RESEARCH NEEDS FOR BETTER WAVE FORECASTING: LEWEX PANEL DISCUSSION

A panel discussion may not be the best forum to produce reasoned arguments and logical proofs, but it does promote spontaneous exchanges. Within the unguarded remarks and the generally loosely woven fabric of conversation among colleagues and friends is an outline of our perceived needs for research to promote better wave forecasting. The problems and research needs presented in this edited panel discussion have existed for many years, but adequate observations from satellites, thoughtful data assimilation schemes, and a better understanding of the underlying physics promise a new day for research in numerical wave forecasting.

M. Donelan

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Peter Janssen	Royal Netherlands Meteorological Institute
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Opening Remarks

DONELAN: The two things that have struck me most about what we have learned in these past few days are that (1) one has to be very careful about understanding winds, and (2) it is apparent that all models do not have sufficiently good agreement that we can be complacent about our understanding of the modeling physics. I would like to ask each member of the panel to comment on how we should focus our attention in the near future to improve wave modeling.

EZRATY: The most important thing to me is the differences I have observed in the various experimental estimates of the wind fields. I am still wondering how we could more accurately take into account the real nature and variability of the wind. I would therefore like to put this question back to the modelers: How do you plan to better describe this wind variability in your models and demonstrate whether it can, in turn, improve the results?

BANNER: There *are* questions about the models. We need to consider the effect of waves on the drag coefficient and then feed that effect into the input source function. Proposed dissipation source functions need to be thoroughly tested, for example, with the extensive set of measurements proposed for SWADE [Surface Wave Dynamics Experiment; Donelan, 1987] in the winter of 1990–91. One could compare the model predictions with various asymptotic limits predicted for a fetch-limited situation.

HASSELMANN: In this symposium we have seen spectral intercomparisons among nine different models. We really do not have a basis for deciding which model is correct or where the model errors lie. I think the problem lies in the wind field. I think knowledge of the wind is necessary to tie down modeling inconsistencies. In SWADE, for example, that should be top priority. The LEWEX analysis, on its present level with these nine models, reminds me very much of the analysis we did with the SWAMP [Sea Wave Modeling Project] Group study [1985], where we had no measured winds at all. We just used several idealized wind field cases to find out how they were working differently. I think one can still do a very nice job in LEWEX on this aspect of the problem, but to assess the model performances in absolute terms may not be possible. My suggestion would be to go beyond the SWAMP level of analysis, look at the problem as a "joint" wave-model plus wind-field analysis problem, and try to do a data assimilation, or inverse modeling, to try to get the best wind field to fit a given wave model, and then to examine the relation between wind field uncertainties and model uncertainties. That would be a new approach, at least from the point of view of modeling.

LEWEX has a very nice SAR [synthetic aperture radar] data set and a nice set of model results with buoy measurements. The LEWEX data set is unique for looking at the SAR wave spectrum and how to invert it. I am more optimistic that we can make progress there than in the identification of the model errors. If we want to understand the problems with the models and to improve the physics, we have to develop third-generation models further, because when we find a problem, for example, we can decide if it is the source function and then fix it. If we have problems with a first- or second-generation model, we are always tinkering with the model results and not with the physics in the model.

JANSSEN: I agree with Mike [Banner] that it is really high time to look at the sea-state dependence of the wind stress. At ECMWF [European Centre for Medium-Range Weather Forecasts], we are attempting to couple the third-generation WAM [Wave Model] with the ECMWF atmospheric model. We already have some encouraging initial results. They show that a young wind sea increases stress by 20% to 30%. When compared with the present WAM, the increased stress will produce faster growth in the initial stages that might be fairly important. So there is a need to couple the planetary boundary layer model with WAM. Second, there is a need for improved knowledge of wave energy dissipation from wave breaking. At the moment, we are using dissipation as a source term, which probably works very well for a wind sea. But we are not certain, when we have complicated wind sea/swell cases, whether this formulation works.

I have two comments on the LEWEX intercomparisons. LEWEX presents a unique opportunity: we are comparing two-dimensional spectra, both measured and modeled. However, it is not clear that the differences are statistically significant. I have not seen any error bars. People are identifying peaks and directions, but probably the number of degrees of freedom in those peaks is so small that I really doubt the differences are significant. Second, I have the impression, looking at the measured spectra, that we should compare only mean parameters, such as mean wave height, mean direction, and mean angular spread.

PHILLIPS: I think this has been a fascinating meeting, and some most remarkable results have been presented. There is a lot about the results that both confuses, as Klaus [Hasselmann] said, and also stimulates. We have a set of models that sometimes produces results that are consistent among themselves, but are very different from what a buoy seems to produce. Sometimes there is no agreement even among models. How then do we decide?

It is clear that we need to improve the connection between the modeling and the observation. Is the wind field the problem? That seems to be the thing that we blame, in the way that fluid mechanicists, if their theory and experiments do not agree, always blame turbulence. We can always blame the wind field because it is not right to start with. Have we used all the physics in the models that we need? I suspect there are a few little bits and pieces even in the third-generation models that are left out. Should one keep track of all the very-low-energy density levels in the ocean that may serve as a starting point for future instabilities? Presumably, that part of the physics is involved, but is it a part that we are going to keep track of? There are a lot of things we can do with the LEWEX data. There is a lot we can still learn from them.

DOBSON: My first comment is one that Bill Pierson will appreciate. I have now been to four conferences of this nature over the last five years, and at every one, the wind speed and the wind field were blamed for inconsistencies in model results. So nothing has changed. Having said that, from an experimentalist's point of view, what measurements might we consider over the next few years in order to fill some of the gaps that I see here?

The first one is a set of careful sea-state versus wind-stress intercomparisons, with microwave sensors present. Klaus will agree with me that that is absolutely crucial to the success of his highly optimistic plans for coupling wave models with atmospheric–oceanic numerical models in the hope of understanding the air–sea fluxes. At the moment, he is saying his wave model does not really understand the drag coefficient, but he is also saying that he will be using his wave model to calculate the drag coefficient over the entire globe in order to learn something about the air–sea fluxes. So I see an inconsistency there, and such measurements might get around that problem.

I see a need for some young wave measurements of the input source function and some detailed quantitative optical/microwave/hydrodynamic field determinations of wave dissipation as well. In particular, for the LEWEX intercomparison, the models should not only have used the same wind field, they should also have used the same wind scaling. I do not think all of them did.

The second thing that really struck me forcefully about this intercomparison was that the buoy measurements, and maybe the SAR measurements too, were woefully inadequate for the job at hand. They did not define the wave field sufficiently for anything to be said about how good the models were, in my estimation. Whoever designs the next wind–wave experiment has to think hard about an adequate measurement strategy. I have to say the same thing about the wind measurements.

DONELAN: From the preceding comments, it seems to me that a few things emerge as representing a quite clear consensus: I vote with the six panel members who insist that source functions need to be improved. Everyone agrees that the wind measurements need to be more carefully dealt with. These seem to be the two issues that are the crux of the matter. There is, of course, a need for much better measurements of waves, as well as of the wind. Klaus has pointed out that the SAR may be a good candidate to measure the waves. Other microwave sensors may be also.

The point has been made, principally by Klaus, that *third-generation* wave models are needed to test the physics. In other words, the model has to be structurally correct before one can hope to use it as a tool to determine where the physics may be in short supply.

Peter [Janssen] raised the issue of statistical tests, which in my view is one of the things that emerges most clearly from intercomparisons of this sort. We do not really have the necessary structure to say what is correct and what is not, or how well one estimate compares with another, although Tom Gerling [to be published] has made some strides in the right direction. We need a consistent set of statistical criteria that everyone agrees on.

Wind Measurements

PIERSON: I have been interested in measurements of the wind for a very long time, even before Skylab and Seasat, when problems of validating the winds recovered by a scatterometer by means of conventional data first came up. It is impossible to get a decent 10- or 20-minute average from a conventional ship anemometer. Most observers are so poorly trained that they often cannot even obtain true wind from relative wind. Most modern ships have microprocessors that could keep a running account of the wind speed and direction, just as if the ships were data buoys. Large improvements could be made, just by automating the present ship observations. The poorest parameter in a conventional ship report is the wind data, but it may be the easiest to correct.

My second point is the propagation of swell. From what I have seen of the various second- and third-generation models, I think many of them do not propagate swell correctly. Wave propagation is equally important in areas of wave generation, so that many of the discrepancies found by Gerling [for example, the tendency for all models to predict the arrival of swell earlier than it was actually measured] may be partially explained by this error. If swell arrives too soon, then it also left the area where it was generated too soon. The waves in the areas of wave generation diminish too soon when the wind dies down. For validating forecasts of sea plus swell with frequency spectra off the west coast of any continent, I think that within one winter, from the data, it will be clear that WAM is not doing it right. You might look at techniques used in the first-generation SOWM [Spectral Ocean Wave Model; Pierson, 1982]. Great circle propagation on a sphere is not difficult. The envelope of each spectral component should be translated at its group velocity each time step, with no change in form. [For this problem, Lagrangian methods are superior to Eulerian methods.]

ARCHER: Regarding this problem of accurate wind measurements from ships, Peter K. Taylor of IOS [Institute for Oceanographic Science, Wormley, U.K.] has been working on it. The only way he has been able to get good wind measurements is with instruments mounted over the bow. They are now so equipping WMO [World Meteorological Organization] ships.

The Inversion Problem

PHILLIPS: I would like to suggest that an effort be made to use all the measurements during LEWEX that were gathered from the buoys, the aircraft overflights, and so forth. Each certainly has its own limitations, but surely they could be put together in some way to get *an* optimum estimate of the wave field. Each of those measurement devices has its own transfer function, and the spectra we see are the end result of those separate transformations. For example, there is a lot more information contained in the SCR [surface contour radar] spectrum, which could serve as a constraint on what you might call the "true" spectrum. Of course, the SCR has its own limitations, but all these sensors are supposed to be measuring roughly the same thing, even though each is reporting something different. It should be possible to produce an optimum estimate of the wave field, using all the information you have available. Such a goal is worth pursuing.

HASSELMANN: If I understand Owen's [Phillips] comment correctly, it is the same question that I was asking about the inverse modeling problem: Can you get from the observed wave data and the observed wind data to an optimal estimate both of the wind and wave field simultaneously? I think you can solve that problem only if you have a wave model for a dynamic interpolation in space and time between the rather few-and-far-between measurements. At the same time, you need the wind input to whatever extent it is available. Then you try to find the best fit to all of the available data that is consistent with the dynamics of the wave model. I think if one tries to go through that exercise with the LEWEX data, one would learn a lot about the models and also about the ability to reconstruct wind and wave data simultaneously. This is the problem we will be facing very much in the future, when we begin to acquire global wind and wave data sets from satellites again. The LEWEX data set is a good opportunity to pick up that challenge, and to gain some experience in one's "backyard," with a smaller data set, over a reasonably well-defined area.

DOBSON: Just a brief addition to that, Klaus. I think that there is another part that needs attention. Of course we have to look at the inversion problem. But we must continue to calibrate the models we have in terms of the data from experiments that we think are good. For instance, the JONSWAP [Joint North Sea Wave Project] data set could be reanalyzed using some of the ideas that came out of Mark Donelan's Lake Ontario experiment, which, as Hans Graber of Woods Hole pointed out to me, allows one to reconstruct the wave direction at a given fetch, knowing the wind direction. Even though you did not have good directional spectra, you could still go back through those data and calibrate against the projection of the wind in the direction of the wave, instead of the wind itself. We need to have a consistent calibration for the model in terms of wind speed before we will progress on other fronts.

Wind Variability

GLAZMAN: A comment about wind variability. In both wave theory and measurements, it is common to use the *mean* wind velocity. However, the corresponding wave number [k] spectra for air motion are dominated by an inertial range that has the form $k^{-5/3}$ or even k^{-3}. The magnitude of this exponent is, in a certain sense, rather small, equivalent to a cascade pattern in the geometry of the wind field or in its temporal history. As a result, the averages are difficult to define; strictly speaking, a "representative" averaging period for the wind does not exist. An alternative approach to the specification of such multiscale fields is being developed, based on fractal and multiple fractal formalisms [Schertzer and Lovejoy, 1989]. This approach appears promising also because it gives an adequate characterization of the highly intermittent [gusty] field of air motion.

Open ocean waves are usually highly developed, whereas in LEWEX, one is often dealing with a rather poorly developed sea. The inverse wave age [ratio of wind velocity to wave phase velocity] is typically greater than one or two, or even three. As a result, there exists a significant portion of the wave spectrum where the energy flows to larger scales. This inverse energy cascade is, I think, important for wave modeling. Since the energy eventually must be dissipated somewhere, the inverse cascade necessitates alternative dissipation mechanisms effective at large scales. For example, one may consider large-scale internal waves or currents as a possible sink of wave energy.

PHILLIPS: Energy transfer to larger scales is already intrinsically in the third-generation model, in the wave–wave interaction calculations.

KATSAROS: I wonder what the wind variability might do to the wave field. The models perform so differently from the measurements. Could it be that these fluctuations in the wind generate something that interacts crosswise? Might there be some kind of extra dissipation or changes in the model assumptions that could come from these subscales that are not described in the wind field? Might there not be errors from the various grids that were used?

JANSSEN: Gustiness has an enormous effect on the growth of the waves, especially the longer waves, which are affected by a factor of 2 or 3. I have been looking only at the large-scale effect, but it is enormous.

PHILLIPS: Perhaps one should reexamine some of the older measurements on wave growth. After all, random functions that depend upon each other in an other-than-linear way are not going to be related according to their means. Perhaps instead of trying to express our models in terms of an average wind speed, we should use the cube root of the average cubed wind speed, or something like that, depending upon the physics that is involved. If we look more carefully at the physics, to find out what function of the wind speed is producing it, we might get a lot less scatter in some of our experimental plots.

JANSSEN: I think we can do that already. The usual wind growth curve is fairly nonlinear. So Gerbrand [Komen] and I have looked at the fluctuation in the spectrum with the proper probability distribution function. From that we can calculate the effect of nonlinearity.

Surface Currents

VALENZUELA: I think we do need better measurement of the wind field, but geostrophic currents may also be important. Local currents can focus and defocus waves. You may have to do a modeling of waves with and without currents. Converging wave rays do not necessarily identify the source location.

HASSELMANN: This is an issue also for SWADE. My view is that currents are not very important in the ocean for most of the waves we are looking at, since we do not have a monochromatic wave field in the ocean but a continuous spectrum. I think a typical eddy current field will quasi-focus only small parts of the spectrum at a given time. The eddies just mix up the wave field, and, as we have a Gaussian wave field anyhow, they will not be noticed in a reasonably broadband measurement of the spectrum. Across a large shear zone like the Gulf Stream, they might be, but I would think that even there the eddies would not be very important. We are planning to do some experiments with WAM, both with and without large eddies, to see what effect they have on the wave field. In JONSWAP, tidal currents of 1 m/s really had a negligible influence on the observed waves. But I agree it is certainly a question to look at.

HOLTHUIJSEN [added in proof]: Recently, in the fall of 1989, Hendrik Tolman and I transported waves across a ring and across a straight section model of the Gulf Stream, courtesy of Scott Glenn of Harvard, with a third-generation wave model that included all relevant wave–current interactions. The computed wave modulations were significant, sometimes creating a significant wave height enhanced from 8 to 10 m in the countercurrent part of a ring. The modulations, in general, were restricted to an area of about two ring diameters.

Friction Velocity

MITSUYASU: In this meeting, I was surprised to find rapid progress in measuring techniques, in analysis techniques, and also in numerical modeling. But I would like to stress the importance of fundamental studies. In my opinion, we have presently exhausted the stock of good results of fundamental studies. So we need again to accumulate good data. I would like to show one example.

These [see Fig. 1] are laboratory data on the growth rate of waves under wind action [Mitsuyasu and Honda, 1982, Fig. 15]. At first sight, the result appears to show a reliable relation between dimensionless growth rate of water waves and dimensionless friction velocity of wind. However, because the coordinates are logarithmic, there is actually large scatter in the data. The scatter is larger for waves containing a surfactant, that is, for waves with a smooth surface. These data were obtained from a very carefully controlled experiment. The friction velocity u_* is also measured very carefully. Therefore, there still remain problems in understanding even such a fundamental process.

PIERSON: The major difference between WAM and other models is that, in WAM, dimensionless variables have been parameterized in terms of u_* instead of the mean wind at a 10-m height. This has a very important implication, having to do with the fully developed sea. The first important parameter from any model is the significant wave height for a fully developed sea. In the recent paper describing WAM [Wave Model Development and Implementation Group (WAMDIG), 1988], one can pick off the asymptotic value for large fetch and put that into the dimensionless energy. With a modest amount of algebra, one can get the significant wave height as a function of the 10-m wind. It turns out to be equal to a constant times the square of the 10-m wind, plus a second constant times the cube [Pierson, 1990]. We have been working for many, many years with the concept that the significant wave height is proportional to the square of the 10-m wind. One could try to see which assumption looks better compared to the Ewing and Laing [1987] significant wave heights for a fully developed sea, expressed in terms of the 10-m wind. The WAM assumptions make quite a difference; for example, they drastically change the behavior of the first-generation GSOWM [Global Spectral Ocean Wave Model]. The waves grow much more quickly at high winds. Up around 15 or 20 m/s, they are much higher than the square law would predict for the WAM drag coefficient. There is a spread of about 5 m in height for three or four of the most popular representations of the drag coefficient in the simple version, where drag coefficient is proportional to some constant plus a second constant times the 10-m wind. The crossover point is about 12 or 13 m/s. Below that, fully developed seas are lower, and above that they are higher. It might be worthwhile to check this discrepancy in as many ways as possible.

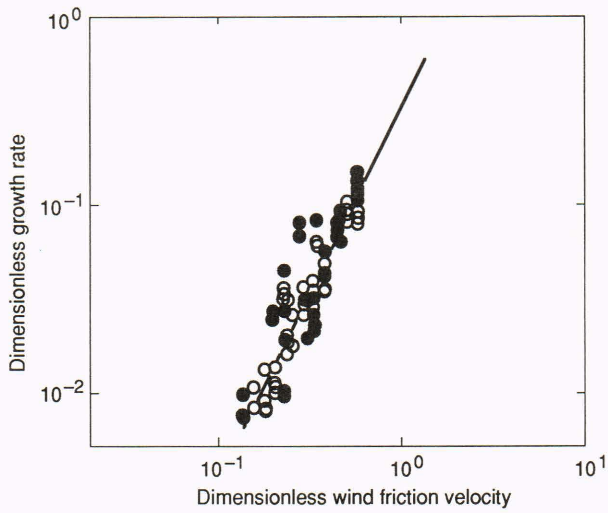

Figure 1. Dimensionless growth rate of waves as a function of wind friction velocity, both with surfactant (solid circles) and without surfactant (open circles). (Reprinted, with permission, from Mitsuyasu, H., and Honda, T., "Wind-Induced Growth of Water Waves," *J. Fluid Mech.* **123**, p. 440, © 1982 by Cambridge University Press.)

HASSELMANN: Both of the previous speakers have made very good points. First of all, what Professor Mitsuyasu was saying is very true. We are now discussing, for example for WAM, switching to a different input source function that has this u_*^2 dependence, based entirely on lab data. We really do not have in my view good convincing field data that would force us to switch, except for some secondary effects regarding the momentum transfer. But what really forces us to switch are these lab data, so I would very strongly support the need to do more basic studies for the modeling. We cannot depend entirely on the field data; we are very much dependent on sorting out the different processes in the lab.

From the point of view of the amplitude, or peak frequency, you can live with the present source function of WAM or with the u_*^2 source function. It does not really make much difference, because the dissipation term can be tuned to get the same results. The main difference between the two source functions is in the momentum transfer, which depends more on the high frequencies. Again, I think we would not have been forced so strongly to consider changing our source function if we did not have these very good lab data.

To come to Bill Pierson's point, it is obviously very important whether we have a u_* or a U_{10} [wind speed at 10-m height] dependence in our source function if the drag coefficient is a function of wind speed. We looked at that question because we are aware, of course, that we would get much higher wave heights at the higher wind speeds than we had before. We talked to a lot of people. The general feeling was that it was okay to go to u_*, and we do indeed get the higher wave heights, but the data supported it. Because most people agreed, I myself was very comfortable just to relax and believe it. But if anybody wants to look at the data more closely and say that we should go back to U_{10}, we would immediately do it, because we really do not care, from the point of view of modeling. We simply put into the model whatever the latest theories on wave growth tell us. In summary, Bill, we did look at the data before we made that change. We were aware that it was an important change at high wind speeds.

PHILLIPS: Underscoring the importance of u_* versus U_{10}, although Professor Mitsuyasu did not mention it, the results he showed were plotted versus u_*, but the *mean* winds at a given value of u_* varied by a factor of 2, as I recall, between the absence or presence of a surfactant. Only when you use the u_* does the scatter collapse. The mean winds corresponding to a given u_* were very different in the two cases.

DOBSON: On Klaus's remarks, there are two important points. One of them is in the usage of the model going from U_{10} to u_*, which I understand Bill was talking about. The other is in the calibration of the model. Both are important; both matter in the final result. You say in the recent WAM paper [WAMDIG, 1988] that we should refer our results to u_*. People who calibrate your model use U_{10}. They have to use some drag coefficient to produce a result in u_* so that they can provide something for you to calibrate your model with.

PHILLIPS [with humor]: Sounds a bit circular to me.

JANSSEN: Regarding the u_* scaling, if you assume the Charnock relation for the roughness, you analyze the boundary layer, then you just end up with u_* scaling. There is no way around it.

DOBSON: That produces a number quite similar to all of the long-fetch U_{10} versus u_* relations if you use the Charnock relation. It does not reproduce the wave age dependence that people like Mark [Donelan] see.

JANSSEN: Oh, no. That is why we are looking at it now.

LEWEX Error Bars

DUFFY: I would like to turn back to an earlier point regarding verification of models and how we do that. Peter Janssen made a comment about error bars, and I am curious if the panelists have some suggestions of how they might be established. In the atmospheric sciences we have fairly decent data over the continents. We can do rms errors, correlation coefficients, and so on. But the data that are around for wave model verification purposes do not seem to be accurate enough to do that. Are the panelists suggesting we might do some data impact studies, perhaps in the Southern Hemisphere, examining different types of data, trying to get an idea of how those data are handled in the models so that we can verify them?

HASSELMANN: I was not myself terribly concerned with this problem of error bars in LEWEX. I think all we have to do is put an error bar on the plot. We know how big it is for most of these spectra anyway. I did not understand Fred Dobson's comment earlier that the data were woefully inadequate to test the models, unless he was referring to a compass error of 30° or so, which occasionally appeared in one of the buoys. But apart from that, we have a fairly good idea of how good these maximum entropy techniques are for reproducing two-dimensional spectra. I had the impression, from the structure of the spectra that we saw, that they could be well reproduced by the maximum entropy techniques. In other more conventional spectra, it is just a question of the number of degrees of freedom. So I did not think it was important. Maybe I am confused there. It would be good practice obviously to put in the error bar so people know how many degrees of freedom you have. But in nearly all the LEWEX data, it really was not a big problem.

DOBSON: The only things I felt badly about were that there was only a single measurement at each ship and that there were big differences between the modeled and observed wave field at each ship. And I thought that these single measurements were inadequate to define the measured wave field. There *were* some excellent wave measurements from the NASA aircraft instruments. I only wish that there had been more.

DONELAN: This raises a more general question. Do we need some statistical structure different from the rather loose one we have now in order to compare models? And should a group like this try to develop that?

Ship As Wave Sensor

BALES: Perhaps the ship is the best wave sensor of all. Knowing the wave field, you can repeat over and over in a towing tank the ship responses, to say 10%, through about sea state 6. In Trondheim, Peter Kjeldsen is recreating the motions of the ship that were measured at sea, given his best estimate of the wave field. Owen Phillips suggested earlier that none of us would agree on which model is most correct. We might consider developing a standard set of ship response transfer functions that could be applied to all types of wave data.

DONELAN: Wouldn't the same thing be true of buoys? How does a ship differ from a buoy in that regard?

BALES: I do not think we have a good handle on the 6-degree-of-freedom motions in a buoy. Buoy manufacturers might disagree. There is a wealth of theory going back thirty years for predicting ship responses. It seems to work very well now, both in unidirectional and bidirectional seas.

HASSELMANN: I think many of you probably know that this idea was followed up by Tucker in his shipborne wave recorder. There is one problem: you can determine the ship response given the wave field, but going back to the wave field from the ship motions is more complicated for a ship than it is for a buoy. That was the main reason that people switched to buoys and gave up the shipborne ocean wave recorder. I think Bill Pierson himself worked quite a bit with those data and was not too happy with them. My recollection was that the data were not as useful as one hoped they might be.

DONELAN [with humor]: Forgive me, Klaus, but I have the suspicion that getting from the ship motion to the wave field is probably no more difficult than getting from the SAR image to the wave field.

HASSELMANN: It is a question of the platform velocity. I know the velocity of a SAR. There is no captain out there fooling around.

PIERSON: The Tucker shipborne wave recorder works best when the ship is hove to, or progressing at perhaps a knot into head seas. There were problems in calibration with the Tucker recorder. The most fascinating thing ever done was to put the accelerometer on what we in the U.S. call a Ferris wheel and measure the acceleration. It worked surprisingly well at very low frequencies. The equilibrium spectral form, proposed by Pierson and Moskowitz, and which led to the SOWM, was developed using these data. Also, you can control the vector velocity of the ship, change its heading every 10° in a steady sea, and get a long record. Then there is the horrible problem of matrix inversion to pull out the spectral components. You could not dream of trying it five years ago, but today you could do it.

Model Seeding Mechanisms

HOLTHUIJSEN: I have been puzzled that in WAM there is no Phillips mechanism. I was not overly concerned until recently. In WAM, it is not really a problem, because an initial spectrum starts off the model. But that initial spectrum has moved out of the model after a few days. If then the wind turns, there is nothing in the new wind direction to start the waves from. So you may have a much slower growth because the initial spectrum has moved out of the model, and there is no Phillips mechanism. I do not quite understand, if the computational effort is marginal, why we do not put that mechanism back into WAM?

HASSELMANN: Maybe we could put that mechanism in as a trigger to get things going. I guess that is the point you are making. It is apparently a very small term if you just consider the measurements of pressure fluctuations in the atmospheric boundary layer and make a reasonable assumption on how they are distributed in the wave number domain. You require the spectral density of that wave number distribution on the dispersion curve. That triggers the growth, and you come up with a factor that is about 10^{-3} smaller than anything that you need in a model to get things going. So I really do not think it is a very important term. The mechanism is still extremely interesting, though, as a physical process. The reason it is small is because it goes as $(\rho_{\mathrm{air}}/\rho_{\mathrm{water}})^2$, rather than simply $(\rho_{\mathrm{air}}/\rho_{\mathrm{water}})$.

But I think Leo Holthuijsen's point was that one would like to have *something* to trigger the waves. He is quite right. The waves start off at very high frequencies. The way they start does not really matter very much, because the time it takes to grow through to equilibrium is short. So the model is not sensitive to how you seed the energy at high frequencies. But you do have to have the energy in there in the beginning. Because WAM has a prognostic cutoff frequency of 0.4 Hz, we very often do not have any energy there if we simply turn the wind. We have to wait until the energy diffuses through the nonlinear transfer, crossing into that part of the spectrum. That process is probably too slow, and we have been discussing whether maybe we should be putting in some seeding energy at high frequencies to get the thing going down there.

PHILLIPS: It is fairly unusual for the sea to be so calm that there is not half a centimeter of fairly low frequency oscillation sloshing around. That would serve as a seed at the low frequencies as well.

HASSELMANN: Well, I think there is some energy there, but it is probably just too weak, because the rms slope is small compared to what you need to get things going in the high-frequency part. In other words, when the wind starts blowing from another direction, you start building up a short wind sea with rather high slope, and the nonlinear transfer can pick up pretty quickly from that and bring it in.

PHILLIPS: Yes, the nonlinear transfer will certainly do it, sooner or later. But I wonder whether in nature the wind input into those longer components is not building the energy up more rapidly.

HASSELMANN: Well, the wind input is in the wave model as well.

PHILLIPS: You have the Miles mechanism, which is building it up too?

HASSELMANN: Well, it is a hypothesis we have not tested, but we have the feeling that this high-frequency, low-background energy that is sloshing around in the ocean all the time, after the model has been spun up, may not be high enough to get the wave spectrum built up quickly enough when the wind turns suddenly. You may be right; if we actually look at the Miles mechanism more closely, it may be adequate, but I don't really think so. I should mention—we did not discuss it in this meeting—that we have been finding with one-year statistics of a quasi-operational forecast study that WAM tends to be too slow in building up rapid events in the ocean. We have a number of different hypotheses as to what the cause of this could be. That is one of the hypotheses that we are considering. But we do not really know at this point what the answer will be.

DOBSON: I have listened to David Burridge from ECMWF talking about this same problem with storms, that is, that they are too slow to spin up in the ECMWF model. He had thought that it probably had to do with some feedback between the wave field and the wind field.

HOLTHUIJSEN [added in proof]: Van Vledder of Delft University recently [summer, 1989] did some tests with the personal computer version of WAM with the Phillips mechanism added. He found only marginal effects on the wave growth in turning wind cases. Apparently, the nonlinear interactions provide enough "seeding."

Operational Significance

KJELDSEN: I have seen the NATO portion of LEWEX grow from the first idea in 1984, under the leadership of Susan Bales and Warren Nethercote, as part of the NATO Research Study Groups [RSG-1 and RSG-2]. Their main interest in LEWEX was as an experiment to both improve safety at sea and aid the efficient operation of vessels in high sea states. What you have seen at this symposium is only a small fragment of the work that actually has been done in the area of modeling, predicting, and applying directional wave spectra, that is, one sea trial consisting of five days of data acquisition in relatively low sea states. A statistical approach to use all sensors simultaneously—airborne, shipborne, and *in situ* wave sensors—has been proposed, wherein each sensor is assigned a weight, which is computed after an assessment of errors based on statistical comparisons with a common key sensor. A more complete account of the RSG-1 is available as a NATO publication [RSG-1 report, 1990].

If wave forecasts are to become practical operationally, I see no way to avoid developing a nonlinear algorithm for wave-current interactions. As a portion of LEWEX, directional spectra were measured in a strong current shear between the Labrador Current and the Gulf Stream. A freak wave was also measured in this area, close to a busy ship route [Kjeldsen, 1989]. The effect of meandering on the directional spectra is pronounced [Saeveraas et al., 1988]. The rms crest-front steepness of the individual waves in the time series is well correlated with the moments derived from the wave spectra.

Wave forecasts and hindcasts have already been run, giving rms crest-front steepness as a new wave parameter. From here, the next step to prepare a forecast for plunging breaking waves is easy and already under preparation, based on data assimilation in real time from satellites, with current and wave data combined.

There is a need for improvement of *in situ* measurements. Within a recent Norwegian experiment in the North Sea, some wave buoys capsized in 11-m significant wave heights. In LEWEX some of the same buoys survived, but the measurement scatter among them, even in low sea states, is too high.

The directional pattern of gravity waves obtained recently in high sea states is different from the results obtained in low sea states during LEWEX. RSG-1 and RSG-2 have therefore put much more effort in sea trials that took place before and after LEWEX [see the articles by Nethercote and Kjeldsen in this issue]. During the transit of the *Tydeman* from Europe to Newfoundland just prior to LEWEX, DeLuis [1988] performed a hindcast with two wave models using UKMO [U.K. Meteorological Office] wind fields as input to both models. There was a discrepancy of 40% between these two models in their prediction of significant wave height during a severe gale in the North Atlantic. With access to several independently prepared national wave forecasts, there exists an opportunity to prepare a weighted forecast to be used for large-scale coordinated operations at sea, such as search and rescue. At present, a one-hundred-year design wave is prepared for the offshore industry, using a hindcast database from only one wave model. The use of a weighted hindcast would be a considerable improvement.

The few days of measurements taken during LEWEX do not provide an adequate basis for an assessment of wave models. Longer-term wave statistics based on full-scale measurements are needed to perform a complete scientific validation of wave models. SWADE can be an important milestone in this area. I agree with Susan Bales that we should develop a standard set of ship response transfer functions from the LEWEX data. Also, I would like to emphasize that we are interested in safety at sea, due to the many accidents we have had in Norway. Therefore, we are interested in the reliability of the wave forecast. In such an evaluation, a long-term study would reduce the discrepancies among the various models that were evaluated in LEWEX.

BROWN: As Peter Kjeldsen has said, we clearly need better data in large sea states. The topics of this symposium include measuring, modeling, predicting, and applying. Most of the emphasis so far has been on the measuring, modeling, and predicting, and very little on the application. I would like to request, on behalf of the ship designers, that more consideration be given to the very narrow band of wavelengths required for ship design, normally in the range of 50 to 150 m.

Extreme Waves

DONELAN: Since two ship people have raised points of this sort, I would like to mention something that struck me this morning. Earlier in the week, we talked about various aspects of the physics that seem to be in short supply. During Mr. Buckley's presentation, I was struck by another thing that seems to me a little surprising. I wonder what the theoreticians in particular think about it, that is, the appearance of these walls of water that are called "episodic waves" or "rogue waves." They appear—at least in the records that I have seen reported, and the ship people can correct me if i am wrong—to occur in only one size, the economy size, the really large size. Everything that we know about waves suggests that all of these things should be scaled, and so you should be able to see similar effects—although you would not notice them with the same degree of panic—on a very much smaller scale in a similar sea. Does that strike you as surprising? Does anyone want to comment on that?

BUCKLEY: We have something of a paradox here. First of all, I believe that as far as the mechanics of nonlinear, energy conserving waves is concerned, what Dr. Donelan suggests regarding the scaling of episodic waves is correct. But as far as observation at sea is concerned, I am not sure that such waves will be observed in smaller-scale seas. The reason for this is that I suspect the two types of episodic wave packets [i.e., "three sisters" and rogue waves] are nonlinear evolutions of the steep, long-crested wave [see Fig. 2]. Both the ship masters and Coast Guard officers whom I have interviewed indicated that this "parent" wave—most common of the episodic types—would be encountered only if a storm with central winds of at least 25 to 30 m/s was in the vicinity [ship masters' comments] or if waves at least 6 m high in a storm were being encountered [Coast Guard officers' comments]. If my conjecture is correct, these wave types will not be seen until the parent waves have been generated.

Given a seaway that is almost invariably short-crested, how do we end up with a single, huge, long-crested wave?

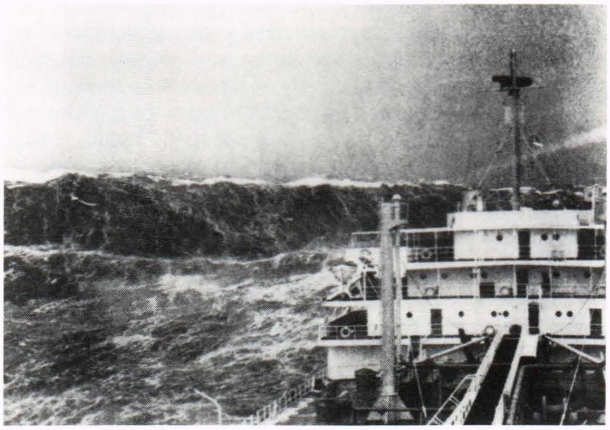

Figure 2. Example of an unusually large long-crested wave. (Reprinted with permission of the American Bureau of Shipping, *Surveyor*, May 1968, p. 23.)

The wave crest is perfectly straight. How does it grow from a group of short-crested waves to a huge, long-crested wave just breaking on the top? There must be a mechanism for that wave to acquire energy; otherwise, it would not grow laterally. There is apparently also a mechanism, and it is obvious in the photo [Fig. 2], for dissipating energy. Otherwise, a large, short-crested wave would result. Visual observations of these waves also suggest that they may be nondispersive, at least within an observer's field of view. The long-crested uniform height of the wave implies that it evolved over a fairly long time, not briefly as in the case of a typical short-crested wave. The governing equations must account for simultaneous acquisition and dissipation of energy, which is different from the usual modeling of conservative gravity waves.

Also, in some of the radar wave images from satellites, the waves are moderately long-crested, but every now and then some are inclined to the general wave direction at fairly sizable angles, perhaps 15° or 20°. Why?

HASSELMANN: If you watch from a plane flying over the ocean, you also see waves going at a different direction from what you expect. These can normally be explained away, by a theoretician at least, as being just random Gaussian fields that you would expect occasionally. But this freak wave that you described—have these waves really been recorded quantitatively so that you can get theoreticians upset, or are they just discussed in narratives?

BUCKLEY: There are several different types of storm-driven waves. So-called episodic waves are those that visually stand apart from the others in the sea. They are very clear, so that observers have absolutely no trouble telling you about them. You suggest they are part of a "random sea," but believe me, these waves stand apart. The type shown in the photo is the most common, as far as I know. Coast Guard officers characterized them as occurring every seventh or ninth large wave in a severe storm.

The other type are the so-called three sisters waves, a group of three waves that intervene in the seaway. Two Coast Guard officers told me you can see these waves coming at an angle of about 30° from the dominant wave direction, with a distinct intersection between this group of three and the other large waves in the sea. Waves of a similar character have been observed to evolve from steep, long-crested, regular waves as the result of nonlinear instabilities [see Fig. 19 in Su et al., 1982]. The intersection was described as "walking toward you." These waves coming in at an angle are also of an appreciably longer period than the others. Ship radars have tracked these wave groups approaching the observers.

PHILLIPS: There is a lot to learn about waves. It is not impossible that there are a few things of this kind still to be learned. After all, it was only twenty years ago that we first realized that a train of finite-amplitude waves was unstable. The Benjamin–Feir instability was discovered fairly recently. And there has been a lot of numerical work on the instability of periodic waves. I would not be a bit surprised if there is not some sort of "instability phenomenon," or maybe you can imagine something on a storm-size scale analogous to the wavemaker developed by Ken Melville [MIT] that changes its frequency. There may be some combination of winds that produces high-frequency waves, and then low-frequency waves that converge at one point to give you a couple of great big waves. The fact that it is long-crested suggests that it comes from a distance. It is not a random local superposition or anything like that. If it is a real phenomenon, it is probably the result of something fairly distant that somehow accumulated in this particular area.

Implicitly, I believe in all of these things. I think they are very challenging to try to understand. We clearly do not understand them now.

HASSELMANN: Owen's description sounds highly speculative. Of course, we do not know what it is, so we just speculate. Let me speculate more conservatively. Maybe these freak waves do not presently come out of our models. But it is quite possible that if you take the small-scale gustiness of the wind into account—instead of having just the normal homogeneous Gaussian fields with a certain, maybe not very large, probability of something drastic happening on a smaller scale—you can get a modulation of that Gaussian field. You suddenly get a large local rms expectation value. Then maybe you could do something in the way of producing freak waves just by chance superposition. But that is just pure speculation.

In the present models, what Bill Pierson was referring to, and I think it is quite true, is that we have not really calibrated or tested the models with respect to the dissipation of swell over long distances. The reason we have not done that is that we do not have good data at this point. And, of course, Bill was also complaining about our dispersion of swell, which Liana Zambresky [to be published] showed in WAM, and we also saw in the NASA model of Dean Duffy [to be published], which does excessively spread the wave energies. On the other hand, I refrained from saying anything about your previous technique, Bill, because you were doing the "water sprinkler" technique, which we know is also not good. So what you really need is a model which has a linear dispersion as the waves propagate, and none of the present numerical schemes do that. On the other hand, looking at the errors that we have, we do not think this dispersion problem is a major one at this point. Otherwise, we would all be much more upset. It is very easy to quantify and understand. If you want to improve it, you just go to a higher-order scheme, if you think it is worth the effort. So I do not think it is a big problem to do that. But just to go back to what we used to use, the sort of pure Lagrangian propagation, with a little bit of jumping around from one grid point to another, does not have the right characteristics for a spreading, finite-bandwidth wave packet.

PIERSON [added in proof]: The water sprinkler technique for GSOWM did not originate with me. The method used in the SOWM can be easily applied to spherical coordinates. Waves do not diffuse, they disperse.

HOLTHUIJSEN: Van Vledder [1983] looked at the statistics of wave groups, and he *did* find that roughly every sixth or seventh *is* the highest wave. So there *is* observational evidence that every sixth or seventh wave is the highest.

PIERSON [added in proof]: Extreme waves are difficult to understand, but they have been modeled. Cummins [1962], Smith and Cummins [1964], and Davis and Zarnick [1964] created extremely high transient wave forms for the study of ship motions. Unfortunately, the analysis tools and theoretical concepts at that time were inadequate. These transient waves are very nonlinear, and these techniques do not appear to have been pursued by naval architects. Presently, two laboratories in Canada and one in the United States have produced extremely high breaking waves for various purposes, but most of their results are not yet available in the literature.

Closing Remarks

PERRIE: Do you think I can hope that all these LEWEX observations will be understood, so that if I change the WAM model or introduce a new dissipation function, I can go back to this data set and check it with the buoy data and all the observations and be able to understand whether I have made an improvement or not?

What about the Geosat winds? I am very naive about how those are derived. Will they improve the wind field? What is the next step beyond this present comparison?

BEAL [added in proof]: The next step will be to produce a permanent record of the LEWEX intercomparisons, including accurate documentation of the measured and modeled spectra. But I really doubt that the LEWEX observations will ever allow one to choose unambiguously which model is superior. As Peter Kjeldsen has commented, a much longer database is required. Geosat passes during LEWEX are sparse, but should at least illustrate the spatial structure of the wind field errors.

DONELAN: That opens an opportunity for me to raise a question regarding the role of future remote sensing systems. How can the planned SIR-C SAR flight be coupled with the European ERS-1 scatterometer to improve our understanding of winds and waves over global scales?

JANSSEN: One could use the SIR-C SAR spectra in a wave assimilation scheme, supplemented by the winds derived from ERS-1, and show that they improve the wind analysis over the ocean. This improved wind analysis should, in turn, improve the wave field analysis.

DONELAN: This seems to be a good point to call it a day. I believe Bob Beal has some closing remarks. Does anyone on the panel have anything else?

PHILLIPS: I would like to thank Bob and the people who were responsible for the local arrangements. They have done a splendid job for all of us during these last three days.

BEAL: To the panelists and to the audience, I want to express my appreciation for your many insights and candid criticism. Your comments will be part of the record, and will certainly influence the way we handle the data and the way that we look at this problem in the years ahead. An important step, of course, will be to produce a written record of the LEWEX results that can be reviewed by the wave community. At the very least, LEWEX has stimulated many new ideas on how to conduct future open ocean experiments, such as SWADE, the ERS-1 validation and application efforts, and the SIR-C/ERS-1 wave intercomparison work. Perhaps the most valuable contribution of LEWEX will have been to serve as a unifying force to bring together those who predict and measure ocean waves with those who must live and operate in them.

On a personal level, I must say that much of the excitement of LEWEX has been its international aspect and the close relationships with colleagues that have developed and will surely endure well beyond this single experiment.

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