sectional areas as the ship's body. But we do not know anything definite about this question. This is the first point I mentioned yesterday which we are now trying to examine by new tests with several different families of models of ship forms with very great model lengths up to 12 m. In this way we hope

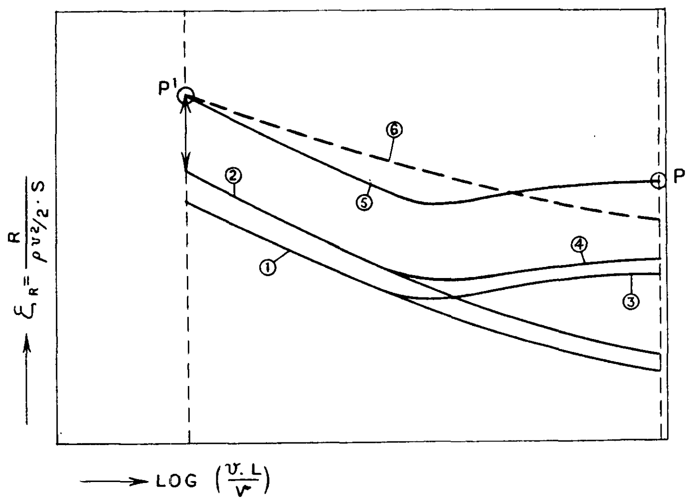


FIG. A.

to get, for these different types of ships, some points on the frictional curves of the smooth bodies and to see how these curves compare with that of the smooth plank.

The second point is that of roughness. With a plank this influence, according to the recent systematic tests in the Göttingen Institute, mentioned also by Professor Bairstow, may be represented by curve (3). This curve is based on the assumption that the degree of roughness increases with the length of the plank, until at the ship length and the corresponding Reynolds' number the real degree of roughness that belongs to a ship's surface is reached. The numerical values of the increase of frictional resistance of planks due to a roughness of structure like that of a ship's surface are at present being examined by special systematic tests in Göttingen. The same increase of frictional resistance due to roughness added to the ship's frictional resistance curve (2) gives the curve (4). Now the distance at the model Reynolds' number between curve (4)—which at very low Reynolds' numbers is identical with (2)—and the model point P′ gives the so-called residuary resistance. This distance, kept constant over the whole range of Reynolds' numbers, gives the transition curve (5) from the model point P′ to the ship's point P under the same assumption as before, viz. that the dimensions of roughness increase according to the law of geometrical similitude. The main practical question will be where this point P will lie in comparison with the Froude curve (6).

In conclusion, we want to know in the first place the curves (2) and (3) and then we shall try to examine these curves by means of those tests which I mentioned yesterday.

Professor L. Bairstow, C.B.E., F.R.S.: In reply to Dr. Kempf, I should like to say that very considerable importance is attached in aeronautics to tests at correct Reynolds' numbers. I believe members of this Conference are visiting Teddington to-morrow, and I hope some of you will take the opportunity of looking at the compressed air tunnel there. It is possible in that tunnel to test scale models at pressures twenty-five times that of the normal atmosphere and obtain a Reynolds' number equal to that on the full scale. The compressed air tunnel has not been working very long, but at the same time comparative experiments have been made on the full scale, employing a special aeroplane to measure lift and resistance. It is very encouraging to find that the agreement between model and full scale tests is very good. I hope that in due course, when the tunnel gets a little more free, it will be possible to take up Dr. Kempf's suggestion and measure the effect of rivet heads and roughness at Reynolds' numbers approaching those desired for naval architecture.

Mr. Payne suggested that at Haslar the minimum test value of $V L = 50$ was satisfactory. I am not surprised and am not anxious to insist on 100 as being absolutely essential. If it could be reached, however, in any experiment, I should try to get it. A method is suggested in my paper by which smaller models might be made to give reliable results, i.e. by making a small roughness near the front of the model and thereby inducing early turbulence in the boundary layer.

I am very much interested in Dr. Horn's proposals for testing a series of similar models in order to investigate the resistance more fully. It is clearly a much more elaborate experiment than can be covered in aeronautics, because we have no wave-making complications. On the other hand, I feel that if the models give resistance curves of the type shown by Dr. Horn it will be a little dangerous to argue from model to full scale: it seems to me important to avoid the transition region since this is susceptible to small changes in experimental conditions.

The Chairman (Sir Eustace d'Eyncourt, Bart., K.C.B., D.Sc., F.R.S., Vice-President): We are much indebted to Professor Bairstow for his most interesting paper. It shows what a close connection there is between the air and the water in the investigation of resistance of bodies moving in a fluid. Therefore, it is particularly valuable that he has given the benefit of his research work to the Institution of Naval Architects, as we are deeply interested in all these matters. I would like to move a very hearty vote of thanks to Professor Bairstow.

THE DESIGN OF SCREW PROPELLERS, WITH SPECIAL REFERENCE TO THE SINGLE-SCREW SHIP.

By G. S. Baker, Esq., O.B.E. (Member of Council), of the William Froude Laboratory.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 11, 1934, Sir William Berry, K.C.B., Vice-President, in the Chair.]

Summary.

In the last few years the author, in association with Mr. A. W. Riddle, has read two papers before the Institution, giving the results of experiments with a series of propellers in open water. These screws were designed for the propulsion of a single-screw hull. They were all of the same diameter, and had the same pitch over the outer parts of the blades, but differed in blade outline, section, and pitch at blade root.

Put broadly, these open-water experiments showed that the efficiency when absorbing the same power at the same revolutions and speed could be varied by the above changes in blade design within the following limits:—

	Lowest Efficiency.	Highest Efficiency.
At or about maximum efficiency, i.e. moderate slip ratios ..	0·61	0·685
At high slips and high loading	0·55	0·575

§ 2. Practically all these screws have now been tested behind a model hull, at two speeds —a few in two fore-and-aft positions—and the results show that, confining attention to only the three-, four-, and five-bladed screws, and keeping the same diameter and general dimensions, the propulsive efficiency of the hull and propeller can be varied from 0·68 up to 0·755 by varying the shape and design of the propeller blades.

§ 3. Broad results are given in §§ 12–16, and some general theoretical notes having a bearing on the experimental work are given in §§ 17–24, to assist in the understanding of the details which follow. Separate sections and tables are devoted to the effects of:—

Number of blades (§§ 25–26).
Rake and outline of blades (§§ 27–30).
Pitch variation (§§ 31–32).
Cross-section of blades (§§ 33–38).

Apparatus and Mode of Experiments.

§ 4. All of the tests mentioned in the paper were made with an internally propelled model hull, i.e. the screw was driven from inboard, and actually propelled the model hull.

As the apparatus and method of testing adopted at the William Froude Laboratory differ materially from the apparatus and methods used on the Continent and in America, a brief account of these is given. An outline of the apparatus is given in Fig. 1 (Plate XXXV.). The general principles embodied in it are:—

(*a*) The measuring apparatus is a self-contained unit on one rigid base, which can be placed in any model.
(*b*) The propeller and shaft tube is a separate unit, connected to the former by adjustable links.
(*c*) Everything is self-recording, and therefore free from observation error.

The propeller is carried on a shaft which is supported in a tube kept full of oil. This shaft is connected to the thrust-measuring gear through two universal joints, and thence through a sliding clutch and the torque meter to the electric motor driving it. The universal joints and the adjustable length of shaft between them serve two purposes. They allow the model to change shape a little without involving any friction on the apparatus, and the propeller can be brought to its correct fore-and-aft position, after all the parts are secured in the hull. The gear is always erected in the model so that, from screw to measuring meters, the shafting is a practically straight and horizontal line.

At the fore-end of the apparatus is the transmission torque meter. Thrust is measured at the after-end of the apparatus by weighing it against weights and a spring. For this purpose the shaft abaft the torque gear is allowed to move fore and aft a distance of 3/32 in. each way. The balance weights on the thrust arm are always arranged to bring the screw very close to its correct fore-and-aft position before taking a record. Revolutions and time are recorded by the usual magnet pens, and speed of advance is taken from a distance time record.

§ 5. The mode of testing a propeller behind a hull is to run the hull at a fixed speed of advance for at least ten experiments. The revolutions in the different experiments vary from a little above those required to propel the model to well below this figure. The screw therefore sometimes over-propels and at other times under-propels the model. The difference between the thrust of the screw and the resistance of the hull is supplied through, and recorded by, the main dynamometer on the Tank travelling carriage, which keeps the model at its steady speed of advance. This "difference" is afterwards plotted to a base of revolutions to find the correct revolutions for self-propulsion, and the quasi-propulsive coefficient and other factors are worked out *at this self-propulsion point for model.*

Most mercantile ships work at or around one speed, but if data for more than one speed are required, the experiments are repeated at this second speed—in this research two speeds were tried. The basic ideas in this method of testing are:—

(1) That ships are usually required to travel to a certain time schedule and over a very restricted speed range.
(2) It is better to know the effect of varying slip, as in our tests, rather than varying speed, since the latter for a good hull (with resistance varying as speed squared) simply means varying Reynolds number.
(3) The self-propulsion point of the model is a *definite* and visible thing, free from personal or theoretical influence, and is not very far removed from the propulsion point of the ship in average weather at sea.

Model Hull.

§ 6. The dimensions and general particulars of the model hull used in the tests are given in Table I., which also gives the dimensions and particulars of a similar ship 400 ft. between perpendiculars. The lines of the model are given in Fig. 2. This comparatively

TABLE I.

DIMENSIONS AND PARTICULARS OF HULL.

Item.	Model 1119.	400-ft. Ship.
Length B.P. (feet)	24	400
Breadth (feet)	3·2	53·3
Draught (feet)	1·32	22·0
Displacement { Lbs. for model	4,600	—
Displacement { Tons for ship	—	9,750
Prismatic coefficient { Total	0·744	0·744
Prismatic coefficient { After-body	0·732	0·732
Trim	Nil	Nil
Propeller diameter (feet)	1·0	16·67
Centre of boss forward of A.P. (normal position) (feet)	0·275	4·58
Centre of boss above keel (feet)	0·57	9·46
$\frac{\text{Immersion of blade tips}}{\text{Diameter}}$	0·25	0·25
Effective horse-power { 12 knots		1,285
Effective horse-power { 14 knots		2,175
Shaft horse-power at tail end, average weather at sea with B 34 $= \frac{\text{E.H.P.} \times 1\cdot27}{\psi}$ { 12 knots		2,160
Shaft horse-power at tail end, average weather at sea with B 34 $= \frac{\text{E.H.P.} \times 1\cdot27}{\psi}$ { 14 knots		3,750

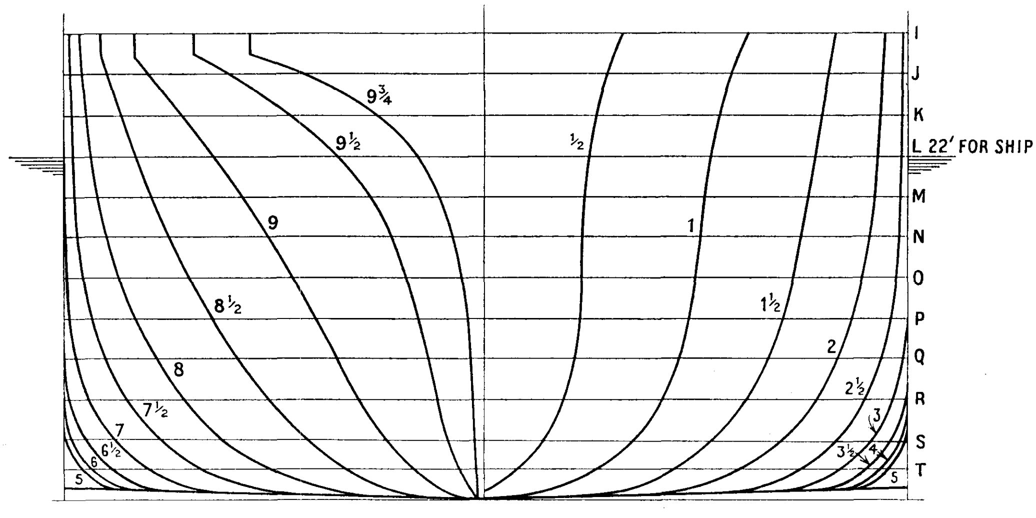


FIG. 2.—BODY-PLAN, MODEL 1119.

large model was chosen with a view to ensuring that as far as possible there should be no "scale" effect present. Results obtained with screws of different diameter in the National and other Tanks support the assumption that the size of screw adopted was sufficient for this purpose. Fig. 3 shows a propeller of the standard rake (15°) in its normal position behind the hull, together with the clearances between propeller and body-post in the model. With all of the screws, the fore-end of the boss was kept in the same position relative to the hull, so that when the blade root was widened the boss was lengthened by extension aft. With a few of the screws the fore-and-aft gap between screw and hull was increased by moving the propeller bodily aft as mentioned in § 30.

§ 7. Taken as a 400-ft. ship, the working speed range of this hull would be about 12 knots, with an upper limit of 14 knots, and the tests were therefore made at speeds corresponding to these. All the tests were at one draught corresponding to loaded condition, with

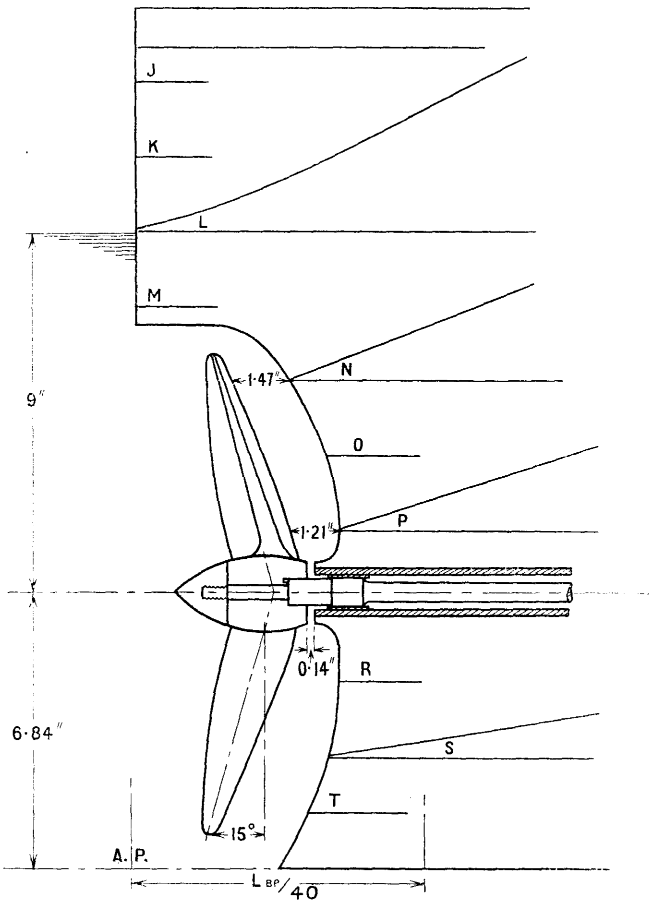


FIG. 3.

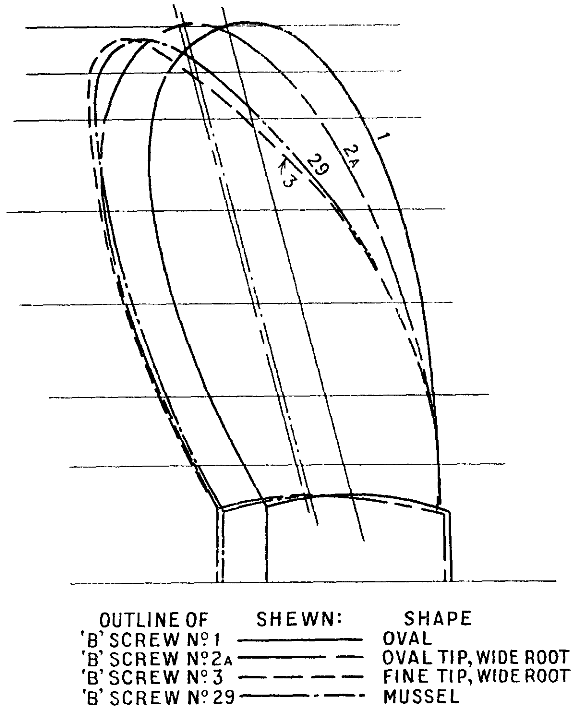


FIG. 4.

no trim. The immersion of the propeller centre for this condition is given in Table I. The total number of propellers tested is thirty-two, these being described in the various tables. Typical blade outlines defining some of the screw blades are given in Fig. 4.

MODE OF ANALYSIS AND PRESENTATION OF RESULTS.

§ 8. Froude's standard method of analysis has been adopted in presenting the results obtained. In this analysis the quasi-propulsive efficiency is broken up into separate factors as follows:—

$$\psi = (1 + w)(1 - t)\,\eta = (1 + w)(1 - t)\,\eta_1 \left(\frac{\eta}{\eta_1}\right) \quad . \quad . \quad . \quad (1)$$

these terms being defined in the Appendix.

The propulsive efficiency ψ and thrust deduction fraction t are obtained directly from the tests. The wake fraction w is obtained from the thrust curves of these screws in open water which were used in obtaining the data given in our earlier papers.

§ 9. It is extremely difficult to present the details of so many different propellers in such a way that their characteristics and the special variation embodied in each can be easily seen. Each screw has been defined, first by its blade outline, then by particulars of two typical sections of the blades, one at the root (excluding any fillets), and the other at 0·3 D. This latter was always representative of the blade section from half to full radius. For each of these sections the camber ratios and blade widths are given in the international nomenclature (see Appendix). The rake is given in two forms, first in degrees of the strickling blade (the international method), and second the rake plus skew of the leading edge, measured in a transverse plane from the leading edge at the boss to the extreme blade tips—with unsymmetrical blade outlines this figure is of some importance when fitting a screw to a hull. The shape of a blade section is defined by its A, W, and T value. The meaning of these terms was given in our 1932 paper, and is repeated in the Appendix. Some scheme of this kind is essential in dealing with blade sections, as the single word "aerofoil" is too indefinite and loose to be useful.

§ 10. These particulars of the various screws are given in Tables III., IV., VI., VII., and VIII. In each table there are grouped all the screws having a given type of variation running through them. As many screws contain more than one variant, these will appear in more than one table, but in comparison with different screws in the different tables.

With each screw are given the propulsive efficiency, the wake fraction (Froude), and the thrust deduction fraction at the two speeds tested. The revolutions required to propel a 400-ft. ship are also given. These revolutions correspond with those for the self-propulsion of the model—the actual revolutions for the ship would be some 3 per cent. to 4 per cent. lower than these.

§ 11. As this work has taken over three years to carry out, it has been necessary to relate the work done at one time with that at another. For this reason certain propellers were always run on each occasion when tests were made, so that some of the screws have been tested a number of times. Small variations were obtained in these retests, but not enough to alter the comparative results of the various screws, unless the difference was less than 1 per cent., and the data in the tables are believed to be a correct version of the effects of the variations tried.

Broad Results.

§ 12. There are two broad aspects which concern the designer in these results: first the variation in propulsive efficiency, and second the variation of revolutions. The maximum change in propulsive efficiency, ignoring the six-bladed screws, which are rather abnormal, is 10 per cent. Comparing the best screw (B 34) with the usual circular-back four-bladed screw (B 8), the improvement is from 0·705 to 0·755, or 7 per cent. at 12 knots. To show that some care is wanted in the choice of shape, attention is drawn to the fact that quite a few of the screws with aerofoil backs give no better results than B 8.

§ 13. The effect of blade shape upon revolutions is partially shrouded in, and not altogether separable from, the effect of reducing the pitch of the blades near the boss. But taking the general effect, the revolutions at 12 knots vary from 80·5 to 86·0 with four-bladed screws. The type of screw which gives the best propulsive coefficient shows an increase varying from 2 per cent. to 5 per cent. (Table VIII.) compared with the revolutions for a circular-back screw (B 8), the lower figure being that adopted for calculation work at the Tank.

§ 14. The efficiency in open water at the thrust and revolutions corresponding to the self-propulsion point of the model is given for each screw just below the propulsive coefficient, and in the next line is given the ratio of these two terms. This ratio shows that on the average the propulsive coefficient is about 20 per cent. greater than the screw efficiency in open water. With four-bladed screws it can rise as high as 27 per cent. and fall to 18 per cent. But with a three-bladed screw having narrow circular-back blades of high camber ratio (B 13A, Table III.) it may rise to 29 per cent., and with a six-bladed screw with narrow blades with very full leading edge (B 18, Table III.) it falls to 14 per cent.

§ 15. The quasi-propulsive coefficient ψ can be broken up by Froude's method into its component parts: hull efficiency (h), open screw efficiency, and relative rotative efficiency $\left(\frac{\eta}{\eta_1}\right)$. In such an analysis the thrust deduction fraction t can be accurately deduced from the tests—it is merely the difference between two measured forces expressed in terms of one of them. But the evaluation of a true mean wake, when this wake is of the order in these tests, is not nearly so determinate. It is tacitly assumed in this part of the analysis that the intake velocity ratio is the same for all the screws, and we know from analytical work this is not very accurate (see Fig. 5, Plate XXXV.). Secondly, with high and variable wakes the curves of thrust (open water and behind model) are of such a kind as render wake evaluation rather inaccurate. Since the efficiency of the screw behind the hull (η)

TABLE II.

VARIATION OF THRUST DEDUCTION FRACTION t WITH VARYING PROPELLER THRUST. SCREWS B 1 AND B 8.

C_T constant	0·55	0·35	0·15	0·10	0·05	0·03
T_C constant	0·22	0·185	0·115	0·09	0·055	0·035
t { without rudder and fin	0·21	0·22	0·23	0·26	0·35	0·6
t { with rudder and fin	0·23	0·25	0·31	0·36	0·64	1·0

Note.—For the last two columns the t values are only approximate.

is deduced from the overall efficiency by taking out the term $(1 + w)(1 - t)$ (see Appendix), any inaccuracy in w will be reflected by an equal and opposite inaccuracy in this " behind " efficiency, and therefore in relative rotative efficiency. The tables should be approached with this in mind, and it follows that small differences in w and therefore in relative rotative efficiency $\left(\frac{\eta}{\eta_1}\right)$ can be ignored, unless they are supported by the general trend in each table.

§ 16. The propulsive coefficients, etc., in the tables are for a condition a little worse than " average weather at sea." Three of the screws have been tested under much lighter loads—in two cases down to zero thrust—to determine whether variation of load, which might be due to variation of weather or of displacement, made any difference to the broad result. With screws B 1 and B 8 (Table VII.) the propulsive coefficient increased 4 per cent. when the shaft horse-power dropped 20 per cent., the change being much the same on both screws. The thrust deduction fraction remained very steady, but increased slowly with large reductions in the screw thrust. This was tried both with and without a rudder and fin, with the same general result, but a more rapid change in t occurred with the fin and rudder present. These results are given in Table II. The resistance of the rudder and fin are included in the thrust augment with these fittings on the hull. At high thrusts it is shrouded and sometimes overbalanced by the improvement in flow induced by the fin, but at zero thrust it is a clear addition to the total resistance.

On general grounds it can be seen that when the propulsive efficiency is zero, t must be unity, and when the thrust of the screw is zero, t will be infinite.

TABLE III.

Effect of Number of Blades.

Screw No. B	11		8		12		13		18		13A		18		4	
Number of blades	3		4		5		6		6		3		3		4	
Blade outline	Oval		Oval		Oval		Oval		Oval		Oval		Oval		Oval	
Type of section at root ..	C		C		C		C		A·25 W·2		C		A·37 W·05		A·37 W·05	
Type of section at 0·3 D ..	C		C		C		C		A·25 W·05		C		A·37 W·05		A·37 W·05	
Camber ratio at root ..	20·0		27·0		33·6		40·5		40·5		40·5		20·0		27·0	
Camber ratio at 0·3 D ..	6·7		8·8		11·0		13·3		13·3		13·3		6·7		8·8	
Blade width at root	21·4		16·0		12·9		10·7		10·7		10·7		21·4		16·0	
Blade width at 0·3 D ..	28·8		21·7		17·5		14·5		14·5		14·5		28·8		21·7	
Rake in degrees	15		15		15		15		15		15		15		15	
$\frac{\text{Rake + skew at tip}}{\text{D}}$	0·20		0·18		0·17		0·16		0·16		0·16		0·20		0·18	
Pitch reduction at root, per cent.	Nil		Nil		Nil		Nil		Nil		Nil		20·0		20·0	
Speed in knots	12	14	12	14	12	14	12	14	12	14	12	14	12	14	12	14
ψ	0·715	0·69	0·705	0·685	0·67	0·68	0·63	0·635	0·66	0·66	0·71	0·71	0·725	0·70	0·73	0·705
η_1 (open)	0·58	0·58	0·58	0·58	0·58	0·58	0·54	0·54	0·575	0·58	0·55	0·565	0·585	0·59	0·58	0·575
$\frac{\psi}{n_1}$	1·23	1·19	1·22	1·18	1·16	1·17	1·17	1·17	1·15	1·14	1·29	1·26	1·24	1·19	1·26	1·22
$\frac{\eta\text{ (behind)}}{\eta_1\text{ (open)}}$	1·09	1·05	1·06	1·05	0·99	1·03	0·97	1·02	1·00	1·05	1·05	1·08	1·07	1·03	1·09	1·07
h	1·13	1·13	1·15	1·12	1·17	1·14	1·21	1·15	1·15	1·09	1·23	1·17	1·16	1·15	1·15	1·15
t	0·215	0·210	0·210	0·210	0·215	0·215	0·215	0·215	0·215	0·225	0·215	0·210	0·210	0·210	0·215	0·215
w	0·44	0·43	0·46	0·44	0·49	0·45	0·54	0·47	0·47	0·41	0·58	0·48	0·47	0·46	0·47	0·46
N	84·8	101	80·5	95·8	77·5	91·5	78·0	92·0	76·5	91·5	86·5	102	84·8	101	81·5	97·5

Within all working limits of slip, all these changes were very much the same with screw B 29 (Table VI.), and it can be said, therefore, that the general conclusions formed from these tests will apply over a fairly wide slip range.

Theoretical Considerations.

§ 17. In the simple theory of the action of a screw propeller we are concerned with two equations:—

The screw efficiency at any radius r, $$\eta = \left(\frac{1 - a_r}{1 + a_t}\right) \frac{\tan \phi_2}{\tan (\phi_2 + \beta)} \quad . \quad (2)$$

and Thrust on the blade at this annulus, $$\mathrm{T} = 4 \pi r \rho \mathrm{V}^2 a_t (1 + a_t) \quad . \quad (3)$$

where β is given by

$$\tan \beta = \frac{k_\mathrm{D}}{k_\mathrm{L}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

k_D and k_L being the lift and drag coefficients for the blade at radius r at its true slip angle;

a_t and a_r are the translational and rotational intake velocity coefficients at velocity V;

ϕ_2 is largely dependent on the pitch angle.

These equations are not sufficiently accurate for screws behind such a model as was used, but they serve to show the relation between thrust and a_t, and efficiency, a_t, and the lift/drag ratio of blade sections.

§ 18. It will be seen that the efficiency of a blade section depends upon its lift/drag ratio, and if a screw blade be so altered at any radius that a_t remains the same and the lift/drag ratio is unaltered, the change has no effect. It also follows that in any screw the best result is obtained when both β and a_t are at the lowest possible values consistent with design requirements.

It is possible, for example, to produce the same reduction of thrust near a blade tip by altering the width or the slip angle of the blade at the tip. Reducing the width reduces the blade area, but increases the camber ration, and with the thin sections near the tip, and within certain slip limits, this improves the lift and lift/drag ratios. Reducing the slip angle will always reduce the thrust, but unless the original slip angle is very high (6° to 8°) the new lift/drag ratio may be worse than before. In certain circumstances, therefore, either change would produce the same efficiency change; but it would not follow that a screw in which *both* changes were made would have a still better efficiency if the two changes led to unloaded blade tips, since this always allows the revolutions to run up.

§ 19. It is by similar reasoning that an explanation can be found for what appears at first sight to be discrepancies in the experiment results here given. For example, the thinning down of the blade sections passing from B 8 to B 37 (Table VII.) effected a small improvement in propulsive efficiency, yet the similar change passing from B 34 to B 39 (Table VIII.) gave a somewhat worse instead of better efficiency. Screw B 8 had high camber ratios and far too much curvature of blade back at the trailing edge all over the inner half of the blades, and reduction of thickness gave much better lift/drag ratios over this part, hence the improvement in the total efficiency. But with a section of the type used in B 34 a much higher camber ratio can be carried than with a circular-back section without any loss in efficiency if the correct slip angle is used. Reduction of thickness in such a case led to no gain in lift/drag at the root, and some loss at and beyond half radius, where the camber ratio is down to 5 per cent. (see also § 38).

§ 20. One other factor comes into this general theory of screw action, viz. tip losses.

We know that the loss of energy in the race and at the blade tips depends upon the radial distribution of the thrust and its general intensity. These tip losses are fairly severe on marine screws, since the aspect ratio of the blades is rarely greater than 4·0. Recent analytical work by Hueber * with an untwisted blade has shown that the tip losses are reduced to a minimum when the tip width is one-third of the root width. We also have some experiment data on this in Report 140 (1922) of the National Advisory Committee of America, in which when a tip with rearward slope was added to a blade it improved the k_L/k_D ratio over the angle range of 0° to 5°, but at high slip angles (above 8°) the result was generally worse. Hence we should expect some advantage in a propeller from skewing back the blade tips, and alternatively, or in conjunction with this, making the blade tips somewhat narrower than is usually done. This conclusion is supported by the general results in Table VI., and by the recent analytical work † of K. Schimamoto.

§ 21. So far attention has been concentrated on propellers in general. But in the highly variable wake behind a single-screw ship, conditions will be very different from those in a uniform current of water, the thrust distribution will be different, and the features necessary for highest efficiency may also be different. To study this effect, some calculations have been made for two of the screws, B 8 and B 34, at two slips, 40 per cent. and 25 per cent., first in open water and then in a variable speed wake, of the type existing behind a single-screw hull. The wake value expressed as a percentage of the speed of advance of the ship is given in Fig. 5 (Plate XXXV.), together with the calculated intake velocities a_t, and the efficiencies of each part of each blade under these four conditions. No allowance has been made for the falling off of lift or lift/drag at the blade tip, or for the effect of boss diameter on root velocity. These effects would be much the same on the two screws, and would result in the extreme tip and root loading being less than that shown. The distribution of a_t is much better with B 34 than with B 8, particularly in the variable wake, when the radial distribution is somewhat nearer that for minimum tip loss than it is in open water. Also it should be noticed that the loss of efficiency in the variable wake is definitely less with B 34 than with B 8 at both slips, and this receives a little support from the η/η_1 values for most of the screws in Table VIII. Incidentally these curves of a_t in conjunction with equation (3) afford a ready means of comparing the relative strains on blades of the types described in the tables.

§ 22. This variation of wake velocity behind the hull has another bearing on screw design. The wake velocity, taken as a percentage of the ship speed, varies at the blade tips from 100 per cent. to about 8 per cent., at half radius from 80 per cent. to about 35 per cent., and near the boss from 70 per cent. to 50 per cent. With finer lines the minimum velocities would be about the same, but the maximum would be restricted to a very narrow belt and be somewhat lower. But there will be the same *general* variation, and the same call for a fairly good average positive slip angle to ensure that the slip angle shall remain positive in all positions during a revolution. This to some extent restricts the possible variation of slip, and therefore pitch over the blade. Where this wake variation is high, as at the blade tips, reduction of pitch would be dangerous; where it is low, as at the boss, pitch variation can be made in reasonable safety. The amount of such change possible would depend on many things. With this hull the real slip is about 36 per cent., and for screws under such conditions an average pitch reduction of 20 per cent. at the root represents our present practice, and gives good results (see Table VIII.).

§ 23. In considering the broad results one other matter has to be borne in mind, viz. the design of the blade roots in relation to the boss. At the blade root there are three variable dimensions, blade width and thickness and boss diameter. Minimum values of the first two are set by strength requirements. If the blade thickness be written as x D

* *Zeit Flug.*, Vol. 24, pp. 249–51 and 268–72. † *Werft Reederei Hafen*, 14, pp. 297–302.

and the boss diameter as b D, it is obvious that with four-bladed screws the flow of water between the blades will be completely choked when

$$\pi b = 4x$$

or with x equal to $0 \cdot 047$ the boss diameter becomes $0 \cdot 06$ D, and with six blades this rises to $0 \cdot 09$ D. In practice the boss diameter varies from $0 \cdot 15$ D to $0 \cdot 3$ D, and Fig. 6 has been drawn to show the nature of the water gaps between consecutive blades over this boss diameter range. The water gap is measured normal to the blade, and plotted on a base of distance from the leading edge of one blade. The blade width at root was rather wide for a four-bladed screw, viz. $0 \cdot 29$ D. The figure has been drawn for three pitch ratios and two typical blade sections, one of circular back and flat face, the other not

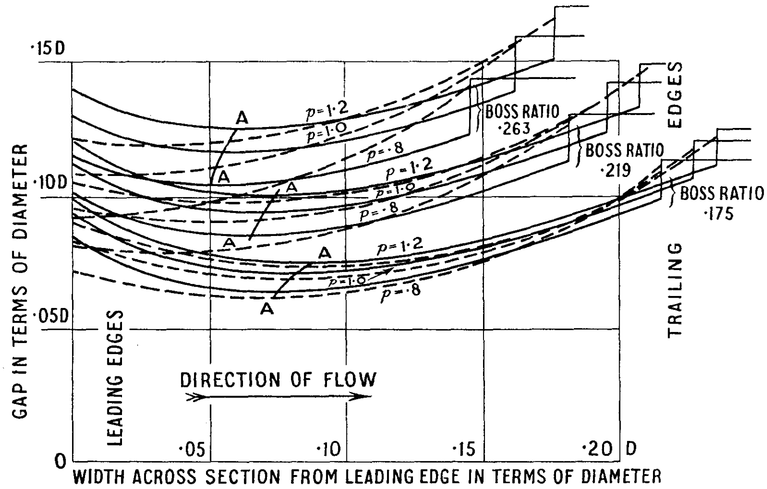


FIG. 6.—VARIATION OF WATER GAP BETWEEN CONSECUTIVE BLADES—FOUR-BLADED SCREWS WITH CIRCULAR BACK AND AEROFOIL SECTIONS. GAP MEASURED NORMAL TO THE STRAIGHT PITCH FACE OF THE SECTION.

——— A $\frac{3}{8}$, W $\frac{1}{2}$, T $\frac{3}{8}$. Aerofoil section, camber ratio $= 0 \cdot 176$.
– – – – C or circular-back flat face section, camber ratio $= 0 \cdot 176$.

unlike the B 33–36 screws. The velocity of the water through the screw at the boss will vary inversely as these gaps.

It will be seen that with the aerofoil section the *slope* of these curves is not affected materially by either boss diameter or pitch ratio, although with circular backs there is a small increase in slope with pitch ratio. Also the smaller the boss diameter the smaller is the minimum gap at A A, and the greater is the *general rate* of expansion in the flow towards the trailing edge. The attempt to increase this rate of expansion beyond a certain figure will cause breakdown, and we can say with certainty from general experience that where the slopes of these lines exceed about 20°, or the average rate of expansion is one in four, trouble will arise.

Other plottings of the same character show that this gap shape is dependent mainly on the shape of the blade section, and mostly on the after-part. Washing back the trailing edge improves it, but even with thick blades this should not be carried beyond about T $0 \cdot 4$ or T $0 \cdot 3$—with thin screws less than this is necessary, and it is not difficult to construct a diagram as Fig. 6 for any screw.

§ 24. It might at first sight appear that the easy solution of part of the trouble would be to increase boss diameter. Thus with blade thickness $0 \cdot 047$ D and a boss of $0 \cdot 175$ D and $0 \cdot 263$ D the expansion going from A A to the trailing edge in Fig. 6 would be about

1·7 and 1·4 respectively. But the bigger boss would only give better results provided there was sufficient fore-and-aft room to fit a long cone behind the boss. If a large boss of spherical or cubic shape is used, it has been shown in my 1927 paper, this means a loss of about 3 per cent. in efficiency compared with a streamlined boss.

Detail Results.

§ 25. *Number of Blades.*—Several comparisons of propellers of the same type of blade sections and varying number of blades are gathered together in Table III. The first set includes four circular-back screws, in which the total blade area was kept constant but divided up between three to six blades. All the blades had the same maximum thickness at the boss, so that the curvature of the backs increased with the number of blades. As the six-bladed screw, and probably the five-, was suffering from the bad water gap at the blade roots, screw B 18 with a finer trailing edge to the sections was tried, and it gives a fairer idea of the effect of number of blades if this is compared with the sequence B 11, B 8, B 12. There is a slow but steady falling off in propulsive coefficient, largely due to loss of screw efficiency behind the hull. If the boss diameter had been increased as the number of blades was increased, so as to maintain the same general root clearance between the blades, a somewhat better result would have been obtained with the five- and six-bladed screws. A similar comparison between two screws with aerofoil backs to the blades (B 19 and B 4) is given in the table, and this shows that the four-bladed screw is better than the three-bladed, now that the root sections are of better shape. A comparison of screw B 13A with B 13 shows that the elimination of three blades from the latter, and the consequent freer movement of water through the screw at the boss, has improved the propulsive efficiency.

§ 26. An extension of this has recently been made in connection with the design of a 10–11-knot cargo vessel. The shipowner agreed to try a five-bladed screw against a four-bladed one, both designed at the Tank, somewhat similar to B 36 as regards sections. The five-bladed screw had a propulsive coefficient of 0·76, whilst the four-bladed screw was 1 per cent. worse—these tests were with rudder and fin on the model hull.

These results appear to justify the statement that the number of blades can be increased with a slight gain in efficiency, provided the blade thickness allows of low camber ratio root sections and good water gap.

Rake and Outline of Blades.

§ 27. The effect of simple fore-and-aft rake is shown by the three screws B 1, B 31, B 32 (Table IV.). These were identical in design except that the blades were raked aft to the varying extent shown. The most noticeable effect of rake was the reduction of both and w, which caused the hull efficiency to fall slightly, this being compensated by a rise in relative rotative efficiency. It seems fairly clear from this that 30° rake is a little beyond the useful value in this ship.

In many of the screws, rake was obtained by change in the radial distribution of the blade area, keeping the angle of the strickle plate at 15° as for B 1, etc. Screw B 2A differs from B 1 only in having a 25 per cent. wider blade root, the blade tip width being the same. Also B 3 has the same blade root as B 2A, but a narrower tip. In both cases the change was made at the leading edge, and since the fore-end of the boss was fixed, it follows that the blade tips moved aft with the increasing rake passing from B 1 to B 2A and B 3 (Table IV.). Here, again, there was a definite improvement in relative rotative efficiency with rake, obscured in the case of B 2A by a small drop in open-screw efficiency, but present in the improved propulsive coefficient of B 3. A similar small gain in efficiency

TABLE IV.

Effect of Rake and Outline of Blades.

Screw No. B	1		2A		3		31		32		4		14		5		15		36		38	
Number of blades	4		4		4		4		4		4		4		4		4		4		4	
Blade outline	Oval		Oval tip, wide root		Fine tip, wide root		Oval		Oval		Oval		Fine tip, wide root		Oval		Oval tip, wide root		Mussel		Wide tip, root as B 36	
Type of section at root / at 0·3 D	A·37, W·05 A·37, W·05		As B 1		As B 1		As B 1		As B 1		As B 1		As B 1		As B 1		As B 1		A·3, W·4, T·28 A·3, W·3		A·3, W·4, T·28 A·3, W·3	
Camber ratio at root / at 0·3 D	27·0 8·8		21·7 8·2		21·7 8·4		28·6 9·6		31·0 10·3		As B 1		21·7 8·4		As B 1		21·7 8·2		22·4 9·4		22·4 7·2	
Blade width at root / at 0·3 D	16·0 21·7		20·0 23·3		20·0 20·4		As B 1		As B 1		As B 1		20·0 20·4		As B 1		20·0 23·3		19·4 20·4		19·4 27·0	
Rake in degrees	15		15		15		22·5		30		15		15		15		15		15		15	
$\frac{\text{Rake + Skew at tip}}{\text{D}}$	0·18		0·21		0·23		0·35		0·45		0·18		0·23		0·17		0·21		0·23		0·23	
Pitch reduction at root, per cent.	Nil		Nil		Nil		Nil		Nil		20·0		20·0		40·0		40·0		20·0		20·0	
Speed in knots	12	14	12	14	12	14	12	14	12	14	12	14	12	14	12	14	12	14	12	14	12	14
ψ	0·72	0·70	0·715	0·69	0·735	0·715	0·72	0·70	0·715	0·695	0·73	0·705	0·74	0·72	0·735	0·705	0·735	0·715	0·735	0·725	0·715	0·695
η_1 (open)	0·585	0·58	0·58	0·575	0·58	0·575	0·585	0·58	0·585	0·59	0·58	0·575	0·60	0·60	0·58	0·575	0·58	0·585	0·59	0·59	0·58	0·58
$\frac{\psi}{\eta_1}$	1·23	1·21	1·23	1·20	1·26	1·24	1·23	1·20	1·22	1·18	1·26	1·22	1·23	1·20	1·27	1·28	1·26	1·22	1·25	1·23	1·23	1·20
$\frac{\eta\ \text{(behind)}}{\eta_1\ \text{(open)}}$	1·07	1·07	1·08	1·06	1·09	1·09	1·09	1·06	1·10	1·08	1·09	1·07	1·06	1·06	1·09	1·08	1·07	1·06	1·06	1·06	1·08	1·06
h	1·14	1·13	1·14	1·13	1·16	1·14	1·13	1·13	1·11	1·09	1·15	1·14	1·16	1·14	1·16	1·14	1·18	1·15	1·17	1·16	1·14	1·13
t	0·214	0·214	0·210	0·214	0·210	0·215	0·204	0·204	0·19	0·19	0·215	0·215	0·21	0·21	0·210	0·210	0·210	0·215	0·19	0·19	0·21	0·21
w	0·45	0·44	0·45	0·44	0·47	0·45	0·42	0·42	0·37	0·35	0·47	0·46	0·47	0·44	0·47	0·46	0·50	0·47	0·45	0·43	0·45	0·43
N	81·5	96·5	82·0	97·5	81·0	97·0	81·0	96·5	81·3	96·7	81·5	97·0	81·5	97·0	82·2	98·0	82·5	98·0	81	96·3	81	96·5

Screws B 1, B 31, B 32 differ only in rake.
Screws B 1, B 2A, B 3 have the same rake but different blade outlines.

Screws B 4 and B 14 and screws B 5 and B 15 have the same rake but different blade outlines.
Screw B 38 has the same root sections as B 36 but wider blade tips.

was shown with screws B 14 compared with B 4, and B 15 compared with B 5, both having the narrow tip wide-rooted blade outline.

§ 28. Mr. J. L. Kent has for some time been testing various hulls self-propelled in waves to determine how propeller losses come about under these conditions. In some cases he has found that a narrow tip aerofoil screw loses its efficiency rather noticeably in waves, mainly through a too rapid increase of revolutions. An intermediate blade outline with medium narrow tip ("mussel" in Fig. 4) was therefore tried, and as this gave good results it became the standard outline for the later screws B 29, B 34, B 35, and B 36.

§ 29. To complete the sequence of propellers, one was made with rather wider blade tips than usual. Screw B 38 is similar to B 36 (Table IV.), except for this increase in tip width. It had the fairly wide root of B 29 with 20 per cent. reduction of pitch at the boss. The propulsive coefficient was 2·5 per cent. worse than before, largely due to

TABLE V.

Effect of Moving Propeller Aft on the Ship. No Rudder and no Fin Present.

Movement equals 0·04 D or 9 in. for 400-ft. Ship.

Screw No.	Type of Blade.	* Rake + Skew / D	Change due to Moving Aft.			
			Quasi-propulsive Coefficient.	Wake Fraction, w.	Thrust Deduction Fraction, t.	Relative Rotative Efficiency, $\frac{\eta}{\eta_1}$
B 1	Normal	0·18	Improved 2%	Unaltered	Down $\frac{1}{2}$%	Up 1%
B 3	Fine tip, wide base	0·23	Improved $\frac{2}{3}$%	Unaltered	Down $\frac{1}{2}$%	Unaltered
B 14	Fine tip, wide base	0·23	Improved $\frac{1}{2}$%	Unaltered	Up $\frac{1}{4}$%	Up 1%
B 29	Mussel	0·23	Improved $1\frac{1}{2}$%	Unaltered	Down $\frac{2}{3}$%	Up $\frac{3}{4}$%
B 31	Normal	0·35	Improved $\frac{1}{2}$%	Unaltered	Down $\frac{1}{2}$%	Unaltered

* Measured from leading edge at root to blade tip, in the longitudinal plane.

increase in thrust deduction. The broad result here is in general agreement with those obtained by Froude and Taylor.

§ 30. To determine whether the improvement obtained with rake or skew back was due to the throwing back of the blade tips relative to the hull, or to the slanting leading edge, six of the screws, of varying shape, were tried in two fore-and-aft positions. The maximum possible movement on the model was 0·5 in. (0·04 D). The effect of this movement is given in Table V.

There is a small improvement in propulsive efficiency derived in part from the better relative rotative efficiency and part from the better thrust deduction fraction. Results published by the Tank in the Transactions of this Institution for 1923 show a slightly greater change in thrust deduction with movement aft of the screw, and little change in wake except for large fore-and-aft movement of screw. These 1923 tests were with a fuller hull (prismatic coefficient 0·79) with which a more rapid change might be expected.

The conclusion from this work therefore is that rake improves the relative rotative efficiency behind the hull, mainly because of the greater clearance between the body-post of the hull and the leading edge of the screw. The advantage of rake in light condition and rough water has been dealt with elsewhere.

TABLE VI.

Effect of Varying Pitch Over the Blades.

Screw No. B	1		4		5		17		19		20		42	
Number of blades	4		4		4		3		3		4		3	
Blade outline	Oval		Oval		Oval		Oval		Oval		Wide oval		Oval	
Type of section { at root at 0·3 D	A·37 W·05 A·37 W·05		As B 1		As B 1		A·37 W·05 A·37 W·05		A·37 W·05 A·36 W·05		A·25 W·3 T·25 A·25 W·05		C C	
Camber ratio { at root at 0·3 D	27·0 8·8		As B 1		As B 1		20·0 6·7		20·0 6·7		21·7 7·0		20·0 7·4	
Blade width { at root at 0·3 D	16·0 21·7		As B 1		As B 1		21·4 28·8		21·4 28·8		20·0 27·5		21·4 28·8	
$\frac{\text{Rake + skew at tip}}{\text{D}}$	0·18		0·18		0·17		0·20		0·20		0·20		0·20	
Pitch reduction at root, per cent.	Nil		20		40		Nil		20		40		Nil	
Speed in knots	12	14	12	14	12	14	12	14	12	14	12	14	12	14
ψ	0·72	0·70	0·73	0·705	0·735	0·705	0·71	0·685	0·725	0·70	0·735	0·715	0·69	0·68
η_1 (open)	0·585	0·58	0·58	0·575	0·58	0·575	0·585	0·59	0·585	0·59	0·58	0·575	0·56	0·55
$\frac{\psi}{\eta_1}$	1·23	1·21	1·26	1·22	1·27	1·23	1·21	1·16	1·24	1·19	1·27	1·24	1·23	1·23
$\frac{\eta}{\eta_1}$	1·07	1·07	1·09	1·07	1·09	1·08	1·05	1·04	1·07	1·03	1·07	1·06	1·07	1·07
h	1·14	1·13	1·15	1·15	1·16	1·14	1·15	1·12	1·16	1·15	1·19	1·17	1·15	1·15
t	0·214	0·214	0·215	0·215	0·210	0·210	0·215	0·210	0·210	0·210	0·20	0·21	0·205	0·21
w	0·45	0·44	0·47	0·46	0·47	0·46	0·47	0·42	0·47	0·46	0·49	0·48	0·45	0·46
N	81·5	96·5	81·5	97·0	82·2	98·0	85·0	101	84·8	101	81·8	97·5	84·5	101

Screw B 42 has pitch 20 per cent. less at leading edge than at trailing edge, over outer half of screw.
In all other screws the pitch reduction varies from that given at the blade roots to nil at half radius.

Variation of Pitch.

§ 31. All of the screws were of unity pitch ratio over the outer half of the screw disc. Other tests had shown that reduction of pitch towards the boss carried a small advantage, and this was tried in three sets of screws. The results are given in Table VI. The first set consisted of three screws each with four oval blades (B 1, B 4, B 5), the pitch being reduced 0, 20 per cent., and 40 per cent. at the boss respectively. Screw B 19 has three oval blades and differs from B 17 only in having 20 per cent. reduction of pitch at the boss. Screw B 20 has 40 per cent. pitch reduction at the boss with A 0·25 sections. The nearest comparable screw is B 6 (Table VII.), which has A 0·25 sections but narrower blades. In all three cases the propeller with reduced pitch at the boss is better than the one with uniform pitch.

The revolutions are increased between 1 and 2 per cent. by the pitch reduction in the first set, but remain unaltered with the wide three-bladed screws. Results from other experiments show that a small increase of the order stated usually follows from reducing the pitch in the manner adopted in these tests.

§ 32. In one screw, B 42 (Table VI.), over the outer half of the screw the pitch was increased from leading to trailing edge. The slip angles at the trailing edge were the same as on B 19, but were reduced to half the standard value at the leading edge. This really gave hollow-faced circular-back blade sections. The result was not good, and this type of variation was not pursued any farther.

Cross-section of Blades.

§ 33. In dealing with a variety of blade section it has been necessary for the sake of brevity to use a shorthand method of describing them. This is given in the Nomenclature Appendix. The experiments commenced by trying a series of screws, B 8, B 7, B 1, B 9, B 6 (Table VII.), in which pitch, outline, etc., remained the same, but the leading edge was made steadily fuller and the trailing edge finer on the backs of the sections passing from B 8 to B 6. The same type of section was used on any screw from blade root to tip. Most good aerofoil sections have a rounded leading edge, but screw B 9 was made similar to the others, except that the leading edge was made quite sharp in the former. In this series the A 0·37, W 0·05 back gave the best result. For all practical purposes the values of w, t, and relative rotative efficiency were not affected by these changes. As screw B 9 with its sharp leading edge and quicker curvature gave worse results than B 1, screw B 10 was made similar to B 1, but with the driving face at the leading edge washed back at the root where the back curvature was severe. The rounding of the leading edge was made the same as in B 1. This eased the back camber of the root sections very considerably, and there was some improvement in propulsive efficiency without any change in revolutions.

§ 34. A second method of reducing the curvature of the backs of the screws is to reduce their thickness. This was tried in this series with screw B 37, which is similar to B 8 in all respects except that the blades were half the thickness. There was a 2 per cent. gain in efficiency, and a 4 per cent. increase in revolutions with the thinner screws. The difference was practically all due to the open screw efficiency. This result is discussed in § 19 and § 38.

§ 35. The change from blades of C to A $\frac{1}{4}$ sections with rounded leading edge was also tried on screws with six blades, viz. B 13 and B 18 (Table III.), and resulted in a quite marked improvement in the propulsive coefficient. Screw B 13 suffered from two defects: the curvature of the backs of the blades was very high except near the tips, and the blade roots were very close together, so that the water gap between them was very poor. When three blades were cut away, the screw becoming B 13A (Table III.), it gave a much better result than B 18, despite the bad circular-back sections and the rather high revolutions of B 13A. This last case is given, not to show the effect of blade section at all, but to illustrate the care required in ascertaining why a certain change produced a given

TABLE VII.

Effect of Type of Blade Section.

Outline of Blades—Oval. Uniform Pitch. Rake 15°.

Screw No. B	37		8		7		1		9		10		6		11		17		26	
Number of blades	4		4		4		4		4		4		4		3		3		4	
Type of section { at root	C		C		A·5, W·05		A·37, W·05		A·37		A·37, W·3		A·25, W·05		C		A·37, W·05		A·32, W·15, T·15	
Type of section { at 0·3D	C		C		A·5, W·05		A·37, W·05		A·37		A·37, W·05		A·25, W·05		C		A·37, W·05		A·3	
*Camber ratio { at root	13·5		27·0		} As B 8		As B 8		As B 8		As B 8		As B 8		20·0		20·0		27·0	
*Camber ratio { at 0·3 D	4·4		8·8												6·7		6·7		8·8	
Blade width { at root	16·0		16·0		} As B 8		As B 8		As B 8		As B 8		As B 3		21·4		21·4		16·0	
Blade width { at 0·3 D	21·7		21·7												28·8		28·8		21·7	
$\frac{\text{Rake + Skew at tip}}{D}$	0·18		0·18		0·18		0·18		0·18		0·18		0·18		0·20		0·20		0·18	
Speed in knots	12	14	12	14	12	14	12	14	12	14	12	14	12	14	12	14	12	14	12	14
ψ	0·715	0·705	0·705	0·685	0·70	0·695	0·72	0·70	0·71	0·70	0·73	0·705	0·71	0·685	0·715	0·69	0·71	0·685	0·71	0·70
η_1 (open)	0·595	0·58	0·58	0·58	0·58	0·58	0·585	0·58	0·575	0·575	0·575	0·57	0·57	0·56	0·58	0·58	0·585	0·59	0·585	0·585
$\frac{\psi}{\eta_1}$	1·20	1·21	1·22	1·18	1·21	1·20	1·23	1·21	1·23	1·22	1·27	1·24	1·24	1·22	1·23	1·19	1·21	1·16	1·21	1·20
$\frac{\eta \text{ (behind)}}{\eta_1 \text{ (open)}}$	1·05	1·07	1·06	1·05	1·07	1·07	1·07	1·07	1·07	1·08	1·08	1·07	1·09	1·08	1·09	1·05	1·05	1·04	1·07	1·06
h	1·14	1·14	1·15	1·12	1·14	1·12	1·14	1·13	1·15	1·13	1·17	1·16	1·14	1·13	1·13	1·13	1·15	1·12	1·14	1·13
t	0·20	0·21	0·210	0·210	0·210	0·210	0·214	0·214	0·210	0·210	0·210	0·214	0·225	0·225	0·215	0·210	0·215	0·21	0·22	0·21
w	0·43	0·45	0·46	0·44	0·44	0·42	0·45	0·44	0·46	0·44	0·48	0·47	0·48	0·47	0·44	0·43	0·47	0·42	0·46	0·43
N	84	99·5	80·5	95·8	81·2	96·2	81·5	96·5	80·5	95·8	80·8	96·8	83·0	99·0	84·8	101	85·0	101	81·5	97·0

* All camber ratios are taken on radial sections.

Screw B 9 is B 1 with a sharp leading edge vice rounded one.
Screw B 10 is B 1 from tips to half radius but with leading edge washed back at the blade roots.
Screw B 37 had blades similar to B 8 but only half the thickness.

result in one propeller before embodying that change in another. There are at least four defects in screw B 13, two of which are mitigated in B 18—the water gap at root and curvature of the back of the sections. Cutting out some of the blades still left the defect in the blades themselves, and in fact aggravated it by the higher slip necessary for propulsion.

§ 36. It was apparent from general experience and the reasoning already given that if blade sections were adopted, such that with the high camber ratio of the root sections the slope of the trailing edge and the curvature of the *back* were kept as low as possible, consistent with good lift/drag ratios, a better result would be obtained. Such sections with the driving face washed back at the trailing edge also assist in the promotion of satisfactory water gaps at the blade roots. Only a few results for blades of this kind in water have been published (by Froude, Stanton, and, recently, Allan), and these for only a few blades. But there are many such sections which have been tested in air in the various aeronautical laboratories, and until better water data are available these afford definite and good guidance as to what is possible in marine practice. One word of caution perhaps is necessary here, viz. there is no cavitation in air, and where the efficiency of any blade in air obviously depends upon the production of high and local suction pressure on the blade backs (as with some full leading edge blades) these should be left out of account. The maintenance of these peak suction pressures requires steady conditions, and when these are not forthcoming—as in a highly variable wake, or when the ship is pitching—such blades would show a fairly rapid falling off in efficiency, and in some cases might lead to cavitation.

§ 37. These aeronautical results have been closely studied, and a series of screws have been made in which the camber of the blade sections was divided, and part put on the driving face by washing it back at both trailing and leading edges of the blade. Towards the root of the blades, up to 50 per cent. of the camber was put on the driving face in one case (B 35, Table VII.). Towards the blade tip, where camber ratios are low, a flat pitch face was maintained except for the rounding of the leading edge—air results showing that at low camber ratios this gives as good and sometimes better results than sections with curvature on the pressure face. Such sections were used in conjunction with the "mussel"-shaped outline of blade, generally with 20 per cent. reduction of pitch at the boss, i.e. with features which general experience and the previous tests had shown to be efficient. These screws are B 33, B 34, B 35, and B 36 (Table VIII.). The type of section used for the back of the blade was A 0·37, except for B 36. For all ordinary screws this can always be built into a good blade section, and gives a sufficiently fine trailing edge to avoid any eddies, if combined with a correctly cambered driving face. Screw B 36 had blade sections fuller than the others towards the leading edge, and with larger rounding at this edge. But it was somewhat worse in efficiency, and showed no advantage in other respects.

All of these screws gave good results mainly due to good screw efficiency. But with all of them the thrust deduction fraction is lower, this gain being balanced by a similar reduction in wake. These two changes are small, but it is generally better to work a screw with as low thrust augment as possible. The revolutions for self-propulsion varied a little with type of section. Generally speaking, large wash back of the trailing edge (i.e. a high T value in the tables), as in B 34 and B 35 (Table VIII.), increased the revolutions. With a section of T 0·3 to T 0·4 at the boss (about the highest useful value) reduced to practically nil at half radius, the revolutions are about 2 per cent. to 3 per cent. above those required with a standard circular-back screw. When calculating dimensions for a screw of this type, therefore, the pitch over the outer part of the blade should be raised about 2 per cent. to 3 per cent. higher than that obtained for a screw of uniform pitch and circular-back sections.

§ 38. Two additional screws were made and tested in this series, viz. B 26 and B 39. Screw B 26 (Table VII.) had the oval outline and uniform pitch of the standard screws, the blade sections being washed back at the root in a *quick* turn common in many foundries.

TABLE VIII.

Effect of Type of Blade Section.

Outline of Blades—Mussel. Rake + Skew = 0·226 D. Four Blades. Pitch Reduction at Blade Root, 20 per cent. in all Cases.

Screw No. B ..	29		33		34		35		36		39	
Type of Section { At root .. / At 0·3 D	A·37 W·36 A·37 W·36		A·37 W·4 T·3 A·37 W·37		A·37 W·4 T·3 A·37 W·2 T·2		A·37 W·5 T·5 A·37 W·3 T·3		A·3 W·4 T·28 A·3 W·3		A·37 W·4 T·3 A·37 W·2 T·2	
Camber ratio { At root .. / At 0·3 D	22·4 9·4		22·4 9·4		22·4 9·4		22·4 9·4		22·4 9·4		11·2 4·7	
Blade width { At root .. / At 0·3 D	19·4 20·4		19·4 20·4		19·4 20·4		19·4 20·4		19·4 20·4		19·4 20·4	
Speed in knots	12	14	12	14	12	14	12	14	12	14	12	14
ψ	0·745	0·735	0·75	0·74	0·755	0·735	0·75	0·73	0·735	0·725	0·73	0·72
η_1 (open)	0·59	0·585	0·605	0·605	0·61	0·605	0·60	0·595	0·59	0·59	0·61	0·60
$\frac{\psi}{\eta_1}$	1·25	1·24	1·24	1·23	1·24	1·21	1·15	1·23	1·25	1·23	1·20	1·20
$\frac{\eta \text{ (behind)}}{\eta_1 \text{ (open)}}$	1·09	1·08	1·08	1·07	1·08	1·07	1·08	1·06	1·06	1·06	1·05	1·06
h	1·165	1·15	1·15	1·14	1·14	1·13	1·16	1·15	1·17	1·16	1·14	1·13
t	0·214	0·214	0·19	0·185	0·20	0·20	0·19	0·19	0·19	0·19	0·195	0·205
w	0·48	0·47	0·42	0·40	0·43	0·41	0·44	0·42	0·45	0·43	0·42	0·42
N	80·8	96·0	80·5	95·5	83·8	100	84·5	101	81	96·3	85	101

Screw B 34 is the same as B 33 from blade root to half radius.
Screw B 33 has the same blade shape as B 29 from half radius to tip.
Screw B 39 has blades similar to B 34 but only half the thickness.

It was given a sharp leading edge as preferred by many firms, but A 0·33 section sto avoid the worst effects at the trailing edge. This was a little better than the circular-back screw, but worse than B 29 (Table VIII.), which also has a flat driving face over the after-part of the blade. It is obvious with B 26 that the root camber ratios are too large, and that the wider root of B 29 helps greatly in this respect.

The second additional screw, B 39 (Table VIII.), was similar to B 34, except that the blade thickness was halved over all the blade. The hull efficiency and thrust deduction fraction were not affected by the change, but the overall efficiency was 2 per cent. to 3 per cent. lower, and the revolutions 1 per cent. to 2 per cent. higher with the thinner screw. A general explanation of the efficiency change being lower in this case and higher with circular-back screws when the thickness is halved is given in § 19. In thinning down the circular-back screws, the high angles at the backs of the sections were eliminated, and B 37 shows the result. But with B 34 there was no eddy-making near the root to remove, and no gain of this kind was possible in B 39. Also with sections of camber ratio below about 0·8, wash back of the trailing edge is undesirable, and sometimes disadvantageous, and the use of T 0·2 over the blade in B 39 was a definite cause of loss except possibly right at the root. The thinner blades used in the tests were much thinner than would be used in common practice, and the general result indicates that nothing material is gained by reducing blade thickness, provided a reasonable camber ratio has been adopted at the root.

§ 39. The intention of this paper is to show, by precept and example, the main features in the design of a propeller. No pretence is made that the particular sections used or the blade area distribution represent the only good practice. But it is claimed that by changes of the kind indicated, varied in degree according to blade outline and general thrust requirements, a definite improvement in screw efficiency compared with standard practice is possible. It is sometimes stated that results obtained in a Tank are not obtained at sea. But apart from viscosity or friction we know that results obtained in open water on one propeller apply to all similar propellers, and in this case the size is large enough to avoid the viscosity effect. In any case the effect of size is common to all the screws, and the relative results will hold good. Some changes in wake will be present on the ship. Some notes on this were given in my 1930 paper, and I hope to deal with this at another time. But there is every probability that the ship wake has a higher velocity near the hull and a steeper lateral gradient at first, toning into practically the same gradient and wake value as the model near the blade tips. But an inspection of Fig. 5 (Plate XXXV.) will show (and other calculations support) that even moderate changes in the wake curve adopted for these calculations would have very little effect on the total efficiency at a given slip, and so far as we are concerned this effect is the same on the two very different screws represented in the figure. We may therefore safely conclude that, apart from cavitational effects, the efficiency and other changes on the model will be repeated on any ship.

APPENDIX.

Nomenclature and Definitions.

In Froude's method of propeller analysis the propulsive efficiency is broken up into separate factors as follows:—

$$\psi = \frac{E}{S_1} = \frac{R}{T}\frac{V}{V_1}\frac{T\,V_1}{S_1}\frac{S_1}{S} \times 6{\cdot}876 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

where ψ = the quasi-propulsive efficiency, or the tow rope power E divided by the shaft horse-power at the tail end of the shaft.

E = effective or tow rope horse-power for the given ship speed V,

S = shaft horse-power absorbed by the propeller,

T = propeller thrust,

R = hull resistance when towed, without propeller at speed V,

S_1 = shaft horse-power absorbed by propeller in open water at a lower speed V_1 such that it is delivering the same thrust T at the same revolutions as before,

$\frac{R}{T}$ = thrust deduction factor $= (1 - t)$ and $t = \frac{T - R}{T}$,

$\frac{V}{V_1}$ = wake factor $= (1 + w)$ and $w = \frac{V - V_1}{V_1}$.

$\frac{T\,V_1}{S_1}$ = the screw efficiency in open water $= \eta_1$,

$\frac{S_1}{S}$ = the relative screw efficiencies behind the ship and in open water $= \frac{\eta}{\eta_1}$.

Equation (5) can now be written

$$\psi = (1 - t)\,(1 + w)\,\eta_1\left(\frac{\eta}{\eta_1}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

N = revolutions for self-propulsion of 400-ft. ship at self-propulsion point of model.

$$C_T = \frac{T}{D_m^{\,2}\,v^2}$$

$$T_C = \frac{T}{\rho\, n^2\, D_m^{\,4}},\ \text{the international thrust constant}$$

where T is the thrust in pounds at speed v in feet per second on a screw of diameter D_m ft. at n revs. per sec., in a fluid of density ρ.

Aerofoil sections are defined by four main characteristics:—

(1) The camber ratio $\frac{t}{b}$ in Fig. 7.

(2) The position of the point H where a line parallel to the struck pitch line P P just touches the back. If H Q $= y\,b$, the section is called A y.

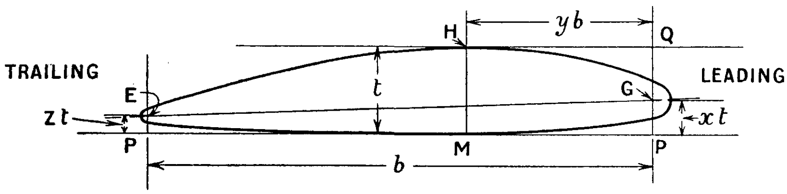


FIG. 7.—AEROFOIL SECTION, A 0·37, W 0·4, T 0·2.

(3) The amount the driving face is washed back from the pitch line P P. If G P is $x \times$ H M, and E P is $z \times$ H M, the section is

$$A_y\, W_x\, T_z$$

the points G and E being taken at the centre of the circle forming the ends of the section.

(4) The breadth of the sections (b) is expressed as a percentage of the screw diameter.

DISCUSSION.

The Chairman (Sir William J. Berry, K.C.B., Vice-President): *We have three very interesting papers to be read to us this morning, each one dealing with an important aspect of the design and use of screw propellers. The experimental study of the design of propellers has during recent years become a most important part of the work at most Tanks in England and abroad, and much valuable information and data are being accumulated. As the type, size, and engine power of ships have varied and increased, a special study has become necessary on many occasions in order to determine the propeller best suited to the new conditions. Thirty years ago we had only a comparatively small amount of useful data, and designers frequently had to extrapolate on the available curves, because the experimental results from model experiments that had then been published were for propellers of low revolutions utilizing small powers at comparatively low speeds. This disability has been removed, but even now with every advance in design of modern ships we find new conditions arising that involve further propeller experiments. In the papers we are to listen to this morning and from the discussions on them, I hope we shall hear something of the latest developments in the several Tanks.

Dr. F. Horn (Member): Mr. Baker has given us an enormous mass of information about general lines and details which have to be followed in the design of an efficient screw propeller in its action behind a single-screw ship. Everyone interested in screw design will study with the most lively interest all these details, many of which are of great practical importance, and they will be thankful to Mr. Baker for having given them the chance of making use of this new and very valuable material.

Naturally the possibilities of variations of a propeller design, even with the diameter and the revolutions kept constant for the same thrust, are so numerous that Mr. Baker's tests cannot cover the whole field. For instance, Mr. Baker has restricted himself to a pitch kept constant over the outer range of the blades, but eventually diminished in the inner part towards the root. Now pitch variation is, in my opinion, one of the salient points in the task of adapting the screw to the special ship form. According to this ship form the radial wake variation has to be regarded as a given curve. Does there exist any systematic method of making use of this curve for getting the most favourable distribution of thrust, and therefore of pitch, over the blade?

I have occupied myself rather intensively with this question, and, especially, I tried to find some guidance through that infinite mass of possible variations. I also induced a young collaborator, Dr. Voigt, to make some tests relating to this question. These tests were made during the last few years in the Berlin Tank. I myself mentioned some first results in a paper read two years ago at the Hamburg Conference. Meanwhile the whole of the tests were elaborately published recently by Dr. Voigt in the periodical *Schiffbau*. As these tests relate very closely to those of Mr. Baker's paper, you may perhaps find it of some interest to hear about the general lines followed in the tests by Dr. Voigt, and about the results. For general guidance I adopted two different methods. The first was, in a sense, somewhat empirical. It is well known that a free-running screw has its best thrust distribution given by the condition in which the so-called induced efficiency of the blade is kept constant over the whole blade. This induced efficiency is defined as

$$\eta_1 = \frac{v}{r\,w}\,\frac{C_a}{C_t}$$

where v is the speed of the propeller; w is the angular velocity; C_a is the augment of axial velocity; C_t is the tangential velocity in the race—both belonging to a disc

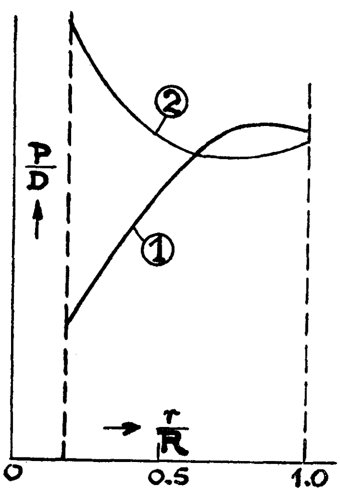
* Remarks made *before* the reading of the paper.

element of radius r. If this induced efficiency is kept constant over the radii of the whole blade, then the thrust distribution will give the optimum overall efficiency of the free-running screw. I have made use of this principle for a screw working in the wake behind a single-screw model. The formula is the same, only that the value of v now varies with radius r according to the varying wake velocities in the screw disc. With a normal wake distribution for a single-screw ship this leads to a thrust distribution that is rather pushed outwards, and to a pitch increasing from the root to the tips. The pitch variation may be somewhat like curve (1) of the sketch: the least pitch at the root, the highest pitch somewhat near the tips. The result of the test was rather satisfactory. The quasi-propulsive efficiency was about 71 per cent., without rudder behind.

The second method was more scientific, namely, on the principle that the best thrust distribution, giving the maximum overall induced efficiency, would be such as gives the minimum race loss for the whole system, ship plus screw. This race loss is defined as

$$\int \rho \, d\,Q \left[\frac{(C_a - C_f)^2}{2} + \frac{C_t^2}{2} \right]$$

where ρ is the density; $d\,Q$ is the element of quantity of water passing through the area element; C_a is the augment of the axial race velocity, C_t is the tangential velocity of the race, and C_f is the absolute velocity of the frictional wake stream measured behind the propeller in the ship—all these three, C_a, C_t, C_f, being mean values over the said area element. The expression "race loss" is, perhaps, not quite apt, because the frictional wake stream is included. That cannot be called a race, but I hope that it will be clear what is meant by the above expression. In Germany we call it "Austrittsverlust." The value of the expression need not be worked out analytically, but graphically by assuming different thrust distributions, giving all the same thrust: $T = \int \rho \, d\,Q\,C_a$. I may add that it is not a difficult thing to make numerical calculations by the method described. It gives a curve of pitch distribution like curve (2) of the sketch.



In both cases the pitch distribution giving the above thrust distribution is systematically deduced from the principle that the local blade profile shall have the smallest value of the proportion: drag to lift coefficient, and that it shall have sufficient breadth to avoid cavitation. By this means one gets, at the same time, the blade outline that is systematically co-ordinated to the pitch distribution.

From the sketch you may see that the second propeller has a pitch distribution which in the inner range decreases rather strongly from the root, and in the outer range increases somewhat again towards the tip. You may remark that this pitch distribution is rather contrary to that chosen by Mr. Baker.

The result of the tests with this theoretically optimum propeller was rather good also, but not very different from the first. In both cases the efficiency shows a rather big improvement when, according to the practically existing circumstances of a single-screw ship, a streamline rudder is placed behind the screw. But in this case the second screw shows still a remarkably higher performance than the first one. This result is very plausible, because the tangential velocities in the inner range, where their energies are mostly regained by the streamline rudder, are greater with the second screw than with the first one. At the working speed the quasi-propulsive efficiency with the second screw and rudder behind was the very good value of about 84·5 per cent., as compared with about 81·5 per cent. with the first screw.

I may further mention that in both cases the thrust deduction factor was very much lower with rudder behind than without rudder. In this respect I was rather surprised that Table II. of Mr. Baker's paper gives an augment of thrust deduction with a rudder as against a vessel without a rudder. Perhaps the rudder was not a streamlined rudder, and, therefore, had a high resistance of its own. Would Mr. Baker answer this last question?

Mr. Baker: It was a plate rudder of the ordinary type, without any gaps at all.

Mr. C. N. H. Lock, M.A., F.R.Ae.S.: I have been for some years engaged in research at the National Physical Laboratory on the subject of aeroplane propellers, both by model experiments and on the theoretical side. I am, therefore, very interested in the work of Mr. Baker on the water propeller, which is of course much less familiar to me. I begin to realize from Mr. Baker's paper some of the extra difficulties that beset the ship's screw designer as compared with the designer of aeroplane propellers. I may mention the much greater effect of the ship's wake and the much greater blade width, which makes the application of a theory very much more difficult. I have, therefore, a great admiration for the way in which Mr. Baker has tackled this problem.

I thought it might be of some interest to the meeting to hear that quite recently, within the last few weeks, we have in our researches on aeroplane propellers succeeded in getting extremely good agreement between theory and experiment, even for propellers of very high pitch. Agreement had been obtained for low-pitch propellers for some years without making any allowance at all for tip loss; but the new results of which I am speaking include a purely theoretical allowance for tip loss which is found to give surprisingly good agreement with experiment, even for pitch ratios up to 2·5, which I imagine is even greater than is likely to be encountered in the case of ship screws. The blade width, on the other hand, is only about one-third of the width of the average water propeller. This success makes me feel ambitious to try the same theoretical methods on the model results with water propellers, of course in the case of free water. The problem in the wake of the ship is obviously very much more complicated. The fact that the allowance for tip loss succeeds up to very high pitch makes me think it possible that agreement might be obtained even in the very extreme case of the broad blade of the ship propeller.

There is one question I should like to ask Mr. Baker. He refers to the improvement obtained by substituting an aerofoil section for a circular arc section. From experience with aerofoils and from aerofoil theory, I should have thought it possible that the circular arc section would be equally good for one particular working condition or incidence, but that the advantage of the aerofoil section would lie in its having a high efficiency over a bigger angle range. I should like to ask Mr. Baker whether any of his experiments bear out this suggestion.

Before hearing this paper I had wondered whether, if this was so, the circular-back propeller might not work all right as a ship's propeller on account of its presumably working always at one particular rate of advance, but I see now from Mr. Baker's paper that, with the screw working in the wake, the large variation in wake velocity round the circle would seem to require the aerofoil section on account of the big variation of incidence round the circle.

Ir. L. Troost (Member): I have only a few remarks to make on what Mr. Baker has told us about the experiments of varying screw propellers behind a single-screw model. In the first place, I can fully endorse the results which Mr. Baker has got with his best set of propellers. We tested a couple of them in our Tank, and in various cases we got the efficiencies which Mr. Baker predicted from his results. Therefore we can regard with much confidence the propellers which Mr. Baker has designed; I would only emphasize that the results of a couple of those propellers with and without rudders turned out somewhat differently. In our experience, for instance, propeller B 34, which is the best of Mr. Baker's set, is not quite so good in combination with a streamline rudder as, for instance, a propeller of the type of B 35. For myself I am a little sorry that Mr. Baker has given his results behind a single-screw ship without a rudder. I should have thought that his valuable information—and I fully endorse what Professor Horn has said on the point—would be still more helpful if he had given his results with a rudder.

As to the differences in quasi-propulsive efficiency that you get when using an aerofoil propeller not well designed, it is perhaps interesting to show you a couple of models where we have tried to make streamlines visible in a very simple way by painting the models with very thin white paint. I am not sure that the streamlines on the model are absolutely the real streamlines, because of the viscosity of the paint, its roughness, and so on; but at any rate you can see some interesting points on them. For instance (*illustrating*), we have one propeller of constant pitch and a badly designed back. There you see that the streamlines on the back make a very sudden turn from the leading edge to the trailing edge; but if you look to the driving face you will see that they are all very regular. If you have a better designed propeller, somewhat like Mr. Baker's B 35, you will see that there is a great improvement, and that the streamlines in that case are much more regular, also at the back of the propeller, and that they are almost the same as at the driving face. I think that a view of those models will show that it is interesting to study not only a section taken at the cylinder, but also a section taken along the diverging streamlines. These sections should be of a good aerofoil form, and they seem to be more important from this point of view than the ordinary cylindrical sections.

Mr. E. V. Telfer, D.Sc., Ph.D. (Member): Mr. Troost's remarks were extremely interesting. About a month ago I had a cast-iron propeller of a single-screw ship which had a most peculiar marking on it. It appeared to be covered with some old paint, and the whole of the streamlines were drawn on the propeller by the salt-water action, and leaving a whole series of granulations on the paint which showed the path of the water, absolutely endorsing what Mr. Troost has found in the model. On the driving face all the streamlines were perfectly coincident; I did not have a case where the driving face was also expanding radially.

Coming to Mr. Baker's paper, I would like to suggest that most of the findings of the experiments can be qualitatively predicted from the ordinary consideration of the vortex theory in relation to what is known as the resistance of an aerofoil. If you take the effect of the number of blades by going from three to six the result is to induce a more negative angle of incidence on the camber ratio screws, and the more the angle of camber ratio becomes negative, the worse is the blade resistance, and hence the efficiency of the screw will drop. On the other hand, once anything is done to keep that angle of incidence the same and to alter the lift of the blade by tilting, you get a definite improvement. Similarly for the thick segmental propeller. When you use that propeller as Mr. Baker did, you induce a very much smaller negative angle of incidence, and the thing practically becomes positive, and you get an improvement in resistance, which is explainable from the experiments. With the aerofoil propeller you cannot eliminate burbling, and by thickening the angle of incidence of the aerofoil propeller you do not induce any improvement.

I cannot follow Mr. Baker's remarks on gap. I do not think that gap in the sense that Mr. Baker has used it has anything to do with the question. I think that most of the things that he has been finding are purely concerned with this drag effect. When Mr. Baker fits a bi-convex section he merely kills burbling at one point. Again, when he refers to easing the back curvature, he is not easing the back curvature at all, but is merely easing the pitch of that section. If we apply the bi-convex section from the original face pitch, we would say that the back curvature has been eased in relation to that line. On the other hand, if we take the pitch at one section, which we are perfectly entitled to do, with that bi-convex section the back curvature is just as bad as before, and the real success of that section is due to its ability to cause burbling at the nose. I think if the paper was used in the light of eliminating burbling you would find that that is a much more consistent explanation than the influence of gap. From the general broad principle I do not think that the gap can be of very great importance. We are concerned with a propeller disc of about 4 per cent. of the total area, and to expect any great effect to arise from 4 per cent. momentum changes does not seem to be very reasonable.

Mr. Baker's investigation of the elemental efficiency of the blade is interesting. I think that is based on the ordinary vortex theory as developed by, say, Froude; but a complete vortex theory requires the influence of the blade with the contiguity factor coming more or less into it. When that is done you will find it is impossible to get these high efficiencies at the root. You have a blanketing effect—not a gap efficiency, but a blanketing. To get to high induced efficiencies at the root you have to come to low loads, and by coming to low loads you are coming to low driving ratios; so that you cannot hope to improve that type of screw. If you take this efficiency plot of Mr. Baker and compare it with the theoretical efficiency method, you will find that the theoretical efficiency is very considerably greater than the actual.

The final remark that I should like to make is in connection with the specification of the bi-convex section, which I mentioned yesterday in discussing Professor Abell's paper. Mr. Baker only takes a different value of the W as compared to the T. As I have attempted to show earlier, that is merely confusing the issue, because that particular chord, if accepted, should then have its face drawn parallel to it, and, instead of just having a standard type of pitch variation, we should allow a departure from the standard to obtain the face pitch adapted parallel to the chord. By that means you have a very simple method of getting the zero lift angle. There is a very simple formula connecting this type of section with the ordinary convex section, and you can very quickly get out the zero lift angle for that section, hence calculating the total pitch in relation to the face pitch. I think that if Mr. Baker goes through most of the models that he has made, on that basis, he will find that most of the actual face pitches that he has been envisaging have nothing to do with sections as such. The real difference between a segment and an aerofoil section is that one suffers from burbling at low angles of incidence and the other does not, and those two clear, broad classes should be very clearly distinguished in any such systematic investigation as the present.

I should like to thank Mr. Baker for his very excellent paper. As a buyer of Tank data I should like to ask Mr. Baker to comment on the difference between his own propulsive efficiencies and those which we can obtain from Japan, where we can go up to 90 to 95 per cent. efficiency, whereas in the Teddington Tank we are restricted to the modest total of 70 to 75 per cent.

The CHAIRMAN (Sir William J. Berry, K.C.B., Vice-President): I move that a hearty vote of thanks be accorded to Mr. Baker for his excellent paper.

WRITTEN CONTRIBUTIONS TO THE DISCUSSION.

Mr. J. F. ALLAN, B.Sc. (Associate-Member): In reading this paper one is impressed by three things: the wide spread of variation that has been covered by these experiments, the complicated nature of the results obtained, and the realization of the fact that it is still a matter of considerable experience and judgment, and, shall we say, a little luck in the selection of the best screw for a particular ship.

These three papers given us by Mr. Baker must represent a tremendous amount of work, and they cover bad screws as well as good ones, so that they present a good ground over which to try out a blade-element integral method, provided the lift and drag coefficients of the sections are available. It is important to develop some such method in order to determine the best possible propeller for given conditions.

On page 339 I should like Mr. Baker to explain the composition of the 1·27 factor used in determining shaft horse-power.

It is noted with interest on page 344 that in suitable cases the author commends the narrowing of blade tips, with consequent increase of camber ratio and efficiency, as this same suggestion was made in the writer's paper at the Spring Meetings.

A question may be permitted regarding slip angle. When the author uses the term, may we assume that it refers to what has normally been called "slip angle" in the past, or has a correction been made for inflow? It is a matter of great importance in relation to the lift/drag ratio of the section.

While agreeing with Mr. Baker that there is an important gap effect towards the roots of the blades, we prefer a different method of looking at the matter to that used in this paper. It appears to us that boss diameter enters only indirectly into the question, and that the controlling factor is gap/chord ratio as understood in connection with the wings of a biplane. This influence is a major factor not only at the roots, but practically all over the disc, and is not much affected by change of camber ratio of sections within practical limits. In our opinion the slight obstructive effect with which Mr. Baker deals at the roots is of less importance than the more widespread gap/chord influence.

Finally, the definition of an aerofoil section given in the Appendix could always be simplified practically by drawing the face tangent parallel to E G, and thus making z and x equal. Some other thicknesses might be added with advantage, as the nature of the curvature between H and the edges is of some importance.

Mr. M. P. PAYNE (Member): This paper is a thorough and fitting sequel to the important paper read by the author before the Institution at the Spring Meetings this year. A characteristic feature of the results of these tests of full single-screw ships is the excess, and in many cases large excess, of the propulsive coefficient above the screw efficiency, not only in the present investigation, but in investigations from other Tanks. It is clear from the results that a part of this advantage is due to a favourable wake, and a part due to the favourable relative rotative efficiency, the latter being as large as 9 per cent. above unity in the best cases. I am not clear from the paper whether these favourable results relate to the model with rudder, and would be obliged if Mr. Baker could inform us on this point, and also indicate the effect of the rudder.

The description of the apparatus and method of experiment is very interesting. Could Mr. Baker please tell us whether the brake effort, measured apparently on the fore side of the stern tube in the model, is corrected for the stern tube friction?

Another point on which information would be welcome relates to the number of runs made at each experiment speed. It is our usual practice to make five runs at each speed with varying revolutions when the screw is working behind the model, and I wondered whether Mr. Baker had some special reason for preferring ten different rates of revolution in his experiments. Perhaps the reason may be due to merchant ships generally being only required to run at and about one speed of advance.

Mr. W. HAMILTON MARTIN (Member): The author's opinion after the tests made by him at Teddington as to whether he considers that multi-bladed propellers (i.e. five or more blades of narrow fore-and-aft section) have definitely proved inferior to other types for application to high-speed vessels having direct drive by high-speed oil engines (or even for geared turbine-driven high-speed liners) would be appreciated. Would not the advantage to be obtained from a more efficient type of engine at the higher revolutions, a lighter one, direct coupled to a multi-bladed screw of a relatively good efficiency but much smoother operation, and thus causing less hull vibration, although turning at the higher revolutions, in the end be preferable to the normal three- or four-bladed screw of higher efficiency which demands a slower, heavy, and less efficient engine, or a high-speed efficient one requiring costly speed-reducing gear, which latter takes up frame space, needs lubrication, attention, increases risk of breakdown, adds to wear, etc., and not infrequently causes noise and sometimes adds to the vibration?

As to the effect of gap influence in the six-bladed propellers, Mr. Baker suggested that a much larger boss might reduce this. Is that not obtained at the cost of blade surface? It would seem possible on a normal sized boss to design the root of the blade of such a section as to be sufficiently strong, and yet give a better gap between blades than those blades tested by Mr. Baker of the six-bladed type. The blades of his propeller at the roots were practically as wide as the boss length. It would seem that if these blades were made of nearly semicircular shape at the root—having, however, their thickest part nearer aft, the section being more like that of an oval cut through its longer axis—this axis could become materially shorter than the length of the boss. The blades would then be narrower at their roots, and yet show a comparatively good gap area between them, with requisite strength.

Mr. Baker has a small eight-bladed propeller model of mine, designed by my father some forty years ago, and although eight blades are probably too much to be efficient, the gap area between these blades is relatively good, although its boss is of relatively small diameter, and would, I think, improve on Mr. Baker's six-bladed models if carried out somewhat like this one, with the oval-shaped root section as indicated by me.

The fact that some 1,500-H.P. high-speed patrol vessels of 400 tons displacement, and of very light scantlings, ran particularly smoothly (and showed very low coal-consumption, giving a wide cruising range) with 7 ft. 6 in. propellers of the six-bladed type at speeds up to $17\frac{3}{4}$ knots (12 knots cruising speed), and that the hull vibration was a minimum and least with six blades, after trying two, three, and four blades on these actual full-scale tests, would seem to prove that there must be something more in it than can be deduced from model propeller tests only. The Admiralty coefficient of these vessels was a high one, around 270. The relative efficiency of these six-bladed propellers was particularly good in these ships, as Mr. Baker knows from results submitted to him.

No Tank test of a model propeller can give any indication of the possible vibration effect of the full-sized propeller on the vessel. The vessels referred to not only had six-bladed propellers, but they had straight water-lines at both ends, very fine at the load-line and full below, with a wide beam, more like the Yourkevitch lines, so as to reduce their wetted surface.

Mr. Troost, of Wageningen, who designed the replace vessels lately, remembers the particularly efficient shape of those former ships, which are said to be still afloat, although over forty years old.*

Now that powers over 40,000 S.H.P. are considered, propellers with very large blade areas are being fitted in the largest liners. One wonders whether a limit has not been reached at which the influence of the wide blades revolving at high speeds may not set up undesirable vibration and hammering effects against the hulls and in the blades, or cavitation aggravate matters and erode their surfaces untimely.

If it were possible to design propellers offering less fore-and-aft width, and yet having a good disc-area ratio (the 7 ft. 6 in. diameter six-bladed ones referred to above in these patrol boats had a disc-area ratio of 0·4 and a pitch of 10 ft. 6 in.), it might prove possible to obtain a vessel with no perceptible vibration in the passenger quarters, which is certainly worth a little loss of efficiency in the propellers, while the resulting gain in

* See Trans. I.N.A., Vol. LXVIII., pp. 86–87 Vol. LXX., p. 140; Vol. LXXII., p. 180; Vol. LXXIV., pp. 152 and 159; Vol. LXXV., p. 263.

propeller revolutions enables either a lower gear ratio, meaning lighter gearing, or, what is better still, an increase in turbine speeds and a gain in efficiency at the forward end of the shafting, with corresponding saving in machinery weights.

It is the opinion of several people with whom I have discussed these matters that more may be expected from experiments on propellers in the direction indicated, and it is to be hoped Mr. Baker's experiments will be extended accordingly, and results be proved in practice with full-sized propellers.

Mr. F. McAlister (Member): Mr. Baker in his latest paper has now completed the work on the B series screws, and in doing so has made an extremely useful addition to the existing literature on the screw propeller.

The real value of the paper to the individual designer can only be assessed when each particular example is accurately analysed and the tendency of the results brought into line with existing theory. In this respect Mr. Baker has himself made a notable contribution in the latter part of his paper, but some of his deductions are open to general criticism.

For instance, in § 22 he recommends an average face pitch reduction of 20 per cent. in the root. Now face pitch, as a measurement, can only be likened to the "length between perpendiculars" in a cruiser stern ship—the measurements have no use in the general interpretation of performance, and are suitable only for constructional purposes. As an example, take screw B 35 in Table VIII.; here the root section has 50 per cent. forward-to-aft tilt from the arbitrary face pitch line reduced by 20 per cent. at this point, and the resulting symmetrical section has therefore a "zero lift angle" at this face pitch. As the face pitch angle is about $58\frac{1}{2}°$, the setting of this section for zero lift would be at this angle to the axis. On the contrary, with a root section such as B 29, in which the zero lift angle is about $12\frac{3}{4}°$, the setting of this section for zero lift would be $71\frac{1}{4}°$. The root sections have thus two entirely different variations in effective pitch, although having the same 20 per cent. reduction in face pitch, a fact confirmed by the 84·5 revolutions for 12 knots in the first, and only 80·8 revolutions for this speed in the second case.

One can put this discussion in another way. If Mr. Baker designed a screw with segmental sections to have the same absorption at 12 knots with the 84·5 revolutions of B 35, he would have to reduce his root pitch by no less than about 50 per cent.!

We thus see that the notation W and T of Mr. Baker's sections is related in an absolute manner to his face pitch, and the reduction of this face from the normal face pitch used as a basis. It is quite possible, therefore, that a propeller of greater root reduction than 20 per cent., with higher forward tilt and lower after tilt, may not only give precisely the same result as B 35, but may be precisely the same propeller. It is thus not possible to be too general in stating that 20 per cent. pitch reduction is an optimum, or that it really means anything at all unless qualified by type of section.

There are many other questions and controversial points one could raise, such as the statement that the propulsive coefficients are for a condition a little worse than "average weather at sea," and whether (rake + skewback) is a valid parameter, but time does not permit me to develop the matter.

May I, in closing, make a plea that whilst from a pure research point of view it is of interest to investigate all variations in thickness, yet, as most propellers are of bronze or cast iron, it would be of greater benefit to naval architecture generally if model screws were tested with thicknesses and root sections giving a reasonable tensile stress for the different materials.

I trust that Mr. Baker will see his way clear to add to our knowledge of propellers by making further extensions and additions to his remarkable B series from time to time.

Mr. G. S. Baker, O.B.E. (Member of Council): The work which Dr. Horn has briefly mentioned has been going on for some time at the Berlin Tank, and we have been able to study the results published in *Schiffbau*. Dr. Weitbrecht has been good enough to let us have the details of the screw with the pitch distribution marked (2) in Dr. Horn's sketch, and one or two screws of this type will be tried in our B series. I find it a little difficult to believe (1) that the race motion can be so simply dealt with as in the second formula of Dr. Horn's, (2) that these two components of energy can be lumped together and their relative values be ignored, and (3) that the effect of balancing the race loss on the thrust distribution over the blade is negligible.

One serious objection to elaborate calculations in designing a screw is the time taken, and when the work is complete there are still many serious surmises in it.

Both Dr. Horn's and Mr. Yamagata's method involve the complete measurement of the wake round the stern of the boat over the whole of the screw disc and getting this all faired up before a start can be made. Mr. Yamagata went to the extent of running a preliminary test before designing his final screws. The purpose of our research is to develop a method of quickly designing propellers for firms at a price and within a time which mercantile firms can afford. All this elaborate preliminary work is impossible, and after measuring the wake curve on three models we believe it is sufficiently accurate to assume a representative wake curve of the type given in the paper.

I am indebted to Mr. Troost for his remarks as regards the confirmatory results he has obtained in Holland with screws of type B 34. Both Dr. Horn and Mr. Troost commented on the fact that we did the model tests without a rudder or fin behind the model. But the purpose was to measure the effect of the changes in the propeller when tried behind the hull, and not the compound effect involved by introducing a fin. Dr. Todd has been carrying out some tests on fin effect, some of which have been done on this same model, and he will be giving these results in a paper this autumn.

With regard to Dr. Lock's comments, we have never attempted to work out the tip losses on a screw propeller. These must vary considerably with shape and width of blade tip, and I doubt whether it can be done for broad blades with the accuracy necessary to make it worth while. He suggested that a circular-back section as good as an aerofoil one could be found for any given working conditions. At any radius of a propeller it is required that the lift constant multiplied by the breadth of the blade shall have a certain value. As stated by Dr. Horn, one chooses the section with the lowest k_D for the particular camber ratio, and it must hold a good value of the ratio k_L/k_D for a range of slip ratio on both sides of the mean slip angle, and the circular-back section fails except possibly at high slips.

Next with regard to Mr. Troost's most interesting experiments with wet paint on a propeller. This paint is swept by the water into a pattern on back and front of the blades. For these to denote the true stream flow through the propeller it would be necessary to reduce the paint viscosity (or adhesiveness to the blade) to a very low figure. The paint is subjected to two forces, one due to the wipe of the passing streams, the other due to the centrifugal force conveyed to it through its adhesion to the rotating blade. I think this latter force could sometimes spoil such an experiment.

My only comment on Dr. Telfer's painted propeller is that you must first know how the marking came about, and then maybe you can find an explanation for it.

Dr. Telfer states that there is no such thing as "gap" effect. But towards the end of his remarks he spoke of a "blanketing effect" due to the blades being too near to each other. I wonder if there is any difference here except the form of word used. If he studies the paper he will find that it contains clear evidence of this gap effect.

With regard to the suggested method of defining a blade section, which he and Mr. Allan have both made, it has at first sight the value of simplicity, which is good. But his method involves working out different pitches all over the blade, and would in some cases lead to more foundry work. The pitch line, if drawn through the extreme ends of any section, would not be the base of the blade, and the face of the mould would have to be cleaned out after it had been strickled up.

Lastly, he spoke of the difference between our propulsive coefficients and Mr. Yamagata's, which rose in one case to 0·91. I am not responsible for Mr. Yamagata's data; that is one matter. Further, we have endeavoured to check what we are getting on the model against the ship, and I believe that these propulsive coefficients are those which are being derived on a ship, and, therefore, we would have no authority to raise them. If you look at the propellers which Mr. Yamagata is using, they are not materially different from some which are described in this paper, nor is his material different; but I hope to speak on that to-morrow when he reads his paper. I think it would be unfair to discuss his paper here without waiting to hear it.

In reply to Mr. Allan, one great requirement of the moment is a series of curves of k_L and k_D in water, particularly of a camber ratio suitable for screw blade tips. I should not like it to be assumed that I "commend" the narrowing of blade tips, but rather that wide tip blades lose efficiency, and a little reduction of tip width enables one to skew back the blade.

The term "slip angle" has many meanings; in general work it means the true slip relative to the mean wake. But in the formula (4) the inflow must be taken into account, as I have explained in my book.*

It is quite true that there is a gap/chord effect, but towards the blade tips it is obviously small, as the stagger of the blades is large, and there are some experimental data (in air) which support this. At the blade root this effect would necessarily be part cause of and be merged in what I have termed simply "gap" effect.

The constant 1·27 used in calculating horse-power in Table I. is intended to take account of the resistance of a plate rudder or rudder-post, bilge keels, wave and wind resistance in average weather at sea.

In reply to Mr. Payne, the model had no rudder on it, and the wake effect, which, of course, was the cause of the high hull efficiency, is genuine hull wake. In a paper read before the Institution in 1927,† in which a propeller was tried behind a long flat plank, the thrust deduction was nil, and in that case a hull efficiency of $(l + w)$, where w was 0, was obtained, and this was checked against pitot tube measurements of the wake.

The method of measuring the friction of the gear takes into account the stern-tube friction. Our object in making tests over a rather wide range of revolutions is to give us sufficient curve to cover both the model and ship self-propulsion points, and in some cases to make sure that with high loading (equivalent to bad weather) the screw shows no sign of change in thrust law.

Mr. Hamilton Martin's question *re* number of blades is one involving more things than water performance. On two occasions we have designed five-bladed screws of as high efficiency as for four-bladed ones, and in some cases of *bad vibration* they would be useful in eliminating it, if due to the propeller. The little eight-bladed screw of Mr. Martin's father had some very low camber ratios, much lower than could be used in this particular case. I have dealt with this effect of thickness and boss diameter in the paper. In certain cases I think there is no doubt that reasonably good efficiencies could be obtained with five- or six-bladed screws.

I am a little lost over Mr. McAlister's remarks. There is some confusion here between slip and pitch angle. It is quite true that the zero lift angle of the root sections of B 34 and B 35 differ by a few degrees, and this is partly the cause of the change in revolutions passing from B 34 to B 35. If segmental sections are substituted, the angle must be that for the requisite k_L. I quite agree with Mr. McAlister that in some cases a section with larger wash back at the leading edge and smaller slip could be substituted for B 35, and for that reason the 20 per cent. reduction should be viewed broadly and not as a fixed thing.

* *Ship Design Resistance and Screw Propulsion,* Vol. II, 1933.

† Trans. I.N.A., Vol. LXIX, p. 275.

To Illustrate Mr. G. S. Baker's Paper on "The Design of Screw Propellers, with Special Reference to the Single-screw Ship."

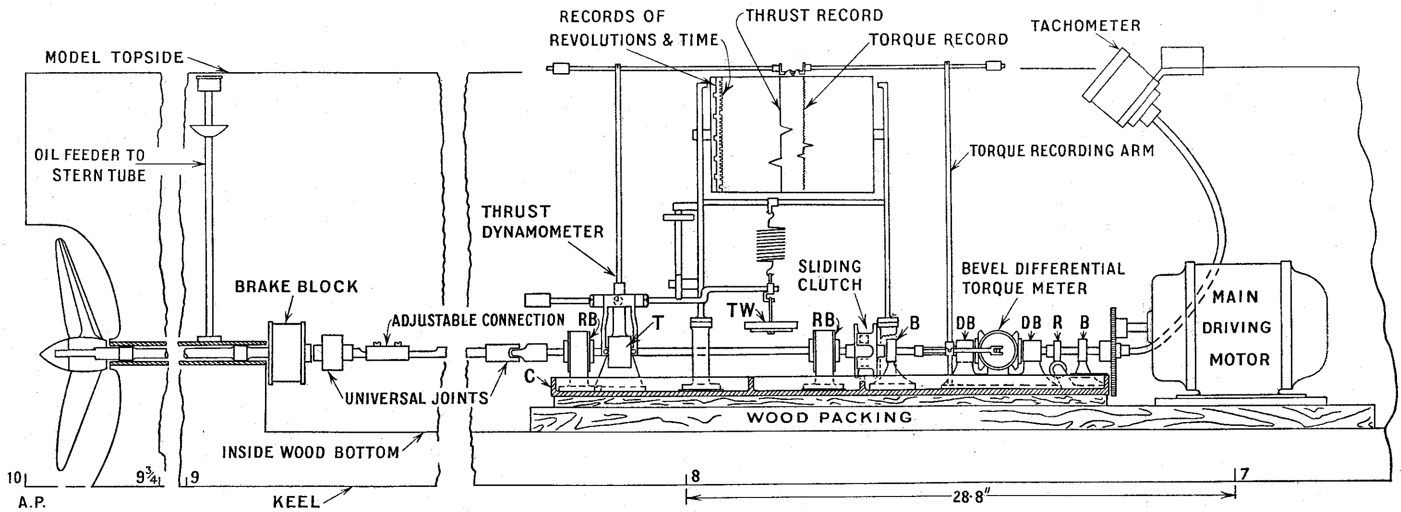


Fig. 1.—Apparatus for Measuring and Recording Propeller Action in Self-propelled Hulls.

B Single Ball Bearing. C Cast-iron Base Plate. DB Double Ball Bearing. RB Roller Bearing.
T Double Ball Bearing taking Shaft Thrust. TW Thrust Weight. R Revolutions Counter.

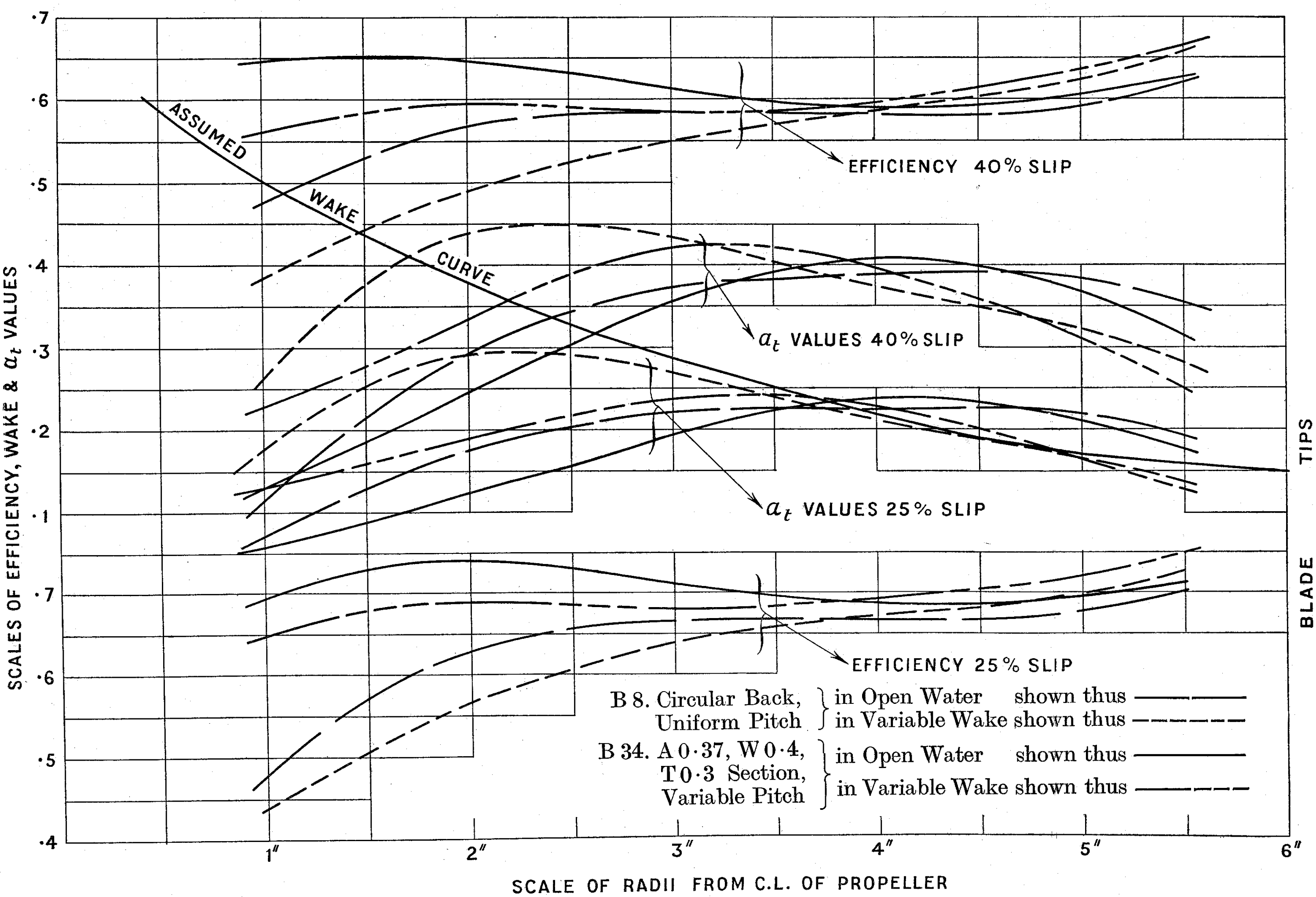


Fig. 5.—Curves of Efficiency and Intake Velocity a_t at Different Radii on Screw Blades in Open Water and in Variable Wake.

PROPELLER CAVITATION STUDIES.

Lieutenant-Commander C. O. Kell (C.C.), U.S.N., Experimental Model Basin, Washington, D.C.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 11, 1934, Sir William Berry, K.C.B., Vice-President, in the Chair.]

Abstract.

Based upon the information that has been obtained from experiments conducted in the variable pressure tunnel at Washington during the past two years, the different forms of propeller cavitation are discussed. The characteristics of face cavitation and two forms of back cavitation are described. Methods of correction as far as are known are suggested.

Tests to determine the effect of gas in solution in water on propeller cavitation are described, and the results are given.

Finally, the calculated lift coefficients for typical propellers at the apparent inception of back cavitation are plotted against cavitation indices, in an effort to obtain a criterion by which back cavitation on propellers may be avoided in design work. These data are compared with a theoretical relation between the lift coefficient and cavitation index, which has been offered by Lerbs as a criterion.

Introduction.

Two years ago Captain E. F. Eggert (C.C.), U.S.N., presented a paper [(1)*] before the American Society of Naval Architects and Marine Engineers on the subject of "Propeller Cavitation." In this paper he discussed the limitations of criteria that had been proposed prior to that time for predicting the speed at which cavitation in marine propellers would occur. In place of these earlier criteria he offered a formula by which we can determine the approximate speed at which back cavitation becomes serious for any ordinary type of submerged propellers. This formula was based upon the results of research work done on the subject until that time in the variable pressure water tunnel at the Experimental Model Basin in Washington.

Since the publication of Captain Eggert's work it has been my privilege to have been associated with the work at the tunnel. This occasion affords a most acceptable opportunity for me to record briefly the developments in the study of cavitation at the Washington tunnel during the past two years, and to benefit from discussion with those who have been engaged on similar work.

Since the installation of the Washington tunnel, work has been constantly carried on to perfect the methods of testing and the development of more efficient equipment for making more accurate measurements. We are aware of the limitations of our present methods of testing, and we do not hold that our results at present are not subject to correction. We

* See Bibliography at end of paper.

are in need of more results from ship trials with which to compare our tunnel tests before we can feel certain that our test methods are such that the results check satisfactorily with the ship performances. We realize that there yet remains much to be accomplished in this direction, but we do believe that the changes that have been made and the work that has been done have resulted in progress in the study of cavitation and the building of better propellers. I refer to these developments because for the most part they are responsible for our obtaining more accurate data upon which to base our studies. The most important of these developments are the more reliable measurements of the speed of water past the propeller in the tunnel, more dependable torque records, and more accurate pressure measurements within the tunnel. The present measurements of water speeds in the tunnel make possible closer approximations of the true slip ratios that are obtained in self-propelled tests. These are vital in obtaining the correct characteristics in the development of cavitation on the test propellers. In an effort to check the accuracy of the representations of cavitation obtained in the tunnel, an observation port has been built in the shell of one of the United States destroyers. From this we hope to study the development of cavitation on propellers of different designs. Another addition which has proved most helpful in our studies is a stroboscopic light, which has made possible quite satisfactory continuous observations of propellers under test. In this way greater accuracy is obtained in determining the speeds of advance at which cavitation becomes apparent, and the manner of inception of cavitation and its development through the range of higher speeds can be studied. It will be recalled from an earlier description of the water tunnel at Washington[(2)] that the tunnel is of the open jet type, which makes possible the illumination of the propeller chamber by exterior light.

Discussion of Forms of Cavitation.

In the past two years a great many propellers have been tested. The different types of cavitation have been clearly and consistently defined. These are face cavitation and back cavitation in its two forms. I should like to briefly discuss each of these forms of the phenomenon, but before doing so it is perhaps well to describe the methods of testing used when these data on the beginning and the developments of cavitation were obtained.

In all standard tests in the tunnel the procedure in model basin self-propelled tests is followed. From the data obtained in the self-propelled tests in the basin the thrusts at the different speeds are calculated from the E.H.P. curves, corrected for the corresponding thrust deduction factors. The correct speeds of advance are also determined, using the wake fraction values obtained in the self-propelled tests. In the tunnel tests the proper pressure according to scale is maintained constant throughout the tests. At each speed the corresponding speed of advance is maintained as constant as possible in the tunnel. The corresponding thrusts are maintained on the dynamometer by a minimum of regulation of the revolutions. Each observation is made for the duration of one minute, and the data are required to be repeated before being accepted. Some slight regulation in revolutions is found necessary in the test interval to maintain a given thrust. This is due to the fluctuation of voltages of the power used at the tunnel. Power readings and revolutions for each observation interval are read from the dynamometer. The advantage of this form of test is that, if correctly done, it gives the sequence of developments of cavitation as they take place on the ship. The results of these self-propelled tests in the water tunnel agree closely with those made in the basin up to speeds at which the efficiencies of the propellers become affected by cavitation.

Face cavitation, if it be present, is found to exist at the lower or throughout the so-called cruising speeds, when minimum slip ratios occur. If the slip be low, the blade sections abnormally thick with sharp leading edges, and with blades close together as in the case of four-bladed propellers, face cavitation will probably exist. If so, it may continue throughout quite a range of speeds, depending upon the increase in the slip ratios with increase in speed. Face cavitation generally sets in at the root sections. This may be accounted for as being due to the effect of blade interference and to the thicker sections at the base of the blades.

Perhaps the most important fact about face cavitation which our data have established is that its presence does not have any effect upon the efficiency of the propellers that we have been able to measure. A great many model propellers which we have tested have been found to be in face cavitation, and in no case when face cavitation has been observed have either the revolution or the power curves in self-propelled tests been affected by it. I believe that our data have answered the question once and for all as to whether or not face cavitation has any appreciable effect on the thrust of a propeller. The fact that it does not can be understood when we remember that it is the back of the propeller blade and not the face that governs in propeller performance.

No doubt there are those who will find it difficult to accept these results as conclusive on the question. They have witnessed instances in which the power of a propeller was affected by cavitation, and the propeller was known to be working in face cavitation from the markings on the blades. In explanation of such cases, it is probable that face cavitation did exist at the lower speeds, and that back cavitation set in at higher speeds, and was the cause of the loss of power. In fact, we have frequently found that both face cavitation and a form of back cavitation exist simultaneously through a short range of speeds. In such cases, as the back cavitation develops and causes the slip ratios to increase by causing the efficiency of the propeller to fall off, the face cavitation decreases, until finally it disappears. When this occurs there is no change in the slopes of the power and revolution curves which would indicate any effect of face cavitation.

What I have said thus far must not be taken to infer that we have concluded that face cavitation is not objectionable and need not be avoided. We know that it is a cause of erosion on the blade face, and we suspect that it may be the source of objectionable and even serious vibrations. We hope to be able to answer this question after full-scale tests.

For these reasons it is fortunate that face cavitation can be easily corrected and avoided. If the propellers be operating with a reasonable slip ratio—that is, not much less than that corresponding to maximum efficiency—to correct face cavitation it usually has been only necessary to round the leading edges of the root sections of the blade. At most, it has been necessary to set back the face of the blade, beginning a short distance back from the leading edge. Because of the shape at leading edges, propeller designs having aerofoil sections do not show face cavitation when operating at or near normal slip.

While face cavitation is not believed to be very serious, in that it can be avoided in properly designed propellers, back cavitation is quite a different matter. Back cavitation in definite stages of development does have a tremendous effect on propeller performance. In discussing back cavitation I wish to consider separately the two forms of it which we have observed at Washington, because the two forms have different characteristics in their inception and development, and they affect propeller performance differently. The first of these forms of the phenomenon is that which first appears as vortices off the blade tips, and develops into leading edge cavitation with increase in tip speed and thrust. The other form of back cavitation is that which first appears as bubbles striking on the following part of the blade back, with reference to the maximum thickness of the blade sections. To distinguish this form of the phenomenon from leading edge cavitation, it will be referred to as "burbling." The word is taken from the name of the analogous phenomenon in aerodynamics.

Consider, first, back cavitation at the leading edge of the propeller blades. It is known to occur at the leading edge and at the blade tips because the lowest pressures on the blade back exist there. With the tip sections of the propeller operating at a high slip, the flow of water about the leading edge of the sections comes from the pressure side of the blade, and at high tip speeds it is unable to round the leading edges of the tip sections and follow the contour of the back of these sections. The rate of development of the phenomenon is dependent upon the shape of the tip sections of the blade, in particular the form of the leading edge, the angle of attack, and the tip speeds. Leading edge cavitation first appears as vortices passing off the blade tips, which form helices in the propeller wake. With each increase in speed and thrust, the pressure in the tunnel remaining constant, there is a corresponding development of these vortices until what appears to be an independent flow is formed across the blade tips, and bubbles appear down along the leading edge of the blade. As the speed is further increased, the width of the flow across the blade tips increases, and the trailing edge of this flow passes off into the propeller wake as wider bands of vortices. The cavitation along the leading edge becomes wider, and finally, when well developed, extends across the entire back of the blade as a part of the independent flow which began at the tip sections. This flow remains intact until it is well clear of the following edge of the blade.

The development of leading edge cavitation on propellers is very similar to that described by Walchner [(3)] and Ackeret [(4)] in their tests with hydrofoils having sharp leading edges, except in one particular. Walchner found that with the development of cavitation on a hydrofoil, set at a given angle of attack, the origin of cavitation moved from the leading edge of the foil to the section of maximum thickness with decrease in the cavitation index. This, he explains, is due to the shifting of the stagnation point with the beginning of cavitation from the pressure side of the foil to a position in front of the leading edge. No similar phenomenon has been observed in our model propeller tests. It is true that "burbling" has been found to set in on the following half of the back of the blades, in some instances while leading edge cavitation existed. However, the inception of "burbling" did not decrease the intensity of leading edge cavitation in any case. It is believed that the explanation of this is that with propellers designed for ships of ordinary types, any change in the angle of attack after cavitation has set in is an increase, which tends to aggravate the conditions that cause leading edge cavitation.

Interesting facts about leading edge cavitation are that it can exist on the wider blades through a wide range of speeds without affecting the efficiency of the propeller, but after the efficiency begins to fall off, progress toward general breakdown of the propeller action is rapid.

It has been found possible to delay the beginning of leading edge cavitation, and in some cases to confine it to tip vortices, by methods which involve some sacrifice in efficiency. It has not been possible to do much towards its correction by altering the form of the thin blade sections at the tips. It can be most effectively delayed or reduced by decreasing the angle of attack at the outer sections of the propeller blades. This can be accomplished by increasing the propeller diameter and accepting a lower pitch ratio with consequent lower efficiency, or by twisting the blade to obtain lower pitch ratios at the outer blade sections.

The second type of back cavitation, which will be called "burbling" in this discussion, is that which forms on the trailing part of the blade, speaking with reference to the maximum thickness of the sections. It is the most interesting form because it presents the most difficult problems for solution.

In the tests of model propellers it has not been possible, thus far, to positively determine the location of the origin of this phenomenon. Our experiments with hydrofoils indicate that the origin is at the maximum thickness of the blade sections. This was also indicated by Walchner [(3)] and Ackeret [(4)] in their tests of hydrofoils. The characteristics of the phenomenon in flow and extent as described by Walchner and Ackeret correspond with our observations on the model propellers only after the "burbling" type of cavitation has been fully developed, and long after the efficiency of the propellers has been affected by it. It is interesting to note here that Sir Charles Parsons succeeded in photographing the different forms of cavitation on propeller models. These records were published by Mr. S. S. Cook [5]. His record of the "burbling" type of cavitation on the blade back is of particular interest because it appears to be identical to that which we have so often observed in the Washington tunnel.

At the first appearance of "burbling" on the propeller model we see intermittent impacts of bubbles on the following half of the back of the blade. It is believed these are condensation impacts, and occur when the bubbles of cavitation reach the zones of higher pressure and collapse with some force on the blade back. The first of these bubbles is most generally seen near the trailing edge of the blade in a definite small area. If the root sections be abnormally thick, "burbling" usually appears first near those sections. It has never been observed nearer the tip sections than about seven-tenths of the radius from the centre of the propeller. With decrease of the cavitation index $\frac{p_a}{q}$,* accomplished by increase in speed, with pressure constant, the area affected is found to expand both radially and across the width of the blade towards the maximum thickness of the blade. When completely developed, most of the following part of the back of the blade is affected and the trailing edge of the combined cavity formation is clear of the blade. At this stage it corresponds to the description given by Walchner [3] and Ackeret [4]. But even in this stage of development the cavitation is confined to the following half of the blade. This was investigated by an interesting series of propellers which were similar in all respects except as to the location of the maximum thickness in the width. In one of these propellers the maximum thickness was located at one-third of the chord length back of the leading edge; in a second one the maximum thickness was in the middle of the chord length as in the typical ogival section. In the third propeller of the series, the maximum thickness was at two-thirds of the chord length from the leading edge. The three propellers were given identical tests. The first propeller, with the longer slope from the maximum thickness to the trailing edge, broke in efficiency at the lowest speed, and its rate of decrease in efficiency was more rapid than in the other two propellers. The third propeller, with the greatest slope in the following half of the blade back, showed the greatest resistance to the effect of cavitation. This is perhaps due to the smaller percentage of the area of the back being affected in the third propeller.

It has been found in some tests that as the propeller efficiency falls off due to "burbling," with the resulting increase in slip, leading edge cavitation may also develop. In such cases, leading edge cavitation tends, with the "burbling" type, to lower the speed at which the complete breakdown of the propeller occurs.

The "burbling" type of cavitation is most sensitive to small changes in the cavitation index. This characteristic is responsible for what appears to be a condition of instability in performance of the propeller within cavitating range of speeds. In some cases this condition of apparent instability has made it impossible to accurately repeat test data within the range of speeds in which the "burbling" type of cavitation exists. This makes it very difficult to predict accurately the power required for propulsion after this type of cavitation has set in.

Experiments with model propellers of the destroyer type have been made that show that a difference in static head of 3 ft. of water makes a very measurable difference in the cavitation index at which "burbling" occurs, and in the rate of its development. This is significant, in that it indicates the necessity of estimating as accurately as possible the depth of submergence of the propellers at the different speeds, when tunnel tests are used to predict the power of ships.

* p_a is the absolute pressure in the tunnel, $q = \frac{\rho}{2} v_r^2$, the dynamic pressure, in which ρ is the density of water and v_r the relative velocity of flow to the propeller blade sections.

Effect of Dissolved Gases in Water on Cavitation.

Tests have been made in the water tunnel to determine if the cavitation of propellers might be affected by gases dissolved in the water. Five model propellers were tested for cavitation in the tunnel in ordinary tap-water by the standard self-propelled methods. All tests were made twice under the same conditions for the purpose of checking results.

Upon completion of the tests in tap-water, carbon dioxide gas was bubbled through the water from the bottom of the tunnel from a cylinder containing the liquefied gas at the rate of about 15 lb. of gas an hour until a concentration of about 120–130 c.c. of gas per litre of water had been attained. This gas content was about twice the gas content of sea-water in the North Atlantic at a temperature of about 60° F.

The self-propelled tests with the five model propellers were then repeated with the water saturated with carbon dioxide, the conditions of the tests being identical with those of the previous tests, except for the dissolved gas. The average temperature of water for all tests was 60° F., with a maximum difference at any time of 4° F. An analysis of water for CO_2 content was made before and after each test. Gas was added as necessary to keep the gas content approximately constant.

Of the five propellers tested, the results of tests from only one showed any differences that might be attributed to the presence of the dissolved gas. The results for the four remaining propellers showed no divergence other than might be due to experimental error.

Although the results of the tests with four of the propellers were consistent in showing no effect of the gas, the results obtained with the fifth propeller raise the question as to whether or not the interval of time in which a given particle of water saturated with gas moves in the low-pressure area is the factor which determines the effect that gas in solution may have on cavitation. This particular propeller, because of its characteristics, cavitated normally at a speed of advance approximately 70 per cent. of the speed at which the remaining four propellers cavitated. Furthermore, the corresponding tip speed of this fifth propeller was about 80 per cent. of that of the other propellers. Since the length of time that a given particle of water was in the low-pressure areas in the back of the blade is determined by these speeds, it follows that in the case of the fifth propeller greater time was available for the separation of the dissolved gas than was available with the other four propellers, and a greater liberation of gas might be expected. Further, cavitation in this instance was more general in extent. The greater spread of cavitation across the back of the fifth propeller perhaps meant greater permanency of the cavity formed, and with gases remaining longer in the cavity their effect may have been cumulative.

For the full-scale propeller the speed of advance and tip speed are increased above those of the model in the ratio of $\lambda^{\frac{1}{2}}$, where λ is the linear ratio between the model and full-scale propeller. The dimensions of the full-scale propeller are λ times greater than those of the model, likewise the length of the low-pressure region. Therefore at corresponding speeds the length of time a particle of water is in the low-pressure region on the full-scale propeller is $\lambda^{\frac{1}{2}}$ times greater than on the model propeller. Therefore it is possible that gas dissolved in water may affect cavitation in full-scale propellers and yet show no effect in tests with the model propellers.

Comparison of Results and Discussion.

From the results of our experiments thus far, it is apparent that back cavitation may first set in at either the tip sections or at any of the thicker blade sections. In propeller design it is therefore necessary that some relation between the cavitation index and the lift coefficient be established whereby cavitation can be avoided. To that end, experimental data have been plotted (Fig. 1) in terms of a cavitation index and lift coefficient for the sections of the test propellers at which leading edge cavitation or "burbling" was first observed. In the case of leading edge cavitation the lift coefficients were calculated for blade sections at 0·9 of the radius from the centre of the propeller. These data are compared with a theoretical curve derived by Lerbs[(6)].

In each case the cavitation index $\frac{p_a}{q}$ was determined as follows. p_a is the absolute constant pressure value at the shaft level that existed in the tunnel during test. This value is the sum of the atmospheric pressure (33 ft. of water), the head of water above

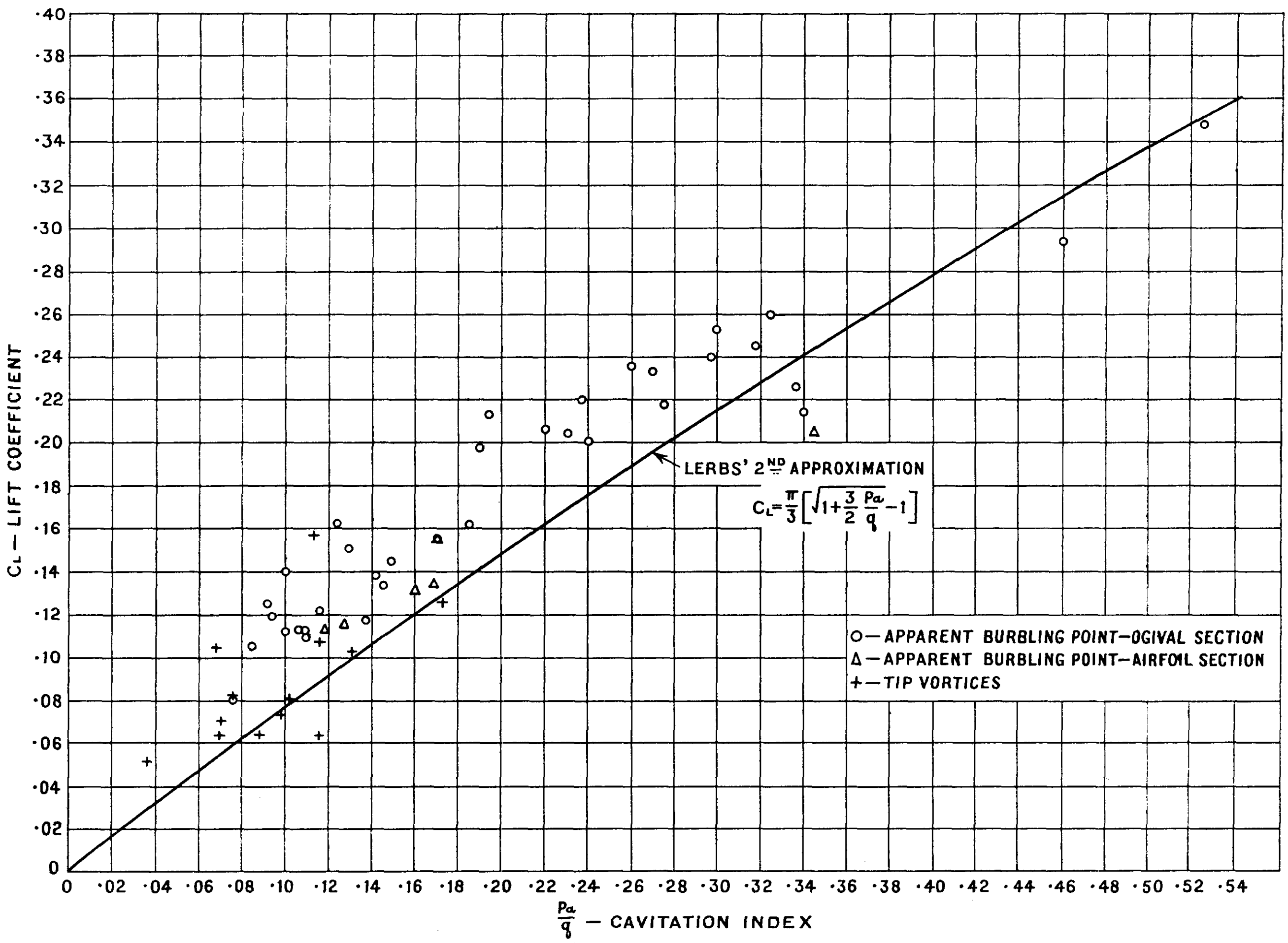


FIG. 1.

the centre of the propeller shaft minus vapour pressure corresponding to the water temperature reduced to scale as required.

q is the velocity pressure, and was computed by $q = \frac{\rho}{2} v_r^2$, where ρ is the density of water, v_r is the relative velocity at the section of the blade at which cavitation was first observed, and is determined according to Eggert's method[(1)].

$v_r^2 = K(\pi f d n)^2$, in which $\pi f d n$ is the speed of the blade section expressed in feet per second. K is the secant squared of the angle between the direction of flow v_r and the plane of the propeller section.

In this, the angle of attack has been assumed to be that corresponding to half the slip, since the inflow velocity increment is assumed to be half of the velocity increase due to slip. Any error in this assumption will be of little consequence in the value of the relative velocity v_r, since the increment of the forward velocity due to the term $\frac{1}{2}\, s\, p\, n$ is a small fraction of the whole. f is the estimated fractional part of the propeller radius, at which " burbling " first became apparent. In the case of leading edge cavitation $f = 0{\cdot}9$.

Then

$$v_r = [(\pi\, f\, d\, n)^2 + (v_a + \tfrac{1}{2}\, s\, h\, n)^2]^{\frac{1}{2}}$$

where $v_a + \frac{1}{2}\, s\, p\, n$ is the forward velocity at the propeller,
v_a is the speed of advance,
h is the pitch ratio of the blade section concerned,
s is the true slip ratio of the blade section,
n is the revolutions per second.

The lift coefficients C_L plotted in Fig. 1 were computed by the formula proposed by Captain Eggert [1].

$$C_L = 5{\cdot}5\,\frac{a + c}{1 + 4{\cdot}4\, b},$$

where a is the angle of attack in circular measure, b is the mean width ratio according to Taylor [7], c is the negative angle of incidence at which the lift becomes zero. This angle is closely measured by the angle between the chord and the median line of the tail of the blade section. In ogival sections this is equivalent to the ratio of the maximum thickness to its chord length or thickness ratio. In so-called aerofoil sections, in which the maximum thickness is located one-third of the chord length back from the leading edge, the angle is more nearly three-fourths of the thickness ratio.

The cavitation index $\frac{p_a}{q}$ values in this plot are approximate because of possible errors in the observation of data. The observed data from which the cavitation indices and lift coefficients were computed were the speeds of advance at which cavitation was first seen and the estimated position of the section of the blade at which cavitation first appeared.

In the same plot is given the equation derived by Lerbs [6] as being an approximation of the limit of lift relative to the cavitation index above which cavitation cannot be avoided. His equation is

$$C_L = \pi/3\left[\sqrt{1 + 3/2\,\frac{p}{q}} - 1\right]$$

In his paper [6] the author uses the " velocity of undisturbed flow " in computing the velocity pressure q. I understand this to be the speed of advance. Inasmuch as his approximate value of C_L is only a function of the cavitation index $\frac{p}{q}$, it is possible, for the purposes of comparison with our observed data, to plot his equation in terms of relative velocities, which has been done.

The apparent cavitation point of most of the model propellers plots above this approximate theoretical maximum lift. This may be attributed to the approximations, referred to above, in estimating the speeds and positions of sections at which cavitation first appears. It is quite probable that the cavitating point is reached at a speed at least one or two knots below that at which cavitation becomes apparent with sufficient definiteness to warrant recording. A corresponding increase in the cavitation index would cause the observed points to fall much closer to Lerbs' theoretical line. It will be noted that data obtained from propellers affected by leading edge cavitation plot nearer to the theoretical line than the data obtained by instances of " burbling." It is believed that this is due to the closer approximation of the correct speed at which leading edge cavitation began, and that no error was made in estimating the location of the blade section, at which cavitation developed. However, these differences as plotted are considered to be of little practical importance. From Fig. 1 it is apparent that Lerbs' equation can be accepted for the present as a danger line, below which we shall, in all probability, be free of cavitation. It does, however, represent a good margin of safety below the speed and thrust at which the propeller efficiencies would be affected by cavitation.

It is interesting to observe here that the unit thrust values for the propellers included in these data at which cavitation became apparent varied as much as 50 per cent. There was a similar spread in the relative velocities at the blade sections involved. The data make it clear that in propellers designed to operate at high speeds the lift per unit area must be kept down if cavitation is to be avoided.

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DISCUSSION.

Dr. J. C. Hunsaker: It should be of interest to add something from the academic and scientific side to the insight on cavitation that our experiments give. At the Massachusetts Institute of Technology we have been concerned for the last two years with research to determine the nature of cavitation, what makes it, and how it can produce its effects. To do this on turbine runners, or propellers, seems out of the question, because the phenomena are there too complicated. We have developed a technique borrowed from aeronautics to reduce the problem to a very elementary form. Through a Venturi nozzle in a pipe, water is forced at varying velocity, varying temperature, and varying air content. There are means for observing at leisure the distribution of pressure along a profile and glass sides to the experimental chamber or Venturi for viewing and photographing the phenomena.

We have found that the distribution of pressure along the profile is significant for cavitation. The average pressure of the stream falls, as it should, towards the throat to a low value, following Bernoulli's equation, and in the expanding region below the throat it will rise again almost to the original value, provided that there is no cavitation and continuity in the flow is preserved. If the flow be speeded up, or the static pressure on the whole stream be lowered until the mean pressure at the throat comes down to 20 mm. of water, the vapour pressure for room temperatures, the picture instantly changes, and instead of the pressure rising in the expanding portion it fails to rise, but remains low for some distance below the throat. At this same time we see a cloud of white steam forming, indicating that we have lost continuity. We have a flash from the liquid phase into the vapour phase in the region of low pressure, and, as the condition is aggravated by higher speed, this cavity of steam will extend farther downstream below the throat and, simultaneously, the low pressure will extend farther downstream. We have excellent correlation between the point of maximum rate of rise of pressure downstream and the visual end of the cavitation volume. The end of the cavitation volume is also opposite the point on the side walls where maximum damage is done to the metal when cavitation is present. The explanation seems to be that water vapour, formed under conditions of low pressure and room temperature at the throat, has proceeded downstream to a place where pressure is higher. It has gone against a rising pressure gradient and is virtually super-cooled. It is unstable and must go back into the liquid state, which it does at a particular region with considerable violence.

We have measured the sound and shock when cavitation is well developed and have a record of acoustic vibration, with heavy peaks of amplitude with a definite frequency of the order of 100 cycles per second. We photographed this white cloud of steam with Edgerton's technique of high-speed motion photography at five thousand pictures per second, and saw that the white cloud was not purely a white cloud, but vanished and reappeared with the same frequency as the shock measured by acoustic and pressure means. The volume of steam collapses back to water and gives rise to a series of trains of pressure waves which appear to cut off momentarily the generation of steam at the throat. The average pressure at the throat is the steam pressure, but one hundred times a second that pressure is higher and lower. The steam appears to be pinched off and turned on again, and so we have a trigger action with a consistent periodicity. Experiments with several profiles show this frequency to vary directly with the velocity and inversely with a linear dimension. The non-dimensional number $f L/V$ is substantially constant.

Cavitation effects we find always downstream and always at a preferred location. A profile will show cavitation, but do no damage if the collapse takes place in clear water and not in contact with the walls.

I suggest, in connection with marine propellers, that the observed loss of efficiency which comes with well-developed cavitation must be due to a change in pressure distribution such as we observed.

With higher air content in the water the violence of the cavitation damage is reduced. We know that with chemically pure water the collapse of vapour to the liquid phase is much more violent. We have made tests in which the damaging effect of cavitation is increased 400 per cent. by reducing the air content of the water from 4 per cent. to $\frac{1}{2}$ per cent. Further experiments are necessary to establish the effect of air content quantitatively.

Mr. G. S. Baker, O.B.E. (Member of Council): There are several things in Commander Kell's paper which are of great interest to us. First of all, we are very much indebted to him for his descriptive notes with regard to cavitation as they have seen it in America; they are most informative and helpful. Secondly, we notice the support which the United States Government is giving in the form of a destroyer which he can use to carry on his work.

The author mentions that the tunnel which they are using is of the "open jet" type, so that you can see what is happening inside the tunnel. In case that description should lead anyone to think that with a "closed" jet you cannot see what is happening, I should like to say that you can see quite well, as we shall be able to show you on Thursday when you go to the Tank at Teddington.

I think that the most interesting part of the paper is the data given in Fig. 1. Although I understand from Commander Kell that this figure is more or less of a general character, I would like to suggest that he should give the camber ratios of the sections which he has used in that work, as the diagram would then be of much greater value.

I should also like to ask him why he uses the calculated C_L from Eggert's not too well supported formula, as given in the paper, instead of actual values for the section? I know that we have practically no data for such sections in water, although we want them very badly, but I think that for the time being aeronautical data could be used, and it may be that Fig. 1 would have a little different appearance if this were done. As it stands, it suggests that a circular-back section will take a higher C_L than an aerofoil section before it breaks down, and that is somewhat against our own experience.

One other matter is worth mentioning in connection with such figures. If v_m is the actual velocity of the water at that part of the back of a blade where burbling commences, this blade moving at a speed v_r, the condition for cavitation is that

$$p_a = \frac{v_m^2 - v_a^2}{2 P}$$

and this gives

$$\frac{p_a}{q} = (a^2 - 1)$$

where a is the ratio v_m/v_a for this section at the working slip angle.

Lerbs' curve is based on the assumption that this ratio is sensibly the same as that over a circular-back section giving the same C_L—which can be easily calculated. But obviously this is only an approximation, since v_m/v_a varies with blade section, particularly at small slip angles. In fact, any such method of representation which ignores the fact that the base of Fig. 1 is actually connected with blade shape is bound to show greater inaccuracy the smaller C_L becomes, as it is at small C_L values that the shape of section is almost predominant, and this is the main reason for the relative spreading of the spots in the figure at this end of the diagram.

Herr H. Lerbs: We should be grateful to Lieutenant-Commander Kell for the complete results on cavitation tests which he has given us from the Washington Tank, as in some cases we have obtained rather different results in our Hamburg cavitation experiments.

On the question of face cavitation, our measurements have shown that face cavitation alone, without back cavitation, if sufficiently increased, leads to a diminution of efficiency, which also agrees with the principle of energy, because the bubble collapse is always followed by a loss of mechanical energy. In special cases of incipient back cavitation this loss of energy can be overcome equally well by increasing the lift coefficient caused by increasing camber ratio as by decreasing drag coefficient due to decreasing skin friction.

Very interesting are the systematic measurements of the influence of dissolved gases in the water upon the screw forces. We have not yet made in our Hamburg Tank any such systematic measurements; we have only made experiments with different screws in fresh tap-water, which at Hamburg is very full of air, and now we have measured thrust depending on times at constant values of vacuum and number of revolutions. We have always found that thrust increased with increasing time, till after some time a constant value was attained which could be always reproduced. The time depends naturally on the vacuum.

Dr. D. P. Riabouchinsky: In connection with the interesting communication of Commander C. O. Kell, I would like to mention some researches on the formation of cavities in liquids carried on at the Laboratory for the Mechanics of Fluids at the University of Paris.

These researches do not directly concern screw propellers, but the general theory which I have developed might also be applied to the latter.

We carefully examined first of all two specially simple cases, the strictly hydrodynamical solution of which could now be developed up to numerical values and be afterwards submitted to experimental verification.

The first case is that of an infinitely long circular cylinder suddenly set in motion perpendicularly to its length in a liquid initially at rest.

The second case is that of the same cylinder moving at a constant speed perpendicularly to its length at the moment when the pressure exerted upon the free surface, bounding the liquid externally, falls suddenly to a new and sufficiently low constant level.

Streamlines in the initial relative motion and the nascent cavities for both these cases are shown on Figs. 1 and 2. The arrows show the direction of motion of the cylinders. The distribution of the initial pressure has also been calculated. (See para. (8) in footnote.*)

The general theory allows us to foretell that in the case of a sphere similar cavities must arise. In the first case the fluid will separate itself from the sphere on a surface surrounding the upper pole, and in the second case on an annular surface along the equator. The experiments which we have carried out have confirmed this theory.

As far as one can see from the above figures, the cavities arising would have to be washed away by the relative stream, and therefore the motion cannot tend to become

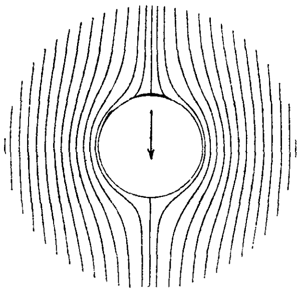

FIG. 1.

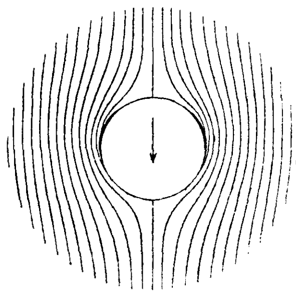

FIG. 2.

permanent, but, as I mentioned in a previous paper (See (1), para. 28), it would tend, in Case 2, to become periodical.

The origin of periodicity in the experiments mentioned by Dr. J. C. Hunsaker could probably be interpreted as above.

The CHAIRMAN (Sir William J. Berry, K.C.B., Vice-President): I beg to move a hearty vote of thanks to Commander Kell for his valuable paper.

WRITTEN CONTRIBUTIONS TO THE DISCUSSION.

Mr. J. F. C. CONN, B.Sc. (Associate-Member): Commander Kell has given an interesting paper on a difficult subject. It is regrettable that he confined himself to a discussion of cavitation in general terms, since it is scarcely possible to study the behaviour of any propeller without complete information regarding its geometrical properties, together with its thrust and torque performance over a range of slip.

His remark that "it is the back of the propeller blade and not the face that governs the propeller performance" can only be greeted with a certain mild surprise. The author does well to stress the possibilities of the time factor in model experiments (page 372) as this is a point which may easily be overlooked.

We should be grateful to the author for any information regarding tests made on

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propellers with crescent blade sections, i.e. sections formed by the intersecting arcs of excentric circles. Aerofoils of crescent shape appear to have certain theoretical advantages, considered as blade sections for fast-running highly loaded propellers.

Lieutenant-Commander C. O. Kell (CC), U.S.N.: I wish to thank the several gentlemen who have contributed to the discussion on this paper.

It is to be regretted that it was not possible for Dr. J. C. Hunsaker to project his slides and film which so clearly describe the work being done on the subject in the laboratories of the Massachusetts Institute of Technology. It was my privilege to see them last week at Cambridge, and they are most instructive. They show clearly the periodic nature of the phenomena.

Mr. Baker points out that our data plotted in Fig. 1 are not complete in that we have not taken into account the blade shape in determining the abscissae of our points, in particular for those points of low C_L. I agree with Mr. Baker, and his point is important. However, at the present time we are not prepared to differentiate for the many possible variations in characteristics of blade shape. Further, I believe that the criteria upon which these data were based were sufficiently well defined to make it possible to show the effects of major changes in blade shapes.

I am very much interested in what Dr. Lerbs had to say relative to face cavitation. After further discussion with Dr. Lerbs, I do not believe the Hamburg and Washington tunnels are in disagreement on this point. Dr. Lerbs has measured loss in efficiency due to face cavitation at very low cavitation numbers, t, where $t = \frac{p_a - e}{q}$, and at very small positive and even negative slips. Under such abnormal conditions of operation, face cavitation will be present to a much greater degree than can be expected in any reasonable design. I think Dr. Lerbs will agree that with conditions in his water tunnel which represent reasonable operating conditions on full scale, where face cavitation has been present, no effect could be determined.

The brief description by Dr. Riabouchinsky of his experiments at the Paris University is most interesting. His description of the formation of cavities on an annular ring along the equator of the cylinder in his second case checks our observations that the burbling type of cavitation has its origin at the position of maximum blade thickness. In this position, and at the equator of the cylinder in the second of Dr. Riabouchinsky's experiments, the relative velocities of water are maxima.

In reply to Mr. Conn, I regret that I am not at this time prepared with detailed information on the performance of propellers with crescent blade sections, together with the geometric characteristics of such sections. It will be noted from Fig. 1 that the sections of most of the propellers tested thus far had the ordinary ogival back. There were a few propellers tested having the so-called aerofoil-shaped sections. It will be noted that as a rule propellers of this type cavitated at lower lift coefficients than comparable propellers with ogival-shaped sections. However, I do not feel that the relative merits in efficiency and non-cavitating characteristics of propellers designed with aerofoil sections and those with ogival-shaped sections have been sufficiently established at present to justify a statement.

THE EFFECT OF INCLINATION, IMMERSION, AND SCALE ON PROPELLERS IN OPEN WATER.

By Dr. Ing. R. DE SANTIS (Associate-Member), of the Rome National Tank.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects July 11, 1934, Sir WILLIAM BERRY, K.C.B., Vice-President, in the Chair.]

INTRODUCTION.

1. Many investigations have been effected in experimental Tanks to detect the influence of immersion and scale on propellers in open water (Taylor, Baker, Kempf, Smith-Keary, Allan); the present work, referring to tests carried out at the Rome National Tank, is a contribution on the same subject. The influence of the inclination of the screw axis over the line of advance is also investigated.

2. Two groups of propellers were tested over a great range of slip, each group consisting of three geometrically similar models. Particulars of the screws are given in Table I.

TABLE I.

	Tank Screw Number.					
	Group I.			Group II.		
	E 12_A	E 12_B	E 12_C	E 13_A	E 13_B	E 13_C
Number of blades	3	3	3	4	4	4
Diameter (d), mm.	297·99	397·32	496·65	297·99	397·32	496·65
Pitch (uniform) (p), mm. ..	366·00	488·00	610·00	366·00	488·00	610·00
Projected area (A_p), mm.^{2} ..	21,375	38,000	59,375	23,355	41,520	64,875
Developed Area (A_d), mm.^{2} ..	26,811	47,660	74,475	28,935	51,440	80,375
Pitch ratio (p/d)	1·228	1·228	1·228	1·228	1·228	1·228
$\dfrac{\text{Projected area}}{\text{disc area}} \left(\dfrac{A_p}{\frac{1}{4}\pi d^2}\right)$ ..	0·3065	0·3065	0·3065	0·3345	0·3345	0·3345

The design of the medium-sized propellers E 12_B and E 13_B is shown in Figs. 1 and 2 (Plate XXXVI.), from that it is easy to define the shape and size of the similar propellers E 12_A, E 12_C, and E 13_A, E 13_C. The screw shaft remained unchanged.

EXPERIMENTS.

3. All the experiments were carried out with a big dynamometer having the following characteristics:—

Maximum thrust to be recorded..	200 Kg.
Maximum torque to be recorded	5 Kgm.
Horse-power that may be developed by the motor..	17 H.P.

This apparatus is fully described in the first volume (1931) of the Annual Reports of the Rome National Tank. Some troubles were experienced with the smallest propellers because of the small thrust and torque recorded by comparison with the great power of the dynamometer, but it is the opinion of the experimenters that even with the smallest screws the results obtained are quite reliable.

The apparatus was carefully controlled in advance, and its exactness was checked no less than twice a day.

Each series of experiments was carried out at constant number of revolutions and variable speed of advance.

The results have been plotted in the form of "thrust constant" $\left(\frac{T}{\rho\, n^2\, d^4}\right)$ and "torque constant" $\left(\frac{Q}{\rho\, n^2\, d^5}\right)$ on a base of "speed constant" $(v/n\, d)$, where

$$
\begin{aligned}
T &= \text{thrust in kg.,}\\
Q &= \text{torque in kgm.,}\\
v &= \text{speed of advance in m. per second,}\\
n &= \text{number of revolutions per second,}\\
\rho &= \text{water density.}
\end{aligned}
$$

From this the efficiency has been calculated and plotted on the same base.

Scale Effect.

4. In order to discover what is usually named "scale effect" the two groups of propellers were tested at a constant immersion and at corresponding number of revolutions as indicated in Table II.

TABLE II.

(*a*) Three-bladed propellers = Constant immersion $i = 1 \cdot 1\, d$.

E 12_A, number of revolutions per second	9·86
E 12_B (first series), number of revolutions per second	8·54
E 12_B (second series), number of revolutions per second	9·86
E 12_C, number of revolutions per second	7·64

(*b*) Four-bladed propellers = Constant immersion $i = 1 \cdot 1\, d$.

E 13_A, number of revolutions per second	8·13
E 13_B, number of revolutions per second	7·04
E 13_C, number of revolutions per second	6·30

As it was ascertained that a "scale effect" was surely present, mainly on torque records, a second series of tests was carried out with the propeller E 12_B at different number of revolutions (see Table II.). So two different curves of thrust and torque constants were obtained with this screw. All results have been shown in Fig. 3 (Plate XXXVI.), and all the spots have been carefully plotted for the personal consideration of investigators.

One may suggest plotting the spots on a base of Reynolds' numbers to find out a law of variation on "scale effect" from model to full-size screw, but it is the writer's opinion that such an expedient would be a treacherous one in the present case because of the small number of screws tested, of their particular shape and size, and of the unknown performances at sea of the full-size screw.

5. As may be observed, the smaller the screw the smaller are thrust and torque and the greater is the efficiency. Besides referring to the well-known "wake-fraction" normally calculated in connection with self-propulsion experiments, it would appear that the smaller the screw the greater is the wake-fraction.

Effect of Immersion.

6. The effect of immersion measured to centre of boss has been proved with the propellers E 12_C and E 13_C (the biggest ones) at four different immersions, i.e.

$$\begin{aligned} i_1 &= 0\cdot 5\,d \\ i_2 &= 0\cdot 6\,d \\ i_3 &= 0\cdot 8\,d \\ i_4 &= 1\cdot 1\,d \end{aligned}$$

All the series of experiments were interrupted when it appeared difficult to maintain the revolutions at an absolutely constant value or when an extraordinary value of the slip was reached. The results have been assembled in Fig. 4, Plate XXXVII.

7. The experiments show an increase of thrust, torque, and efficiency when increasing immersion, but at constant slip both thrust and torque are rapidly becoming constant; at $i = 0\cdot 8\,d$, immersion has no more influence at practical slip.

Effect of Inclination.

8. To carry out this research propellers E 12_C and E 13_C have been used. The inclination was effected on a horizontal plane through the screw axis. Propeller E 12_C was tested at 3° and 6° of inclination, propeller E 13_C was tested at 3° and 6° on one side and 3° and 6° on the other side of the line of advance, that is to say, the middle line of the Tank. The immersion was maintained constant at $1\cdot 1\,d$. The results have been plotted in Fig. 5, Plate XXXVIII. For the present research the "thrust constant" is taken as $\frac{T \cos \alpha}{\rho\, n^2\, d^4}$, where α is the angle between the screw axis and the line of advance.

9. The conclusion may be drawn that from the point of view of propulsion efficiency it is immaterial whether or not we adopt an inclination of the screw axis between 0° and 6° with this type of screw, in any case thrust and torque have shown a tendency to increase and efficiency to go down.

TABLE III.

mm.		Feet.	mm.^{2}		Square Feet.
297·99	=	0·9777	21,375	=	0·23009
366	=	1·2008	23,355	=	0·25140
397·32	=	1·3035	26,811	=	0·28860
488	=	1·6010	28,935	=	0·31146
496·65	=	1·6294	38,000	=	0·40904
610	=	2·0013	41,520	=	0·44693
			47,660	=	0·51302
			51,440	=	0·55371
			59,375	=	0·63913
			64,875	=	0·69833
			74,475	=	0·80167
			80,375	=	0·86518

DISCUSSION.

Ir. L. Troost (Member): It is really interesting to see that in the tests which Dr. de Santis has carried out, two of his screws, being on different scales, have produced results which, as far as I know, are entirely different from those formerly obtained by investigators in so far as, going up with the size and increasing the Reynolds' number, Dr. de Santis gets efficiencies that are lower than those with screws of the smallest diameter. I am sure that he knows the work of Dr. Kempf which was published in 1927, and the recent publications of Dr. Kempf and Mr. Allan on the subject of scale effect. I should be very glad to hear the opinion of Dr. de Santis as to why increasing the size of his propellers gives results so different from those of other investigators.

Mr. G. S. Baker, O.B.E. (Member of Council): There is one question that I should like to ask Dr. de Santis, and that is in connection with the tests with the inclined axis. When we started doing screw propeller tests we inclined the axis of the shafts exactly the same as on a ship, but we gave it up for this reason, that the carrying frame influenced the results, and it was extremely difficult to measure the resistance of that carrying frame. We could never be certain of it, and, since the effect of the inclination was comparatively small, we thought that the errors introduced by the carrying frame might be even greater than the inclination factor. I should like to ask Dr. de Santis how he got rid of the influence of the carrying frame so as to be able to yield results with the high degree of accuracy which is necessary to produce the diagrams in his paper.

Mr. J. F. Allan (Associate-Member): I find this paper very upsetting from the scale effect point of view, as the results obtained by Dr. de Santis appear to be in direct contradiction to what we have come to expect. I think the general conclusion has been that the influence of scale effect shows itself by a rising tendency in the thrust coefficient curve, and, to a lesser degree, in the torque also, so that there is a rising tendency in the efficiency curve. I should like to know if Dr. de Santis can give us any suggestions regarding the difference of torque. The steadiness of the thrust curve seems to indicate that you are beyond the region of the scale effect, and it leads one to suggest that there may be some discrepancy in the measurement of the torque. Certainly it is the experience of all of us that it is impossible in the torque apparatus to eliminate this question. I have not had time to calculate accurately the Reynolds' number of the smallest screws tested, but it seems to me that it is two and a half to three times 10^5 at $0 \cdot 7$ of the diameter. If there is serious scale effect under these conditions there will be pretty serious difficulties in using model results. Analysing the steady thrust coefficient and rising torque coefficient further, we may say that no thrust scale effect may be taken, for practical purposes (see my paper read at our recent Spring Meetings), to mean no lift scale effect, and therefore no change in circulation. The scale effect would therefore have to be practically all in the frictional drag and in an upward direction. The only reasonable conclusion we can reach is that the torque measurement is in error.

As a matter of interest in connection with inclination of screws, it may be of value to state that some years ago at Dumbarton, in connection with a special investigation, we inclined the screw frame 6° in a vertical direction; that is, the shaft was rising towards the propeller, the propeller being run ahead into open water. In that case, with 6° of upward tilt at the front, the diminution in efficiency was about $4\frac{1}{2}$ per cent. over the working range, or a diminution in efficiency of about one-third due to the thrust and two-thirds due to the torque. That result substantially corroborates Dr. de Santis' investigation, but at the same time I do not think that one can say that it is possible to ignore that effect. A matter of $4\frac{1}{2}$ to 5 per cent. is a serious consideration.

Dr. F. Gebers (Member): In the Vienna Tank we have made similar tests with inclined propeller shafts, and we found that up to 10 degrees of inclination the influence is not very great, if the propeller is immersed deeply enough. It gives rise, in the direction of the shaft, to nearly the same thrust and to quite the same torque as with propellers running in the direction of the shaft. We have not yet published these results, but I think it would be of value if we were to do so.

Written Contributions to the Discussion.

Mr. M. P. Payne (Member): This is a most interesting paper, which in a short space gives us detailed information of great importance regarding propeller apparatus. It is noted that the broad conclusion that immersions greater than 0·8 diameter of the centre of screw have little effect on the thrust and the torque at practical slip ratios is in keeping with other investigations at Haslar and elsewhere. The comparison between the three- and four-bladed screws, too, is much on the lines of what previous results would lead us to expect.

A rather surprising result, to my mind, is the slight decline in thrust and the greater decline in torque with increase in diameter of screw beyond 1 ft. The combined effect is to produce a perceptible decrease in screw efficiency as the diameter increases from 1 ft. to 1·6 ft. This is contrary to what one would expect, and also it is not in keeping with results obtained by other experimenters on screws of such comparatively large diameter. I should be glad to know if the author has any further experimental information to support this result, and whether he could offer us any explanation.

Mr. J. F. C. Conn, B.Sc. (Associate-Member): The author of this paper has obtained some remarkable results. He finds that scale effect on model propellers of relatively large dimensions shows itself as a diminution of efficiency with increase of size. This result could be disputed by the method of *reductio ad absurdum*, but it is also contrary to the findings of previous investigators, and the disagreement is so pronounced as to invite investigation.

From the results given by Mr. Allan in a paper read before this Institution in March of the present year it is possible to apply what may be termed a "blade element criterion." For camber ratios greater than 0·10, a Reynolds' number of at least $2 \cdot 5 \times 10^5$ must be attained where Reynolds' number is defined as $\frac{V b}{\nu}$, where $V =$ velocity, $b =$ breadth of section at 0·7 radius of the blade, and $\nu =$ coefficient of kinematic viscosity. From the particulars given in the paper I have worked out the Reynolds' number for propellers 12_A, 12_B, and 12_C at unity values of $\frac{V}{D n}$. These are, respectively, $4 \cdot 74 \times 10^5$, $7 \cdot 27 \times 10^5$, and $10 \cdot 23 \times 10^5$, and therefore above Mr. Allan's limiting values.

Hence one is inclined to question the accuracy of the experimental results. Will the author please state how his tests were made? Were the propellers tested on a propeller boat or on the usual type of Froude gear? The supporting struts of the latter may give rise to grave interference effects, and the "idle" correction for the resistance of the shaft and supporting gear is exceedingly variable. Incidentally, this same correction would be difficult if not impossible to assess in those tests which deal with the effect of inclination.

The curves of torque and thrust constants shown on Plate XXXVII., giving the effect of immersion, are exceedingly interesting. As the slip increases, both torque and thrust fall away in a manner suggestive of cavitating conditions, showing that air was probably being drawn down by the propellers.

Will the author explain his remark on page 382, where he states that "all the series of experiments were interrupted when it appeared difficult to maintain the revolutions at an absolutely constant value, or when an extraordinary value of the slip was reached"?

Mr. R. DE SANTIS (Associate-Member): My thanks are due to all the members who have taken an interest in my paper and especially to those who have made remarks upon it.

As to scale effect, I must say that the doubts expressed by Mr. Allan, Mr. Conn, and others on torque results are also my own doubts, but I could not mistrust a set of results which appeared to have the same degree of accuracy, or inaccuracy, compared to similar results obtained on immersion and inclination effects, which results can hardly be questioned. Discrepancies between investigators of this kind of research are not unusual owing to many difficulties to be overcome (constant degree of accuracy, or inaccuracy, on torque measurements between wide limits with the *same* apparatus, absolutely guaranteed similitude on geometrical lines, exactly corresponding testing conditions, influence of form and distance of the dynamometer frame on the flow régime, etc.), and sometimes accordance was reached when secondary different and not unimportant effects in testing conditions were surely present, which is itself a proof that there was not complete agreement. In the early days scale effect and immersion effect were not observed and the law of similitude was checked when testing propeller models which we now consider too small and at too low immersion to avoid these effects. More recently discrepancies were noticed, but they were small, so that they were easily attributed to inaccuracy in the recording instruments and to the difficulty of making models on exactly similar lines. Lately, immersion effect showed itself to be definitely important and so the most irregular results appeared. Can we affirm that the problem is definitely solved? Certainly I cannot be sure that in our tests we got rid of all the difficulties met with by previous investigators, far from it; I have given results that must be considered, I think, with the same caution as results previously obtained, and, like these, they should be taken into account until further and more extensive and accurate investigations are made.

The dynamometer used is of the Gebers type; it has no oscillating frame.

As to inclination effect, I am very glad to hear from Mr. Gebers that he has obtained results in accordance with mine. To Mr. Baker I must explain that, as he knows, frame effect in Gebers' measuring apparatus is reduced to the effect of the screw shaft only. This is extremely small and by measurements of idle resistance it did not appear substantially to mask inclination effect, so that was ignored.

To Mr. Allan I would say that inclination in a vertical plane was not considered in order to get rid of gravity effect; the direction of the plane (horizontal or vertical) should not be important if, when testing in the vertical plane, the gravity effect and an eventual immersion effect are taken into account and duly separated from the inclination effect. But assessing different effects may be a treacherous method of investigation, and it is better to reduce as much as possible any secondary effect and isolate the principal one.

I agree with Mr. Conn that immersion effect influences thrust and torque in "a manner suggestive of cavitating conditions." This is, in fact, a striking feature of these tests and may lead to interesting considerations. I would explain that, at deep immersion, the tests were interrupted when an extraordinary value of the slip was reached, but at less immersion we were forced to diminish gradually such a maximum value of the slip because of an increasing difficulty in maintaining the pre-calculated number of revolutions on a constant point. As Mr. Conn suggests, this fact was probably due to an incipient and periodical sucking of air from the surface.

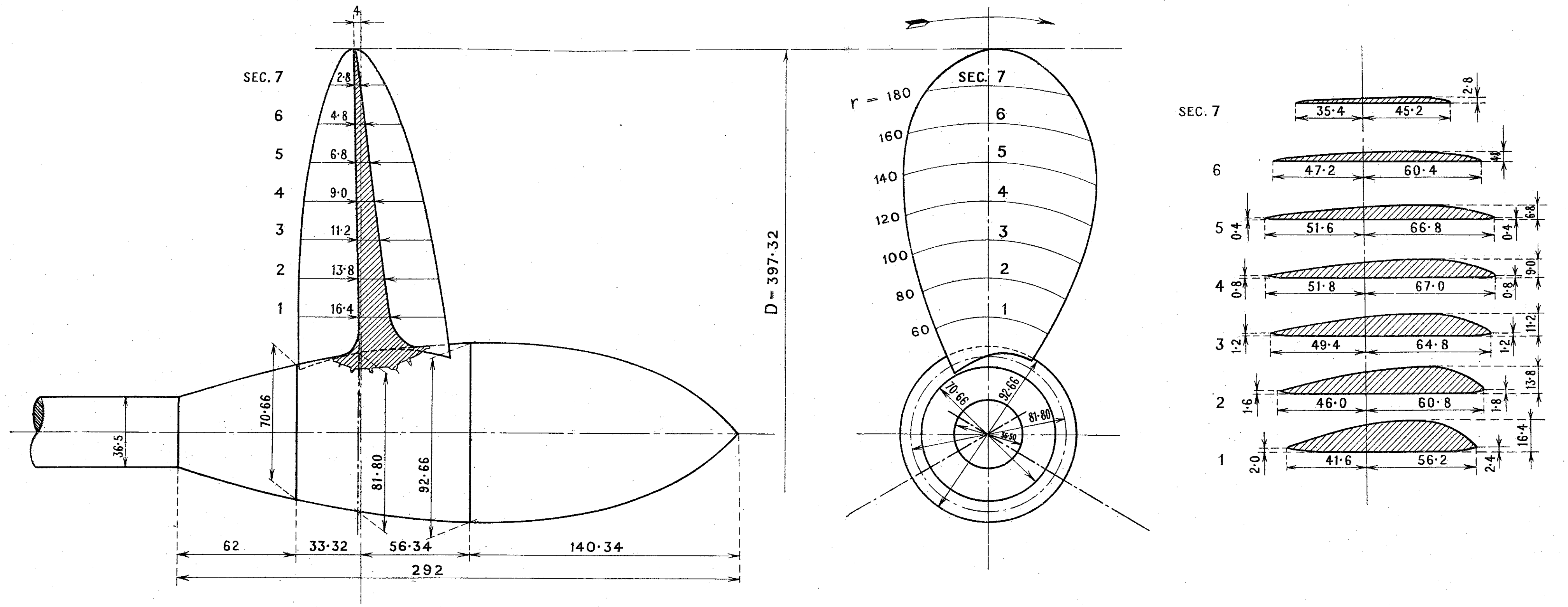

FIG. 1.—PROPELLER No. E 12_B.

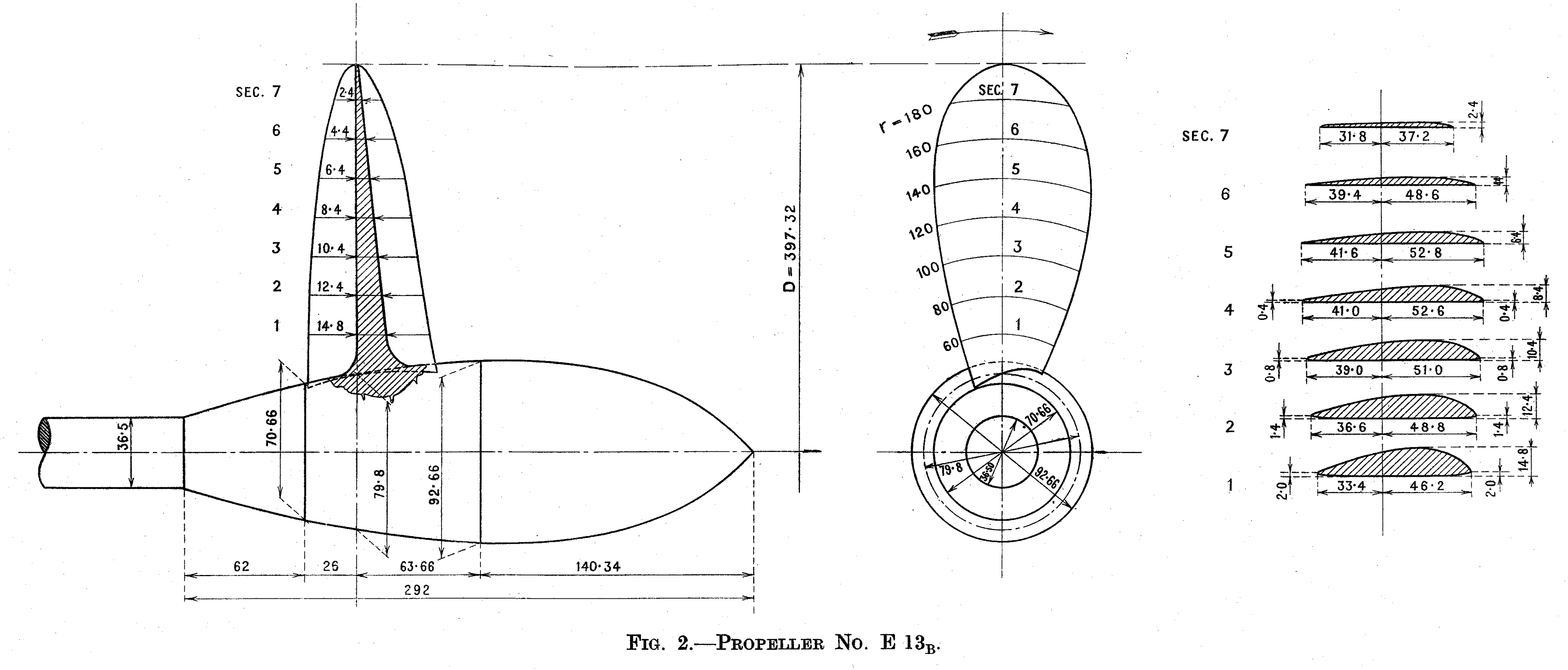

FIG. 2.—PROPELLER No. E 13_B.

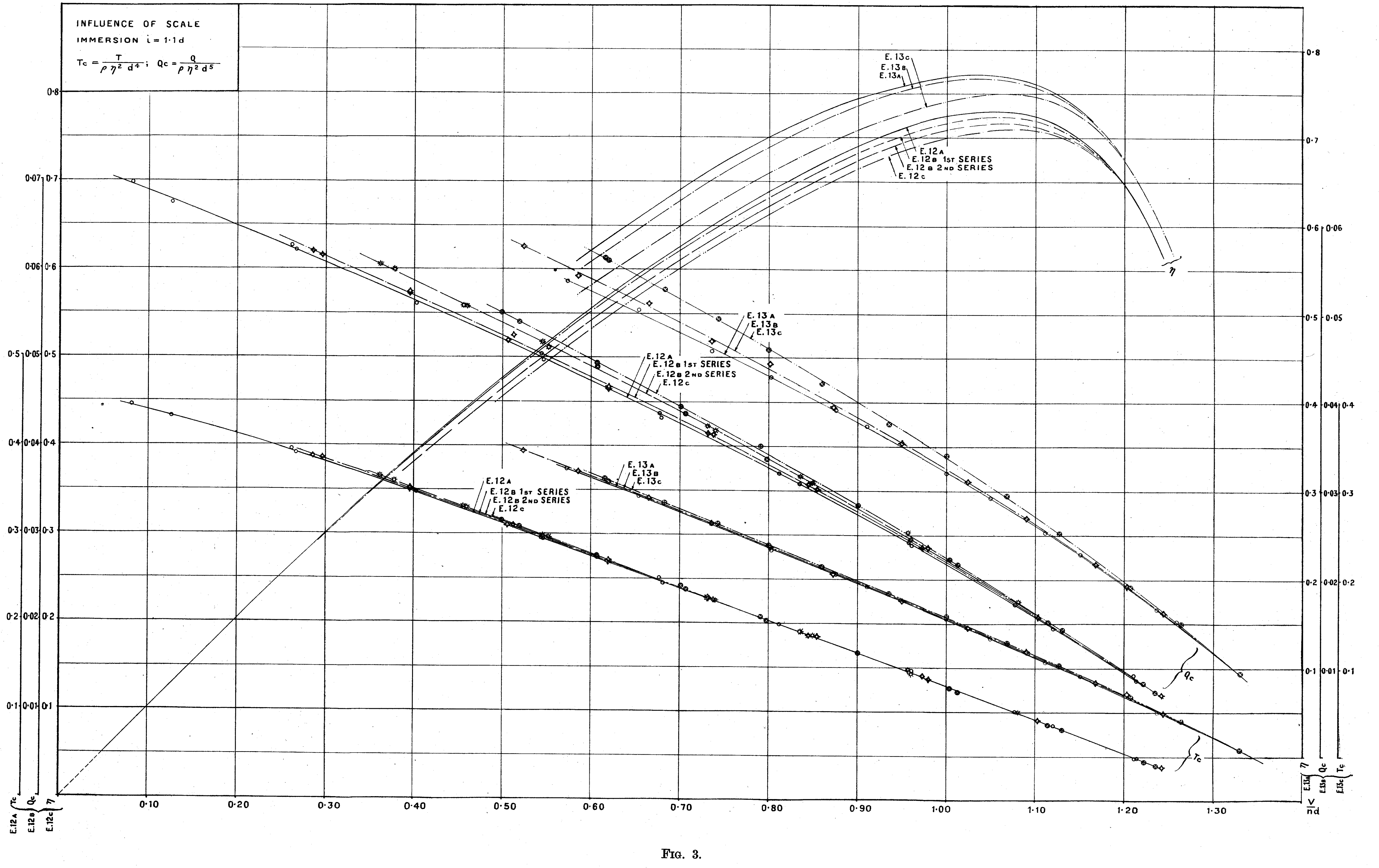

FIG. 3.

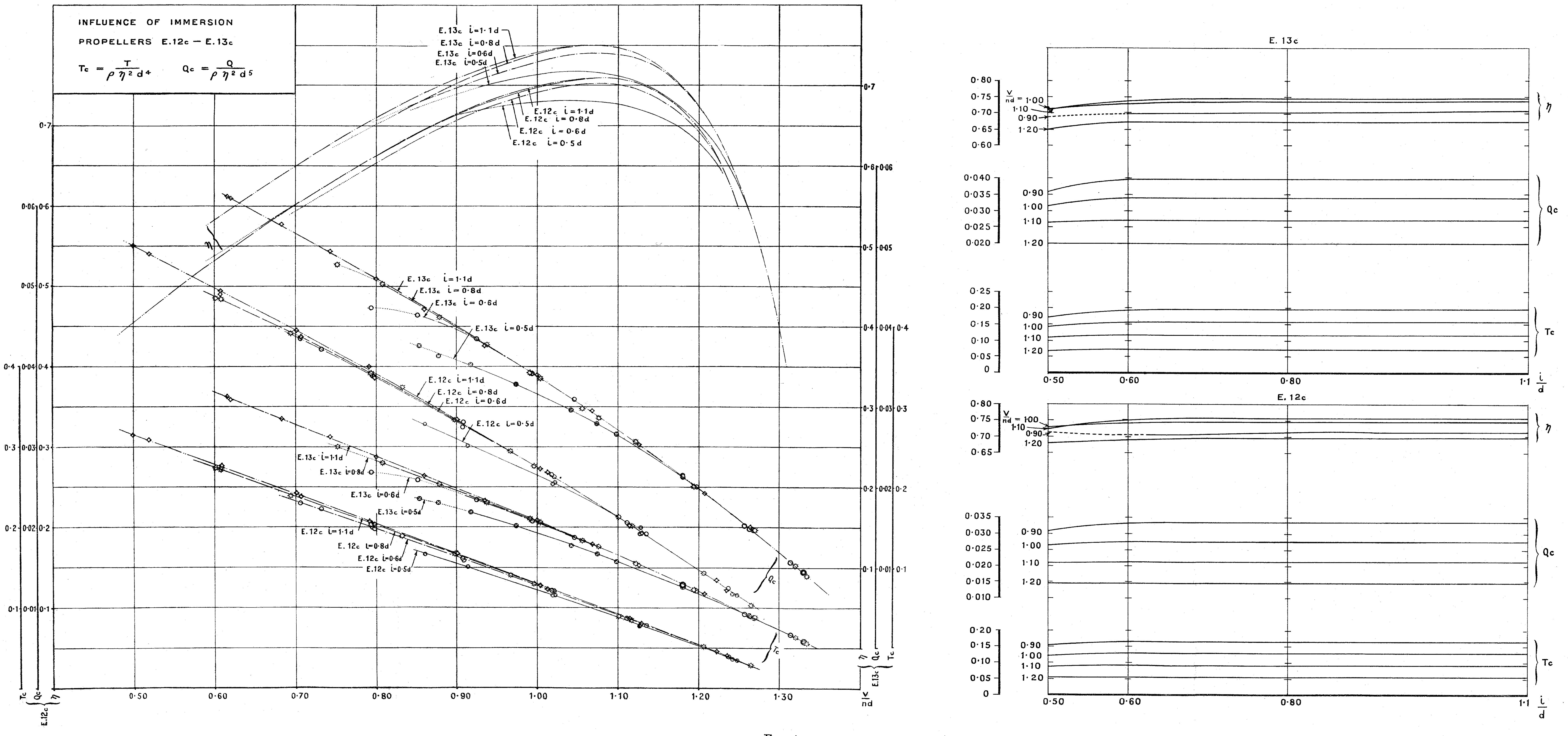
INFLUENCE OF IMMERSION
PROPELLERS E.12c — E.13c
Tc = T / ρ η² d⁴
Qc = Q / ρ η² d⁵
E.13c i=1·1d
E.13c i=0·8d
E.13c i=0·6d
E.13c i=0·5d
E.12c i=1·1d
E.12c i=0·8d
E.12c i=0·6d
E.12c i=0·5d
η
Qc
Tc
E.12c
E.13c
V/nd
E. 13c
E. 12c
V/nd = 1·00
1·10
0·90
1·20
i/d

Fig. 4.

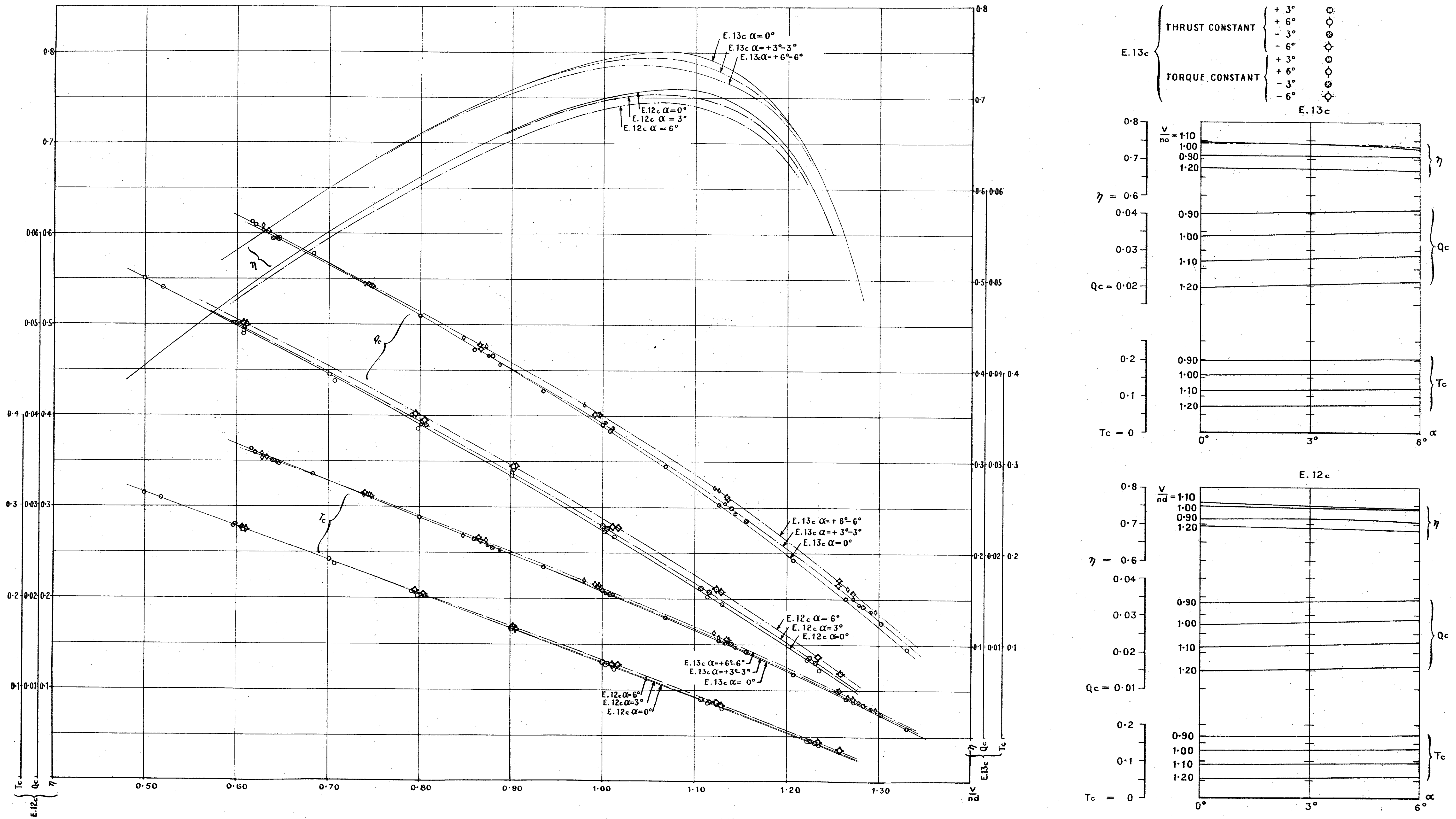

Fig. 5.—Influence of Inclination.

Propellers E 12_c–E 13_c. Immersion $i = 1 \cdot 1\ d$. $T_c = \frac{T \cos \alpha}{\rho\ n^2 d^4}$. $Q_c = \frac{Q}{\rho\ n^2 d^5}$.

MODEL EXPERIMENTS OF THE COMBINED EFFECT OF AFT-BODY FORMS AND PROPELLER REVOLUTIONS UPON THE PROPULSIVE ECONOMY OF SINGLE-SCREW SHIPS.

By MASAO YAMAGATA, ESQ., Teishinsho Ship Experiment Tank, Tokyo.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 12, 1934, Sir ARCHIBALD DENNY, Bart., LL.D., Honorary Vice-President, in the Chair.]

INTRODUCTION.

FOR the purpose of obtaining the highest propulsive economy of a single-screw ship, the form of hull, especially that of the aft-body, should not be designed without regard to the number of revolutions of the propeller to be fitted. In consequence, it is necessary for ship designers to know the combined effect of aft-body forms and propeller revolutions upon the efficiency of propulsion of single-screw ships. It is, however, a matter of great regret that, so far as I am aware, the results of the systematic model researches on this subject have not been published. For instance, the experiments at the Hamburg Tank * were limited to the study of the effect of the variation of the frame-line shape of aft-body for a particular propeller model, and those at the Washington Tank † to the study of the effect of the variation of propeller revolutions, and incidentally of its diameter for a particular ship model.

At present I am conducting systematic model experiments at Teishinsho Tank in Tokyo, in order to explore for 120-metre single-screw cargo ships the combined effect of these two factors upon the propulsive economy. The present paper is the first report of the results of this experimental investigation, and its scope is confined to a description of the combined effect of frame-line shapes of aft-body and propeller revolutions.

SHIP MODELS EMPLOYED.

Four ship models, i.e. models Nos. 195 to 198, which represented 120-metre single-screw cargo ships of fairly normal forms with raised sterns, which were made to a scale of 1:20, were employed for the present research, and they had the following principal dimensions, coefficients of fineness, etc.:—

Length between perpendiculars	6·000 m.
Breadth extreme	0·800 m.
Load draught	0·355 m.
Load displacement	1,265·6 kg.
Block coefficient	0·743
Longitudinal prismatic coefficient	0·754
Midship section coefficient	0·986
Longitudinal C.B. from amidships	0·072 m. forward.

* G. Kempf, *Werft Reederei Hafen*, 7, August 1928.
† D. W. Taylor, *Trans. North-East Coast Inst.*, 1930–31.

These models had the same shape of transverse sectional area curve and the same form of fore-body, the differences being confined to the shape of frame-lines and accordingly of water-lines of aft-body. Changing the shape of frame-lines from U to V causes a decrease in the vertical prismatic coefficient, whose values for the aft-bodies of models Nos. 198, 195, 196, and 197 were 0·948, 0·920, 0·876, and 0·837 respectively. They were fitted with all appendages except bilge keels, which might, when not properly located, confuse the conclusions of the present investigation. The body-plans, together with the bow and stern arrangements, are shown in Fig. 1 (Plate XXXIX.).

Resistance Tests.

Since the recent developments in self-propulsion tests and propeller design methods have been remarkable, the values of isolated ship and propeller model tests look quite different from what they did. Particularly from the results of the resistance tests of single-screw full-lined ships and open-water tests of so-called "wake propellers," whose face pitches etc. have been designed, taking the radial variation of mean annular wake into consideration, any important data can scarcely be obtained. In the present research, however, the former tests were carried out in accordance with the usual research procedure at various experiment Tanks.

Each ship model was run in smooth water on level trim at the load displacement to ascertain its resistance. In Fig. 2 (Plate XXXIX.) the results are plotted in our standard non-dimensional form,* and in Fig. 3, four effective horse-power curves for 120-m. ships, calculated from the results shown in Fig. 2 by Froude's skin friction constants corrected to our standard temperature of 15° C. to correspond to clean-ship conditions in salt water without any allowance for wind and wave resistances, are given on a base of speed in knots. It will be found that the smaller the vertical prismatic coefficient of the aft-body the smaller the effective horse-power over the working speed range of 10 to 15 knots, although the reduction is not great. This percentage reduction decreases with the increase of speed, and varies, for example, from 7 per cent. at 11 knots to 4 per cent. at 14 knots, down to 3 per cent. at 15 knots between models Nos. 197 and 198, whose aft-bodies had the extreme V- and U-shaped frame-lines respectively.

Wake Tests.

In order to obtain the necessary information for designing propellers, the mean annular wake distributions at the propeller position were measured on each ship model at various speeds by means of thirteen blade-wheels † of mean radii from 2·5 to 14·5 cm. These measurements were carried out under the same test conditions as those at the resistance tests, with the exception that the rudder was removed from each model to simplify the experimental arrangement. For example, in Fig. 4 (Plate XXXIX.) the results at a model speed of 1·6 m./sec., corresponding to 13·9 knots for 120-m. ships, are given in the form of mean annular wake fraction, expressed in terms of model speed, on a base of radius of annular ring, and in Fig. 5 the mean wake fractions over the five circular discs of radii from 9 to 15 cm. are shown on a base of the vertical prismatic coefficient of the aft-body. It will be seen from Fig. 5 that the larger the vertical prismatic coefficient of the aft-body the larger the mean wake fraction over the circular disc, until its radius diminishes to 11 cm., under which size the mean wake fraction first increases and then falls off with the increase of the vertical prismatic coefficient.

Next, for the purpose of obtaining the wake at every point over the propeller discs, similar tests were repeated, using pitot tubes instead of blade wheels. Fig. 6 (Plate XXXIX.) shows the equi-wake lines at the propeller position of each model at a speed of 1·6 m./sec., and

* Report of Teishinsho Ship Experiment Tank, No. 2, 1932.

† G. Kempf und G. H. Hoffmann, *Werft Reederei Hafen*, 7, January 1924.

Fig. 7 (Plate XL.) gives the distributions of wake fractions over the annular rings of radii of 6·5, 8·5, and 10·5 cm. It may be generally said from Fig. 7 that the wake distribution over any annular ring becomes more uniform with the increase of the vertical prismatic coefficient of the aft-body. However, the mean annular wake fractions calculated from Fig. 7 do not coincide with those shown in Fig. 4, the former being always smaller than the latter. I believe that this discrepancy, which increases with the decrease of the radius of the annular ring, arises mainly from the use of pitot tubes in the boundary layers. The speed measured by a pitot tube is absolutely correct only when the motion of the fluid is irrotational; nevertheless the motion of the water behind the body-post is no doubt rotational.

Propeller Models Employed.

Four speeds of propeller revolutions at 3,000 S.H.P. were aimed at for each 120-m. ship, namely, 70, 100, 130, and 160 per min. All the propellers were of the four-bladed and

Propeller Number.	R.P.M. aimed at.	Diameter in cm.	Pitch Ratio (Variable) at 0·7 R.	For Ship Model Number.
155	70	29·92	0·943	195
156	100	24·88	0·820	195
157	130	21·89	0·720	195
158	160	19·92	0·638	195
159	70	29·87	0·963	196
160	100	24·62	0·845	196
161	130	21·60	0·750	196
162	160	19·65	0·660	196
163	70	29·78	0·981	197
164	100	24·28	0·891	197
165	130	21·28	0·795	197
166	160	19·28	0·720	197
167	70	29·94	0·931	198
168	100	24·96	0·802	198
169	130	22·00	0·704	198
170	160	20·00	0·630	198

similar type with aerofoil sections, the expanded area ratio being taken as 0·407, the blade thickness fraction 0·045, and the boss diameter 4·8 cm., paying, for the sake of simplicity, no special consideration to strength, cavitation, etc., for full-sized propellers.

To obtain the necessary information for designing propellers, the preliminary self-propulsion tests were carried out on each ship model with each of the four propeller models which had been selected as the most suitable for the present requirements among our standard series propellers, whose diameters and face pitches are of very wide ranges. Such preliminary self-propulsion tests, together with wake measurements by blade wheels, are the indispensable experiments for our propeller design practice.

Examining these sixteen test results, the speed of each model corresponding to that of a 120-m. ship at 3,000 S.H.P. and the thrust required at this speed were determined for each propeller model to be designed. The mean annular wakes for each ship model at each attainable speed were read from the wake distribution diagrams, such as Fig. 4 (Plate XXXIX.), obtained by blade wheels.

Using these data—i.e. the attainable speeds, required thrusts, and mean annular wakes—four propellers for each ship model, making sixteen in all, were designed by the usual method * at our Tank, not allowing for so-called "scale effects" between ships and models. The dimensions and particulars of propellers Nos. 155 to 170 thus designed are tabulated on page 388, and their general plans are given in Figs. 8 to 23 (Plates XL. and XLI.).

Self-propulsion Tests.

Under the same conditions as those of the resistance tests, each ship model was tested, self-propelled with each of its four propellers, by our usual method for self-propulsion tests, namely, at what is known as the ship point of self-propulsion, not allowing for bilge keels, foul bottom, wind, waves, etc. It should be borne in mind that since in these tests, for the sake of convenience, the positions of the centres of all propeller models were taken unaltered on ship models, as shown in Fig. 1, the working positions of the smaller propellers were higher than those in actual cases. Figs. 24 to 27 (Plate XLII.) give the results of these sixteen self-propulsion tests in our standard non-dimensional form, and in Figs. 28 to 31 (Plate XLIII.) the S.H.P., R.P.M., and propulsive coefficient curves for 120-m. ships are shown on a base of speed in knots. As seen in these figures, the propeller revolutions at 3,000 S.H.P. do not exactly coincide with those designed, the maximum departure being 3 per cent.; hence, in Fig. 32 (Plate XLIII.), the attainable speeds and propulsive coefficients associated with thrust deduction fractions at 3,000 S.H.P. are plotted on a base of R.P.M., and three fair curves are drawn for each ship model. Moreover, in order to facilitate the discussion of the test results, these speeds and coefficients taken from Fig. 32 for the definite revolutions from 70 to 160 per min. are shown on a base of vertical prismatic coefficient of aft-body, with optimum lines.

Conclusions.

From Figs. 32 and 33 (Plate XLIII.), which summarize the test results, the following conclusions will be drawn:—

(*a*) Both the attainable speeds and propulsive coefficients at 3,000 S.H.P. for the same revolutions quickly increase at first and then gradually fall off with the increase of the vertical prismatic coefficient of the aft-body.

(*b*) As the propeller revolutions increase, the optimum vertical prismatic coefficient of the aft-body becomes smaller, though the rate of the decrease is not remarkable. Therefore, in order to obtain the highest propulsive economy, the shape of the aft-body frame-lines should be determined, taking the revolutions into consideration.

(*c*) The adoption of large slow-running propellers always improves the propulsive performance considerably.

(*d*) The measured thrust deduction fractions, which range from 0·17 to 0·23, do not show the regular change with the variation of the shape of the aft-body frame-lines or propeller revolutions. I think this is probably due to the magnified effect on this fraction of small experimental errors in resistance and thrust measurements. But it seems to be possible to point out that this fraction does not materially change with the variation of propeller revolutions over the range from 100 to 160 per min., and first diminishes and then increases with the increase of the vertical prismatic coefficient of the aft-body.

It should be remembered that these conclusions cannot be absolutely true, unless all the propellers employed were the optimum under the given conditions. Experience has shown that the propellers designed by our method may be generally said to be quite close to the optimum, though, strictly speaking, they may not be the optimum. Therefore, I firmly believe that the above conclusions can apply approximately to all ships similar to those dealt with in the present research.

Lastly, I should like to emphasize that resistance tests need not be made for single-

* A. Shigemitsu, Report of Teishinsho Ship Experiment Tank, No. 1, 1931.

screw full-lined ships, because the E.H.P. measured can scarcely give us any reliable data for judging their propulsive performances; hence it seems to be preferable that such resistance tests should be replaced by the preliminary self-propulsion tests already described.

APPENDIX.

Symbols Used.

Symbol.	Dimensions. kg.	m.	sec.	Remarks.
ρ	1	—4	2	Density, i.e. mass of unit volume of water.
L	0	1	0	Length of ship.
V	0	1	—1	Speed of ship.
V′	..	..		Speed of ship in knots.
R	1	0	0	Resistance of ship.
Rf	1	0	0	Frictional resistance.
Rw	1	0	0	Wave-making resistance.
∇	0	3	0	Immersed volume.
g	0	1	—2	Gravitational acceleration.
$v = \dfrac{V}{\nabla^{\frac{1}{6}} g^{\frac{1}{2}}}$	0	0	0	Relative speed.
$r = \dfrac{R}{\rho \nabla^{\frac{2}{3}} V^2}$	0	0	0	Relative resistance.
$r_f = \dfrac{R_f}{\rho \nabla^{\frac{2}{3}} V^2}$	0	0	0	Relative frictional resistance.
$r_w = \dfrac{R_w}{\rho \nabla^{\frac{2}{3}} V^2}$	0	0	0	Relative wave-making resistance.
E.H.P.	..	..		Effective horse-power.
N	0	0	—1	Revolutions of propeller.
T	1	0	0	Thrust of propeller.
Q	1	1	0	Torque of propeller.
$n = \nabla^{\frac{1}{3}} \dfrac{N}{V}$	0	0	0	Relative revolutions.
$t = \dfrac{T}{\rho \nabla^{\frac{2}{3}} V^2}$	0	0	0	Relative thrust.
$p = \dfrac{2 \pi N Q}{\rho \nabla^{\frac{2}{3}} V^3}$	0	0	0	Relative power.
S.H.P.	..	..		Shaft horse-power.
$\eta = \dfrac{E.H.P.}{S.H.P.}$	0	0	0	Propulsive coefficient.

Note.—The suffixes "m" and "s," representing "model" and "ship" respectively, are added to the above symbols when necessary.

DISCUSSION.

Dr. G. Kempf (Member): Mr. Yamagata has made an important contribution as a result of investigations into full single-screw ships for which we must be very grateful to him because they are especially difficult experiments, as we learn from his conclusions.

He says on page 389: "Lastly, I should like to emphasize that resistance tests need not be made for single-screw full-lined ships, because the E.H.P. measured can scarcely give us any reliable data for judging their propulsive performances." I agree entirely that it is very difficult to get reliable information from resistance curves only, especially for full-lined ships. I think this implies that the wake also might not give reliable information, because the wake is nothing else than the resistance. So these measurements of wake must also be considered as not necessarily conclusive, as Mr. Yamagata tells us himself.

He says on page 388: "The speed measured by a pitot tube is absolutely correct only when the motion of the fluid is irrotational; nevertheless the motion of the water behind the body-post is no doubt rotational."

I may state this in advance; Fig. 33 on Plate XLIII. seems to me most illuminating for the whole subject. A marked improvement of after-body is clearly shown with increasing vertical prismatic coefficient up to a coefficient of 0·9; whilst with a still higher coefficient, i.e. with more vertical sections, again a slight deterioration is measured.

From Fig. 6 of wake distribution it may be seen that the wake changes, from model No. 196 to model No. 195, to quite a different condition, obviously the flow around the aft-body and perhaps its rotation changes also.

In the Hamburg Tank we made similar observations, but I think one should endeavour to find out how the flow on the after-body may be set up. I will try to show it by the diagram (Fig. A). The space left behind the aft-body from the ship in motion must be filled up by the two parts of the flow, viz. the bottom-stream and the side-stream. The distribution and proportion in which they do it depend on the form of the aft-body sections, and these are responsible to a great extent for the resistance and the direction of rotation in the wake. In Fig. A I have drawn the two extreme forms of sections belonging to models Nos. 197 and 198. In model No. 197 a much greater proportion is left to the bottom-stream to fill up the space behind the aft-body. In model 198 the greater proportion is left to the side-stream, and as its energy is much less it may fail in filling up the greater space originating near the surface. Then the bottom-stream will rise into that space, and push away the side-stream. This will result in a wave crest on the surface separating from the curvature of the hull much more forward than on model No. 197. In such cases we have observed a great vortex on the after-body of a diameter equal to the depth. I am sure that this can be avoided by suitable measures, even with vertical sections, of the aft-body, and therefore I wish to emphasize that the results of Mr. Yamagata (Fig. 33) should not prevent us from giving further study to the problem of improving propeller efficiency. This is connected with an equal distribution of wake produced by vertical sections, and can be attained without introducing drawbacks in the resistance caused by an early departure of flow from the curvature of the hull.

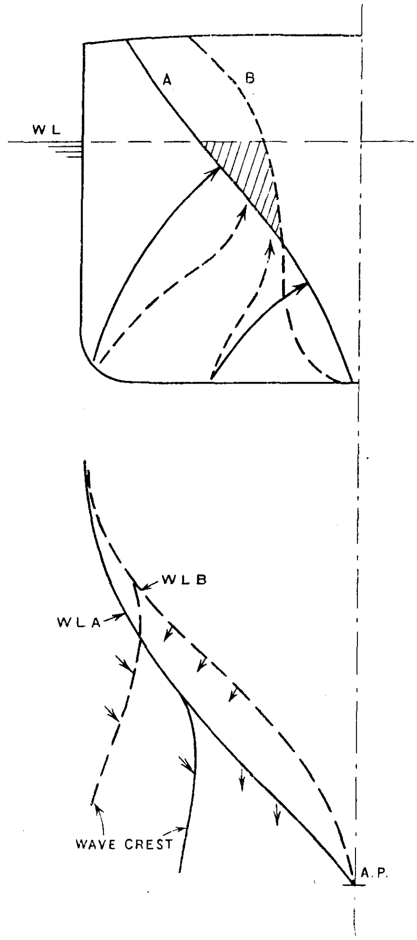

FIG. A.
Curve A, Model 197.
Curve B, Model 198.

All these researches on the after-body of full-lined ships are extremely delicate, but we must always bear in mind that the lines of the model are really fuller in regard to potential flow than the lines of the real ship, and no one knows what will happen in reality. Only one thing is quite certain: that the model is in a more difficult situation than the ship, and that all the conditions for the ship are better than for the model.

My remarks are not directed against Mr. Yamagata's paper; we are all in the same boat. We must make experiments with models of full-lined ships; we must see it through, and find some way to improve the after-body of full-lined ships.

We must be very grateful to Mr. Yamagata who has given us such a lot of valuable information about this difficult task.

Mr. G. S. Baker, O.B.E. (Member of Council): I would like to add my own thanks to those which Dr. Kempf has expressed to the author for putting such a valuable paper before us. It is really a complementary one to my own which was read yesterday, and it requires to be read with it. In the discussion on my paper yesterday Dr. Telfer asked: Why was it Mr. Yamagata was able to obtain a propulsive coefficient of 0·91 when the maximum given in my own paper was 0·755? For one thing, you have to remember that we were designing for fixed revolutions of about 80 for 12 knots, and Mr. Yamagata has allowed himself to go over the whole range of revolutions from about 50 upwards. That has given him 2 or 3 per cent.

Next, in our tests, the experiments were made without any rudder and without any fin, and Mr. Yamagata has done everything he could to improve the stern of his boat. He has a good fin, with a perfectly smooth rudder, and everything at the stern of his model nicely smoothed out. As a result he obtained a quite considerable fin effect, which you can assess at about 3 to 8 per cent.

Next, he has made his tests in the manner common on the Continent at what is called ship-propulsion point, that is, with the thrust nearly equal to the resistance of the ship without any allowance for weather, sea, or for appendages; whereas, in our case, we leave all these things on, and our propulsive coefficient is given with a thrust loading of about 30 per cent. higher than the thrust given by Mr. Yamagata, although it is applied to exactly the same thing.

If you look at Figs. 28 and 31 (Plate XLIII.) you will see the effect there of thrust loading on the efficiency, which is falling off quite considerably and rapidly when the loading increases. All of these differences must be taken into account if you are comparing Yamagata's results with those given in my paper. The difference must be at least equal to these three things which I have mentioned.

There is one other broad issue which is raised in this paper, namely, What is the correct method of designing a propeller? Mr. Yamagata here assumes that you have learned the whole story as regards the effect of changing from U-shaped sections to V-shaped sections in the after-body, when you have put Dr. Kempf's vane wheels behind and measured the distribution of wake, and that he can then design a propeller from various aerofoil data for that wake.

Now, if that is the case, I would like to ask the author one or two questions. If you take the largest diameter screws in each set and compare them, they all have practically the same diameter. The thrust deduction coefficient is almost identical, and therefore they are being designed for the same revolutions at the same thrust at the same speed with the same diameter. If you compare the pitch ratios they come out within 2 or 3 per cent. For all practical purposes, therefore, they are almost identical screws, and each is designed by what he calls the optimum method. The efficiency obtained differs by as much as 11 per cent., and I think the author should be able to give us an explanation of why and where that 11 per cent. comes in. If that method is not right, will he tell us what other difference, he supposes, is left out and is not included in his paper?

As regards the details in the paper, these are largely matters of experimental fact, and one can only compare them with existing data and ideas. But here again one is in a little difficulty, because the mode of presentation is rather special to Japan than general, and I would like to ask him, if it is not too much trouble, whether he would mind putting together his data in the same form as I have done in my paper, and producing a table like the tables there, with camber ratios and other data on the international system, and with the normal Froude wake, thrust deduction, etc., values. The data then would be much more easily compared with other data in this country.

Lastly, with regard to the general result of change from U to V sections, that is a subject which I dealt with in my paper which was read at Liverpool two years ago, and the general remarks which I gave there—the broad results, at least—agree with those given here by Mr. Yamagata. One thing I suggest is that in dealing with Fig. 33 the optimum line which he draws there might and could be drawn to somewhat smaller abscissa values at the higher revolutions.

One other small point is just a matter of confirmation. If you pick out the screws for any given set of conditions—I have tried two—the pitch reduction at the boss, to give the best result, comes fairly near the 20 per cent. pitch reduction given in my paper.

Dr. Ing. H. M. Weitbrecht (Member): As copies of the papers were issued only a few days before the Meeting, it was impossible for me to see if the results of the careful and profound investigation of Mr. Yamagata were the same as the results given in the papers by Dr. Kempf and myself, read before the *Schiffbautechnische Gesellschaft* in 1930.

According to my present opinion, previous results and those of the present time can only be applied to real ships with some caution. Divers investigations, relating to the scale effect in models of ships and propellers, have been published (O. Schlichting and H. M. Weitbrecht, *Jahrbuch Schiffbautechnische Gesellschaft*, 1933; F. Gütsche, "Hydromechanische Problème des Schiffsantriebs," Hamburg, 1932; J. F. Allan, Trans. I.N.A., 1934).

Revolutions, speed of advance, blade section, and hull of ship are changed in the experiments of Mr. Yamagata within broad limits; therefore it is necessary to investigate very carefully if the advantages, which have been found for a certain model in connection with a propeller of definite form of blade sections for a definite speed, will not vanish or be increased when applied to the corresponding ship.

I feel similar doubts regarding the results, which Mr. Baker gave us yesterday in his interesting paper, especially regarding the efficiency of propellers with four, five, and six blades. The breadth of the blade is in inverse proportion to the number of blades, likewise the Reynolds number; therefore, scale effect will be much more noticeable the higher the number of blades.

In experiments with ship-model hulls without screw, these relations can be considered by making small variations in the amount of the quasi-propulsive coefficient for the different types of ships and screws. Transferring the results of self-propelled models to the real ship, especially with a low number of revolutions and one screw, the scale effect calls for a complicated calculation or it will be necessary to introduce a new coefficient.

Dr. E. V. Telfer (Member): We should like to have a pronouncement from Mr. Baker explaining the differences between the quasi-propulsive coefficients given in his own paper and in this one of Mr. Yamagata's, because the experimental differences are not appreciated by people who merely see the figures and think they can be taken at their face value. At the same time, Mr. Baker mentioned in his paper that he had investigated the influence of pitch and radial loading on the propeller, and he states that a 20 per cent. reduction in the pitch at the root produces a 2 per cent. increase in propeller coefficient. That 2 per cent. (plus a possible 1 per cent. for the difference in the revolutions lost) would not be much for those high propulsive values, but I daresay they can be gradually approached and the differences explained. On the other hand, a high propulsive coefficient arises with the form of U-shaped section that Mr. Yamagata has developed. But this is obtained at the expense of a very heavy wake, which is being regained at the propeller.

Referring to Fig. 5 (Plate XXXIX.), the author uses the expression "mean wake fraction," which I do not think is correct. I question whether the fact of his two wakes not agreeing is due to that particular error in obtaining the mean value.

Mr. Baker mentioned in reading the two papers (his own and the present one) that so far as pitch values are concerned, the average face-pitch reduction here was 20 per cent. I must disagree with him in that respect, for the two real or effective pitch reductions are identical. You will notice that all pitch reductions in Mr. Yamagata's paper relate to a perfectly flat section without any after tilt; in other words, the zero lift at that section is at its full value in relation to the face pitch.

The after part of the blade sections used by Mr. Baker can be taken, on an *effective* pitch basis, to add practically another 10 to 15 per cent. to that 20 per cent. reduction. Therefore, to compare these papers, we have to consider the pitch that Mr. Baker has taken as really a 35 per cent. reduction, or something of that order, whilst Mr. Yamagata is taking 20 per cent.

I should like Mr. Yamagata to tell us a little more of the detailed method of arriving at the best form, or whether, for example, he is using what amounts to the Helmholtz method of integration based upon the optimum coefficient at each radius, and not an optimum propeller efficiency. I have taken out for one single-screw ship the radial variation in possible efficiency for a whole host of pitch variations, and it is surprising how little pitch variation really affects the problem between, say, all the most likely pitch values we can get. The maximum variation produced is within an efficiency of the order of about 5 per cent.

I should like to have from the author some statement of which method he used in order to determine the pitch variation which he subsequently tested out in his actual experiments.

In conclusion, I would like to congratulate the author on the extraordinarily promising nature of the paper, and I agree with Mr. Baker that the paper is of extreme value.

Mr. M. P. Payne (Member): I agree with the other speakers in congratulating Mr. Yamagata on the thorough nature of the programme of the experiments he has undertaken, and he merits our best thanks for bringing forward so much detailed information as is contained in this paper. The charting of the velocity distribution in the wake of the propeller must have required very considerable pains, and the results are very interesting, and well worth all the trouble taken.

The curves demonstrate the restricted zone of what can be called the core of the wake, and the fluctuating conditions in which the propellers in full cargo ship forms work. We have had little experience with such full cargo ship forms at Haslar. We did have some about seventeen years ago, when in particular, modifications in after-body fullness were tried. It was then found that with the fuller runs there was generally an increase in wake, but the propulsive advantage was generally offset by a decline in screw efficiency. As a result, the net propulsive efficiency came to depend to a large extent on the augment of resistance, which factor, as in the present paper, was found less sensitive to fullness of run than the wake. In the Haslar experiments, wakes as large as 100 per cent. and hull efficiencies of the order of $1 \cdot 6$ were obtained. Despite such favourable results, however, it was found impracticable with the model and propeller variations tried, in the limited time then available, to obtain propulsive coefficients more favourable than $0 \cdot 65$ at the corresponding revolutions of the ship's propellers. In the author's experiments the propulsive coefficients ranged from $0 \cdot 65$ to $0 \cdot 9$, but the more favourable figure is associated with very low revolutions, and it is possible that these could not be worked to in practice.

In this connection it would be appreciated if the author could give us particulars of the performance of the propellers which he tried in open water.

Then with regard to the author's concluding paragraph, it seems to me that although a strong case is undoubtedly made out for self-propulsion tests on these full ships, it would be imprudent to abandon resistance experiments altogether, particularly as the variations in fore-body fullness would have a direct bearing on the resistance and only an indirect influence on propeller performance.

Also, I would venture to say that, generally speaking, you can obtain results from resistance experiments more quickly than you can with the self-propelled model, and the speed with which results are obtained is looked upon as being very important at Haslar.

Professor T. B. Abell, O.B.E., M.Eng. (Vice-President): In the paper I had the honour to present to the Institution yesterday I made a plea for uniformity of technique in self-propelled model experiments, and I do not think I could have had a better argument in its favour than the discussion which has taken place this morning. It seems to me more important to have uniformity of presentation of the actual measurements than it does uniformity of technique in carrying out the experiments, because it not only occasions endless discussion, which may be interesting and useful, but it causes waste of time of everybody concerned in trying to reconcile results of experiments when the actual technique of the experiments is so very different.

Mr. W. R. G. Whiting, M.B.E., M.A. (Member of Council): In so far as this paper is purely a matter of Tank procedure and results, I should hesitate to say anything. But standing amidst the very considerable forest of variations described, I think the man who has from time to time to deal with the actual decisions about design may turn to Fig. 33 and take a great deal of comfort from it. The fact is that, in spite of the niceties and intricacies of the discussion, when the whole matter is examined over the models having a range of after-body vertical prismatic coefficients of 0·876 to 0·948, there is at any given revolutions per minute no variation in efficiency of commercial significance. That is to say, you may design the after-end of your ship over a very wide range of form to suit your own particular previous knowledge, and as far as this paper goes there is nothing to indicate that you will lose very seriously in efficiency in going from one form to another; particularly is that true at the higher revolutions where, as you see, the curves are quite definitely flat. From model Nos. 195 to 198 there is not ½ per cent. variation in efficiency between the different forms.

The other vitally important matter which the engineering firms may consider is the extreme loss of efficiency which, on these data, you must accept if you desire to place fast-running machinery in your vessel. The question of the revolutions to which you are tied by the machinery makers is in fact of altogether greater importance than any customary variation in the after-end of the vessel.

Mr. M. Yamagata: First of all I should like to express my hearty thanks to all those who discussed my paper not only for their kind remarks but for the valuable information which many of them have supplied, which adds very greatly to the value of the paper itself. In particular I am very much indebted to Dr. Kempf for giving us the interesting results of the wake measurements made at the Hamburg Tank, and to Mr. Baker for explaining fully the difference between the propulsive coefficients given in his and my papers.

With regard to Drs. Kempf's and Weitbrecht's remarks about the scale effect in self-propelled models, it is, of course, true that it should depend upon the absolute curvature of the hull and blade section as well as their size and speed. I am very glad to hear from Dr. Weitbrecht that the results of the investigation in this connection will be published shortly.

Mr. Baker asked for an explanation of why and where the difference of 11 per cent. in propulsive coefficient came in. To this question the following two facts may be given as the chief reasons. The first is that the propulsive coefficient, i.e. the ratio of the effective horse-power to shaft horse-power, cannot be said to be physically correct as the efficiency of propulsion, owing to the absence of the term representing the wake energy regained at the propeller. Therefore, if we could calculate the true efficiency, taking this wake energy into consideration, the above difference would become much smaller. The second is that the wake distributions over annular rings, by which the efficiency of a propeller is considerably influenced, vary with the frame-line shapes of the after-body, as shown in Fig. 7.

I quite agree with Mr. Baker's and Professor T. B. Abell's remarks that the mode of presentation of the test results at experiment Tanks should be internationally standardized. I keenly hope, as the representative of our Teishinsho Tank, that such a desirable result will be achieved in the near future. Mr. Baker asked me to translate the present experiment results into the same forms as those in his paper. I am very sorry, but it is impossible for me to do so at the moment, although it is quite an easy task, because I am now in London and have no detailed experiment data and there is not time enough to get them sent from Tokyo.

Dr. Telfer is right in saying that the expression in Fig. 5 (Plate XXXIX.) does not give the true mean wake fraction. I use, of course, this expression only as an approximate formula, because I cannot find the theoretically correct expression for the true mean wake. He also asked me to explain our calculating method of finding the optimum propellers under given requirements. The general theory of our method was described, with some actual examples, by Dr. Shigemitsu in our Report referred to in my paper, so I would not like to repeat it on this occasion. With regard to the detailed calculation procedure of each propeller treated in my paper, I regret that I cannot show it, for the same reason as that stated in answer to Mr. Baker.

To Mr. Payne's remarks that it is possible that the propulsive coefficients ranging from 0·65 to 0·9 could not be worked to in practice, I would like to reply by the following fact. All the single-screw cargo ships recently built in Japan have shown very good propulsive performance, and their propulsive coefficients based upon the S.H.P. measured at the sea trials by both the torsion-meters, which were specially designed at our Tank, and indicator cards, have ranged from 0·8 to 0·9, as predicted at our Tank. With regard to his request to present the open test results, I am sorry I did not test the present propellers in open water in accordance with our usual practice, as stated in my paper.

I am indebted to Mr. Whiting for giving us the supplements to my conclusions from the practical point of view.

The Chairman (Sir Archibald Denny, Bart., LL.D., Honorary Vice-President): Gentlemen, we are indebted to Mr. Yamagata for the great care which he has taken in performing his experiments, and for the clearness with which he has put down the data for our information and discussion. I am sure you will agree with me that we should give him a very hearty vote of thanks.

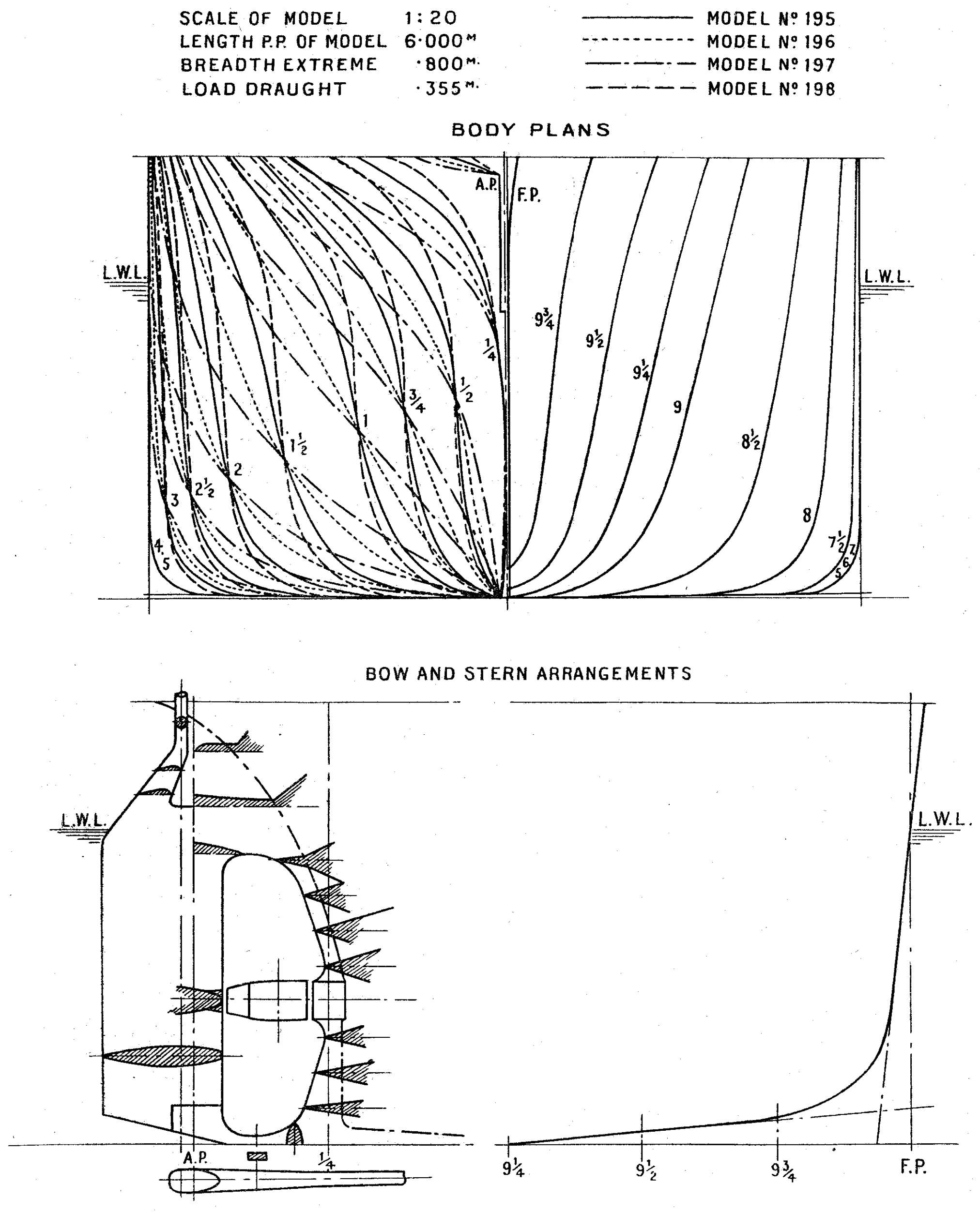

FIG. 1.—SHIP MODEL NOS. 195, 196, 197 AND 198.

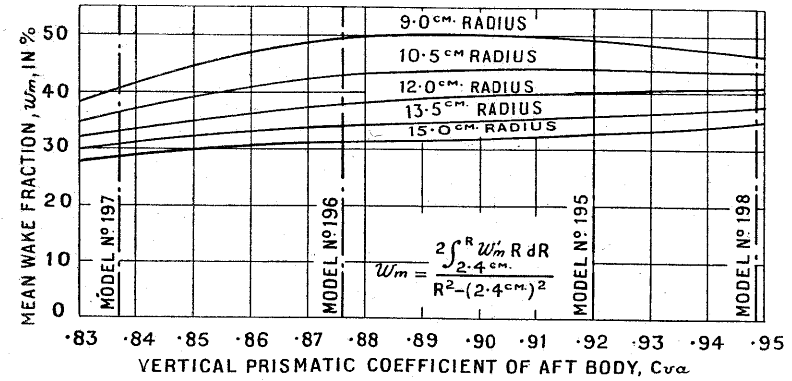

FIG. 5.—MEAN WAKES OVER CIRCULAR DISCS. $V_m = 1\cdot6$ M./SEC.

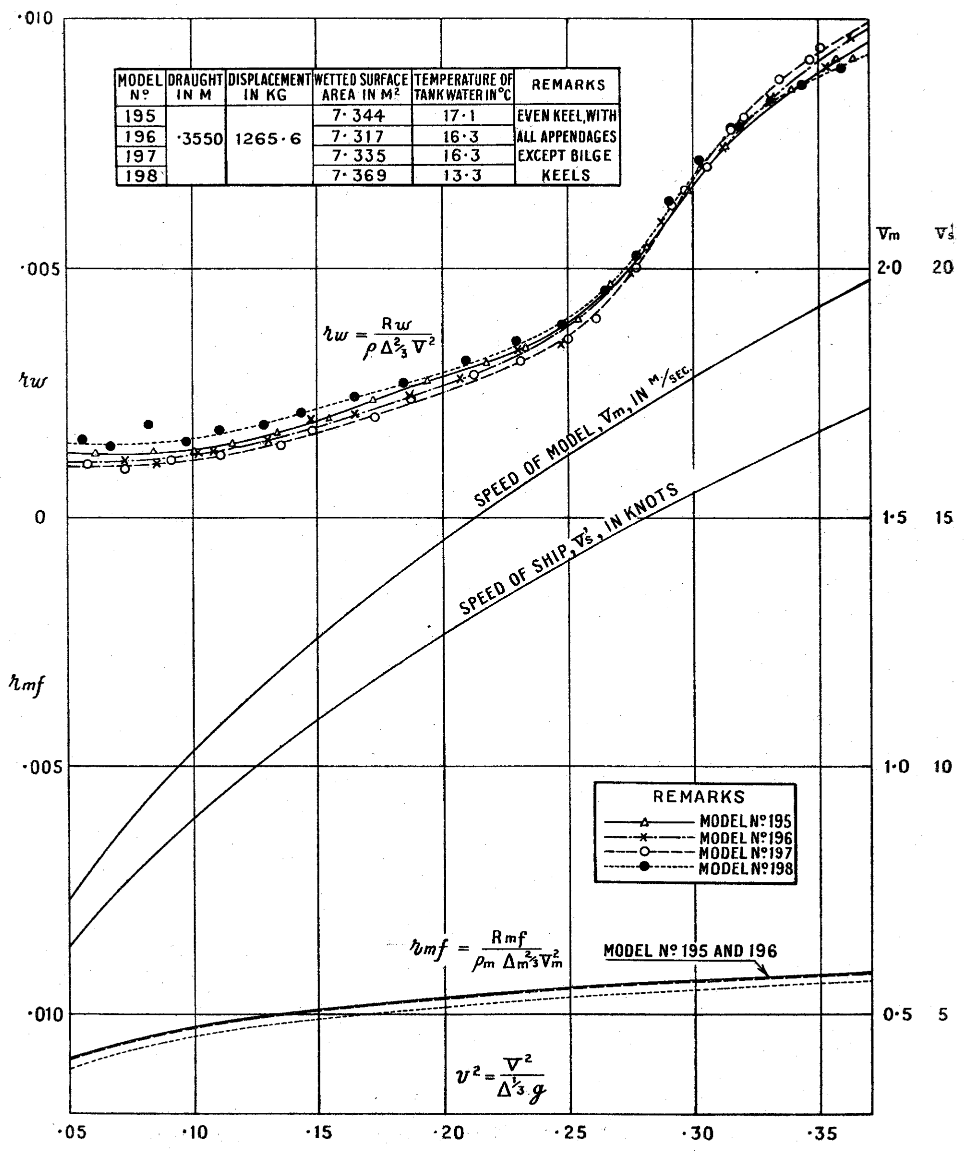

FIG. 2.—RESULTS OF RESISTANCE TESTS. $L_m = 6\cdot00$ M.

NOTE.—In the above figure Δ should read ∇.

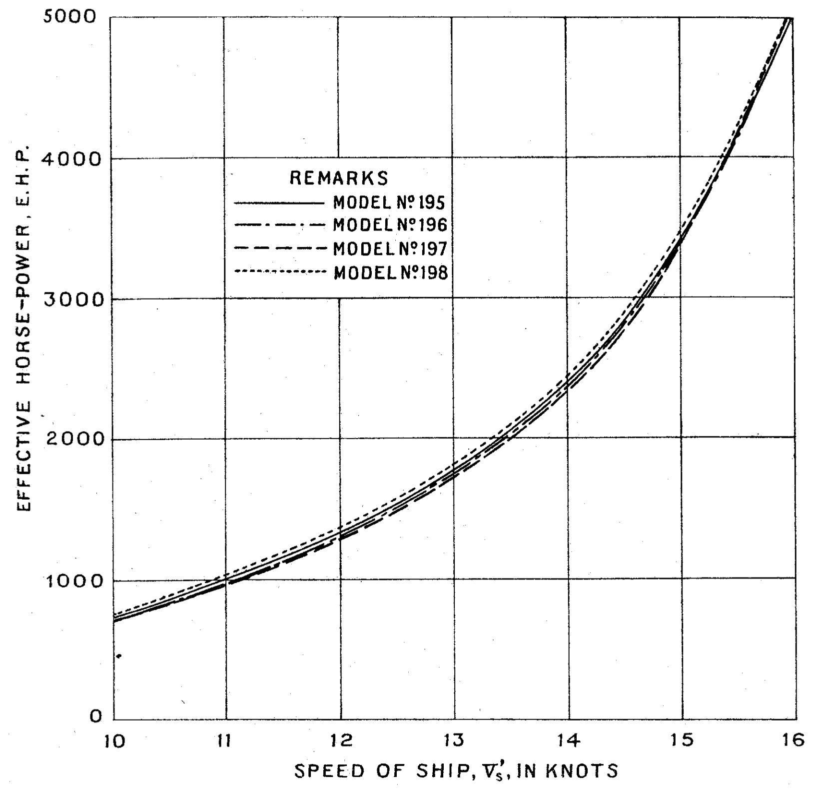

FIG. 3.—EFFECTIVE HORSE-POWER CURVES FOR 120 M. VESSELS.

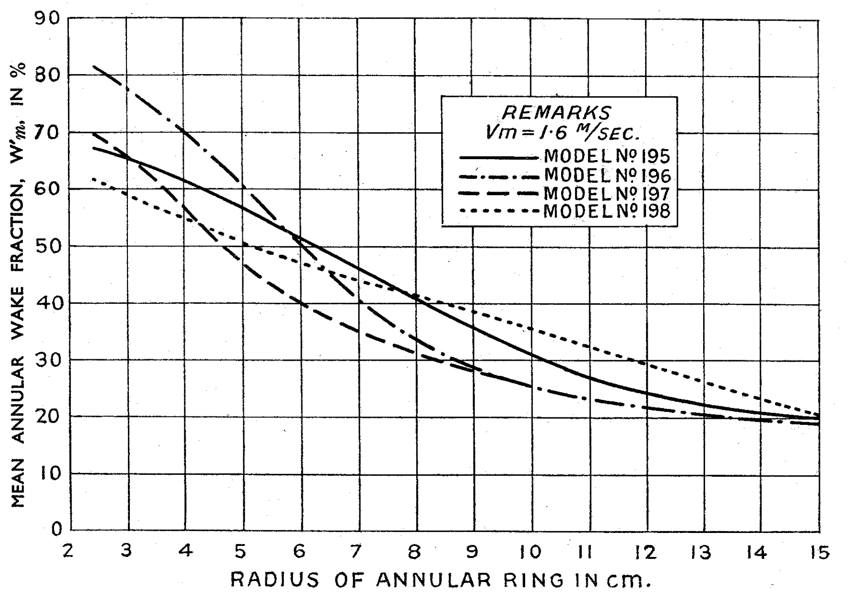

FIG. 4.—MEAN ANNULAR WAKE DISTRIBUTIONS MEASURED BY BLADE WHEELS.

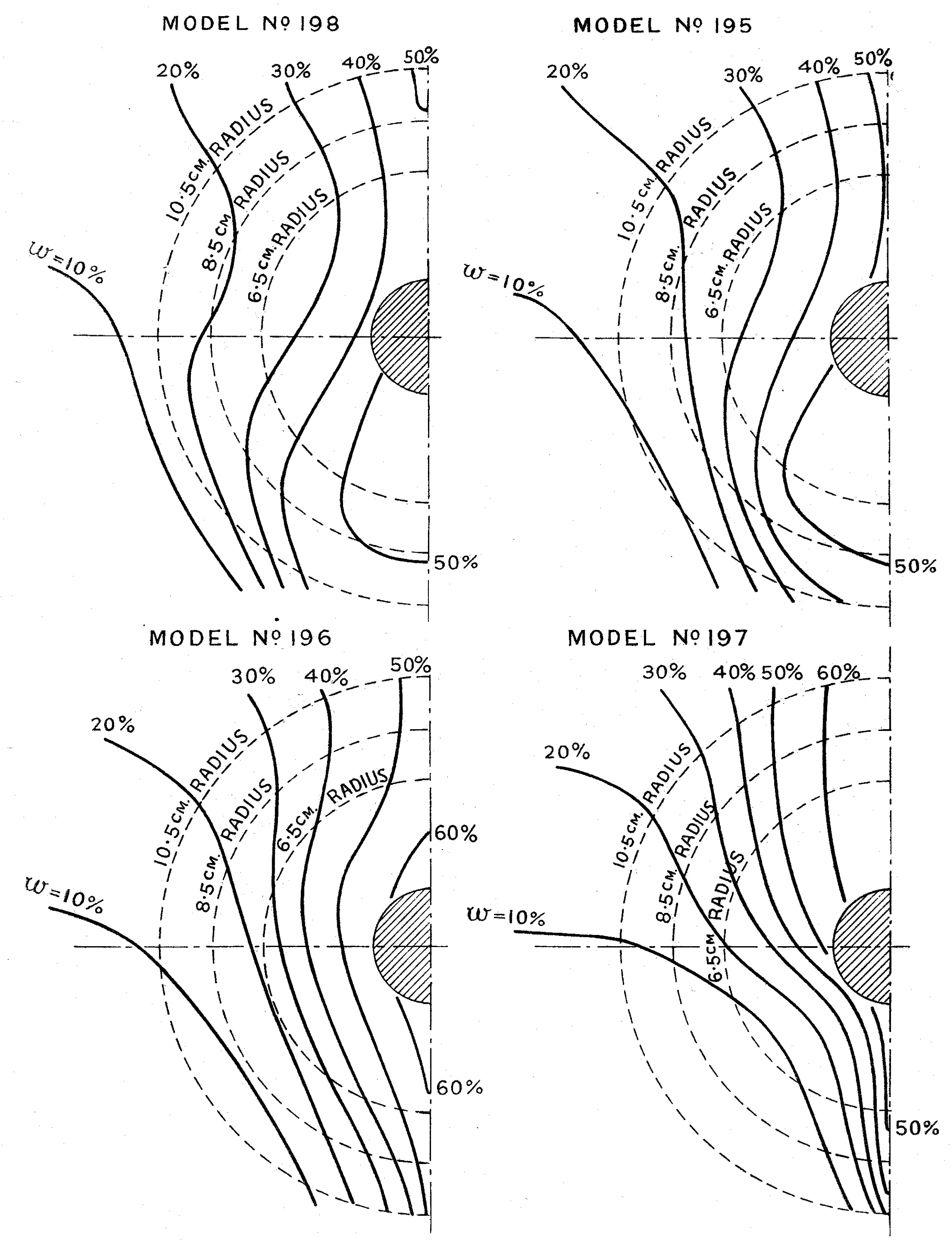

FIG. 6.—WAKE DISTRIBUTIONS MEASURED BY PITOT TUBES. $V_m = 1\cdot6$ M./SEC.

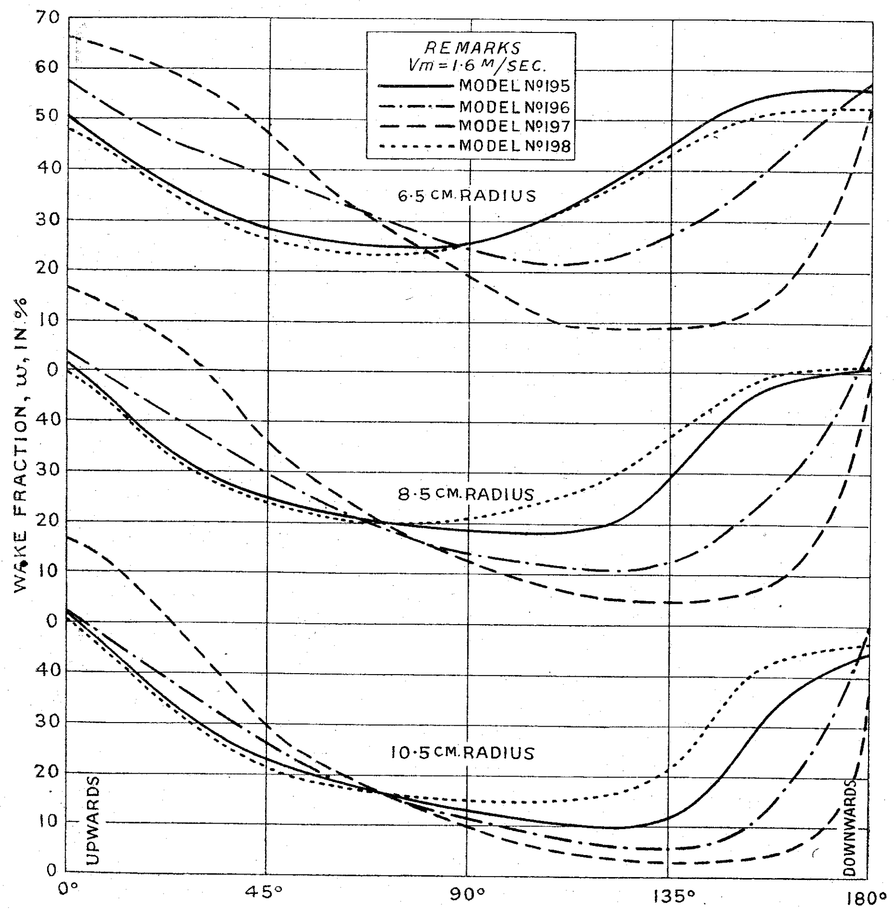

FIG. 7.—WAKE DISTRIBUTIONS OVER ANNULAR RINGS.

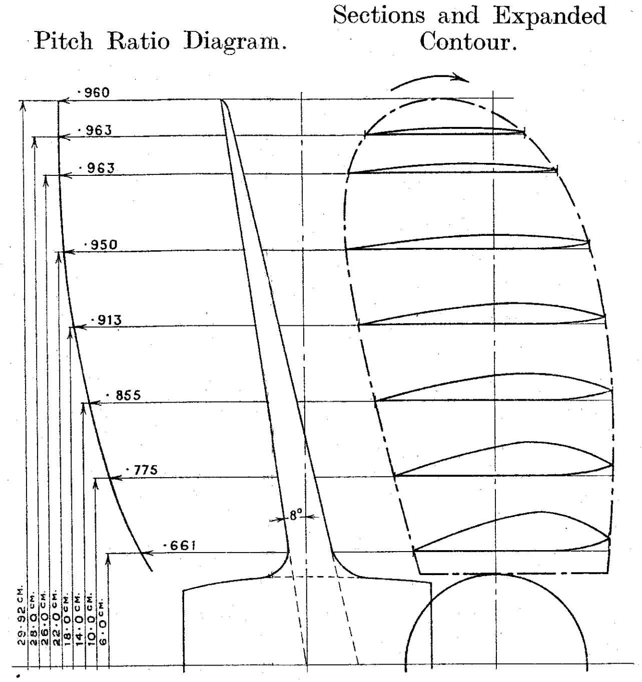

FIG. 8.—PROPELLER NO. 155.

Diameter	..	29·92 cm.
Boss ratio	..	0·160
Pitch ratio at 0·7 R.	..	0·943
Expanded area ratio ..	..	0·407
Blade thickness ratio	..	0·045
Number of blades ..	..	4

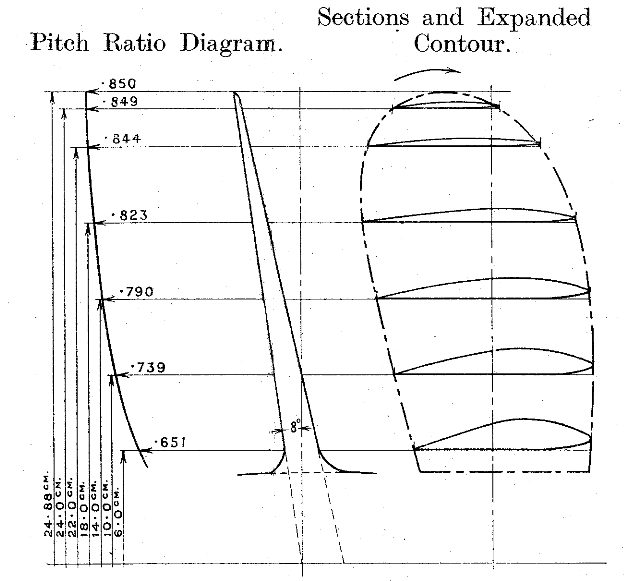

FIG. 9.—PROPELLER NO. 156.

Diameter	..	24·88 cm.
Boss ratio	..	0·193
Pitch ratio at 0·7 R.	..	0·820
Expanded area ratio	..	0·407
Blade thickness ratio	..	0·045
Number of blades ..	..	4

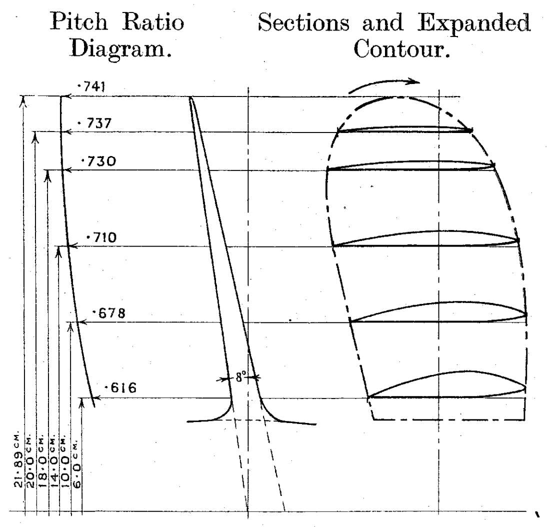

FIG. 10.—PROPELLER NO. 157.

Diameter ..	..	21·89 cm.
Boss ratio	..	0·219
Pitch ratio at 0·7 R.	..	0·720
Expanded area ratio	..	0·407
Blade thickness ratio	..	0·045
Number of Blades	..	4

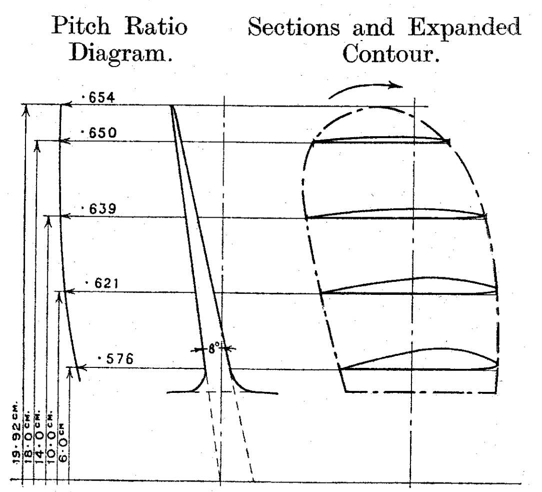

FIG. 11.—PROPELLER NO. 158.

Diameter ..	..	19·92 cm.
Boss ratio	..	0·241
Pitch ratio at 0·7 R.	..	0·638
Expanded area ratio	..	0·407
Blade thickness ratio	..	0·045
Number of blades	..	4

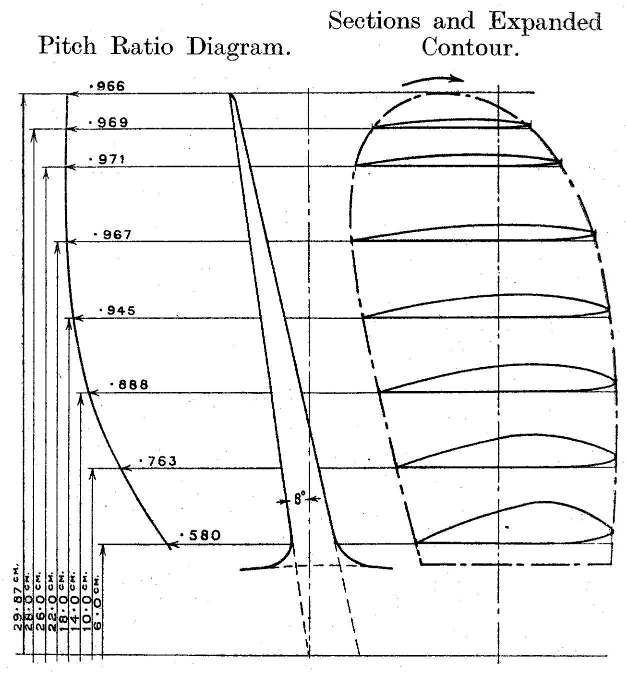

FIG. 12.—PROPELLER NO. 159.

Diameter	..	29·87 cm.
Boss ratio	..	0·161
Pitch ratio at 0·7 R.	..	0·963
Expanded area ratio	..	0·407
Blade thickness ratio	..	0·045
Number of blades ..	..	4

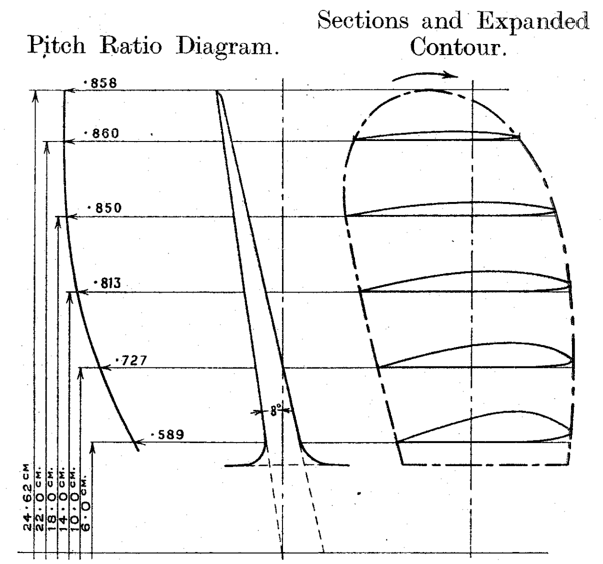

FIG. 13.—PROPELLER NO. 160.

Diameter	..	24·62 cm.
Boss ratio	..	0.195
Pitch ratio at 0·7 R.	..	0·845
Expanded area ratio	..	0·407
Blade thickness ratio	..	0·045
Number of blades ..	..	4

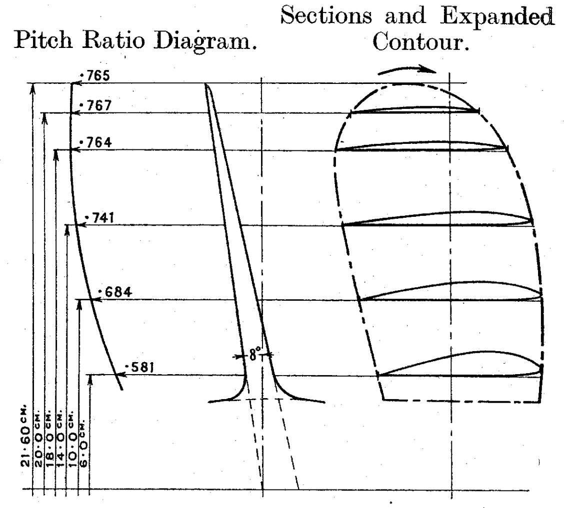

FIG. 14.—PROPELLER NO. 161.

Diameter ..	..	21·60 cm.
Boss ratio	..	0·222
Pitch ratio at 0·7 R.	..	0·750
Expanded area ratio	..	0·407
Blade thickness ratio	..	0·045
Number of blades	..	4

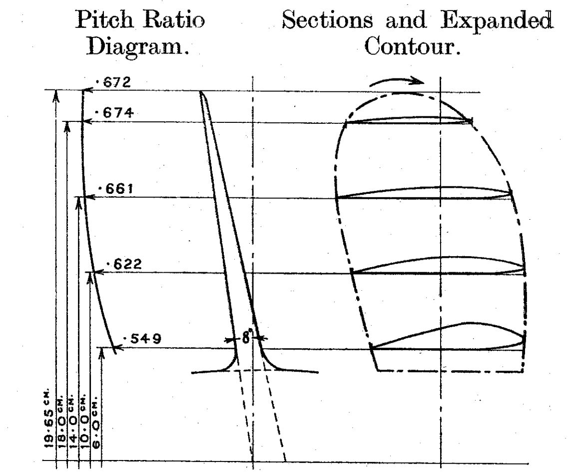

FIG. 15.—PROPELLER NO. 162.

Diameter ..	..	19·65 cm.
Boss ratio	..	0·244
Pitch ratio at 0·7 R.	..	0·660
Expanded area ratio	..	0·407
Blade thickness ratio	..	0·045
Number of blades	..	4

To Illustrate Mr. Masao Yamagata's Paper on "Model Experiments of the Combined Effect of Aft-body Forms and Propeller Revolutions upon the Propulsive Economy of Single-screw Ships."

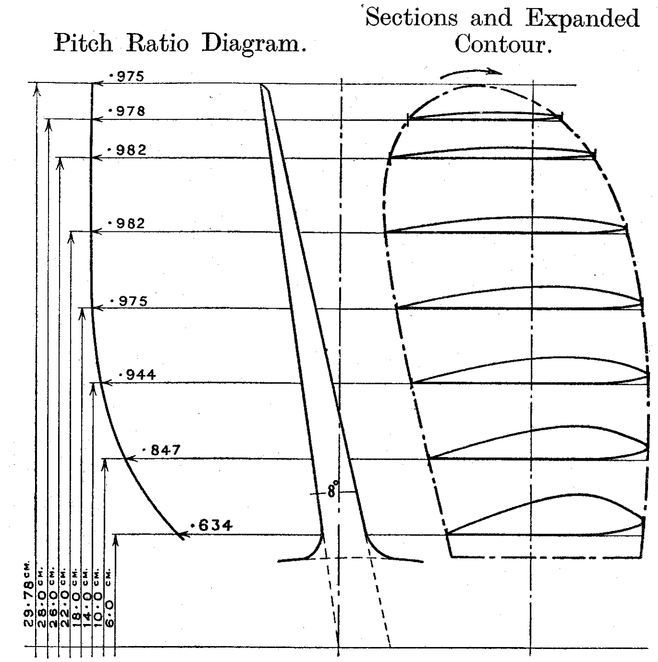

FIG. 16.—PROPELLER NO. 163.

Diameter	29·78 cm.
Boss ratio	0·161
Pitch ratio at 0·7 R. ..	0·981
Expanded area ratio ..	0·407
Blade thickness ratio ..	0·045
Number of blades	4

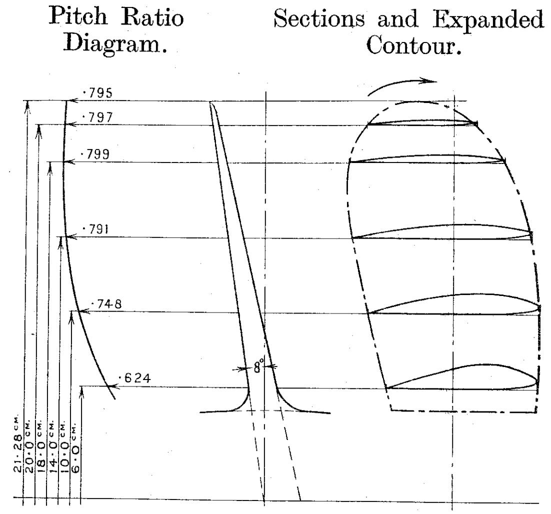

FIG. 18.—PROPELLER NO. 165.

Diameter	21·28 cm.
Boss ratio	0·226
Pitch ratio at 0·7 R. ..	0·795
Expanded area ratio ..	0·407
Blade thickness ratio ..	0·045
Number of blades ..	4

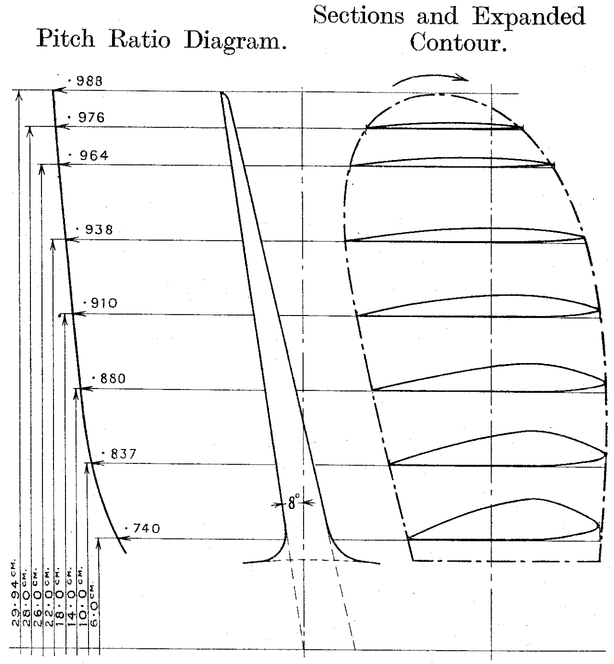

FIG. 20.—PROPELLER NO. 167.

Diameter	29·94 cm.
Boss ratio	0·160
Pitch ratio at 0·7 R. ..	0·931
Expanded area ratio ..	0·407
Blade thickness ratio ..	0·045
Number of blades ..	4

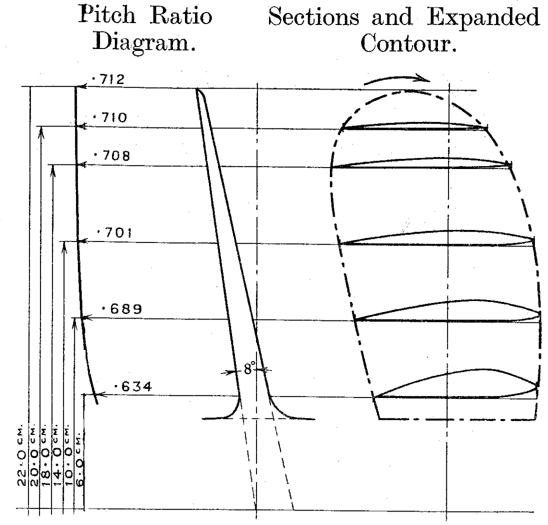

FIG. 22.—PROPELLER NO. 169.

Diameter	22·00 cm.
Boss ratio	0·218
Pitch ratio at 0·7 R. ..	0·704
Expanded area ratio ..	0·407
Blade thickness ratio ..	0·045
Number of blades ..	4

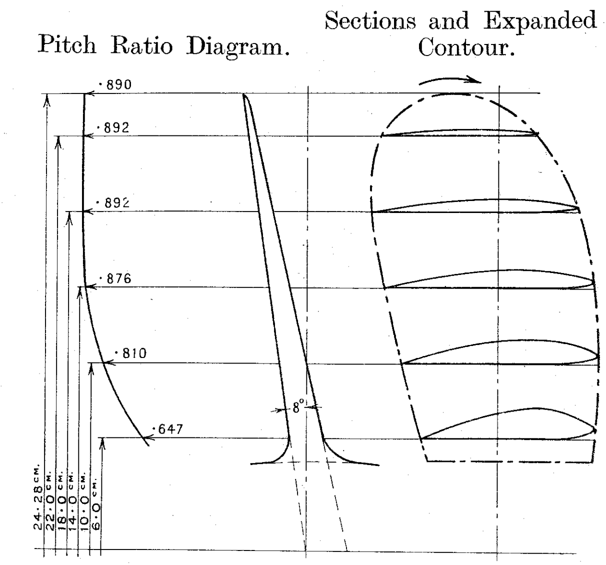

FIG. 17.—PROPELLER NO. 164.

Diameter	24·28 cm.
Boss ratio	0·198
Pitch ratio at 0·7 R. ..	0·891
Expanded area ratio ..	0·407
Blade thickness ratio ..	0·045
Number of blades	4

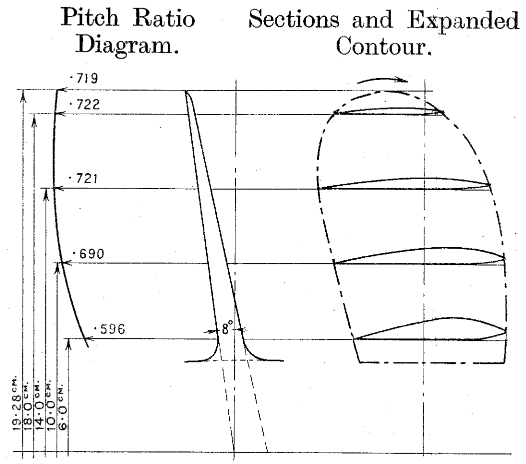

FIG. 19.—PROPELLER NO. 166.

Diameter	19·28 cm.
Boss ratio	0·249
Pitch ratio at 0·7 R. ..	0·720
Expanded area ratio ..	0·407
Blade thickness ratio ..	0·045
Number of blades ..	4

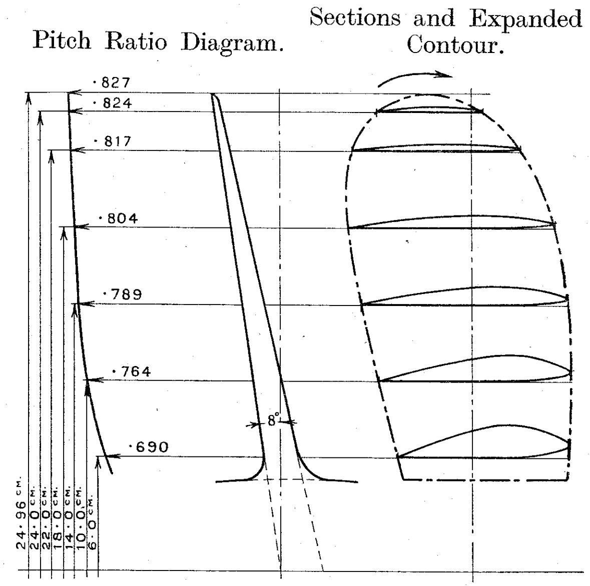

FIG. 21.—PROPELLER NO. 168.

Diameter	24·96 cm.
Boss ratio	0·192
Pitch ratio at 0·7 R. ..	0·802
Expanded area ratio ..	0·407
Blade thickness ratio ..	0·045
Number of blades ..	4

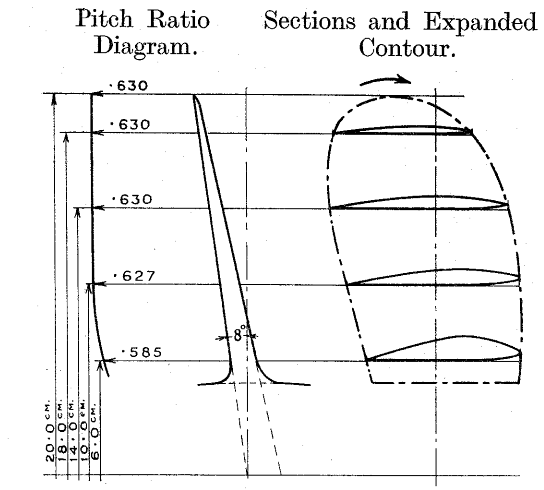

FIG. 23.—PROPELLER NO. 170.

Diameter	20·00 cm.
Boss ratio	0·240
Pitch ratio at 0·7 R. ..	0·630
Expanded area ratio ..	0·407
Blade thickness ratio ..	0·045
Number of blades ..	4

To Illustrate Mr. Masao Yamagata's Paper on "Model Experiments of the Combined Effect of Aft-body Forms and Propeller Revolutions upon the Propulsive Economy of Single-screw Ships."

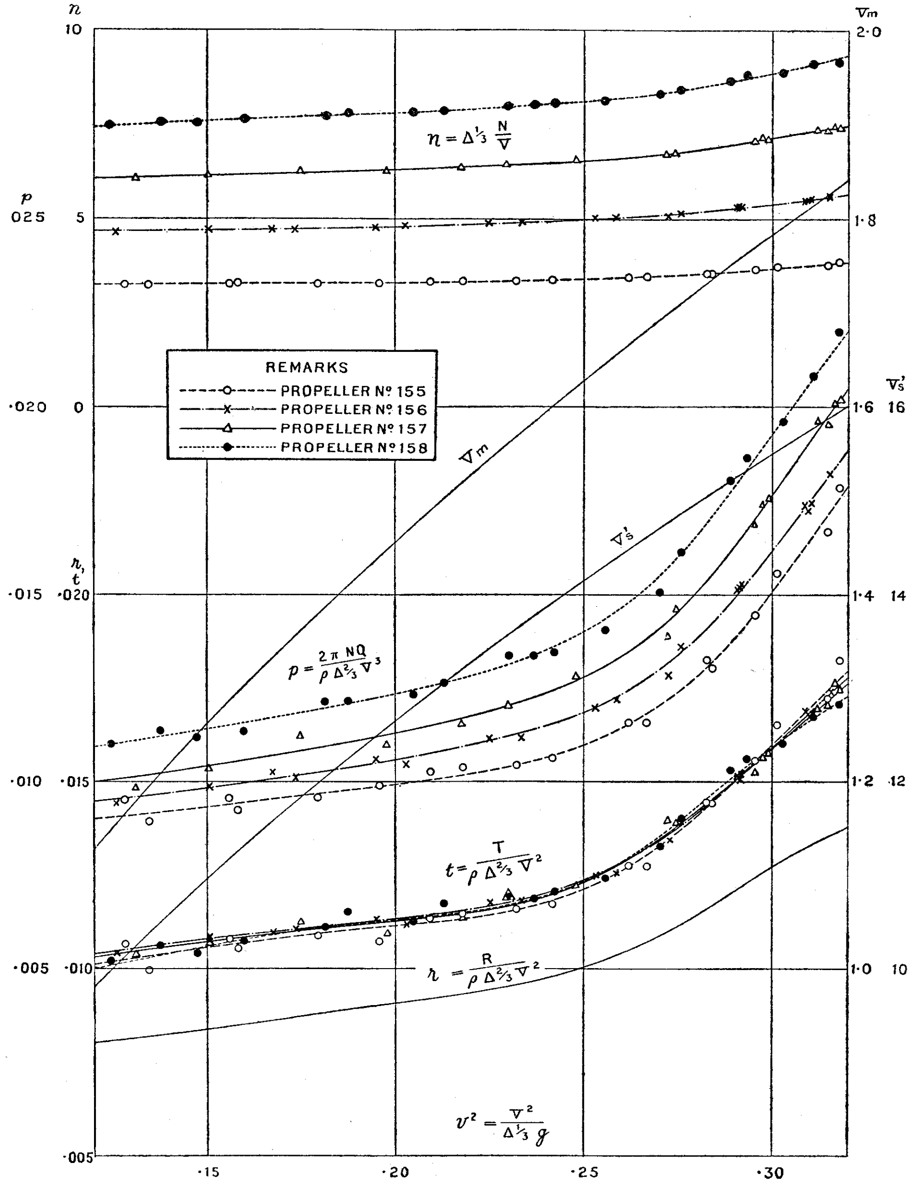

Fig. 24.—Results of Self-propulsion Tests of Model No. 195.

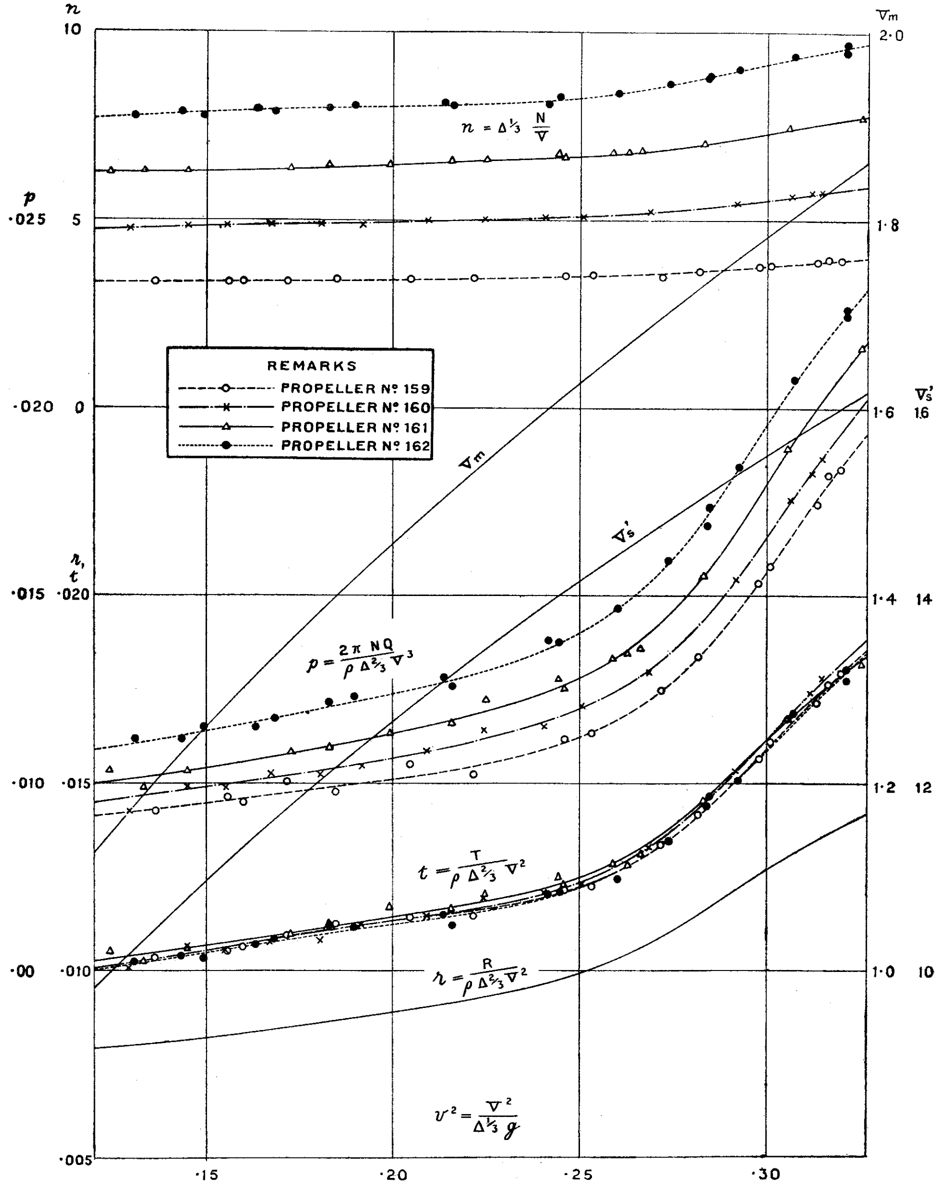

Fig. 25.—Results of Self-propulsion Tests of Model No. 196.

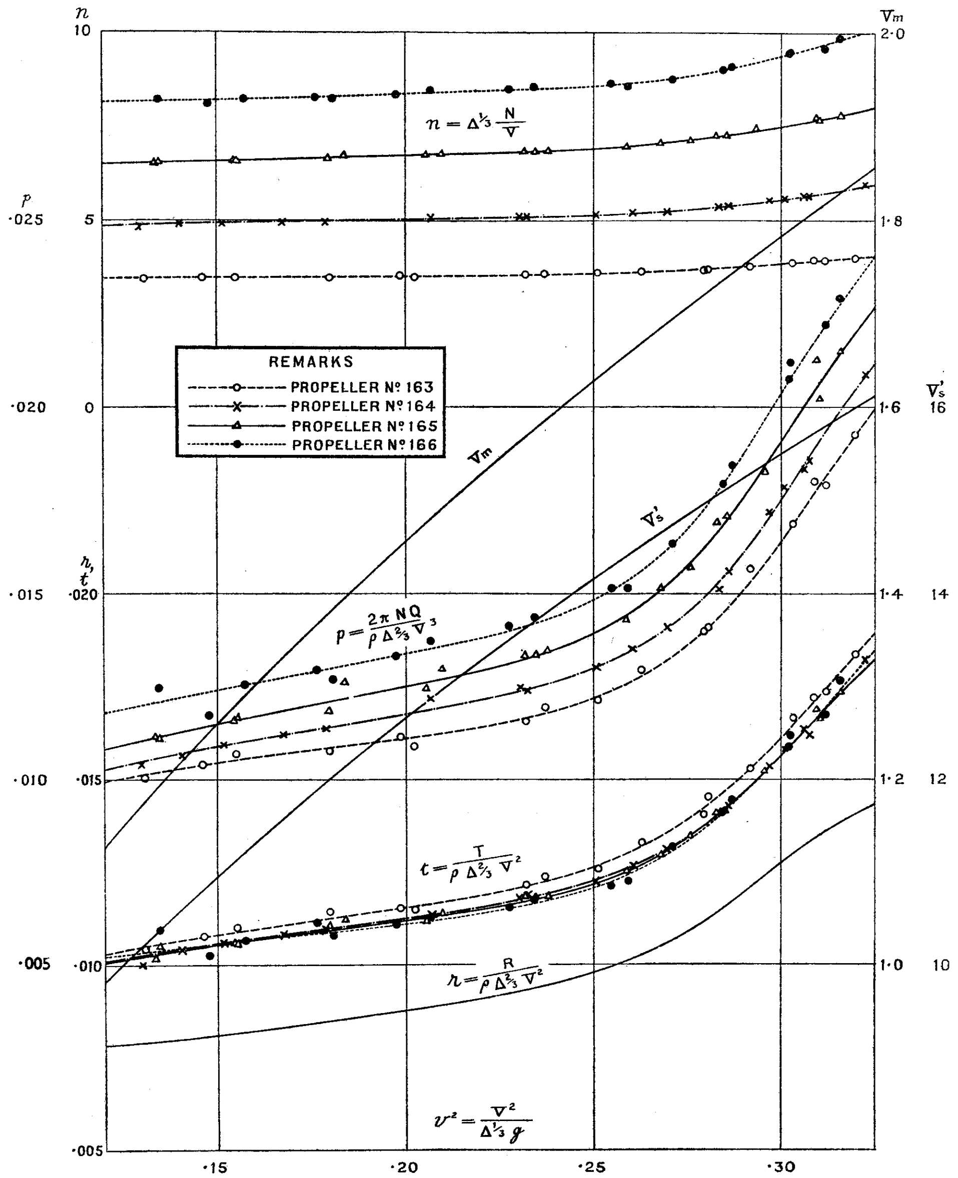

Fig. 26.—Results of Self-propulsion Tests of Model No. 197.

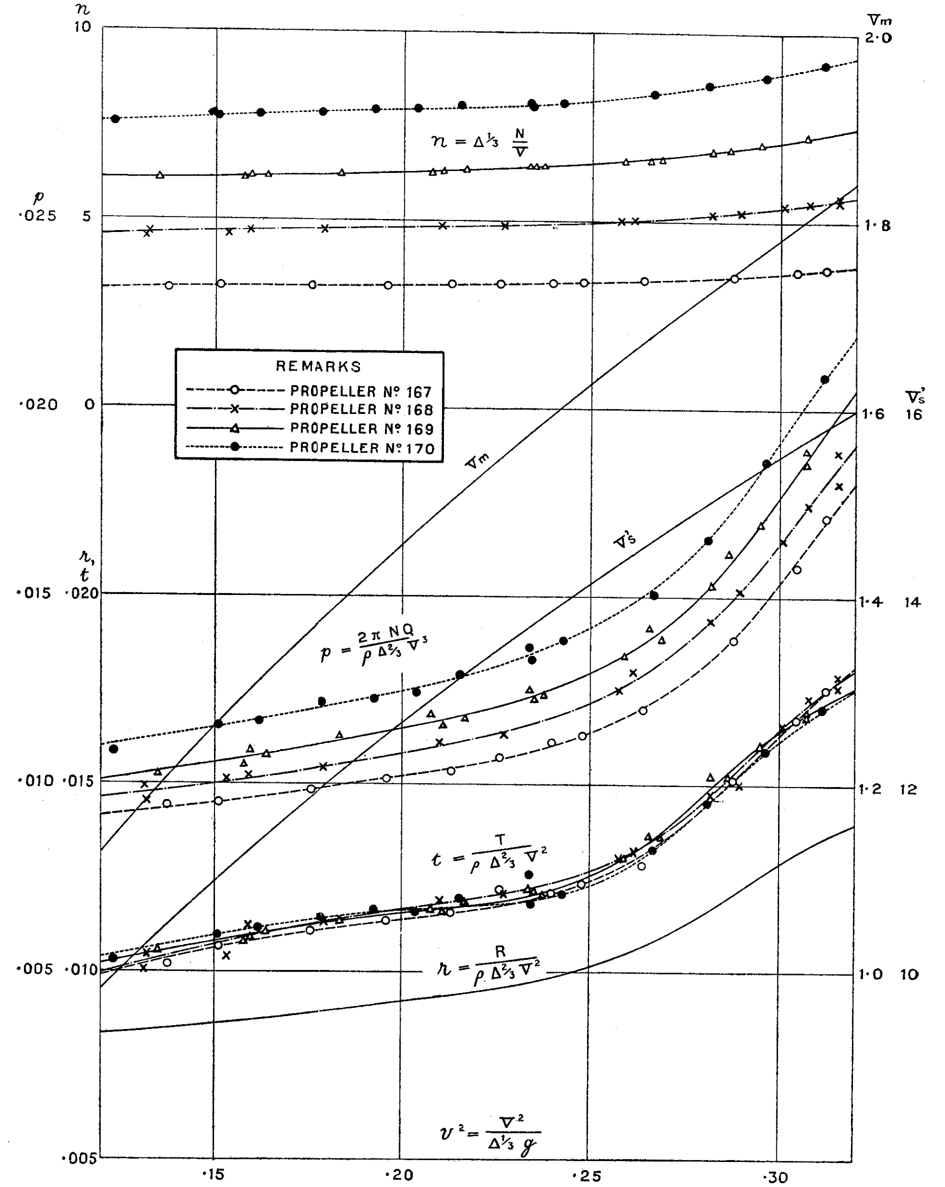

Fig. 27.—Results of Self-propulsion Tests of Model No. 198.

Note.—In the above figures Δ should read ∇.

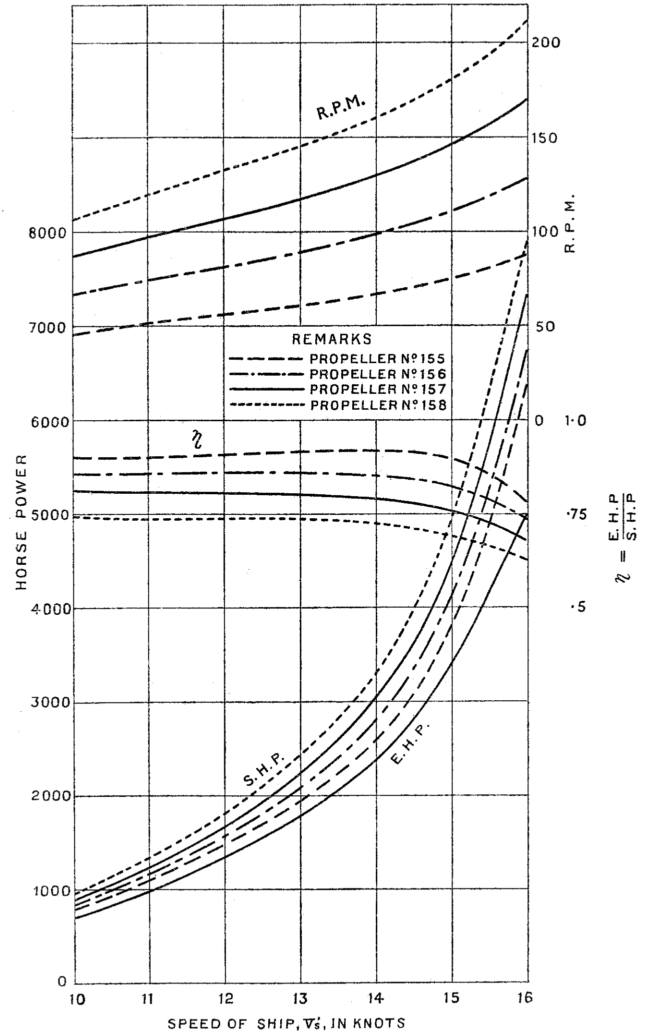

Fig. 28.—S.H.P., R.P.M. and η Curves for 120-m. Vessel. (Model No. 195.)

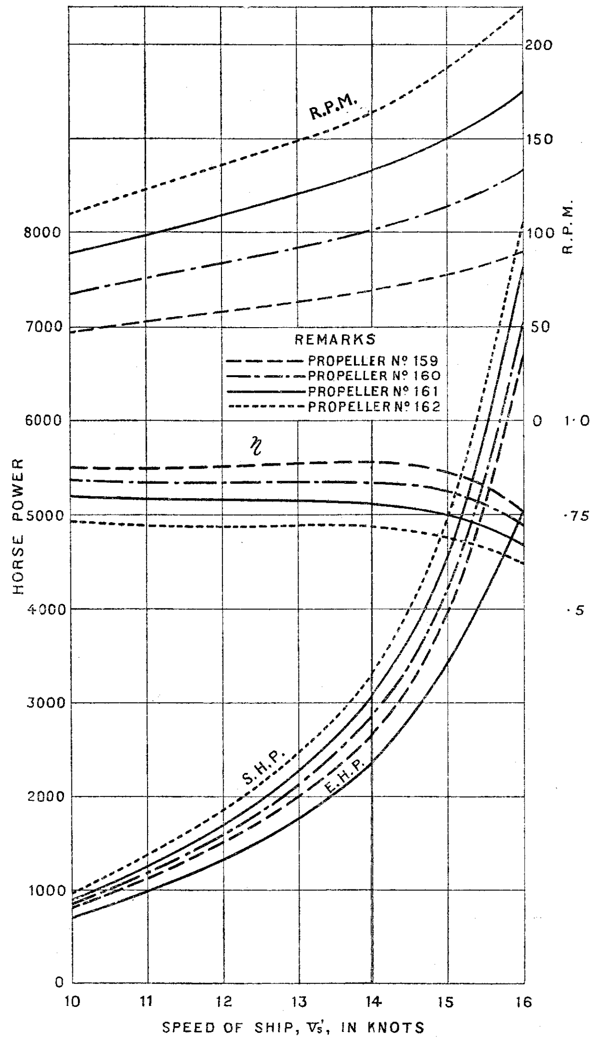

Fig. 29.—S.H.P., R.P.M. and η Curves for 120-m. Vessel. (Model No. 196.)

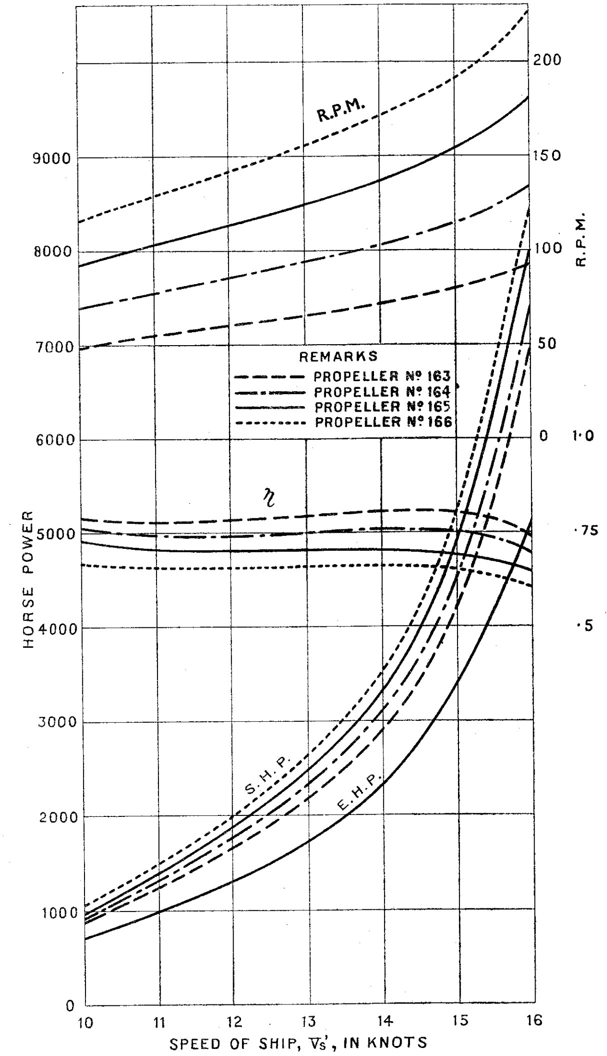

Fig. 30.—S.H.P., R.P.M. and η Curves for 120-m. Vessel. (Model No. 197.)

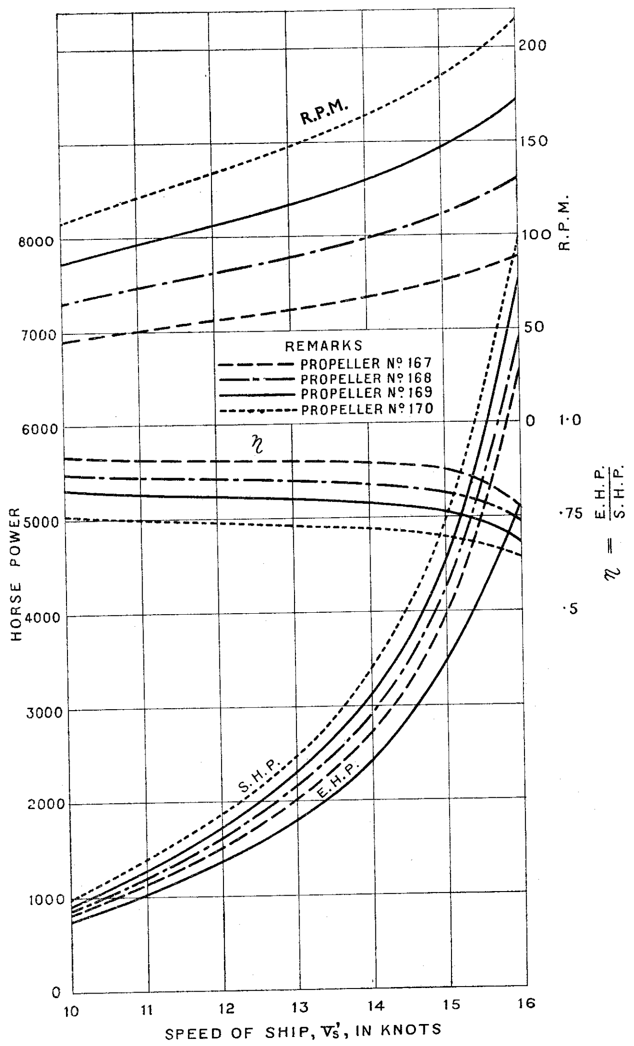

Fig. 31.—S.H.P., R.P.M. and η Curves for 120-m. Vessel. (Model No. 198.)

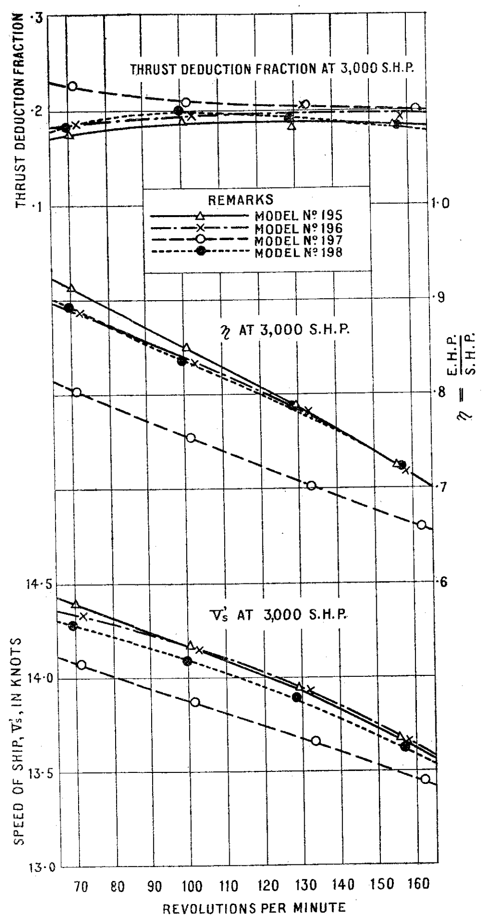

Fig. 32.—Estimated Results at 3,000 S.H.P. on a Base of R.P.M.

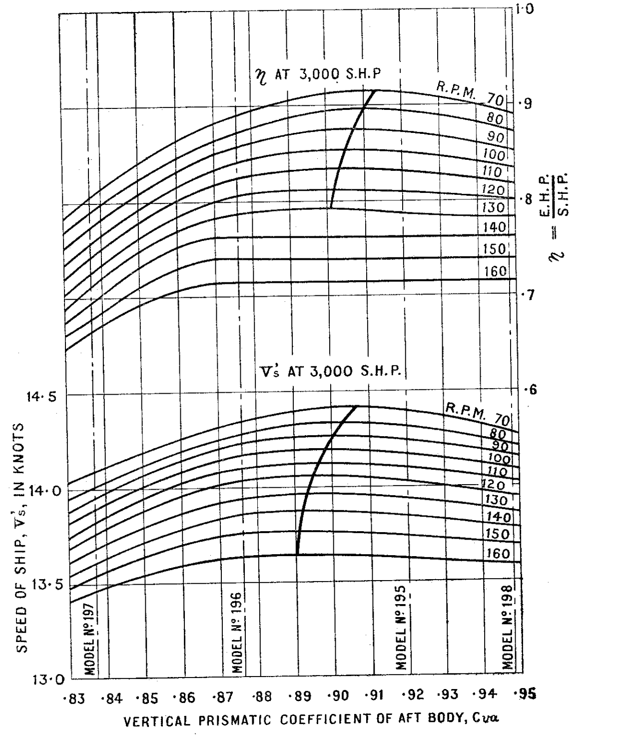

Fig. 33.—Estimated Results at 3,000 S.H.P. on a Base of C_{va}.

SHIP PERFORMANCE IN RELATION TO TANK RESULTS.

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THE primary objective of all experiment Tank work is the accurate prediction of ship performance from the results of model experiments. It is accordingly felt that no apology is necessary in placing before this conference some observations based on an extensive experience of speed trials as a contribution to the growing interest manifested in this aspect of our work.

The purpose of the present paper is not to bring to your notice ships in which favourable speeds have been successfully obtained, but to consider the analytical and statistical aspect of speed trial records, particularly in their relation to the basis for the prediction of ship performance from model experiment results. To enable this to be done, a systematic and detailed analysis of the results of many ship trials is necessary, and a fairly thorough yet readily applicable system of analysis, which in its original form was evolved by R. E. Froude, has been in use at Haslar for many years. So far as the author is aware, the method has not been previously published, and as it forms the medium from which the considerations in the present paper are derived, a brief description of the method has been included in Appendix I. The method, which has been considerably amplified in recent years, has been found consistently serviceable for comparing model and ship, for indicating any abnormalities in performance, such as the effects of cavitation and shallow water, if present, and for exposing any errors in trial records.

§ 2. The provision of experiment Tank facilities is now so widespread, and the powering of ships from model results such a generally accepted process, that many are apt to overlook the fundamental assumptions that render it possible for prediction of ship performance to be made with any pretensions to accuracy from model data. This successful prediction was not always possible, and many peculiar and untrustworthy deductions were made from model experiments before William Froude propounded the all-important Law of Comparison for interpreting the resistance of similar ships and models in 1868. The realization of this simple but fundamental truth, coupled with the careful and painstaking method of experiment first introduced by Froude and now so universally followed, still forms the foundation on which our prediction of ship performance rests. Certain qualifications as to the unreserved application of this Law of Comparison, particularly as applied to the resistance of ships, have been suggested by many eminent authorities in the profession from time to time. The only one generally adopted, so far as the author is aware, is, however, that propounded by Froude himself as a result of his experiments at Torquay on the skin friction of thin planks.* As recently as last year, a Conference of Tank Superintendents † recommended

* See Report to Admiralty by W. Froude, dated 1872 (printed in Reports of British Association of 1872 and 1874).

† See Report of Congress of Tank Superintendents, The Hague, 1933.

that not only Froude's method, but also his actual coefficients, should continue to be used in the presentation of ship results. In view of the admittedly speculative character of Froude's extrapolation of the results of his plank experiments to ships, a fair amount of attention has been devoted in this paper to an examination of the degree to which this confidence is borne out by speed trial results. If the tow rope resistance of the ship is incorrectly assessed, then many other elements in the trial analysis are also incorrect.

§ 3. I would like first briefly to review the records that are available for trial analysis. In H.M. Navy, the ship trials include a series of four or more runs at each speed in a range of progressive speeds on a measured mile course, generally in deep water, shortly after the ship has come out of dock. On each of these individual runs at a given speed, various records are taken, and from these, among others, the following data are deduced: viz. mean of means speed, mean of means revolutions per minute, and mean of means shaft horse-power. As regards model data, at the Haslar Tank the model is first towed without appendages to obtain the resistances at various speeds. The model results are then corrected to the standard temperature of 55° F., and a curve of effective horse-power for the ship is deduced, using R. E. Froude's O_M and O_S constants.* A series of runs with the model still naked are then made with the screw propellers, approximately of correct diameter, working behind the model.† From these results the hull efficiency elements are deduced, viz. wake, augmented resistance percentage, and relative rotative efficiency. In addition, the screw efficiency is also used in the trial analysis; this is not usually derived from experiments on the ship model itself, but is assessed from the results of a methodical series of screw experiments.‡

§ 4. A discussion on the accuracy of the results and reference to the probable errors that occur from time to time in the records are given in Appendix I. Ideally, the model T.H.P. $Ⓣ_m$ § should agree with the propeller T.H.P. $Ⓣ_r$ throughout the speed range, though actually in the analysis more reliance is placed on the parallelism of these curves rather than on their identity. $Ⓣ_m$ varies directly as the wake factor, but the $Ⓣ_r$ curve is much more acutely sensitive to it, and is as an average proportional to the third or fourth power of the wake factor. It is probable that the wake percentage in the ship is less than in the model on account of the smaller proportion of skin friction resistance in the ship, though the correction on this account is uncertain. The results of experiments on models (described in Appendix II.) suggest a possible reduction of 5 per cent. to 6 per cent. in passing to the ship. Actually, however, it has been found that appendages (in warship models), when fitted, have resulted in an increase of wake of about 5 per cent. compared with the naked model. It is therefore possible that the wake percentages deduced from our naked model experiments apply to the ship within fairly narrow limits. In this connection it should be remembered that, as a rule, in the forms dealt with at Haslar the wake percentages (and also the augmented resistance percentages) are not large. As regards the augmented resistance per cent., the experiments described in Appendix II. suggest that it is not an unreasonable inference to say that the figures for augment in models should not be greatly modified in passing to full scale on account of the skin friction correction, and experiments on models fitted with appendages show that their effect on augment is small. The augmented resistance percentage in the model relates to model resistance at the point of model self-propulsion. If it related to equivalent ship resistance at the equivalent

* See Trans. I.N.A., Vol. XXIX., p. 304.

† For method, see Trans. I.N.A., Vol. XXIV., p. 231.

‡ See Trans. I.N.A., Vol. L., p. 185.

§ It may be stated here that part of the trial analysis consists of the plotting as ordinates, to a base of speed, the following quantities: (*a*) Ⓔ, the E.H.P. constant; (*b*) $Ⓣ_m$, the T.H.P. constant, i.e. Ⓔ/Hull efficiency; (*c*) $Ⓣ_r$, the T.H.P. constant estimated from the revolutions of the propellers (*d*) Ⓓ, the D.H.P. constant, i.e. $Ⓣ_r$/Screw efficiency; and (*e*) Ⓘ, the S.H.P. constant. For further details, see Appendix I.

speed of the ship self-propulsion a slight increase in the figure would obtain. On the whole it would appear probable that the augment percentage may be greater than is deduced from our model experiments. This conclusion receives some support from trials in which propeller thrusts have been recorded,* and, if accepted, would result in Ⓣ_m being increased by a small percentage.

§ 5. Apart from the wake referred to above, the pitch ratio is an uncertain factor in the estimation of propeller T.H.P. Ⓣ_r. Ideally, the pitch represents the advance per revolution at zero thrust, and it was stated by R. E. Froude in his 1908 paper that it could be taken to be 1·02 times the face pitch of the propeller. This figure was obtained as a result of an analysis of a number of trials of ships a little prior to that time. The ratio between the analysis pitch and the face pitch can be conveniently referred to as the *pitch factor*. Its exact significance was the subject of subsequent discussion at the meetings of this Institution,† and its correct assessment is of considerable importance in trial analysis, owing to its bearing on the computation of slip (and consequently thrust) at which the propeller is working at known revolutions for a given speed. It has been the practice for many years to deduce the value of the pitch factor from trial results, and in all recent cases the factor of 1·02 is found to be unduly low. A good average based on the trials of more than fifty ships is 1·05, and is found to be almost invariable, if abnormalities such as cavitation are ignored. The figure of 1·05 allows for an estimated addition to the naked E.H.P. for appendages, and also assumes that the wake and augmented resistance per cent. are correctly assessed. If Ⓣ_m is under-stated, as is suggested in § 4, then the pitch factor of 1·05 is also in the low side.

§ 6. The D.H.P. Ⓓ will naturally be subject to the same corrections as the T.H.P. discussed in § 5, and, in addition, its value is dependent on the screw efficiency. The screw efficiency is evaluated from the curves in R. E. Froude's 1908 paper at the appropriate real slip, pitch ratio, and disc-area ratio of the propeller. Any correction to the screw efficiency due to faulty assessment of wake or pitch-factor as discussed above is in practice of a small order. The screw efficiency is, however, very sensitive to the blade-area proportion. The largest disc-area ratio of the model propellers was 0·75, but many of the propellers fitted in ships subsequent to the experiments have had analysis disc-area ratios of the order of unity, and a considerable extrapolation of the experiment data has been found necessary. In view of the comparatively rapid decline in screw efficiency with increase of disc-area ratio, this extrapolation can only be regarded as speculative, and moreover the small boss of the model screws leads to ambiguity in the interpretation of the disc-area ratio. R. E. Froude preferred to make a discount for that portion of the blade within the boss. This discount was taken as 20 per cent. of the blade area, and the analysis disc-area ratio was conventionally reckoned on this basis. For modern screws it is considered that such an allowance is excessive, and the disc-area ratio has been alternatively computed by relating the developed blade area to the area of the annulus between the boss and the periphery of the propeller. With the lower disc-area ratios thus obtained the reputed propeller efficiencies are increased. Again, it would appear from general considerations that the screw efficiency would be somewhat greater in the full scale than in the model, and this receives some support from the published results of experiments on large-scale propellers. On the other hand, the boss of R. E. Froude's model screws is proportionately smaller than the boss of the ship screws, as a result of which the efficiency is over-stated to a small degree. In addition, the blade thickness ratio of the model propellers was only some two-thirds of that of the ship's propellers, and although the effect of this will vary with the type of propeller, it is probable that the model screw efficiencies are favoured thereby. Thus the effect of the difference between the model screws and the ship screws

* See Trans. I.N.A., Vol. LVII., p. 301; also *Transactions N.E. Coast Inst.*, 1925, paper by G. S. Baker, "Measured Mile Trials."

† See Trans. I.N.A., Vol. LXII., pp. 316–21.

as regards geometry and scale is in the direction of cancelling out, though our knowledge is not yet sufficiently definite to enable us to say whether the efficiency is likely to be greater or less in the ship than in the model. Bearing in mind that it is difficult to determine the efficiency of a model propeller within 1 per cent. or 2 per cent., and that the ship results are less accurate than this, it would seem fair to conclude that the model figures may be regarded as reasonably applicable to the ship. The subsequent portion of the paper generally endorses this viewpoint.

§ 7. A feature to which great importance is attached in all trial analysis is the *propulsive coefficient*, i.e. the ratio of the E.H.P. of the ship (generally ex appendages) to the S.H.P., as it can be regarded as an overall figure representing the efficiency of propulsion. This can be compared with a similar coefficient obtained synthetically from the model data, which has been termed the *quasi*-propulsive coefficient.* This coefficient is the continued product of the reputed screw efficiency, relative rotative efficiency, and hull efficiency divided by the appendage coefficient. The ratio of the propulsive coefficient to the quasi-propulsive coefficient has for some years past been evaluated in our trial analysis. Taking a general average of all speeds of trial (neglecting a few doubtful records), we obtain the following figures for the mean ratio in the types of ship named. The figures in column (1) are evaluated on conventional lines, those in column (2) relate to screw efficiencies appropriate to disc-area ratios outside the boss.

TABLE I.

RATIO PROPULSIVE COEFFICIENT/QUASI-PROPULSIVE COEFFICIENT.

Type of Ship.	Mean of all Speeds (ex-cavitation).	
	(1)	(2)
Capital ships	0·82	0·79
Cruisers	0·92	0·86
Destroyers	0·95	0·85

It is of course so far satisfactory that the ratios in the above table fall short of unity, albeit by a different margin in the various types of ship. In order that the quasi-propulsive coefficient should be brought to a truly comparable basis with the actual propulsive coefficient an allowance must be made in the former for the air resistance of the ship and the bearing friction of the propeller shafts. The proportional effect of these factors will naturally vary with speed in the same ship, and, having regard to differences in top-hamper and in size and character of machinery, can hardly be expected to be the same in different types of warship. It is a moot point as to how far we should expect the skin friction O_S values to allow for the effect of laps and rivets on the ship. Their effect is admittedly important, and will vary with the type of ship to some extent. Another relevant point is "form effect" in skin friction resistance. In our model work it is virtually treated as residuary resistance, and as a result the ship resistance estimate is exaggerated thereby, though probably only to a small extent. Although not important, the correction for this factor would decrease by a small amount the ratios in Table I., and is therefore contrary in tendency to the effect of an allowance for the other three factors mentioned. Here again the correction would be of a different proportion in the several types of ship.

* See Trans. I.N.A., Vol. LXIII., p. 25.

Another factor by no means negligible is that of weather. Speaking generally, the weather conditions for all the trials have been satisfactory, but, obviously, ideal conditions such as obtain in the model experiments are rare, and some allowance should therefore be embodied for the effect of wind and sea. A number of important investigations, both analytical and experimental, have been made from time to time to ascertain the effect of some of the above factors. So far as the author is aware, their actual magnitude is still a matter of some uncertainty. Individually their effect is not serious, but collectively their importance cannot be lightly dismissed. As a plausible estimate it is suggested that it is not unreasonable to assess the magnitude as 10 per cent.—some may prefer a still higher figure. Bearing this in mind, the average value of 90 per cent. in column (1) and 83 per cent. in column (2) is not such a serious departure from unity as would at first sight appear, although the amount of the difference cannot be regarded as satisfactory. The various propeller data used in the analysis have already been reviewed, and in the next section of this paper further evidence relating to the validity of the method adopted for assessing the skin friction correction is examined. Before leaving Table I. it is of interest to note that although for the reasons detailed above, differences in the quasi-propulsive coefficient factors for different classes of ships might well be anticipated, the order of the difference in column (2) is more rational than in column (1), and can be regarded as lending some support to the alternative basis of computation adopted.

Basis for Computing E.H.P.

§ 8. The remarks in § 5 suggest that the augmented resistance as usually deduced from model results is generally greater than that deduced from Froude's model propeller data, and that a possible explanation lies in an under-statement of the pitch factor. Correct assessment of the pitch factor within narrow limits is of great importance, since a 1 per cent. change in the factor leads to a change in the computed thrust of about 3 to 4 per cent., depending on the slip ratio. In the model propeller experiments the pitch factor was found to be 1·11, 1·09, 1·07, and 1·05 for nominal face pitch ratios of 0·8, 1·0, 1·22, and 1·46 respectively. If the pitch factor of the ship screws be correctly assessed by the model screw experiments, a further increase in thrust and consequently of ship resistance is indicated. Such an implied increase in ship resistance, if genuine, would increase the propulsive coefficient, and of course its ratio to the quasi-coefficient (see § 7), which on the whole appears a step in the right direction.

§ 9. In the Report of the Skin Friction Committee * examples were given of the computation of the skin friction resistance by various data which emphasized the uncertainty underlying the estimate of this portion of the resistance. The estimates varied widely, those obtained from Dr. Kempf's formula being much higher than others, and those obtained from Dr. Gebers' formula being on the low side. It is of interest to compare the resistances of different ships with the skin friction element computed on (1) Haslar data, (2) the formula of Dr. Kempf, and (3) the formula of Dr. Gebers, with the thrust imputed to the propellers. So far, in the present paper, when the thrust of the propeller has been considered, it has been regarded as that derived from its characteristics in association with known revolutions. A computation of thrust can also be made from S.H.P. if the efficiency of the propeller and its speed of advance through the (wake) water are known. The thrusts so computed are designated *S.H.P. thrusts*, and are found to be reasonably in accord with the thrusts derived from revolutions. The degree of agreement is obviously dependent on the assumption as to pitch factor, and alternative factors of 1·02 and 1·1 have each been considered. For the smaller ships, the pitch ratios of the propellers are generally higher than those for capital ships, and a pitch factor less than 1·1 (possibly 1·08) would be experimentally justified for the former, and would result in a much closer agreement of

* See Trans. I.N.A., Vol. LXVII., p. 108.

the thrusts computed from revolutions and S.H.P. thrusts. It should be observed that a deduction of 5 per cent. has been made from the S.H.P. thrusts to allow for bearing friction, and it is assumed that the screw and hull efficiencies are as deduced from model experiments. The thrusts and resistances deduced from the various assumptions for a large number of ships are compared in the tables below, average figures being given for the classes of ship named—the S.H.P. thrust being taken as unity in each case. In Table II., the figures are evaluated on conventional lines, using the analysis disc-area ratio and analysis pitch ratio as defined by R. E. Froude in his 1908 paper. In Table III., the disc-area ratio has been based on the annulus outside the boss, and the corresponding correction applied to thrust and efficiency, using Froude's data.

§ 10. There is seen to be better overall agreement between the ratios for the different classes of ship in Table III. than in Table II., suggesting that the corrections for the disc-area ratio are in the right direction, and they may perhaps be regarded as serviceably acceptable.

TABLE II.

Ratio of Computed Propeller Thrusts and Augmented Ship Resistances to S.H.P. Thrusts.

Type of Ship.	(1) Assumed Pitch Factor, 1·1.	(2) Assumed Pitch Factor, 1·02.	(3) Haslar.	(4) Kempf.	(5) Gebers.
Capital ships	1·03	0·83	0·90	1·08	0·86
Cruisers	1·12	0·89	0·97	1·10	0·95
Destroyers	1·17	0·87	1·00	1·11	0·98

TABLE III.

Type of Ship.	(1) Assumed Pitch Factor, 1·1.	(2) Assumed Pitch Factor, 1·02.	(3) Haslar.	(4) Kempf.	(5) Gebers.
Capital ships	0·98	0·79	0·86	1·01	0·82
Cruisers	1·06	0·83	0·91	1·04	0·89
Destroyers	1·06	0·76	0·90	1·00	0·88

In computing the thrust from ship resistance, no allowance has been made for the effect of air resistance, surface discontinuities, and weather on account of the lack of sufficiently precise data. Some addition for these factors should be applied to each of the last three columns in the above tables, and it is suggested that 5 per cent. is by no means an excessive amount. Bearing this in mind, the Kempf formula would appear to over-state the resistance by nearly the same percentage as the Haslar data under-states it, but it can be assumed that the Kempf formula includes an allowance for surface discontinuities, in which case the excess would be reduced slightly. Gebers' formula gives results which are a little lower than Haslar. These conclusions assume that screw efficiencies, wake, and bearing friction, etc., are correctly assessed for the ship. In Tables II. and III. it is considered that the limits of variation of estimated thrusts and screw efficiencies have been allowed for. The figures suggest that the O_M and O_S values, also Kempf's formula, are reasonably reliable in a comparative sense as between various ships, although possibly less so in an absolute sense.

§ 11. It would also appear from Tables II. and III. that an average value approaching 1·1 for the pitch factor is nearer approximation to the truth than the factor of 1·02.

This means that the pitch factor of the ship screws is on the average of the same order as that of the model screws as determined by Froude, Taylor, and other experimenters. Although, however, the initial adoption of the 1·02 factor was rather arbitrary, and amounted to a tacit assumption that the model screw predictions in this respect were largely in error and the ship resistance predictions from model results serviceably correct, it is still in the majority of our ships an appropriate figure to adopt for designing screws to develop a thrust horse-power derived from the estimated naked E.H.P. qualified by the hull efficiency.

§ 12. Apart from the absolute value of the various comparisons of thrust, a point of interest arises in the trial analysis that in our longest ships, namely, *Hood*, *Renown*, and *Repulse*, the thrust deduced from model E.H.P. shows some excess over that from propeller revolutions; in the analysis nomenclature, $Ⓣ_m$ shows a fair excess over $Ⓣ_r$. Further, in these ships, the quasi-propulsive coefficient factor is nearer unity than in shorter ships of the same type. This result suggests that the decline of the skin friction coefficient with increase in length may be steeper than has actually been assumed (for coefficients, see paper by Professor T. B. Abell), though a larger number of trials would be necessary before any change would be justified.

Thrust Measurements.

§ 13. The destroyers whose trials have been considered include the flotilla leader *Mackay*, in which the thrusts of the propellers were measured on trial.* The measured thrusts (in tons) of the deep load trials compare with the deduced thrusts and the augmented resistances as in the following table.

TABLE IV.

Speed (Knots).	Recorded Thrust.	Thrust from Revolutions (Pitch Factor, 1·1).	S.H.P. Thrust.	Augmented Resistance from Model.	
				(Kempf).	(Haslar).
32·4	115·6	122·5	119·0	112·2	100·9
31·2	108·9	111·8	107·6	103·9	93·7
25·9	64·4	62·3	68·6	64·5	58·1
20·5	33·7	32·2	35·8	30·8	27·2
15·2	16·9	15·1	25·2	14·2	12·4

The detailed analysis revealed that cavitation was present at the two high speeds, also that the S.H.P. is over-stated at the lowest speed.† On the average, the measured thrusts may be considered as about 7 per cent. less than the S.H.P. thrust. This result has not been confirmed by trials in other warships in which the propeller thrusts have been measured. In another destroyer, the recorded thrusts were about 7 per cent. greater than the S.H.P. thrust. On one cruiser, measured thrusts were about 15 per cent. greater than S.H.P. thrust, and on yet another cruiser a good agreement between the measured thrust, S.H.P. thrust, and Haslar augmented resistance was obtained. Although on the whole the measured thrusts are generally in fair excess of the Haslar augmented resistances, the above variations are too wide for any other deductions to be made than that the methods hitherto employed for measuring thrusts on trial are not sufficiently reliable for the purpose in view in this analysis.

* See Trans. I.N.A., Vol. LXII., p. 301.

† See Appendix I.

Cavitation.

§ 14. The performance of high-speed ships is almost invariably prejudiced to a greater or less extent by cavitation, and its existence and quantitative extent is readily revealed by the analysis as described in Appendix I. As this phenomenon is not present in the model, the prediction of ship performance from model results is invalidated thereby. The ratios in Tables I., II., and III. apply to speeds at which there is no cavitation.

We find that the occurrence of cavitation is generally associated with either a high-pressure coefficient or a high tip speed of propellers, subject to the immersion of the screws being satisfactory. For destroyers, the limit of tip speed, if cavitation is to be definitely avoided, is found to be about 105 knots, which is associated with a pressure coefficient of about 0·65 ton per sq. ft. of developed blade area. Experience shows that these are severe limits to work to, and force of circumstances frequently compels a compromise in design with acceptance of some risk of cavitation. For larger ships of slower speeds, experience has shown the general desirability of working to pressure coefficients not exceeding 0·75 to 0·85 ton per sq. ft., with the tip speed below 130 knots. It must be stated, however, that these criteria have at times been exceeded without penalty.

Shallow Water.

§ 15. Some of the trials brought under review in this paper were run on the Polperro Measured Mile Course—now superseded by the adjacent Talland Course. The depth of water on the Polperro Course is about 27 fathoms, the actual depth depending on the tide. A few cruisers have been run on this measured mile, and although published shallow-water data indicated that an increase of more than 10 per cent. in the resistance at high speeds was to be expected, in point of fact no increase was indicated by the trial analysis. In another case, of a large ship at about 30 knots speed, a loss of speed of more than 1 knot was estimated from the published data, but the results obtained were sufficiently favourable as to lead one to doubt whether any loss of speed was incurred by the ship. It would appear that for water of this depth the model experiments fail to represent ship conditions, possibly due, among other causes, to the omission of propellers, and perhaps to the skin friction correction being different from that normally assumed owing to the restriction in flow by the limitation of depth.

Conclusions.

§ 16. The trials brought under review in this paper comprise a total of some sixty warships, and the limitation of type must be borne in mind in considering the conclusions, particularly when quantitative deductions are under discussion. The desirability of a still more extensive survey will be readily appreciated when regard is paid to (*a*) the inexplicable differences in performance on trial occasionally shown by ships of the same class, and (*b*) the fact that a large proportion of the trials relate to capital ships, and of necessity the ships of the war-time navy are in a great preponderance in this class. Their machinery was direct-driven turbines, and the propellers accordingly worked at higher slips than the latest vessels, in which the tendency is for the various coefficients to approach nearer to unity than is shown in Table I., and generally in columns (1) and (3) of Tables II. and III.

The general conclusions derived from the survey of the analysis of warship trials may be conveniently summarized as follows:—

(1) The augmented resistance of ships when the skin friction is computed by the O_M—O_S method is a few per cent. less than the estimated propeller thrusts.

(2) While this difference is unsatisfactory, it is not large enough to detract from the accuracy of prediction if the deficiency is corrected by an empirical factor, based on experience with the type of ship under consideration.

(3) When the skin friction of warships is computed by Dr. Gebers' formula, it leads to still lower resistances.

(4) The resistance of ships when the skin friction is computed by the formula of Dr. Kempf is somewhat on the high side, though probably to no greater extent than that using the O_M—O_S system is low.

(5) The formula for thrust given by R. E. Froude in his 1908 I.N.A. paper (see Appendix I.) is well substantiated for full-size screws by the analysis.

(6) The screw efficiencies given by R. E. Froude's 1908 data are fairly comparable with those obtainable in the ship propellers.

(7) The pitch factor of propellers in warships is nearly 1·1 as an average, and not the conventional value of 1·02 as recommended by R. E. Froude. Despite this, however, a factor of 1·02 may be regarded as satisfactory for propeller design for naked E.H.P. (on the O_M—O_S basis), though this needs to be increased to 1·05 if appendages be included.

(8) There is a probability that the naked model wakes (in warships) are serviceably correct for ship, but the augmented resistance percentages deduced from the model should be increased by a small margin in passing to the ship.

(9) The prejudicial effect of shallow water of a moderate depth on the performance of a warship appears to be exaggerated by published model data.

(10) While the accuracy of the torsion meter and revolutions records on trials is serviceably acceptable, it is of some importance for the purpose of analysis that greater consistency be obtained. A really dependable means of recording the thrust on trial would be a great asset in resolving our most important doubts.

Finally, I would venture the opinion that it will be by the collation and dissemination of ship results in comparison with the relevant model data that a solution of our common problem will be achieved. It is in that spirit and not with any sense of finality or completeness that this paper is offered.

I have to express my thanks to the Director of Naval Construction (Sir A. W. Johns, K.C.B., C.B.E.) for permission to publish the information contained in this paper, and to Mr. R. W. L. Gawn, R.C.N.C., of the Haslar Staff, for assistance in its preparation.

APPENDIX I.

Method of Analysis.

For each mean of means of the speeds of trial, the following data are obtained:—

(1) E.H.P. derived from naked model resistance corrected by R. E. Froude's O_M and O_S constants.

(2) T.H.P. derived from model resistance and hull efficiency, namely, E.H.P. divided by hull efficiency.

(3) T.H.P. derived from revolutions per minute on trial. This is calculated from R. E. Froude's screw data, from the known propeller dimensions, using the 1·02 pitch factor, revolutions per minute, and speed of ship corrected for wake, the latter determined by model experiments.

(4) D.H.P., i.e. calculated S.H.P. at propeller. It is equal to (3) divided by screw efficiency, as given by R. E. Froude's screw data.

(5) S.H.P. as measured on trial.

All the above powers are converted into constant form by dividing by the product of (displacement)$^{\frac{2}{3}}$ × (speed)^3, i.e. ($\Delta^{\frac{2}{3}} . V^3$) and multiplying by 1,000. The constant method not only affords a ready comparison between various ships, but also between trials at different draughts of the same ship. Moreover, it automatically allows for the small differences of displacement that necessarily occur during a series of progressive trials. Thus

$$\text{E. H. P. constant} = \text{Ⓔ} = \frac{\text{E.H.P.} \times 1{,}000}{\Delta^{\frac{2}{3}} \,.\, V^3}$$

$$\text{T.H.P. constant} = \text{Ⓣ}_m = \frac{\text{Ⓔ}}{\text{Hull efficiency}}$$

$$\text{T.H.P. constant} = \text{Ⓣ}_r = \frac{\text{T.H.P.}_{\cdot r} \times 1{,}000}{\Delta^{\frac{2}{3}} \,.\, V^3}$$

$$\left(\text{where T.H.P.}_{\cdot r} = \frac{B\,(p + 21)}{p} \,.\, D^2 \,.\, y \,.\, V_1{}^3\right)$$

$$\text{D.H.P. constant} = \frac{\text{Ⓣ}_r}{\text{Screw efficiency}}$$

$$\text{S.H.P. constant} = \frac{\text{S.H.P.} \times 1{,}000}{\Delta^{\frac{2}{3}} \,.\, V^3}$$

The five H.P. constants are plotted on a speed base, together with the following data:—

(1) Slip function $x = \dfrac{R \,.\, p \,.\, D}{V_1}$ in the usual notation for each shaft.

(2) Ratio $\dfrac{\text{S.H.P.}}{\text{D.H.P.}}$ for each shaft.

(3) Propulsive coefficient.

(4) Pressure coefficient of the screw propellers.

Apart from the utility of the diagrams from a design point of view and in their bearing on prediction of performance, they are instructive in indicating any abnormalities obtaining on trial such as cavitation, shallow-water effects, and errors in the trial records.

Thus any abnormal departure in (1) and (2) from the general mean value at any speed is a fairly certain indication of error in the trial records at the speed in question. Further evidence as to the particular record or records in error and the amount of the same can be obtained from the comparison of the H.P. constants. In any case the importance of which is considered sufficient to justify the labour involved, a more detailed check on the records is obtained by analysing the individual runs at a given mean of means speed. This involves the tentative assumption of a tidal law appropriate to the conditions, but invariably affords conclusive evidence regarding the accuracy of the data. In effect, the propeller revolutions are taken as a speed log.

As an indication of the accuracy of trial data, the following brief summary may be of interest:—

(*a*) *Speed.*—If a sufficient length of approach is possible on the measured mile course to ensure steady conditions, then on a clear day in fine weather the speed should be correctly timed to within $\frac{1}{5}$th of a second with a calibrated stop-watch. The possible error on this account is small and varies from about $\frac{1}{5}$th to $\frac{1}{10}$th of 1 per cent., according to the speed. On cloudy or misty days this accuracy is not possible, and larger errors may occur in the individual runs and the mean of means speeds. Inaccuracies in the mean of means speeds can also be attributed to inexact elimination of tide and wind, if any, by the mean of means method. It is seldom that the diagrams suggest that the speed record is unsatisfactory, though implied errors of as much as one per cent. have been met with in some cases. A small error in the speed leads to a large error in the estimate of computed propeller thrust and power since it affects the slip. If a speed be over-stated, then the values of Ⓣ$_r$, Ⓓ, and Ⓘ at this speed will be low and not consistent with the values of Ⓔ and Ⓣ$_m$. Slip function will be low and S.H.P./D.H.P. unduly large.

(*b*) *Revolutions.*—Steady conditions and fine weather are also important factors in the accuracy of the revolutions record. A wind across the course may detract from the accuracy of the revolutions record due to the necessity of using the rudder. The ratio S.H.P./D.H.P. is a very crucial check on the accuracy of the revolutions record, it should approach constancy or change only very gradually throughout the speed range and be free from irregularities on the different shafts. This ideal condition is rarely obtained, variations such as are shown in Figs. 1, 3, and 4 (Plate XLIV.) being not uncommon. The T.H.P. and D.H.P. are critically affected by a revolution's error. If an electric timer is used, it is difficult to see how errors greater than $\frac{1}{20}$ per cent. can occur, and it might be thought even with counters, an average accuracy of $\frac{1}{2}$ per cent. should be attainable. Actually, implied errors of 2 per cent. or more are not infrequently met with. Such inaccuracies are most serious from the point of view of this analysis since an error of 1 per cent. in revolutions implies a correction of about 5 per cent. or more in $\textcircled{T}_r$ and $\textcircled{D}$. Fortunately for our purpose, such errors seldom occur throughout the whole of the speeds of a progressive trial.

If cavitation is present, the actual thrust of the screw will not be so great as that computed, and the phenomenon will manifest itself by an abnormal increase in $\textcircled{T}_r$ with regard to $\textcircled{T}_m$ at all speeds at which it is present. There will also be a large increase in D.H.P. relative to S.H.P. and a rise in the slip function. The effect of shallow water would be similarly indicated except that it would be likely to be manifested at lower as well as at the highest speeds. Actually the trials under review have been largely made in deep water.

(*c*) *Shaft Horse-power.*—Any error in revolutions per minute will directly affect the S.H.P. record in the same proportion and not to a multiple proportion as it does the D.H.P. In addition, any torsion-meter error will be directly taken over to the S.H.P. record. Speaking broadly, and considering the average results of a number of similar ships, the torsion meter can be regarded as a fairly serviceable instrument. The difficulties in the way of accurately measuring the torsion of a shaft are very great, and it is therefore to be expected that the analysis would show that torque would be the least trustworthy of the trial records. Our experience indicates that errors of 5 per cent. in the torque on a shaft are to be reckoned with. Indeed, at low speeds when the torque is only a small fraction of that developed at full speed, the torsion meters show variations to be as large as 10 per cent. or more, and in these cases the records are, of course, of no use for the purpose in view in the analysis.

While in the present state of our knowledge there is some uncertainty in deducing the ship performance, it is considered that the general accuracy of the model experiments is of a satisfactorily high order, particularly as regards resistance. There are various experimental difficulties underlying the measurement of thrust and torque, and it is doubtful if the accuracy of these quantities can be assessed at a closer figure than 1 per cent. Probably the accuracy of the hull efficiency elements is of the same order.

Typical diagrams of the quantities computed in the analysis are shown in Figs. 1 to 4 (Plate XLIV.).

Fig. 1 relates to a capital ship. The various curves are fairly regular and compare well among themselves. This diagram is broadly representative of the trial results of the type of ship in question.

Fig. 2 relates to H.M.S. *Mackay*, a flotilla leader, and is of interest because it includes the T.H.P. constant deduced from the measured thrust as well as the usual constants. The excess of the measured thrust over $\textcircled{T}_r$ and $\textcircled{T}_m$ is greater than discussed in § 13, because the appendages are not included in the computation of $\textcircled{T}_r$ and $\textcircled{T}_m$. A prominent feature of the diagram is the large excess of the S.H.P. constant, i.e. $\textcircled{I}$ at the lowest speed. The other H.P. constants are in well-graded sequence at this speed, and the curve of slip function (i.e. x) is regular. The evidence of the curve is therefore strongly suggestive of a torque error. The $\frac{\text{S.H.P.}}{\text{D.H.P.}}$ curves show that the over-rating exists on both shafts, but is more marked on the port than the starboard shaft. Large torque errors are frequently met with at such low speeds due, probably, to the small twist of the shaft to be measured at such speeds, but the apparent magnitude of the excess in Fig. 2 is abnormal.

Fig. 3 is a diagram typical of the trial results of a destroyer. The progressive excess of $\textcircled{T}_r$ over $\textcircled{T}_m$ at high speeds, coupled with the concomitant diminution in the ratio of S.H.P. to D.H.P., is indicative of cavitation.

Fig. 4 is a representative diagram for a cruiser and has been chosen because it gives an example of an error in the revolutions record. The discontinuity in the curves of $\textcircled{T}_r$, $\textcircled{D}$, and

$\frac{\text{S.H.P.}}{\text{D.H.P.}}$, and to a less extent x and (I), is suggestive primarily of an over-statement of revolutions per minute at the middle speed of trial.

APPENDIX II.

Variation of Wake and Augmented Resistance with Skin Friction.

If it were conceivable for a model to be produced whose surface was so smooth that the skin friction resistance exactly corresponded with that of the ship, we could assume with some confidence that the hull efficiency elements for such a smooth model would reasonably apply to the ship. Such a refined degree of smoothness cannot be obtained, but some idea of the possible effect of a reduction in skin friction resistance can be inferred on reasonable assumptions if we have results for a model whose skin friction has been artificially increased. In 1882, R. E. Froude made some interesting experiments with this object in view.

A model of the liner *City of Rome* was tried with screw propellers, both single and twin, in various positions. The model was tried with three conditions of surface, namely:—

(*a*) Paraffin wax as usual.
(*b*) As (*a*), but covered with calico.
(*c*) As (*b*), but the calico covered the after-half of the model only.

The differences in wake between (*a*), (*b*), and (*c*) were found to be proportional to the differences in resistance. It is not unreasonable to assume that the differences in resistance between the three surfaces are due to the differences in skin friction resistance alone, and it is then a logical step to assume that the wake percentage of the ship can be deduced from the results by assuming the same rate of change with skin friction resistance as was found in the experiments. With these assumptions it was deduced that the wake percentage for the ship was some 5 per cent. to 6 per cent. less than for the model, the deduction being a little greater for single than for twin screws.

It may be of interest to mention that the experiments with screws in various positions indicate that it is the forward speed of the wake rather than its width that is affected by the change in skin friction resistance of the model surface.

This deduced correction for wake in passing to the ship has been confirmed by similar experiments made in recent years on three models of very different types with surfaces roughened by sawdust.

In all these experiments, the augmented resistance per cent. was only slightly affected by the roughened surface, and, so far as the results go in this respect, they can be held as showing that the modification to the augmented resistance per cent. in passing from the model to the ship is small on the score of skin friction correction alone.

DISCUSSION.

Mr. G. Hughes, B.Sc., Ph.D. (Associate-Member): This interesting paper gives an account of a method of model experiments which has not been described before—what is commonly called the Froude, or " behind," method. It has been used at the William Froude Laboratory but in rather a modified form. That is, in the experiments we used to do there we actually reproduced the ship's screw in the model test, and we used to take all our model results and apply them to the ship. But that has now been superseded in favour of the self-propelled model test, and this latter method is, I think, the one generally employed in Tank work, and I think this will be endorsed by the various Tank representatives who are present at this Conference.

There are many advantages attaching to the self-propelled method, the chief of which is probably the elimination of the correction for the resistance of the frame carrying the propellers behind the model hull. It is extremely difficult to measure this resistance with the accuracy demanded in modern work for predicting ship results.

There is no direct comparison between ship and model in the Haslar method, such as we have in the self-propelled test, i.e. thrust for thrust, power for power, and so on, but I should like to make a comparison with our results in this way. Our method of predicting for a ship at the William Froude Laboratory is to deduce the shaft horse-power for the ship by taking the estimated effective horse-power from the model resistance (using Froude's skin friction correction), dividing by the propulsive coefficient as found in the model tests, and adding an appropriate percentage for appendages, air and weather resistance, such as previous ship and model comparisons have shown to be necessary. At this power the revolutions are read off from the model power-revolutions curve. Such a power-revolutions correlation assumes no scale effect between model and ship propellers and the same wake conditions also, and the correlation is independent of weather considerations. We find, when we do this, that our estimates of power are very close, but that the revolutions are generally a few per cent. greater on the ship than were predicted. Therefore, between model and ship there is either or both of, firstly, scale effect on the propellers, and, secondly, scale effect on the wake conditions.

To compare with Mr. Payne's data, the ship revolutions must be applied to the model curves. At the same revolutions we find the ratio of shaft horse-power at the propeller for ship to model to lie between 0·85 and 1·0, generally nearer the former figure. The comparisons we have of thrust are not so many, but these show much the same ratio, between 0·90 and 1·0, again generally nearer the former figure. These figures show that there is not much difference in efficiency between model and ship propeller; if any, the ship propeller is slightly more efficient than the model at the same revolutions and speed. This statement assumes the same wake for ship and model, and must be accepted with this reservation.

The corresponding ratio in Mr. Payne's paper is S.H.P. to what he calls the D.H.P. as calculated from the model, less, say, 5 per cent. for transmission loss in the ship, because, I think, according to the paper, the S.H.P. is not actually at the propeller.

Mr. M. P. Payne: That is so.

Mr. Hughes: I think 5 per cent. is a reasonable figure to take for transmission loss. If we leave out the abnormal values shown in Fig. 2, the various curves in Figs. 1 to 4 (Plate XLIV.) show a range of from 1·1 to 1·6 for this ratio, or, taking off 5 per cent., from 1·05 to 1·5. In this ratio the S.H.P. is measured on the ship and the D.H.P. calculated from Froude's screw data applied to the measured ship speed and revolutions and assuming the model wake. It is true that the type of vessel dealt with by Mr. Payne differs very much from those usually tested at the William Froude Laboratory, but a reasonably steady value of the ratio $\frac{\text{S.H.P.}}{\text{D.H.P.}}$ (not necessarily unity) is to be expected if Froude's data can be applied to ship analysis in the manner shown in the paper.

Apart from the range of value of this ratio, the actual values suggest that the ship propellers are doing far more work for the same revolutions than the model propellers, whereas our results show that the ship propellers do not do quite so much. In the one case, in Fig. 2, where Mr. Payne shows ship thrust measurements, the ratio of ship to calculated thrust is much the same as the power ratio—something like 1·5—which suggests that the Froude calculation is very much under-estimating the work done by the ship propellers, in both thrust and torque.

Our evidence, as above stated, is that when the model is made to reproduce the ship, as nearly as possible, in both hull and propeller, as in self-propelled work, the ship propeller creates rather less thrust and torque for the same revolutions, and there seems no reason to suppose that it should be otherwise in naval vessels. The conclusion, I suggest, is that there is too little direct comparison between model and ship in the Haslar method, which necessitates the employment of artifices, such as the analysis pitch ratio, in applying Froude's screw data. Also, we have found, particularly with high-speed work, that the estimation of the resistance of the frame carrying the propellers behind the model may lead to error.

The CHAIRMAN: Do you shroud your shaft and so get rid of that?

Mr. HUGHES: No; you cannot altogether. The resistance, even with the most stream-lined form, is quite appreciable. The velocity of flow past the frames is quite high, and the correction to be applied makes a very big difference to the thrust to be imputed to the propeller.

I should like to ask Mr. Payne whether he has any explanation for the wide range of values of $\frac{\text{S.H.P.}}{\text{D.H.P.}}$ in his curves, and for the high general level of these values. For instance, it would be interesting to know how the thicknesses and camber ratios of the propellers used in the vessels referred to compare with those of Froude's screws. Mr. Payne made a particular reference to Appendix II. and I should like to comment on that. It is quite possible that the conclusions drawn from the method suggested in Appendix II. to find the variation of wake with resistance may be right. But it seems to me that the method is quite indefensible. Mr. Payne suggests taking different roughnesses of the surface of the model and finding the variation of wake with resistance; thence deducing the ship wake from the model wake, model resistance, and estimated ship resistance. But in the one case we have constant length of body and different roughnesses of surface, and, in the other, varying length of body with the same surface. It does not follow that the variation of wake with resistance is the same in the two cases, and I do not think the conclusions are justified.

Dr. G. KEMPF (Member): As Mr. Payne has referred to my work, I may be allowed to make some remarks on his paper. We cannot be grateful enough for the enormous amount of experience which he has given us with the permission of Sir Arthur Johns.

I am especially concerned with such calculations, because I had the honour some years ago to put before your Institution a paper on the skin friction measurements which we made. I am very well satisfied with the rather good agreement between the results given in the paper and the formula which I gave. It is not so astonishing that the skin friction, which I measured on the *Hamburg* and *Bremen*, is a little in excess of the warship's skin friction because the friction of a merchant vessel is certainly greater than the friction of the skin of a clean warship, and even for merchant ships we have found in our comparisons that we must take a little smaller value than was given originally by myself. We now take, normally, some 2 per cent. less, and I think that this will agree much better with Mr. Payne's figures.

There still remains some uncertainty about form effect. As to how we intend to use it in practice some description was given yesterday by Professor Horn in the discussion. He gave the method which Mr. Schlichting suggested in a paper which he gave before the German Institution of Shipbuilders in 1932. Regarding that method we intend to make tests with a family of five models of different sizes of certain forms. Such a series are in hand now at the Hamburg Tank and we hope to forward the results in due time.

Regarding the data of the real ships, we have the same method for measuring the speed as the American Navy and American merchant ships now use—treating the whole ship as a pitot tube. We make a hole in the stern and two holes on each side of the bow, and we assume that on the bow the real potential flow is there on the model and on the ship. And so we may get an entirely fair comparison between model and ship. We calibrate the pitot tube on the model and take the same percentage for the pitot tube on the ship. The result is rather good. We had such a device on the first runs of the *Bremen* and *Europa*. The *Europa* has now been towing such a log constantly the whole way from Europe to America and back. That log gives from moment to moment the speed, recording it on a drum on the bridge. It is a record long enough to go round this hall, perhaps. I may mention what kind of results we have received. When in shallow water the captain is very glad to have such a record of speed, especially when foggy weather occurs, because he then knows from the lowering of the speed that the ship is passing through shallow water. The speed may have run down two-tenths of a knot or more, and one can find out the ship's position on the map; also, after she is given a little more rudder, one can see the speed go down, say, one- or two-tenths of a knot. Another thing is interesting: they also carry another log in the middle of the ship. But if she comes into shallow water this log gives a higher speed because the water is accelerating round the ship at the bottom. On the other hand, in rough weather, the speed may suddenly drop when she meets a series of high waves and only very slowly will she reach her former speed again. So we can get a full record for the whole voyage, and it is only with such a continuous record of speed that one can be quite sure that all the measurements will fit together.

For measuring power we used as a torsion meter a very simple device—the "Maihak-Schaefer" wire arrangement; we found it accurate to within 1 per cent., and we checked it by measuring, for example, the frictional loss on the shaft bearings. There were seventeen bearings on the ship. We had three torsion meters on the shaft, and the measured reduction of torsion was 5 to 6 per cent.—measured many times.

Those measurements are necessary to get a real basis for comparison, because, as Mr. Payne said, "a small error in the speed leads to a large error in the estimate of computed propeller thrust."

Another point must be borne in mind if trials are run with and against the tide. Captains often tell us that they think the revolutions are different when the ship is running against the tide or with the tide at the same speed. By calculating the inclination of the surface of the water one comes to a range of 1 to 2 per cent. for the difference of thrust when there is a very high tide. So it is quite reasonable that there may be differences in revolutions when going up and down the tide.

Regarding the analysis of model propellers I suppose this has been done by Mr. Payne only because there were no other data available. I think Mr. Payne will agree that in future it would be more exact if we tested the real model screw.

Mr. M. P. Payne: Yes.

Dr. G. Kempf: It is a pity that one cannot study the paper fully enough before having to discuss it, but we must be very grateful to Mr. Payne for having published his valuable experience, which will help us greatly to come nearer the truth.

Mr. E. Wilding, C.B.E. (Member): As an old Haslar man I would like to thank Mr. Payne for the paper, which in many ways recalls the traditional Haslar style; because since R. E. Froude retired it is a type which has been rather absent from our Transactions and it is with some relief that one sees that the style and manner are not forgotten. It is also interesting to learn that the processes with which one was familiar thirty years ago had proved so satisfactory that they have been developed to meet the more recent needs.

I have only one regret and comment to make on the paper as a whole, namely, that to me—as I think to most of us—it is very overcrowded, and that many seemingly small points might better have been eliminated for the present and dealt with more fully another time. In such work as this, of course, no point is really small. It may be a counsel of perfection, and an older generation is entitled to its grumble.

Proceeding to deal with the paper in some detail, I have attempted for a considerable number of cases, although not so large a number as mentioned by Mr. Payne, to follow out the Froude or Haslar type of analysis for ships of a somewhat fuller prismatic coefficient than those indicated by Mr. Payne—that is, for merchant ships of what are called the "intermediate" classes; and whilst one gets, as one always does in trial analyses, various discrepancies to account for, if one can adopt, instead of the Froude pitch ratio of 1·02 times the nominal pitch ratio, a factor of 1·07—which is not very different from Mr. Payne's figure of 1·05—in general one gets very tolerable agreement.

The next point I should like to make really concerns the opening paragraph. Ship performance includes service results. Few records, so far as I know, have been published for merchant ships over long periods of service. The question hardly arises in the author's province, he would say, for he has run very few merchant ship models; yet there is a gap between the Tank results and the *average* of a ship's performances for which few data are available. Marine engineer superintendents say that their recorded data will not comply with the scientific accuracy of Tank work—perhaps not—and they are reluctant to publish the data and compare them, even when they have the model data to compare with.

With the permission of the Institution I would like to add as an appendix to Mr. Payne's paper one such record of comparison so that, in part, we can fill in the gap.

Many years ago the late Mr. James Doran, of Philadelphia, then Superintendent of the American Line—he had been one of Farragut's engineers—began to keep full and, for the date, unusually careful records of each voyage of some ships; and by his courtesy two full years' records for the *New York* and about one and a half years' records for the *Paris*, and also one full year's records for the *Paris* altered to *Philadelphia* (by bossing at the stern instead of A frames), were placed at my disposal for analysis.

No model results were obtainable, but the trial trip results of the *City of Paris* in 1889 were got, and additional records for fast voyages of the *City of Paris* were found, although they were not equal in quality to those from *Philadelphia.*

The I.H.P. record for the voyage was unusual, I believe. It was obtained from a set of cards taken every day between 5 p.m. and 6 p.m.—that is, after the fires had been cleaned in the first dog watch—and these I.H.P. figures reduced for the day in accordance with (revs.)^3. Those records were not perfectly accurate, but they were probably the best that could be done in service. The daily average was meaned to get the I.H.P. average for each voyage. A corrected mean draft for the voyage was made from the full data recorded by the log, of the consumption of water ballast and coal from the average of arrival and departure. That, I remember, was a rather tiresome job. Then a piece of luck came my way.

I was at the time under Mr. R. E. Froude at Haslar, and as a preliminary to the *Lusitania* and *Mauretania* design, the then D.N.C., Sir Philip Watts, sent the lines of the *Campania* and *Paris* to Froude to run, and from the data for once, in a general way, the gap which I have referred to could be bridged. The figure for propulsive coefficient on trial came out between 0·49 and 0·50 compared with the naked model taken in the ordinary way and with reciprocating engines. I think you will agree (those of you who remember the coefficients of that time) that it was not a bad result for that date. By making the assumption that the ratio of E.H.P. to I.H.P. would remain substantially constant in service, the average I.H.P. under trial conditions for each voyage could be got from the E.H.P. for the recorded speeds and drafts. The service I.H.P., as I have already mentioned, was as correct as we could hope to get it.

Comparing the I.H.P. per voyage with derived "trial" I.H.P. at the same average speed and draft, a resistance factor could be arrived at which would represent the effect of fouling, of the weather, e.g. fog, and the wind and waves as compared with measured mile conditions. In fact, service conditions compared with trial conditions. This factor ranged from 1·04 to 1·76. The lower value is in accordance with other published results for good voyages, chiefly German, but I do not know of any published results of every voyage throughout a long period or of long-period averages.

Such would seem to be a not unsuitable pendant to a paper on ship performance in relation to Tank results, because the model results were obtained at Haslar, although long before Mr. Payne went there. I should perhaps add that the results check very reasonably with the apparent slip—as, of course, they should. I give the following summarized averages of *the resistance factor*:—

City of Paris: Trials at Skelmorlie (March 21, 1889).

Mean of pairs of runs. Topsail schooner rig, yards on foremast. Dimensions, 523 ft. by 63·4 ft.

Speed, knots	15·13	18·70	21·20	21·73
I.H.P. total	5,750	11,440	18,100	20,100
Propulsive coefficient ..	0·500	0·500	0·491	0·498

On these results, with the yards removed, a propulsive coefficient of 0·50 was assumed for the voyages in 1892–95–96, the resistance factor including the effect of all other changes from trial trip conditions.

Voyages all between Southampton (Needles, I.O.W.) and New York (Sandy Hook, L.V.) and *vice versa.*

City of Paris: Fastest Voyages.

		Knots.	Resistance Factor.
Going West ..	July 20–27, 1892	20·48	1·175
Going East ..	September 28 to October 5, 1892	19·45	1·12

Resistance factor being defined as average I.H.P. for voyage divided by I.H.P. for same draught and speed under trial trip conditions as deduced from model experiments.

City of Paris: Yearly Averages, all Voyages.

			Runs.	Average Resistance Factor.
1895	Westbound	January 5 to December 21	14	1·307
1896	Westbound	January 4 to December 20	12	1·274
1897	Westbound	January 2 to April 3	3	1·595
1895	Eastbound	January 16 to December 31	14	1·192
1896	Eastbound .. .. (3 good voyages only)	January 15 to December 30	3	1·138
1897	Eastbound	January 13 to May 5	3	1·386

City of New York: Yearly Averages, all Voyages.

A sister ship. Trial records not complete, but as far as they go not very different. Reduced with the same factor from the same model results.

			Runs.	Average Resistance Factor.
1895	Westbound	January 12 to November 30	14	1·336
1896	Westbound	February 1 to December 26	15	1·399
1895	Eastbound	February 13 to November 20	13	1·294
1896	Eastbound	January 22 to January 6, 1897	16	1·273

In 1901 the after-end of the *City of Paris* was reconstructed with shaft bossing webs in lieu of the A frames (which had caused much trouble), new propellers were fitted, and the balance and running of the engines was much improved. No corresponding model trials were made, and the available trial trip records were not good. But as far as they went they indicated that the propulsive coefficient had been raised from 0·50 to 0·54 from the naked model basis, and the service results generally bear this out when reduced in the same way, using the 8 per cent. better propulsive coefficient. The name was changed to *Philadelphia.*

Philadelphia: Year 1902, Averages for all Voyages.

		Runs.	Average Resistance Factor.
Westbound	January 11 to December 27	17	1·297
Eastbound	January 22 to January 7, 1903	17	1·211

Apart from the improved basis performance, the much greater regularity of running is to be noted, i.e. freedom from interruptions.

I know the results are old, but Mr. Payne's paper encourages me to tender them by his reference in Appendix II. to the model of the *City of Rome*. I believe that the earliest model of the *City of Rome* was run by William Froude, and the later model, referred to in Appendix II., with the various surfaces, was run by R. E. Froude after his father's death?

Mr. M. P. Payne: Yes.

Mr. E. Wilding: I have one other contribution to make to the discussion, which calls attention to the need—I have already done it on previous occasions—of great care in applying model results to ship conditions.

Mr. Payne has referred on page 404 to the results published for shallow water data. I do not know whether he made any experiments, but there are, of course, quite a large amount of published data. I had occasion about five years ago to examine the results of certain ships that were being run in very shallow water—shallow in the technical sense, that is, shallow both relatively to the size of the ship and also relatively to the speed. There were some rather startling results.

The model was sent to be run at the National Physical Laboratory in both deep and shallow water. The first startling result I got was a propulsive coefficient between 1·20 and 1·40 on service. Well, you cannot expect your ship to do that. The law of conservation of energy holds, and coal bills are paid for in hard cash. There was obviously something wrong. Puzzled, I sent the facts to the Superintendent at Teddington and suggested that he had omitted the Q or an equivalent that is so familiar to all R.N.C. students in displacement sheets. I received a formal reply that the model results as sent were quite in order. But facts are stubborn things and Froude's law of comparison holds. So I suggested to Mr. Baker that the flow had become largely two-dimensional, and there was an effect due to the sides of the Tank. Mr. Baker very properly took me up at the first convenient opportunity and satisfied himself (and me) that that at any rate was not the cause. The ship was abroad and I could not pursue that particular case much farther.

There were then building four more ships on the Clyde, to run under not very dissimilar conditions—that is, in water which was technically in both senses "shallow." The usual model experiments were run in the Teddington Tank, in both deep and shallow water, and in due course we compared them with the trial results. Sir Archibald Denny will remember that I gave a short paper eighteen months ago on one of the points that arose in reduction of the trial results, i.e. a method of deriving the H.P. delivered to the screws from the machinery I.H.P. for which I was satisfied we were getting fairly good results, for they were of a good ordinary commercial standard—probably within 3 or 4 per cent. There was nothing abnormal in the shape of the ships. The results we got at Skelmorlie and in the Gareloch were satisfactory, the propulsive coefficients on the basis of $\frac{\text{D.H.P.}}{\text{E.H.P.}}$ being uniform right through the speed range and for the different ships, having eliminated the engine friction, and of the order of 0·65 to 0·67.

But when we came to tackle the shallow water results we again came up against the shallow water Tank data. There is no doubt that the last runs in shallow water, which were made between Helensburgh and Cardross, gave us fairly good data. We had run our model and the data agreed substantially for deep water, and at all moderate speeds we could run without any increased resistance due to the shallow water, as Mr. Payne suggests. The increase when we got nearer to the critical speeds was nothing like what the model data presented. We were still getting propulsive coefficients of well over 100 per cent. after elimination of engine friction. But we were getting steadily 65 to 67 per cent. in deep water. There was clearly something wrong with the model data. I have come to the conclusion that the experimental published data so far as they are accessible in this country for *all* shallow water work at speeds of about 70 per cent. of the critical speed, i.e. of the wave of translation, are frankly not worth the paper they are written on, and we have yet to learn how that rewriting has got to be done. The error is not a 10, 15, or 20 per cent. factor, but the model results are over 100 per cent. more than they should be.

I am not challenging the model data as observed. I have no doubt the data are correct observations of what was done. I am challenging the boundary conditions—to use the mathematical phrase—and assuredly the boundary conditions are not right. There is something going on under the surface which we do not fully know about and which no past experiments in Tanks have taken into account.

Meanwhile, I suggest to Tank Superintendents that any data from experience in shallow water (in the technical sense) require to be treated with the very greatest care.

Sir EUSTACE H. T. D'EYNCOURT, Bart., K.C.B., D.Sc., F.R.S. (Vice-President): When I had the honour of being at the Admiralty, Tank experiments were made for a very large number of designs for different classes of warships, and on no occasion, I think, did I ever send up a set of lines to Mr. R. E. Froude, or later to Mr. Payne, and get information from the Haslar Tank which was not substantially accurate, or which ever let us down.

It is an immense tribute to the memory of Mr. William Froude that results of that order could be relied upon.

The great value of the Tank is to give rapid and sufficiently accurate information to the ship designer; the ships were often designed very quickly, but we had the results within a few days and with sufficient accuracy to proceed with the design of the ship as a whole. Of course, one had to use previous knowledge and experience in estimating the propulsive coefficient, but granted sufficient care in assuming a correct propulsive coefficient, the results were always satisfactory.

I think I may say the same thing for the Tank at Teddington, of which Mr. Baker has given results of experiments made for shipowners. The great benefits which have been derived from this information in reducing H.P. and consequent coal consumption have already been referred to.

In his conclusion No. (9) Mr. Payne says: "The prejudicial effect of shallow water of a moderate depth on the performance of a warship appears to be exaggerated by published model data." For the benefit of the uninitiated he might qualify that by some allusion to the anything but prejudicial effect in the case of destroyers running in shallow water. When they were tried in very shallow water on the Maplins, we often used to gain about a knot or even more when the ship was almost touching the bottom, but of course that was due to the wave position. That is a line of inquiry which I think wants pursuing further.

The remarks of Dr. Kempf were very interesting where he referred to ships passing over shallow water. I think the relation of the form of wave to the speed and to the depth has never really been thoroughly elucidated, and our new Tank at Teddingon is specially arranged for this kind of investigation.

All these points, both major and minor, have to be taken into consideration; but the practical user of the Tank results wants to look at the matter from a broad point of view; and while there is still much to elucidate in the whole research investigation, the meetings we have had this year have shown the immense value of Tank research work and the data obtained therefrom.

I will add one small point which I think has not been mentioned hitherto. In trying our very big ships, like the *Renown*, *Repulse,* and *Hood*, when we ran their trials on the new measured two-mile course off Arran, where we have 80 fathoms of water under the ship, we had posts there not only at each end of the two miles but also in the middle. I think in every case, when we took the speed over the first mile we got a slight increase during the second mile, showing that although the ships made a very long turn they were still accelerating to some extent; not very much, perhaps, but possibly an eighth of a knot or something of that order; the acceleration was going on during the second mile.

Mr. E. Wilding, C.B.E. (Member): May I say that in running on the Arran Mile my experience has been exactly the same with merchant ships. Although not so fast as warships, they do accelerate in the second mile.

The Chairman (Sir Archibald Denny, Bart., LL.D. (Honorary Vice-President): This paper is of great value as showing the continuity of practice between Dr. William Froude and Mr. Payne, illustrated in the system adopted in the Haslar Tank. I can compare that with our experience at Dumbarton.

My brother, William, had come to the conclusion that the Admiralty coefficient or "constant" was not constant, and he introduced the system of progressive trials. The results so far obtained were used by Dr. Froude in his papers read in 1876 and were held to advance very considerably the work that he had been doing previously.

When we started work in our Tank in 1883 we had a mass of progressive trials of ships which we had built. We were building others which were being progressively tried and we wished in the first instance to get coefficients for what we call "propulsive efficiency." So the first few years were spent in that work. But we also started a broad scheme of research which, while useful so far as it went, has never been completed because we never had time; current work was always pressing. But what we did find was that we were getting increased experience; getting more coefficients.

I was the estimator at that time and we used Kirk's analysis—a method we found very successful for our estimates—and had all the data on sheets for ships of 100 ft. It was then natural for us to use the same tabulation in the Tank.

We got more and more confident as we had results worked out, and in 1888 we used Tank investigation exclusively for the *Princesse Henriette* and *Josephine* with great success. This gave us such confidence that very shortly we depended upon the Tank, and have done so ever since.

We used Froude's method for passing from model to ship. Then we began using screw propellers, testing them along with the ship, and used the original method of Froude—cords and pulleys. But, of course, as the revolutions went up it was a difficult thing to do; in fact it became impossible. So we put in positive gearing, and our propeller truck has gone through three or four phases. We have now got our propeller truck, if not perfect, very nearly so.

We have a very simple method of ascertaining the torque with the driving electric motor. We get the driving torque from the armature which is resisted by an equal and opposite torque from the magnets. We support the motor vertically and drive straight through the gear, and we simply measure the torque on the casing which contains the magnets working against a spring. It is very simple and gives no trouble—there is no correction to make.

When Dr. Hughes was raising some objection to the frames which carry the screws on the propeller truck when making trials, I asked him if he had not shrouded them. He was not in favour of that idea. But we have a system of shrouding, and it does away with all the necessity for idle trials.

We use the separate screw truck, as Froude did, behind the model. We have also used for single-screw vessels internal propulsion for the model, but the bulk of our vessels are twin screws or multiple screws; so we have generally used the old system of Froude's. What has been the effect? Well, I do not think you have heard of our making great failures in our contracts. We have not. We have obtained good results by the methods we have used, and these we have improved as we went along. But we obtain good results from the model to the ship. What we predict by the model comes out in the ship. What more do you want?

We have never had a failure. We have always obtained our speed. I can only think of one ship with which we had a result which we could not explain. We made models of that vessel not once but twice. We repeated the trials, and we still have not been able to explain why the recorded results were not as good as we thought they should be; but the ship was satisfactory to the owners.

The advantage of these discussions is that they may help us to explain these little discrepancies.

Let me give you one example. We built four identical vessels—machinery, screws, and everything else. We tested them in the Tank and predicted the trial results, but we got different results in the ships. We knew why. In Scotland we do not always have the very best of weather; sometimes it rains, and in the case where the results were not as good as predicted this was due to difference in the paint surface. In the first and fourth ships the paint was good and hard, and the result predicted was attained; while in the other two ships, which showed slightly less speed, we knew the paint was not so good. That shows the importance of skin friction.

There is another thing we would like very much to have, which Admiral Hiraga mentioned in his paper, and that is an accurate thrust meter. Admiral Hiraga said that his results have always shown that the thrust was greater than was estimated. Not very many such trials have been made, and I have heard this stated before; but were the results correct? Well, our people are developing a thrust meter which they claim will give accurate results. I have seen the apparatus, and as far as I could judge it should be successful; that is to say, all the provisions have been made to get accurate results. It will be fully tested later on, and I hope it will be successful, because that is one of the most useful instruments we could possibly have.

* Mr. G. Hughes, B.Sc., Ph.D. (Associate-Member): Mr. Payne has had a very wide experience in the comparison of model and ship results, and I do not wish in any way to suggest, as subsequent speakers seem to have gathered from my remarks, that Mr. Payne's estimates for the ship from model tests are wanting in accuracy. One feels envious of the trial data which Mr. Payne must have had at his command, and no doubt Mr. Payne can use his power ratios for prediction purposes with every confidence, especially as he has so many vessels of the same type to deal with. It is the intermediate stages in the comparison which are in question, and his power-ratio figures differ so much from those which we have obtained directly and they show such a wide variation in themselves that some explanation seems necessary. Naturally, one does not feel very happy in accepting any conclusions *re* wake, augment of resistance, etc., which are dependent on the analysis.

* Mr. W. R. G. Whiting, M.B.E., M.A. (Member of Council): Attention may be given to the sentence in Section (9) in which the author states that a deduction of 5 per cent. has been made from the S.H.P. thrusts to allow for bearing friction. Although this

* Written contributions.

allowance is not uncommon in such analyses of performance, and although more than one of the speakers in the discussion concurred in this allowance, it seems open to doubt whether so large a margin, made for the purpose of balancing the analysis, should be ascribed to this cause. Vessels of very small power present no difficulty in regard to a high estimate of the frictional losses abaft the thrust block. That one horse-power should be wasted in the stern tube of a 20-H.P. fishing boat may well be. But that 1,500 H.P. should be similarly dissipated in a line of shafting transmitting 30,000 H.P., such as is likely to be found in many of the ships on which Mr. Payne's practice is founded, requires a larger measure of faith.

If any appreciable part of so huge a loss occurred in the bearings located within the ship, the cooling arrangements would reveal the fact. Does the evidence of the quantity of oil or other cooling medium and its rise of temperature suggest that any single plummer block is responsible for a loss which exceeds 5 or 10 S.H.P., if running in first-class condition?

The bearings outside of the hull proper are, of course, in a different category, and the surrounding sea-water may be relied on to keep the journals cool. But it will be found, when the actual arithmetic of the case is put down, that the frictional drag requisite to absorb power on the scale of the 5 per cent. assumption is no less than that demanded by the brakes of motor vehicles! If this were indeed so, the rate of wear-down in a stern tube should be so rapid that the vessel would never be out of the hands of the repairing staffs.

If there is no evidence of an incontrovertible nature on the engineering side to support this assumed loss of 5 per cent. through shafting friction, then I suggest that the analysis of propeller action should balance the discrepancy in some other fashion. That analysis already contains two items for which explicit experimental evidence is not available, viz. pitch factor and relative rotative efficiency, and it is quite possible to assign to either of these unknowns the greater part of the improbable frictional allocation.

Mr. M. P. Payne (Member): Dr. Hughes referred at some length to the advantages of self-propelled models. You, Sir Archibald, expressed your conviction that requirements are met by non-self-propelled models. We have a new Tank at Haslar, but unfortunately its completion as regards equipment has been slow. We have had to think about new means for driving propellers, and we have not omitted to provide for self-propelled models in our new Tank.

Personally, my sympathies are to an extent rather with Dr. Hughes. I have a feeling that there is much to be said in favour of self-propelled models in some cases. So that actually, although he rather assumed the role of a critic, he was really preaching to the already converted. I must thank him for the information he gave us, however.

The V . L basis of regarding the extrapolation of model wake to that for the ship is, I think, the correct one, but so far as is known there is no way of obtaining guidance as to the manner in which this large extrapolation is to be made. It has, however, been arbitrarily assumed that the difference in wake between model and ship varies with the skin friction in a proportionate manner to its variation in the model, as the skin friction of the latter is modified. Regarded from this standpoint the extrapolation is not great, as, broadly, the skin friction of the ship is some 30 to 40 per cent. less than the model, while the skin friction of the model was increased in some cases by over 50 per cent. and in others over 100 per cent.

I am very interested to know that Dr. Kempf's modified figures show closer agreement. I must confess that the form of his formula has always attracted me, and I think that if his modified figures were put into my paper it would suggest that skin friction computed from his formula is very close to the truth. I should like to thank him, as I am sure everybody here will, for his explanation of the results with the log of the *Europa*.

Mr. Wilding's remarks form a very interesting appendage to my paper. I am evidently not alone in regarding the published shallow-water data for ships as being somewhat wide of the mark. We have found it so in a number of cases, though none so glaring as that stated by Mr. Wilding.

Sir Eustace d'Eyncourt, in referring to the question of depth of water under the bottom of a destroyer, actually used the expression "when the ship was almost touching the bottom." I would submit that that does not come within my definition of water of a *moderate* depth. I thank Sir Eustace for his very kind remarks concerning our work at Haslar.

*With regard to Dr. Hughes' comments on the variation in the ratio of S.H.P. to D.H.P., I have been unfortunate in that examples chosen to exhibit certain features happen, in the cases of Figs. 1 and 2 (Plate XLIV.), to display values of the ratio which are unusually high. It must be emphasized that we are considering a function that is very sensitive indeed to minor variations in a large number of factors; the circumstances in which errors may occur in most of these quantities have been dealt with at length in the paper. With the system of analysis described, the mean value of the ratio S.H.P. to D.H.P. is about 1·2 for twin-screw ships at non-cavitating speeds, the actual value varying slightly with the type of ship and the effective pitch ratio of the screw. A comparison of the values of $\textcircled{T}_m$ and $\textcircled{T}_r$ in Fig. 2 with those in Fig. 3 (which apply to a very similar vessel) suggests that generally the values of $\textcircled{T}_r$ in Fig. 2 are low—an inference which is confirmed by reference to the analysis of trial results of other destroyers. It would appear, therefore, that the recorded revolutions or the effective face pitch of the propellers is understated, and a small increase in either of these would result in an appreciable reduction in the S.H.P. to D.H.P. ratio.

Fig. 1 relates to one of the earliest of the quadruple-screw ships with direct-drive turbines; the revolutions and the slip are high and the pitch ratio of the propellers very low. The departure of the pitch factor from the conventional value of 1·02 is greater than in the other ships. The comparison between S.H.P. and D.H.P. was rendered further uncertain by the experimental difficulties of accurately determining the wake with the four-shaft arrangement. In these circumstances the ratio of S.H.P. to D.H.P. has been found to be generally somewhat higher, although the extent in this instance is quite exceptional. The specific reason or reasons for the unusual excess do not appear from the analysis; it seems they are probably those outlined in the case of Fig. 2.

The absolute magnitude of the S.H.P. to D.H.P. ratio is dependent on the pitch factor assumed. The conventional value of 1·02 is low, but I have been reluctant to abandon it altogether on account of the large amount of data obtained by its use, and the fact that the exact value for a ship screw is unknown. Probable values reduce the ratio in question to a figure generally a little below unity, which is fairly in accord with the figures quoted by Dr. Hughes.

I am unable to agree with Dr. Hughes that the lack of a direct comparison between model and ship by the Haslar method in any way reduces its utility.

In reply to Mr. Whiting, the 5 per cent. bearing friction loss is regarded rather as a conventional figure; its absolute value is of secondary importance for the *general* purposes of analysis. I have often felt the need for authentic experimental data in this connection, and am inclined to consider that the loss would be more accurately assessed by taking it as proportional to revolutions (see remarks in Trans. I.N.A., Vol. LXIX., p. 158). I agree with Mr. Whiting in finding it difficult to imagine that 5 per cent. of the S.H.P. is lost at full power.

The CHAIRMAN (Sir Archibald Denny, Bart., LL.D.): Gentlemen, I want you to give Mr. Payne a very hearty vote of thanks.

* Added after the Meetings.

To Illustrate Mr. M. P. Payne's Paper on "Ship Performance in Relation to Tank Results."

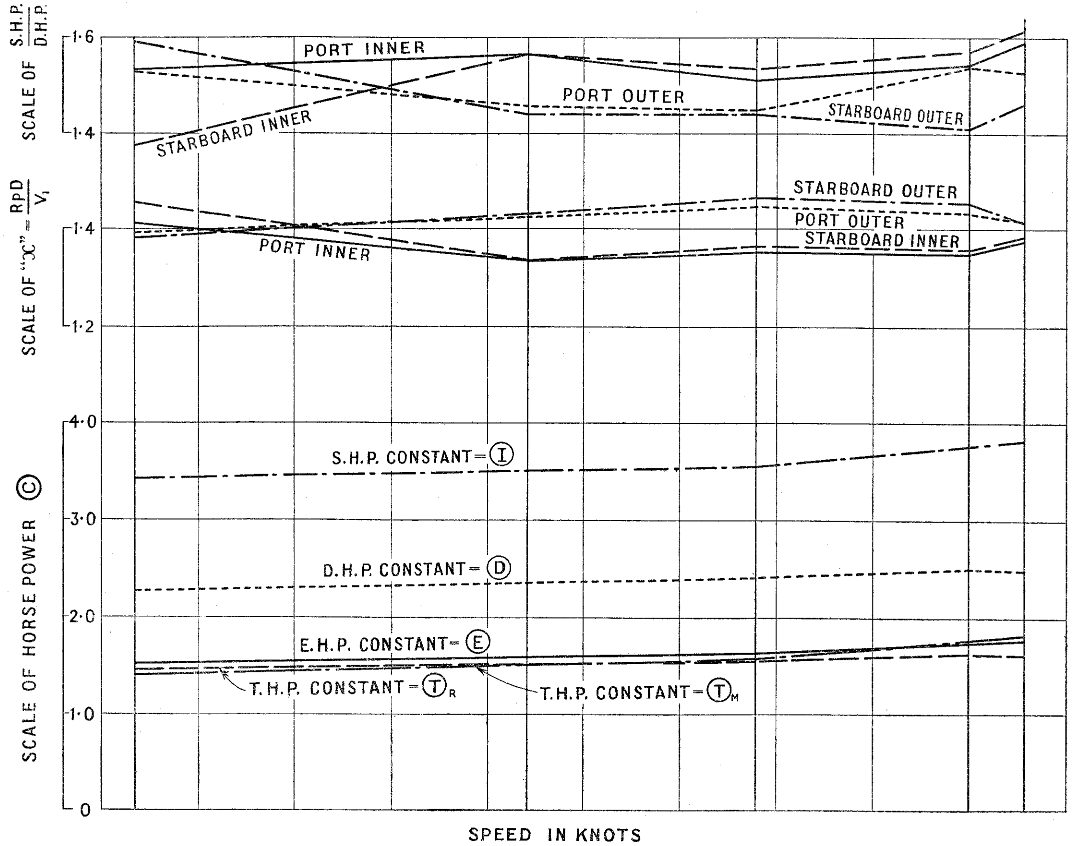


FIG. 1.

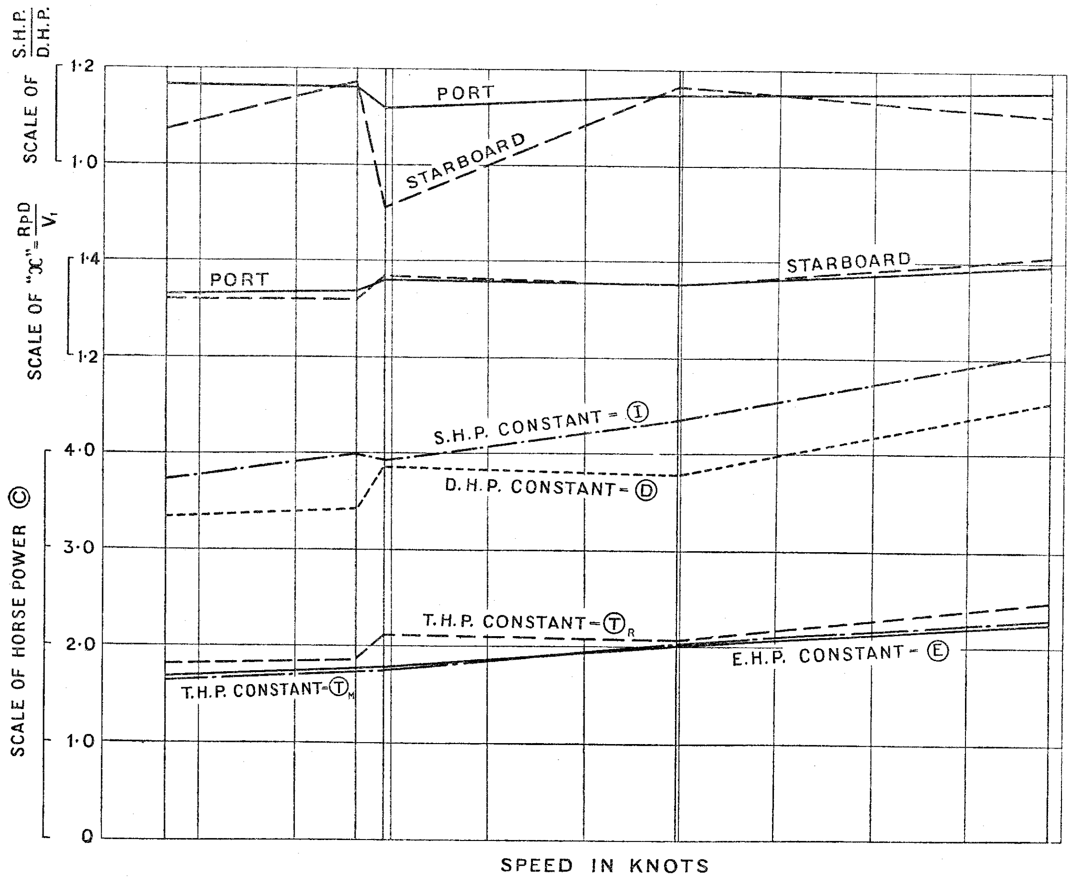


FIG. 4.

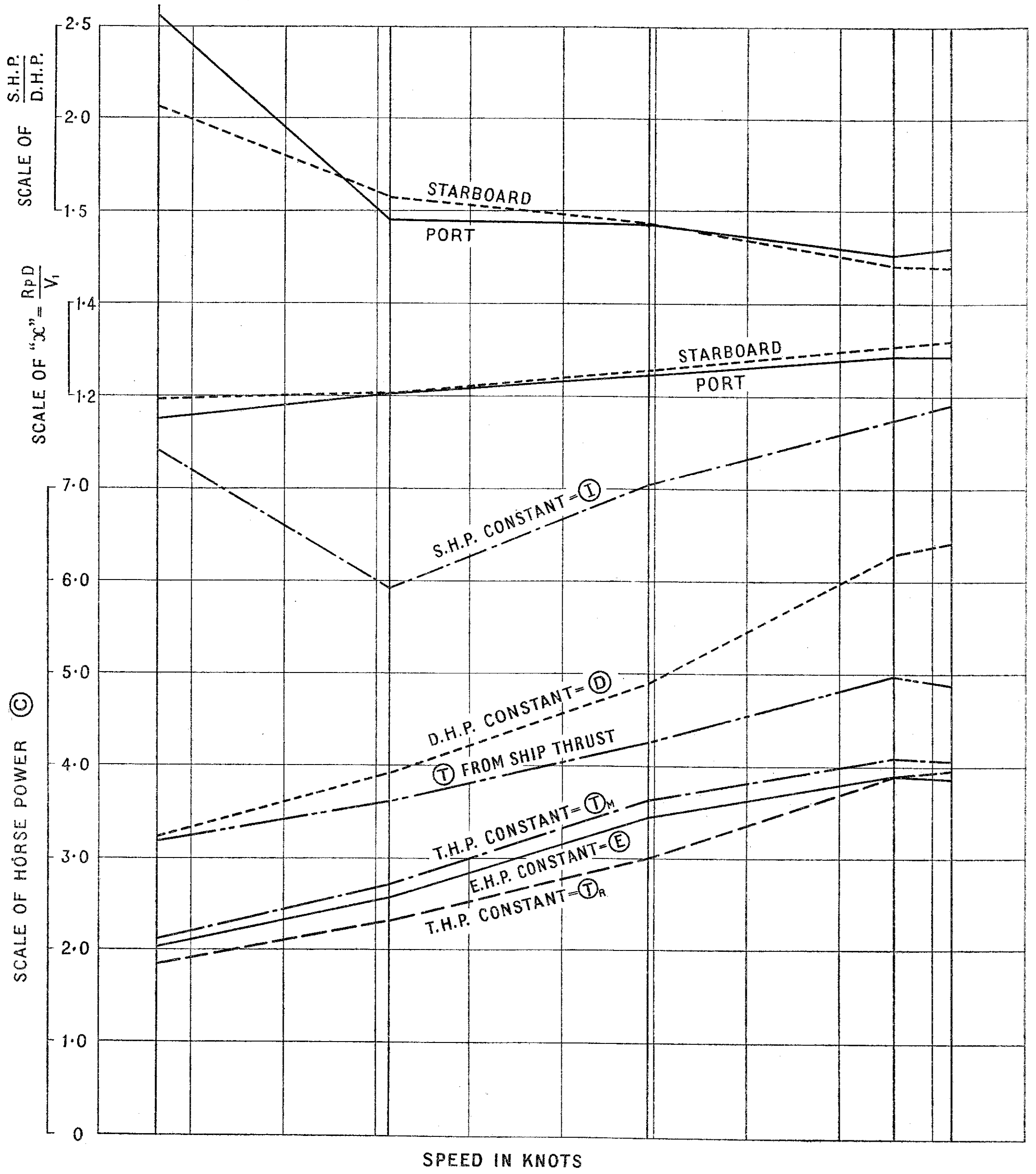


FIG. 2.

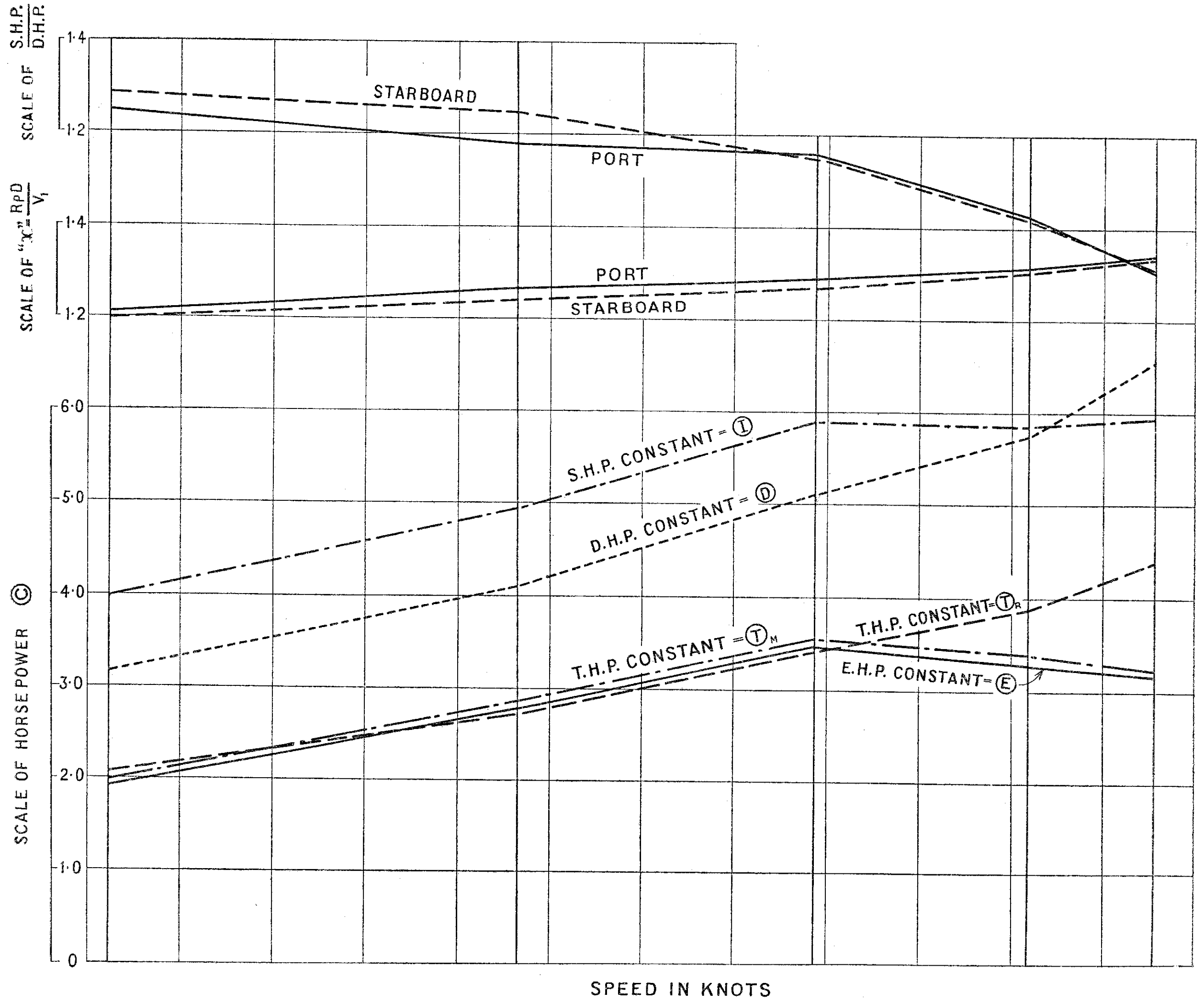


FIG. 3.

ON THE THEORY OF DOUBLE SYSTEMS OF ROLLING OF SHIPS AMONG WAVES.

By Monsieur E. G. BARRILLON, of the Bassin d'Essais, Paris.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 12, 1934, Sir WESTCOTT ABELL, K.B.E., M.Eng., Vice-President, in the Chair.]

I PROPOSE to show that by considering the balance sheet of the energy concerning the rolling motion of a ship and without integrating the general equation of motion we can arrive very simply at conclusions representing sufficiently closely the observed phenomena. A more complete theory cannot be enunciated so long as we do not know more exactly the law governing the couple and the force representing the effect of waves upon the ship.

We will first consider the case of rolling in still water. Consideration of the energy balance sheet gives us the variation of the period in terms of the amplitude.

We will next examine the case of rolling among waves. Here the result of the theory of the energy balance sheet is the prevision of the law of resonance with the explanation of the existence of a double series of rolling.

The notation employed is as follows:—

T_s = period of rolling in still water for an infinitely small amplitude,
y = amplitude of one roll,
T_y = period of one complete roll in still water for amplitude y,
C_s = stability couple in still water,
θ = instantaneous angle of inclination to the vertical,
T_w = period of waves,
$X = \left(\frac{T_w}{T_s}\right)^2$.

ROLLING PERIOD IN STILL WATER IN TERMS OF AMPLITUDE.

(T_y a function of y.)

Let T_s be the period of rolling in still water for very small amplitudes, and $C_s = A\,\theta + B\,\theta^3$, the law of the stability couple.

The coefficient A enables us to determine the inertia I of the whole oscillating mass, ship and water, by the formula

$$I = A\left(\frac{T_s}{2\,\pi}\right)^2$$

For a roll $\theta = y \cos \frac{2\,\pi}{T_y}\,t$, where T_y is the period corresponding to the amplitude y, if we consider two successive quarter periods we shall have:—

Work done by the Inertia Couple.	Work done by the Righting Couple.	Work done by the Damping Forces.
$-\frac{1}{2}\,A\,y^2\left(\frac{T_s}{T_y}\right)^2$	$\frac{A\,y^2}{2}+\frac{B\,y^4}{4}$	$-f\,(y)$
$\frac{1}{2}\,A\,y^2\left(\frac{T_s}{T_y}\right)^2$	$-\frac{A\,y^2}{2}-\frac{B\,y^4}{4}$	$-f\,(y)$

The work done by the inertia couple during the righting period is

$$-\frac{I}{2}\left(\frac{d\theta}{dt}\right)^2_{max.}=-\frac{1}{2}\,A\left(\frac{T_s}{2\,\pi}\right)^2\times\left(\frac{2\,\pi}{T_y}\right)^2 y^2$$

Combining the balance sheet of the righting quarter period and the balance sheet of the next quarter period, we eliminate the unknown factor f, and we obtain

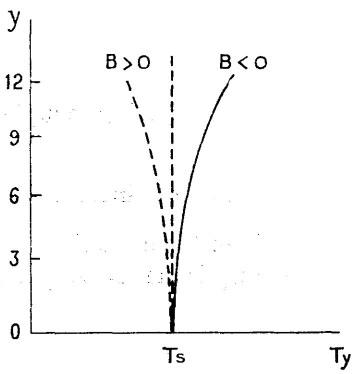
$$A\,y^2\left(\frac{T_s}{T_y}\right)^2=A\,y^2+\frac{B\,y^4}{2}$$

$$T_y=T_s\sqrt{\frac{1}{1+\frac{B}{2\,A}\,y^2}}$$

If $B>0$, that is, if the curve of stability is over its tangent at the origin, $T_y<T_s$, and this will mean that the period will be shorter as the amplitude increases.

If $B<0$, that is, if the curve of stability is below its tangent at the origin, $T_y>T_s$, and the period will be longer as the amplitude increases.

Example: $\frac{B}{2\,A}=-\,0\cdot00264.$



y	$\frac{B}{2\,A}y^2$	$1+\frac{B}{2\,A}y^2$	$\frac{T_y}{T_s}$
0	0	0	1
3	0·024	0·976	1·012
6	0·095	0·905	1·050
9	0·213	0·787	1·127
12	0·379	0·621	1·27

The verification of the results of this dynamical investigation was made by means of models.

The law of variation of T_y in terms of y is due to the existence of the θ^3 terms in C_s, and depends upon θ^3 in consequence of the calculation of stability, that is, having regard to the steepness of the walls and their curvature.

Double Series of Rolling on Regular Waves.

(A) By studying the rolling of models on a regular series of artificial waves we see that certain models take up two different periodical motions without any change either in the form of the model, or in its distribution of weight, or in the period of the wave T_w, or in its height. Each of these motions, once started, continues indefinitely with a period equal to that of the waves which cause it. According to the conditions of the initial motion, one or other of these two possible movements becomes established and continuous.

(B) The study of the phenomenon was made experimentally in a Tank with artificial waves created by a moving shutter and a damping arrangement. Observations were made on the amplitude of the roll and on the phase Φ of this movement in respect of the waves. The plotting of the successive positions of the model and of the orbital motion of the waves was obtained photographically. The model was not constrained by any mechanical attachment other than two light spiral springs having their axes in the mean position of the axis of oscillation.

In order to trace the curves of resonance (i.e. curves of variation of the amplitude of rolling and of the phase in terms of the ratio of the periods of the model and of the waves) under conditions such that the experimental variations of the waves did not interfere, the whole series was obtained on the same wave. The form and height of centre of gravity of the model were constant, but by displacing a weight transversely one could vary the period of rolling of the model in still water, which enabled us to vary the ratio of the period of the model to that of the wave. Such curves or resonance relate to one form of hull and not to one resonator, because the moment of the inertia of the model varies.

(C) The theoretical explanation of the phenomenon can be found by making out a balance sheet of the exchanges of energy during each quarter period. First between a maximum elongation and the passage through the vertical, and then between the passage through the vertical and the next maximum elongation. Experiment showed that the movement is quite sinusoidal and without jerk,* and it has the same period as the wave. Let us call this amplitude y and take as the law of motion

$$\theta = y \cos \frac{2\,\pi}{T_w} \,.\, t$$

and let

$$X = \left(\frac{T_w}{T_s}\right)^2$$

The model is at any moment subject to an inertia couple, to a righting couple, to an energy couple due to the wave, and to the damping couple.

(*a*) The inertia couple is taken as $I\,\frac{d^2\theta}{dt^2}$, where I is the moment of inertia of the model and of the water carried along. I is determined by experiment on rolling in still water in which we observe the period T_s of the oscillations whose amplitude is very small. I is then given by the equation

$$I = A\left(\frac{T_s}{2\,\pi}\right)^2$$

A is a constant given by a statical inclining experiment in still water or by calculation. The work done by the inertia couple is $-\frac{1}{2}\,A\,y^2\,X$ during righting, $\frac{1}{2}\,A\,y^2\,X$ during inclination from the vertical.

(*b*) The righting couple C_s is defined by the curve stability, either by calculation or by observing the statical inclination in still water:—

$$-\,C_s = A\,\theta + B\,\theta^3$$

Its work during the righting period is $\frac{A\,y^2}{2} + \frac{B\,y^4}{4}$, and during the inclining $-\frac{A\,y^2}{2} - \frac{B\,y^4}{4}$.

* Battement.

(*c*) The energy couple is taken as equal to

$$C_0 \cos\left(\frac{2\pi}{T} + \phi\right)$$

The work of the energy couple during the righting period is $y\frac{C_0}{2}\left[\frac{\pi}{2}\sin\phi - \cos\phi\right]$, and during inclination $y\frac{C_0}{2}\left[\frac{\pi}{2}\sin\phi + \cos\phi\right]$.

C_0 and ϕ are unknown.

(*d*) The work of the damping couple is for the same hull and for the same wave (which was the case in the experiments) taken as an unknown function of the amplitude alone, $-\frac{1}{2}f(y)$. This work might be compared to the damping work in still water, but it is not evident *a priori* that the two phenomena are identical.

In describing the total work of the couple as zero during righting and during inclining we obtain two equations:—

$$-\tfrac{1}{2}A' y^2 X + \frac{A y^2}{2} + \frac{B y^4}{4} + y\frac{C_0}{2}\left[\frac{\pi}{2}\sin\phi - \cos\phi\right] - \frac{f(y)}{2} = 0$$

and

$$+\tfrac{1}{2}A' y^2 X - \frac{A y^2}{2} - \frac{B y^4}{4} + y\frac{C_0}{2}\left[\frac{\pi}{2}\sin\phi + \cos\phi\right] - \frac{f(y)}{2} = 0$$

These two equations determine y and ϕ in terms of X.

We have put before $y^2 X$ a coefficient A′ instead of A because it is not evident *a priori* that the increase of inertia due to the water carried along is the same among waves as in still water, and, moreover, the axis of oscillation is not necessarily the same in both cases. We deduce from these two equations

$$\frac{A'}{A}X = 1 + \frac{B}{2A}y^2 - \frac{C_0}{A_y}\cos\phi$$

$$\sin\phi = \frac{f(y)}{\pi C_0 y}$$

To a value of y corresponds a value of $\sin\phi$ and two opposite values of $\cos\phi$, therefore two values X_1 and X_2 of X. These two values X_1 and X_2 will satisfy this equation:—

$$\frac{A'}{A}\left(\frac{X_1 + X_2}{2}\right) = 1 + \frac{B}{2A}y^2.$$

Let

$$X_p = \frac{X_1 + X_2}{2}$$

then

$$\frac{A'}{A}X_p = 1 + \frac{B}{2A}y^2$$

On a graph, if one gives as abscissæ the values of X and as ordinates the values of y, the middle points of the horizontal chords describe a parabola.

This parabola is determined in practice by the point of maximum amplitude (y_0) and one other point.

In tracing this parabola one has at its corresponding point $y = 0$, and hence the value $\frac{A'}{A}$, and therefore of A′.

Calling γ the horizontal half-chord, for a value of y we have

$$\frac{A'}{A}\gamma = \frac{A'}{A}\frac{1}{2}(X_2 - X_1) = \frac{C_0}{A_y}\cos\phi$$

$$\gamma = \frac{C_0}{A'y}\cos\phi$$

For the top of the curve one has $\gamma = 0$, and therefore $\phi = \frac{\pi}{2}$.

Having regard to the value of $\sin\phi$, we have, for the general equation of the chord,

$$\gamma = \frac{C_0}{A'y}\sqrt{1 - \frac{f^2(y)}{\pi^2 C_0^2 y^2}}$$

As we ignore the functions C_0 and f of y, let $C_0 = M y^m$ and $f = R y^r$.

(M, m coefficient and exponent of the energy couple, R, r coefficient and exponent of the couple of resistance.)

We then have

$$\gamma = \frac{M y^m}{A'y}\sqrt{1 - \frac{R^2 y^{2r}}{\pi^2 M^2 y^{2m+2}}}$$

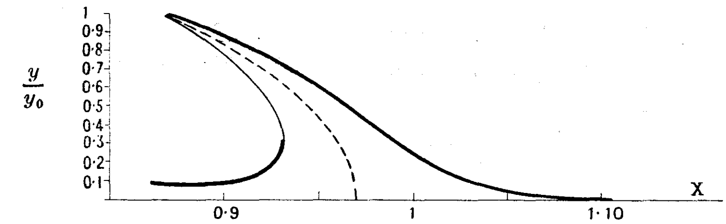
Calling y_0 the maximum amplitude $\frac{R^2}{\pi^2 M^2}\cdot\frac{y_0^{2r}}{y_0^{2m+2}} = 1$,

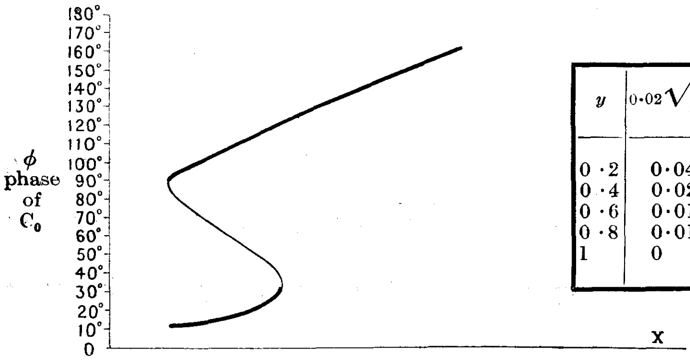
$$\gamma = \frac{M}{A'}\, y^{m-1}\sqrt{1 - \left(\frac{y}{y_0}\right)^{2r-(2m+2)}}$$

$$\gamma^2 = \lambda\left(\frac{y}{y_0}\right)^{2m-2} - \mu\left(\frac{y}{y_0}\right)^{2r-4}$$

γ^2 is the sum of two powers of y, and the experimental curve allows us to determine λ, m, μ, r (but we must have $r \geqslant 2$ and $m < 1$).

We then calculate the value of ϕ. The following Fig. 1 has been drawn on the assumption that $m = 0\cdot5$ and $r = 2$.





y	$0\cdot02\sqrt{\frac{1}{y}-1}$	$0\cdot97-\frac{1}{10}y^2$	X_1	X_2	$\sqrt{1-y}$	ϕ_1	ϕ_2
0·2	0·040	0·966	0·926	1·006	0·894	26	153
0·4	0·024	0·954	0·930	0·978	0·775	39	140
0·6	0·016	0·934	0·918	0·950	0·532	51	129
0·8	0·010	0·906	0·896	0·916	0·447	64	116
1	0	0·87	0·87	0·87	0	90	90

FIG. 1.

(D) If we plot as a curve the values of y in terms of X, theory gives a curve of resonance having one of the two following forms:

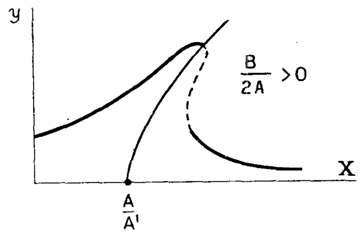


FIG. 2.

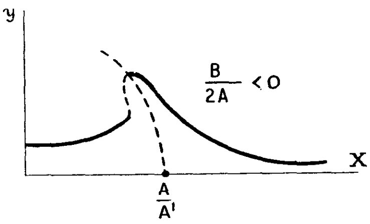


FIG. 3.

The curves in thick lines correspond to the experimental points. The experiments give simultaneously a curve of the phases of the model in relation to the wave (and not of the phases of C_0).

In all cases where $\frac{B}{2A} > 0$, that is, where the curve of stability is over its tangent, we have found experimentally curves, as in Fig. 2, and in all cases where $\frac{B}{2A} < 0$ the curves take the form shown in Fig. 3.

The diameter through the centres of the horizontal chords found experimentally is a curve that does not pass through the point $X = 1$, but through a point $X = \frac{1}{\mu}$.

The experiments showed for all cases where $\frac{B}{2A} > 0$ a value of $\mu > 1$ and inversely.

	$\frac{B}{2A}$	μ
M R 1	− 3·29	0·92
M R 2	− 5·13	0·86
A	− 0·532	0·99
Su	+ 0·368	1·05
C	1·092	1·02
B	1·136	1·02
M S 3	4·34	1·14
M S 4	4·64	1·05

The theoretical parabola of the centres of the horizontal chords is

$$X = \left(1 + \frac{B'}{2A} y^2\right) \frac{A}{A'}$$

which cuts the axis of X at the abscissa point $\frac{A}{A'}$. The value of μ given in the preceding table is therefore the value of $\frac{A}{A'}$, or a ratio of the total inertia (water and ship) in rolling among waves (A′) to still water rolling (A).

Fig. 4 shows that there exists a possible connection between the value of $\frac{A}{A'}$ and the value of $\frac{B}{2A}$.

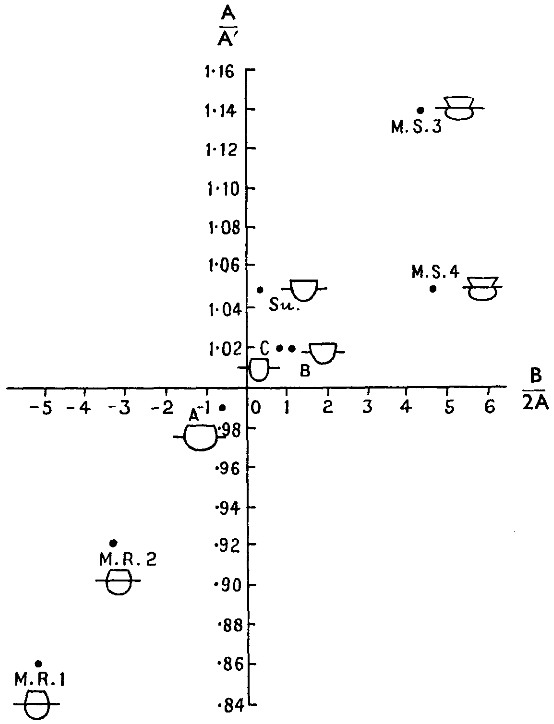


FIG. 4.

(E) The following table gives the relation between the maximum calculated amplitudes and the maximum experimental amplitudes for various curves of resonance, assuming that the function $f(y)$ is the same for rolling in still water and rolling among waves, and that the phase of the couple is the same as the phase of rolling ($\phi = \Phi$):—

	Experiment.	Calculation.
M S 3	0·138	0·146
M S 4	0·186	0·164
C	0·208	0·176
M R 1	0·262	0·294
M R 2	0·275	0·393
B	0·357	0·346
Su	0·382	0·383
A	0·515	0·600

The classification from experiments and from theory is the same (with the exception of model M R 2).

NOTE.—New experiments made on model M R 2, with an unimportant modification, have given 0·420 and 0·550, so that the whole classification is now correct.

DISCUSSION.

The CHAIRMAN (Sir Westcott Abell, K.B.E., M.Eng., Vice-President): I am glad to see that after many years the question of rolling of ships is once more being studied—shall I say theoretically—as I do not think it has been done for a very long time. I happen to know that our old friend, Admiral Taylor, has expressed the opinion on more than one occasion that some of us who devote so much time to resistance might with advantage pay attention to some of the phenomena of rolling. I am very glad, therefore, that Mr. Barrillon has started us again on the investigation of these phenomena. If I remember rightly, at the time when William Froude was investigating the question of rolling there were seriously divergent views expressed by M. Bertin. In the old days of naval science we had long discussions as to the manner in which the extinction curve could be expressed; there were great differences of opinion, and I do not yet know which is the best solution.

I think we are very much indebted to Mr. Barrillon for his contribution to this subject; he has made very clear to us the phenomena we are discussing.

Dr. F. HORN (Member): Without knowing that Mr. Barrillon was going to read a paper on this subject, I recently made a little experiment which completely confirms the character of the phenomenon of double systems of rolling that Mr. Barrillon has stated and analysed. It may be of some interest to you to learn how I carried out these experiments. They were not made in water, but in the following manner. It is well known that one can imitate the rolling of a ship in still water by materializing the curve of the successive points F of the centres of displacement. I shall call it the "F curve." One lets it roll on a plane, loaded with weights so arranged with respect to the point F in the upright position that the correct values of metacentric height and radius of inertia are realized. Of course, one can easily verify by this simple means the dependence of the period of non-synchronous rolling from the amplitude. Now, instead of a plane, a movable batten was used, the normal position of which was horizontal, and this batten was caused to oscillate in such a manner as to make the tangent of a trochoidical wave. It is quite a simple matter to make a mechanism which fulfils these conditions. If you put on this batten a materialized F curve loaded in the manner described, you can easily study on this simple apparatus all the phenomena of oscillations in waves. In this way, one can in fact verify that with the same proportion $\frac{T_w}{T_s}$ (wave period to ship's rolling period in still water with infinitely small amplitude) one gets two different amplitudes. Whether one gets the one or the other depends on the conditions of starting the movement. By successively varying the wave period T_w, and therefore the proportion $\frac{T_w}{T_s}$, and by measuring the amplitudes so produced, one may easily get a curve of the same character as Mr. Barrillon has deduced in his paper.

I may conclude by congratulating Mr. Barrillon for the extremely able manner in which he has analysed these very interesting phenomena.

Professor H. R. MØRCH (Member): I presume that Professor Barrillon has made use only of large models for his rolling experiments. If not, I am afraid he will have to consider the effect of surface tension. At Trondhjem we have done a lot of rolling experiments with models about 70 cm. in length (about 30 in.), in still water and amongst waves. For inclining experiments and for rolling in still water we controlled the effect of surface tension by putting a sheet of oil on the water surface. With such a sheet of oil we got much easier rolling. In this connection we worked out a theory to cover this case. I hope in the near future to finish the work and get it published.

The Chairman: I am very pleased to see that in the two papers we have before us this morning, one by Mr. Barrillon and one by my colleague Dr. Havelock, they have been so good as to translate their mathematical analysis into the energy equation. I speak rather feelingly because, having to deal with some of the efforts of our mathematical friends, I find their methods of analysis are rather beyond the ordinary naval architect's vision or capacity. If the energy analysis is used, it is much easier for the ordinary student to make a mental picture of what happens. Further—I speak subject to correction—the use of the energy equation is also better, as far as the first differential is concerned. Beyond that stage my mathematical friends tell me it is better to go back to their ordinary methods. But as the naval architect is concerned largely with the first differential, I hope this practice of Dr. Barrillon, Dr. Havelock, and some others will be followed in translating the results of their investigations into language that the ordinary naval architect can more easily appreciate.

Professor E. G. Barrillon: Dr. Horn spoke of experiments in rigid dynamics showing cases of double régime. The earlier experiments on small vibrating elastic bodies are well known to those who study organ pipes. In my experiments the aim was to determine the unknown moment due to the action of the wave on a floating body; the problem is one of hydrodynamics with an unknown function, and cannot be replaced by a problem of rigid dynamics. Professor Mørch has spoken of the influence of the surface tension. This force acts certainly on floating models. Using different scales of models and comparing these results with those on ships we have found that the results from the smallest models are sometimes nearer to those on the ship than those of large models. In my paper the surface tension is mixed up with the other forces acting on the floating body.

The Chairman: I wish to thank Mr. Barrillon for his excellent paper which has given us much to think about. I do not think we quite realized before how necessary it was to study the conditions of synchronous rolling. We have always said that if a shipmaster finds circumstances where there is synchronous rolling, he had better change course and get out of it. Mr. Barrillon has tackled the problem the other way; he has accepted the fact that you may get synchronous rolling motion for some time, and has shown us how to deal with it. I would like you to give him a very hearty vote of thanks for his paper.

Written Contributions to the Discussion.

Mr. M. P. Payne (Member): Professor Barrillon here presents us with an elegant attack on two problems of the more advanced type, using the principle of the Conservation of Energy in place of the more usual equation of torque and angular acceleration. The information is very welcome, and is a worthy addition to the work of Froude, Bertin, and Scribanti.

The first problem is the expression of period as a function of amplitude in a manner which will give sufficient accuracy without undue calculation. I note that the motion is postulated as simple harmonic, although the stability couple is such as not to cause this, but this will not seriously affect the result. A hasty comparison with Scribanti's work * suggests that Professor Barrillon's formula is a close approximation to the exact solution. I would question the relevance of the damping forces mentioned in this section; with simple harmonic motion assumed and with no decrement introduced between successive quarter periods, I suggest that the problem is properly one of unresisted rolling. Otherwise, moreover, the final formula should contain some function of the damping if it is to accord with observation.

The second problem concerns resisted rolling among waves, and in particular the phenomenon of resonance. It is a general solution of the case of non-isochronism at large angles of roll, first dealt with quantitatively, I believe, by Rankine for the Committee of Designs, 1871. The case of unresisted rolling is easily derived by putting $f(y) = 0$, and

* Trans. I.N.A., Vol. XLVI. (1904).

merely consists of an increase in the horizontal half-chord γ at any value of y, the phase difference ϕ being constant and equal to 0 or π. The effect of the departure of the stability curve from the tangent at the origin, determined by B, on the amplitude rear resonance is clearly demonstrated either with or without resistance. The double system of rolling appears to be a consequence of the existence of damping, since then two supplementary angles of ϕ are defined; but it would appear that these are associated with two distinct values X_1 and X_2 of X, and, therefore, since the same wave period was employed, with apparently two distinct values of T_s, which does not accord with the statement in paragraph (A). I should be obliged if the author would explain this in greater detail for the benefit of those who, like myself, have not his facility in mathematics, and I would suggest that the addition of a diagram illustrating a train of waves and models in two corresponding positions, one for each motion, would be of assistance.

There is one further point on which I should like to comment, that is the possible relation indicated between A/A′ and B/2 A. There seems at first sight little possibility of more than a fortuitous connection between the virtual mass of the ship and the shape of the sides near the water-line, and I would like to ask Professor Barrillon if his experiments have completely ruled out the possibility of A/A′ being dependent on amplitude. I suggest this because it would seem that the mid-chord parabola is determined by two points at fairly large amplitudes, and the intersection with the base may be somewhat uncertain. The suggestion implies a small change in the formula for period in terms of amplitude which the experiment results would, no doubt, verify.

Finally, I should like to say that in my opinion the thanks of the Institution are due to Professor Barrillon for reviving this interesting subject with such a useful paper.

Professor E. G. Barrillon: With reference to the rolling in smooth water, Mr. Payne says that I have taken into consideration a harmonic motion inconsistent with the law adopted for the couple of stability. If one were dealing with a rigid body revolving round a fixed axis, Mr. Payne's remarks would be applicable, but in this particular case one is dealing with an entity consisting of a rigid body plus the water carried along with it. Moreover, the rigid body does not revolve round a fixed axis. Experience shows that the law of variation of θ as a function of t is really harmonic; so far, I have been unable to trace any deviation from the law $l^{-\lambda t}\cos t$ for an isolated oscillation. I may add that the relation between T_y and T_s can be quite independent of the damping; this is particularly the case when the damping couple is proportionate to $\frac{d\theta}{dt}$.

In the case of rolling on a swell, the law for which is illustrated in Fig. 1 (page 424), by grouping together two points on one same horizontal line, for one value of y there are obtained two values for X, say X_1 and X_2. As indicated by Mr. Payne, these points are obtained for two different values for T_s. This is not the double régime phenomenon. In order to find in Fig. 1 the double régime phenomenon, one has to group together points which are on the same vertical line in Fig. 1. These points correspond to one value for X and one value for T_s, as stated in paragraph (A).

In regard to the relation between A/A′ and B/2 A, there may be here a fortuitous connection only; it may, however, be that the form of the ship's sides in the vicinity of the water-line exerts an influence upon the nature of the waves set up by the rolling, and that the experimental virtual inertia does not depend solely upon the water carried along, but also upon the waves set up by the ship. The value for A/A′ must therefore depend upon the amplitude of the relative rolling.

The intersection of the mid-chord parabola with the axis is certainly not determined with any very great precision. The fluctuation may lie between 0·95 and 0·97, for example, for a particular value of A/A′. The main uncertainty in the measurements does not reside in the amplitudes but in the phases.

WAVE PATTERNS AND WAVE RESISTANCE.

By Professor T. H. Havelock, F.R.S.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 12, 1924, Sir Westcott Abell, K.B.E., M.Eng., Vice-President, in the Chair.]

Introduction.

1. It is not my intention to discuss in this paper practical problems of ship resistance, but rather to review briefly certain points in the mathematical theory of ship waves and wave resistance. In doing so, I shall not attempt to give the derivation of formulæ or any mathematical analysis of them; my main object is to give a descriptive or qualitative account of some of the mathematical expressions and to show how in some cases deductions may be drawn from an inspection of them.

The wave pattern made by a ship is familiar both from observation and as a subject of mathematical study, and it is equally fascinating from both points of view. Perhaps the earliest theoretical account is that given by Kelvin in 1887 in his well-known lecture on ship waves to the Institution of Mechanical Engineers. That lecture was based on mathematical work of which a later improved version was published by Kelvin in 1904,* and it is this later work which is usually quoted now in the text-books. The ship, in that work, is idealized to a point disturbance travelling over the water and at the same time sending out groups of waves which combine in such a way as to produce the characteristic pattern of transverse and diverging waves. The early history of this idea of wave groups and group velocity is also of some interest. In a letter written to Stokes in 1873, William Froude describes the motion of a group of waves, how the group as a whole advances with a less velocity than that of the waves composing it, wave crests advancing through the group in its motion and appearing to die away at the front while new ones are formed at the rear; he writes, in his letter from Torquay, "In my long experimental tank or canal here, I have frequent opportunity of noticing this in the propagation of artificially generated waves. I have not, indeed, yet investigated it quantitatively, because my hands are full: but at a later date when experiments on the oscillation of models will be the work in hand, I shall have to establish regular appliances for the generation of waves, and the investigation to which I refer will be comparatively easy." It was in 1876 that Stokes gave the kinematical explanation of group velocity, a more general account being given shortly after by Rayleigh. This was followed in 1877 by Osborne Reynolds' dynamical theory of group velocity, connecting the flow of energy and the rate of work of the fluid pressure in a train of waves; it is this latter point of view which is of fundamental importance in the theory of wave resistance.

Much work has been done since then, both on the detailed structure of wave patterns

* *Edin. Roy. Soc. Proc.*, Vol. XXV. (i), "On Deep Water Two-dimensional Waves produced by any given Initiating Disturbance"; "On the Front and Rear of a Free Procession of Waves in Deep Water"; and "Deep Water Ship Waves."

and on the calculation of wave resistance, and more recently on the comparison of calculated results with experiment; but the fundamental principles remain the same, and it is these which I wish specially to keep in view in the following notes. We begin by considering freely moving wave patterns; that is, not forced waves produced by the motion of a ship, but waves moving freely and steadily over the surface of the water under the action of gravity alone. We imagine the pattern to be produced by the mutual interference of simple plane waves moving freely in all directions, their phases and velocities being suitably adjusted; the elementary properties of the pattern are described from this point of view. Then, considering the waves produced by a ship, we see that these must approximate, at a sufficient distance to the rear of the ship, to such a freely moving pattern; this is illustrated by calculations made for certain ship models. Finally, it is shown how the wave resistance can be obtained from considerations of energy when we know the structure of the wave pattern formed at a great distance in the rear of the ship.

Free Wave Patterns.

2. The simplest form of free waves on the surface of water consists of simple harmonic waves with straight parallel crests, the procession of waves extending over the whole surface. If the velocity of the waves is c, the wave-length is $2\pi c^2/g$ for deep water; so that if we take an origin O in the surface and take O x in the direction of propagation, the waves might be represented by

$$\zeta = \sin \frac{g}{c^2}(x - c\,t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where ζ is the surface elevation, and we have taken the waves to be of unit amplitude.

Suppose now that the waves are travelling in a direction making an angle θ with O x, and that the wave velocity is $c \cos\theta$; then, with O y in the surface and perpendicular to O x, the waves are now represented by

$$\zeta = \sin \left\{\kappa \sec^2\theta\,(x\cos\theta + y\sin\theta - c\,t\cos\theta)\right\} \quad . \quad . \quad . \quad . \quad (2)$$

where we have written $\kappa = g/c^2$.

An equal procession of waves moving in a direction making a negative angle θ with O x is given by

$$\zeta = \sin \left\{\kappa \sec^2\theta\,(x\cos\theta - y\sin\theta - c\,t\cos\theta)\right\} \quad . \quad . \quad . \quad . \quad (3)$$

Superpose these two sets of plane waves, and we have a wave pattern given by the sum of (2) and (3), or

$$\zeta = 2\cos(\kappa\,y\sin\theta\sec^2\theta)\sin\left\{\kappa\,(x - c\,t)\sec\theta\right\} \quad . \quad . \quad . \quad . \quad (4)$$

These have sometimes been called corrugated waves. We may get a rough idea of the result by drawing parallel straight lines to represent the positions of the crests and troughs of the component systems at a given instant; and so we get the picture of a diamond-shaped pattern, covering the whole surface and moving steadily in the direction O x with velocity c.

We now generalize by supposing that we have simple straight-crested waves like (2) travelling forward in all directions included within 90° on either side of O x. Superposing these component plane waves will give a surface elevation

$$\zeta = \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \sin\left\{\kappa\sec^2\theta\,(x\cos\theta + y\sin\theta - c\,t\cos\theta)\right\} d\theta \quad . \quad . \quad . \quad (5)$$

and this will represent a free wave pattern of some form travelling steadily parallel to O x with velocity c.

We may again obtain a rough picture of the result by simple graphical methods. Suppose we represent a component plane wave of (5) by parallel straight lines showing the crests and troughs at, say, the instant $t = 0$, in the manner shown in Fig. 1, the full lines representing crests and the broken lines troughs.

Now draw similar lines on the same diagram for a large number of values of θ in the range from $-90°$ to $+90°$. It is instructive to take, for instance, intervals of 10° and to

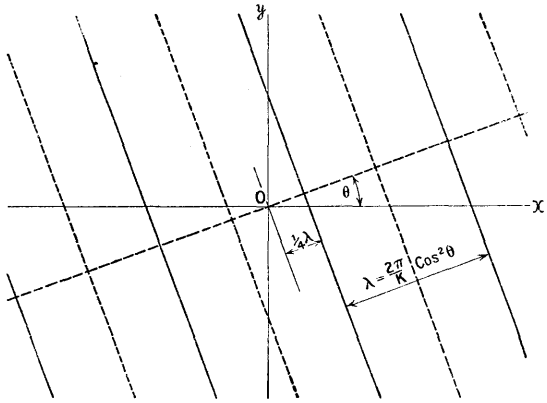


FIG. 1.

draw 19 sets of lines as in Fig. 1. Such a diagram is not given here, as there is too much detail for reproduction on a small scale; but it is interesting to see the picture of a familiar wave pattern emerging from such a diagram. The curves which we see in process of formation are shown in Fig. 2.

These curves are, of course, the envelopes of the lines of constant phase of the component waves, and their mathematical equations are most easily obtained by expressing

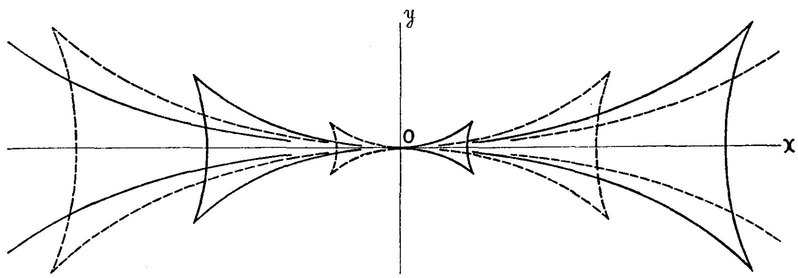


FIG. 2.

that fact. When we look into the formation of the curves we see that they represent places where component crests, or troughs as the case may be, combine together to give prominent features of the pattern; on the other hand, we may say that at points at some distance outside the region covered by these curves the component crests and troughs tend to cancel each other out on the average. We arrive in this way at the picture of a wave pattern of transverse and diverging waves, with a focus point O, and extending in advance of this point as well as to the rear; the whole forms a freely moving pattern travelling forward with steady velocity. It need hardly be said that this description of the pattern represented by (5) is only a first approximation; detailed mathematical analysis is necessary for a more correct and intimate knowledge of the surface elevation.

Examine more closely one of the curves of Fig. 2, say the portion O A B which is shown in Fig. 3 along with the crest lines of the component plane waves.

We find that the transverse part A B is made up from those plane waves whose direction angles range from zero up to an angle θ_1, which is such that $\cos^2\theta_1 = \frac{2}{3}$, or $\theta_1 = 35° \, 16'$ approximately; the diverging part O A comes from the plane waves whose directions range from θ_1 to 90°. The angle between the crest line O A and the central line O B is 19° 28′, nearly. To complete our picture we require some information about the height of the waves in the pattern defined by the expression (5). All that need be said here is that, following a curve such as B A O, the height is fairly constant over the central portion of the transverse wave, increases in the neighbourhood of a crest point A and then decreases along the diverging wave to zero at the point O.

It may also be noted that the wave-length λ of a component plane wave being $(2\pi c^2/g)\cos^2\theta$, these wave-lengths range from $2\pi c^2/g$ to zero.

3. Consider for a moment the difference in these general results if the water, instead of being very deep, is of given finite depth h. The relation between velocity and wave-length for a simple plane wave is different, and, moreover, there can be no plane wave

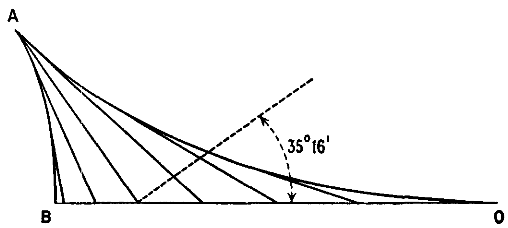


FIG. 3.

whose velocity is greater than $\sqrt{(g h)}$. Suppose we build up a pattern like (5) when the velocity c of the pattern is less than this critical value $\sqrt{(g h)}$. We could trace the envelope curves in the same way and obtain a wave pattern similar to Fig. 2. The chief difference is that the wave pattern widens out; the angle of the cusp line is greater than the value 19° 28′ for deep water and it increases with the velocity c. In addition, the transverse waves become less curved, the angle θ_1 of Fig. 3 being less than the value 35° for deep water and becoming less as the velocity c is increased.

If the velocity c is made greater than the critical value $\sqrt{(g h)}$, we see at once that we must omit a central portion of the integration in (5), because the component plane waves can only begin to exist at such an inclination θ that their wave velocity $c\cos\theta$ is equal to $\sqrt{(g h)}$. On working out the wave pattern in more detail, it is found that it consists then of only diverging waves.

4. We return to the expression (5) for deep water. The origin O was taken at a fixed point, but it is more convenient to take a moving origin for the co-ordinates at the focus point of the wave pattern; so in what follows we shall write x instead of $x - c t$. Further, for brevity we shall write

$$(x, y) = \kappa \sec^2\theta \,(x\cos\theta + y\sin\theta) \quad . \quad . \quad . \quad . \quad . \quad (6)$$

We may call the surface elevation given by

$$\zeta = \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \sin(x, y)\, d\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

a simple sine pattern.

We could also have used a form

$$\zeta = \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \cos(x, y)\, d\,\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which may be called a simple cosine pattern. The general form of the pattern is the same in both cases, with the necessary changes in wave heights due to the interchange of crests and troughs. It would be of interest to have a more detailed mathematical and numerical analysis of these two simple forms.

In (7) and (8) the amplitudes of the component plane waves are taken to be the same for all directions. We may now proceed to a final generalization by supposing that in each case there is an amplitude factor depending upon the direction of each component; adding the two forms, we arrive at a general expression for a freely moving wave pattern, namely

$$\zeta = \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} f(\theta) \sin(x, y)\, d\theta + \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \mathrm{F}(\theta) \cos(x, y)\, d\theta \quad . \quad . \quad . \quad . \quad (9)$$

It is true that the amplitude factors may alter considerably our picture of the pattern, especially if they have pronounced maxima or minima; however, we shall see that most cases which have been calculated for ship models can be reduced to terms like (9) with simple amplitude factors.

Ship Waves.

5. We have been dealing so far with a free wave pattern; that is, we have supposed the system to be completely in existence at some instant, and then afterwards it moves freely and steadily forward.

Consider now the disturbance produced, in a frictionless liquid, by a moving ship or by a disturbing pressure system moving steadily forward. At some distance in advance of the ship there can be no appreciable disturbance, as we suppose it moving forward into still water. In the immediate neighbourhood of the ship the disturbance will be of a complicated character. But as we go further and further to the rear, the surface disturbance must approximate more and more to some freely moving wave pattern following on with the same speed as the ship.

For instance, if a long cylindrical log is moved with steady velocity c at right angles to its length, the disturbance at a great distance in the rear must approximate to a simple plane wave of velocity c, whose wave-length is therefore $2\pi c^2/g$. It could be expressed by (1), taking some suitable point as the origin O, and including some definite amplitude factor; this amplitude factor would be the important thing left to be determined from the form of the cross-section of the cylinder and its velocity. Similarly for an ordinary ship form, the waves at a great distance in the rear must approximate to some freely moving wave pattern such as we have been considering; and for some suitable origin O, in or near the ship, they must therefore be expressible in the form (9), with amplitude factors $f(\theta)$ and $F(\theta)$ depending upon the form of the ship and the speed. Without going into the details of calculating these expressions we shall now examine a few cases in order to illustrate the types of wave pattern which occur in such problems.

Point Disturbance and Sphere.

6. On account of its historical interest we may mention first the travelling point

disturbance examined by Kelvin in the paper to which reference has already been made. In this case the waves in the rear approximate to the form

$$\zeta = \mathrm{A} \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \sec^2 \theta \sin (x, y) \, d\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

where A is a constant, and we use the notation specified in section 4.

We may describe this as a sine pattern with an amplitude factor $\sec^2 \theta$, which varies from unity at $\theta = 0°$ to infinity at $\theta = 90°$. We have seen, in section 2, that the transverse waves of the pattern come from the range 0° to 35° approximately, while the diverging waves come from the rest of the range 35° to 90°, taking one side of the central line O x. Thus we should expect the diverging waves in this case (10) to be increased in magnitude

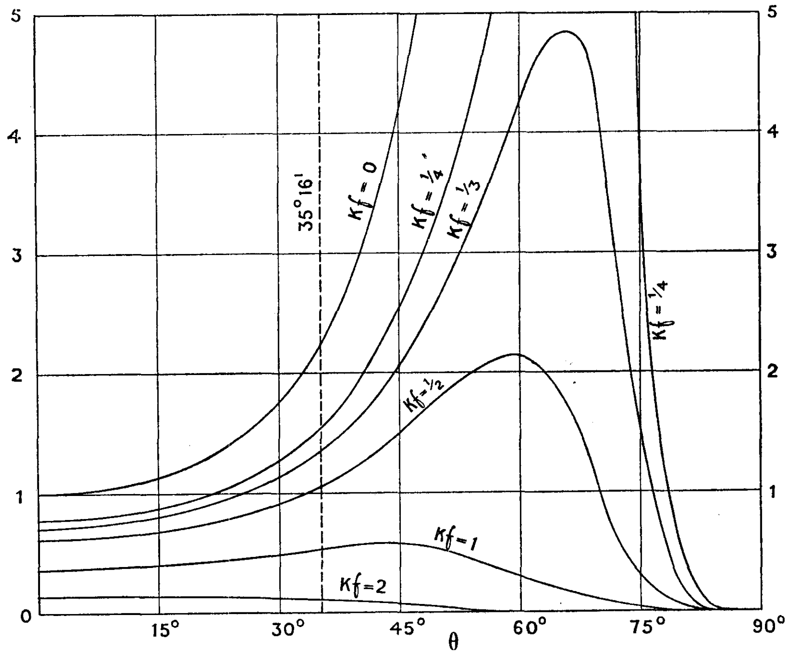


FIG. 4.—GRAPHS OF $\mathrm{SEC}^4 \theta \, e^{-\kappa f \sec^2 \theta}$ FOR DIFFERENT VALUES OF κf.

compared with those for the simple sine pattern (7); these features are unduly prominent in the Kelvin pattern in comparison with those made by an ordinary ship model. Incidentally we may note that the factor $\sec^2 \theta$ causes the expression (10) to have an infinite value at the focus point.

An interesting contrast is for a small sphere, of radius a, submerged in the water with its centre at a depth f and moving with velocity c. The expression is now

$$\zeta = 2 \kappa^2 a^3 \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \sec^4 \theta \, e^{-\kappa f \sec^2 \theta} \sin (x, y) \, d\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

the focus point O being vertically above the centre of the sphere.

In Fig. 4 are shown curves of the amplitude factor $\sec^4 \theta \exp. (- k f \sec^2 \theta)$ for different values of κf, that is, of $g f / c^2$.

From these curves we get at once some idea of the relative importance of the transverse and diverging waves for different depths or for different speeds. We see that the effect of increasing depth, at the same speed, is to diminish relatively the diverging waves.

But these are perhaps details of purely theoretical interest, and we turn now to some cases of ship models.

Models of Great Draught.

7. We consider first a model of great draught, of uniform horizontal cross-section throughout and with parabolic lines; this is a model which has been investigated by Mr. W. C. S. Wigley,* working at the William Froude Laboratory. Fig. 5 shows the horizontal section.

Taking the origin O at the mid-point, the equation of the curve A C B is $y = b(1 - x^2/l^2)$,

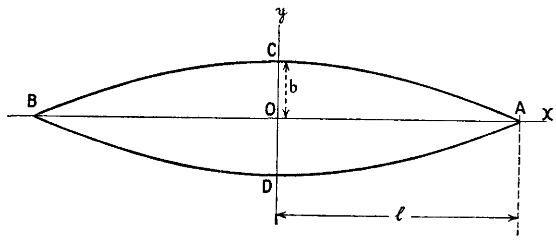


Fig. 5.

the beam being $2b$ and the length $2l$. It can be shown that, on the usual mathematical theory, the waves in the rear of the model approximate to

$$\zeta = \frac{8b}{\pi \kappa^2 l^2} \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \{\kappa l \cos(\kappa l \sec\theta) - \cos\theta \sin(\kappa l \sec\theta)\} \sin(x, y)\, d\theta \quad . \quad (12)$$

This might be regarded as a sine pattern with a somewhat complicated amplitude factor; but fortunately we can dissect it into simpler components, for it is identically equal to

$$\zeta = \frac{4b}{\pi} \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \left\{ \frac{c^2}{gl} \sin(x - l, y) + \frac{c^2}{gl} \sin(x + l, y) \right.$$
$$\left. + \frac{c^4}{g^2 l^2} \cos\theta \cos(x - l, y) - \frac{c^4}{g^2 l^2} \cos\theta \cos(x + l, y) \right\} d\theta \quad (13)$$

Here the pattern is seen to be the combined result of superposing four simple patterns, two focussed at the bow and two at the stern. The first two are simple sine patterns, with constant amplitude factors at a given speed; they may, in fact, be attributed directly to the finite angle of the model at the bow A and at the stern B respectively. The other two terms in (13) are cosine patterns, with an amplitude factor $\cos\theta$ in each case; although one is focussed at the bow and the other at the stern, it is more appropriate to regard these two terms together as representing the resultant effect of the curved sides A C B and A D B of the model.

A matter of great interest is the mutual interference of these four patterns according to the speed, the extent to which it is possible to make the crests of one pattern coincide with the troughs of another and the speeds at which maximum effects of this kind occur; however, these points are better considered in connection with the corresponding wave resistance.

* *Proc. Roy. Soc.*, A, Vol. 144, p. 144.

Notice first the magnitudes of the terms in (13). The bow and stern systems are factored by $c^2/g\,l$, while the effect of the curved sides has the factor $c^4/g^2\,l^2$. Hence at low speeds the bow and stern provide the greater part of the wave system, but as the speed increases their relative importance becomes less. Then we have the effect of the amplitude factor $\cos\theta$ in the last two terms of (13). Remembering the distinction between transverse waves and diverging waves in a simple pattern, and that $\cos\theta$ diminishes from unity at 0° to about 0·8 at 35° and then to zero at 90°, we may describe the result in general terms: the effect of the gradual change of slope along the curved sides of the model compared with the finite angle at bow and stern is to diminish the relative importance of the diverging waves. This point is amplified further in the following model.

8. In this model one end, say the stern, is drawn out to a fine point. The model is again of great draught and is of uniform horizontal section throughout. Fig. 6 shows the form of the horizontal section.

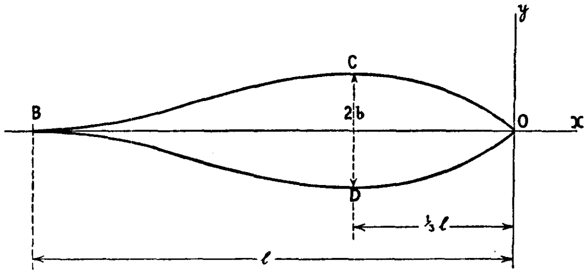


FIG. 6.

Taking the origin O at the bow, as in the diagram, the equation of the curved side O C B is

$$y = -\frac{27\,b}{4\,l^3}\,x\,(x+l)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (14)$$

The maximum beam $2\,b$ occurs at one-third of the length from the bow.

The wave pattern in the rear is given, in our abbreviated notation, by

$$\zeta = \frac{27\,b}{2\,\pi}\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}}\left\{\frac{c^2}{g\,l}\sin(x, y) - 4\left(\frac{c^2}{g\,l}\right)^2\cos\theta\cos(x, y) - 6\left(\frac{c^2}{g\,l}\right)^3\cos^2\theta\sin(x, y)\right.$$
$$\left. - 2\left(\frac{c^2}{g\,l}\right)^2\cos\theta\cos(x+l, y) + 6\left(\frac{c^2}{g\,l}\right)^3\cos^2\theta\sin(x+l, y)\right\}d\,\theta \qquad (15)$$

Here we have five simple patterns, the first three focussed at the bow and the last two at the stern. The first term in (15) is the simple sine pattern due to the finite angle of the model at the bow; we notice there is no similar term for the stern because the angle has been smoothed away completely at the stern. The last four terms of (15) taken together represent the resultant effect of the curved sides O C B and O D B of the model. The general inferences are the same as for the previous model; but we notice that we have now, in (15), patterns with an amplitude factor $\cos^2\theta$, and for such the relative importance of the diverging waves is still further diminished.

EFFECT OF DRAUGHT.

9. Another point about which we may make some broad deductions from the formulæ for the wave patterns is the effect of the draught of the model. In the previous cases

we have supposed this to be very large, or theoretically infinite. Let us suppose now that the model is of uniform horizontal section down to a depth d below the surface and is then cut off by a horizontal plane. For our present descriptive purpose, we may make some simplifying assumptions in deducing the formulæ for the wave system, but these need not be investigated here; it is sufficient to state the general result. The effect of making the model of draught d, instead of infinite draught, is simply to introduce into each of the terms for the component patterns, in say (13) or (15), an additional amplitude factor, namely

$$1 - e^{-\kappa d \sec^2 \theta} \qquad (16)$$

Since $\kappa d = g d / c^2$, the value of this factor depends upon the speed. Fig. 7 shows curves of this quantity (16), for different values of κd, for the half range of values of θ from 0° to 90°.

From inspection of this diagram we see at once that, for a given speed, if the draught is diminished the transverse waves of the pattern become less important. We may put

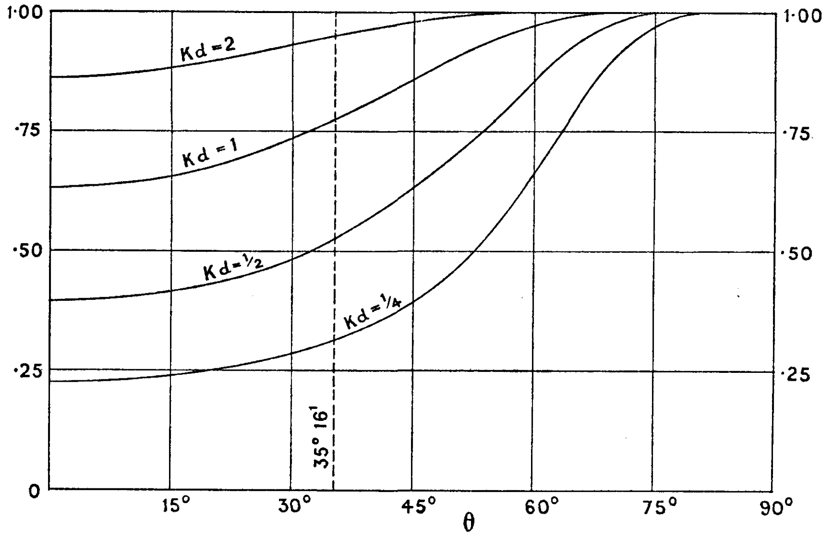


FIG. 7.—GRAPHS OF $1 - e^{-\kappa d \sec^2 \theta}$ FOR DIFFERENT VALUES OF κd.

alongside this a remark drawn from observation; for instance, in Taylor's *Speed and Power of Ships* there is the statement: "Narrow deep ships have wave patterns whose transverse features are more strongly accentuated than those of broad shallow ships."

This might, of course, be anticipated without any mathematical expressions. For the effect of a plane wave on the surface is only appreciable down to a depth of, say, half its wave-length. But of the component plane waves which combine to make the pattern, that which is travelling in the same direction as the ship has the greatest wave-length, and in inclined directions the wave-length is proportional to $\cos^2 \theta$ and diminishes to zero at 90°. Thus as we diminish the draught, for a given speed, the first components to be affected are those of longest wave-length and those are the components which provide the transverse waves of the pattern. However, the mathematical expressions enable us to obtain at least a rough quantitative estimate of the effect.

WAVE RESISTANCE.

10. We turn now to the calculation of wave resistance, and for this purpose it is essential to have a knowledge of the wave patterns we have been considering. Throughout

all this work we are assuming the liquid to be frictionless; or, rather, we suppose that frictional resistance and the effects of viscosity have been treated separately and so eliminated from the wave problem in order to make it more amenable to calculation. It is true that the most direct idea of wave resistance is to regard it as what it is in fact, namely, the combined backward resultant of the fluid pressures taken over the hull of the ship; but this is by no means the simplest method for purposes of calculation.

On the other hand, by a direct application of the method of energy and work, we shall see that we only need to know the wave pattern at a great distance in the rear of the ship.

Denote by S the position of the ship at any instant, by A and B two infinite vertical planes in given fixed positions at right angles to the direction of motion of the ship, the plane A being in advance of the ship and the plane B to the rear.

Consider the rate of increase of the energy of the fluid in the region between the surface of the ship and these two planes, and consider also the forces operating at the boundaries of this portion of fluid. The fluid possesses kinetic energy due to its motion and potential energy arising from alterations in the surface elevation. Calculate the rate at which total energy, kinetic and potential, is flowing into the region in question across the plane B and call this E (B). A similar calculation would give E (A) for the rate at which total energy is flowing out of this region across the plane A. At any point of the plane B let p be the fluid pressure and u the component fluid velocity inwards at right angles to this plane. The fluid to the left of B is doing work on the fluid to the right at a rate $p\,u$ per unit area at each point of the plane; summing up for the whole plane, we call W (B) the rate at which work is being done on the fluid in question across the plane B. Similarly, $-$ W (A), calculated in the same way for the plane A, is the rate of work across that plane upon the fluid between the two planes. Finally, if R is the resultant resistance to the motion of the ship and c its velocity, the ship is doing work on the fluid at a rate R c. Hence, equating the total rate of work upon this portion of fluid to the rate of increase of its total energy, we deduce a general expression for R,

$$\mathrm{R}\,c = \mathrm{E}\,(\mathrm{B}) - \mathrm{W}\,(\mathrm{B}) - \{\mathrm{E}\,(\mathrm{A}) - \mathrm{W}\,(\mathrm{A})\} \quad . \quad . \quad . \quad . \quad (17)$$

This holds for any two fixed planes, one in advance of the ship and the other to the rear. If we take plane A further and further in advance, the quantities E (A) and W (A) approximate to zero, since the ship is advancing into still water. And if we take B further and further to the rear, the disturbance approximates to a free wave pattern such as we have considered in the previous sections and we can calculate the quantities E and W for any plane of that free wave pattern. Thus we have finally

$$\mathrm{R}\,c = \mathrm{E} - \mathrm{W} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

where E and W are calculated from the free wave pattern to which the disturbance approximates at a great distance in the rear of the ship.

11. This method is familiar in its application to plane waves with straight parallel crests. It is probable that the first calculations of wave resistance were those made in this way for plane waves, the argument being usually expressed in terms of group velocity. For simple harmonic waves of height h the average total energy is $\frac{1}{2}\,g\,\rho\,h^2$ per unit area of surface; thus the quantity E of (18) is $\frac{1}{2}\,g\,\rho\,h^2\,c$ per unit length parallel to the crests. The quantity W is exactly one-half of this amount; or, as it is usually expressed, the group velocity is one-half the wave velocity. Hence from (18) we have $\mathrm{R} = \frac{1}{4}\,g\,\rho\,h^2$, where R is the wave resistance per unit length of the cylindrical body to whose motion the waves are due.

It is rather curious that this method has not been used for obtaining the wave resistance from the wave pattern produced by ordinary ship forms. The formulæ in use at present have been developed by other methods. In some cases they have been found from the resultant fluid pressure on the ship. Another method is to introduce an artificial kind of fluid resistance, calculate the rate of dissipation of energy, and so ultimately arrive at expressions for the wave resistance. All these methods must lead to the same results if carried out correctly; but perhaps the most natural method is that outlined above and embodied in the general expression (18).

It has been shown recently that the necessary calculations can readily be extended to wave patterns of the general type which occur in ship waves.* The results may be given here, without going into the detailed analysis.

Suppose first that we have a free wave pattern given by

$$\zeta = \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} f(\theta) \sin(x, y)\, d\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

and suppose that the amplitude factor $f(\theta)$ is an even function of θ, so that (19) is equivalent to

$$\zeta = 2 \int_0^{\frac{\pi}{2}} f(\theta) \sin(\kappa\, x \sec\theta) \cos(\kappa\, y \sin\theta \sec^2\theta)\, d\theta \quad . \quad . \quad . \quad (20)$$

We can write down the velocity potential of the fluid motion for the wave form (19) and so obtain the pressure and velocity at any point of the fluid. The quantities E and W of (18) can then be calculated, with suitable limitations on the function $f(\theta)$ which amount to ensuring that E and W are in fact finite and calculable. Under these conditions it is found that E — W for the pattern (19) is given by a remarkably simple expression, namely,

$$\mathrm{E} - \mathrm{W} = \pi\, \rho\, c^3 \int_0^{\frac{\pi}{2}} \{f(\theta)\}^2 \cos^3\theta\, d\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

Hence the wave resistance of a body moving with velocity c and leaving in its rear a pattern (19) would be given by

$$\mathrm{R} = \pi\, \rho\, c^2 \int_0^{\frac{\pi}{2}} \{f(\theta)\}^2 \cos^3\theta\, d\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

12. Suppose, for illustration, that the amplitude factor is independent of θ and that we have

$$\zeta = h \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \sin(x, y)\, d\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

a simple sine pattern, with h possibly a function of the velocity c. This is certainly a hypothetical case; (23) is like the first term of (13) or (15), so presumably the sort of body which would produce this wave pattern would be the bow of a ship of great draught, but without any sides or stern. However, without inquiring any further into that, if the wave pattern is (23), then from (22) the corresponding wave resistance would be

$$\mathrm{R} = \pi\, \rho\, c^2 h^2 \int_0^{\frac{\pi}{2}} \cos^3\theta\, d\theta = \tfrac{2}{3}\, \pi\, \rho\, c^2 h^2 \quad . \quad . \quad . \quad . \quad . \quad (24)$$

* *Proc. Roy. Soc.*, A, Vol. 144, p. 514, 1934.

We might even carry this calculation a step further and divide the integration into two parts: (i) from $\theta = 0$ to $\theta = 35°\ 16'$, (ii) from $\theta = 35°\ 16'$ to $\theta = 90°$; and we might associate the first part of R so calculated with the transverse waves of the pattern, and the second part with the diverging waves. On that basis we easily find that for (23) the transverse waves account for about 77 per cent. of the wave resistance, and the diverging waves for the remaining 23 per cent.

The formula for R given in (22) was for a sine pattern (19), but the same expression holds for a similar cosine pattern. For instance, to compare with (23) we may take the case

$$\zeta = h \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \cos\theta \cos(x, y)\, d\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24a)$$

which is like a term of (13) or (15) giving the effect of the curved sides of the model. For this pattern the corresponding wave resistance is

$$\mathrm{R} = \pi \rho c^2 h^2 \int_0^{\frac{\pi}{2}} \cos^5\theta\, d\theta = \tfrac{8}{15} \pi \rho c^2 h^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

If we make a similar division into transverse waves and diverging waves we find that the former now account for a greater proportion of the total resistance, about 86 per cent. However, this is, no doubt, carrying the dissection too far; the wave pattern as a whole should be treated as a single system.

13. As an example of (22) we may consider the model with parabolic lines for which the wave pattern was given in the expression (12). We have at once the wave resistance given by

$$\mathrm{R} = \frac{64\, b^2 \rho c^2}{\pi \kappa^4 l^4} \int_0^{\frac{\pi}{2}} \{\kappa l \cos(\kappa l \sec\theta) - \cos\theta \sin(\kappa l \sec\theta)\}^2 \cos^3\theta\, d\theta \quad . \quad (26)$$

On expanding this expression we have

$$\mathrm{R} = \frac{32\, b^2 \rho c^2}{\pi \kappa^4 l^4} \int_0^{\frac{\pi}{2}} \{\kappa^2 l^2 + \cos^2\theta + \kappa^2 l^2 \cos(2\kappa l \sec\theta)$$
$$- 2\kappa l \cos\theta \sin(2\kappa l \sec\theta) - \cos^2\theta \cos(2\kappa l \sec\theta)\} \cos^3\theta\, d\theta \quad (27)$$

And this leads to

$$\mathrm{R} = \frac{32 \rho b^2 c^2}{\pi} \left[\frac{2}{3}\left(\frac{c^2}{g l}\right)^2 + \frac{8}{15}\left(\frac{c^2}{g l}\right)^4 + \left(\frac{c^2}{g l}\right)^2 \int_0^{\frac{\pi}{2}} \cos^3\theta \cos(2\kappa l \sec\theta)\, d\theta \right.$$
$$\left. - 2\left(\frac{c^2}{g l}\right)^3 \int_0^{\frac{\pi}{2}} \cos^4\theta \sin(2\kappa l \sec\theta)\, d\theta - \left(\frac{c^2}{g l}\right)^4 \int_0^{\frac{\pi}{2}} \cos^5\theta \cos(2\kappa l \sec\theta)\, d\theta \right] \quad (28)$$

The result has been put into this form for direct comparison with the expression for the waves given in (13), where they are analysed into four simple patterns, one for the bow, one for the stern, and two for the combined effects of the curved sides of the model. From this, and the calculations of the previous section, we can now identify the origin of each of the terms in the expression (28). The first term is the resistance due to the bow and stern patterns as if each existed alone, while the second term is similarly due to the curved sides calculated separately. The last three terms of (28) have been left in the form of integrals; these integrals have been tabulated for numerical work, but we are only considering here some general inferences. These three terms represent the mutual interference of the four simple patterns contained in (13), and it is obvious from the power of the factor $(c^2/g\,l)$ whence they arise. The first of these represents the interference of bow and stern patterns, the second the interference of bow or stern with entrance or run, and the last term the mutual interference of the two patterns from the curved sides or, as one may say, the interference of entrance and run. It is these last three terms in (28) which have oscillating values, and so give rise to the well-known humps and hollows on the curve of wave resistance.

14. We have seen that the wave pattern left behind by a ship can in general be put into the form given in (9); we have described this as sine and cosine patterns with known amplitude factors. The calculation of the quantity E − W can readily be extended to this general form and we obtain then the wave resistance for any general case.

We first put (9) into the equivalent form

$$\zeta = \int_0^{\frac{\pi}{2}} (F_1 \sin A \cos B + F_2 \cos A \sin B + F_3 \cos A \cos B + F_4 \sin A \sin B)\, d\theta \quad (29)$$

where $A = \kappa\, x \sec\theta$, $B = \kappa\, y \sin\theta \sec^2\theta$, and the F's are functions of θ, in general, and of the form of the ship and its speed. The calculation of R follows as in the simpler case of (19), and leads to the general result

$$R = \tfrac{1}{4}\,\pi\,\rho\,c^2 \int_0^{\frac{\pi}{2}} (F_1^2 + F_2^2 + F_3^2 + F_4^2) \cos^3\theta\, d\theta \quad . \quad . \quad . \quad (30)$$

The determination of the functions F is, of course, another matter. Approximate methods in use at present amount to replacing the ship by some equivalent distribution of sources and sinks; the functions F then usually appear as integrals taken over the surface of the ship, or over its longitudinal section for a first approximation. One of the outstanding problems of ship wave resistance is the improvement of methods for determining these functions; the line of attack open at present would seem to be by further steps of mathematical and numerical approximation, assisted and corrected by comparison with experimental results.

The object of the present paper was to recall some of the elementary properties of wave patterns and their production by the mutual interference of simple plane waves, to illustrate these by examples from ship models, and further to emphasize the direct connection between the wave pattern and the wave resistance.

DISCUSSION.

The Chairman: (Sir Westcott Abell, K.B.E., M.Eng., Vice-President): My colleague, Dr. Havelock, is an authority on the mathematical analysis of wave phenomena. He has given us a general history of the problem, and has put it into a form which appeals to the ordinary run of naval architects.

Dr. Havelock has been working for quite a number of years now on the analysis of the various component parts of ship resistance. Many people have worked on this problem. I do not want to throw cold water on anybody's enthusiasm, but having had something to do with ship calculations in various forms I have found that the evaluation of a function which Dr. Havelock suggests is not quite an easy matter. But, quite apart from the actual numerical evaluation, I think we are learning a lot by the analysis of the various components. We are learning more and more about the question of interference, and to some extent these fundamental researches guide us in avoiding the effects of interference. After all, we must get a certain resistance, whatever we do; but we can choose certain components in such a way that we get a minimum rather than a maximum resistance.

Professor E. G. Barrillon: Being a great admirer of Professor Havelock's work, I do not wish to criticize his paper, but on the contrary I would say that formula (22) on page 440 seems to me to be a beautiful discovery.

The different questions on which I should be glad to obtain some further details are the following:—

(A) Is it possible to extend his method of the wave pattern to the case of a focus moving on a circular trajectory. Did Professor Havelock study that case?

(B) With the notations of Fig. 1 (page 432), the equation of a crest line is

$$\Delta / x \cos \theta + y \sin \theta = \frac{\pi}{4\,\kappa} (1 - \cos 2\,\theta)$$

whence by derivation

$$\Delta' / - x \sin \theta + y \cos \theta = \frac{\pi}{2\,\kappa} \sin 2\,\theta$$

the length of the part of Δ' contained between the axes is a constant; this property gives the geometrical definition of the Kelvin wave crests as developed curves of a four-cusped hypocycloid.

By further derivation we obtain

$$\Delta'' / - 2 \cos \theta - y \sin \theta = \frac{\pi}{\kappa} \cos 2\,\theta$$

Comparing Δ and Δ'' we find the radius of curvature

$$|r| = 6 \sin^2 \theta - 2$$

and remembering that the height at any point of the Kelvin crest line is

$$h = \frac{\sec^3 . \theta}{\sqrt{1 - 3 \sin^2 \theta}}$$

it is found that

$$h = \frac{1}{\sqrt{r \,.\, \cos^3 \theta}}$$

This form seems to agree with the very simple and elegant form of equation (22), and must conduce to a physical explanation of this formula.

(C) Turning back to Fig. 2 on page 432 it would be very desirable to hear from Professor Havelock something about the physical reasons leading to the retention in the practical system of waves of only one-half of the total system obtained. In resistance calculations we only need the form of the field at infinity, but the very simple experience of Kelvin's fishing-line shows that in the vicinity of the origin the wave pattern given by theory gives a good idea of the experimental pattern, but that only one half of the theoretical pattern should be retained.

(D) I have had no time to see if formula (9) is sufficient for the most general case, and should be interested to hear from Professor Havelock what general application he gives to this formula.

(E) Regarding function F, containing the key of the wave resistance, I would like to mention the two-dimensional case of the obstacle, where the only condition of compatibility of the form of the wave and of the form of the obstacle at one end is sufficient to give the value of the resistance.

Mr. W. C. S. Wigley, M.A. (Associate): Professor Havelock has given a very lucid description of the physical basis of the methods employed in wave-making calculations. In fact, it is so lucid that I am a little afraid that if a student gets hold of it he will think he knows all about the calculation of wave resistance because he has understood the paper; but he will not; he will know all about the theory of it, but the difficulty in practice is the determination of the functions F, etc., which Professor Havelock mentions in his last equation. That difficulty has been overcome only for two limiting cases of floating bodies. We still do not know how to find those functions F, etc., for any form whose breadth and draught are neither of them negligible. Most of the calculations which have been made for ship-shape forms apply to fine forms, the waterlines of which do not make a great angle with the direction of motion. Fair agreement in this limiting case can be obtained between calculation and measurement.

It is also possible to approximate to the case of a skimming boat whose draught is negligible. Attempts have been made by Dr. Weinblum, in Berlin, to find agreement between experiment and theory for this case; but he found generally that the theoretical resistance was some 15 to 20 per cent. too low. Explanations of this have been put forward on the basis of some additional resistance in the case of skimming boats. That, however, is hardly part of the subject.

There is one point about which I do not want anybody to be misled, viz. the model of mine to which Professor Havelock has referred. The actual model that I described in my paper had a length of parallel body which caused two further wave systems to appear. They are wave systems caused by the curvature of the form; and in fact, the system which appears where the bow curvature ends and the parallel body begins is exactly the same as the system which is caused by the curvature of the stern. The system commencing at the forward end of the stem curvature is the reverse of that, so that those two cancel out when you take out the parallel body. I only mention this because it might lead to some confusion if anybody looks at my results together with those of Professor Havelock.

The method of calculating the wave resistance from the energy has, as Professor Havelock remarks, not been used. It is difficult to know why. The method has been developed this year by Professor Havelock, not only here, but in a paper which, as you probably know, he published recently in the *Proceedings of the Royal Society*.

As you will see from this paper, the method provides an excellent way of picturing the physical phenomena of the motion. It will enable us to link the wave profile aft of the body with the resistance. I think it is going to be useful in that way in practice; but I do not think it is going to help very much as far as any calculation in connection with ships is concerned; that is, the numerical calculation apart from theoretical investigation. It has explained one phenomenon which has rather interested most mathematicians engaged on this work. It was found that, whatever formula you used for expressing wave resistance, you always came down to an integral which resembled somewhat in form Professor Havelock's equation (30). It generally contains only two of the F functions. I think I am correct in saying that, in Professor Havelock's equation, in a symmetrical body two of the four functions would disappear. We did not really know the reason for this form of the resistance equation. The formula which has been derived by Dr. Hogner in Sweden in gliding bodies was expressed in that way. Michell's formula for the more ship-shape forms was expressed in that way; the results which Professor Havelock has himself obtained for deeply submerged bodies could also be expressed in the same way. We thought there must be some connection there; we did not know what it was until Professor Havelock produced this energy connection with the wave profile, which, of course, tells us that in every case, whether we are approximating or not, the final result for the resistance will be an integral of this sort.

The CHAIRMAN: I think we should like to weigh up Mr. Barrillon's remarks. Really we do see somewhat alike, and I think we shall advance in our knowledge of the wave phenomena as the result of these discussions. On the other hand, while I much appreciate the possibility of dealing with it by the energy method as Dr. Havelock suggests, one might visualize a photograph being taken which would give us the resistance,—yet I rather fancy it will not be quite so simple to express the form variation of the ship in this way. Those who run experiment Tanks will know better than I do how a very small change indeed in a model may appreciably affect the resistance. If we cannot express it on paper with some degree of accuracy, I do not think we can reduce the question of resistance to one of mathematical calculation. But as the analysis goes on we get a better mental picture of the phenomena. As long as we know the fundamental points, we can make progress with our experiments, and I think the best result, or at any rate the best hydrodynamical result, is always obtained finally by experiment.

Professor T. H. HAVELOCK, F.R.S.: I found Professor Barrillon's remarks extremely interesting. The geometrical construction he gave for the Kelvin pattern confirms the remark I made about the fascination of the subject, in spite of our familiarity now with the details of that pattern. In regard to the formula (22) for the resistance, that result was obtained by direct straightforward calculation. As in other similar cases, when a result so obtained has a specially simple form, one might expect to obtain it more simply by some elementary physical considerations. It is quite likely that the suggestion made by Professor Barrillon may lead to some such explanation of the form of the result. I am very much obliged to the speakers and to you, Sir, for your kind reception of the paper.

The CHAIRMAN (Sir Westcott Abell, K.B.E., M.Eng., Vice-President): I think we owe a very great debt of gratitude to Dr. Havelock and to the various speakers for their contributions. I will ask you to accord a vote of thanks to Professor Havelock in the usual manner.

WRITTEN CONTRIBUTIONS TO THE DISCUSSION.

Mr. M. P. PAYNE (Member): This is beyond any doubt a valuable contribution to our Transactions. Recent years have seen a steady advance in the mathematical theory of ship resistance, and an accumulation of data based on comparisons of theoretical and experimental results.

A certain knowledge of hydrodynamics is now fairly widespread among naval architects, but the steps from this to the extremely complicated problem of ship wave resistance have, in general, only been intelligible to professional mathematicians. Without overburdening us with too much detail, the present paper gives a lucid exposition of the line of reasoning followed. In particular, the manner in which the terms of the functions are divided to show the relative effect on wave patterns of the shape and endings of ships and the broad effects of beam and draught, and hence their effect on wave-making resistance, is most instructive.

To the naval architect and student alike, it is a most welcome contribution, and is a worthy summary of the author's original work on this intricate subject. It is to be hoped that Professor Havelock will, on some future occasion, read us another paper on his further researches.

Professor T. H. HAVELOCK, F.R.S.: I add some further remarks in reply to Professor Barrillon. The double pattern of Fig. 2 (page 432) represents a free, unforced wave system. A possible theoretical solution for the forced waves produced by a ship in a frictionless liquid is one with a wave pattern in advance of the ship as well as to the rear. On this solution we may superpose a freely moving double pattern in such a way as to cancel out all the wave motion at a distance in advance of the ship, leaving waves only in the rear; the combined result is the usual practical solution. A mathematical device for obtaining the practical solution directly from the equations is to introduce into them, after Rayleigh, a simple kind of fluid friction which is ultimately made vanishingly small. It is usually stated that the theoretical solution with waves in advance is an unstable state of motion, while that with waves only to the rear is stable; the function of the fluid friction is to ensure that the solution obtained is the stable motion which would be ultimately established in an actual liquid. I do not think that this statement is strictly correct. I do not know that the point has been examined definitely, but I think that the solution for a frictionless liquid, with waves in advance as well as to the rear, would be found to be a stable state, provided we are allowed to assume the motion given in existence at some instant throughout the whole infinite liquid. The simplest physical explanation of the practical solution, with waves only in the rear, lies rather in the fact that the ship begins moving at some instant and moves forward in water previously at rest; if we picture the ship as sending out waves in all directions, it is found that these only combine together to form a wave pattern to the rear of the ship.

In regard to the expression (9), I consider this to be quite general, in view of the fact that it includes two arbitrary functions. To illustrate this, suppose we had a number of simple patterns with different focus points; for any one, with its focus at the point (x_r, y_r), we should have formula (9) with functions f_r and F_r and with $x - x_r$ and $y - y_r$ instead of x and y respectively. Writing down the sum of such expressions, expanding and rearranging the terms, the combined result could again be put into the general form (9).

Mr. Wigley's paper referred to in Section 7 was published in the *Proceedings of the Royal Society* (A, Vol. 144, p. 144); it was a comparison of theory and experiment for a model with parabolic lines and with parallel middle body. I am indebted to Mr. Payne for his appreciation of the main object of the paper, to provide a broad outline of certain aspects of theory.

TRIALS OF THE TRAINING SHIP *CRISTOFORO COLOMBO* WITH TWO CO-AXIAL CONTRARY-TURNING SCREWS.

By Colonel F. Rotundi, del Genio Navale, R.I.N.

[Read at the Summer Meetings of the Seventy-fifth Session of the Institution of Naval Architects, July 12, 1934, Sir Westcott Abell, K.B.E., M.Eng., Vice-President, in the Chair.]

With the consent of the Italian Ministry of Marine I have the honour to place before the Institution of Naval Architects the results obtained with the auxiliary propulsion machinery of the *Cristoforo Colombo*, driving two co-axial screws which revolve in contrary directions.

1. These two adjustable two-bladed propellers are fitted to two concentric shafts, each driven by an electric motor. Plate XLVI. shows the ship in dry dock, and from this the arrangement of the propellers can be seen. The main characteristic features of the propellers are:—

	Screw Turning Clockwise.	Screw Turning Counter-clockwise.
Diameter	2·90 m. = 9 ft. 6 in.	2·90 m. = 9 ft. 6 in.
Uniform pitch	2·90 m. = 9 ft. 6 in.	2·90 m. = 9 ft. 6 in.
Ratio of pitch to diameter ..	1	1
Number of blades	2	2
Diameter of boss	0·82 m. = 2 ft. $8\frac{1}{4}$ in.	0·66 m. = 2 ft. 2 in.
Total projected area	1·0150 sq. m. = 10·77 sq. ft.	1·0600 sq. m. = 10·82 sq. ft.
Total fractional pitch	0·1667	0·1692

2. The fitting to the *Cristoforo Colombo* of co-axial contrary-turning screws follows the researches and experiments carried out by the Royal Italian Navy under the auspices of that distinguished naval architect, General Giuseppe Rota, one of the most influential and convinced advocates of the double propeller.

3. I may be permitted to give at this stage a retrospect on this important question, and to refer to papers by General Rota.

The first application of co-axial contrary-turning screws goes back to 1839, when Ericsson fitted them to the *Robert F. Stockton*. Since then they have been resorted to in isolated instances for small craft, and their use has become the general rule for torpedoes. In every case, the rotation of the propellers in opposite directions is obtained by gearing or some other intermediate device coupled to a single engine, which has resulted in a loss of efficiency. Professor A. G. Greenhill * dealt with the problem in an interesting paper

* "A Theory of the Screw Propeller," by Professor A. G. Greenhill, Trans. I.N.A., 1888.

which forms a supplement to his theory on propulsion by propellers. Mr. W. J. Luke * carried out in the Tank at Clydebank a number of systematic experiments with models of screw propellers and hulls, which were dealt with in his paper read in 1914. General Rota † made two series on systematic experiments, one with a large steam launch belonging to the Castellamare di Stabia Royal Shipyard, the other with models of propellers in the Tank of the La Spezia Royal Arsenal. The experiments are recorded in his two papers read in 1909 ‡ and in 1922.‡

In connection with the results recorded in his two papers, and on taking up the position of Chairman of the Committee on New Construction—the *Cristoforo Colombo* having been entrusted to me—General Rota pressed forward the experiments with this system of propulsion, having obtained the consent of the Secretary of State for the Navy to the carrying out of tests on a large scale, which were destined to be of great interest in regard to possibility of improving the propulsive efficiency of ship machinery.

4. The principal dimensions of the *Cristoforo Colombo* are:—

Length between perpendiculars	66·50 m. = 218 ft.
Length overall	78·30 m. = 257 ft.
Max. breadth outside framing	14·80 m. = 48 ft. 6 in.
Mean draught	6·45 m. = 21 ft. 2 in.
Height	10·85 m. = 35 ft. 6 in.
Displacement	3,500 tons.

The ship has been built for sail navigation. In form and dimensions it fulfils the normal requirements set down for those sailing units of the Italian Navy which are renowned for their seaworthiness and manœuvring qualities. It is similar also in regard to stability, a particularly important feature in the case of the *Colombo*, seeing that it has three decks for accommodation on board.

The auxiliary propelling machinery consists of two Tosi Diesel engine driven electric generating sets, the engines being coupled to two direct-current dynamos.

The two internal-combustion engines are four-stroke cycle, single acting, non-reversing, each having a normal output of 1,150 S.H.P. at 330 revs. per min. The two dynamos are of the Marelli type, each of 640 kw., 230 volts, with exciting current at 110 volts.

The two Marelli electric motors are double armature machines, supplied at 220 volts, with a 110-volt exciting current, each developing 2 × 400 S.H.P. at 120 revs., corresponding to the running at half power of the screws. Each motor is connected to a dynamo by a Ward-Leonard coupling.

The shafting is dealt with in the Appendix.

5. The *Cristoforo Colombo* has carried out, since July 1928, numerous cruises for the instruction of the Naval Academy Cadets, navigation under sail alternating with mechanical propulsion. The problem of co-axial shafts rotating in opposite directions may be considered to have been solved satisfactorily, as there only remain for further consideration improvements in a few points of detail and the simplification of some items.

6. During the summer of 1930 comparative tests were carried out on the Punta Chiappa–Portofino measured mile, both with the co-axial screws turning in contrary directions, and with propulsion by a single four-bladed screw connected to the two shafts after these had been locked together to revolve in one direction only.

The characteristic features of the two co-axial screws are those above referred to. The

* "Further Experiments upon Wake, and Thrust Deduction," by W. J. Luke, Trans. I.N.A., 1914.

† "The Propulsion of Ships by Means of Contrary-turning Screws on a Common Axis," by G. Rota, Trans. I.N.A., 1909.

‡ "Further Experiments on Contrary-turning Co-axial Screw Propellers," by G. Rota, Trans. I.N.A., 1922.

single four-bladed screw was made by substituting for one of the two screws another of the same characteristics but of reversed pitch. The two blade couples, fitted crosswise on the locked shafts revolving as one, are therefore not in the same transverse plane perpendicular to the axis of rotation.

7. The speed trials of the ship with the co-axial screws revolving in opposite directions were made on June 6 and 7, 1930, on a smooth sea and with no wind; the ship had left the dock with a clean hull on June 3rd.

The trials were made at a mean displacement of 3,240 tons. The results were as follow:—

Speed, Knots.	Average Speed of Screw, Revs.	Total Power, S.H.P.	P.C.E. (naked hull).	$E = \frac{\text{P.C.E. (naked)}}{\text{S.H.P.}}$
10·02	131·2	925	449	0·485
11·12	149·3	1,381	678	0·491

8. The trials with the ship having a single four-bladed screw were run on June 27 and 28, 1930; the ship had left the dock with a clean hull on June 20th. During the trials there was some swell and a breeze; the trials in this instance were therefore considered as preliminary, since their results could not be made comparable with those obtained when the two screws revolved in contrary directions, when, as stated, there was no wind and the sea was smooth.

The second series of trials were therefore repeated on July 6th.

The displacement in this instance was 3,526 tons, therefore slightly above (by about 9 per cent.) that at which the trials with two screws turning in opposite directions were made, on June 6 and 7, 1930. This was owing to the fact that the ship had soon to start on a summer cruise, and had to be put rapidly in trim for that purpose.

Speed, Knots.	Average Speed of Screw, Revs.	Total Power, S.H.P.	P.C.E. (naked hull).	$E = \frac{\text{P.C.E. (naked)}}{\text{S.H.P.}}$
10·13	150·9	1,199	491	0·410

9. From the results obtained in the trials at sea on June 6 and 7, the ship having the two propellers revolving in opposite directions, and in those of July 6 with a single four-bladed makeshift propeller, it can be seen that the gain in propulsive efficiency with contrary-turning screws compared with the one screw is about 18 per cent. in the case of the *Colombo.*

10. *Supplementary Tank Tests.*—There remained to elucidate the point in regard to the influence of the method followed in the building up of the four-bladed screw.

For this purpose numerous tests on a large scale were made in the La Spezia Tank with the two systems of propulsion.

The auto-propulsion tests showed that the efficiency of the four-bladed makeshift screw was practically the same as that of an actual four-bladed one.

The whole of the Tank tests made with the model of the *Colombo* confirmed the greater efficiency of propulsion realized by two contrary-turning screws, the difference in their favour being about 20 per cent., a figure approximately the same as that ascertained by the speed trials at sea.

11. The results obtained with the *Cristoforo Colombo* are therefore worthy of attention. The testing of screws different in many respects from those already experimented with deserves to be enquired into very thoroughly, as also does the testing of the action of one screw, the other being immobilized. It is not possible to count upon the *Cristoforo Colombo* for this purpose, since she has to ensure her regular service; with the concurrence of General Rota, these further researches are to be made in the National Tank installation at Rome, which is under his supervision, the tests being extended to different classes of hulls.

APPENDIX CONCERNING THE SHAFTING.

The electric propulsion motors are marked (2) and (1) in Plate XLV. They are coupled respectively to the outer shaft (3), and to the inner one (4).

The stern tube is fitted with two steel bearings (12) lined with white metal, in which rests the outer screw shaft; to this, the last length of the outer shaft is connected by means of a special coupling (18).

The outer line of shafting is in a few lengths, and these are connected together by sleeve couplings made in two halves. The first length forward carries the armature of motor (2) and the thrust disc (5).

The connection aft of the thrust device carries the toothed ring (25) of the turning gear and the friction disc (27) for the brake. To the forward end of the driving shaft length is fitted a flanged sleeve on which is a movable ring in two parts (32) for locking the two shafts together. This comes into action when required to make comparative trials of propulsive efficiency between the contrary-turning screws and the single one. When the shafts are locked together, the aft screw is replaced by a similar one having a reversed pitch and mounted at an angle of 90°, or of 180° with reference to the forward one.

The outer tail shaft is provided with two inside bearings (13) for supporting the inner tail shaft (4). This latter is in four hollow lengths connected together by bolted flange couplings.

On the forward length are keyed the armature of motor (1) and the thrust disc (6).

At the forward end are the rings (26) and (28) for the turning gear and the inner shaft brake. The first length rests on bearings of the usual type, the other parts of the inner shaft being supported on three internal adjustable bearings, each in three parts. These are made easy of access from the outside, from the outer shaft couplings.

Lubrication of the inside bearings is in two forms; those aft of coupling (18) are lubricated by water circulation, whilst those forward are under forced lubrication.

With a view to separating entirely the two methods of lubrication from one another, two internal glands (16) and (17) are provided, having a free space between them and adjustable from the outside through ports in coupling (18).

The oil which flows through the hollow in the inner shaft is introduced at the forward end of the shaft (33) and carried to the other end, whence it flows into the space existing between the two shafts through ports cut in the inner shaft.

Oil is forced under pressure through all the inside bearings until it finds a free outlet at the forward end of the outer shaft, where it is caught and delivered to the oil tank (34).

The lubrication of the tail shaft bearings is effected by water circulation, the water entering under pressure at points (15) and (14).

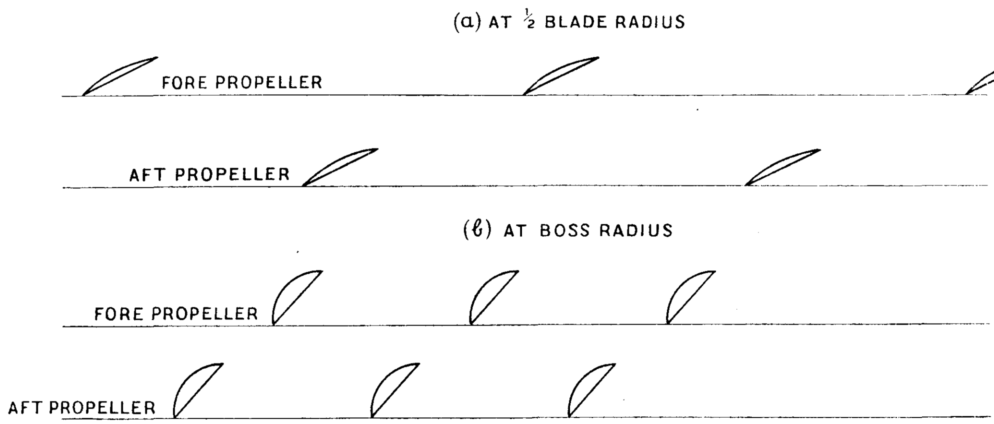
The two tail shafts are of steel containing 3 per cent. of nickel, and are provided with brass linings of a length corresponding to the bearing surface. All the bearings are lined with white metal.

DISCUSSION.

Mr. G. S. Baker, O.B.E. (Member of Council): Colonel Rotundi's paper is a statement of results obtained, and must be accepted as giving facts for the conditions under which the tests were made. They show that with contrary-turning screws a gain of 18 per cent.

in efficiency can be obtained compared with a single four-bladed screw—*under these conditions.* The author states that the results are "worthy of attention." Any attention given to the subject must be of a critical character to see whether the conditions were sufficiently general for the results to have any general application, and I propose to speak on the paper in that way.

First of all, one has to see whether those conditions were at all in line with standard modern practice with cargo vessels or intermediate liners. The vessel chosen for the experiments is an exceedingly broad and rather deep model. If you work out the beam for a length of 400 ft., it gives you 89 ft., which is considerably out of proportion for the usual type of vessel. The actual length of run is not given in the paper; but the length required to avoid serious eddy-making on this hull would be a minimum of 130 ft., and I do not think this ship could have had more than about 110 ft. The result would be, therefore, that all over the stern of the boat and around the propeller there must have been consider-



Cristoforo Colombo.

With tandem propellers each of two blades set at 90° to each other; rear propeller set back 0·211 diameter from fore one; blade gaps at two radii.

able eddy-making, a condition which removes it quite considerably from those existing on a modern boat.

Secondly, this condition was rather aggravated by the vessel being out of dock two and a half weeks when the four-bladed screw was tried, against half a week with the contrary-turning screws. That would make the eddy-making at the stern much worse, and would account for 3 or 4 per cent. in the difference in propulsive efficiency.

But the most difficult part of the paper is the sentence on page 449, "the four-bladed makeshift screw was as efficient as an actual four-bladed one." For such a vessel as this, with a good four-bladed screw of a modern type with a fin behind it, and care taken to keep the screw as far aft as possible, we should expect an efficiency between 0·5 and 0·6, against the 0·41 obtained here, and after quite careful examination of the conditions given in the paper I am fairly certain that we could obtain it. In this lies the whole crux of the paper.

The author may say that if you change the screw in that way you are altering the conditions under which the test was made; but a designer who is balancing in his mind between a contrary-turning screw and a four-bladed screw could and should regard each condition with the most favourable stern for the particular screw he is going to try.

As to the experiments themselves, however, there is one criticism which I put forward in order to make a suggestion. The rear propeller is set back a considerable distance relatively to the forward one. In the figure I have shown, for the four-bladed combination, the blade sections at two radii, on a fixed radius development. It will be seen that the blades of the rear propeller are working almost in the "wash" of the blades in front—a condition which cannot make for good efficiency. I should imagine it would not be difficult to fix the aft propeller at different angular positions and select one which would separate the blades and bring them into freer working conditions, and then carry out a new trial.

General G. ROTA, R.I.N. (Member): I wish first to thank Colonel Rotundi for the kind way in which he dealt with the part I took in applying to the *Cristoforo Colombo* the system of co-axial contrary-turning screw propellers.

Perhaps I may add a few words on the history of that application. When the programme for the construction of the training ship was in preparation, a Diesel electric engine was proposed for a single electric motor for screw propulsion, as an instructive example for the cadets. I thought it would be useful to take advantage of the occasion for an application of the system of propulsion by two co-axial contrary-turning screws, as I was a convinced advocate of that method with its consequent advantages. I contemplated having two blades for each screw, with the possibility of disposing both in a vertical position so as to have a minimum resistance when the ship was under sail.

The proposal was accepted, and great care was taken to ensure good results, especially in regard to the mechanical difficulty which would arise in the use of the two contrary-turning shafts. The results obtained were very satisfactory, absolutely no inconvenience arising from this point of view during the trials and during the ordinary service of the ship. The gain in efficiency with the disposition of screws adopted, as compared with the ordinary single screw, is evident and is a confirmation of my expectations. The case now described is neither a model experiment nor a large-scale experiment, but a real application of the system to ordinary continuous service. In my opinion we have all the elements necessary for the application of the system to a cargo boat, for example, with possible economy. We have had in Italy similar success with this general system of disposition of screws on a flying boat, which made it possible for the well-known aviator Agello to attain a prodigious speed in the air at Lake Maggiore, which is a proof of the foresight shown by Sir A. G. Greenhill in 1909,* when discussing the paper I presented to the Institution of Naval Architects explaining the first experiment I made.

Dr. E. V. TELFER (Member): The only point I wish to make is to ask the author if he can tell us precisely which constituent of the hull efficiency has had the most marked influence. Going over some of General Rota's previous work on contrary-turning screws, whilst their effectiveness is not very great on twin-screw ships yet they seem to give an entirely different result for single-screw ships. The principal result of the contrary turning shows half the thrust reduction. If that particular point were to be followed up, then designers would have the advantage of fairly high efficiencies at the present moment from the single screw and would be attracted to a means of halving the inevitable thrust reduction. That is to say, we now have a thrust reduction of 21 per cent. That is our usual minimum. If these experiments suggest that we can get down to 10 per cent. reduction, then, apart from the criticism which Mr. Baker made, designers would be forced to realize that we had a new mechanism at our disposal which is not available with an ordinary screw propeller.

The CHAIRMAN (Sir Westcott Abell, K.B.E., M.Eng., Vice-President): While we are given a certain amount of information in regard to the performance of the twin propellers in this case, yet I think we must all realize that there are many factors, some of which have been pointed out, whereby we could not directly apply these results to another ship. I notice that Colonel Rotundi said in his paper that they are exploring other developments

* Trans. I.N.A., Vol. LI., p. 84.

in the Tank, the *Cristoforo Colombo* not being available. It would be of interest to put into the Transactions as many of these results as are available before the volume is completed, and we should be very grateful for any further data that may be sent in by the author.

It only remains for me to thank Colonel Rotundi and the Italian authorities for being so good as to give us this information.

WRITTEN CONTRIBUTIONS TO THE DISCUSSION.

Mr. M. P. PAYNE (Member): The excellent work of Colonel Rotundi's talented countryman, General Rota, on this subject is well known to members of our Institution, as it forms an important contribution to our Transactions. It is a matter of great satisfaction that the author, in following up the previous work, has laid the results before our Institution, and our best thanks are due to him for this. In the paper the many mechanical problems which had to be faced in designing the machinery installation are not referred to in detail, but the manner in which they have been overcome on so large a scale cannot but excite our admiration. This, however, is the province of the engineer; speaking from the point of view of the naval architect, it would be of great interest if we could be informed of the weight of the concentric shaft arrangement as compared with a straight drive of the same revolutions and power.

There is a point relating to the machinery of the ship concerning which it would be appreciated if an opinion could be given. In a single-screw ship the variation in wake with depth below the surface causes a variation in the pressure on the propeller during a revolution, and this in turn produces a steering tendency in a direction depending upon the direction of revolution of the propeller. Could the author please say how the manœuvring of the ship compared when fitted with the contrary-turning screws and with the single screw?

With regard to the trials of the ship, it is noted that when the four-bladed propeller was fitted, the revolutions for a given speed were greater than when the contrary-turning screws were fitted. It is possible that the efficiency of the single screw was prejudiced comparatively on this account. The photograph suggests that the screw aperture would accommodate a larger propeller, and it would be interesting to know whether the relative performance could have been improved by fitting a propeller of different dimensions.

Colonel ROTUNDI: I am very pleased that the President and members have found my paper interesting and deserving of discussion.

I regret I am unable to agree with Mr. Baker in his criticism, since this is centred on the possibility, which I had of course already taken into account, of being able to design a four-bladed propeller that would cause the propulsive efficiency of the single propeller to rise from 0·41 to a value between 0·5 and 0·6. Taking the lesser of these two figures, it would be necessary, in order to increase the efficiency, to increase perceptibly the diameter of the propeller, and this would modify the comparison between propulsion by co-axial contrary-turning propellers and propulsion by a single propeller.

According to the correct views of General Rota, such a comparison has to be based upon parity in the propeller diameters. If, therefore, the expedient for arriving at a single propeller in the manner described under heading (6) in the paper had not been resorted to, we would have had to make a propeller of the same diameter as the makeshift one, and not having the disadvantage of different planes for the blades. This disadvantage, however, was not really prejudicial to the propulsive efficiency, as is shown by the self-propulsion tests in the Spezia Tank [see Section (10) in the paper]. Further, the expedient resorted to is a convenient one, and one which simplifies matters.

After the trials at sea, four-bladed propellers of larger diameter and better proportioned than the makeshift one were tested in the Tank. These, of course, gave a better efficiency.

It would, however, not be right to compare propellers having different diameters; in order not to vary the terms of comparison the right thing to do in this case would have been to vary also the diameter of the co-axial propellers, which was not practicable. The requirements to be followed in the design, construction, and working of a ship not primarily built for the making of experiments imply—as in the present instance—sacrifices and the drawing up and setting aside of schemes: a state of things which, fortunately, does not arise in the case of technical investigations and laboratory tests. Hence the self-propulsion experiments had to be further prosecuted in the Tank, since the ship was no longer available.

The analysis of the test results already obtained, and of the results of further tests which General Rota is to carry out in the Rome Tank, will be co-ordinated with an analysis of the trials of the *Cristoforo Colombo*, as suggested by Dr. Telfer. An exhaustive and conclusive discussion between the experts will then be possible. Meanwhile, in the absence of another propeller-driven ship of normal proportions, equipped with contrary-turning propellers, it may be said that the training ship *Cristoforo Colombo*, which I designed to meet most stringent requirements as regards the hull, made it possible, under General Rota's direction, to arrive at a satisfactory solution of the mechanical problem of propulsion using contrary-turning propellers; the results obtained in the trials at sea, confirmed by Tank tests, were in agreement with the claims made long ago by General Rota, notwithstanding the limitations which surrounded the experiments.

I hope, as the President has suggested, that the results of the forthcoming experiments will be put before the Institution.

I regret not having the comparative weight elements which Mr. Payne asked for. The weight of the shafting complete with brasses, bushes, brakes, propellers, etc., was estimated at 37 tons, an ample figure if we bear in mind, in the first place, its safe and accurate working and in the second place, the possibility of a subsequent reduction in weight, should this measure be found necessary and convenient, in view of the eventual addition of other mechanical parts. With regard to the running stability of the ship, no comparative trails have been carried out with the two systems of propulsion, always owing to the difficulty of disposing of the ship for trial purposes, but none of the numerous navigation reports issued by the ship's officers are at all unfavourable.

CONCLUDING PROCEEDINGS.

The Chairman (Sir Archibald Denny, Bart., LL.D., Honorary Vice-President): Gentlemen, that completes our programme, and I consider that this Conference has been of immense value. The papers have been admirable and original, and the discussions have been crisp and good. I think that when the volume comes to be published it will be found to be of immense value, and I presume it will form the basis for further discussions among the Tank Superintendents. They have all come here together, and we have had, not only Superintendents, but critics of the Superintendents; that is an advantage.

But who has organized this Conference? We shall have no other opportunity of thanking Mr. Dana and his staff for the admirable arrangements which have been made for our mutual culture and advantage, and I move a hearty vote of thanks to him and his staff.

Mr. R. W. Dana, M.A., O.B.E. (Secretary): Sir Archibald and Gentlemen, I cannot but rise and thank you extremely heartily for your very flattering words. I can assure you it has been a great pleasure to organize this particular Summer Meeting, because it has, owing to the Conference, been a very remarkable and interesting one.

I confess, when the subject first came up a year ago, that some of us were in doubt as to whether in the height of summer, with so many other things going on, we should be able to get adequate discussions and persuade people to sacrifice beautiful mornings in

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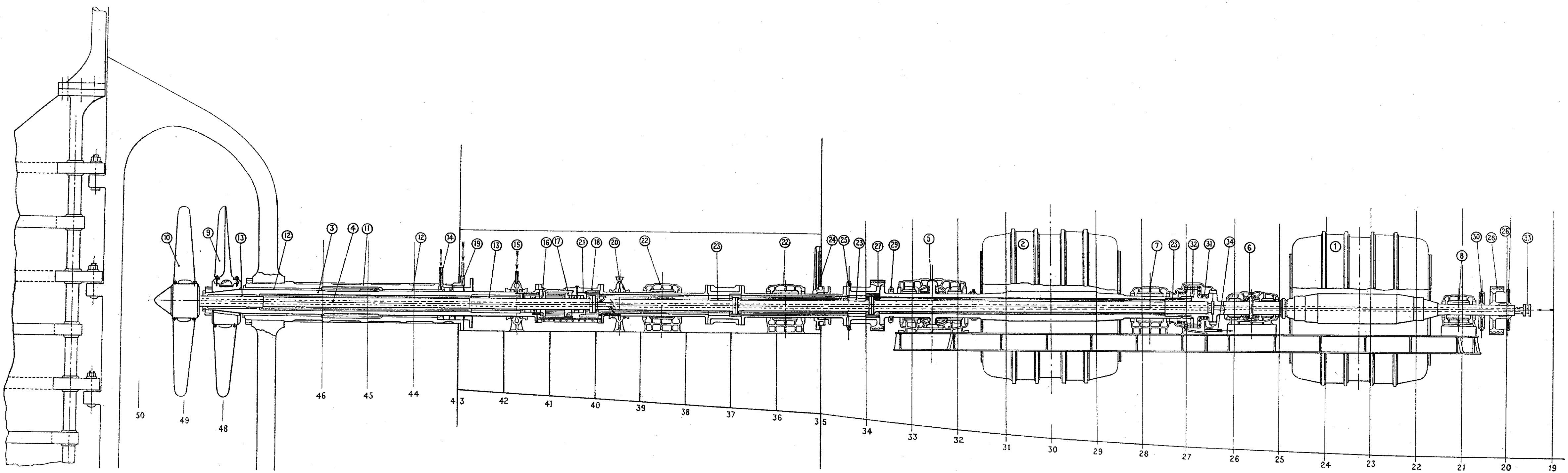
To Illustrate Colonel F. Rotundi's Paper on "Trials of the Training Ship 'Cristoforo Colombo' with Two Co-axial Contrary-turning Screws."

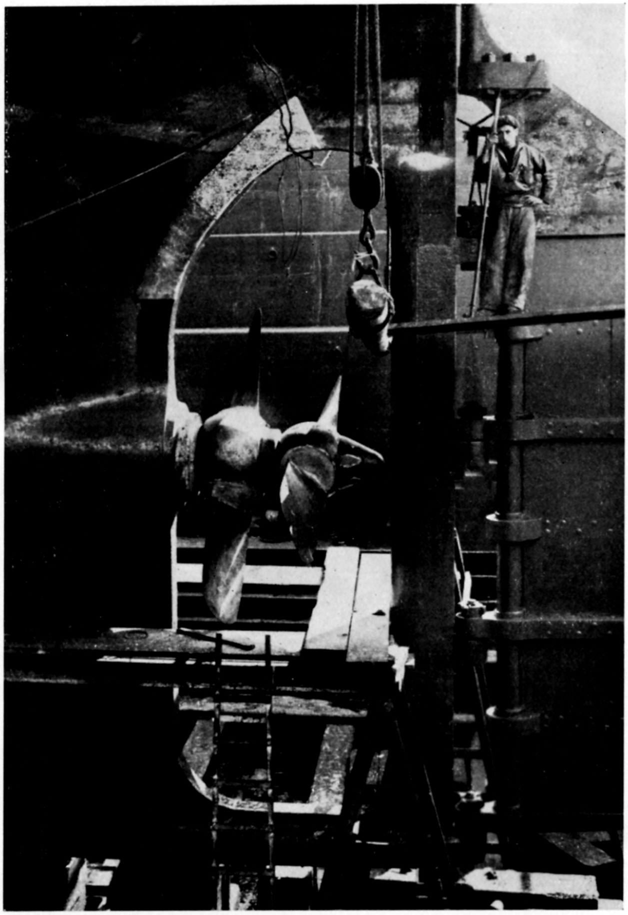


REFERENCES.

1. Electric motor driving inner shaft.
2. Electric motor driving outer shaft.
3. Outer shaft.
4. Inner shaft.
5. Bearing and thrust collar for outer shaft.
6. Bearing and thrust collar for inner shaft.
7. Bearing for armature of electric motor.
8. Bearing for armature of electric motor.
9. Propeller driven by outer shaft (clockwise).
10. Propeller driven by inner shaft (counter-clockwise).
11. Stern tube.
12. Gland bearing for outer tail shaft.
13. Gland bearing for inner tail shaft.
14. Water inlet for cooling outer shaft.
15. Water inlet for cooling inner shaft.
16. Gland preventing sea-water leakage.
17. Gland preventing lubricating oil leakage.
18. Muff coupling with opening for examination and adjustment of gland on inner shaft.
19. Oil supply to gland on outer shaft.
20. Oil supply to gland on inner shaft.
21. Oil connection for inner shaft.
22. Bearing for outer shaft.
23. Bearing for inner shaft.
24. Bulkhead gland neck ring in two pieces.
25. Turning gear wheel for outer shaft.
26. Turning gear wheel for inner shaft.
27. Brake rim for outer shaft.
28. Brake rim for inner shaft.
29. Gear wheel for circulating water pump drive for the outer shaft.
30. Gear wheel for circulating water pump drive for the inner shaft.
31. Coupling for the shafts with locking arrangement.
32. Disc in two parts for securing the shafts.
33. Lubricating oil inlet.
34. Lubricating oil outlet.

PROPELLER SHAFTS.

To Illustrate Colonel F. Rotundi's Paper on "Trials of the Training Ship 'Cristoforo Colombo' with Two Co-axial Contrary-turning Screws."



Cristoforo Colombo in Dry Dock.