Capsizing of Small Trawlers

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Read at a Joint Meeting of the RINA with the Institution of Engineers and Shipbuilders in Scotland in Rankine House, 183 Bath Street, Glasgow on February 20, 1979, Mr B. N. Baxter, M.Sc., Ph.D. (Vice-President, IESS) in the Chair and in London at a meeting of the Royal Institution of Naval Architects on April 26, 1979. Professor J. B. Caldwell, O.B.E., B.Eng., Ph.D., F.Eng. (Vice-President, RINA) in the Chair.

SUMMARY: Results are presented of an investigation into the behaviour in rough water and breaking waves of two inshore fishing vessels having almost identical principal dimensions and displacement, but with different statical stability characteristics.

In breaking waves, hydrodynamic conditions exist which may endanger a small fishing vessel with an inadequate reserve of stability. It is concluded that the margin of stability for small inshore fishing vessels as required by the IMCO criteria appears to be insufficient to prevent capsizing in certain possible sea conditions.

1. INTRODUCTION

Public concern over the safety of fishermen has centred largely on occasional major disasters involving the losses of whole vessels and their crews. Examples of such disasters were the losses of the ROSS CLEVELAND[1], ST ROMANUS[2] and KINGSTON PERIDOT[3] (two of them in exceptionally bad weather off Iceland) in January and February 1968. More recent examples include the loss of the large stern trawler GAUL[4] and the inshore trawler TRIDENT[5] in February and October 1974 respectively.

As a result of the trawler losses in 1968 a Committee of Inquiry into Trawler Safety was set up under the chairmanship of Admiral Sir Deric Holland-Martin. The final report[6] of the inquiry was published in 1969 and made many recommendations to ensure the safety of fishing vessels. One of the main recommendations on design and construction was that the Board of Trade (currently the Marine Division, Department of Trade) should seek powers to lay down statutory requirements on the stability of newly built trawlers. The committee also considered that the Intergovernmental Maritime Consultative Organisation's (IMCO) stability criteria would provide a suitable starting point for stability standards with scope retained for amending the standards in the light of experience.

In due course of time the recommendations of the Committee of Inquiry were adopted by the Government and legislation followed which resulted in the Fishing Vessels (Safety Provisions) Act 1970[7] and ultimately the Fishing Vessels (Safety Provisions) Rules 1975[8]. The Rules cover all aspects of trawler safety and include requirements for freeboard, stability, fire protection and freeing ports. Although the recommendations of the Holland-Martin Inquiry were directed primarily to vessels of 24·4 metres in length and above the 1975 Rules extend downwards to fishing vessels of 12 metres in length.

In the light of trawler losses in the mid 1970s and other similar casualties, it has become clear that the causes of loss and of capsize in particular are often complex. Moreover, such capsizing accidents cannot be explained on the basis of the simple statical stability criteria, such as the IMCO stability criteria. Recent research has shown that there are several different ways in which a ship may capsize. It follows that the stability regulations may well be inadequate in some respects if some important modes of capsize are ignored.

Some of the hydrodynamic aspects of capsizing are currently under investigation at NMI and the purpose of this paper is to report on model experiments which could help explain one of the modes of capsize at sea for inshore trawlers. The findings of these experiments are relevant not only to those who have to investigate losses at sea, but also for providing the basis of improved future regulations, which, in the long term, will result in the design and construction of safer fishing vessels.

2. THE IMCO STABILITY CRITERIA

The IMCO stability standards for fishing vessels[9] are based on an extensive survey of various national stability regulations; on statistical and other analyses on intact stability casualty records[10] and the experience of the different fishing fleets throughout the world. The standards are expressed in terms of minimum values for certain key features of the righting arm or $\overline{GZ}$ curve. These features are the metacentric height (a measure of the steepness of the curve at small heel angles), the maximum lever and the angle at which it occurs, and the area under the curve up to specified heel angles (typically 30° and 40°).

All of the above features quoted together represent a reasonable measure of stability. One quantity taken in isolation can be deceptive. The metacentric height in particular is sometimes quoted as a measure of stability; this can be misleading, since a large metacentric height is no guarantee of adequate righting levers at large angles of heel. The IMCO stability criteria are therefore considered to reflect the view that the shape of the $\overline{GZ}$ curve has a strong influence on the safety of fishing vessels and their survival from capsize in extreme weather conditions.

The IMCO stability criteria are included in the Fishing Vessels (Safety Provisions) Act 1975[8] and are described in a recent paper on Fishing Vessel Safety[11]. These criteria have recently been endorsed at the International Convention for the Safety of Fishing Vessels[12] held at Torremolinos in 1977. It is of interest to note that the angle of vanishing

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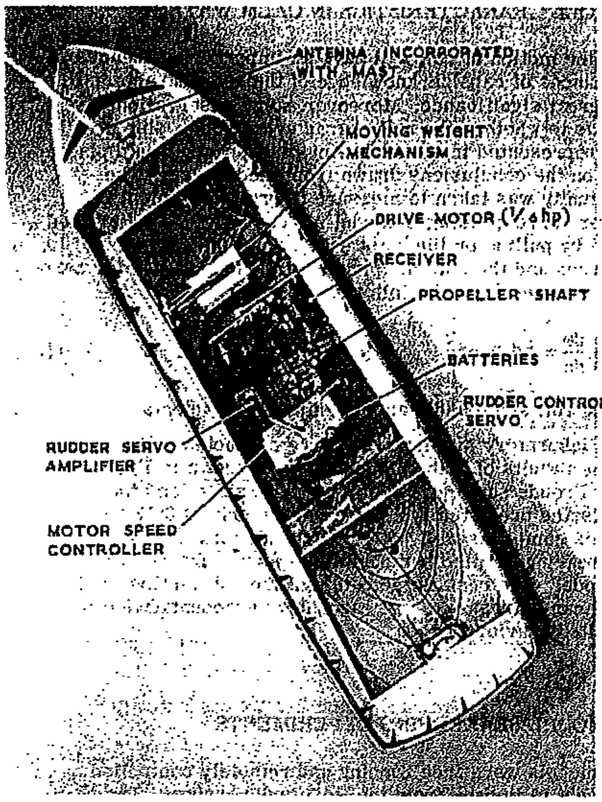


Fig. 3. Instrumentation in Model A

stability is not included in the above criteria although it has been included in several national regulations[13,14]; this angle is usually specified as at least 60°.

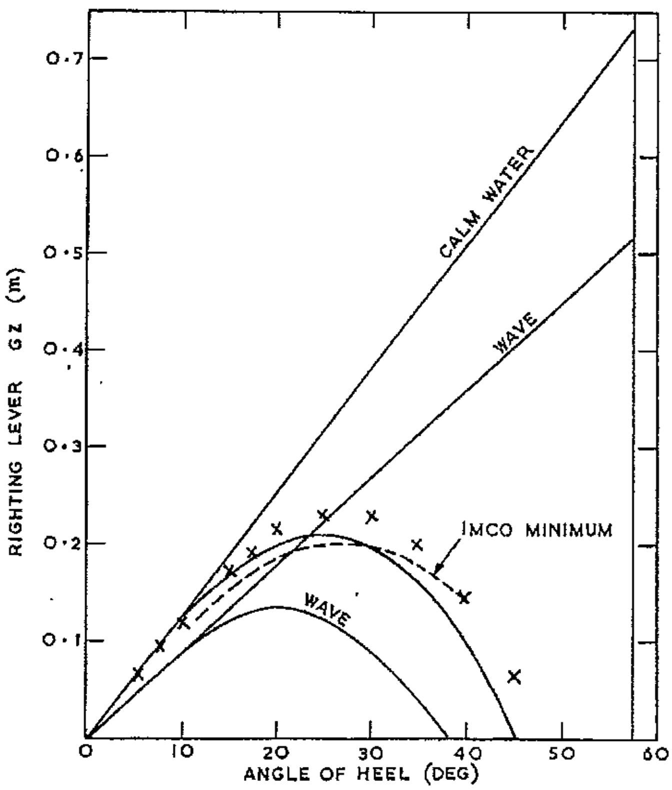
3. MODEL EXPERIMENTS

The model experiments were carried out in No 3 tank at NMI. Two models were used in the experiments and their principal particulars are given in Table I. Models A and B had hulls representative of vessels built in the late 1960s and early 1970s which are still in service today. Model A represents an inshore trawler built of steel with a transom stern whereas model B represents an inshore trawler built of steel with a cruiser-type or round stern. Both models were used as free-running radio controlled models; the scale of each model was 1 : 15. General arrangement drawings of the transom stern and round stern designs are shown in Figs. 1 and 2 respectively.

TABLE I

Model	A	B
Length (L_{OA}) metres	25·91	24·36
Length (L_{PP}) metres	22·09	21·44
Breadth mld metres	6·86	6·71
Depth mld metres	3·35	3·35
Draught amidships metres	2·48	2·49
Draught forward metres	1·83	1·79
Draught aft metres	3·13	3·19
Trim by stern metres (relative to datum line)	0·69	0·33
Rake of keel metres	0·61	1·07
Sheer forward metres	0·99	1·57
Sheer aft metres	0·46	0·54
Freeboard at bow metres	2·15	2·76
Displacement tonnes	167·6	160·0
Transverse GM metres	0·732	0·908
Vertical centre of gravity VCG metres	3·153	2·58
Free-surface correction metres	0·008	0·032

The scale of the models was chosen to match the capability of the wavemaker and this resulted in models of 1·7 m in length. This size of model initially limited the inclusion of



STABILITY CHARACTERISTICS

	CALM WATER	IMCO MINIMUM	ON CREST OF WAVE
AREA 0-30° (m RAD)	0·075	0·055	0·05
AREA 0-40° (m RAD)	0·10	0·09	0·057
AREA 30°-40° (m RAD)	0·026	0·03	0·007
MAX GZ (m)	0·21 AT 24°	NOT STATED	0·14 AT 20°
GZ AT 30° (m)	0·196	0·2	0·088

X MEASUREMENTS OBTAINED FROM GZ APPARATUS

FOR WAVE CURVE, TROCHOIDAL WAVE (H = 4 m, L = 70 m) HAS BEEN ASSUMED WITH CREST AMIDSHIPS

Fig. 4. Stability Curves for Design A

STABILITY CHARACTERISTICS

	CALM WATER	IMCO MINIMUM
AREA 0-30° (m RAD)	0·074	0·055
AREA 0-40° (m RAD)	0·126	0·09
AREA 30-40° (m RAD)	0·052	0·03
MAX GZ (m)	0·405	NOT STATED
GZ AT 30° (m)	0·399	0·2

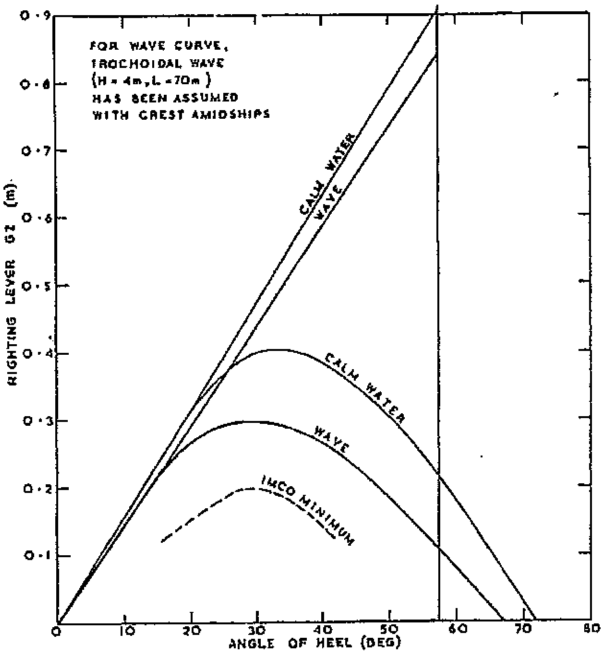


Fig. 5. Stability Curves for Design B

motion measuring equipment although it was possible to arrange a moving weight mechanism which could be operated remotely to simulate the effects of a sudden gust of wind. A pitch and roll gyroscope was fitted to model A for subsequent experiments in which the motions were telemetered to No 3 tank carriage.

A photograph of the inside of model A (Fig. 3) shows the typical instrumentation layout of both models.

4. STABILITY IN CALM WATER

The hydrostatic particulars were computed from the hull offsets of both designs using the SIKOB computer program and statical stability curves obtained. As a check on the stability, model A was fitted to a GZ apparatus, which for any angle of heel measures the hydrostatic moment acting to restore a free floating model to the upright. Fig. 4 gives the stability curve for design A and a comparison between calculated and measured GZ values. A close comparison cannot be expected at angles beyond which the deck edge becomes immersed and for design A this occurs at about 16°. Fig. 5 gives the stability curve for design B. The minimum stability required by the current IMCO criteria is also indicated in Figs. 4 and 5.

In an attempt to explain the large differences in the stability curves of designs A and B, further calculations were made. The sheer line of design A was increased to correspond exactly with design B and new stability curves were obtained: (i) for the modified design A with the original trim and GM and (ii) for the modified design A with same trim and GM as design B. A comparison of these new stability curves with the original curves is shown in Fig. 6. It can be seen that the increase in sheer for design A (modified design A) together with a decrease in stern trim accounts to a large extent for the differences in stability curves of designs A and B.

5. ROLL CHARACTERISTICS IN CALM WATER

Since the motion of roll will have an important influence on the chances of capsize, knowledge of the roll characteristics is of great significance. Moreover, some reservations[15] exist as to whether the roll characteristics of a ship are truly represented in a model. Any discrepancy would place doubt on the conclusions drawn from model tests. The opportunity was taken to measure the roll decrement of a trawler corresponding to model A. The trawler was excited in roll by pulling on the mast with a rope, the roll period measured and the roll damping coefficients deduced for zero forward speed from the following equation:

$$-\frac{d\phi}{dn} = a\phi + b\phi^2 \qquad (1)$$

ϕ being roll angle and a and b damping coefficients.

In the laboratory similar tests were conducted on model A and the results of both experiments are shown in Fig. 7. Using Froude scaling (model period $\times \sqrt{\text{scale}}$) and the model results, the predicted roll period of the ship is 6·12 seconds and this compares well with 6·5 seconds measured on the ship, bearing in mind her condition was not precisely that of the model. The results indicate that the roll motions of the model can be regarded as reasonably representative of the vessel's behaviour at sea.

6. MODEL SEAKEEPING EXPERIMENTS

Both models were free running and remotely controlled through a radio link from the No 3 tank carriage. The majority of the data collected from the experiments was visual, supported by cine films, although subsequently motions were recorded. The test procedure adopted for the model experiments was to manoeuvre the model on various courses such as head to sea, following, beam and continuous circular manoeuvres etc at corresponding ship speeds of up to $10\frac{1}{2}$ knots. When desired, the roll of the model in waves was accentuated by operating the moving weight mechanism within the model, simulating a sudden gust of wind striking the vessel.

The experiments conducted can be conveniently considered in three parts.

6.1 Experiments in Coastal-type Waves

The characteristics of typical coastal waters are given by Darbyshire[16]. They are commonly used in model experiments for the prediction of the performance of ship designs expected to operate in near coastal waters. Irregular waves as defined by Darbyshire corresponding to full-scale wind forces 7 and 8 with significant wave heights of 3·30 m and 4·5 m respectively, were generated for the initial experiment.

The behaviour of each model was perfectly satisfactory in both sets of waves. Model A shipped a little water over the whaleback in force 8 and during circular or zig-zag manoeuvres rolled considerably more than the round stern design, model B. In following seas little difference was observed between the models and the transom stern on model A did not appear to handicap progress in these waves. There was however a tendency to broach in both models.

Although the models were of almost the same displacement, the transverse metacentric heights, GMs, were considerably different. The value for model A corresponded to 0·732 m full-size, whereas the value for model B corresponded to 0·908 m. The smaller GM for model A resulted in a more tender hull and this was responsible for the greater roll motion than model B. The application of the gusting wind mechanism had no significant effect on the behaviour of the model. There was no reason to suppose from these experiments that either design would experience any real difficulty in Darbyshire coastal-type waves up to wind force 8.

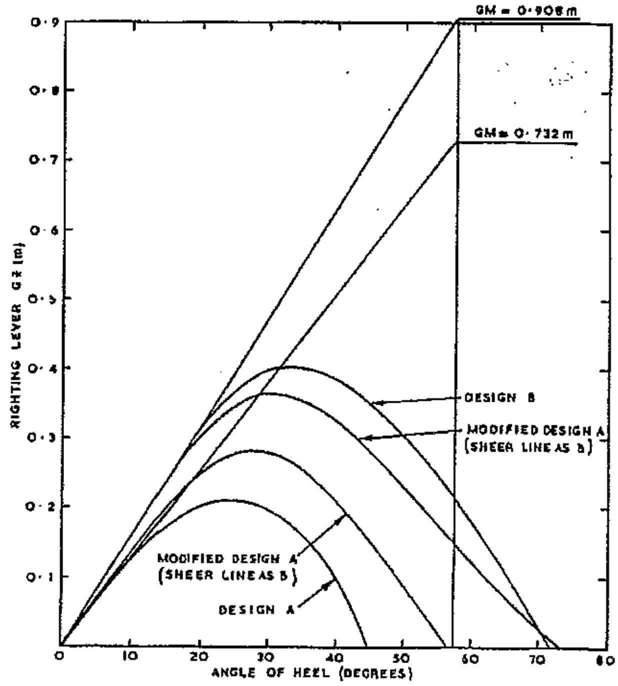


Fig. 6. Stability Curves for Designs A and B

6.2 Experiments in Breaking Waves

With the tide against the wind it is probable that waves become shorter and steeper and it is not unreasonable to surmise that in these circumstances breaking waves replace the less steep coastal-type waves. Breaking waves defy exact definition but an approximate analysis of the breaking wave spectrum measured in the tank is shown in Fig. 8 and for comparison, Darbyshire type waves for wind forces 7 and 8 are included in the diagram.

An alternative sea state of breaking waves was used for the subsequent model experiments; these were generated from waves which corresponded to full-scale significant heights of 3·2 m with their wave length shortened. The maximum wave height produced by the resulting breaking waves in any given sample corresponded to 4·9 m full-size. These waves were most realistic. A photograph of model A under test in breaking waves is shown in Fig. 9.

The motions of both models in head seas were severe. Considerable water was shipped on board both models and sometimes struck the deckhouse front. Model A rolled the most and proved harder to keep on course. In following seas both models broached and ended up beam-on to the waves in defiance of the rudder action, with model A rolling severely when in a quartering sea position.

The freeing port arrangements in the two models differed significantly. Model A's arrangement allowed water to flood the deck as the vessel rolled into a wave and the time taken to discharge water from the deck was considerable. This of course depended on the amount of water shipped but periods equivalent to 2 minutes, full-scale, were noted.

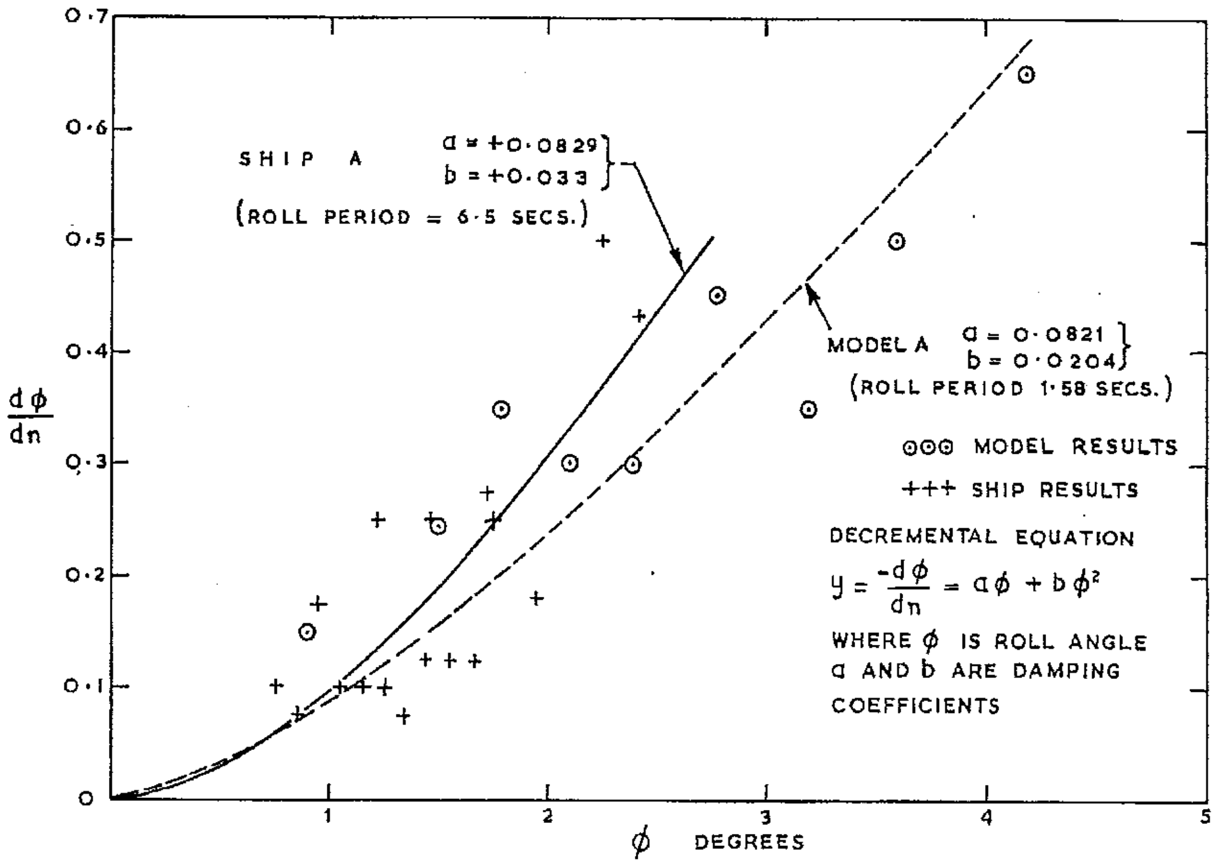


Fig. 7. Roll Damping Coefficients for Ship and Model A at Zero Forward Speed

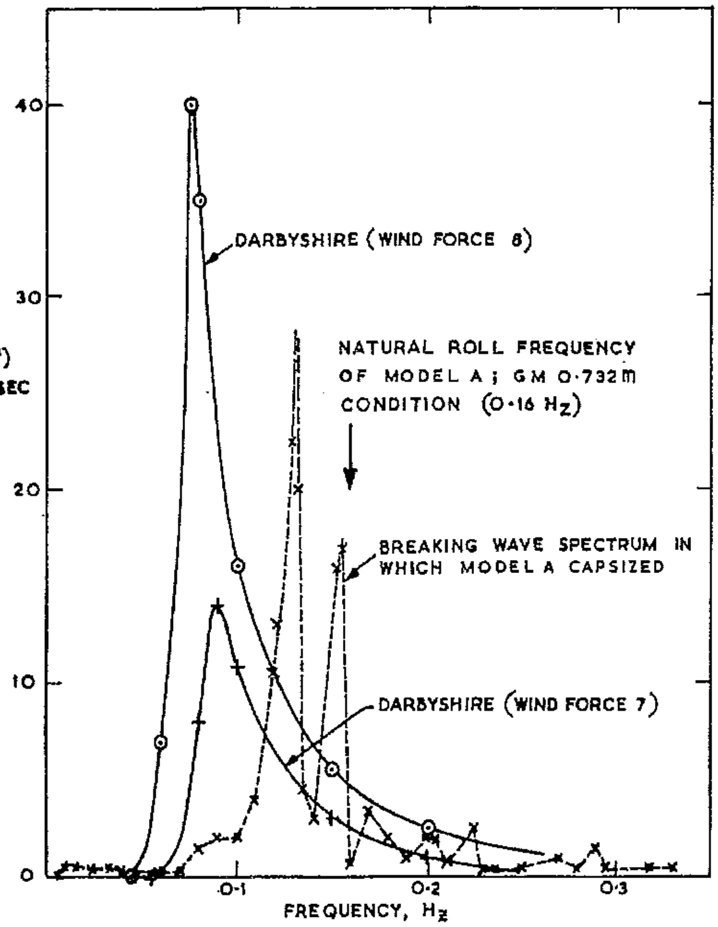


Fig. 8. Wave Energy Spectra

Once circling manoeuvres were attempted model A was immediately at risk. Model A capsized on a number of occasions, usually when it was caught in a beam to sea position.

There were two distinct types of capsize: the classic one in which the hull, when balanced on the crest of a wave, and *without* water on deck, immediately lost waterplane inertia and hence stability; and the second one, where a wave overwhelmed the bulwarks and produced a rolling moment greater than the restoring moment naturally present in the hull. These types of capsize were filmed and several frames have been reproduced in Figs. 10 and 11; these figures show sequences of the two types of capsize discussed.

It is significant to note that the elapsed time for each capsize was 10 to 20 seconds only (full-scale) or about the time of two to three roll cycles and this was occasionally shorter when the model was kept stationary in beam seas. Fig. 12 shows the sequence of events when the model was struck by a breaking wave in beam seas.

Fig. 13a shows a record of the roll motion and capsize of model A without water on deck whereas Fig. 13b records the capsize of model A after being overwhelmed by a breaking wave. Fig. 13c records the capsize of the same model when held beam to the seas; in this case the leeward deckedge rolled under and collected water on deck resulting in a capsize four roll cycles later. All the above records were for the model capsizing to leeward, when the roll angle exceeded 40°.

In contrast, model B survived circular manoeuvres in the breaking waves with comparative ease. The motions were however, severe and considerable quantities of water were shipped. On several occasions the model survived a test period equivalent to about 1 hour full-size. Every attempt was made to bring about a capsize, but the extra roll stiffness inherent in the hull due to the large GM clearly contributed to its survival.

6.3 Experiments in Breaking Waves—Modified Loading

The object of these experiments was to discover the loading or changes in stability characteristics that were necessary for model A to survive the breaking waves and to find the condition for model B which would bring about capsize.

The metacentric height, GM, of model A was accordingly altered to correspond to that of model B. Experiments in the breaking waves were then repeated and no capsize took place. Motions were extremely severe and decks very wet but an impression was gained that a condition for survival had been reached. This straightforward increase in the roll stiffness had proved successful and the difference in performance between model A and model B now appeared to be due to differences in hull design.

Model B was tested in four further conditions with its displacement being kept constant. Variations were thus restricted to changes in metacentric height, GM, and these are given in Table II.

TABLE II

Condition	Displacement (tonnes)	GM (m)
(initial tests)	160	0·908
(a)	160	0·732
(b)	160	0·574
(c)	160	0·504
(d)	160	0·400

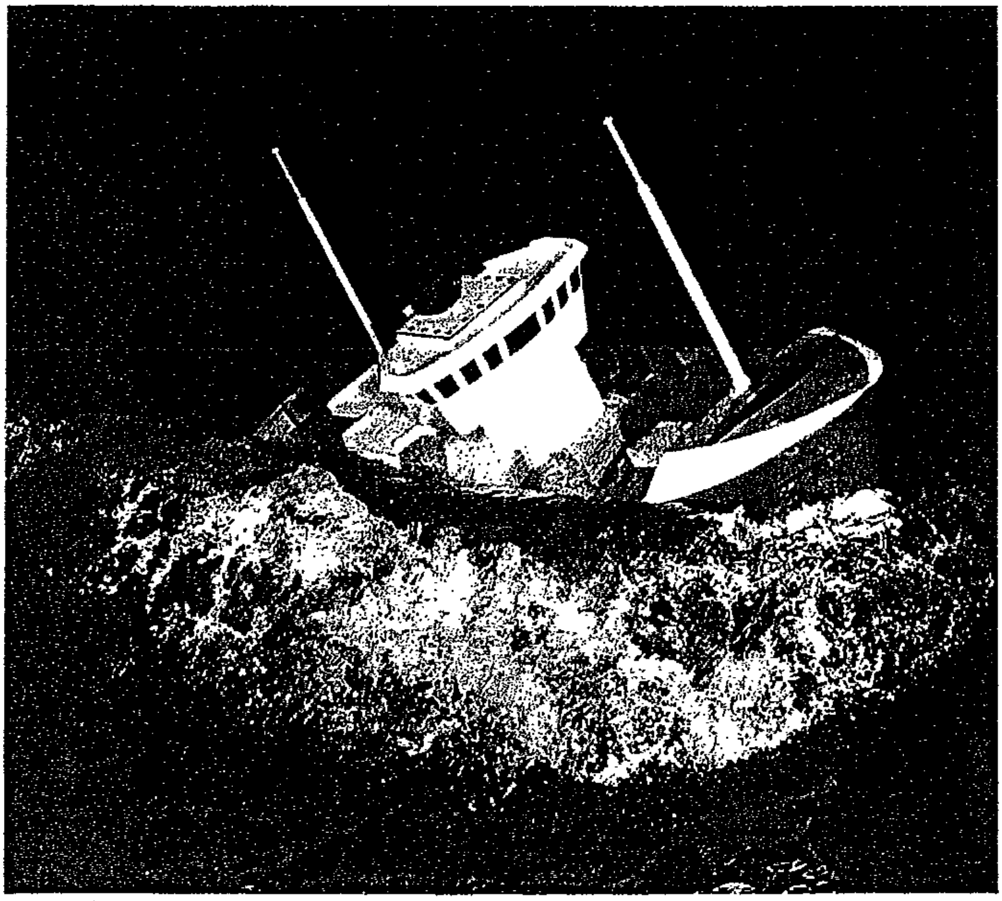


Fig. 9. Model A Under Test in Breaking Waves

The corresponding stability curves are plotted in Fig. 14.

Of the four further conditions tested with model B, two produced capsize. These were conditions (c) and (d) where GM was 0·504 m and 0·400 m respectively. It was noticeable that as the roll stiffness was reduced through reductions in GM the model rolled to larger angles in breaking waves. Condition (a) was selected to agree with the condition of model A in the initial experiments and model B survived. This showed that survival did not depend entirely on the absolute value of GM, but rather that the character of the stability curve played an important part.

Figs. 4 and 14 indicate that condition (a) for model B produces greater dynamical stability than the equivalent condition for model A, despite having the same GM; this difference in dynamical stability is brought about by differences in hull design and trim.

7. CONCLUSIONS

The model experiments have shown that model A, with a metacentric height, GM, corresponding to 0·732 m full-size capsized in breaking waves of modest severity, of a height and length which the vessel could conceivably encounter in service. The sequence of events during capsize occurred very rapidly. Reasons for capsize suggested by the results of the model experiments are:

(i) Model A with a GM corresponding to 0·732 m full-size had insufficient stability at rest. The effects of this were clearly visible in the model where roll, even in a modest sea state, was excessive. Lack of sufficient roll stiffness is suggested as the main cause contributing to capsize. Subsequent experiments in which either displacement or GM was increased suggested that the fault lay not so much in the hull shape but rather in the CG position which led to a simple deficiency in GM.

(ii) The effect of model A's transom stern on performance was difficult to assess accurately. Model behaviour appeared to be slightly inferior to the round-stern design of model B in following seas, water at times flooding on the after deck, but this seemed not to be a factor in producing capsize. Both models tended to broach in following seas.

(iii) The effect of a sudden gust of wind striking a vessel at sea cannot be ignored, but when simulated on the models it had little effect. The worst case would be with the hull balanced momentarily on the crest of a wave but in the cases where the model was adequately stiff it was relatively undisturbed by the gust.

On the evidence of the behaviour of these two models alone, the margin of stability for the small inshore fishing vessel as required by the IMCO criteria seems insufficient. Both models experienced capsize when their stability at rest was close to the IMCO minimum. The survival condition for model B was obtained with the maximum righting lever, GZ, fractionally above the IMCO value but greater overall stability was present due to a higher angle of vanishing stability than that implied by IMCO.

If IMCO recommendations for minimum stability are to be reconsidered then greatest emphasis should be placed on the maximum righting moment and its position on the stability curve together with due consideration of a minimum value of the angle of vanishing stability while retaining the requirements for GM.

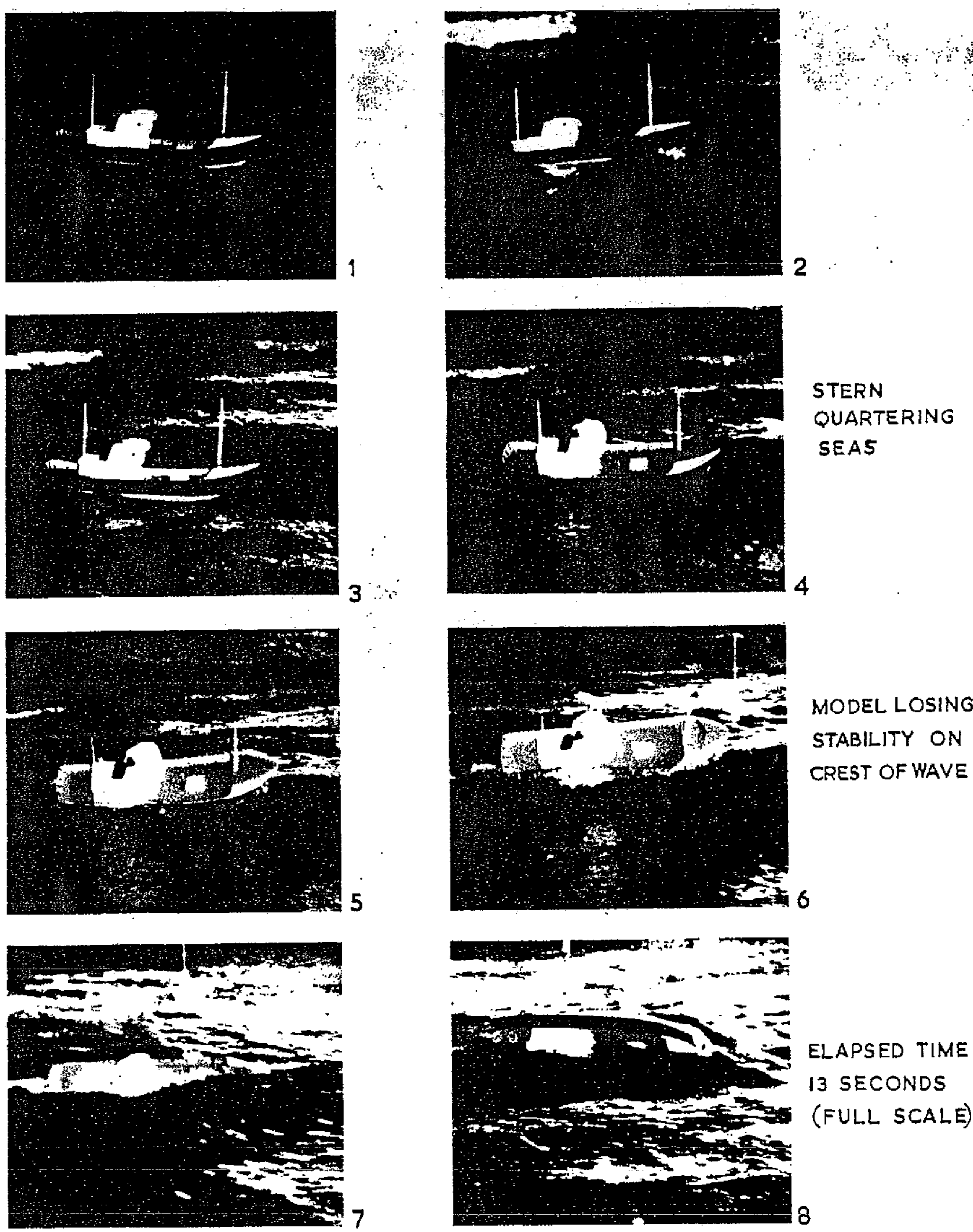


Fig. 10. Model A Capsizing with No Water on Deck

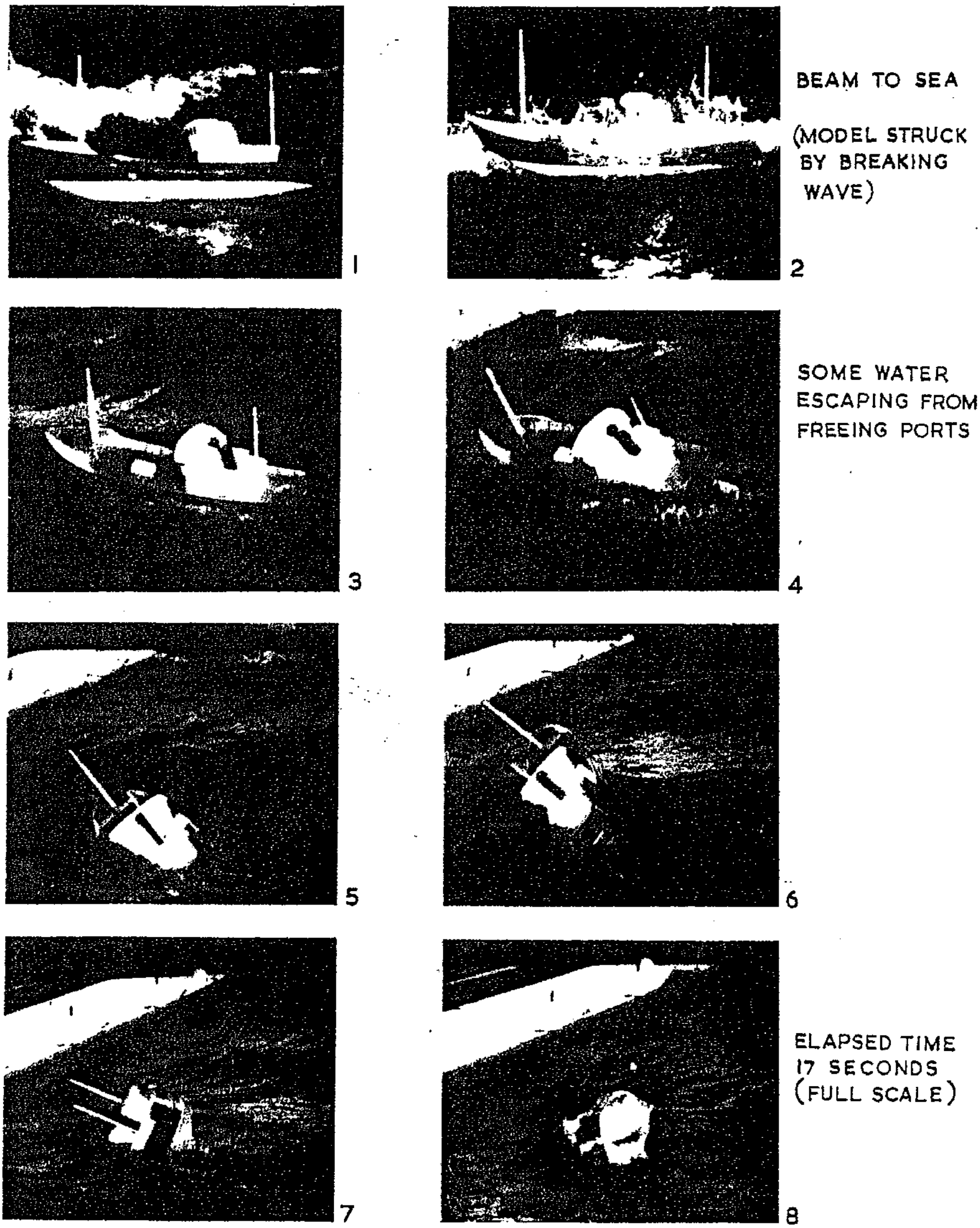


Fig. 11. Model A Capsizing Shortly After Taking Water Over Bulwarks

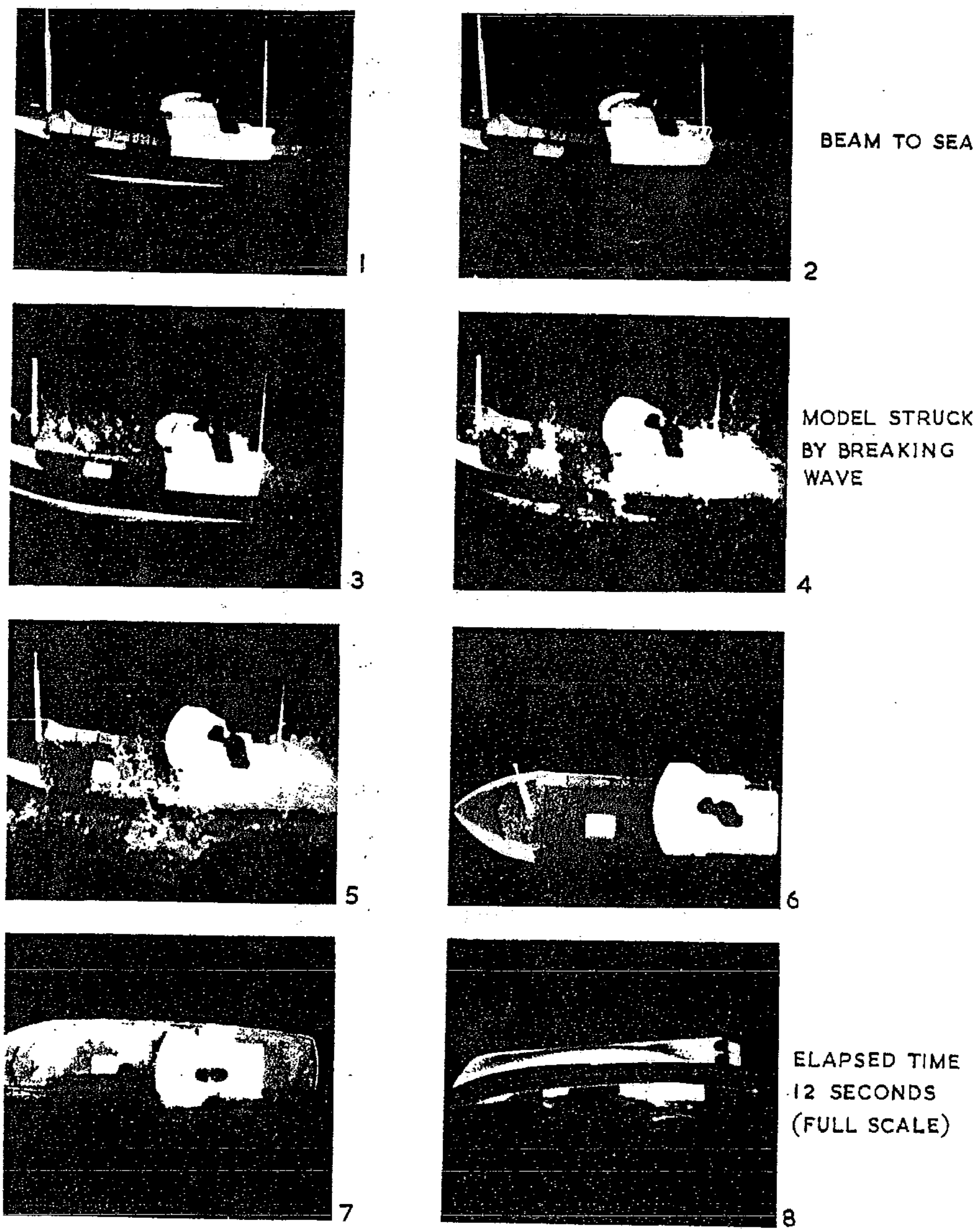


Fig. 12. Model A Capsizing Whilst Holding Station in Beam Seas

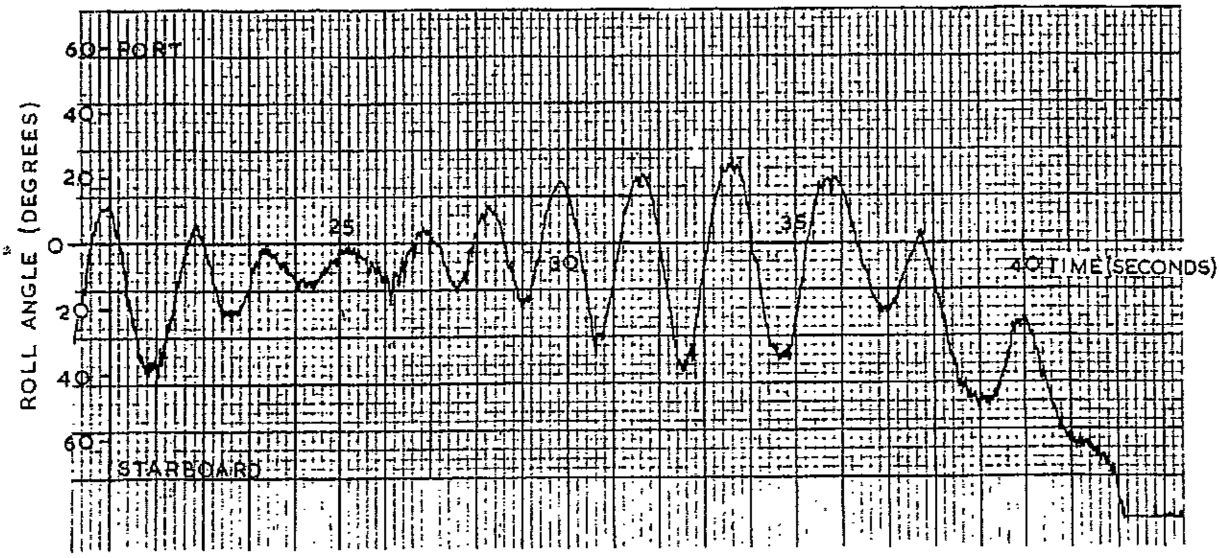


Fig. 13a. Sequence of Capsize for Model A Without Water on Deck

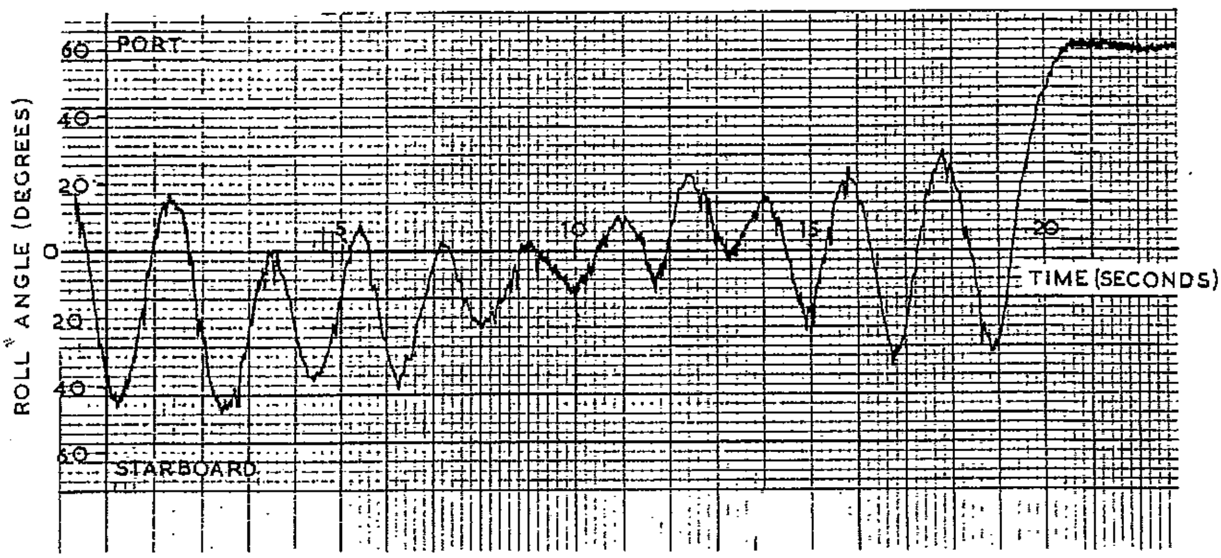


Fig. 13b. Sequence of Capsize for Model A after being Overwhelmed by a Breaking Wave

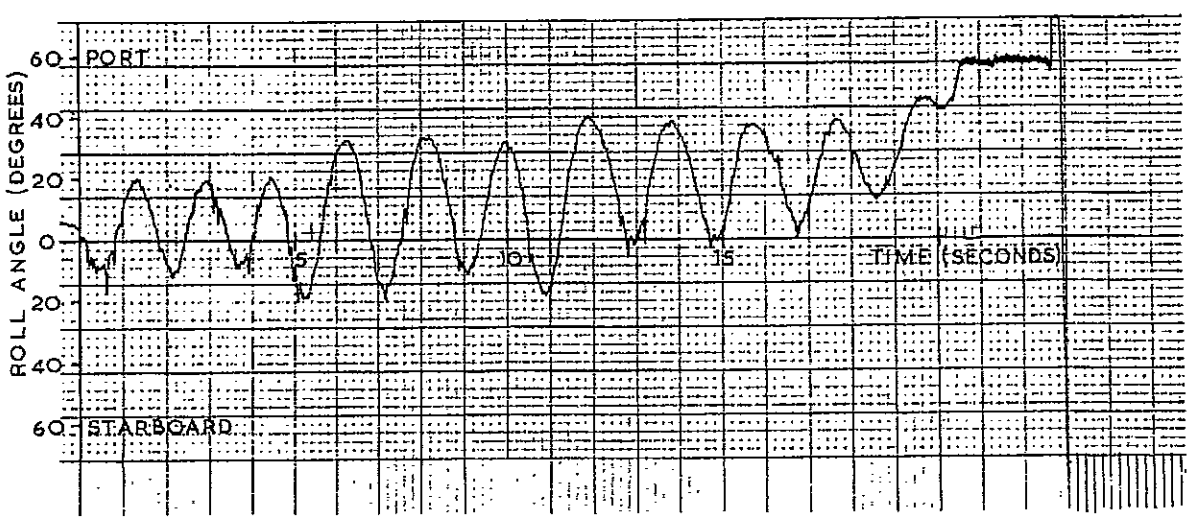


Fig. 13c. Sequence of Capsize for Model A Whilst Holding Station in Beam Seas

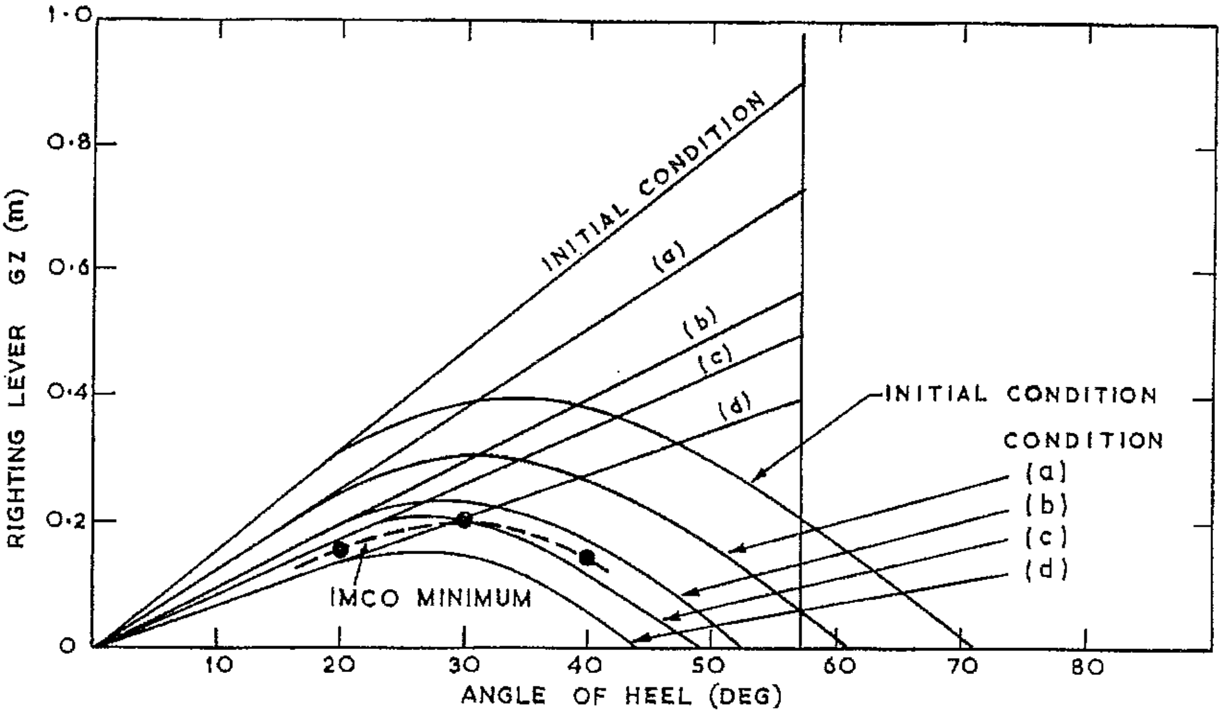


Fig. 14. Stability Curves—Design B

ACKNOWLEDGEMENTS

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It must be emphasised that all opinions expressed in this paper are entirely those of the author and do not necessarily reflect those of the Department of Trade or Department of Industry. The paper is published with the permission of the Director, National Maritime Institute.

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DISCUSSION IN GLASGOW

Mr D. G. M. Watson, B.Sc. (Fellow): The importance of this paper needs no emphasising. Dr Morrall names five ships lost in recent years and regrettably this is by no means a complete list. Whilst there may be several causes, or contributory causes, there must be a presumption that there is one over-riding cause and there is good reason to believe that this is capsizing in steep seas.

Vessels of the size of most of those lost have, together with smaller ships, fished the stormy waters off the coast of Britain for very many years. It is worth asking the question whether the rate of casualties has worsened in the last decade or whether we have become more sensitive to the tragedy brought to a fishing community by the loss of a ship and its crew. Can Dr Morrall provide any statistics bearing on this?

If the rate of casualty has increased, do we know to what to attribute it? The vessels on the face of it are bigger and apparently better founded—but is this true? Has the weather worsened? Do the other hazards in the sea area play any part—the increase in shipping numbers, sizes, speeds? The advent of radar encouraging speed in fog?

Does the increase in ship size and in the power of the engines encourage fisherman to stay at sea and continue fishing in gale conditions in which an early generation of fishermen would have either hove to or scurried to port?

I think some of these factors may contribute but believe that capsizing is the prime cause of a majority of the ship losses. I believe further that a significant number of modern vessels are much more prone to capsize than they ought to be.

In the early 1970s I discussed fishing vessel design with a very forthright skipper, at the time acknowledged as the top skipper of the Scottish inshore fleet. He prophesied a sequence of disasters saying that too many new fishing boats were being designed by inexperienced naval architects and built by yards without the background knowledge that existed in the traditional wooden shipbuilding yards.

He saw the prime stability needs as being a good GM and good freeboard. For GM he said that his current vessel had a GM of 3·8 ft and he would not want any less on his next vessel—this is needless to say well above any rule requirement.

On freeboard, he warned of the way in which fishermen will overload if they have a good catch and demanded a good freeboard amidships with a full cargo and a strong sheerline leading to good height of bow. He also recommended a pronounced rake of keel increasing the immersion of the rudder and thereby reducing the possibility of a broach.

His ideal vessel was very similar to vessel B and I believe he now owns a sister ship. Our own design developed at about the same time and built by an Irish shipyard has a strong resemblance stemming, I think, from a common mentor.

Focusing in on Table I, it is notable that the improved GM of B is obtained wholly as a result of the lower KG of that ship, as its KN is in fact somewhat less than that of vessel A.

An examination of the design given in Figs. 1 and 2 leads to no explanation of how the VCGs of these vessels differ by such a significant amount. The depths moulded are identical and the sheer of B is greater than that of A. The accommodation appears approximately the same, and the engine of A looks bigger than that of B.

One would conclude that the two VCGs are not compatible with one another, unless the explanation lies in the form of the lines of the two vessels with A being of a much more pronounced 'V' form.

It would be very valuable if the body plans of each of these vessels could be included in the paper as an examination of the lines is obviously highly relevant to drawing any sensible conclusion.

I presume that the VCGs of both vessels have been carefully checked by inclining experiments of surviving sister vessels as otherwise the deductions made from model tests carried out at GMs based on these apparently incompatible KGs may be somewhat misleading. Although I cannot believe the VCG difference of 0·57m between these two vessels, each of a moulded depth of 3·35m, I must admit to a strong preference to many of the features of the design of vessel B. If the difference in VCG is confirmed as a true difference, can any indication be given as to what features brought it about? Was it differences in the scantlings of the hull, the use of aluminium for the superstructure or differences in deck gear fitted?

The experiments seem to me to be totally valid within the constraints of the information relating to the ships on which they were based.

Mrs J. Faulkner: I am interested in the effectiveness of freeing ports.

The author mentions in Section 6. 2: 'The freeing port arrangements in the two models differed significantly', and infers, I think, that those on Model A are open scuttle type.

What type are on Model B? It is difficult to see from Fig. 2.

(i) Are there available data which indicate the most efficient size and number of freeing ports for a given well-deck area, freeboard and bulwark height?

(ii) What are the relative merits of freeing ports with top-hinge or pivot flaps compared with open ones?

(iii) Are hinged freeing ports liable to be forced shut as a low freeboard ship heels into waves?

From Dr Morrall's experiments, it seems that the time to clear the well-deck of water can appreciably exceed the time to capsize in certain sea conditions.

Professor D. Faulkner, B.Sc., Ph.D., R.C.N.C. (Fellow): With the loss of the 23 m trawler TARRADALE II during the recent weekend 10-11 February, this paper could hardly be more topical. Modest in its claims the paper is lucid and well presented. To me its most interesting findings are:

(a) confirmation that in breaking waves both models broached and ended up beam-on to the waves in defiance of the rudder action,

(b) the elapsed time for each capsize was only 10 to 20 seconds (full-scale) or about 2 to 3 roll cycles or shorter if the vessel is stationary in beam seas,

(c) GM clearly contributes to survival, but is not a sufficient criterion by itself,

(d) differences in hull design and trim are important to dynamical stability, large sheer and stern trim being beneficial,

(e) the IMCO criteria are insufficient to prevent capsize in certain possible sea conditions,

(f) Vossers' reservations [15] as to whether the small angle roll characteristics of a ship are truly represented in a model can now surely be regarded as allayed from the roll decrement test results.

The role of, and derived features of, freeing posts seem to be unclear, but this has been mentioned by a previous speaker.

Hopefully, out of this work will emerge revised IMCO regulations. One must always be tempted to try to formulate some statistical approach to dynamical stability [17,18] in view of the risk of flooding and the stochastic nature of the excitation. But observation (b) just mentioned must surely caution against such an approach, quite apart from the formidable analytical and validation difficulties involved. Fortunately, however, it does seem, on the evidence of these two model tests, that there is support for the widely held view (reflected in the current IMCO stability criteria) that the shape of the $\overline{GZ}$ curve has a strong influence on the safety of fishing vessels in extreme weather. This, therefore, warrants further study.

The $\overline{GZ}$ curves of Figs. 4 and 5 indicate that vessel B, which has adequate stability, differs from vessel A, which has inadequate stability in three main respects. For vessel B GM is higher, there is a rather less rapid fall-away from the initial tangent, and the range of stability is greater. These observations apply both to the calm water and wave curves, though here again the reductions in the latter are more favourable for form B.

It has recently been suggested by Brown [19], in a paper dealing with stability at large angles and hull shape considerations, that statistical relationships exist between some simple hull form characteristics and the righting lever at 30°. He has derived expressions for $\overline{GZ}_{30}$ for a variety of naval ships in various forms involving main dimensions, GM, KB, C_p and C_m. Unfortunately, it is not possible to derive with any accuracy these last three parameters from the data provided in the paper. It would seem that $\overline{GZ}_{30}$ might be considered to be an important parameter for trawlers in that it could give at least some indication of the shape of the GZ curve and of preferred features of vessel B which I mentioned just now. From Figs. 4 and 5 the values for vessels A and B are $\overline{GZ}_{30} \simeq 0{\cdot}2$ m and $0{\cdot}4$ m respectively, which are 27% and 44% of the respective GMs–again showing a feature in favour of vessel B. Roll angles of 30° or so seem to be significant from the capsize records (Fig. 13).

There is no doubt that expressions such as those derived by Brown are useful at the design stage when considering changes in form and dimensions. It would therefore be valuable if the author can be prevailed upon to examine whether Brown's equations for GZ_{30}, together with the well-known ones for KB and BM, are reasonably applicable for the trawler forms considered. At angles above 30° the above water form dominates the GZ curve, and it seems unlikely that simple expressions can be obtained for this part of the curve. In this respect an area based criterion may still be best, but the author's views on this would be welcome since water entrapped, and other effects (which are not easily predictable), may dominate real behaviour.

We are extremely fortunate in having such a timely, well presented and useful paper for our joint meeting. The author deserves our warmest thanks.

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Mr J. E. Tope (Fellow): As a visitor to the IESS I am grateful for the opportunity to take part in the discussion of this paper by Dr Morrall. As a result of its presentation here to-night and later in London, this paper will I am sure attract much comment and I hope, stimulate support for further investigation. Although the Transactions of both our Institutions contain many papers on the general theme of transverse stability, to which this is a valuable addition, it is clear that much remains to be done in the future.

In Section 2, Dr Morrall warns against using a single parameter to judge adequate stability and this is good advice. However in this practical world we usually have to use that which is reasonably attainable and not that which we would like and against this background I would ask Dr Morrall if:

(i) he would accept the observation in para 5, Appendix D, of the Holland Martin Report [6] that if a single parameter has to be used, the area up to 40° is as good as any; and

(ii) he would object to use of GM alone as a parameter when its value is judged against a formulation based upon an analysis of existing similar vessels with the aim of achieving, say 20 cm righting arm at 30°. Such a simplified criterion has been recommended by IMCO and adopted by some countries.

As I studied the stability curves in Figs. 4 and 5, I asked myself if it would be interesting to break down GZ into its two generally accepted components of $GM\sin\theta + M_0S$. Doing this showed me, in general terms, that for Model A the value of M_0S was never positive but for model B this particular stability element had slight positive values between 10 and 20°. This analysis was done because I recalled a paper [20] presented many years ago to the IMCO Working Group on the Safety of Fishing Vessels by the French delegation. This paper summarised the results of tank tests which had been carried out to explain, if possible, why two trawlers with similar GZ curves had not behaved the same way in a seaway, one having capsized. This investigation suggested that positive M_0S values (sometimes called form stability) up to 20° indicated a reasonable survival capability against capsizal in swell conditions.

Therefore when Dr Morrall is asking for more intact stability would it not be helpful to examine the stability which is already there, to see if deficiency of form stability is not a significant factor?

It would be helpful to the reader if it was indicated in Figs. 4 and 5 if the righting levers have been computed on the basis of fixed or free trim.

In presentation Dr Morrall amended his paper to note that both 'parent' vessels were steel.

Secondly, in Table I, are the datum lines for both models used to compare and measure trim horizontal i.e. the trims shown in the table are those which would be derived from draught of water marks at the perpendiculars?

I would welcome Dr Morrall's confirmation that in Section 5 I am invited to accept the hypothesis that because there is a reasonable relationship between the roll damping coefficients of Model A and a corresponding full-size trawler, it follows that the total behaviour of Model A is representative of the behaviour of the corresponding vessel at sea. The time that I spent, many years ago, working in an experimental establishment has left me with the belief that prediction of ship performance, in absolute terms, based upon the results of model tests requires the use of several ship to model correlation factors which have to be up-dated by ship trial analysis. Do such correlation factors exist in terms of capsizal? Therefore I find it difficult, on the strength of the argument in this paper, to discard my belief that in terms of seakeeping performance, model tests are more valuable on a comparative rather than absolute basis.

The interesting distinction between the experiments described in Section 6 is the fact that Model A, when manoeuvred in breaking waves, readily capsized. I would find it easier to accept the corollary that therefore the vessel would capsize, if, in some way, I could be convinced that the model manoeuvres could be described as being within the scope of prudent seamanship.

Turning to the question of stability criteria, I would point out that the RINA celebrated its own centenary before its Transactions contained reports of intact stability criteria which had been agreed internationally. The difficulty in finding a common view on what is adequate stability becomes more complicated when, as in this paper and also in most people's minds, it is linked with survivability because it is then necessary, in all fairness, to consider other things such as ship handling and efficient use of closing appliances. In order to take into account, in some way, these subjective factors the present criteria were based upon an analysis of past experience. As I understand it, the aim of current research is the establishment of relationships between sea state, principal ship dimensions and characteristics, in terms of probability of capsizal, and no doubt model tests as described in this paper make a valuable contribution towards expressing the mechanism of capsizal in mathematical terms.

Eventually this will lead to the improvement of the IMCO criteria and Dr Morrall is personally involved both nationally and internationally, in the work which has this as its objective. The United Kingdom Government has provided, and is continuing to provide, funds to support this type of research which advances knowledge in this field. Thus there is official recognition and support of the need to advance the state of the art and it is very likely that there are those who would wish to see more support, leading to quicker advance. As a potential user of the results of their efforts I stand on the touchline, listening and watching with considerable interest to the discussions of the mathematicians, hydrodynamicists and other experts and hope that they will be able to agree upon improvements to the IMCO criteria should these be necessary within a reasonable period of time.

On the basis of the results from testing two models, this paper concludes that provision of adequate intact transverse stability is a sound defence against capsizal. However, Dr Morrall goes further than that and although he has chosen his words very carefully, with a few caveats, the message that most of us will receive on reading this paper is that, again on the basis of these two tests, the IMCO criteria are inadequate for inshore fishing vessels.

My first reaction is to recall those vociferous inshore fishermen who, through the media, were telling the country that the Department of Trade was stupid to impose the IMCO standard of stability on vessels less than 80 ft (24·4m) because in their experience such a standard was much too high for their vessels.

My second reaction is to point out to Dr Morrall that if he says that under certain circumstances a particular statutory limit is too low for reasonable safety he must expect to be asked under what circumstances and by what absolute amounts would he increase specific parameters. Formulation of safety standards involves accepting the probability of an event occurring and in this case, would an increase in stability, reducing the probability of capsizal, also increase the probability of danger to men on deck due to a stiffer working platform? On the basis of this paper and the present position of the current research programmes I do not see the case for amending the present regulations now and I would be interested to hear if Dr Morrall agrees with this view. I ask the question because it has to be recognised that a heavy responsibility lies upon representatives of Government and Industry alike when they quantify minimum acceptable levels of safety, a commodity which can never be guaranteed in the absolute sense.

There is another source of uncertainty to be appreciated if one is using stability parameters to compare the behaviour of a model, including capsizal under laboratory conditions, to what might have happened in the case of an accident at sea when there are no survivors who can speak with knowledge about the loading condition at the time of the casualty. Under these circumstances it is only possible to make intelligent assumptions and so estimate the corresponding stability characteristics. This is critical, since in some cases only a small adjustment to the estimated position of the vertical centre of gravity might be needed to move the vessel from one side to the other of a safety line defined by stability parameters. In such cases it should not be assumed, even by Courts of Formal Investigation or similar legal inquiry, that the calculations are perfectly representative of the actual vessel at the time of the assumed capsizal.

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Mr A. W. Gilfillan, M.Sc. (Member): Dr Morrall and his colleagues at NMI are to be congratulated for this most interesting paper which draws us back to a timely consideration of the fundamentals upon which much of our current practice is based. This paper is in essence a practical demonstration of an effect which is all too often described by lengthy equations.

I found the basic conclusion of the study comforting, as I have always been surprised that a minimum range of stability was not included as one of the IMCO stability criteria.

By inspection of Table I, it can be seen that the KM, the sum of the last three lines, is significantly different for the two designs, due to the difference in beam and the adoption of a transom stern for design A. Could the author please supply values of KB, BM and C_{IT} for each vessel? Can the author also indicate the accuracy to which the GM of the model is measured and what measures were taken to obtain the correct radius of gyration for the model?

In Fig. 6, the author compares the righting lever curves for designs A and B and adjusts that for design A to have the same sheer line and GM as design B. To complete the comparison, it would have been more sensible to adjust the breadth of design A to that of design B. The comparison is based on constant GM and in this respect design B is superior to design A. However, design A with modified sheer line can tolerate a higher KG than design B to obtain the same area under the GZ curve or range of stability. As both vessels have the same depth, design A would appear to be capable of accepting a higher load on deck, which is an important feature in the operation of small fishing vessels.

Fig. 9 gives a dramatic impression of the effect of a breaking wave on model A. The wave itself looks quite steep. Can the author indicate the maximum wave steepness obtained in the wave spectrum used for the experiments?

It is interesting to compare this study with results obtained from similar work. For example, the work on the coastal tanker EDITH TERKOL, described by Kure and Bang in their paper aptly titled 'The Ultimate Half Roll' (Ref. 21), results in curves being prepared of the lowest righting levers for the load and ballast conditions for which the vessel did not capsize, (Fig. 15). The only similarity between the two curves is the angle of vanishing stability. In this case there is, of course, a displacement effect and the dynamic stability for the two curves up to 60° is 190 and 130 tonne-metre radians for the ballast and load conditions, respectively.

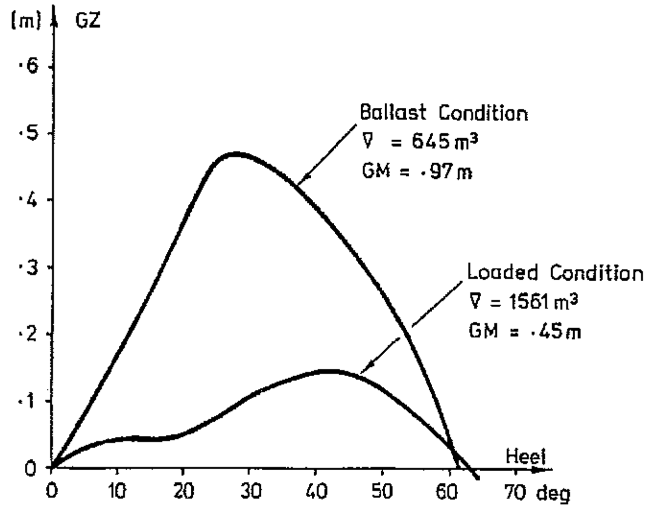

Fig. 15

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Professor J. F. C. Conn, D.Sc. (Fellow): There must be general agreement that ships should be stable in all practical working conditions. Although the theory of the stability of floating bodies was established, notably by French investigators, very many years ago, their analyses were restricted to the vessel at rest in still water.

Experience has shown that smaller ships have higher casualty rates due to instability than larger ships, but we have found it exceedingly difficult to decide what are the necessary and desirable standards of stability in any arbitrary case. Apart from the wide variety of ship forms and the different storm characteristics of the waters in which they operate, there is uncertainty as to the worst conditions for loss of stability of a ship in its position relative to the seaway. The author has shed some light on this aspect of the matter for his inshore fishing vessels.

Since a mathematical analysis is unlikely to supply all the required answers to the general problem, and is certain to be both difficult and complicated, recourse can be made to experiment. In these days of manoeuvring tanks and radio-controlled models, it should be possible to advance our knowledge of stability in various sea states and this the author has succeeded in doing.

His two models are of low block coefficient and representative of their class. My only criticism of them is that comparisons would have been easier if the freeing port arrangements had been identically similar for both ships.

The two types of capsize are most interesting. The familiar dangerous conditions arising from breaking waves, quartering seas, beam seas and broaching are clearly demonstrated.

One is left in no doubt as to the value of ample GM, ample righting levers and a wide range of stability. But how is 'ample' to be defined? In his paper [22], Kato recommends the work ratio as a criterion for capsizing. This he defined as the ratio of useful reserve dynamical stability to the maximum kinetic energy of the ship after being struck by the gust which caused the shipping of water. The value of the ratio was given as 1·53 and based upon an actual capsize at sea.

It is startling to find that the IMCO criteria are so inadequate for the vessels considered in the paper, especially since in fishing operations there can be additional heeling moments caused by trawling, etc.

One hopes that the author's experimental investigations will be continued and extended into a wider field in a systematic manner. Dr Ferguson and I contributed our mite to this subject in a paper [23] read here in January 1970. Our tests might be repeated with radio-controlled models, one of fine and one of full form, to clarify the first effects of motion on stability. Experiments could then be made, as in the present paper, with the two models in various sea states, to explore the separate effects of, for example, GM and freeboard. I am tempted to guess that the results of circle trials with these models would help to clarify our understanding of stability in a seaway.

We are greatly indebted to the author for an interesting and informative paper.

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Professor C. Kuo, B.Sc., Ph.D. (Fellow), Let me begin by stating that ship capsizing is an extremely difficult problem to solve effectively because we need to balance safety against design efficiency. The matter is not helped by the fact that we can all understand what is a 'capsize' situation and what is a 'non-capsize' situation, i.e. whether the ship floats upright or upside down, and this leads to the erroneous belief that a mathematical formulation of the problem would be equally clear-cut. The main hurdle is the large number of parameters or variables involved—damping, inertia, restoring, excitation. To incorporate even a few parameters would make the capsizing study very challenging.

I do not believe the problem is insoluble. Indeed I am very confident that we can make good progress in tackling this problem. However, it is very important that we encourage able people to work on capsizing in a clear-sighted and logical way. Otherwise there is the danger of further confusing the problem.

As regards this paper, I must admit I am in a dilemma on two accounts. Firstly, I do not believe the motion behaviour of a vessel can be explained simply by juggling a static item such as metacentric height and it is essential to treat capsizing from a vessel motion viewpoint. Secondly, I have experienced great difficulty in understanding much of the content. This latter statement may come as a surprise to Dr Morrall and, indeed, some of those present here, since the term 'an incomprehensible paper' is usually reserved for describing a publication which contains a large number of mathematical equations liberally sprinkled throughout the text. One can certainly not criticise the author for that as his paper contains one solitary equation and has lots of pretty pictures! My real problem here lies in dealing with the large number of assumptions presented in the paper. If I accept them as sacrosanct, then there would be no difficulties. However, if I challenge any one of these assumptions and no explanations are given in the text I immediately find myself in a difficult situation. Let me therefore take this opportunity to highlight just a few of the points by asking the author for explanations of the following:

(i) Justifications for Performing Physical Model Experiments

After the Introduction and IMCO Stability Criteria sections, the author immediately goes on to describe the models and the experiments and then presents his results. The key assumptions here are: (a) physical model experiments can yield guides to the reality of ship capsizing, and (b) these particular experiments being carried out at the National Maritime Institute can fulfil this requirement. I am sure Dr Morrall must have excellent reasons for wanting to perform experiments but he must not assume that we can understand the reasons or agree with his views if he provides no explanations. My question is therefore:

'Will the author please let us have the justifications to show that his set of physical models can be used correctly to examine ship capsizing?'

(ii) Presentation of Model Results

Fig. 8 is a plot of wave spectrum against wave frequency and at first glance looks most impressive. It shows, for example, that the 'natural roll frequency of Model A' coincided with a peak of the 'breaking wave spectrum' and indirectly provides a reason for capsizing of the model. However, a closer study of this Figure raises many questions in my mind and these are:

(a) Darbyshire's spectrum is given for two 'sea states'. My understanding is that Beaufort numbers cover a range of wind speeds, e.g. Beaufort 8 has a speed range of 34-40 knots. What does Dr Morrall mean when he presents a spectrum corresponding to wind force 8?

(b) Dr Morrall chose Darbyshire's wave spectrum approach because 'they are commonly used in model experiments for the prediction of the performance of ship designs expected to operate near the coastal waters'. Can the author confirm that the two models selected for the experiments actually operate totally in coastal waters? Can he indicate whether the designs do operate in non-coastal waters?

(c) No mention is made of encountering. Can we assume that the effect of encountering is not included? If so, will Dr Morrall explain the relation between a one dimensional spectrum and randomly manoeuvred model tests?

(d) The author mentions that 'breaking waves defy exact definition'. Does Dr Morrall imply this is true for everyone? If so, is he not aware of the publications by Longuet-Higgins and others on breaking waves? Why is there no comparison between the measured 'breaking wave spectrum' and theory?

(e) The graph gives the natural frequency of the model to be 0·16 Hz and I find this value to be very low for a model of the proportions used in the experiments. Can Dr Morrall outline the details of these calculations or present time histories of the actual measurements?

Suppose we assume that the model natural frequency is incorrect, then it will not coincide with the 'breaking wave spectrum peaks' and, bearing in mind the questions raised here, can Dr Morrall give us his view regarding what he believes to be the real message of Fig. 8?

(iii) Modes of Capsizing

In Section 1, the author states that 'the purpose of this paper is to report on model experiments which could help explain one of the modes of capsize at sea for inshore trawlers'. May I ask Dr Morrall to explain which mode of capsizing his paper is explaining?

Mr A. C. D. Crow, M.Sc.: The writer would particularly like to thank the author for presenting this paper, because the subject treated is a very complex one and one for which analytical methods have not as yet been able to produce realistic results. The author has used model experiments and has taken measures to ensure that the model-ship correlation is reasonable.

Firstly drawing some conclusions from the results of the experiments: Fig. 14 shows the GZ curves for model B where the GM has been reduced in steps to establish a corresponding condition to model A. The capsizing condition for the given imposed sea state lies between (b) and (c) corresponding to GM values of 0·574 m and 0·504 m respectively. This lies very close to the IMCO standard shown. Fig. 4 similarly shows the IMCO standard close to the GZ curve for model A which was below its capsizing condition. This seems to imply that the IMCO standard is inadequate even as a minimum standard for this type of ship, no margin of stability being allowed for. It is somewhat disturbing that this has also been shown [24] to be the case for other types of ship, viz. EDITH TERKOL.

The author suggests a modification to the IMCO rules in specifying the angle of vanishing stability as at least 60°. This is the least that could be done although at 60° the watertightness of a fishing vessel is somewhat in question. Maybe a minimum GZ value at an angle at which down flooding could reasonably be avoided, say 45°, should be specified. The minimum angle of maximum GZ should also be increased from 25°. These considerations are apart from the need for a general increase in the reserve of dynamic stability.

The author compares the stability of models A and B and concludes that 'the fault lay not so much in the hull shape but rather in the CG position which led to a simple deficiency in GM'. I would draw his attention to the capsize conditions of the two models; Fig. 14 shows model B to be just above the capsize condition with GM = 0·574 m and the author indicates in Section 6·3 that a GM = 0·908 m for model A was just above the capsize condition. These two conditions appear to be corresponding conditions for the two models. Thus it can be inferred that the difference in the hull shape has the same effect on stability as a change of GM = 0·908 − 0·574 = 0·334 m. This is almost twice the difference, 0·176 m, between the static GM values for the two original models. This indicates that the hull shape seems to have a more significant influence in the dynamic situation, and the difference between the two model hulls is an important consideration.

The writer would like to conclude with a proposal that two standards of stability for fishing vessels should be established:

(i) For vessels already in service which would be a minimum standard with an adequate margin for safety. This would avoid heavily penalising such vessels.

(ii) For vessels in design which would then have a built-in reserve of stability over and above that in (i) so that the next generation of fishing vessels would be free of this stability problem, as far as possible. The extra cost involved in this would be minimal, say 2% of initial cost [25].

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Dr S. Kastner: Dr Morrall has presented us with new results of model experiments on capsizing, and I find it quite delightful to know that experimental research work in this still rather underdeveloped area with respect to a theoretical solution is still proceeding. However, in comparing these experiments in a tank with the open water tests conducted at Lake Ploen in Germany during the sixties [26] or later in San Francisco Bay [27], or more recently in Japan [28], one might expect to have fewer problems in measuring the model behaviour and the respective model environment in a towing tank. Since Dr Morrall's results are mainly based on observations, with relatively little experimental data given, it is hard to evaluate his results and to put them into some perspective.

Certainly the comparison given of the two typical trawler designs has been a worthwhile task, and the quasistatic GZ-curves have been thoroughly investigated. This comparison of the righting arm curves of different ships shows that only the same conditions of the ship ought to be compared, always applying the same procedure, i.e. taking trim into account, or accounting for the trim equilibrium at heel, in order to make calculated righting arm curves according to any given criteria comparable.

In reading Dr Morrall's paper, a few further questions arise. Can the measured data be evaluated with respect to the ship speed, ratio of encounter frequency to natural roll frequency, resonance considerations? What was the dynamic impulse of the moving weight mechanism? A similar method was used by Hattendorff in tests on the stability of fast cargo liners in Hamburg in 1974, and he gave corresponding data in his report [29].

It seems to be the tenor of Dr Morrall's paper that as soon as sufficiently large waves broke, model A was in danger. However, even breaking waves do not defy theoretical analysis and modelling, see the recent work by Longuet-Higgins [30].

Why do the measured spectra, as shown deviate to such an extent from the initially suggested theoretical spectra? What is the reason for choosing a wave length of 70 m?

Dr Morrall deduces from his experiments that—at least in breaking waves—broaching-to played an important role. I fully agree on that, but then the steering characteristics of the model come in, and they must be modelled according to scale. By Froude's law, the rudder response rate scales according to $\dot{\delta}_M = \dot{\delta}_S (L_S/L_M)^{+1/2}$, which results in about 9°/sec for these models. It is doubtful whether remote controlled manual steering allows for such a high helm response rate, therefore an autopilot would be advisable.

The differences in the freeing port arrangements between models A and B have been pointed out by Dr Morrall. It seems to be important to make the outflow of water through freeing ports similar to the full scale ship. Shipping water and bulwarks add another parameter to the problem, which needs more attention. Therefore, in the open water model tests cited, only the buoyant ship body was modelled.

From the open water model capsizing tests, a lot of capsizing features in natural seaway conditions have already been detected and published, and some qualitative agreement with numerical simulation has been obtained, among others the broaching-to capsize in following or quartering sea with extremely steep and breaking waves has been measured, similar to Dr Morrall's results. Now we are at a stage where specific model testing must be launched in close collaboration with developing theory, in order to determine certain coefficients in particular or to check specific theoretical results. In that way we might get as much information as possible from any further experiment for the benefit of advancing our knowledge on the capsizing problem. The planning of experiments is a very important part of the total work, and I suggest closer co-operation between those involved in stability research.

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Mr D. M. Clubb: Firstly, I feel the paper would have been better entitled 'Capsizing of Small Fishing Vessels', as neither of the vessels considered is a trawler. Design A appears to be mid-water (or pelagic) trawler, whilst Design B is a seiner-trawler rigged for seine-netting only. The differences are important as different types of fishing involve differing systems of working, ship-handling, gear, catch-handling and resultant forces acting on the vessel.

Secondly, whilst not knowing anything of Derbyshire's characteristics of waves I would point out that it would be inappropriate to confine the investigation to the behaviour of the vessels in coastal waters. Vessels similar to Design A operate in the S.W. Approaches, off the West Coast of Scotland, around Shetland and all over the North Sea. Vessels of Design B range even further afield. One seine-net vessel went across the Atlantic to Newfoundland in the Spring of 1978 to undertake a programme of research for the Canadian Government. Three Scottish great-line vessels, little bigger than Design B fished off the East Coast of Greenland in November and December 1959, whilst others regularly work grounds at Rockall, Faroe, etc.

The question of freeing ports has given rise to some considerable discussion. The criteria by which fishermen judge freeing ports are very different from those of the designer. The first criterion is that the opening must be small enough to prevent marketable fish from going over the side. Secondly, it is a distinct advantage if there is an opening through which stones, weed, 'duff' and undersized fish can be shovelled. Ridding the deck of water is not seen, by many fishermen, as being of great importance. The floating bulwarks, as in Design A, best fulfil the first criterion, and are widely found in wooden seine-net vessels, whilst the rectangular ports best fulfil the second criterion. It must be remembered that water on the deck of a fishing vessel does not have an unimpeded flow to the scuppers as the deck is divided up by pound boards, and there are other obstructions such as fish boxes, seine-warps, baskets and so on.

It must be remembered that a fishing vessel is quite unlike a merchant vessel in that the fishing vessel loads her cargo at sea, i.e. major changes in the ship's stability take place at sea. I feel that the minimum criteria and selected loaded conditions for judging the stability of merchant ships are not suitable for application to fishing vessels and the stability book produced to comply with the demands of the DoT is of little help to most skippers because they do not understand it.

Calculations have been made for a 72 ft wooden seine-net vessel somewhat similar to Design B. The stability book gave information for the following conditions:

(i) leaving port
(ii) leaving fishing grounds with a catch of white fish
(iii) leaving fishing grounds with a catch of pelagic fish in bulk
(iv) arrival in port with a catch of white fish
(v) arrival in port with a catch of pelagic fish in bulk.

However these do not cover the most vulnerable condition, namely having made a big 'drag' after having spent some four or more days on the fishing grounds looking for fish (Condition (vi)). In such circumstances quite a large part of the fuel and fresh water would be expended and ice melted. An additional investigation was therefore made. For the purposes of calculation it was assumed that there were 300 boxes of fish on deck (some 15 tons), 150 empty boxes (1 ton) and the cod-end with the equivalent of some 50 boxes of fish was being lifted inboard by means of the gilson (the upper block being some 32 ft above the keel). (It must be remembered that there have been great advances in net design in the last 10 years and a 'drag' of 300 boxes is not exceptional and 400 boxes not unknown). Allowances were made for free surface in the fuel and water tanks, but no allowance was made for the weight or free-surface effect of any water on deck.

In Condition (i) the vessel had a GM of 3·16 ft (larger than the initial GM of Design B—see Table II of Dr Morrall's paper) but in Condition (vi) this was reduced to some 2·2 ft (between Conditions (a) and (b) in Table II). Thus it can be seen that the GM can quickly approach the IMCO minimum and could well fall below this level even in calm conditions. To avoid this risk, the skipper of this vessel now watches his fuel state carefully and refuels if he is unable to find fish early on during the trip.

Finally it was suggested at the meeting that vessels may be lost because they are staying at sea when they should be running for shelter. The vessel discussed above made for a Danish port in the gale in October 1978 during which the GRAMPIAN GLEN was lost. The skipper reported that he had considerable difficulty in steering in the short steep seas whilst in the long entrance channel. This would suggest that even vessels with cruiser sterns and good stability characteristics are not free of the danger of broaching-to. In fact, a seaman may prefer to stay out in deep water with plenty of sea-room rather than put the ship at greater risk in worse seas while making for the safety of a port.

Dr R. F. McLean: The analysis of stability criteria is seldom an easy task. The wide range of parameters involved in ship stability certainly does not make the task any easier. Dr Morrall has given a clear insight into many aspects of the problem. His paper has a distinct experimental flavour which considerably enhances our knowledge on how vessels are liable to behave. However, with the wide range of parameters available should not an experimental approach have been preceded by an extensive theoretical study? This would have highlighted the important parameters and how they affected stability. This is undoubtedly the most economical way to carry out an overall study since the equivalent experimental investigation would take several decades and would incur large costs. Could Dr Morrall comment on the relative costs involved? The choice of an experimental model is made much easier after the theoretical studies are completed and puts more credibility on the experimental and theoretical results.

Dr Morrall has covered a number of parameters but the way he chooses to simulate wind effects perturbs me. Can he explain why he chooses a moving mass to simulate wind effect? There are many aspects of this which should be investigated. The moving mass with not simulate force; it will give a mass as a time varying parameter which leads to a complex analytical problem and will certainly not enhance the understanding of stability.

Mr A. M. Ferguson, M.Sc., Ph.D. (Fellow): Referring to Section 5, I note that Dr Morrall used the technique of force rolling the ship to determine the damping coefficients. The rolling period is, of course, also directly related to transverse stability and it has been shown [23] that the rolling period and hence transverse stability will vary with forward speed, as will most hydrostatic properties. It has also been noted as a result of some recent work at Glasgow University by the writer and Mr M. R. Renilson (Ref. 31) that the transverse stability will further vary depending on the ship's position relative to the sea wave position, whether it be a following sea, beam sea or head sea.

I would be interested to hear the author's opinion on the effects of forward speed on stability with regard to the results of the research contained in this paper.

REFERENCE

31. Ferguson, A. M. and Renilson, M. R.: 'The Variation in Transverse Stability of a Ship at Speed in a Following Sea'. To be published.

Mr S. Lipiner (Junior Member): The problem of safety of life at sea is particularly urgent and of interest since it involves not only loss of money i.e., the vessel and cargo, but mainly because it involves loss of life. Statistical data and well known cases, some of which were mentioned in the paper, constantly remind us that we cannot build a ship to sail with absolute safety.

Stability is one of the most important aspects of ship safety. It has long been the subject of intensive study and many experiments have been conducted to move the theory or to provide naval architects with information to improve the stability of future designs.

The phenomenon and mechanism of capsize is still unsolved and I hope it will not take many years to clear the ideas behind it. So any article or paper on this subject is warmly welcomed and the author is to be congratulated for presenting us with such a paper.

However, I have a few questions to ask. The way I understand it is that only the effect of waves was taken into account. I wonder if experiments or calculations have been made (together with the effect of waves) taking into account the effects of loads such as icing, shift of mass on deck and wind pressure. Certainly, these would influence the time it takes the ship to capsize and the angle of vanishing stability.

Another aspect which interests me is whether an attempt was made to develop a new or existing theory on the mechanism of capsize based on these results, and if so, what was the outcome.

In his first conclusion the author suggests that 'the fault and main cause contributing to capsize, lay not so much in the hull shape, but rather in the CG position'. Could this mean similar modes of capsize for other ship types?

The paper shows that stability calculations based on a statical approach are misleading and that dynamical phenomena should be included. The model tests of capsizing phenomena form a very good basis for studying various modes of capsizing and consequently for the selection of dangerous situations. The conclusions to be drawn from these tests are of great value in this respect.

Mr D. A. Vassalos (Junior Member): It was very interesting to note the comments and measurements given in Section 5. I would just like to take this opportunity to ask Dr Morrall a few questions regarding the roll damping characteristics.

(a) Equation (1) is very loosely used in the literature and there are doubts regarding its validity and accuracy. What justifications does the author have in using this relation?
(b) How did the author obtain the two curves shown in Fig. 7?
(c) In the last sentence of Section 5, Dr Morrall states: 'The results indicate that the roll motion of the model can be regarded as reasonably representative of the vessel's behaviour at sea'. Bearing in mind that the measurements are only up to 4° maximum, can one really use such a low range of rolling angles to draw conclusions that could apply to a range of considerably larger rolling angles?

Mr B. N. Baxter, M.Sc., Ph.D. (Fellow): I enjoyed reading the paper and listening to the short delivery by the author and would like to ask two questions.

The curves in Figs. 4 and 5 show the various values of GZ in the statical condition and a wave condition with the crest of the wave amidships.

G. S. Baker and Miss E. M. Keary wrote an INA paper in 1918 which dealt with the effect of the loss of stability of a ship on a wave when the crest of the wave was parallel to the centreline of the ship. The same G. S. Baker and D. M. Baker, son, wrote an INA paper in 1941 dealing with the loss of stability on a wave as the wave approached the ship head on. For similar ships, the loss of stability in the former case is about three times the loss in the latter. The curves are drawn for the less onerous case and one of the slides shows clearly that the model is being broached and about to turn over when on the crest of a wave that is parallel to the centreline. Knowing that the loss of stability is greater in this case, I would like to know why calculations were not considered under these circumstances?

The criteria proposed by Rahola in 1939 were based on the results of enquiries on 34 ships which had either capsized or were lost, and he proposed minimum values of GZ at various angles of heel. These proposals now form the basis of several other national criteria. The Inter-Governmental Maritime Consultative Organisation Stability Regulations, which were proposed in 1962, are similar in principle to the Rahola criteria.

I think, therefore, that great care should be taken not to extrapolate the evidence from two models only to include the statement that the IMCO criteria are insufficient for small inshore fishing vessels. It may have been better to have stressed that the IMCO criteria should be regarded as the absolute minimum and any standards used should be in excess of these.

The final paragraph of the paper states that if the IMCO recommendations for minimum stability are to be reconsidered, then greater emphasis should be placed on the maximum righting moment and its position on the stability curve. I am not certain what this means. The curve of righting moments is obtained by multiplying the ordinate of a curve of statical stability by the displacement, and if the displacement is considered to remain constant, then the curves are similar, differing only by a vertical scale factor. Because of this, I cannot understand the emphasis being placed on the position of the maximum righting moment because its position remains unchanged.

At a meeting on the Stability of Small Ships held last year at the NMI I suggested that the cost of constructing small trawler hulls full size and carrying out stability experiments on them was relatively small and should be undertaken to try and resolve the argument about the degree of comparability between the motions and stability of a model and the full size ship. I repeat this suggestion and conclude by stressing that more full scale work on stability is needed as well as the continuation of work on models.

DISCUSSION IN LONDON

Mr D. Bailey (Member): In his first conclusion, the author mentions subsequent experiments conducted on Model A. I was closely involved with most of the work described in this paper and in fact three extra conditions were examined in which both displacement and GM were increased. To complete the record it may be of interest if the results obtained are added to the paper.

Model A was loaded to give the corresponding ship conditions shown in Table III.

TABLE III

Condition	Displacement (tonnes)	GM (m)	% Increase in Displacement	Freeboard at Bow (m)
(i)	178·5	0·875	6·5	2·1
(ii)	222	0·948	32·5	1·95
(iii)	239·5	0·981	42·9	1·6

In each case hull stiffness increased and the corresponding stability curves are plotted in Fig. 16. As a comparison the initial curve for the model and the IMCO minimum are added from Fig. 4.

The experiments were carried out in the same breaking waves and in no instance did capsize occur. Of particular interest was condition (iii) in which the freeboard was greatly reduced due to the extreme displacement. This resulted in very considerable deck wetness yet the model survived. Now, although the effect of extra displacement cannot be completely isolated from these additional experiments it does seem that hull loading has a significant part to play and I would welcome the author's opinion. In Fig. 6, had design A been modified (at the same GM) to produce less sheer and therefore less freeboard, then the stability curve would have fallen below the original and represented a condition of apparent capsize. However, the additional tests have shown that with reduced freeboard but increased displacement capsize is avoided.

Finally, if we examine the curves in Fig. 16, all three represented in the additional experiments lie comfortably above the IMCO recommendation and I agree with the author in his

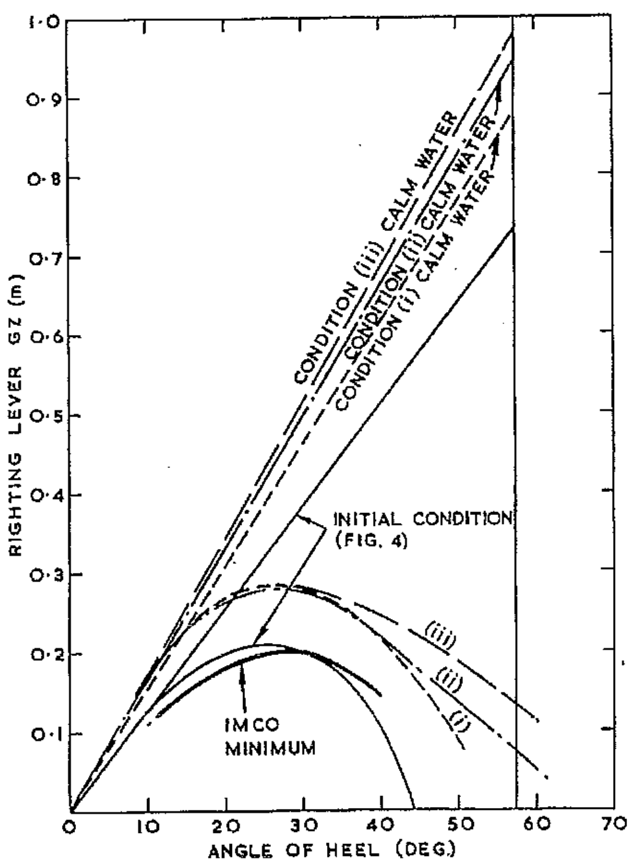

Fig. 16. Stability Curves for Design A (Additional Conditions Tested)

suggestion that current IMCO guidance for the smaller fishing vessel needs to be re-examined. More evidence is of course required, but if similar experiments to those described can be initiated over a range of typical modern designs then the results will offer information upon which such a revision could be based.

Mr H. Bird (Fellow): This paper was originally intended to be a joint paper but I felt it necessary to withdraw since publication of certain conclusions seemed premature both from my personal point of view and that of the Department which I represent.

I agree strongly with this statement, Section 1, bottom of first column. I have made it myself many times. With criteria of the IMCO type one cannot explain, even in any qualitative sense, capsize in relation to any definition of sea or weather state.

The scale of the models was chosen to be $1/_{15}$ (1 = 1·7 metres). This was really a compromise between what was desirable, in my view, to explore very severe sea states, limits of the wavemaker and the need to accommodate propulsion, control and motion recording mechanisms in the model. In January 1975 when we were planning this model programme it was calculated that to test up to the sea state corresponding to Force 12 would entail reducing the model to about 0·70 m., i.e. a scale of $1/_{32}$ approximately. Choice of the most suitable scale for such models is clearly a most important consideration.

In the conclusions which have arisen from these experiments it must be remembered that the sea state was limited by the model scale to about Force 8. (The IMCO Regulations do not state any maximum).

I find it difficult to accept the statement (Section 4) that a close comparison between calculated and measured GZs at angles greater than 16° cannot be expected. Differences can only be due to calculation or experimental inaccuracy or different assumptions about hull geometry. This kind of disagreement could, in my opinion, have been eliminated, and Dr Morrall and I are now trying to resolve the matter.

In Section 6.1 it is stated that—'the transom stern did not appear to handicap progress'—I witnessed some of these experiments and it was my impression that the transom stern model A shipped more water on deck and rolled with a more lively motion. This behaviour is reflected in Section 6.2.

As stated in Section 6.2 the freeing port arrangements were quite different between the two models, the open slots in model A allowing almost free flooding of the deck when submerged.

The statement (Section 6.3) that 'the differences in performance between model A and model B now appeared to be due to differences in hull design' is not very helpful. The essential differences are:

(i) Stern shape

(ii) Freeing port design

(iii) Height of G in relation to waterline.

The experiments do not elucidate any of these effects explicitly. The statement in Section 6.3 that 'the character of the stability curve played an important part' is most probably true but I think it could be misleading, on the basis of this very limited project, to imply that the other principal features I have mentioned can be neglected. Naturally it would greatly simplify the development of much needed improved stability regulations if we only had to think about the properties of the GZ curve.

When this project was first conceived over four years ago great importance was attached to both the effect of stern shape and the effectiveness of freeing ports. No conclusive guidance has emerged on these features, probably through lack of sufficient time to investigate fully. I feel strongly that, as freeboard is reduced, freeing port area should be increased up to 100% of bulwark area at zero freeboard (i.e. no bulwarks at all). The round stern model B had ports with shutters hinged at the top and these proved very effective in reducing water entry. I realise that with shutters there can be a maintenance problem due to corrosion but with use of modern materials, e.g. nylon bushes, I think this could be overcome.

The importance of the third feature of higher VCG of model A was not established either, apart from its influence on GM. It may be noted from Table I that the much higher VCG in Model A is, to a large extent, compensated by a higher metacentre. In a report of some capsize experiments carried out in the United States (USCG Ref: CG-D-4-76, page 59) the importance of high KG/draught ratio is mentioned in relation to sway coupling. From Table I this is about 1·27 for model A compared with 1·04 for model B. Perhaps the author or other hydrodynamicists would like to comment on the possible importance of this feature of model A.

The foregoing comments are not intended to detract from the value of the research reported here or in any way to discourage the author, but rather by way of a caution to interested readers that much more work needs to be done.

These comments and opinions are my own and do not necessarily represent the official views of the Department of Trade.

Professor G. Aertssen (Fellow): The experiments in breaking waves on these two small trawlers are extremely interesting. It is indeed surprising that in a coastal Beaufort 8 a trawler having a GM as high as 0·73 m and meeting the IMCO standards may capsize. This prompted me to make comparisons with two trawlers, BELGIAN LADY 45 m in length, beam 8·6 m, and JOHN 51·5 m length and beam 9·4 m. Both have sailed from Belgium to Iceland for 20 years. Seakeeping trials were carried out and a paper was presented to NECIES in 1964 [32] which contained the stability curves for the ships, not in still water but poised on a wave of ship length and a height of 3 m. Positions were considered on the crest and in the trough, and for each position the righting moment was calculated as well as the heeling moment due to free surface effects, water on deck, icing and wind. The means of the crest and trough positions were taken since, generally speaking, the ships were not on the crest long enough to be endangered. It is appreciated that the righting moment for both ships is in excess of the heeling moment between 15 and 45°. As it would be difficult in a Conference to obtain agreement on the topics of such calculations, the mean of the righting lever GZ is also given for each ship and these stability curves are above IMCO requirements. As a consequence, as I said on Professor Burcher's paper [33], the still water stability curve for both trawlers is again above the IMCO minimum stability curve. Finally, the behaviour of BELGIAN LADY and JOHN confirms the validity of the IMCO requirements.

Here I have a point on Dr Morrall's paper. There is much to be learnt from a comparison of the behaviour of trawlers A and B in breaking waves. It is significant that such design features as freeing ports may contribute to a different sea behaviour, and Dr Morrall is congratulated on having discovered the reasons for the better sea behaviour of trawler B, disregarding the stability curve. In this respect Fig. 14 showing the stability curve of trawler B for decreasing values of GM and the comparison with IMCO is of great importance. This figure, with its basic information on capsizing, might well serve for a contemplated revision of the IMCO criteria. Trawler B is of better design, as she withstood a breaker of 4·9 m, the GM being no more than 0·57 m. Curve b is exactly the stability curve of the BELGIAN LADY, and this is a second reason for selecting this curve b as a basis for future discussion. I suggest an increase of 20% on the IMCO righting lever. Automatically the angle of vanishing stability will be at least 55°, as it is on BELGIAN LADY. Actually, this 20% might be considered as an allowance on IMCO for all kinds of inferior design or workmanship—freeing ports, skylights, etc.

REFERENCES

32. Aertssen, G., Ferdinande, V. and de Lembre, R.: 'Service Performance and Seakeeping Trials on Two Conventional Trawlers.' Trans. NECIES, Vol. 81, 1964-65, p 37.

33. Burcher, R. K.: 'The Influence of Hull Shape on Transverse Stability.' Paper 9, RINA Spring Meetings 1979.

Mr J. A. Tvedt (Fellow): I am very pleased to have the opportunity to discuss a paper such as that by Dr Morrall. Like a previous contributor, I also would like to ask whether we can be given the body plan, so that we could readily compare the paper with that by Dr Wright and Mr Marshfield [34].

Dr Morrall referred to the lively behaviour of the model in the film. It could be debated whether it is because of the lively behaviour, but it would be helpful if we could have some record, perhaps on the supplementary experiments, to indicate just what kind of motion the model experienced. I believe that the supplementary model experiments were a little more instrumented so that such records could be obtained. Perhaps one could get a comparison between the last motions and the first motions that we saw on the film.

On the question of range, there seems to be general agreement between Professor Aertssen, Professor Dahle and Dr Morrall, and possibly also on the position of maximum GZ. The HELLAND-HANSEN GZ curve seems to fall just short of the BELGIAN LADY, which equals curve b in Dr Morrall's Fig. 14, so that maybe one need not go quite as far as suggested in Professor Dahle's paper as regards the range, at least above a certain size of vessel.

REFERENCE

34. Wright, J. H. G. and Marshfield, W. B.: 'Ship Roll Response and Capsize Behaviour in Beam Seas.' Paper 10, RINA Spring Meetings 1979.

Professor E. A. Dahle: I was impressed by the model, by the instrumentation and by the film shown by Dr Morrall. I think that the film could be a good guide for designers as well as for skippers operating such ships to show capsizing of small vessels. I would indeed like to obtain a copy of the film for my own institution.

I have an important difference that I would like to point out regarding the waves used in Dr Morrall's experiments, and those used by us in the HELLAND-HANSEN case [35]. Dr Morrall is talking about a spilling wave of about 5 m, which is not so dangerous as the waves that we used in our experiments. As we showed in our film, we tried both types of waves. In the first part of the film we showed the dangerous plunger type, and the 5 m wave was enough to capsize the HELLAND-HANSEN. However, in the big tank, where we could not produce any plunger waves, we had to use the spiller, like Dr Morrall, and then we had to increase the 'capsizing' wave up to 7·5 m. Consequently, Dr Morrall's excitation is smaller than ours.

Regarding the creation of plunging waves in the open sea, I think that a current has to be present. Tidal currents are strong far out from the coast in some locations in Norway, and these may produce the plungers when the current is running against the waves. The design of the UK vessel shown in Dr Morrall's film, is one of the worst I have ever seen for operation in the open sea. The forecastle is open, the deckhouse aft is not extended to the sides, and, what is more important, when it encounters rather moderate waves, it turns upside down. That is what I do not like at all, because the vessel becomes a death trap. You have almost no chance to save your life in that case. If only the vessel would rest like the HELLAND-HANSEN at below 90° of heel for some minutes, there is a chance to save yourself, as 10 of 12 people did in the HELLAND-HANSEN case. Hopefully, designers who see Dr Morrall's film will learn that lesson.

Finally, with regard to the freeing port area, it is obvious from our paper that increase of the freeing port area reduces the heeling angle for the smaller waves, but for the largest plunging wave that we used, 6·5 m, there is no difference between the heeling angle with or without freeing ports, so that the freeing port area is of no interest for the 6·5 m wave.

My conclusion would be that the trend of the design shown in the paper is in principle much like the design of HELLAND-HANSEN, and that I think such design is dangerous. The HELLAND-HANSEN was 22 years old. Two other Norwegian vessels, which most probably capsized in 1976 and 1978, were of similar design. Those vessels were 20 and 16 years old. They had survived until the day they capsized, and the skippers were competent as regards handling. Thus, it may not prove anything at all that a vessel can survive for 20 years. The day the capsizing happens, it may well be the responsibility of the designer.

My questions to Dr Morrall are

(i) which erections are included in the GZ-curves shown in Figs. 5 and 6?

(ii) is it correct that in the film the vessel turned upside down, even with an intact wheelhouse?

(iii) what is Dr Morrall's opinion regarding the design of vessels A and B as regards their safety against capsizing?

REFERENCE

35. Dahle, E.A. and Kjaerland, O.: 'The Capsizing of M/S HELLAND-HANSEN. The Investigation and Recommendations for Preventing Similar Accidents'. RINA Spring Meetings 1979, Paper 11.

Mr D. Clarke, B.Sc. (Eng), Ph.D. (Member): I should like to make a brief point on breaking waves. We have heard various opinions about how spilling breakers or breaking waves occur, and whether one needs to have a current or shallow water, to create the necessary conditions. I would like to say that I have listened to similar arguments on many occasions in connection with the United Kingdom wave energy programme, and Professor Kuo mentioned the work of Professor Longuet-Higgins, a recent paper from whom has demonstrated that plunging breakers can occur in deep water, so that one does not necessarily need the aid of shallow water or currents. Stephen Salter, at Edinburgh University, using his small deep tank in connection with wave energy research, has been able to produce these plunging breakers experimentally by the correct combination of several waves of particular wave length and phase. He is able to produce plunging breakers at will, just at the right spot in the tank, which appears to varify the theoretical work of Professor Longuet-Higgins.

Mr A. W. Gilfillan, M.Sc. (Member): Since my contribution at the presentation of this excellent paper in Glasgow and receipt of the pre-prints of the other papers we have had today I have been wondering whether the traditional curve of righting lever is the best presentation for dealing with large angle stability. Fig. 6 compares the values of GZ for designs A and B and although the two designs have the same dimensions the comparison is made about two different positions of KG. It is the effect of KG on GZ which I would suggest is not fully demonstrated in the traditional presentation and I have therefore been searching for an alternative method of presenting GZ curves up to very large angles. This is particularly necessary for self righting lifeboats where one is interested in the stability through 360°.

Fig. 17 shows an alternative presentation of GZ obtained by plotting the relative locii of the centre of gravity G and the point Z about the keel K. The distance GZ is always parallel to the waterline and this presentation enables changes in GM on the value of GZ to be readily demonstrated as the distance from point K to the intersection between the line drawn

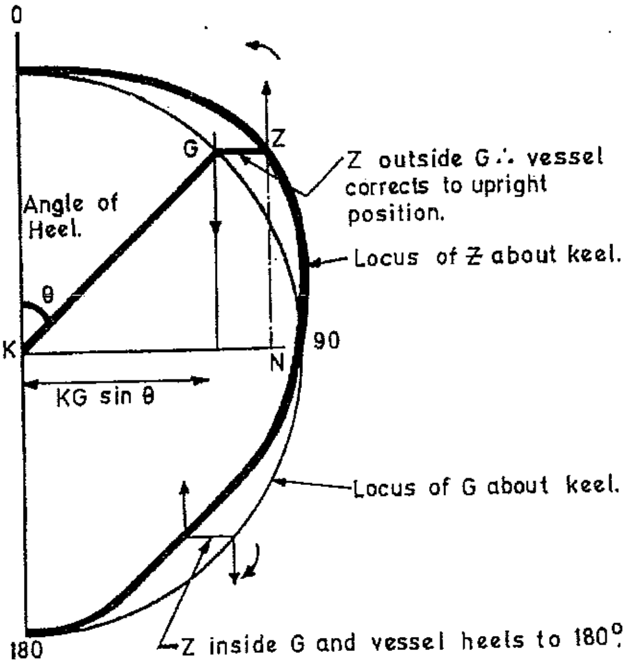

Fig. 17. Stability Angular Plot

normal to the waterplane through Z and the 90° radial is the value KN. Z will always travel up and down this vertical line according to the position of G. A further feature of this presentation is that the vessel will always return to the upright position when the locus of Z is outside the locus of G and will heel over to an upturned point of stable equilibrum when the locus of G is inside the locus of Z.

Fig. 18 applies this presentation to the righting lever curves for designs A and B for which M_A and M_B are the respective metacentres and G_A and G_B are the respective centres of gravity. The KM value for A is substantially greater than that for design B and at small angles the KN values for design A and design B are about the same, so that it is only the KG sin θ correction which results in the values of GZ for design A being less than those for design B. At angles greater than 30° the value of KN for design A falls below that for design B and that together with the KG sin θ correction

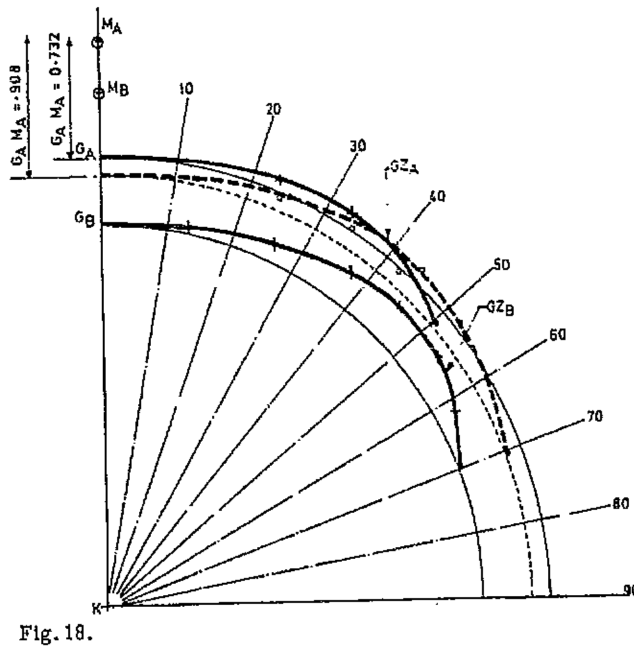

Fig. 18.

results in the lower values of GZ and a lesser range of stability being obtained for design A. When the sheer line for design B is applied to design A then the value of KN for design A increases substantially over that for design B. The fact that design A has a higher KM permits a greater value of KG to be obtained for the same GM and hence the KG sin θ term for design A is greater than that for design B resulting in the GZ values for design A being less than those for design B. The importance of this presentation is that all these factors are demonstrated readily on a comparative basis on a single diagram.

These comparisons demonstrate firstly the illusory nature of an increase in stability through an increase in KM and secondly the comparative merits of transom stern for design A and increased sheer line of design B. It is interesting to note that if one studies the Fig. 16 of Ref. 35 and plots the centres of gravity and buoyancy on each of the diagrams it is apparent that the lever BG sin θ acts to heel the vessel rather than to correct it and that the righting lever curve will only become effective once the crest of the breaking wave has passed the trawler.

A similar capsize might be initiated with both designs A and B and the righting lever curve could only act to upright the vessel once it has been pushed to an extreme angle by a breaking wave. It appears obvious that the only criterion which is relevant in this event is the range of stability.

WRITTEN DISCUSSION

Mr W. Mackay (Member): The use of model data in evaluating seaworthiness appears to break new ground, and if valid in this application, could lead to a re-appraisal of stability standards for this class of vessel. The fact that the IMCO criteria are no longer recommendations but statutory minimum criteria has perhaps led builders to lower standards in some cases. Owners' demands for the installation of the sophisticated deck machinery and catch preservation equipment which are now current may have been accepted on the basis that the vessel still complied with the criteria, albeit with a reduced margin.

A striking point to emerge from consideration of Figs. 4 and 5 is the variation in loss of initial stability on a wave crest. Whereas model A shows a 30% reduction in GM, the loss for model B is only 8%. The extreme knuckle incorporated in the aft body of model A, which gives a very full waterplane in calm water, appears to be responsible. The resultant large value of transverse metacentre could lead to acceptance of a high vertical centre of gravity on the basis of what appears to be a reasonable GM. It would also appear that a simplified stability criterion based on freeboard, breadth and GM is not applicable to hull forms of this type.

The low value of vertical centre of gravity of model B, while desirable, would be difficult to achieve with present-day crew protection and deck machinery installations on multi-purpose fishing vessels.

The value of this paper would be greatly increased had further tests been done for the vessels with minimum freeboard representative of bulk catch operations. This might also illustrate the relative merits of initial GM and freeboard in determining whether permanent ballast should be fitted.

Mr M. R. Renilson, B.Sc. (Junior Member): The author is to be congratulated on a stimulating and useful paper. He identified two modes of capsize and it is the classic mode in which the writer is interested. At Glasgow University we have done some experiments to determine the amount of transverse stability lost when a ship is poised on a crest in a following sea—and compared this with the change in transverse stability at the same speed in calm water. We were looking for the possibility of a loll angle developing which would set up a yawing moment causing a capsize by the 'broaching' mechanism described by Paulling et al [27,36]. It is interesting to note that the author did not associate any capsizes with 'broaching', as an initial heel angle will produce a yawing moment which will either cause a sharp turn, or require a large rudder angle to maintain a steady course, both of which will cause a further heel which will contribute to a capsize by this mode. The expected pattern in this case would be an initial heel followed by a further heel in the same direction leading to the capsize. Comparing Figs. 13(a) and 13(b), it can be seen that the capsize without water on the deck follows this type of pattern rather than the build up in amplitude of the oscillations resulting in a capsize when there is water on the deck. The writer would like to ask the author, therefore, if it were possible that the classic capsize mode could be related to a heel induced yawing moment in the way described above and observed by Paulling.

The other point in which the writer is interested is the effect of the superstructure on the righting moment. Although it is appreciated that the superstructure cannot be classed as fully watertight, it would appear that only a small amount of water could enter it in the time span involved in a capsize. Therefore, it must be appreciated that the superstructure will increase the righting moment at high angles of heel and this is not accounted for in the rules. At first glance this would appear to be all right as the superstructure is adding to the stability and so can be thought to give a 'safety margin'. However, since the rules are influenced by full scale results, this 'safety margin' is already assumed in the regulations. The fact is that the successful ships have this 'safety margin' which is not required, or even discussed, when assessing whether a new ship will be as safe. This leads to the false assumption that the new design has a safety margin above this, owing to its superstructure. Basically, then, using the present rules, a ship less safe than the existing standards could be generated by reducing the efficiency of its superstructure. The write advocates further thought into the effect of the superstructure and would like to ask the author how he allowed for the superstructure openings in his model tests, and whether he would consider re-testing the same models with varying superstructure.

Finally, the writer would like to thank the author for an interesting paper which has provoked much thought by all those concerned with the safety and operational characteristics of ships in severe seas.

REFERENCE

36. Paulling, J. R. and Wood, P. D.: 'Numerical Simulation of Large-Amplitude Ship Motions in Astern Seas'. Seakeeping 1953-73, SNAME Pub. June 1974.

Mr J. Donaldson (Fellow): I have always been amazed that so much effort over the years has been put into the quest for speed and so little into the seaworthiness of small vessels and it must be heartening to all seafarers that Dr Morrall and his colleagues at NMI are now turning their attention to this important aspect of ship design. As the Royal Navy found out many years ago there is little merit in designing the fastest vessel in the world if it can operate only in limited wave conditions.

In the breaking wave situation Dr Morrall states that in following seas both models broached in defiance of the rudder action. What action was taken?

As the rudder depends for its steering effect on water flowing past it from forward to aft would the author agree that if the direction of flow is reversed there could result a loss of directional control and at worst the rudder could have the reverse effect; in other words putting the rudder over in the normal way to correct an incipient broach could possibly accentuate the broach rather than correct it?

The designers of sailing vessels consider it highly desirable to build into the hull form good directional stability and as Model A was fitted with a moving weight mechanism did Dr Morrall consider inducing heel into the model when running in calm water in order to ascertain its ability to hold a straight course and, if so, could the results be made available?

Does it necessarily follow that a vessel with good directional stability is less likely to be broached in a following sea or is the problem considerably more complex?

On the premise that the more seaworthy and seakindly a vessel is made the less expertise is required to handle it I feel that greater effort needs to be made by naval architects to produce balanced hull forms and in this context Admiral Turner's Metacentric Shelf Theory INA 1937 deserves mention; even if one cannot accept his reasoning the end result seems to produce a satisfactory vessel.

I suspect that the difference between the vertical centres of gravity of the vessels quoted in Table I is mainly due to the heavier deck machinery on Model A and additional permanent ballast in Model B but perhaps this could be confirmed or explained.

Mr K. G. Evans, R.C.N.C. (Fellow): I have read this paper with great interest and noted that two modes of capsize were identified, viz on the crest of a longitudinal wave and also in a beam sea.

The first mode is quasi-static and primarily due to loss of waterplane inertia, although other factors are involved. The second mode is primarily due to the weight of water on the weather deck causing an upsetting moment aggravated by the bulwarks trapping it and further enhancing the free-surface loss of inertia effect. It is my tentative view that without bulwarks, if stability is adequate to avoid capsize in the first mode, it is very unlikely to occur in the second. Would the author care to give his opinion on this please?

For many years it has been the practice to relate strength of ships to the rough sea environment but for a century naval architects have generally been content to use the artificial concept of the curve of statical stability in flat calm seas, as a criterion for capsizing in waves. At the International Conference on Stability of Ships and Ocean Vehicles at the University of Strathclyde in 1975, I proposed that a routine quasi-static calculation for stability be carried out (particularly for small ships) for the wave crest condition for an L/9 wave profile, and I gave an example[37]. I have also used a different 'operational envelope' concept of stability, using a Stability Diagram, successfully used for warship designs I have been associated with over the past 30 years[38]. Most recently it was employed for the ISLAND Class offshore patrol craft, which have superior stability characteristics (including a damaged condition) to similar trawlers afloat. Would not the author agree that trawlers would be far safer by following such a mandatory design procedure?

REFERENCES

37. Proc. Int. Conf. on Stability of Ships and Ocean Vehicles, University of Strathclyde, 1975.

38. Evans, K. G.: 'Designing Ships of the Future'. IESS, 1972.

Mr D. J. Eyres, M.Sc. (Fellow): From the practical fishing boat designer's point of view, the difference in the stability curves is quite revealing. Can the author please indicate for what service condition the GM values are obtained, and are erections included in the KN curves? Does the steel boat (A) with high KG have any permanent ballast which is not uncommon as opposed to the similar wood boat?

Most stability data prepared and approved for small fishing boats include stability curves lifted directly from the KN curves at a mean draught displacement. The paper would appear to indicate that the effect of stern trim can adversely modify the stability curve and this would not normally be recognised.

Concerning the question of behaviour in waves, the various observations on shipping and clearing water from decks also on broaching and helm response, evidence suggests that these might not be entirely correct because of the scaling effects.

The conclusions of the paper, apart from indicating the desirability of establishing an angle of vanishing stability, probably 60° in any criteria, must also bring into question the relaxation of the IMCO requirement for the maximum GZ to occur at 30° or more in the case of fishing boats where a lower angle of 25° has been effectively permitted.

Mr J. E. Tope (Fellow): In his contribution to the discussion in London Professor Aertssen drew attention to curve b of Fig. 16, which he described as IMCO minimum plus 20% and pointed out that he considered this a reasonable standard on the basis of the safe operating experience of two named vessels.

It is interesting therefore to mention that this same standard viz IMCO plus 20% is required, under the national regulations of the Netherlands and United Kingdom, to be met by new fishing vessels using a particular fishing method. The type in question are those which tow their fishing gear from the outboard end of a boom (sometimes called beam trawlers).

This higher than IMCO minimum recognises that the outreach of the boom increases the upsetting moment if the trawl becomes fastened against an obstruction on the sea bed. The Dutch regulations would further increase this stability standard in the direct ratio of the vessel's engine power divided by $0{\cdot}8L^2$ (L is length in metres) to take account of a greater pull in the gear due to higher engine power.

Contrary to an opinion expressed by another author at the meeting this action does indicate that some regulatory authorities are prepared to seek a standard higher than the minimum recommended by IMCO. However, this was done in respect of a particular type of vessel and only after a careful analysis of the relevant operating experience.

To date there has been no action to invite Governments to accept this as an increase right across the board for all fishing vessels covered by the IMCO recommendation. In fact this recommendation invites Governments to give special consideration and if necessary, take into account those adverse influences eg beam wind, icing up etc which affect stability. Therefore it is necessary to distinguish those circumstances which are general and suggest the need to raise the present minimum standard from those which are localised and special which can be taken into account by adding to the existing standard. Dr Morrall says that under 'special circumstances' the IMCO minimum criteria might be inadequate; could he indicate into which of these two categories he would place the circumstances which he has in mind?

Mr J. Nicholson, B.Sc. (Member): May I first congratulate the author on his choice of topic. It must surely appeal to all naval architects, and give rise to many requests for more information, eg how accurately can the model tests be expected to predict a vessel's actual capsizal point in relation to the waves assumed, and what scale effect is to be expected? Has the effect of varying wave height or proportion been investigated in relation to the capsizing point?

As a matter of interest, could the author explain why the GZ apparatus and the SIKOB program cannot be expected to agree on the value of GZ after deck edge immersion, and which he considers to be more correct?

The first conclusion, ie that a GM = 0·732 m provides insufficient roll stiffness leading to capsize, is rather puzzling. In Section 6.3 model B survived with GM = 0·574 m and yet the author suggests that 0·732 is insufficient. Surely a comparison of the GZ curves for GM = 0·732 m shows that the lower GZ of model A around 30° is the real difference causing model A to capsize? It seems most unlikely that the present IMCO criterion of GM = 0·35 m could be raised above 0·732 m as suggested since many vessels operate with less GM.

Criteria related to high angles are purely academic and ignore the practicalities. Such angles do not occur in normal rolling, and if they did, they would:

(a) render the crew inoperative,

(b) cause shift of weight, enhanced by the failure of securing and pounding arrangements,

(c) permit the entry of water through the various openings which may not always be closed, or stay closed.

Suggestions of an angle of vanishing stability as a criterion are, I believe, an example of treating effect rather than cause and should therefore be discouraged.

It is stated that greatest emphasis should be placed on the maximum righting moment and its position on the stability curve; however this emphasis on the peak is misleading. For example some fishing vessels have the maximum GZ near 90° whilst some tankers have their maximum below 25°, but which is safer? It is the actual values in the 20°-40° range which seem significant with the initial stiffness up to 20° largely controlled by the current GM requirement.

In the Summary and Introduction, criticism of the simple statical approach of the IMCO criteria is implied; however the Conclusions do not appear to offer a radically different system. Is further work being undertaken?

A further point which deserves mention is the substantial uplift in the GZ curve (illustrated by Fig. 6) due to sheer and trim, a feature which designers would do well to incorporate. United Kingdom fishing vessels do not have a load line nor the calmness of harbour waters to guide their loading. Skippers must use their judgement as to when to cease loading and this will be influenced by the midship freeboard amongst other things. It is important that designers do not reduce the sheer below existing practice, thereby removing this 'hidden' reserve.

Mr J. F. Leathard, B.Sc., Ph.D. (Fellow): I want to consider the results of this interesting paper along somewhat different lines from those taken by the author. At various points in the paper there is an inference that the apparently inferior performance of Model A in breaking waves is due to some inherent characteristic of the hull form. In Section 4, for instance, stern trim and lack of sheer are suggested as factors contributing towards the relatively poor still water stability curve. Only in Item (i) of the Conclusions does the author touch upon what I believe to be the fundamental difference between the two vessels, namely, the much higher position of the centre of gravity in Model A compared with Model B.

This theme may be developed most simply by deriving the values of righting lever for both forms on the assumption that KG = 0, i.e. the so-called KN values. Unfortunately, the information given in Figs. 4 and 6 for Model A does not allow the calculation to extend beyond about 45° but the figures up to this angle of heel are shown in Table IV below.

TABLE IV

Angle of Heel	KN m	
	Model A	Model B
10°	0·67	0·61
20°	1·28	1·21
30°	1·78	1·70
40°	2·13	2·07
45°	2·22	2·19

The available range of data, therefore, shows a slight superiority for Model A. Consequently, if the VCG of Model A could be lowered so as to coincide with that for Model B—and this is, at least, feasible, since the depth moulded is the same in each case—very similar GZ curves would be obtained for both models.

The stability characteristics for survival discussed in Section 6.3 may now be analysed in an attempt to find some consistency between the two models. Increase in the GM for Model A to 0·908 m, to coincide with the initial value for Model B, does not lead to similar positions of the VCG. This is primarily due to the relatively greater BM for Model A associated with the fuller waterlines of the transom stern; approximate calculations suggest a waterplane inertia factor of about 0·66 for Model A compared with about 0·59 for Model B. The corresponding GZ curve for Model A may be derived from the given data, as shown in Table V.

TABLE V

Anlge of Heel	GZ m
10°	0·15
20°	0·26
30°	0·29
40°	0·22
45°	0·13

Comparison with the GZ curves for Model B in Fig. 14 shows this survival curve for Model A to lie more or less within boundaries formed by the GZ curves for conditions (a) and (b) from about 20° angle of heel up to around 45°-50°. At angles below 20°, the curve for Model A improves since it has to run into the origin at a greater slope governed by the higher GM. Since condition (b) for Model B apparently represented a lower limit for survival and condition (a) is specifically mentioned as producing survival, some consistency is apparent in the characteristics of the stability curves.

On the basis of the above analysis, the author's comment at the end of Section 6.3 that 'survival did not depend entirely on the absolute value of GM, but rather that the character of the stability curve played an important part' is strongly endorsed. Indeed, an examination of the relevant survival data gives some guidance as to what the character of the stability curve might have to be. In terms of the IMCO format, these are shown in Table VI, together with the corresponding IMCO values for comparison.

TABLE VI

Item	IMCO	Survival for Models A and B
GM	0·35 m min	0·60/0·90 m
Area 0°-30°	0·055 m rad	0·090 m rad
Area 30°-40°	0·030 m rad	0·045 m rad
GZ at 30°	0·20 m	0·29 m
Vanishing stability	—	50°-60°

The comparatively wide range of GM values is a reflection of the variation in waterplane fullness. It is possible that the relatively greater roll stiffness associated with the top end of the range could be combined with an angle of vanishing stability at the bottom end of the 50°-60° range and vice versa.

Clearly, any guidance based upon experiments with only two models must be tentative and firmer recommendations must await the availability of more extensive data. But it seems to be indisputable, as stated by the author in his conclusion, that for the size and type of fishing vessel tested, operating in the realistic sea states adopted, some increase in the minimum IMCO stability levels is required. This is, perhaps, the most important lesson of the paper which should be pondered by all practicing naval architects.

Mr M. J. Napier (Fellow): Congratulations are due to all concerned for instigating these studies as this kind of work should certainly lead to greater safety. We are often asked for advice on ships with stability problems, and as one example was the TRIDENT's sister ship this paper is doubly interesting to us.

At the TRIDENT investigation I suggested that in the following seas applicable when she disappeared the stern could be lifted for a significant period allowing any water which came on deck to run forward where it could not escape--sheer was minimal, the whaleback was largely open, freeing ports stopped well abaft the bulkhead and evidence was given that deck pond boards were in position. Thus it seems essential to extend freeing ports well forward, unship deck pond boards on passage, or at least cut drainage slots (perhaps with plastic coated wire mesh) and probably fit a watertight bulkhead near the aft end of a whaleback. The author's comments would be appreciated.

Differences in characteristics between fishing vessels are very pronounced (see the VCG figures for models A and B although the depths are the same). On such vessels the characteristics are typically well above IMCO criteria except when large quantities of fish are aboard and thus the TRIDENT had often survived worse loading conditions.

We are very pleased that the Department of Trade has recently tightened up on stability for new fishing vessels, effectively raising the standard required. It is refreshing to compare their attitude with another leading European fishing nation.

Almost all fishing vessels with poor stability have too little freeboard, and extra ballast reduces it further. Thus ballasting can make the stability worse, but generalisations are dangerous--because of the format of the rules, raising the wheelhouse on one vessel made compliance with IMCO criteria better (it was a high freeboard vessel where the maximum permissible VCG rose as displacement increased and the effect of the extra steel weight more than offset the higher VCG). One of the commonest faults in fishing vessel construction is for builders to use heavier scantlings than necessary. Unless there is a large factor of ignorance in the design calculations, this has ruined the stability of many craft.

We would like to see a minimum bow height insisted upon and cannot really see why the authorities basically ask for the maximum GZ to occur at 25° or more on a fishing vessel where hatches are open at sea, but at 30° or more on a cargo ship (or fishing vessel used for carrying cargo). Insistance on 30° would necessitate laying up most of the fishing fleet, but it seems desirable at least to aim for it on new designs. It is occasionally possible to have better areas under a GZ curve with 25° maximum than 30°, but...........

Everything is of course a compromise and to a fisherman a good sea boat is essential—how often we are told 'I have no stability worries about her—she has a grand comfortable slow roll'. What is the answer—strict rules or education?

To conclude we have only one criticism—Design B is of steel, not wood as indicated. Otherwise just congratulations and thanks.

AUTHOR'S REPLY

The interest that has been shown in the many aspects of the stability problem, and in the safety of fishing vessels in particular, confirms the view that the subject is of great importance. This paper has attracted a wide ranging discussion of generally high quality which has appreciably enhanced the paper's value. For this the author is grateful and encouraged to continue on this important subject of small ship survival.

A study of the stability parameters for the casualties used in defining the IMCO standards shows that in some cases the actual stability at the time of casualty, judged to be due to insufficient, was in fact higher than in the homogeneous arrival condition. This paradox was repeated by an examination of the data for some existing ships; this showed that for some cases in the homogeneous arrival condition the stability was below that derived as a minimum from the analysis of casualties.

The significance of this overlap between levels of stability which are considered 'safe' and 'unsafe', apart from illustrating the problem associated with this type of analysis, indicates that stability is not necessarily the prime agent that determines safe operation. Recent casualties of fishing vessel have strengthened this view and other factors are considered to be equally effective, notably

(i) the efficient use of closing appliances, and

(ii) prudent navigation in a seaway.

The latter covers the question of being at sea in bad weather conditions. However, the higher the standards of stability and seaworthiness the longer the vessel can remain at sea in bad weather and thus there is a direct relationship between stability and safety.

There may be some arguments about the actual standards of the IMCO stability criteria, but good standards of stability should attempt to provide a margin against all occurrences, both anticipated and unforseen i.e. extreme weather conditions of a specified severity and the unknown human element. Because of these unpredictable factors, every effort should be made to provide standards of stability well in excess of any stipulated minimum.

Reply to Discussion in Glasgow

Mr Watson questions whether the rate of casualties has worsened over the last decade or whether we have become more sensitive to the resulting tragedies. The list of UK fishing vessel losses from all causes is given in Table VII and needs no explanation.

One fact that is now emerging from the present financial climate is that fishermen are now fishing in higher sea states than in the past, resulting in the vessel and crew being exposed to a greater risk of loss. Mr Watson's point on the manner in which the increases in ship size and power of engines are used in today's fishing vessels is most relevant.

Mr Watson mentions the importance of background knowledge in good fishing vessel design and the author is in full agreement with this point of view. The stability requirements of a top Scottish skipper as outlined are sound principles that should not be overlooked by any fishing vessel designer.

The VCGs of both vessels have been carefully checked by inclining experiments on an actual vessel in one case and a surviving sister vessel in the other. The reason for the large VCG difference between the two vessels can be accounted for in part by the ballast carried by design A and

TABLE VII

	1970	1971	1972	1973	1974	1975	1976	1977	1978
Deep sea trawlers	NA	NA	3	5	6	4	2	8	10
Others	NA	NA	21	26	22	43	33	29	27
Total	15	13	24	31	28	47	35	37	37

the heavily constructed superstructure and wheelhouse of design B.

Freeing Ports

An investigation was carried out at NMI in 1976 into the relative effectiveness of different types of freeing ports typically fitted on small fishing vessels. Since maximisation of the flow rate at which the decks can be cleared of water increases the likelihood of survival, then the design of freeing port is considered to be of some importance. It appears that little or no previous work has been done on this specific topic. Water on deck on a vessel in a heavy seaway will move in a complex manner which does not readily lend itself to easy analysis, and so the experiments were concerned with the freeing ports acting as orifices (rather than weirs), using a static rig with the openings at half scale. The straightforward analysis adopted enabled qualitative comparisons to be made between different freeing port openings.

The investigation found that the discharge coefficients for conventional freeing ports appear to cover a relatively small range of values, usually 0·6 to 0·65, and this implies that a simple statutory area requirement for port size for a particular ship is a satisfactory approach. At larger angles of list and roll when the ports are submerged, they are not contributing to survival of the vessel. However, ineffective ports would certainly impede the discharge of water from the decks and generally prolong the time when the ship is endangered. There is therefore some relationship between port area and ship safety.

In order to satisfy the fisherman's preference for dry decks to maximise fishing time, hinged flap type freeing ports can be recommended as these minimise the inflow through the freeing port on to the deck. Attention should, of course, be paid to the practical design of such flaps to ensure that they cannot be jammed through corrosion or from fouling by gear (or be deliberately wedged shut).

The freeing ports on design A consisted of a slot running continuously for about half the length of the vessel whereas those on design B consisted of hinged doors in the bulwark, opening outwards. The experiments also indicated higher discharge coefficients for the slot type of freeing port compared with the hinged door type; with this latter type the discharge coefficient improved slightly when the hinged door was removed.

Mrs Faulkner was the first of many contributors to question the effectiveness of freeing port arrangements and the available data for their design for a given deck well configuration. The present UK rules for the areas of freeing ports are based on the considerable operating experience of the fishing industry although these areas are usually much lower than the requirements of the 1966 Load Line Convention; this point was mentioned in the discussion to Ref. 11. To be fair it was agreed to accept the existing arrangements until such time as superior and more effective freeing ports could be advocated. In the author's view there is scope for making the arrangements for clearing water from the decks more effective simply by increasing the freeing port area to allow for those that become partially blocked due to the fishing operation and the miscellaneous gear stowed on deck.

General Remarks

Professor Faulkner raises the question of the relevance of formulating a statistical approach to dynamical stability in view of the risk of flooding and the stochastic nature of the excitation. The author is not in favour of establishing a mathematic model representing the random variation of the relevant parameters associated with initial stability and the reserve of dynamical stability as a means of establishing the risk of capsizing. A more useful approach would be to make use in principle of the semi-probabilistic methods advocated by structural engineers to give some useful design guidance on capsize prevention. From such an indirect method, a safety factor concept for the ship as a measure of safety against capsize could be derived. A recent example of this type of safety concept has been advocated by Professor Faulkner[31] and has been investigated by Kure[32].

An examination of the equation for GZ_{30}, given in Ref. 19 with the well-known ones for KB and BM was made and a comparison of results is given in Table VIII where the figures in brackets are the actual values in metres.

TABLE VIII

	GZ_{30}	KB	BM
Vessel A	0·29(0·20)	1·61(1·48)	1·99(2·70)
Vessel B	0·41(0·40)	1·59(1·48)	1·80(2·01)

It would appear that the equations do not give consistent results for the trawler forms considered, (possibly due to the different geometry of the hull forms and the effect of trim on stability).

In the author's view an area-based criterion is desirable at angles above 30° to provide an adequate reserve of residual stability to withstand the effects of external forces and in particular, those caused by extreme seas.

As in all seakeeping experiments the value is in the comparative rather than the absolute results and this was the main reason why the experiments were designed around two vessels; design B was selected for her reputation as a good sea boat. One of the points made in the paper is that the probability of capsize can be reduced if a vessel's stability is in excess of the IMCO criteria. Moreover, the IMCO criteria do not adequately deal with the righting moment curve above 40° of heel. A further improvement in safety suggested by the tests is for an adequate reserve of stability to be specified at these higher angles of heel. This reserve of stability or area under the curve above 40° should be based on good design practice to ensure that the range is at least 50° (note that design A had a range of only 45°). If the current stability rules are to be reviewed in the future then all factors affecting stability must be taken into account; it would seem prudent and responsible to learn from recent experience and to give guidance on good design practice for improved ship safety in rough seas.

Mr Tope raises several pertinent questions on stability: the observation made in the Holland-Martin Report that if a single parameter has to be used 'the area up to 40° is probably as good as any' should not be taken out of the context of the report. The use of GM alone as a parameter when its value is judged against a formulation based upon an analysis of existing similar vessels is a practical way of judging stability in the absence of other information. However, such a simple formulation is, of course, more than likely to be misleading if the rolling period test is used to establish the GM since the roll period varies considerably with bilge keel size for which no corrections are made. The resulting GM from this test can at best be an approximation.

An examination of form stability as advocated in Ref. 20 is always an interesting exercise in looking at the initial stability of a vessel. Accepting the fact that residuary stability and initial stability are not the only elements which determine behaviour at sea, setting limits to these values would not in the author's opinion reduce the risk of capsizing in all cases. There is the inescapable fact that adequate freeboard ensures a good residuary stability and this, with a sufficiently low centre of gravity, usually leads to a safer vessel.

The righting levers in the paper have been computed on a free-trim basis.

Mr Gilfillan rightly points out that it would have been more sensible to adjust the breadth of design A to that of design B in comparing the righting levers for the two designs; the design A with modified sheer would indeed be capable of accepting a higher load on deck. The values of KB, BM and C_{IT} for design A are 1·48 m, 2·70 m, and 0·68 respectively and for design B are 1·48 m, 2·01 m, and 0·58 respectively.

The measured GM of the models is considered to be accurate to the order of ±2 mm; the radius of gyration was adjusted to correspond to approximately 0·4 × beam in the absence of known values for each vessel. The maximum wave steepness of the breaking waves used in the experiments was estimated to be in the ratio of 1 : 7.

Professor Conn's remarks are most welcome and several observations of importance are made. The point that comparisons would have been easier if the freeing ports arrangements had been identical for both models is of course valid, but the differences were based on the actual designs for these vessels. The reference to Kato's work is most informative especially since more recent work from Japan[33] makes use of the ratio of useful reserve dynamical stability to the maximum kinetic energy of the ship after being struck by the gust.

Professor Kuo takes me to task for not treating capsizing from a motion viewpoint and for trying to explain it from a physical basis. Of course there is room for both points of view but at the moment there is no suitable mathematical model available to describe the capsizing experiment reported in the paper.

Professor Kuo takes a haughty if not academic view of the assumptions implied in the paper and has asked for a number of explanations. The justification of the assumption that physical models can be used correctly to examine ship capsizing is that the physical model, however imperfect, represents the real world much better than a more idealised theoretical model; Professor Kuo must be aware of the limitations (and assumptions) associated with modelling large amplitude motion with six degrees of freedom plus rudder in breaking seas; the coefficients for such a mathematical model must either be calculated (most without amplitude and time dependency) or based on model results. It would appear that model experiments do have some uses.

The information contained in Fig. 8, the wave energy spectra diagram, must be treated at its face value as it does not attempt to infer reasons for capsizing of the model but merely contains facts that are relevant. For an explanation of the wave energy spectrum at wind force 8, the author commends the reading of an excellent textbook on the subject by Bishop and Price[34].

Professor Kuo questions the use of Darbyshire's wave spectrum in the model tests. The reason for its use is that it not only represented waves in which a particular trawler capsized but its narrow band of energy is not unlike that found in the North Sea which is characterised by its short fetch.

Comments on breaking waves and on encountering have been made in reply to Dr Kastner; there is of course no relationship between a one dimensional wave spectrum and the manoeuvre used in the model tests. The significance is that the waves from this spectrum are uni-directional and the orientation of the model is relative to the wave direction. The model was not randomly manoeuvred but encountered waves at random in circling manoeuvres.

The information contained in Fig. 8 is of course scaled to correspond to the full-scale environment and should not be mistaken for model values.

Professor Kuo wonders about the mode of capsizing with which the paper is concerned, to which the author would refer him to Section 6.2 for a detailed explanation of the two distinct types of capsize dealt with.

Mr Crow seems to be in agreement with the author's views that the IMCO standard is questionable when the stability of the models was near to that required by IMCO for the imposed sea states of the tests, as a result of which no margin of stability was available.

The suggestion of a minimum GZ value at the angle of downflooding is worthy of consideration, and should be taken into account. Similar proposals to these have been given in my reply to Mr Tope.

The proposals for two standards of stability, although unavoidable in practice would perhaps lead to dual standards of safety which should be avoided. The important point is that minimum levels of stability to promote safety from capsize are difficult to define with precision but, in the author's view, the next generation of fishing vessels should at least have higher design standards of stability than some of those built in the past.

Dr Kastner seems to be in agreement with the author's views on the scaled steering characteristics for the model. The rudder response rate was scaled using a servo mechanism on the model to give the characteristics of powered steering. An auto pilot was considered but the model was not sufficiently large to accommodate such a system; although an autopilot is highly desirable, its performance in steep and breaking waves has yet to be tested.

Dr Kastner mentions the importance of modelling freeing ports and the problem of shipping water over the bulwarks. These details certainly need further attention if the practical problems of seaworthiness are to be resolved. I fully endorse the view that specific model testing must be carried out in close harmony with developing theory.

Dr Kastner raises the question of whether the measured data can be evaluated in respect to ship speed and ratio of encounter frequency to natural roll frequency etc. The speed of the small radio-controlled models was not measured in the wave tests due to the practical problems involved; however, as Fig. 8 illustrates, model A capsized in breaking waves when the encounter frequency (in beam seas) was coincident with the natural roll frequency. Although model A capsized in a condition close to resonance in at least one instance, the conditions of capsize in breaking waves could not be entirely attributed to this cause. However, had the natural roll frequency (or stiffness) of this model condition been diffferent, then it would have operated away from the resonant frequency of the wave system. On changing the stiffness, the model was more resistant to capsize.

The wave energy spectra in Fig. 8 are for the measured spectra of the waves generated in the initial tests corresponding exactly to those defined by Darbyshire for wind force 7 and 8 respectively. The measured spectra for breaking waves used in the subsequent tests are also given in this figure.

The author is aware of recent work by Longuet-Higgins on breaking waves but his doubts on the exact definition were in differentiating between a breaking and spilling wave, for example, in the absence of detailed measurements of the wave crest profile; the wave heights were of course measured in the usual way and the wave spectra are presented in Fig. 8 as already stated.

The reason for selecting a 70 m long wave for the stability calculations when the vessel was assumed to be poised on a wave crest was that it was consistent with earlier calculations carried out for design A: it would seem reasonable to expect the largest variation of statical stability to occur in waves of the same length as the ship and Miller et al[35] have shown that in following sea conditions, a model can be at risk in waves between 1 and 1·5 times the length of the vessel.

Mr Clubb mentions the importance of freeing ports from the fisherman's point of view and draws attention to effect of deck fittings in restricting the flow of water on deck to the scuppers. The adverse effect of stowing fish in boxes on the deck together with other items has been clearly demonstrated and the author is grateful for this vivid illustration of how stability can quickly be reduced at sea; the advice on how this risk can be avoided by refuelling if fish are not found early in the trip appears to be sound. A vessel with a cruiser stern and good stability characteristics will not be immune from broaching-to in short steep waves where difficulty with steering will be experienced and the information given on this aspect is most valuable. The model tests with the two designs indicated that the transom stern design was no more liable to broach than the cruiser stern design.

Dr McLean has raised a valid argument in favour of preceding any experimental study with an extensive theoretical study where a large number of parameters are involved. However, at the present time no satisfactory mathematical model is available to cope with the six rigid body motions, rudder motion, the effect of water on deck and the effect of wind, all at large amplitudes of motion. Although some simplifications can be made, no mathematical equation exists for considering a vessel in breaking waves while under way. In the absence of such a mathematical model, the experimenter has the difficult task of selecting a small range of parameters that will hopefully give some insight into the physical nature of the problem. The task of simulating a steady or gusting wind in a small scale model moving in waves is not easily resolved; despite its limitations, the moving weight system used in the capsizing experiments demonstrated that the effects of a suddenly applied load, such as wind, were of no significance.

Dr Ferguson rightly points out that rolling period and transverse stability will vary with forward speed. The findings of the recent work at Glasgow University are of interest in indicating that the transverse stability will further vary depending upon the ship's orientation to the wave pattern. The effects of forward speed on roll period for the experiments described in the paper, although apparent in some of the tests, were not considered significant in relation to the reduction in period due to the increase in roll damping at large amplitudes of motion and when water was on deck.

Mr Lipiner questions why the effect of waves only was taken into account and not other factors. The aim of the experiments was to investigate the behaviour of the models in a limited number of conditions and although the shifting of cargo could have been considered, the effect of wave action and water-on-deck proved to be sufficient to promote capsize in some instances; the effects of icing and other static loads were not considered. No attempt was made to develop a new theory on the mechanism of capsize based on the model results, but an effort was made to explain the reasons why one model capsized and the other did not in identical wave conditions.

Mr Vassalos wonders what justification there was for using equation (1) for deducing the roll damping coefficients in view of doubts expressed about its validity and accuracy.

The equation is based on Froude's assumption that the resistance to rolling is composed of two parts, one of which varies as the angular velocity and the other which varies as the square of this quantity. The values of the coefficients can easily be obtained from a curve of extinction and this is one reliable method of comparing values measured on the model with those obtained from a full-size vessel.

Mr Vassalos mentions another important point raised by others as to whether the motions of the model can be regarded as reasonable representative of the ship on the basis of the roll damping experiments. Of course, as already mentioned, the importance of the seakeeping experiments is in their comparative rather than absolute basis; it was somewhat reassuring that for small amplitudes of rolling the damping coefficients were in reasonable agreement. The author would be less confident on the other hand if the roll period and damping coefficients for model and ship were completely incompatible although the question of scale effect remains unresolved.

Dr Baxter asks for clarification of the final paragraph of the paper and, in particular, of the meaning of the recommendation for greater emphasis on the maximum righting moment and its position on the stability curve. The implication of this statement is that the maximum righting moment, which should ideally be nearly constant for different loading conditions, should not only be adequate to withstand the likely upsetting moments but should occur at an angle of say 30° rather than 25°. Dr Baxter's proposals for stability experiments on full-size trawler hulls to resolve the correlation problem between motions and stability of a model and full size ship are well taken and are under active consideration at NMI.

Reply to Discussion in London

Mr Bailey refers to three extra conditions that were examined in subsequent experiments in which both displacement and GM were increased. The records of these experiments are most valuable and complement the results given in the paper. These additional results highlight the importance of hull loading in questions of stability; the extreme displacement condition (iii) in which the freeboard was greatly reduced did not capsize despite the considerable deck wetness. This contrasts with the original much lighter displacement condition of model A which resulted in capsize. It is the author's opinion that the original condition can be considered as a 'ballast' condition in which the righting moments, and therefore resistance to capsize, are much less than those in the extreme displacement condition.

Mr Bird has raised some interesting questions on the background problems of the model experiments with which he has been associated. He is right in saying that model A with transom stern did ship more water on deck and rolled with a more lively motion. It was not, however, more difficult to steer in following waves compared with model B with a cruiser stern and as both models showed the same tendency to broach, the transom stern did not appear to handicap progress in this respect. One other essential difference of design not mentioned by Mr Bird is that of sheer, particularly at the forward end.

The author's views differ on the statement that no conclusive guidance has emerged on the effect of stern shape and the effectiveness of freeing ports. Although the effect of the stern shape was not separated out from the other form and parameter variations, the indications of the experiments were that the transom stern design appeared to have slightly inferior seakeeping qualities in following waves compared with the cruiser stern design. The superior effectiveness of the hinged type freeing port was evident from the experiments and although of great importance, did not appear to influence the possibility of capsize.

Mr Bird mentions the importance of the vertical centre of gravity, apart from its influence on GM, and cites some capsize experiments carried out in the United States. It has been established, mainly from the capsizing experiment on the EDITH TERKOL, that in extremely light load conditions, when the KG is large relative to the draught, large sway-roll coupling moments may exist which could cause capsizing. There was no evidence of this effect in the author's experiments and this would only be expected to occur when the KG/draught ratio exceeds 1·4.

Professor Aertssen mentions his paper on the BELGIAN LADY which I remember reading and enjoying at the time and I am very pleased that this earlier work has been referred to. The fact that curve (b) for design B given in Fig. 14 is identical to that of the BELGIAN LADY is interesting and that both curves are well above the IMCO minimum curve is most relevant. The reasons for selecting this curve (b) for future discussion are helpful and the proposal for an increase of 20% on the IMCO righting lever is worth considering; this would indeed automatically increase the angle of vanishing stability to at least 55°. It is, however, worth investigating the consequences of such a change on the motion behaviour of vessels in general before making such a recommendation, although the present IMCO minimum curve does appear to be inadequate for some fishing vessels when exposed to severe seas.

Mr Tvedt rightly points out the subjective nature of the term 'lively' to describe the behaviour of the model. A more quantative measure of the model's motion can be given in terms of extreme roll amplitudes (without capsizing taking place) rather than the conventional significant values which are more appropriate for normal seakeeping qualities. Typical extreme roll amplitudes for model A and model B in following quartering seas were 25° and 20° respectively. Mr Tvedt seems to be in agreement with the author's views of an extended range of stability similar to curve b in Fig. 4 or that of the BELGIAN LADY.

Professor Dahle points out the difference, with which the author agrees, between the less dangerous 'spilling' waves used in the author's experiments and the potentially dangerous breaking waves used in Professor Dahle's experiments. Professor Dahle mentions the importance of effective freeing ports to reduce the heeling angle for the smaller waves and suggests that they are of no consequence in plunging waves. In the author's experiments with model A at the extreme displacement (see Table III of Mr Bailey's discussion) the model survived when the decks were continuously awash no matter how efficient the freeing ports; their action would therefore have been irrelevant. Moreover one capsize was obtained with absolutely no water on deck in which case the freeing ports were redundant.

In response to Professor Dahle's question on erections, the deckhouse erections only were included in the calculation of the GZ curves in Fig. 6. The author confirms that the model did capsize and remained upside down even with an intact wheelhouse.

In the author's opinion, the level of stability of design A was insufficient to provide adequate safety against capsizing in the wave conditions tested. In contrast, the design and the level of stability of design B was considered more than adequate to prevent capsizing.

Dr Clarke rightly points out that recent work of Professor Longuet-Higgins has demonstrated that plunging breakers can occur in deep water so that the presence of shallow water and currents is not always essential for the generation of this type of wave.

Mr Gilfillan wonders whether the traditional curve of righting levers is the best presentation for dealing with large angle stability and then shows a most original and interesting alternative presentation in his Fig. 17. This figure is most logical and gives a graphic description of what might be called a stability 'phase plane' diagram. Fig. 18 reveals many interesting features of interest and allows all these factors to be demonstrated on a comparative basis on a single diagram. Mr Gilfillan describes how these comparisons demonstrate such facts as the illusory nature of an increase in stability through an increase in GM.

Reply to Written Discussion

Mr Mackay rightly points out that the IMCO recommendation for intact stability is now the statutory minimum criteria for fishing vessels over 12 m in length. The danger of minimum standards, is, of course, the temptation to just comply with the regulations rather than design a vessel with a generous margin of stability. The owner's demands for the installation of sophisticated deck machinery are recognised as a problem in raising the centre of gravity and possibly reducing the margin of stability. A low value of vertical centre of gravity, as in design B, would in fact be difficult to achieve in practice with present day crew protection and deck machinery installations on multi-purpose fishing vessels without a significant amount of ballast.

Mr Mackay makes a perceptive remark on the need for model tests in bulk fish conditions with minimum freeboard and bow trim. These tests might well illustrate the relative merits of initial GM, freeboard and bow height, and displacement in determining whether permanent ballast should be fitted.

Mr Renilson's remarks on the work at Glasgow University are noted with much interest as are his informative comments on broaching. Although the capsize without water on deck in the author's experiments was associated with a minor yawing moment prior to capsize, it could not be related directly to the mechanism described by Mr Renilson or as observed by Professor Paulling.

Mr Renilson rightly points out that the superstructure is not accounted for in the rules (the IMCO criteria are concerned only with properties of the righting moment curve up to 40° or the flooding angle) and its effect on the stability curve usually occurs above 40° of heel. The reason for this state of affairs is because of the non-watertight integrity of the superstructure and the possibility of wheelhouse doors and windows being left open etc. It is not strictly correct, therefore, for both the above reasons, to argue that a safety margin is already assumed in the regulations.

No allowances for the superstructure openings were made in the model tests and the differences in superstructure configuration between the two designs would not warrant further tests.

Mr Evans raises the question of the effect of removing the bulwarks on the two modes of capsize cited in the paper. He contends that if the stability is adequate to prevent capsizing in the first mode without bulwarks, it is very unlikely to occur in the second. The author is in general agreement with this proposition with the proviso that with some freeing port arrangements the bulwarks act as an effective extension to the vessel's freeboard. On the other hand, despite the fact that they provide protection to fishermen (and the fish on deck), they do tend to retain large quantities of 'green' water shipped over the side.

Mr Evans mentions the importance of considering the stability for the vessel poised on a wave crest for an L/9 wave profile and describes how this method has recently been employed for the ISLAND class offshore patrol craft. The author believes that there is much merit in advocating this method which could easily be carried out by design offices. However, in order to apply this approach to small merchant ships, an appropriate 'operational envelope' would have to be defined and agreed as good design practice.

Mr Donaldson has raised some interesting points on broaching and the effectiveness of the rudder in this situation. The author would like to confirm that when the models broached, in defiance of rudder action, there was a temporary loss of steering control when broaching took place, only to be regained moments later. Of course the danger from broaching is increased when the decks become flooded and this increases the risk of capsizing.

Mr Donaldson questions the possibility of accentuating a broaching situation by putting the rudder over in the normal way on the assumption that the direction of flow over the rudder is reversed. The author has no evidence that the flow over the rudder is reversed in a pre-broaching situation, and although it is likely that the flow would be retarded, some flow would still be provided by the propeller. The author also holds the view that a full form with good directional stability is less likely to broach in a following sea. However, other overriding factors may predominate such as the afterbody lines, the stern shape and the trim of the vessel; some of these factors are discussed in a paper on broaching[36].

Mr Donaldson's remarks that the difference in the vertical centres of gravity between the two vessels could be due to the heavier deck machinery on design A and additional permanent ballast in design B are most perceptive and add to the information already given by the author on this point.

Mr Eyres rightly points out that the effect of stern trim can adversely modify the stability curve and for this reason the author advocates the use of 'free trim' stability calculations for stability assessment.

Due to an error in the original transcript of the author's paper, it was incorrectly stated in the advance copies of the paper that design B was built of wood. This vessel was in fact built of steel and the text has been amended accordingly. The author considers it unlikely that design A with a relatively high KG had any large quantity of permanent ballast.

Mr Eyres raises the question of possible scaling effects on shipping and discharging water on deck and on broaching and helm response–important issues concerning the correctness of the model experiments. In the author's opinion these are not as significant as the likely scale effect on the roll damping between model and ship. It is, however, worth mentioning again that the model tests in question, as in seakeeping experiments in general, are comparative rather than absolute. The helm response of the models used in the experiments was of course scaled to correspond to that of the full-size vessels.

Mr Tope mentions the interesting point that the standard of IMCO plus 20% mentioned by Professor Aertssen is in fact the same as that required under the national regulations of the Netherlands and United Kingdom, to be met by new fishing vessels often referred to as 'beam trawlers'.

Mr Tope raises the important question of the need to distinguish those circumstances which are general and suggest the need to increase the present minimum standard of stability, from those which are localised and special which can be taken into account by supplementing the existing standard. He is correct in believing that a separation from the general to the particular circumstances which affect stability should be attempted, but in practice it is often easier to raise the existing standards, as in the case of beam trawlers where fishing gear is towed from the outboard end of a boom, than to just add to the existing standard. In the author's view there are special circumstances in which the IMCO minimum criteria may well be inadequate such as when a vessel is subjected to breaking waves, as indicated in the paper. In this case the circumstances are special and there would appear to be a case for not only raising the minimum standard of stability but also ensuring that there is an adequate reserve of stability at heel angles greater than 40° which would give a minimum angle of vanishing stability in excess of 50°.

Mr Nicholson was one other contributor to question the reason why the measured and calculated GZ values did not agree for model A after deck edge immersion. Unfortunately the assumptions made in the calculations concerning the extent of the superstructure and whaleback did not correspond exactly to that of the model and some differences were expected. Also, the bulwarks were not allowed for in the calculations and this may have been another source of error; some small experiment errors have to be admitted despite the fact that the measurements for a particular angle of heel were repeatable.

Mr Nicholson has incorrectly stated that the present IMCO criterion of GM = 0·35 m could be raised above 0·732 m. The author does not agree with this proposal and it was not suggested in the paper.

Mr Nicholson raises the practical implications of stability criteria related to high angles of heel. He is right in believing that weights may well shift and water may enter through various openings at the high angles of heel. However, in the author's opinion it is most unwise to ignore the righting moment after 40° has been reached; it is possible for vessels to heel to large angles in practice and to survive. The fact that design A had a small residual stability above 40° and a range of stability of only 45° can in no way be regarded as good design practice and in the author's view will not give a margin of safety against capsize as in the case of design B. It is also worth mentioning that the author would prefer the position of the maximum GZ for a fishing vessel to occur at around 30°, rather than at the minimum value of 25° recommended by IMCO; furthermore the character of the stability curve should be similar in appearance to that of condition (a) for design B shown in Fig. 14, which happens to have a range of about 60°.

Dr Leathard's remarks on the fundamental difference between the two vessels being the much higher position of the centre of gravity of model A compared with model B are noted and agreed. In his analysis of the situation, Dr Leathard compares the stability characteristics for survival of both models and concludes that some consistency was apparent. On the basis of this analysis the statement that 'survival did not depend entirely on the absolute value of GM, but rather that the character of the stability curve played an important part' made in the author's paper was strongly endorsed.

Dr Leathard seems to be in agreement with the author's views that for the size and type of fishing vessel in question, operating in the realistic sea states adopted, some increase in the minimum IMCO stability level is required. It must, of course, be re-emphasised that this guidance is based upon experiments with two models and the author would agree with Dr Leathard that firmer recommendations must await the availability of more extensive data.

Mr Napier mentions the importance of freeing ports in the region aft of the whaleback to allow any water running forward to escape. The author is in agreement with this practical suggestion and would like to see (a) the extension of freeing ports well forward and (b) the whaleback made into an extensive watertight compartment to prevent water being trapped there.

Mr Napier raises the question of unshipping deck pond boards on passage which is sound commonsense to allow any water on deck to drain away quickly and without hinderance. His idea of providing drainage slots with plastic coated wire mesh for the pond boards is also worthy of serious consideration to allow water to drain through what might otherwise be restrictive boards and freeing ports.

Mr Napier wonders whether strict rules or education would make for a better and safer sea boat. Of course, as in most cases of ship design, the end product is one of compromise but the author is of the opinion that practical experience is essential as are the views of the fishermen, together with a better understanding of the importance of stability as a fundamental element in the design and operation of the vessel.

The author is very grateful to all the contributors. In answering some of their remarks, further important points have emerged which in general support the view that in matters of ship safety much remains to be done in order to replace the IMCO 'static' stability regulations by modern knowledge which is being successfully applied to other fields of engineering.

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