1 A.I. Andrianov and A.J. Hermans
'Hydroelastic analysis of floating plate of finite draft'

1.1 Discusser - C.M. Linton

Does your solution of first draft order take into account the fact that the depth of fluid beneath the plate is h-d rather than h?

Reply:

Nonzero draft is taken into account by expanding the Green's function and velocity potential as the Taylor expansion series with respect to draft order. Correspondingly, all terms in our analysis are expanded with respect to draft d (however, we have no terms which contain h-d). Terms of zero and first draft order are taken into account. In addition, we have boundary condition at z=-d. We consider the situation when the draft d is much smaller then the water depth h; numerical results are presented for this situation.

2 A. Babarit, G. Duclos, A.H. Clement and J.C. Gilloteaux 'Latching control of a power take o oscillator carried by a wave activated body'

2.1 Discusser - D.V. Evans

Doesn't the latching requirement introduce a non-linearity into the differential equations? Have you correctly modeled this in your analysis?

Reply:

There is not any non-linearity into the differential equations of the motions. Non linearities come from the fact that we switch between latched equations and unlatched equations time to time. The result on the output is that it is a succession of transient motions. As we have considered the equations of the motion in the time domain, particularly for the radiation problem the Cummin's decomposition, we are able to deal with transient motions, so modelisation is correct.

2.2 Discusser - R. Rainey

Your latest results (shown at the workshop - not those in the paper) give capture widths close to the $\lambda/2\pi$ theoretical limit, over a wide range of frequencies (0.75-1.0 rad/s). And the buoy weight is only 300 tonnes. This looks promising - can you give the figure for the annual average power (before conversions losses) in the wave climate defined by data point "barra" in the WERATLAS data?

Reply:

I have not the figure for this data point, but for a point located near Yen-island on the west coast of France, the annual average power is about 140kW with latching control at a location where annual wave power flux is 23kW/m. Capture width is so equal to about 6 meters. However, I would insist on the disadvantages of the presented device, as

- it is tight moored
- it has end stags
- it is restrained to move in heave only

in comparison with Pelanis or SEAREV.

2.3 Discusser - G. Klopman

Since there is non-linearity involved in the latching mechanism, it might be that the effectiveness of the latching control depends on the (significant) wave height. Is this the case?

Reply:

As latching is applied when the velocity vanishes, it doesn't depend on the amplitude of the wave height. If x is the controlled motion for an incident wave $\eta(t) = a\sin\omega t$, for a wave $\eta'(t) = 2a\sin\omega t$, the controlled motion will be $2x$.

3 A. Ballast and P.J. Zandbergen
'Fully Non-linear Diffraction Calculations of a Floating Sphere in Regular Waves'

3.1 Discusser - J. Grue

We have tested out various numerical beaches, including a variant of the beach you are using, your equation (2), finding that that beach leads to severe reflection of energy, and significant pollution of the wave-field, for wave with finite, large amplitude. With reference to Clamond, Fructus, Grue, Kristiansen (2005), J. Comp. Phys., we have introduced a damping pressure on the following form: $p = \nabla^{-1}(\gamma \nabla \phi)$, ∇ the del-operator, $\gamma(x, y)$ the function determining the beach, and ϕ the potential at the free surface. This damping absorbs any steep wave, as found by computations. A volume of fluid may also leave the computational domain without causing reflection.

Reply:

Thank you for the comment. We will certainly have a look at it.

3.2 Discusser - H. Bingham

Some time ago when I was working with a similar model we found that using a circular (or ovular) outer boundary to the domain actually sends all reflections directly back to the the body. You may find that a rectangular boundary works better.

Reply:

Thank you for the comment. A circular domain is more or less necessary given the restrictions on our grid. Given that shape, a circular beach seems to be the most efficient use of available space. However, given your comment, we should check whether a rectangular shape of the beach (making less efficient use of the available space) is not indeed the better choice.

5 G.J.D. Chapman and R. Porter
'Scattering of obliquely incident waves by submerged ridges'

5.1 Discusser - M. Meylan

Can this theory be extended to surface piercing bodies. What about floating bodies?

Reply:

Yes, it can be applied to any arbitrary surface bottom mounted, submerged or surface piercing. We believe that the Rayleigh Ritz method presented offers significant potential advantages as the form of the Green's functions enables many of the terms to be eliminated. For the case of a surface piercing body we believe this solution method could simplify the boundary value problem significantly.

6 M.-Y. Chen and C.C. Mei
'Nonlinear harbor oscillations excited by random incident waves'

6.1 Discusser - J. Grue

Your method may be useful to estimate how the energy on the low-harmonic modes varies with respect to the shape of the harbor and the protection. In some exotic cases, e.g. when considering wave-energy extraction, one may want to tune the geometry of the harbor in such a way that the response at low and/or high frequency is large. In your case you may want to adjust the geometry such that the response at low frequencies becomes small. Please comment on possible adjustment of the geometry for reduction of the low-harmonic response.

Reply:

Indeed by numerical experiments, our method can be used to design a harbor so as to to give a preferred low-frequency spectral peak. A crude first estimate is to use the linearized theory first to decide the harbor geometry, so as to get the desired peak corresponding to Helmholtz mode for example. The spectral shape can then be computed by the 2nd order theory. To choose the harbor geometry in order to get a prescribed spectral shape is a stochastic inverse problem. Laborious numerical experiments may be necessary before an inverse theory is found. A practical way to reduce the low frequency peak is to induce flow separation at the tips of breakwater.

7 H. Chung and C. Fox
'Transition conditions at the interface between floating plates'

7.1 Discusser - A. Korobkin

In nature it is happening that one edge of ice floe is sitting onto another ice floe. Is it possible to model this type of connection with springs, as you are doing?

Reply:

As long as the edge conditions are given in the context of linear elastic plate model, the problem can be solved by the Wiener-Hopf technique. In this case, the displacement is continuous at the transition. But bending moment of the two plates have some relationship. Shear force may be the free or linked for the plates, depending on the details of the transition.

14 R. Eatock Taylor
'Wave-maker ramp functions in numerical tanks'

14.1 Discusser - H. Schäffer

Even for regular-wave experiments, the initial transients tend to be associated with the generation of long waves. As even dissipative beaches or other passive absorbers almost fully reflect these long waves, they tend to disturb (regular-)wave tests for a long time, especially for flumes with wavemakers not equipped with an active wave absorption control system.
Did you study the effect of the duration of the ramp (e.g. by varying the coefficient γ in your ramp1) on this inevitable long-wave generation?
As the long-wave generation is presumably related to the gradient of the wave envelope through the momentum balance this would probably require the use of a nonlinear model (second order would suffice).

Reply:

I have made some limited comparisons of the effect of γ in ramp1, but the conclusions from these are no different from what one might intuitively expect: Smaller γ leads to smaller long wave components. When the wavemaker is turned off, these presumably would die down more quickly than waves generated without a ramp.
A second order estimate of the long wave behavior at the wave front was given by Molin (Ship Technology Research 2001). Our comparison with the fully non-linear boundary element analysis in the time domain shows good agreement with the simple linear theory, including for the long waves near the front. These results were for the wavemaker with ramp3, in which the intensity of the long waves and associated second order effects seem to be small. It would be possible to estimate these more accurately using the wave envelope, obtained from the FFT analysis discussed in the presentation.

15 D.V. Evans and M.H. Meylan

'Scattering of flexural waves by a pinned thin elastic sheet floating on water'

15.1 Discusser - A. Korobkin

I guess that not one condition ($u = 0$) at the pinned point, but two conditions ($u = 0$ and $|\nabla u| = 0$) must be mentioned in the problem formulation. How we can formulate the conditions at the point, which is not pinned but simply supported? Can you solve the problem with the plate pinned along a curve?

Reply:

For a pinned curve I agree two conditions are required ($u = \partial u/\partial n = 0$) but I am not sure I need the second condition, or indeed how I would satisfy it, in the pinned points problem I am considering here. Although $\partial g/\partial r = 0$ at $r = 0$ the gradient of the total displacement is not zero once the incident displacement is included.

17 T. Gazzola, A. Korobkin, S. Malenica and Y.-M. Scolan 'Three-dimensional Wagner problem using variational inequalities'

17.1 Discusser - U.P. Bulgarelli

Do you can proof that the bilinear form you use $a(u, v)$ at left hand side of your inequality is coercitive in half space $z \leq c$ without any extra condition on $z = 0$? Thank you.

Reply:

There is no additional condition on the surface $z = 0$. However, we employ the condition in the far-field with the aim to make the bilinear form $a(u, v)$ coercitive (see eq. (8) in the abstract).

18 J.M.R. Graham, S.J. Sherwin, T.E. Kendon and M.J. Downie 'The Prediction of Viscous Damping of Large Floating Bodies in Waves'

18.1 Discusser - S. Malenica

Could you comment on CPU-time? How many sections along the ship do we need?

Reply:

Typical time for 5 cycles of oscillation, using 3 viscous sections along the hull, is 8 hours on a 300MHz single processor PC. Nearly all of this time is for the viscous computations. The 3-dimensional potential flow computation (B.I.E.M.) takes only a very small proportion of the time.

18.2 Discusser - R.W. Yeung

1. In your discussion of the treatment of the viscous core solution, you suggested to supply the inviscid-fluid solution as input to the viscous flow and there is no feedback of the viscous-fluid solution to the inviscid flow. Our experience with the FSRVM (Ref 1.) method suggested that the information has to go both ways, since the separated flow will affect the background inviscid flow. This is automatically embedded in the FSRVM formulation; but in your scheme, it would be difficult to justify since the potential, hence the future position of the body, can not be determined ω/σ the viscous-fluid feedback.

2. In the presented comparison of damping with our experiments and FSRVM predictions (Yeung & Roddier, 1998), it would be nice to extend it to higher frequency. Further, in the same work, we indicated that the added moment of inertia is altered by viscosity. Did you observe similar behavior?

3. Can you explain what treatment is made to the vortices in your modeling when they approach the "rigid surface"?

Reply:

1. In our viscid-inviscid treatment of the problem of a freely floating body the potential flow is first calculated. This flow is used to "drive" the viscous calculation. The viscous flow generates viscous (separated) flow forces on the body which combined with the original potential flow forces are used to compute a corrected body-motion response. This corrected motion combined with the potential flow is then used to recompute the viscous flow field and so on. This iteration is continued until convergence, typically 3 or 4 steps. Hence there is a feedback to the radiation part of the potential flow via the body response. For forced motion there is no feedback and no iteration is necessary. The process neglects wave radiation by the rotational part of the flow field. We believe this component to be very small.

2. We also observe that the viscous force contributes to both the damping and the inertia. The sizes of the contributions are similar but the viscous force is relatively a larger proportion of the damping than of the inertia since the potential inertia is large.

3. Vortices which cross the mean free surface are removed. However because a rigid lid approximation is used, few vortices do.

20 J. Grue
'A nonlinear model for surface waves interacting with a surface-piercing cylinder

20.1 Discusser - A. Korobkin

Could you please explain how do you use Fourier transform in the plane with a hole.

Reply:

I put the function inside the hole to zero. The main point is to invert the equations by using Fourier transform.

20.2 Discusser - H. Bingham

When you insert a non-rectangular body into your rectangular Fourier-grid, it seems that you will get very large gradients in the solution near the boundaries. How do you propose to deal with this?

Reply:

Practical algorithms for computations have to be developed. We know that Fourier transform (Fourier series) has strong convergence properties. In practice this is a question of resolution.

25 A. Jensen, S. Mayer and G.K. Pedersen 'Dynamics of a collapsing breaking wave'

25.1 Discusser - A. Korobkin

Thank you for very nice and important presentation. Your measurements with PIV and calculations with NSE are very clear and provide data about velocity field and breaking wave profile. In the case of collapsing breaking waves, it would be interesting to place a vertical plate in front of the breaking place and follow the wave impact onto this plate. This will be so-called Bagnold-type of the impact with air-cavity trapped. The impact is usually of short duration, this is why it is not easy to resolve the impact details. My suggestion is to combine your experimental data for the breaking wave impact onto the vertical wall in the surf zone and numerical data by NSE with the semi analytical impact theory (rather simplified due to short duration of the impact) and to obtain fine resolution of the impact pressures.

Reply:

No reply

29 O. Kimmoun, B. Molin and E. Fontaine 'Experimental study of the wave response of a two-dimensional rectangular barge in very shallow water'

29.1 Discusser - A. Korobkin

1. This problem is very suitable for asymptotic modeling with, say, lubrication approximation beneath the barge and inviscid model outside the barge. The lubrication approximation can be used if Reynolds number multiplied by the aspect ratio of the gap is small. When it is not so, the inertia terms must be taken into account in the gap flow equations. Viscous model beneath the barge should be matched with the inviscid model outside the barge with the help, say, conservations conditions. It is expected that such a combined asymptotic model may provide rather good results.

2. Did you perform experiments with the same barge but rounded corners?

Reply:

2. No.

29.2 Discusser - G. Zilman

In your analysis the difference between experimental and theoretical results is attributed to the viscous effects, which depend on Re number. For the full scale the effect of viscosity may be quite different. What is your opinion about that? May it happen that for the full scale the agreement between reality and theory will be better?

Reply:

When we have compared our experimental results with a linear potential flow model, we wanted to know the limitation of this linear model in term of shallow water problem. We found the way to adjust the maximum response at the resonance period was to adjust the viscous effect through the drag coefficient. But the physical process seems more complex. And we have know to determine in these experimental results the fact of viscous damping and the fact of hydrodynamic cushioning effect (only this fact can be transpose to reality).

30 G. Klopman, M.W. Dingemans and B. van Groesen 'A variational model for fully non-linear water waves of Boussinesq type'

30.1 Discusser - H. Bingham

The expression or the Hamiltonian (2.3) is positive definite, depending on $\left(\frac{\partial\phi}{\partial x}\right)^2$, $\left(\frac{\partial\phi}{\partial z}\right)^2$ and ζ^2, regardless of how you approximate $\phi_x(z)$ and $\phi_z(z)$; so why do you claim that other Boussinesq methods result in a non positive definite H?

Reply:

The Hamiltonian density in Eq. (2.3) is the one associated with the exact non-linear irrational water wave problem, as derived by Zakharov (1968). However, for practical applications some approximations are necessary. In our method, these approximations are such that the positive-definiteness of H is conserved.
On the other hand, for instance, a simple derivations of the approximate Hamiltonian from which a set of Boussinesq-like equations follow, yields an associated Hamiltonian density $H = \frac{1}{2}\rho\left[(h+\zeta)\phi_x^2 + g\zeta^2 - \frac{1}{3}h^3\phi_{xx}^2\right]$. This may become negative for short waves, for which the approximation was not to be valid anyhow, but may lead to numerical instabilities.

Not all Boussinesq-like equations are derivable from an Hamiltonian approach, or any other variational approach. See further the discussion in Dingemans (1997, pp. 583-590).

31 A. Korobkin and S. Malenica
'Modified Logvinovich model for hydrodynamic loads on asymmetric contours entering water'

31.1 Discusser - P.D. Sclavounos

Is it possible to extend this theory to account for a known gradually varying in space profile of the water particles on the free surface with their acceleration being significant?

Reply:

Yes, this is possible. Velocities and accelerations of both the body and the free surface can be functions of time. Moreover, the body velocity can be evaluated in free-fall motion, once this velocity is not prescribed.

32 E.J. Kreuzer, W.M. Sichermann 'Slender Body Theory Approach to Nonlinear Ship Motions'

32.1 Discusser - C.C. Mei

Your theory succeeds in accounting for nonlinearity fully in a 2-D slice in the slender-body approximation. The slender-body approximation is heuristic, ... (indecipherable) and pragmatic. Have you thought of how to improve it when motion amplitude is large? Surely, when motion is very nonlinear, the assumption that neighboring cross sections interact weakly (basis of slender-body approximation) would fail!

Reply:

When the motion and wave amplitudes are small, the slender body theory can be developed by a strict order of magnitude analysis. When these restrictions are released, the slender-body assumption (namely that the longitudinal component of the surface normal is of the order of the ship slenderness) is still valid for large sway, heave, and roll motions. Pitch and especially yaw motions lead to an increase of the normal component perpendicular to the control sections. While the pitch angles remain moderate even in steep waves, large yaw motions represent a violation of the slender-body approximation. The effect could be reduced, when the control sections in front of the ship are adjusted to the actual forward direction.

32.2 Discusser - E.O. Tuck

In this paper and that of J. Xia, nonlinear effects are found to be significant in response of ships to sinusoidal plane waves. But as soon as nonlinearity is permitted, superposition is not permitted. Hence the results for sinusoidal waves cannot be used in real situations. How do you deal with the response to a real sea?

Reply:

On one hand, the investigations clearly reveal situations where the application of linear methods no longer leads to reliable results. On the other, the simulations of nonlinear ships motions in irregular waves are limited to a few minutes of duration due to large CPU time requirements, such that simulation-based nonlinear response estimates become unfeasible. However, a possibility to assess the maximum ship loads and motions is the use of transient design wave sequences. These wave sequences are constructed such that they produce within a few minutes maximum responses which are equal to corresponding long-term maximum responses.

32.3 Discusser - R.W. Yeung

In such a method of obtaining solutions, it appears that for Eqn. (6) and (9) the domain for solving for ϕ at the new time step is different from the domain or region where where ϕ_w and η_w (incident wave field) is defined. The information of ϕ_w therefore cannot be "transferred" to the new domain in a straight forward way without some degrees of extrapolation. I wonder if you can comment on how to circumvent this difficulty. I also note that in terms of consistency, it is difficult to justify the nonlinear terms of the incident waves and keep the nonlinear terms of the body-generated waves (or potential).

Reply:

The idea of solving for the difference potential ϕ only has been employed by various authors, especially for diffraction calculations in steep waves. From a mathematical point of view, the total potential Φ may be decomposed into arbitrary components as long as every component satisfies Laplace equation independently. This holds also for the potential of the incoming wave ϕ_w. When ϕ_w is described by an analytical solution (e.g. see Rienecker and Fenton, JFM 1981), then ϕ_w

is also available (although non-physical) above the surface of the incident wave. Another point related to the $2D+t$ approximation is that ϕ_w does not satisfy necessarily conservation of mass in the two-dimensional cross-section in which ϕ is defined. This shortcoming, however has to be considered from the point of view of the approximation.

32.4 Discusser - J. Greenberg

Did the author use computational data for Φ on the sections at x and $x + \Delta x$ to estimate a-posteriori Φ_x on the boundary. I am curious if this term was small relative to the terms retained in the pressure computation on the body.

Reply:

We expect the x-derivative of the total potential Φ to be of leading order in head or following seas due to the presence of the incident wave. However, the derivative ϕ_x of the body-generated component should be insignificant according to the slender body theory, such that ϕ can be approximated by a solution of the two-dimensional Laplace equation. An a-posteriori estimation has not been performed at present state, but it is considered by the authors to be a suitable indicator for the validity of the slender body approximation.

33 N. Kuznetsov
'The two-dimensional water-wave problem for multiple finite docks'

33.1 Discusser - C. Linton

You have considered the case of infinite depth. Is it possible to treat the case of constant finite depth in a similar manner?

Reply:

In the case of constant finite depth, there exists a conformal mapping that rotates the internal portion of the free surface in a similar manner. However, it is necessary to check whether the derivatives of the variable coefficients arising in the finite depth case have the right sign.

38 P.L.-F. Liu, T.-R. Wu and W. Mo
'A three-dimensional numerical model for wave-structure interactions'

38.1 Discusser - J. Greenberg

Have you tried any other interface tracking method. Like level set methods?

Reply:

No

41 P. McIver

'Are there trapped modes in the water-wave problem for a freely-floating structure?'

41.1 Discusser - D.V. Evans

You have demonstrated they exist by constructing a double body solution. Do you think it is possible to find a single-body solution?

Reply:

To satisfy the resonance condition it is required to find a wave-free potential ϕ_0 that has no dipole component in the far field. On the other hand, it seems that to obtain a suitable streamline that may be interpreted as the surface of a structure the near field is required to have a dipole component. We have unable to construct such a ϕ_0 without using separated singularities that naturally lead to a double-body solution.

41.2 Discusser - J.N. Newman

Is there only one profile, as implied by your slide, or a continuous family of profiles, which satisfy both conditions?

Reply:

The wave-free potential used in the construction has the form $\phi_0 = x(\phi_s + \phi_d)$ where ϕ_s is a pair of wave sources at $x = \pm\xi$ and ϕ_d is a pair of wave dipoles at $x = \pm\xi$. In the presentation a solution was shown for a single value of x. If x is continuously varied then a continuous family of structures is produced.

42 C.C. Mei and M. Chen
'Second-order Diffraction and Refraction of Water Waves'

42.1 Discusser - X.B. Chen

I'd like to have a discussion with you on the issue of inconsistence raised by Professor Eatock Taylor in one previous workshop: the limit of the 2nd order incoming waves (in finite water depth) associated with two fundamental wave frequencies is not that of a regular wave when the two frequencies tend to each other. A solution to this mathematical inconsistence is proposed in my notes attached to this form.

Reply:

I am not aware of any inconsistencies in the existing 2nd order progressive waves. Your resolution of Prof. Eatock Taylor's question is best discussed with him. Thanks for sharing it with me. I shall look at it.

42.2 Discusser - J. Grue

Please comment on potential changes in the computations and the wave field if you carry on to cubic order in wave slope. Will nonlinear dispersion play a role in the refracted wave field, which has a changing amplitude and wave slope? It has been quite a number of studies on third-harmonic wave refraction. The effect of changing fundamental wavenumber has not been much discussed, and you may actually estimate this effect from your formulas. I do appreciate that your that your presentation was given for second order waves!

Reply:

At the second order the incident and reflected waves without the scatterer (cylinder, harbor or shoal) are the second order parts of Stokes wave. At the third order, the corresponding terms must correspond to the third order part of Stoke waves, hence would contain the amplitude dependence in the nonlinear dispersion relation. This relation must aspect the scattered waves at the third order. In any case our scheme can be continued to give the solution that satisfies the weak radiation conditions.

43 B. Molin, E. Jamois, C.H. Lee and J.N. Newman 'Non-linear wave interaction with a square cylinder'

43.1 Discusser - J. Grue

Can you please comment on tests with geometries that do not have sharp corners, meaning that there are no physical singularities present in the flow field?

Reply:

In its present form, the numerical code uses finite differences with a rectangular grid. Smooth geometries must be modeled in a step-wise manner inducing singularities.

43.2 Discusser - J. N. Newman

Is it useful to use this approach for an outer solution, matched to an inner domain with an unstructured grid to treat more general bodies?

Reply:

It is our plan to couple this method, in an outer domain, with a more flexible one, in an inner domain. Our ultimate goal is to model the wave response of a LNG-carrier moored to a GBS structure, in the open-sea.

44 O. Motygin

'Trapped modes for surface-piercing cylinders below and above the cut-off frequency'

44.1 Discusser - M. McIver

Do you know if there are any trapped modes for multiple submerged bodies below the cut-off?

Reply:

As far as I know, the existence of trapped modes is stated in papers by Jones and Ursell for a single totally submerged body.

44.2 Discusser - D.V. Evans

It is true that a trapped mode (or edge wave) below the cut-off for a single surface piercing body implies the John condition does not hold. I.e. does such a body always support a trapped mode?

Reply:

In the problem in question any body, which supports trapped modes below the cut-off, does not satisfy John's condition. I believe the reverse assertion is not generally true, i.e. there exist geometries not satisfying John condition and not supporting trapped modes. Existence of such geometries for a particular case of the problem under consideration ($k = 0$) follows from results by N.Kuznetsov (2003, published in Comptes Rendus).

47 F.G. Nielsen
'Some Hydrodynamic issues related to offshore wind turbines'

47.1 Discusser - M. McIver

Are interactions between turbines important?

Reply:

Yes, interactions is important. Downstream distance between turbines are normally in the range 5-10 diameters. The wake of the turbine has reduced velocity and increased turbulence. The increased turbulence helps mixing in "fresh air" in the wake, but also cause increased dynamic loading on the downstream turbine.

47.2 Discusser - J. Grue

Bad weather conditions are known to cause problems for wave power converters, and may even damage them. Can you comment on how wind turbines will perform an survive in bad weather?

Reply:

As the wind velocity increases, the turbine thrust increases until rated power is reached (typically at 12-14 m/s). As the wind velocity increases further the wind thrust is reduced by pitching the rotor blades. That means the wind forces are not extreme at extreme wind velocities. The wave forces in extreme conditions may be handled by design procedures developed for offshore structures. The greatest challenge is to obtain such a design within acceptable costs.

49 G. Oger, P. Ferrant, B. Alessandrini
'Free Surface Impact in a Biphasic SPH Simulation'

49.1 Discusser - A. Korobkin

1. You compared the numerical results with the experimental data. For the vertical acceleration of the wedge, the initial stage of the curve is not in well agreement. Could you please give an explanation for this disagreement?

2. In the figure for the second derivative in time of the wedge trim angle, high-frequency oscillations of the experimental results are strongly pronouneed. Do you know a reason for these oscillations?

Reply:

1. This disagreement leads to a still opened question: Does the smooth evolution of the experimental vertical acceleration time history at the impact beginning instant is due to air cushion effect? Our first monophasic SPH simulation gives a very stiff evolution around this instant, whereas our biphasic simulation seems to match better with the experimental data at this critical instant, showing a smoother (more progressive) evolution. Unfortunately, we do not succeed in matching exactly the experimental result at this initial stage, leading to a difficulty, for us, to conclude efficiently.

2. These oscillations are clearly due to structural vibrations of this wedge made of aluminum. The water impact generates a strong deceleration of this wedge, with a self-righting of this solid, coming with these vibrations. The piezo-resistive accelerometers captured all of this signals. Some more details are provided by the authors of these experiments themselves; [6].

52 O. F. Rognebakke and O. M. Faltinsen
'Sloshing induced impact with air cavity in rectangular tank with a high filling ratio'

52.1 Discusser - K. Takagi

I am expecting that, if you keep shaking the tank, small bubbles increases in the water. As a result, the sound speed of the water decreases. Thus, the phenomena can not reach the steady state. Do you a comment on it?

Reply

During the experiments, the number of bubbles floating in the fluid increases as you point out. This means that the average sound speed in the fluid decreases. Regardless, a 'global' steady state is reached for regular oscillatory tank motion with impacting sloshing flow. The free surface motion seems to be the same between oscillations. However, pressure measurements in the impact zone show no steady state. The reason for this is not the change in sound velocity in the fluid; the model-scale pressures are below the acoustic pressure level. What matters are small changes between impacts in the very local free surface profile, as well as compressibility effects of air in bubbles in the impact zone. The air cavity oscillations studied in this work exhibit a behavior similar to a damped free vibration problem. The initial conditions are determined by the sloshing induced impact.

52.2 Discusser - S. Malenica

How do you extrapolate the model scale impact pressures to full scale?

Do you intend to take into account the flexibility of the containment system, which, at full scale, is relatively soft and may influence the pressures/loads significantly?

Reply

Your questions touch upon the major difficulties and challenges in performing model tests to find full-scale sloshing induced impact loads. Froude scaling is not applicable when compressibility effects of entrapped air matter. The Euler number is then important. Hydroelastic effects due to flexibility of the container walls may influence the load, if the loading duration is in vicinity of the natural period of relevant structural modes. At the very least, the structural eigenperiod should be properly scaled. We are currently performing experimental and theoretical work on hydroelastic sloshing.

55 H. Schäffer
'On the Dirichlet-Neuman operator for nonlinear water waves'

55.1 Discusser - J. Grue

It is known that the method by Craig and Sulem is rather limited in terms of the convergence radius. This means that it may be good when the waves are very small, but poor when the wave slope is moderate to steep. We have found that this may lead to severe instability problems in time-stepping simulations. A discussion on this point is given in Fructus, Clamond, Grue and Kristiansen (2005), J. Comp. Phys.

Reply

The perturbation methods clearly have their limitations and I look forward to study your approach. Even so, Bateman Swan and Taylor, using a method very similar to that of Craig and Sulem, have succeeded in modeling focusing waves in deep water, which are highly nonlinear in short spans of time and space. Personally, I have used the perturbation method for nonlinear shoaling waves up to the point of depth-induced wave breaking without encountering instability.

55.2 Discusser - D. LeTouze

Comment: It is very nice to have this comparison finally formally written!
Question: In these methods, an important issue is the dealiasing of the expression, which has to be done at each step of the recursive process. Do you have an idea of how the various are achieving this dealiasing?

Reply

This comparison of seemingly different perturbation methods is based on a continuous representation, where aliasing is not an issue. However, practical implementations are discrete, and if the physical phenomena studied are not sufficiently well resolved, dealiasing (or a less stringent smoothing procedure) is typically needed to avoid numerical instability. However, among the references, some seem to consistently apply alias-free products, while others don't.

57 I.V. Sturova
'Waveguide properties of the elongated rectangular structures'

57.1 Discusser - D.V Evans

You conclude that trapped modes only exist in the floating elastic strip if the draft $d > 0$. But doesn't the shallow-water approximation imply that the term $\frac{\omega^2 d}{g}$ should be neglected compared to 1 in equation (10) of your abstract?

Reply:

Indeed, this term is small in the shallow-water approximation. However the account of this term does not lead to any additional complication of the problem.

58 M. Sueyoshi, Kishev and Kashiwagi 'A particle Method for Impulsive Loads caused by Violent Sloshing'

58.1 Discusser - Y. Kim

The pressure on wall is dependent on the boundary condition as well as the pressure solver. Can you describe how you treated the wall B.C. during impact?

Reply

In the MPS method, the wall boundary condition is treated as the particle number density. It is described by

$$n_i = \sum_{i=1}^{N} w(t_{ij}) + C_i$$

where C_i is the contribution of ghost particles arranged in the region of the wall boundary.

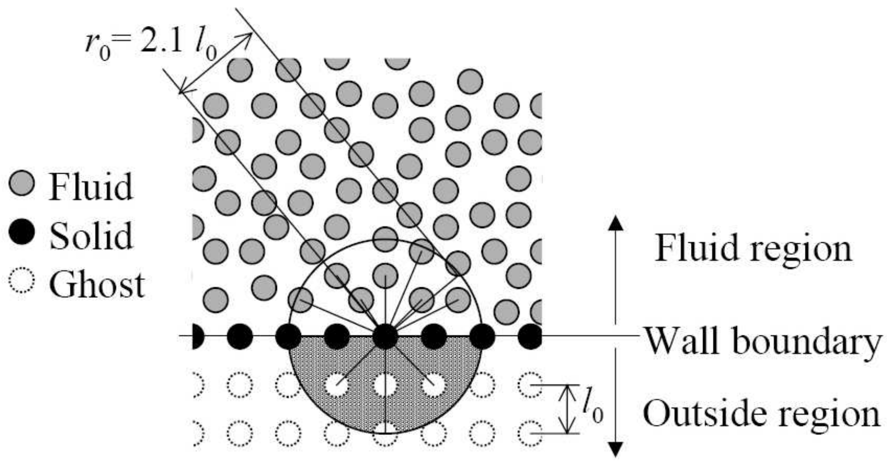

Figure 1: Sketch of particle arrangement around wall boundary.

58.2 Discusser - D. le Touzé

-In your simulations, your free surface patterns are very fragmentated. Do you have an explanation of this feature?
-Can you give a rough figure of the computational costs of your method?
-What advantages and drawbacks of your method do you see, compared to the SPH method?

Reply

-We should improve
1. The calculated oscillatory pressure
2. Accuracy of spatial discrete models is not enough in region where the number of particles is small
3. Surface tension model.

- Table A shows an example of computation time for 2D dam-breaking flow by using the present method. Figure B shows an dependency of computation time on number of particles. Computation time is almost in proportion to N in the range of 400,000 -1,000,000.

-Actually, we have not compared our method with the SPH method. In theory, our method treats incompressible flow without artificial weak compressibility. But we must carry out time consuming iteration process for Poisson equation of pressure.

Table A : Computation time of the present method

Number of Particles	Number of Time steps	Computation Time(sec.)*	Computation Time/step (sec.)
690	669	30	0.045
2280	1523	411	0.270
8160	2217	3879	1.750

* These results are obtained on a usual PC(Intel Pentium4 3.0GHz)

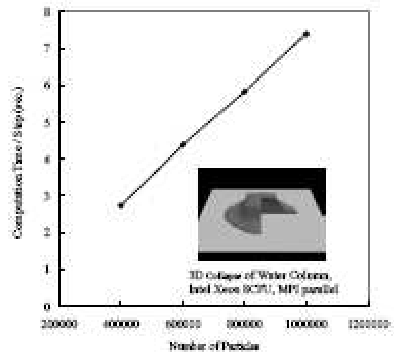

Figure B : Dependency of computation time on N by using parallel MPS code.

59 K. Takagi and J. Noguchi
'PFFT-NASTRAN Coupling for Hydroelastic Problem of VLMOS in Waves'

59.1 Discusser - R. Eatock Taylor

For someone like me who is interested in hydroelasticity, this floating wind power plant is a marvelous problem, which throws up many challenges. I wondered if you had tried to obtain some approximate results, before embarking on the analysis shown here. For example, did you try to use a 2D analysis for the flow past the submerged elements, with some appropriate simple representation of the lifting struts?
I would also like to ask why there is a difficulty in matching the hydrodynamic load distribution to the FE model. Usually one can evaluate consistent loads using the element shape functions (for example using energy or virtual work formulations).

Reply:

We tried the strip method for the first trial, but results are not good.
Since we have no detailed structural data at the conceptual design stage, it is very useful to use the bar-element, which requires only the information of rigidity. Then, we create surface panels for the hydrodynamic analysis. Thus, we cannot use common panels for the structural analysis and the hydrodynamic analysis.

59.2 Discusser - A. Korobkin

It looks like the submerged parts of the structure are rather close to the water surface. Do you expect that nonlinear effects, due to the proximity of the free surface are of importance for the presented design? Do you think that trapped modes can be obtained for such a structure?

Reply:

The depth of the lower-hull is 10m, while the wave height is 6 m. Therefore, we do not expect the nonlinear effect. We should be careful on the trapped modes. However, in practice, the effect of the trapped mode is supposed to be small, because of the submerged lower-hulls and small size of the struts.

60 P. Taylor, D. Walker and R. Rainey 'On the New-Year Wave at Draupner in the central North Sea in 1995'

60.1 Discusser - J. Grue

We have performed several experiments on steep irregular waves in wave tank and measured the kinematics using PIV. The results are published in Grue et al., Appl. Ocean Res. (25), 2003. We have not been able to measure any kind of particle escape. Of course, the very, very steep waves break, but that is a forward breaking, and in deep water this is a very weak breaking. As to modeling it is evidenced through comparison with experiments, that the Nonlinear Schrödinger equation is very useful to model the evolution of a wave field (for finite time) and to predict steep wave events. To model the behavior of the crest of a very steep wave a full model is required, using $Dp/Dt = 0$ as condition at the free surface, up to overturning.

Reply:

The "escape" of particles you refer to are in the photograph I showed, which is now Fig.8 in my forthcoming paper in the Newman Honorary Volume of J. Eng. Maths (2007). It is a common enough sight at sea, but does appear to be difficult to reproduce in the wave flume, perhaps because of surface tension. However, growth in crest elevation after focus is certainly seen in the laboratory, see Fig. 6a in Chaplin's paper in Vol 6 No. 2 of the Int. J. Offshore & Polar Eng. (1996). Spectacular jets, rising near vertically, have also recently been observed in large-scale tank tests, see the paper by Takuji Waseda in Vol 4 of the KANRIN J. Japan. Soc. Naval Architects and Ocean Engineers (2006).

I disagree that the non-linear Schrödinger equation is very useful for predicting steep wave events. In deep water, it predicts none of any statistical significance, as shown in the paper by Dysthe et al. at this workshop, and especially in their subsequent paper in Vol 542 of JFM (2005). In the Draupner depth, it predicts none at all, because the depth is below the cut-off for the Benjamin-Feir instability.

By contrast particle "escapes" are more frequent in the Draupner depth, as I show in my forthcoming paper cited above. Hence my suggestion, made in detail there, that rogue waves are better viewed as a strongly-nonlinear phenomenon, inherently beyond weakly-nonlinear theories like the non-linear Schrödinger equation.

60.2 Discusser - E. O. Tuck

You quote Haver of Statoil as saying that "... big waves are different ...". In view of your analysis, which highlights vertical particle motions of an extreme nature, it would be interesting to know just in what manner Haver believes they are different. Moderately large waves may break in the usual boring way, by spilling, plunging, etc. But really big waves may have such vertical motions as to retain their integrity longer.

Reply:

Haver means that the big waves are theoretically different, and I agree with him, and with you. But it is still a minority view. It is easier to roast a pig in a synagogue, than it is to challenge the non-linear Schrödinger equation.

60.3 Discusser - P.D. Sclavounos

Has there been a statistical analysis of real wave records of the conditional distribution of the wave kinematics, conditional upon the wave elevation beeing larger than a certain large threshold? This statistical analysis may reveal useful information on the physics, rate of occurrence and magnitude of large waves.

Reply:

I believe Ken Melville of UCSD may be doing something along these lines, with his airborne measurements of waves in the Gulf of Tehuantepec. I have seen some of his preliminary results, which suggest that all the highest wave crests are breaking.

62 E.O. Tuck
'Can lateral asymmetry of the hulls reduce catamaran wave resistance?'

62.1 Discusser - R. Rainey

Your conclusion that yawing the hulls is not beneficial, does not hold for aircraft. One of the reasons that the Boeing 707 was superior to the Douglas DC-8, was that its engine pylons were yawed (i.e. not aligned exactly fore-and-aft). It was found during wind tunnel tests that this reduced the drag. I assume by eliminating induced drag caused by non-zero angle of attack.

Reply:

I did not say that it is not beneficial, only that it is less so that choosing a better full separation. Importantly my paper refers only to wave resistance, not induced drag. Indeed, I made the cavalier assumptions that there is no induced drag at all! This is justified in the case of catamarans by demanding that there is zero net circulation about each hull waterline, for all ζ values. If this is not so, the result is wholly unfavorable, i.e. the total wave resistance plus induced drag is bend to be greater.

62.2 Discusser - J. Grue

I understand that production of additional vorticity will increase the resistance (energy loss). Please comment on how to organize the vorticity so that the waves generated by the ship become reduced, and in turn lead to a reduction in wave resistance.

Reply:

Vorticity by itself increases resistance. However, the combination of sources and vorticity can reduce it. That is, the vortices might cancel some of the waves made by the sources. The point of my paper is to show that this effect is small, and in particular that it vanishes (subject to certain approximations) when the hull separation is optimal from the point of view of minimizing (source-only) wave resistance.

63 R. Wemmenhove, G.E. Loots, R. Luppes and A.E.P. Veldman 'Simulation of greenwater loading by a three-dimensional two-phase numerical model'

63.1 Discusser - T. Khabakhpasheva

Did you compare the results of calculations of the pressure for these 2 cases of impact conditions:
1. impact by incompressible fluid
2. impact by incompressible fluid with aerated thin layer.

Reply:

We did not simulate situation (2) yet, since this is a quite unphysical situation for a first wave impact on the wall. For a typical first wave impact, aeration will be significant some time after the peak pressure level at the wall is reached. However, at that time the breaking wave has retreated back already. After subsequent wave impacts though, the comparison between situations (1) and (2) can be useful.

63.2 Discusser - M. Dingemans

I observed that you have water pockets. I wonder whether you observed oscillating behavior of those cylindrical air pockets, just as was observed 30 years ago at the Delft Hydraulics with breaking waves on a slope of a dike. The behavior then found was similar to oscillations of gas bubbles described by Prosperetti.

Reply:

We didn't do a lot of research on those kinds of air pockets, yet we made simulations on rising spherical bubbles. The spherical bubbles show an oscillating behavior, evolving into an elliptical shape (for sufficiently high surface tension) or breaking up. The shape of the rising bubble has been compared with the results of Prosperetti.

63.3 Discusser - G. Colicchio

How do you model the surface tension? Do you use the level-set function to calculate the curvature of the air-water interface?

Reply:

We are using the VOF (local level set) function to model surface tension. The surface tension is determined by calculating the curvature of and the normal to the free surface, depending on the (VOF) fluid fraction F.

68 J. Zang, P.H. Taylor and R. Eatock Taylor 'Non-linear interaction of directionally spread waves with FPSO'

68.1 Discusser - J.N. Newman

Some conventional QTF's are very sensitive to the incidence angle(s), requiring very large M to describe them completely. Thus this is a very attractive alternative to reduce the computational cost. But if your results are sensitive to the spreading parameters you may need to look at many different spreading distributions. Can you comment on this trade-off?

Reply:

We agree that it would be desirable to look at a few different spreading functions as part of a systematic design study. We have not done this, but suspect that the idea we have proposed would still be advantageous: the number of spreading functions should certainly be much less than the number of paired headings. For diffracting structures exposed to severe locally wind-driven storms, directional spreading in the incoming wave fields is usually large, so standard deviations of spreading $\sigma_\theta \approx 20^o$ to 30^o are reasonable; given that this implies waves distributed over perhaps a total angle of $\sim 60^o$, it would be reasonable to repeat our procedure for perhaps 2 or 3 mean approach directions. Of course, the response of structures to long distance swell is a different issue for which more computations may be needed.

68.2 Discusser - G. Klopman

The wave field at each frequency appears to be spatially inhomogeneous (Fig.1), whereas a multi-directional random wave field is known to be spatially homogeneous (wave height is the same everywhere). As a consequence, the results (e.g.QTF) depend on how the coordinate system origin with respect to a structure is chosen. This seems to be due to the fact that at each frequency, the phases of the contributing M wave directions are chosen locked or equal, while in a multi-directional random wave field, they should be chosen random for each of the M directions ($M >> 1$).

Reply:

The aim of our approach is not to examine response at an arbitrary point in an inhomogeneous wave field. Rather, we aim to capture an extreme design condition through use of a wave group focused at a particular point in space. This is the NEWWAVE idea, which we are implementing for directional seas.

68.3 Discusser - P. Ferrant

I have a question about numerical efficiency. You show how to reduce computational demand by summing all waves of same frequency in a single incoming wave. A suggestion would be to sum also on frequencies and consider the incident wave as a time depending quantity, and solve the 2nd order diffraction in the time domain. Could you comment on that? The second part of my question is related to the computing demand of your method. Thank you.

Reply:

It would certainly be possible to proceed as suggested by the discusser. We know that the time domain second order analysis developed in his group has had considerable attention paid to issues of efficiency. It is difficult to provide a useful answer concerning the resources required by our method, other than by comparing it with other methods run on the same non-standard computer installation we are using.

68.4 Discusser - X.B. Chen

It's an interesting work which shows the importance of wave directionality even in drift force computations. My questions are (a) Your QTFs of mean drift forces are obtained by the far-field formulation or by the near-field one? If the near-field formulation is used, have you some numerical convergence test? (b) You showed QTFs of vertical mean drift force to which the 2nd-order set down of incoming waves may have an important contribution. In this respect, I'd like to invite from Professor Eatock Taylor a discussion on my notes [1] in which a solution is proposed.

[1] Chen, X.B. "The set-down of the second-order Stokes waves", personal notes, 2005.

Reply:

a) Convergence tests using near and far field methods to compute mean drift forces have not suggested any problems with the quadratic panel method implementation we have adopted.

b) Dr Chen's unpublished notes provide a very interesting follow-up to the question posed at the 4th IWWWFB (Eatock Taylor, 1989), concerning the behavior of second order low frequency vertical drift forces in the limit as the difference frequency tends to zero. This is linked to the behavior of the low frequency set-down, which does not tend in the limit to the classical mean set-down in regular waves. In the 1989 Abstract a corrective remedy to the apparent inconsistency was suggested, based on the multiple scales expansion given by Mei (1983). It was shown that to achieve consistency, the classical Stokes set-down in regular waves, $-a^2k/(2\sinh 2kd)$, must be supplemented by the term $-a^2kgc_g(2\omega+kc_g\mathrm{sech}2kd)/[4\omega^2(c_g^2-gd)]$, where c_g is the group velocity, k is wave number, g is the acceleration due to gravity, ω is frequency and d is water depth. Chen has now provided a simpler analysis leading to the same result. He shows that this additional quantity, C, may alternatively be obtained by introducing the term $-Cgt$ into the second order regular wave potential. He also shows that the resulting contribution to set-down is much more significant than the classical Stokes term. This effect appears to have been widely overlooked in second order analysis.

Eatock Taylor, R. (1989) Is there an inconsistency in the treatment of low frequency second order vertical forces? Proc. 4th IWWWFB, Oystese. Mei, C.C. (1983) The Applied Dynamics of Ocean Surface Waves, pp. 607-620. Wiley Interscience.