DEPARTMENT OF THE NAVY
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ON SHIP MODEL TESTING FOR THE PREDICTION OF
EXTREME CONDITIONS IN CONFUSED SEAS

by

William E. Cummins

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ON SHIP MODEL TESTING FOR THE PREDICTION OF EXTREME CONDITIONS IN CONFUSED SEAS

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William E. Cummins

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ABSTRACT

The problem is considered in which information is needed as to the nature of extreme conditions a ship is likely to encounter during a definite short period of operation in a well defined random sea. Various model experiments are designed which are of increasing complexity and validity, each being an “optimum” design for a given degree of sophistication. The approach is based on the statistical properties of the ship-seaway system, and the resulting experiments are intended to provide a maximum of valid information from a small number of runs.
PREFACE

This little paper was a contribution to one of the most stimulating and important technical meetings it has ever been my good fortune to attend. The paper is concerned with a problem of “tankery” of quite narrow scope, and if the solution to this problem were all that was achieved, republication would hardly be justified. However, several aspects of the paper have broader implications, and because the proceedings of the Wageningen Symposium have limited distribution in this country, reissue as a TMB report seems desirable.

At the meeting the paper received extensive discussion, out of all proportion to its objective. This was undoubtedly due, in part, to its favorable position near the front of the advance copy of proceedings, as it was apparently more extensively read than many papers of greater merit. At the same time, the readers seemed to find it provocative and the resulting discussion was quite vigorous. Although many of the points in this discussion were completely sound, some of the comments were quite irrelevant. And some of the aspects which I consider most important received no discussion at all!

After three years, there seems to me to be four factors of the paper which have significance.

1. The introduction is a frank statement of the bargaining between the ship designer and the model tester when the designer brings a problem to the towing tank. Although the substance of this statement is certainly familiar to everyone concerned with model testing, its appearance on paper seemed to be appreciated by many of those present.

2. The paper is an attempt at the rational design of an experiment in which the random nature of the phenomena under investigation is the dominant feature. Although such problems are not new, they have rarely been considered in ship model testing. The present attempt is not completely successful, but I feel that this sort of analysis is of increasing importance as we attack increasingly difficult problems.

3. The analysis is based on the fact that the “memory” of a ship in longitudinal motions is quite short. Thus, most of the history of motion is irrelevant, and the instantaneous behavior can be predicted from a knowledge of only the immediate past. This fact, although well known, has not been fully exploited to date, and I believe it has a number of interesting consequences.

4. In my own opinion, the most important aspect of this paper was not noted by any of the discussers. This is the determination of the Fourier coefficients for a short sample of a Gaussian wave record. The joint distribution of these coefficients is completely determined by the relations [7a] through [7e], and by means of this distribution short samples can be constructed at will. To the best of my knowledge, these results are original.
INTRODUCTION

The growing emphasis on research in seaworthiness is due to real needs of the ship designer and the ship operator. Many of these needs are not new, but technical advances elsewhere have made them more critical. Hence, there is stimulation in all parts of the profession to be more aware of seakeeping problems. This intellectual climate can only result in more and more problems being sent to the towing tanks.

Many of these problems will raise great difficulties in the design of a satisfactory experiment. Such difficulties can arise from three different sources. First, the problem must be defined so specifically that the model tester will have a clear objective. This may not be easy, and very possibly the requesting agency will be forced to make concessions which will result in an investigation which is incomplete. Then, there will always be limitations arising from the capabilities of the towing tank facilities. Finally, there may be serious technical problems in establishing a suitable set of test conditions. This paper is concerned with a problem in which troubles from each of these sources become important.

The problem might arise in the following way. An operational requirement is imposed on a given vessel that it be designed to carry out a certain operation in as wide a variety of sea conditions as possible. The operation is adversely affected if, say, an acceleration at a certain location on the vessel exceeds a certain value. (The limiting condition could just as well arise from pitch angle, roll angle, structural strain, or perhaps a quality, such as wetness.) Consequently, it is specified that it be possible to carry out the operation in all sea states up to a given severity.

The ship designer must evaluate his design against these requirements, so he goes to the towing tank. He feels that he has a very specific problem, and is somewhat surprised when the model tester asks for much more information. First of all, the question of sea state must be carefully spelled out. The ship designer may specify merely a mean wave height. This is, of course, a very unsatisfactory definition, and after a bit of bargaining it is agreed that the hypothetical seaway will be the fully developed, unidirectional, Neumann seaway which has the given mean height. This is a serious concession on the part of the ship designer, since such seaways are not necessarily typical.

Next, since the motion of the sea surface is random, it is, in principle, possible for the critical value of the parameter to be exceeded in any given seaway. Therefore, it makes sense to discuss this event only in terms of probability of occurrence. When the ship designer is asked if he will be satisfied if the critical value is not exceeded more often than, say, once in a hundred operations, he is not likely to be very happy, since he prefers to design for certainty. However, since this probability is an essential factor in the design of the experiment, it must be specified.

Once this difficulty is overcome, it is evident that the longer the operation takes, the more likely it is that the given value will be exceeded. Therefore, the length of the critical period of the operation must be specified.
Having settled these questions, the model tester puts further restrictions on the investigation. Most likely the only facility available is a conventional towing tank equipped with a wavemaker at one end. Therefore, the experimenter states that the tests can be carried out only in long crested waves, simulating (more or less) head and following seas.

Feeling much less confident in the investigation than when he arrived, the ship designer returns to his office. The model tester is left to think about the most difficult problem of all: how is he to set up a test which will yield as much meaningful information as possible.

This is the problem in design of experiment with which we are concerned. We shall consider a sequence of possible solutions. Bluntly, none is completely satisfactory. In spite of the mathematical treatment which will be used, it will be clear that the argument is intended to be plausible, rather than logical (as it is in most designs of experiments). The limitations of equipment have important consequences as to the validity of the design, and the simplifications which are made to overcome theoretical difficulties may be even more serious. Recognizing this, it is believed that the analytical approach is revealing, and some of the proposed solutions have merit.

**STATEMENT OF THE PROBLEM**

We shall assume that the ship designer and the model tester have agreed upon a set of conditions. These will, of course, include all data concerning the model speed, longitudinal radius of gyration, etc. A limiting sea state for the full-scale operating and a corresponding sea spectrum have been established, perhaps using Reference 1.* It is desired to investigate extreme motions which will occur with a probability not greater than $p$, where $p$ is small, during a time $t_o$. It is agreed that the tests will be carried out in head and/or following seas, and the desired seaway will be approximated by some configuration of long crested waves.

We shall make the following assumptions:

(a) The spectrum of the wave elevation at the center of gravity of the model is $G(f)$. This is obtained from the spectrum for the elevation at a fixed position by the usual mapping procedure. It is assumed that this spectrum arises from a one-dimensional wave train moving along the length of the basin. The area under the curve $G(f)$ is $E$, and is equal to twice the variance of the surface elevation.

(b) The damping in pitch of ordinary ship forms is large. In fact, it is so large that a time $T_c$ can be found such that the model has no "memory" of events, beyond a time $T_c$ in the past. Further, $T_c/f_n$ is not large where $f_n$ is the natural frequency of the model. An equivalent statement is that in a declining angle experiment the motion is essentially quenched after a small number (perhaps two or three) oscillations.

(c) Since we are dealing with extreme motions, it is not desirable to assume that the response of the model to the seaway is linear. However, we are probably safe in assuming

---

*References are listed on page 22.
that the motion is "almost" linear. That is, any nonlinear effects are not surprising departures from that which would be expected if the motion were truly linear.

We shall consider a sequence of experiment designs of increasing sophistication which are intended to satisfy these conditions.

### REGULAR WAVES (I)

It may be argued that the extreme conditions we seek can arise only in a confused seaway, and tests in regular waves can have little or no meaning. Perhaps this is true, but if the model tester has no facility for generating confused seas, he must design the most meaningful test possible in regular waves. We shall see that the situation is not as bad as it seems at first glance.

The simplest scheme is to test the model in a regular train of waves of height equal to the mean height of the seaway, 0.83√E. (See Reference 1, page 7.) The wave length might be selected as that corresponding to the peak frequency of the spectrum. Additional tests might be carried out at other frequencies for which the spectrum has significant magnitude, either with the same wave height or with wave heights reduced by some arbitrary rule.

This scheme is not completely irrational, since the wave lengths are selected on the basis of wave lengths which will occur in the actual seaway, and in any seaway it is to be expected that a succession of waves of approximately the same length and amplitude may be expected to occur. However, a basic objection is that the probabilistic nature of the problem is completely ignored. Elevations much greater than the mean occur in the actual seaway, and the test will yield no information about the effects of such elevations. Hence, the value of this test for a study of extreme motion is doubtful.

### REGULAR WAVES (II)

The preceding design can be improved by using certain results originally due to Kac, which are given in Rice's paper.² An estimate of the number of encountered waves during the time t₀ is the first requirement. This estimate should not be based on the average time between successive peaks, since this procedure tends to overemphasize the high frequency components. A more reasonable estimate is obtained by taking one-half the expected number of zeros, a quantity more sensitive to the long period elements of the spectrum. Using a result quoted by Rice, the number of waves encountered during the time t₀ is

\[
N_e = t_o \left[ \frac{1}{E} \int_0^\infty f^2 \ G(f) \ df \right]^{1/2}
\]  

[1]

The average period is then \( t_o/N_e \) and the corresponding frequency is \( N_e/t_o \).
The expected number of times that the wave elevation will exceed a given value $c$ is

$$N_e \exp (-c^2/E)$$

We are concerned with a situation in which the probability of this event is small, and if we assume that the probability of the event's occurring more than once is of second order, we have

$$p = N_e \exp (-c^2/E) \text{ very nearly} \quad [2]$$

This equation then yields a wave amplitude for the test. The frequencies, unfortunately, are not well determined. However, it would seem reasonable to test at a series of wave lengths, perhaps including the range between $f_n$ and $N_c/t_0$, for which $G(f)$ has a significant amplitude.

The objections to this design are: (a) a single high peak in the actual encountered wave record is replaced by a succession of identical peaks, possibly causing an exaggerated effect; and (b) the correspondence of wave amplitude to wave frequency is not considered, and some of the combinations tested may be extremely unlikely. We can conclude that the design may be seriously overconservative.

**SHORT-TERM REPRESENTATION OF THE SEAWAY**

We have assumed that the model has no memory of events beyond a time $T_c$ in the past. Hence, its motion at time $T$ is determined if we know $r(t)$, over a period $(0, T)$ where $T \geq T_c$.

We shall use this fact to construct more sophisticated experiment designs. But first, we must construct a representation of $r(t)$ over the range $(0, T)$.

Over $(0, mT)$ where $m$ is a very large integer, the wave elevation has the representation

$$r(t) = \sum_{k=1}^{\infty} \left( \alpha_k \cos \frac{2\pi kt}{mT} + \beta_k \sin \frac{2\pi kt}{mT} \right) \quad [3]$$

where $\alpha_k$ and $\beta_k$ are normally distributed random variables with variances

$$\overline{\alpha_k^2} = G(k/mT)$$

and

$$\overline{\beta_k^2} = G(k/mT)$$

and

$$\overline{\alpha_k \beta_k} = 0 \quad [4b]$$

$$\overline{\alpha_j \beta_k} = 0 \quad \text{for all } j, k \quad [4c]$$

$$\overline{\alpha_j \alpha_k} = \overline{\beta_j \beta_k} = 0 \quad \text{for } j \neq k \quad [4d]$$
Over \((0, T)\) we have

\[
r(t) = \frac{a_0}{2} + \sum_{n=1}^{\infty} \left( a_n \cos \frac{2\pi nt}{T} + b_n \sin \frac{2\pi nt}{T} \right)
\]

[5]

where

\[
a_n = \frac{2}{T} \int_{0}^{T} r(t) \cos \frac{2\pi nt}{T} \, dt
\]

\[
b_n = \frac{2}{T} \int_{0}^{T} r(t) \sin \frac{2\pi nt}{T} \, dt
\]

Using Equation [3],

\[
a_n = \frac{2}{T} \int_{0}^{T} \cos \left( \frac{2\pi nt}{T} \right) \sum_{k=1}^{\infty} \left( \alpha_k \cos \frac{2\pi kt}{mT} + \beta_k \sin \frac{2\pi kt}{mT} \right) \, dt
\]

\[
= \frac{2}{T} \sum_{k=1}^{\infty} \left( \alpha_k \int_{0}^{T} \cos \frac{2\pi nt}{T} \cos \frac{2\pi kt}{mT} \, dt + \beta_k \int_{0}^{T} \cos \frac{2\pi nt}{T} \sin \frac{2\pi kt}{mT} \, dt \right)
\]

Then

\[
a_n = \frac{m}{n} \sum_{k=1}^{\infty} \frac{k}{k^2 - m^2 n^2} \left[ \alpha_k \sin \frac{2\pi k}{m} + \beta_k \left( 1 - \cos \frac{2\pi k}{m} \right) \right]
\]

[6a]

Similarly,

\[
b_n = \frac{-m^2}{n} \sum_{k=1}^{\infty} \frac{n}{k^2 - m^2 n^2} \left[ \alpha_k \left( 1 - \cos \frac{2\pi k}{m} \right) - \beta_k \sin \frac{2\pi k}{m} \right]
\]

[6b]

---

**NOTE:** The terms in which \(k=mn\) in the above summations are indeterminate. It may be assumed that they have been replaced by their proper values, as obtained from the corresponding integral expressions.
We now have the \( a_n \) and \( b_n \) expressed as linear combinations of the \( \alpha_k \) and \( \beta_k \). We can conclude that the joint distribution of the \( a_n \) and \( b_n \) is normal (Reference 3, page 70). The theorem which is referenced holds for linear combinations of a finite number of variables, but the two systems of coefficients in the present case are infinite. However, we shall be concerned only with a finite set of the \( a_n \) and \( b_n \), and these can be approximated by linear combinations of the \( \alpha_j \) and \( \beta_j \) for \( j \leq k \). Each set of approximating sums satisfies the theorem, and the \( a_n \) and \( b_n \) are the limits of the approximations as \( k \) approaches infinity. Hence we can conclude that any finite set of the \( a_n \) and \( b_n \) have a joint multinomial normal distribution.

This distribution is completely determined if we know the means, variances, and covariances of the \( a_n \) and \( b_n \). The means are obviously zero. The variance of \( a_n \) is

\[
\overline{a_n^2} = \frac{1}{\pi^2} \sum_{k=1}^{\infty} \frac{(k/m)^2}{[(k/m)^2 - n^2]^2} \left[ \frac{\alpha_k^2 \sin^2 \frac{2\pi k}{m}}{m} + \beta_k^2 \left( 1 - \cos \frac{2\pi k}{m} \right)^2 \right]
\]

Inserting the values of \( \alpha_k^2 \) and \( \beta_k^2 \),

\[
\overline{a_n^2} = \frac{1}{\pi^2 T} \cdot \frac{1}{m} \sum_{k=1}^{\infty} \frac{(k/m)^2 G(k/mT)}{[(k/m)^2 - n^2]^2} \left( 1 - \cos \frac{2\pi k}{m} \right)
\]

Set

\[
\Delta x = 1/m \quad \text{and} \quad k\Delta x = x
\]

and allow \( m \) to approach infinity. Then

\[
\overline{a_n^2} = \frac{1}{\pi^2 T} \int_{\Delta x}^{\infty} \frac{x^2 G(x/T)}{(x^2 - n^2)^2} (1 - \cos 2\pi x) \, dx \quad \text{[7a]}
\]

Similarly,

\[
\overline{b_n^2} = \frac{1}{\pi^2 T} \int_{\Delta x}^{\infty} \frac{n^2 G(x/T)}{(x^2 - n^2)^2} (1 - \cos 2\pi x) \, dx \quad \text{[7b]}
\]

\[
\overline{a_p b_q} = \frac{1}{\pi^2 T} \int_{\Delta x}^{\infty} \frac{x^2 G(x/T)}{(x^2 - p^2)(x^2 - q^2)} (1 - \cos 2\pi x) \, dx \quad \text{[7c]}
\]

\[
\overline{b_p b_q} = \frac{1}{\pi^2 T} \int_{\Delta x}^{\infty} \frac{pq G(x/T)}{(x^2 - p^2)(x^2 - q^2)} (1 - \cos 2\pi x) \, dx \quad \text{[7d]}
\]

\[
\overline{a_p b_q} = 0 \quad \text{[7e]}
\]
We shall also be concerned with the random variable $c_n$:

$$c_n^2 = a_n^2 + b_n^2 \quad [8]$$

$$c_n \geq 0$$

This variable is the amplitude of the sinusoidal wave which is the resultant of

$$a_n \cos \frac{2\pi nt}{T} \quad \text{and} \quad b_n \sin \frac{2\pi nt}{T}$$

It has the probability distribution

$$P(c_n > c) = 1 - \frac{2}{\pi} \int_0^{\pi/2} \int_0^{\pi/2} \frac{\rho}{\sqrt{a_n^2 b_n^2}} \exp \left[ -\frac{\rho^2}{2} \times \left( \frac{\cos^2 \theta}{a_n^2} + \frac{\sin^2 \theta}{b_n^2} \right) \right] d\theta d\rho \quad [9]$$

**REGULAR WAVES (III)**

We first select a set of frequencies at which to test. For each $f_i$ chosen, we select a $T_i$ such that

$$T_i = \frac{n}{f_i} \geq T_c \quad [10]$$

where $n$ is the smallest integer satisfying this condition. (NOTE: A certain amount of judgment is in order in choosing $n$, since if $(n-1)/f_i$ is only slightly less than $T_c$, where $n$ is the value defined above, it is perhaps preferable to choose the next smaller integer.)

Letting $T = T_i$, the variances $a_n^2$ and $b_n^2$ can be computed. Then, using Equation [9] and the probability $pT_i/t_o$, an amplitude $c_i$ can be determined. This assumes that if the time $t_o$ is divided into a set of subintervals of duration $T_i$, the probabilities that $c_i$ will be exceeded in the various subintervals are independent. This is of course not true, since high waves tend to come together, and the probability of being exceeded in two or more adjacent intervals is not necessarily of second order. However, the method described is conservative, since the error will cause the actual probability that $c_i$ will be exceeded in a given time $t_o$ to be less than $p$ (but not by an order of magnitude).

The basic hypothesis of this design is that extreme conditions arise from a succession of almost regular waves which persist just long enough for the model motion to become steady. Single high waves which are of short duration in comparison with $T_c$ are assumed to have no significant effect. The virtue of this design lies in the fact that the amplitude for each test frequency is selected by means of a probabilistic model which takes the frequency into account.

The chief fault lies in the fundamental hypothesis. There is no guarantee that extreme effects are associated only with almost regular waves. Although a succession of
regular waves is known to produce severe motions, we can not be sure that extreme effects will arise only in this way. Also, even though it may very well be that extreme effects for motion occur under these conditions, the same may not be true for other conditions which could be of concern. Wetness, for instance, might be more severe under a completely transient condition in which there is a single high wave of relatively short length. To proceed further, we must depart from the regular wave pattern, and consider designs in which the model is advancing in a continuously changing surface configuration.

**COMPLEX WAVES**

The most elementary procedure for obtaining a complex wave train is to generate a train of short waves and then to generate a train of longer waves which will overtake the first train. We suppose a facility is available in which such a seaway can be created. A reasonable design which makes use of this technique might be the following.

A basic frequency is chosen as in the previous design, and values of $n$ and $c_i$ selected, as before. A second frequency $f_m$ is then chosen from one of two frequencies, $(n-1)/T$ and $(n+1)/T$. Assume

$$a_n = c_n / \sqrt{2} , \quad b_n = c_n / \sqrt{2}$$

By Reference 3 (page 62), the mean values of $a_m$ and $b_m$, when values of $a_n$ and $b_n$ have been assigned, are

$$a_m = \frac{a_m a_n}{a_n^2} a_n , \quad b_m = \frac{b_m b_n}{b_n^2} b_n \quad \text{[11]}$$

Then

$$c_m^2 = a_m^2 + b_m^2 \quad \text{[12]}$$

This method of choosing $c_m$ is perhaps crude, but is probably as good as the problem justifies. The resulting wave form can be interpreted as the most probable wave of the form

$$c_n \cos (2\pi f_n t - \phi_n) + c_m \cos (2\pi f_m t - \phi_m)$$

when the value of $c_n$ has been assigned.

It is evident that this method can be extended further, perhaps by using both the adjacent frequencies. In principle, if the basin is long enough, there is no limit to the complexity of the wave which can be generated. We shall not investigate such extensions, since the design procedure is obvious.
The wave form encountered by the model is continually changing, since the two component waves will advance at different celerities. The severity of the motions will change during the run, but the statistical variation will be far more restricted than in the actual seaway. At the same time, the motion will be much more realistic than in a regular train of waves. It is clear that the variety of conditions encountered during a given run will be greatest when \( n \) and \( m \) are small integers. To this end, it is wise to choose as small a value of \( T_i \) as possible.

The objection to this design is again the uncertainty that we are creating the conditions which will lead to extreme effects. Although this wave train will provide a wide variety of wave encounters, all legitimate candidates to produce extreme effects, we are sampling a very restricted portion of the entire population of wave encounters.

THE CONFUSED SEAWAY

There is one way of overcoming the difficulties just mentioned, to reproduce the exact spectrum of the seaway. Since there exists at least one facility in which this might be possible, and there will shortly be others, the merits of such a design should be considered. There is no question that the sampling of wave encounters is proper (within the restriction to long-crested head and following seas).

We are concerned with events which will occur during a period \( t_o \) with a probability not greater than \( p \), where \( p \) is very small. This will require observations over an extensive period of time, at least as great as \( t_o/p \). In general, such a test will require many runs. Further, each of these runs must be carried out in statistically independent wave trains. Making the runs in a single irregular wave with different starting time (as could be done with the Experimental Towing Tank facility) would not be satisfactory, since the results would be biased by the characteristics of the given wave sample. No extreme effects which did not exist in this sample could be investigated.

Hence, for this presumably ideal design, there exist two major difficulties: the amount of testing required to achieve any degree of confidence is likely to be enormous, and it must be possible to generate an indefinite number of programs for independent confused seas, all selected from the same spectrum.

CONCLUSIONS

We have considered a sequence of designs for an experiment to investigate extreme effects which a vessel will experience during a given time interval in a given sea state. Since it was supposed that the only facility available for making these studies is a conventional long towing tank with a wavemaker at one end, the designs were for long crested head and following seas only. While some of the more elaborate designs have merit, it was found they have definite faults, even the "ideal" model seaway which has the proper spectrum.
The difficulties with this latter scheme are the great amount of testing required to investigate events of low probability, and the requirement for an unlimited number of programs which will produce seaways with given spectrum. For the other designs, the faults lie in the fact that only a restricted portion of all the wave encounters which might produce extreme effects are being sampled.

**DISCUSSION**

J.H. CHADWICK  (Summary of Dr. Chadwick's oral remarks):

(Do. Chadwick states that the inaccuracy of the seakeeping tests in irregular waves is considerably larger (say, 20, 50 percent) than that of the normal still-water tests in the model basins. This involves that many more experiments have to be carried out before a reliable determination of the influence of certain parameters can be made. He doubts therefore whether the tests in two-dimensional irregular waves are of any use.

He prefers in this age of automation the application of computer technique, and advocates realistic mathematical models of ship behaviour instead of tests with small-scale physical paraffin wax models).

K.S.M. DAVIDSON:

Presuming to say something this morning, I have the great advantage that I have not read any of the papers! What Prof. Pierson said about extreme conditions has interested me for a long time. He said a little of what is on my mind and of what Prof. Weinblum talked yesterday, and also Prof. Dresden. I think it is really a fact it is very hard to object to, that man has never designed for the extreme conditions. I recall two years ago, when you had the bad floods in the wintertime in the Northern part of Europe, I heard that the water had been 18 inches higher than had occurred in 100 or 200 years. Just imagine a globe 8000 miles in diameter and men figuring the height of these dykes to 18 inches! It is an unbelievable thing. My question is: why not pour more dirt on top of it? But man is always trying to limit himself. Captain Saunders told us this morning about ships' rolling, but he did not say what he said once before, I think: that is is a duty of the naval architect to preserve the ship under all conditions. But after he has designed his ships then he would still worry about them. It is my contention the naval architect does not provide for 100 percent safety for a ship under the worst conditions. There is always a worse condition for any ship, and it is just a matter of time or luck, or whatever you wish to call it, until any ship can in principle get into trouble. The fact that they do not normally is simply that God is good to you. But if they did get into trouble more often, we would jolly well do something about it. Looking at the whole problem and the whole work on seakeeping in recent years, the thing that made the most impression on me in the last four or five years, is that it is becoming clearer and clearer that the magnitude of motion can be a large figure. I remember that we
had quite a lot of choppy seasailing in the Gulf Stream this summer. It was blowing fairly hard and another boat happened to be sailing quite close to us and we could watch it. I suppose you would have to say that the waves had just about the wrong length for the boats. They were very short choppy waves, but I remember distinctly seeing at least half of the draft of that boat out of water. The whole boat was actually thrown up into the air and that meant that she was practically out of the water, because the lower part of the keel of a yacht does not have displacement in it. The lower part of the draft is just a fin keel. I am sure she was thrown practically out of water as far as the displacement is concerned. But there was no danger involved; she was in perfectly safe condition. People have often in a bad storm on a yacht found that the anchor chains had slipped right out of the chain locker and been deposited somewhere else in the boat because of the high accelerations. So in dealing with the problem as a general thing I do say that we must recognize, and I think we are recognizing, that we have very large forces and very large magnitudes to deal with. And that being so, what I think we ought to stress very heavily is, not so much the extreme conditions that the ship might be subjected to once in, say, a hundred years or longer, but what you can do to improve the ship. I must say I am very much less interested personally in predicting what is going to happen under some condition that must in the nature of things be rather arbitrary than in what is the real effect of radial alterations of ships. We cannot be expected to do miracles in order to keep the seakeeping qualities of the ship at the highest level and neglect other important aspects of the design.

I would like to elaborate just a little bit on what I consider a very important remark made by St. Denis. He said something new has been added. I think quite a bit of what we are saying, and the way these papers read, sounds as if nothing new has been added since Mr. Froude. Much of it sounds as if it were Mr. Froude himself, with his orderly mind, talking. However, Dr. St. Denis says that we have random phase, and I agree that this is an extremely important point. What I mean is this. We have papers such as those by Cummins, Marks, and St. Denis, which are essentially prescriptions or cook-books that will tell us how we should now test models in confused seas. Presumably they tell us we ought to go out and do this and this, as the cook-book says, and the tests will all work out very neatly. Now what those papers actually describe, as I read them, are formulas by which we will determine certain parameters of spectrum, of transfer functions, and so on. Now these are the parameters and these are the things that we mainly think about in our old-fashioned model testing, if you will, where there is no random phase, where there are no statistics. We just go out and measure the parameters. But I would like to emphasize (I think we know it but I think it is worth emphasizing) that when we have this random phase, we definitely have at least one and maybe several more dimensions. Every time we estimate a parameter, there are going to be errors and these errors are no longer a fraction of 1 percent, 1 percent, 5 percent, 10 percent; they are perhaps more than 20 percent, 30 percent, 50 percent, or even 100 percent.
P. DuCANE:

In view of the subject discussed in Dr. Cummins' most interesting paper submitted in Part I of this Symposium, I would like to ask what he would consider to be the best design for an experiment at model scale which would be likely to reproduce slamming conditions; presumably this should be in irregular seas, especially as it is found at full scale that the sea on the bow causes the biggest slamming.

E.V. LEWIS:

This paper is a thoughtful discussion of possible experimental methods of obtaining practical information from model tests regarding ship behavior at sea. The writer agrees as to the importance of tests in head seas, but feels that the author's final conclusions are unduly pessimistic.

There is much to be said for the idea that the primary interest is in extreme values of the quantities which can be observed in model tests (e.g., of amplitude of pitch, bow immersion, etc.). There can be no doubt that the problem of the maximum value to be expected in a very long period, such as the lift of the ship, is extremely difficult and as yet unsolved. It is of particular importance in determining hull loads or stresses for structural design. For maximum values to be expected over shorter periods of time—days or hours—as considered in this paper, however, it is believed that definite answers are now possible.

There can be no doubt that the best approach to the problem of head (or following) sea behavior is the use of long-crested irregular seas in the tank—the last of the schemes discussed in the paper. The answer to the author's problem of extreme values involves then a judicious combination of the following:

1. Increasing the length of the experimental sample,
2. Shortening the length of time involved in the specification of ship performance,
3. Bridging the gap by extrapolation, making use of statistical extreme-value theory.

Considering these items in turn, the irregular wave pattern in use at the Stevens E.T.T., for example, runs for approximately 1–2 minutes. At ordinary speeds, five or six runs can be made in different parts of this pattern without duplication. Furthermore, by additional wiring of the present stepping switch, seven different patterns from the same spectrum can easily be made available. Thus a total sample of 42 runs could be obtained for statistical analysis, and in principle an unlimited number could be obtained simply by connecting up additional stepping switches. However, even the easily available 42 runs would be equivalent to $2\frac{1}{2}$ hours full scale (scale ratio: 1:100), which seems to be quite a good sample. (It would cover perhaps 1200–1400 oscillations.)

The next step would be to try to formulate the ship problem in terms of a shorter time period. For example, instead of specifying that a certain value is not to be exceeded during
one or more exercises lasting perhaps 12 hours each, an attempt should be made to indicate a lower value which should in general not be exceeded in, say, 4 to 6 hours. Since, as the author points out, the model test is a cooperative undertaking involving both the ship designer and the experimenter, the designer can undoubtedly make his contribution here when he fully understands the problem.

Finally, the use of statistical theory should bridge any remaining gap between experiment and ship. This is a problem for the statisticians, of course, but it would appear that presently available theory can extend extreme value data for 4 or 6 hours to give a good indication of what is to be expected in 12 to 24 hours, or perhaps longer. Thus it is believed that a complete answer is now within our reach.

One problem of the model experimenter not mentioned by the author is the effect of the wind in distorting the shapes of the waves and causing breaking crests. This effect is not ordinarily obtained in the tank and introduces some doubt in attempting to make exact quantitative predictions. It can probably be taken into account empirically after some correlations have been made between model and full-scale data.

Returning finally to Dr. Cummins' consideration of the use of model tests in regular waves to give useful information on ship behavior at sea, it is believed that any attempt to pick an extreme or critical regular wave on the basis of the sea characteristics alone is doomed to failure. However, much can be learned by considering the band of frequencies in the spectrum which is of particular importance to the ship motions; i.e., waves which are:

1. roughly of ship length or longer, and
2. of frequencies in the vicinity of synchronism.

This point of view has been presented elsewhere by the writer, and will be discussed at the 8th International Towing Tank Conference. There are many other ramifications which could be explored, but the important point is that the worst wave for the ship may not be at the peak of the spectrum, nor the highest wave in the visible complex pattern.

Incidentally, the value of model tests in irregular waves for simple qualitative comparisons should not be overlooked. For this purpose a comparatively small number of runs will suffice, so long as the wave pattern is reproducible. The irregular sea in a long tank is now proving to be a very useful tool in such comparative evaluations.

W.J. PIERSON, JR:

I would like to add just a few comments on the problem of extreme values. Dr. Cummins has discussed this problem to a certain extent in his paper. We are at times interested in the worst thing that can happen to a ship in a given seaway or perhaps even on a given voyage or during the lifetime of the ship. Dr. Cummins has suggested that in order to find out something about this we should perhaps try the model in that sea which would be present for a wind of perhaps 40 knots under certain conditions, and run the ship long enough in such a sea to be
sure that the most unfavorable combination of events occurs to that ship. The theories on which this work is built are linear and to push them at this stage to extreme values is, I feel, premature. We know so little about nonlinear effects and our statistics are so inadequate for extreme values that I hesitate to claim anything more than a fairly close resemblance between cause and effect in what has been observed experimentally.

Now this is not a situation which is unique to naval architects alone. It is a problem that a statistician must face in every design problem, and in general it is not answered. When one wishes to design a dam, the problem of the worst flood conditions and the heaviest possible rainfall over the lifetime of the dam is not solved. When one wishes to design a bridge, the problem of the worst wind force on that bridge is hardly solved. Thus most extreme value problems are not solved.

We have certain probability density functions which we believe represent the processes we are studying. One of them is the Rayleigh distribution, which supposedly represents the envelope of the process under study. The Rayleigh distribution predicts that if we watch waves with a mean height of 1 foot long enough we will get a height of 100 feet. We know this is not true simply because of the fact that the average length of the wave is only 10 feet, say, and you can't have a wave of 100 feet high and 10 feet long. But our statistics don't tell us this. Many of the nonlinear problems in this work are not solved. They have not been treated, and they will be very difficult to treat. I might mention that my colleague, Mr. Tick, has done some very interesting things recently in solving nonlinear problems, and his work will be published shortly.

However, things are not quite that bad on the other hand because nature solves her own nonlinear problems. We saw this in the wave flume which we visited yesterday. The waves break. They break at random. They break at every position in the tank. They limit their height to length ratios by an automatic process. That process is not yet understood in a statistical sense. I'm fascinated, for example, by the problem of determining the probability density function for the number of whitecaps on the surface of the sea per unit area. Why do the waves break in a stationary random process? When we look at these things, we see that in general for most conditions (not worrying about the effects of spindrift and cases where the entire top of the wave is blown off by the wind and other more drastic occurrences) the waves are not too steep and not too severe when they break. This suggests that perhaps an analogue technique will solve our extreme value problems for us, at least to a pretty good approximation. The analogue technique is the model of the ship in the waves. If we are careful enough to provide breaking waves in the tank and if we come to some sort of agreement as to what the spectrum of the waves is like and make enough measurements and reach enough decisions, then a model ship will solve our nonlinear problems for us. I don't know how well; I don't know how nonlinearity scales. Do nonlinear problems scale according to Froude's law? There is a nice question. I suspect that they do, and therefore we have a possibility for solution.

Now there is one point from an oceanographic point of view concerning the extreme value problem. The ship will meet the worst conditions not by going through a sea produced
by a 30-knot wind for 100 years, but by running into one typhoon or severe extra tropical hur-
ricane—a condition in which we have 50- to 70-knot winds under extremely unfavorable condi-
tions. The problem of extreme values for a ship, in my opinion, is a problem for the oceanog-
raphers in determining how to forecast the waves well enough, so as to be able to say that on
this particular course over this number of years the worst sea that the ship will encounter will
be characterized by a spectrum which is a certain function; these waves will have a signifi-
cant height of . . . pick a number, gentlemen . . ., I think it is high. I think it is a lot higher
than most of us realize. I think that people at sea underestimate the significant height of
waves in most visual observations and I would just say, don't be too conservative, because
the ocean can be a very ferocious place.

M. ST. DENIS:

I want to take one last minute of this discussion, just to leave you with the impression
that I have not been floored for the complete count by Dr. Chadwick. Now there is much
wisdom in what Dr. Chadwick has said and I do agree with him. Of course, he has forced us
to come to violent grips with reality and in doing so I am afraid that he is a man who, having
been invited for dinner, pretends to stay for supper, and not only that, but also to dictate
the menu!

Now this is asking quite a bit from a technique that has only been recently developed,
to have the answers for all the problems in seakeeping that face the naval architect. It is
ture that there are a lot of shortcomings to the technique, with either the mathematical one or
the experimental one, and that these problems have not as yet been solved. But we must re-
member that this technique is something like four years old. And really, how much can we
expect of it? Some five years ago nobody was working with this. We started. We have gain-
ed some momentum; just look at the papers that have been presented today. And I am sure
that with the momentum so gathered, the future does not look altogether as bleak as
Dr. Chadwick has presented it. I hope that even he will have some faith in us.

E.C. TUPPER:

Dr. Cummins mentioned in his paper the difficulties of measuring peak values of motion
but on occasion it may well be that it is just these peak values in which we are most inter-
ested. So it could be that these methods will not provide the answer in themselves to the
more critical points. I found Dr. Cummins’ paper most interesting as a statement of the way
in which the tank experimenter has to tell the ship owner exactly what he may ask to be tried
as opposed to what he would like to have. It does seem a pity that we have spent years try-
ing to persuade shipowners of the great value of ship tank testing and now we have to tell
him that is not quite such a powerful method as he might think. I think we do want to avoid
this feeling of, “This is what we can tell you,” and I think we must also consider whether
we cannot possibly answer the shipowner's question as he stated it originally, sometime in
the future. After all is said and done, the proof of the value of our work must essentially be
the full-scale performance. Dr. Cummins was possibly a little pessimistic for I think he has
one piece of important information which he ignored in his paper and which in my opinion is
the saving grace in this work; that is the shipmodel correlation data which can be obtained
from properly instrumented ship and model experiments. I think that may well enable us to
use simpler types of experiment to determine the ship's response.

G. VOSSERS:

The author has brought to the attention of the ship model experimenter some very
important points concerning the limitations of the model experiment, especially in relation
with a study of extreme conditions.

I wholeheartedly agree with the conclusions that a model experiment for predicting the
extreme conditions which may occur on a certain vessel requires either an enormous amount
of testing or is insufficient.

We found the last year in our seakeeping laboratory the same difficulties for designing
fruitful experiments for different shipowners, especially concerning wetness and speed loss.
In this seakeeping laboratory an additional specification is playing an important role: the
wave direction.

However, is the prediction of extreme conditions for a certain ship the only question a
designer may put? Our experience the last year is that the question not very seldom is also
put in the form: In which direction will certain changes in the form of the ship influence the
behavior in waves? When the question is put in this form, it is possible to carry out an
experiment, which may give some guidance for the design of the ship, by testing several
modifications of the ship in certain wave configurations.

Even if the wave conditions are not such that the extreme conditions can be expected,
the comparison of the modifications of the ship may show useful results. I think that will be
the most important task of the seakeeping basins at the moment.

The final proof of this method of testing is, of course, the comparison with full-scale
conditions.

We are, therefore, very pleased, to be able to duplicate the full-scale tests, with the
three destroyers of the Royal Netherlands Navy (see chapter 26), in our model basin with
three models of these destroyers. The results of these correlation tests will be very important.

W.H. WARNSINCK:

The introduction of this paper is a masterpiece of sound reasoning. The seeds of his
Directors 1946 SNAME paper found fertile soil in Dr. Cummins' mind; the introduction alone
would have made a very valuable contribution to the Symposium.
Two facets of Dr. Cummins' paper might be brightly illuminated. "Extreme conditions" or "events of low probability" have nothing to do with typhoons or hurricanes, but occur during almost any ship's voyage of, say, four months. They occur, in fact, every time a ship's captain has to decide, based on his seamanship, about the necessity of slowing down or changing course.

As second point, Dr. Cummins shows us the limits of the sometimes overrated possibilities and advantages of model tests—and in daring to disclose these limits he does the model experimenter a great favor. He shows the profession that, even when following the wake of Professor Davidson's 1951 Complex Seas paper by sifting out every unnecessary, every impractical, and every unrealistic part of a systematically founded program, there still remains for each ultimate goal—designing a seakindly ship—a very long time and money-consuming test program.

Having realised how long such a far-from-complete but sufficiently informative model test program would be lessens the advantages of seakeeping model tests over full-scale trials. A ship may spend weeks at sea before encountering the sea conditions wanted; a model test program, giving us the same amount of information, might take just as long as that. Far from wanting to belittle the tremendous possibilities of wavemaking model tanks, I might stress the realisation that full-scale trials, compared with model tests, are not as inefficient as sometimes assumed.

Finally, ship motions students, wise professors and young pupils alike, are begged to remember in their enthusiasm for theory and model tests, especially when studying events of low probability: ships are manned. Not only will these men, captains and mates, cause or stop highly interesting phenomena; they are, at sea, in their own ship, in dirty weather and heavy seas, the most valuable source of information and experience an e.i.p.-investigator can ever wish to have. Model tests lack wise sailors' judgement.

AUTHOR'S REPLY:

My little paper, which has a very limited objective, and is directed toward those specialists concerned with "tankery," seems to have stirred up a hornet's nest of discussion. I have been accused of being a pessimist, condemned for daring to discuss extreme values, and criticized because I am in favor of model experiments. I have aroused the fears of some for admitting that model tests do not answer all questions the ship designer asks, and I have been praised by others for frankly stating the difficulties which the model tests face in attempting to answer these questions. All this is as unexpected as it is gratifying. Considering the wealth of excellent papers being presented at this meeting, I am flattered by all this attention. You will pardon me if I attribute this response as much to the position of the paper near the front of the volume as to the quality (or lack of it) of the paper.

With Dr. Chadwick, I agree almost completely. I believe his philosophy is sound and his objective is commendable. However, its achievement is still a long way in the future and
in the meantime we will have to consider many problems imposed upon us by the ship designer. I am afraid they will not wait. And I suspect that before the computational method is satisfactorily developed, model tests will have told us a vast amount about the behavior of ships in waves, even if we cannot carry out adequate measurements of all the parameters of concern under all conditions.

Mr. DuCane has asked how to design an experiment to reproduce slamming conditions. This is a very difficult question, actually much more difficult than the question posed in this paper. Before it can be answered, I would need to know the type of information desired. If only information about the magnitude of the hydrodynamic pressures is wanted, I see nothing wrong with tests in regular waves. In our present state of ignorance, I would simply try to find a variety of regular waves in which slamming occurs with varying degrees of severity. This should give the orders of magnitude of conditions for which one must design. It should also reveal the pressure time histories one can expect, and other details of the phenomena. On the other hand, if you desire some knowledge of the statistics of slamming, this will certainly require tests in random seas, and, I am afraid, in short-crested seas as well. Tests that I have seen at Wageningen suggest that the incidence of slamming is very sensitive to the two-dimensional distribution of the spectrum. A valid test would probably require the most realistic reproduction of the seaway that can be achieved.

Since Dr. Davidson admitted that he did not read my paper, he would not mind if I say that most of his comments are irrelevant. However, as we have come to expect of Dr. Davidson, his comments are always stimulating, and sometimes profound. His concluding remarks about the errors that we can expect in estimating parameters associated with seakeeping are an example. There is a warning here for us all, and I, for one, will not argue when he suggests that our predictions may be off by 100 percent! When one is dealing with phenomena which are probabilistic in their nature, and which are not truly reproducible in our models, large errors are inevitable. Dr. Davidson has remarked that I am trying to provide a "cookbook" for the design of an experiment. Of course he is right, but I must warn that unless the cook who uses this particular recipe knows what he is about, someone is going to get a bad case of indigestion.

Dr. Pierson does not admit that he has not read my paper, though on the basis of internal evidence he has apparently not gone beyond the title. Needless to say, much that he has said is also irrelevant, though from the way in which the discussion was given, one would think that it was a detailed account of the sins that I have committed. Shocked that a naval architect should have the effrontery to discuss extreme values, he has proceeded to create a straw man and to belabor it thoroughly.

I quite agree with much that Dr. Pierson says. However, it has virtually nothing to do with the problem under discussion. He speaks of the difficulty of treating hurricanes and typhoons. I quite clearly state that I am concerned with conditions in which the sea has an approximate linear description; in other words, not so very severe. If this excludes all
conditions of interest, then perhaps we had better discard all this beautiful statistical theory Dr. Pierson has sold us. He mentions breakdown of distribution functions over time periods as great as the life of a ship. I consider quite short time intervals, lasting perhaps a few minutes, or at most a few hours. If it is not proper to discuss extreme values over such a period, then there is a certain Hydrographic Office publication written by Dr. Pierson which needs rewriting. He further objects that extreme conditions are likely to be nonlinear. Of course they are! And the ship model provides one of the most powerful techniques for handling nonlinearities. Incidentally, Dr. Pierson, Froude scaling has nothing to do with linearity or nonlinearity. It is simply the condition arising when accelerations at model scale (i.e., gravity) are to be the same as accelerations at ship scale (i.e., gravity).

Professor Lewis has raised some important questions which must be answered with care. First of all, he feels that I am "unduly pessimistic." I would prefer to interpret my attitude as one of dissatisfaction that we cannot simulate prototype conditions better. This feeling is the inevitable consequence of the fact that the only perfect model of a physical system is the system itself. Certainly I was not attacking model testing in confused seas. Such tests have their place, and an important place it is. However, as Dr. Chadwick has suggested, such tests are likely to be impracticable and completely uneconomical when you are trying to make quantitative investigations of extreme conditions of the sort that I discussed in this paper. Suppose, for instance, that this operation we are considering takes 30 minutes, and the probability of a certain condition being exceeded is desired to be less than 0.05. By taking 42 runs in the Stevens tank with different seas having the same spectra, he can provide a total running time equal to 2½ hours full scale, or the equivalent of five operations. Many more than 42 runs would be required to establish conditions of such low probability. And further, as Dr. Chadwick has said, there will be other parameters to investigate, such as the effect of speed. The investigation quickly blows up into an enormous number of tests, and it becomes completely uneconomical. Further, if such a program were to be attempted, it would be valid only if many different wave programs corresponding to the same spectrum were used. If, as Professor Lewis suggests, the same program were used, with the model starting at different times in order to obtain different time histories of wave encounter, the results would still be heavily biased by the detailed statistics of the program. Any extreme conditions which are not inherent in this particular sample will never be encountered. And since the sample is relatively short, such limitations could be serious.

Professor Lewis suggests that the problem can be simplified if the time of operation is reduced by the designer. It has been assumed that in the early part of the project, during the bargaining operation between designer and model tester, this time has been reduced to a minimum. This certainly simplifies the problem in every way.

He suggests that statistical extreme value theory can be used to advantage. Here, part of Dr. Pierson's argument is relevant. The theory of extreme values depends very much upon the tail of the distribution function, and nonlinear effects can seriously modify the shape
of the curve in this region. It must be remembered that the distribution function is obtained from empirical observations which lie for the most part in the linear range. Prof. Lewis mentions the distortion of waves by wind, and this may be an important problem. I hope that the oceanographers can give us some information on this. An equally important factor may be the effect of strong wind upon the motions and steering of the vessel itself. This is an area for research.

If the limitation on the hypothesized operation is associated with motions, and the wave lengths are of ship length or greater, then certainly conditions near synchronism are the most important to investigate. But I have been concerned not only with motions, but also with such problems as wetness. During a certain investigation of just the sort we are considering here, we were concerned with green water coming over the forward deck. We found that the worst condition—but certainly not the worst motions—occurred with waves which were only three-quarters of the ship length. And this wave length just corresponded to the peak of the spectrum for the seaway which was being investigated.

Mr. Vossers' confirmation of the vast amount of testing required to investigate adequately problems of extreme conditions is most welcome. It is clear that when the additional parameter of heading is subject to variation, the labor will be enormously increased, and a test design, following the procedures suggested in this paper, may be far from simple. Nevertheless, a much more satisfactory investigation should be possible.

This problem of extreme values is certainly not the only type of problem the designer gives us. In fact, it is a very limited problem and we can hope, because of its difficulty, that it is not placed before us very often. This paper was written because we have had problems of this type several times, and because the theoretical considerations involved in the attempt to solve them exhibited certain aspects which seemed to be of general interest.

I regret that Mr. Tupper feels that I am letting the tank experimenter down when I reveal the difficulties of designing a proper experiment. However, I cannot agree that the existence of these difficulties should be a closely guarded secret. If they do exist, and my experience is that they are always present in one form or another, then we should admit this fact to all concerned. If we had not proved our value to the designer on the basis of what we can deliver, then perhaps we had better go into some other business.

The problem of ship model correlation is one of the most difficult we face in seakeeping research. Certainly we can never hope to demonstrate the precise correlation which might be possible in the smooth-water case, because we cannot precisely duplicate, or even define, the sea in which the ship trials are carried out. Therefore, the correlation must be at best statistical, and we must not be surprised if we occasionally find large discrepancies, as Dr. Davidson has suggested.

Since I feel that Commander Warnsinck is really more on the customer side than on the model basin side, his approval of my description of the bargaining process is most welcome, and my faith in the policy of stating the difficulties that exist is strengthened.
I can agree completely as to the importance of full-scale work in seakeeping investigations, and I feel that trials will always play a most important role, much more so than in smooth-water programs. The factor of the human element is fundamental, and its simulation in any sense at model scale will be almost impossible. At the Taylor Model Basin we devote a major portion of our effort to sea trials, and I can only see this phase of activity increasing.

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<td>ALUSNA, London, England</td>
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The problem is considered in which information is needed as to the nature of extreme conditions a ship is likely to encounter during a definite short period of operation in a well-defined random sea. Various model experiments are designed which are of increasing complexity and validity, each being an “optimum” design for a given degree of sophistication. The approach is based on the statistical properties of the ship-seaway system, and the resulting experiments are intended to provide a maximum of valid information from a small number of runs.