into the air, allowed to drain for some time in a temperature of 32° or upwards, and then be washed in fresh water, it will be found to be nearly quite free from salt, and the water produced from it may be drunk."

During the Antarctic cruise of the *Challenger* the writer of this notice made some experiments to decide the question whether or not sea-water ice is a mixture of pure ice and sea water or brine. The *melting point* of salt-water ice of various sources was carefully observed, with the following results. Ice formed in a bucket of sea water over night melted at − 1·3° C. The bulk of ice formed was insignificant compared to the volume of water in which it was formed, so that this was a specimen of *bonâ fide* sea-water ice, without admixture of snow or spray. In the same way the melting point of pack-ice was determined. The freshly collected ice began to melt at − 1° C.; after twenty minutes the thermometer had risen to − 0·9°, and two hours and a half afterwards it stood at − 0·3°, having remained constant for about an hour at − 0·4°. Another portion of the ice rose more rapidly in temperature, and when three-fourths of the ice was melted, the thermometer stood at 0° C. In the case of the ice frozen in the bucket, the melting point remained constant for twenty minutes at − 1·3°, after which no observations were made, so that we do not know if this ice, formed under the most favourable circumstances, showed the same irregularities as the pack-ice, picked up out of the sea; but as the bulk of ice experimented on did not much exceed 10 cubic centimetres, the greater part of it must have melted in the twenty minutes. Indeed as the amount of ice formed in the bucket did not sensibly alter the composition of the water left liquid, there seems to be no reason why the ice should not be a homogeneous substance.

Adhering brine can have no influence on the melting point of ice, consequently, if sea-water ice consists of pure ice with entangled brine, it must melt at 0° C. If its melting point is different from 0° C. then the solid matter of the ice is not pure ice. We have seen that frozen sea water has a melting point of − 1°·3, which is fairly constant, and that pack-ice, which must necessarily be formed by the freezing of salt water, the congealing of spray, and the accumulation of snow, begins to melt about − 1°, the temperature gradually rising as the constituents of lower melting point are liquefied. It is thus readily apparent how it is that Scoresby found that such ice "allowed to drain for some time in a temperature of 32° or upwards," produced in the end potable water. The salt-water ice of low melting point effectually prevents the intermingled snow from melting, which finally remains practically intact, and of course can be drunk on being melted.

Dr. Petterson on purely chemical grounds comes to the same conclusion. He says (p. 303): "Those who support the common theory that sea ice is in itself wholly destitute of salt, and only mechanically incloses a certain quantity of unfrozen and concentrated sea water, must confess that we in this case ought to find by chemical analysis exactly the same proportion between Cl, MgO, CaO, SO_3, &c, in the ice and in the brine as in the sea water itself." That this is not the case is shown by a number of analyses of sea-water ices in which the proportion of Cl : SO_3 varied from 100 : 12·8 to 100 : 76·6, the average proportion in sea water being 100 : 11·88. The results of his investigations may be summarised as follows:—

Ocean water is divided by freezing into two saliniferous parts, one liquid and one solid, which are of different chemical compositions. Taking the relation Cl : SO_3 as standard of comparison, the most striking feature of the freezing process is that the ice is richer in sulphates, and the brine in chlorides. The extraordinary variation, both in saltness and in chemical composition of every individual specimen of sea ice and sea brine, shown by the tables, depends on a secondary process, by which the ice seems to give up its chlorides more and more, but to retain its sulphates. Hence the percentage of chlorine is no indication of the saltness of the ice, though it may to a certain extent be taken as an index of its age.

In connection with this part of the subject, the author cites Prof. Guthrie's work on Cryohydrates, and gives the following table:—

Cryohydrate of		Contains				Solidifies at
NaCl	...	76·39	per cent. water		...	− 22° C.
KCl	...	80·00	,,	,,	...	− 11°·4 C.
$CaCl_2$	...	72·00	,,	,,	...	− 37° C.
$MgSO_4$	...	78·14	,,	,,	...	− 5°·0 C.
Na_2SO_4	...	95·45	,,	,,	...	− 0°·7 C.

Supposing that these cryohydrates are formed in the freezing of sea water, it is easy to see how, as the temperature rises, the chlorides melt out first and leave the ice richer and richer in sulphates.

Before concluding this notice, attention must be called to a statement in a note at the foot of p. 318: "As a thermometer immersed in a mixture of snow and sea water, *which is constantly stirred*, indicates − 1°·8 C., we may regard," &c. This can be true only if the temperature of the atmosphere is − 1°·8 C.; if it is 0° C. or higher, the temperature of the sea water will assuredly rise to the melting temperature of snow, or 0° C.

Even though it should turn out that chemically pure ice does, as the author suspects, melt suddenly without previous contraction *as ice*, the discovery of the existence of a *minimum density point* of ice, not chemically pure, which includes all the ice on the globe, is one of the very highest importance.

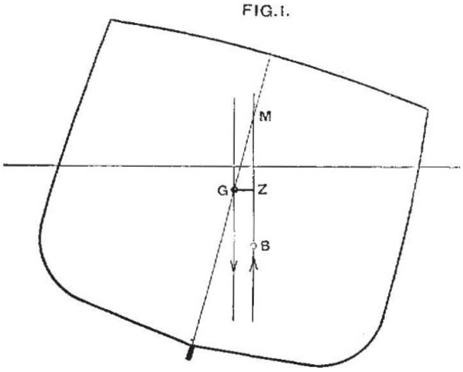
It is to be hoped that we shall soon have a further instalment of work on a subject so large and so important, and with which the author has shown himself so well qualified to deal. J. Y. BUCHANAN

THE STABILITY OF MERCHANT STEAMSHIPS

I PROPOSE to state, and in part to restate, the more important scientific considerations concerning the stability of merchant steamships which the investigation of the *Daphne* disaster has brought to light, following the main lines of the second part of my Report, which has been published *in extenso* in several newspapers. In this case, as in all cases touching the complicated question of ship stability, it is very necessary to be careful not to draw hasty inferences or any inferences at all which are not strictly deducible from the facts or principles established.

It is desirable to guard the reader in the first place against considering the cases of the ships *Daphne* and *Hammonia*—which I have had occasion to associate somewhat closely in my Report—as identical in more than a certain number of features, there being other features in respect of which there is little or no resemblance. I will presently point out both the resemblances and the differences, but first let me remind the reader unfamiliar with naval science what is meant by a curve of stability, quoting the Report as far as may be necessary for the purpose. Fig. 1 may be taken as the transverse section of a vessel inclined at an angle of 15 degrees to the upright. The total weight or gravity of the vessel will act downwards through the centre of gravity G, and the total buoyancy will act upwards through the centre of buoyancy B, as the arrows indicate. It will be obvious that the vessel cannot rest in the inclined position with these forces and no other operating upon her; she must revolve until gravity and buoyancy act in the same vertical line, but in opposite directions. The further she is inclined the more will the ship be immersed on one side and emersed on the other, and therefore the further out will the centre of buoyancy move. Now as neither the gravity nor the buoyancy need be altered in amount by mere inclination, and as they are equal and opposite in direction, it follows that, whatever the inclination, the force acting will always be the same, but the leverage, marked G Z, will vary as the centre of buoyancy moves. At 30 degrees inclination, for example, G Z is much greater than it is in Fig. 1 at 15 degrees. In Fig. 2 these lengths are set up as

FIG. 1.



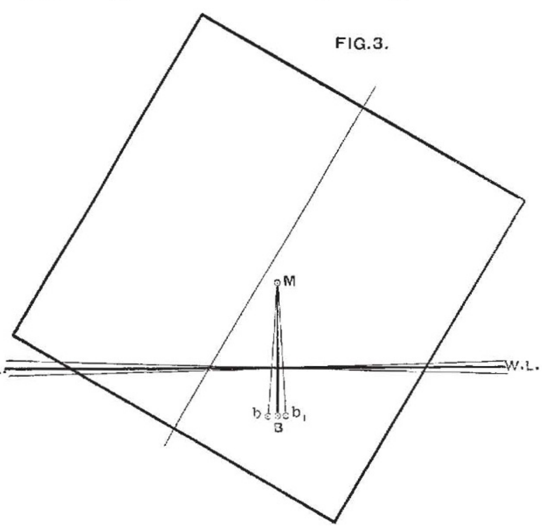
ordinates of a curve, and similar lengths for inclinations of 45 and of 60 degrees are similarly set up; the curve drawn through their upper extremities is this vessel's "curve of stability," observing that the base line is divided into equal lengths for equal angle intervals on any convenient scale.

As regards the "metacentre," I must explain here, as

FIG. 2.

I did in my Report, that in former times, when "initial stability" alone was calculated, the word "metacentre" had a much more limited meaning than it possesses now. It formerly had relation to the upright position of the vessel, in which case the buoyancy acts upwards through the centre line of the ship's section—along G M, for example, in Fig. 1. After receiving a slight inclination,

FIG. 3.



the vessel has, as we have said, a new centre of buoyancy, and the buoyancy itself will act upwards along a fresh line slightly inclined to what was previously the upright line, and will intersect it at some point, M. This point was called the "metacentre," and if we suppose the angle in Fig. 1 to be very small (very much less than 15 degrees), then the M shown there approximately marks the "metacentre." When a ship is much more inclined, the point at which two consecutive lines of the buoyancy's upward action will intersect may not be, and often will not be, in the middle line of the ship at all, but this point is nevertheless called the "metacentre," and the use of the word in this extended sense has recently become general. In Fig. 3 is shown a floating body of square section, inclined in the water at an angle of about 30 degrees. W L is the water line or line of flotation; B is its centre of buoyancy. By giving it a "slight" inclination from the position, it will of course have a new centre of buoyancy given to it. If we incline it one way *b* will show this, if we incline it the other way *b′* will show it, and for each of these positions there will be a new line of action or buoyancy. But these lines of action, together with that through B, will all meet or intersect in one point, and this point (M) will be the metacentre at 30 degrees of inclination. In Fig. 4 I have shown curves of stability for a prismatic body, with the centre of gravity in the centre of form, and also with that centre in some cases raised and in others placed below the centre of form. In this figure the draught of water is taken at 3/25ths of the total depth of the prism. In Fig. 5 I have given curves of stability for the prismatic body with the centre of gravity and the centre of form taken as coincident, but with different draughts of water. In Fig. 6 I have given the curve of stability of a similar prismatic body, immersed 2/5ths of its depth, and having its centre of gravity situated 6 inches below its metacentre. These figures serve to illustrate very clearly the error involved in the assumption that with stability at the upright position and stability at 90 degrees—or but little instability at the latter, which is what some authors have instructed the profession to be content with—there need be no apprehension of any deficiency of stability at intermediate angles of inclination. They show that with square sections and prismatic forms there may be various dispositions of centre of gravity and draughts of water, with which stability in the upright position and again at 90 degrees are not proofs of safety, but indications of the gravest danger.

With these figures before us, we now have both the *Hammonia* case and the *Daphne* case amply illustrated, and can carefully distinguish between the two. The *Hammonia* case—as put forward by Mr. Biles, who conducted her calculations—is that of a high-sided vessel with her stability reaching a maximum soon after she had inclined 30 degrees; and she therefore finds her analogy in one or other of the cases shown in Fig. 5. In the latter figure it will be seen that with the centre of gravity in the centre of form all positive stability vanishes at an inclination of 45 degrees in the two cases A and B; but the *growth and decline* of the stability are very different indeed at the different immersions. When the immersion is smallest the stability rises in a steep curve (A), attaining a comparatively large maximum something under 20 degrees, and then declines, more gradually than it rose, as the inclination goes on. By increasing the immersion from 3/25ths to 5/25ths the curve B is produced, and here we see a vast change of stability, the curve, which rises very slowly from the base line, never reaching one-fourth the maximum ordinate of curve A; only attaining its maximum beyond 30 degrees of inclination, and then declining less slowly than it rose, until it vanishes. Immerse the body to double the last immersion, and we find in curve C that now, instead of vanishing at 45 degrees, the stability only there begins, rising to a small maximum a little beyond 60 degrees and vanishing at 90 degrees. It is in curve B that we find a state of things very closely analogous to that disclosed by the *Hammonia* curve, which I now give in Fig. 7. In both cases the stability increases but slowly; in both it reaches early a maximum; and in both it disappears altogether before the vessel is more, or much more, than inclined through half a right angle. The case of the *Daphne* resembles this in the slowness with which the stability increases as the vessel is inclined, this slowness being due to the same causes in both cases doubtless; but an examination of the triple-branched curve of her stability given in Fig. 8 shows that the analogy between the two cases ends at quite a moderate angle of inclination, say 30 to 31 degrees. In this figure (8) the curve A is constructed on the assumption that the ship was free to take water on board as the main deck became immersed; the branch B presumes the poop to have been watertight; and the branch C is calculated to show how the stability curve would have increased until the bulwarks came under water, provided these bulwarks had been watertight. It will at once be seen that the *Daphne* cannot be regarded as analogous to the *Hammonia* or to the curve B in Fig. 5, in so far as the stability at very large angles is concerned. On the contrary she would have more resembled the case of Fig. 6, provided her sides had gone as high as her topsides and been there decked over. The *Daphne's* curve A ceases to rise soon after the main deck becomes immersed, and then falls rapidly away in the same manner and for the same reason as all ships lose stability when, or soon after, the freeboard has become exhausted. It must therefore be clearly understood that it is in the early stages of the two curves that the cases which I have had to make public find their resemblance; at the later stages the *Daphne* illustrates the consequences of the immersion of the deck, while the *Hammonia*, by losing all stability before the deck became immersed, opened up a state of things which startled her builders, surprised the profession, and confounded the text-books, and

FIG. 4.

CURVES of STABILITY of PRISMATIC BODY of SQUARE SECTION

DRAUGHT of WATER 8/25ths of DEPTH.

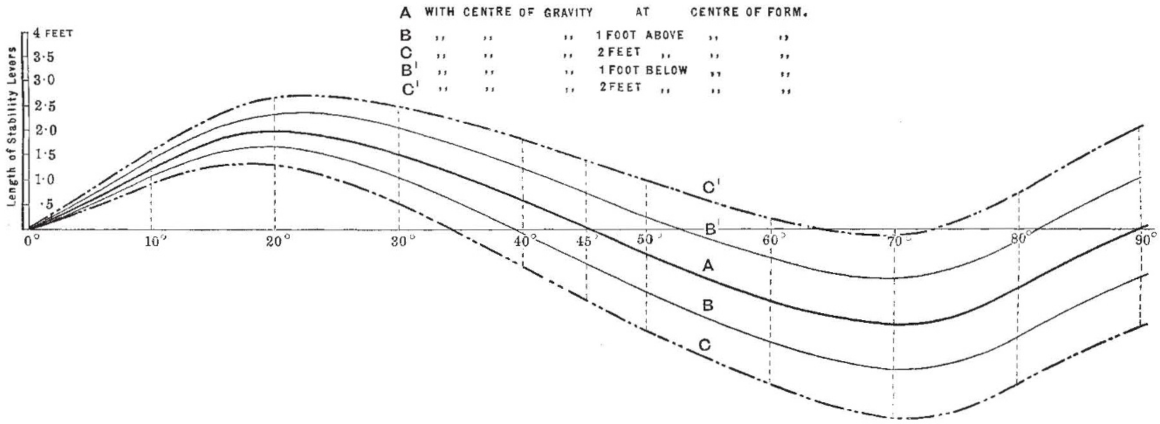


FIG. 5.

CURVES of STABILITY of PRISMATIC BODY of SQUARE SECTION

WITH CENTRE OF GRAVITY AT CENTRE OF FORM.

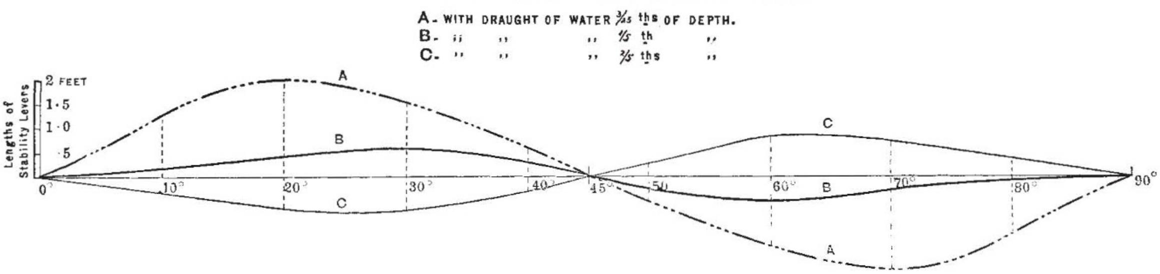
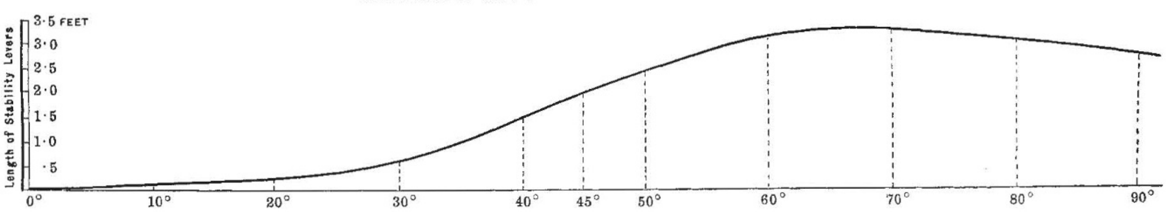


FIG. 6.

CURVE of STABILITY of PRISMATIC BODY of SQUARE SECTION

DRAUGHT of WATER 2/5ths of DEPTH.

WITH CENTRE OF GRAVITY 6 INS. BELOW THE METACENTRE.



must force extended calculations upon all those who hereafter undertake to launch ships upon the stability of which any doubt can by possibility exist.

It is pretty widely regarded as a remarkable fact that there should have been any deficiency in the knowledge of shipbuilders concerning the conditions or possible conditions of the stability of ships at their launching draughts. But to me this deficiency seems the most natural thing possible. It needs no explanation to those who remember what immense transformations and extensions have come upon the shipbuilding trade during even my own professional experience. I well remember looking with wondering interest in Sheerness dockyard at the first iron ship ever seen there; and yet the construction of iron ships had become so universal fifteen years ago that I wrote my work on "Shipbuilding in Iron and Steel" to meet a widespread necessity, the idea of writing descriptions of wood ships having already passed away. I equally well remember the building at Sheerness of the first screw steamship ever constructed there; but where now are any but screw steamships built for ordinary ocean work? Some sailing ships and some paddle steamers doubtless are built even now; but the screw steamer has almost undisputed possession of the world's ocean trade. With these changes have come in wholly new developments of shipbuilding science, and the present is not by any means the first instance in which it has fallen to my lot to point out errors of doctrine—false deductions from former practice—which were misleading the shipbuilder. In the case of the strains to which ships are subjected, the deductions made by the most eminent men who discussed the subject scientifically at the end of the last and the beginning of the present century seemed to me to be irreconcilable with the conditions of modern ships, and after lengthened investigations I found that they were not only wrong, but in some cases the reverse of the truth, and I contributed to the Royal Society a paper on the subject which has brought modern theory and modern practice into better relationship. In the matter of stability it was most natural that as we abandoned the employment of wind as our propelling power—which of course imposed upon ships the necessity for large stability to withstand the wind-pressure—shipbuilders were able to resort to greater proportionate length and to enlarged proportionate area of midship section; and thus to bring about conditions in which large initial or early stability, so to speak, fell out of demand. Nor is it easy to say when deficient stability would have come under close investigation, had it not been for the accident of certain ships of very *low freeboard* coming under consideration at the Admiralty, as explained in my Report. These led to the calculation of stability at successive angles of inclination, and to the method of recording the results in the form of the "curve of stability" previously described. But besides the *change* of the seagoing ship, there has been the enormous extension of its employment, our carrying trade on the sea having increased by leaps and bounds. Every one knows that when the demands of commerce are very urgent, science and scientific research are apt to be neglected. The necessity for great carrying power and speed at sea has been attended by an equal necessity for quickening the loading and discharging of ships in port, and consequently steam windlasses and cranes, and many other modern appliances involving upper weight have come into vogue, and their effect upon stability has not been always considered. From these and other causes there has been brought about that somewhat extensive employment of ships of small stability, or of no stability at all in themselves, to which it lately became my duty to direct attention. It is no doubt the general belief that a high-sided ship having some initial stability, will, as she inclines, gather large additional stability, and will retain some even at very large angles; that, as my Report states, has greatly encouraged people to be satisfied with very small initial stability, in some cases with none at all, and even less than none. Many steamships of large tonnage have been built of late years for influential steam companies and other owners, which ships are totally incapable of floating upright without the aid of ballast or of cargo, and which cannot be unloaded in dock without being held upright with hawsers attached to the shore. Such ships, even when capable of floating unballasted without capsizing, can only do so by lolling over at large angles of inclination, and there finding a position of stable equilibrium. When carefully watched over and stowed with suitable cargoes, these ships can usually be made safe at sea, and sometimes even safer than ships with larger initial stability but less range—a circumstance to which undue prominence has perhaps been given, and which has diverted many from the grave elements of danger which more often are associated with small initial stability. "There is not the least doubt, however, that a very small initial stability given to many modern mercan-

FIG. 7.

CURVE of STABILITY of S.S. "HAMMONIA" as launched.

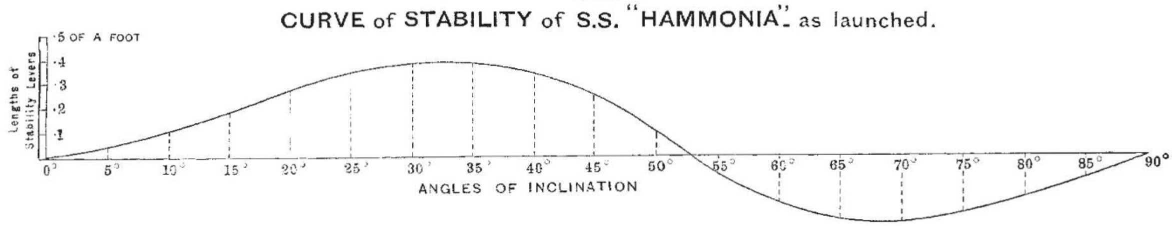
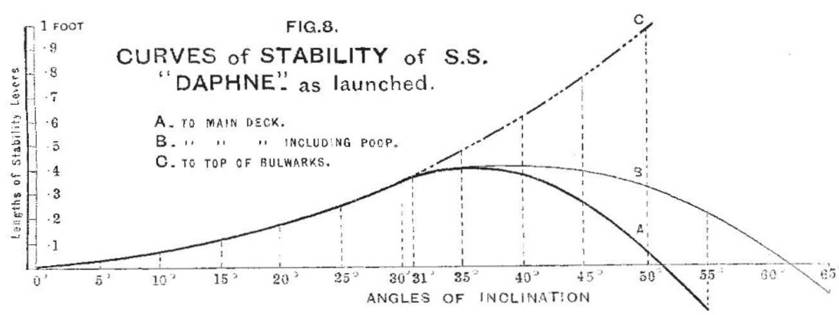


FIG. 8.

CURVES of STABILITY of S.S. "DAPHNE" as launched.



tile steamships—given in the belief that much more is sure to be gained as the ship inclines (within large limits) —has resulted in the capsizing of many ships at sea, and in grave danger to many that are still afloat, not in the same manner, because not in the same condition as to lightness as the *Hammonia* and *Daphne*, but from other not less real deficiencies." Sad and serious as this statement is, I repeat it here with perfect confidence in its accuracy.

Sometimes such vessels are brought into a condition of apparent safety by the stowage of their own coal, but as the coal is consumed their stability diminishes, they capsize, disappear, and the word "missing" is recorded against them in an official return. No means exists, notwithstanding all our shipping legislation, for insuring that the facts will be brought to light—indeed, at the official inquiry which follows under the present conditions, the question of stability may not even be mentioned. As the stability of a ship is often an intricate matter which can be effectually controlled only by close and careful calculation, and as no Government department is at present charged with the duties even of collecting, recording, and making known those dimensions and particulars of ships which determine their stability, the matter must be left to right itself. Maritime ships of small stability incur dangers from, and are doubtless lost by, the operation of causes which are but very imperfectly appreciated.

It is under the urgent pressure of a very rapidly growing mercantile steam marine that the shipbuilding trade has somewhat, I fear much, outrun the companionship and regulation of science. It is only quite recently that the necessity for developing their scientific staff and appliances has been borne in upon the minds of shipbuilders. There never, even yet, has been so much as a training school or college established by them for the education of young naval architects and draughtsmen throughout the country. But the Admiralty have had their dockyard schools at work for nearly forty years; school after school of Government naval architecture has been established; the Institution of Naval Architects has been formed, and done invaluable work, for more than twenty years; and some private shipbuilders have at length entered with spirit and enterprise upon the labour of developing the practice of scientific naval architecture. No part of my duty in connection with the *Daphne* inquiry has been so agreeable to me as that of bearing witness to the admirable efforts of several Clyde firms in this respect; and there is no result that can follow from the inquiry which I should esteem so highly as the emulation of their efforts throughout our shipbuilding establishments generally—unless, indeed, it were that of a general awakening of *shipowners* to their great and enduring responsibilities in this matter.

EDWARD J. REED

INTERNATIONAL POLAR RESEARCHES

AT the present moment, when every student of modern science is anxiously awaiting the result of the labours of the international observation parties which have for nearly a year been self-imprisoned around the Pole, I venture to make the following suggestions relating to international Polar researches.

The state of the ice in the Arctic seas is, as is generally known, very changeable during various seasons. It is thus impossible beforehand to draw conclusions as to the probable state of the ice one summer by its state the year before, and this circumstance has greatly impeded active researches in the Arctic regions. From time to time valuable and expensive expeditions have been despatched, but these have in most instances been unfortunate enough to encounter the adverse seasons, and the purely geographical gain has in consequence not been in proportion to the cost. At other seasons, on the other hand, when the ice seems to have promised a far advance northwards there has not been any expedition ready to take advantage of the circumstance. Had there at certain times and seasons been expeditions prepared to use the opportunities which have presented themselves, and in the right locality, I have not the least doubt that a very far advance into unknown Polar regions might have been made at a very small cost. In spite of the, in many respects, exceedingly valuable discoveries which have resulted from these expeditions to geology, meteorology, and other modern sciences, they seem certainly on the whole as if they had been started under an unlucky star, which is, in my opinion, caused by the circumstance *that the period and season selected have not been the proper ones*. What we have thus gained has generally been obtained with great loss of time, money, and valuable lives. A most remarkable contrast to this is, however, the voyage of the *Vega*, which from beginning to end seemed to have been attended with success only, as the forced wintering, when having practically accomplished its object, only tends to heighten the charm of this venture.

From the experience we have gained of the changes in the ice, it is however evident that Polar researches have hitherto, in one respect at all events, been effected in an erroneous manner, and great loss of money and life caused thereby. The *geographical* researches around the Pole should in my opinion be conducted in a different manner. Instead of, as has hitherto been the case, that finely equipped expeditions are despatched at random and at unconsidered periods, an arrangement should be made between the various European nations to equip a certain number of expeditions, which should be despatched *every* summer to the *same* locality *during a period of ten to eleven years*. During a period of this length it is probable that the conditions of the ice, which we may assume undergo periodical changes, have run their cycle, and during certain years of such a period opportunities would undoubtedly occur which would enable a very far penetration into the Polar basin.

The expenses attending such expeditions would, if skilfully arranged, not exceed those of one of the costly ones which have hitherto been despatched, while they would not result in the great loss of life which seems to attend the larger one or two years expeditions under which ambition naturally leads the members to venture on any undertaking which may give returns equivalent to the expectations of the equippers.

Hitherto the Dutch alone have arranged their expeditions to the Polar regions in a systematic manner. They have, as is generally known, for some years regularly despatched an expedition every summer to the regions around Spitzbergen and Novaya Zemlya; but that they have not, geographically, obtained any great results may be ascribed to the circumstance that they have employed sailing vessels instead of steamers. Neither have they in all probability laid special stress on geographical achievements in these parts; the expeditions hitherto despatched may thus be considered as mere pioneering ones. From next year it is, however, the intention of the Dutch to employ a steamer instead of a sailing vessel, and then their researches will, no doubt, be more fruitful.

It is now admitted by every student that Polar researches are of great importance in several respects, and the establishment of the international circumpolar stations is a proof of this, while the manner in which these have been arranged seems to promise to be the first step towards a series of researches in the Arctic regions, which would, as the meteorological ones, be best carried out through an international cooperation. In order to advance in the unknown Polar basin, it appears to me to be essential to abandon the random expeditionary attempts hitherto persevered with, and organise instead systematic researches. And if these are carried out by international