EIGHTH
INTERNATIONAL WORKSHOP ON
WATER WAVES AND FLOATING BODIES

Volume 2: Discussion Records

St. John's, Newfoundland
23-26 May 1993

WORKSHOP PRESENTERS

EIGHTH INTERNATIONAL
WORKSHOP ON WATER WAVES AND FLOATING BODIES
ST. JOHN'S, NEWFOUNDLAND, CANADA
23-26 MAY 1993

Discussion Record

Session: I

Title: Nonlinear Ship Responses in Head Waves

Presenter: L.J.M. Adegeest

Discussant: M. Tulin

Question #1:

Can you tell us how it comes about that the wave forces and moments, and the motions behave linearly with sea state, while the midship bending movement does not?

Question #2:

What is the importance of the dynamic rise at the bow for the midship bending movements, and how can you estimate it accurately?

Answer #1:

Nonlinear motions are caused by nonlinear <u>total</u> forces ($a = M^{-1}F$), nonlinear bending moments are caused by nonlinear motions/acc./vel. and nonlinear forces on a part of the body. The latter contributions show relatively much more nonlinear behaviour than the total force.

i.e. A wedge-shaped body which is given a pitch excursion in still water still experiences a linear vertical force but the bending moment will contain a second harmonic.

Answer #2:

The dynamic rise certainly affects the bending moments. It can be accounted for by defining the actual wetted surface as the result of the incident wave, the position of the body and the dynamic rise or mainly the amplitude of the radiated wave. I didn't make this correction. I assumed that the amplitude of the radiated wave is of a significantly smaller magnitude than the relative motion defined by the incident waves and body position.

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Discussion Record

Session: X

Title: Validation Gives New Insights Into Nonlinear Inviscid Flow Computations

Presenter: V. Bertram

Discussant: H.B. Bingham

Question:

You showed a change of up to 200% in the wave resistance calculations by adding only 25% more panels. Can you be sure that these calculations are converged?

Answer:

No, I cannot be sure. But further grid variation tests are an indication that the agreement is not a coincidence. The major message is that resistance is numerically extremely sensitive. The flow details (pressure distribution, wave cuts) were quite similar for both grids. Small differences in the details (and coarser geometry approximation) have large effects on resistance.

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Session: X

Title: Validation Gives New Insights Into Nonlinear Inviscid Flow Computations

Presenter: V. Bertram

Discussant: P. Martin

Question:

A comment. In the boundary elements community, there is interest in exploiting CAD technology. Thus, a CAD system yields a nice representation of the geometry, using rather high order approximations. This could (should?) then be used directly in the BEM code, instead of approximating the geometry afresh, using flat panels, say.

Answer:

I agree. Even if flat panels of constant source strengths are used the expression $n_x dA$ in $R_w = \int p\, n_x\, dA$ could be computed with higher accuracy from a CAD description. This would neither increase complexity or CPU-time requirements for the wave resistance code. However, I am not aware of anybody doing this so far.

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Discussion Record

Session: X

Title: Validation Gives New Insights Into Nonlinear Inviscid Flow Computations

Presenter: V. Bertram

Discussant: K. Mori

Question:

What is the main cause for the difference between the results by Grid 1 and Grid 2; hull geometry or the pressure itself?

Although it is the 2-D flow by a submerged doublet, we could have almost the wave profile even by the double size panel when quadratic expression for the source strength is introduced. The higher expression for the variables may weaken the grid dependency (proc. of continued Workshop on Wave Resistance, 1980).

Answer:

I believe a major contribution comes from a smoother distribution of elements due to automatic grid generation based on a CAD description for Grid 2.

I am not convinced that higher-order panels are superior for complex 3-D geometries such as real ship hull geometries. Their value for simple geometries is beyond doubt. For sensitive geometries like bulbous bows grid generation might be so much easier for first-order panels that it compensates the longer computations using more elements.

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Session: X

Title: Validation Gives New Insights Into Nonlinear Inviscid Flow Computations

Presenter: V. Bertram

Discussant: J.N. Newman

Question (Comment):

I would like to endorse the suggestion, made in the oral presentation, that we should have a menu of low/high-order panel codes. Robust convergence implies that the results should not depend strongly on either the number of panels or the order of the approximations. We have been trying to develop a Rankine solver with this generality, based on B-splines of arbitrary order. Preliminary (two-dimensional) results will be presented at the Numerical Ship Hydrodynamics Conference in Iowa City, in August.

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Discussion Record

Session: III

Title: Force Calculations using the Linearized Radiation Potentials at Steady Forward Speed

Presenter: H.B. Bingham

Discussant: I.Y. Gong

Question:

When we solve the hydrodynamic problems in time domain within linear theory, the formulations are parallel to the corresponding formulations of frequency domain through Fourier transforms. In view of this, the features appearing in the frequency domain calculation results also appear in time domain solutions. For example, the irregular frequency effects in frequency domain calculations may appear in the impulse response function in time domain as an oscillating component as time elapses.

In case that there is a forward speed, U, there exists another critical frequency component corresponding to $U\omega/g=1/4$, which results in another fluctuating component in time domain solution, and is difficult to decay. How do you treat those problems? And do you have any Fourier-transformed results of your time domain calculations? I also want to know the major difference of your formulations with those of Liapis & Beck (1985). Thank you.

Answer:

The oscillation corresponding to $\tau=1/4$ is clearly visible in the impulse response functions. It decays approximately like 1/t and nothing special is done to it. Yes, I have Fourier transforms and the initial frequency appears as a sharp peek. The formulation is essentially identical to Liapis & Beck.

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Session: III

Title: Force Calculations Using the Linearized Radiation Potentials at Steady Forward Speed

Presenter: H.B. Bingham

Discussant: M. Kashiwagi

Question:

In reference to the difference in the force calculation predicted by two kinds of pressure integral; one is by a combination of (6) and (7), the other is by (8), I want to point out an analytical difference between the two. Eqs. (7) and (8) say that the following holds:

$$A_{jk}=\iint_{S_0} U\frac{\partial\phi_k}{\partial x}n_j dS=\iint_{S_0}\phi_k m_j dS \quad (*)$$

But if we don't include the steady perturbation potential from the beginning, we can transform as follows:

$$A_{jk}=U\iint_{S_0}\left[\frac{\partial}{\partial x}(\phi_k n_j)-\phi_k\frac{\partial n_j}{\partial x}\right]dS=-U\iint_{S_0}\phi_k\frac{\partial n_j}{\partial x}dS$$

In the case of j=5, $n_5=zn_1-xn_3$, thus:

$$\frac{\partial n_5}{\partial x}=-n_3+z\frac{\partial n_1}{\partial x}$$

Therefore with the relation $m_5=Un_3$ shown in your abstract, we get:

continued...

$$A_{5k}=\iint_{S_0}\phi_k\,[Un_3-Uz\frac{\partial n_1}{\partial x}]\,dS$$

and:

$$A_{5k}=\iint_{S_0}\phi_k m_5\,dS-U\iint_{S_0}\phi_k z\frac{\partial n_1}{\partial x}\,dS$$

This second term is generally not zero, which differs from (*)!

Answer:

Yes, that explains the difference. Thank you.

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Session: III

Title: Force Calculations Using The Linearized Radiation Potentials at Steady Forward Speed

Presenter: H.B. Bingham

Discussant: P. Martin

Question:

Are you saying that Tuck's theorem is not a theorem?

Answer:

No. I am questioning its applicability to a formulation which is linearized around the free-stream. Tuck's theorem relies upon the basis flow satisfying the body boundary condition, which it does not when:

$$\Phi^{(0)}=-Ux$$

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Discussion Record

Session:

Title: An Absorbing Beach for Numerical Simulations of Nonlinear Waves in a Wave Tank

Presenter: Y. Cao

Discussant: S. Grilli

Question:

First of all, I think, it should be pointed out, the idea of introducing a modification of the free surface condition was supposed by Le Mehaute, about 20 years ago. Then, people - e.g. Israel & Orszag - introduced sponge layers, and, more recently, this was re-applied by Cointe. Thus, I think, it should be better to call your absorber a sponge layer, or something like that, and not a beach, since in any case, it does not resemble a beach in any way.

In the assessment of performance of radiation or absorbing boundary conditions, I think the first test case to check, should be one with clean incident wave of permanent form, e.g., in the present nonlinear case, a "streamfunction" wave. In your case, the use of a simple wavemaker to generate a fairly nonlinear wave introduces high-order harmonics, i.e. modulation in your incident wave train. Hence, it is hard to check whether changes in the wave are due to reflection or modulation in the wave train.

In our experience with radiation conditions, in the nonlinear model we developed, we found out, a simple Sommerfeld condition could fully absorb a nonlinear wave of permanent form. Hence, for a narrow banded wave train, I think a good combination could be a Sommerfeld condition based on the phase velocity of leading frequency wave, and a sponge layer tuned to absorb higher frequency oscillations. Can you comment on this?

In any case, until you have run many cases with different wave characteristics (L,T), and with a permanent form nonlinear wave, it is hard to tell whether your method is efficient or not.

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An Absorbing Beach for Numerical Simulations of Nonlinear Waves in a Wave Tank

Answer:

The terminology is confusing. Different terms (e.g. absorbing boundary condition, wave damping, Newtonian cooling, sponge layer, absorbing beach) have been used by researchers for the same purpose.

We are interested in initial boundary value problems and we agree that the wave train generated by the wavemaker is subject to nonlinear modulation and wave reflection. But the envelope frequency of the modulation and that of the reflected waves should remain distinct. Combining the Sommerfeld condition and a sponge layer as you suggested may work well. However, it should be pointed out that for our initial boundary value problems, we do not assume that the leading frequency is known a priori and that determining the phase velocity of the leading frequency wave accurately during the time marching procedure is not an easy task. In fact, we have tried the combination of the Sommerfeld condition and our energy absorber, which did not work as well as when the energy absorber was used alone. We are not claiming that our technique is better than others because we haven't found a satisfactory means of assessing the performance of the absorber for general nonlinear waves. However, our technique does have its advantages (e.g., clear physical meaning and easy implementation).

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Title: An Absorbing Beach for Numerical Simulations of Nonlinear Waves in a Wave Tank

Presenter: Y. Cao

Discussant: K-h. Mori

Question:

We had tried also numerical absorber. We supposed an absorber consisting of horizontal parallel plates where the vertical velocity components are imposed to be zero. We got successful results. Refer to our paper, Jour. of Soc. of Naval Arch. of Japan, Vol. 173, 1993.

Answer:

Thank you for bringing our attention to your work and for the paper you sent us later. The horizontal plate absorber below the free surface works very well with your N-S solver. However, it seems that the horizontal plate may introduce additional, unnecessary difficulties in solving potential flow problems using a boundary integral technique.

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Session:

Title: An Absorbing Beach for Numerical Simulations of Nonlinear Waves in a Wave Tank

Presenter: Y. Cao

Discussant: A. Clément

Question:

Could you give some spectral characterization of the efficiency of absorbing beaches? Do you think that the function $\nu(\chi)$ has to be tuned to the frequency of the incoming waves, if any?

Answer:

We did not examine the spectral characteristics of the beach since we had not yet found a satisfactory measure of the efficiency of the beach for nonlinear waves. In our study, we attempted to develop a beach which is insensitive to the frequencies of incoming waves and we do not intend to tune the function $\nu(\chi)$ to a specific frequency. However, tuning of $\nu(\chi)$ would probably increase the efficiency.

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Session: V

Title: An Absorbing Beach for Numerical Simulations of Nonlinear Waves in a Wave Tank

Presenter: Y. Cao

Discussant: J.S. Pawlowski

Question:

I would like to point out that there are good options for truncating a fluid domain by other means than an absorbing beach with artificial damping. Namely a gluing with an external perturbation solution can be made consistently in 3-D, and consistently under the weak-scatter hypothesis in 2-D. I am not sure, also, if you can efficiently control that the rate at which you take energy out by an absorbing beach and do not introduce spurious effects in the flow.

Answer:

We agree with you that there are other options for truncating a fluid domain. Matching to (or gluing with) a simplified external solution has several problems, as mentioned in our abstract, including difficulty of implementation, high computational overhead, and poor modelling of behaviour of outer part of the fluid at the truncation when the nonlinearity is still strong. The rate at which the energy is being taken out by the absorbing beach can be controlled by an energy flux balance for the beach area.

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Session:

Title: An Absorbing Beach for Numerical Simulations of Nonlinear Waves in a Wave Tank

Presenter: Y. Cao

Discussant: D.K.P. Yue

Question:

I have a number of problems with your work:

1. the purpose of an absorbing beach is to damp out the wave (or signal), in this case ϕ (e.g., $P = \nu\phi$ would ensure that $\frac{d\phi^2}{dt}$ is negative definite). A beach that reduces the energy would have the opposite effect for example for a 'negative' energy wave component or signal.

2. repeatability over integral (forcing) periods is a valid test only if the nonlinear wavetrain is supposed to be periodic with the same period - this cannot be postulated in general even for very simple situations.

Answer:

1. $\frac{d\phi^2}{dt}$ is obviously not negative definite (even with $P = \nu\phi$). Our treatment ($P = \nu\phi$) ensures no local energy input to the fluid by the energy absorber. While $P = \nu\phi$ may put energy into the fluid at times.

2. We agree with you.

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Session:

Title: An Absorbing Beach for Numerical Simulations of Nonlinear Waves in a Wave Tank

Presenter: Y. Cao

Discussant: T. Vada

Question:

Is there any particular reason why you add the dissipation to the dynamic boundary condition rather than the kinematic? Do you think that the performance of the beach would have been different if dissipation was added to the kinematic condition? If the latter would happen to be better it should be preferred even though the physical interpretation of the extra term is more difficult.

Answer:

The reason we added the extra term to the dynamic condition is that it has a clear physical meaning, that is energy absorption. The addition of the term to the dynamic boundary condition does not violate the physical laws, namely the mass and energy conservations. Adding an extra term to the kinematic boundary condition will violate the mass conservation. The kinematic boundary condition states that the fluid particles on the free surface remain on the free surface. The extra term will cause fluid particles leave the free surface or the particles in the field enter the free surface.

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Discussion Record

Session: VII

Title: Optimization of Ship Wave Resistance in Shallow Water

Presenter: X-N. Chen

Discussant: V. Bertram

Question:

Why do you optimitize for near-critical speeds? Ships usually do not operate at near-critical speeds.

Your error margin in R_w is considerably larger than 10%. How can you be sure that your "optimized" hull is better than your original hull? You should always include a margin of uncertainty in your optimum, i.e., a bandwidth of section curves in your case.

Answer:

It is true that a ship would not normally operate at near-critical speeds. However, a fast ship must transit the critical hump in order to reach the favourable supercritical range. This is why our first attempt was aimed at reducing the maximum value. Another reason was that our original equation is valid only at near-critical speeds. Meanwhile, we hope to have extended its range of applicability considerably. So future optimizations will be possible also at more realistic off-critical speeds.

It is not true that our average error margin is considerably large than 10%. Actually, the average discrepancy between our calculations and experiments in the supercritical range is only about 3%. As usual in such applications, our optimization is based on the heuristic principle that even though the absolute values of calculated resistance are not sufficiently accurate, the trend toward resistance reduction may be realistic. Of course, this needs to be verified by model experiment before one can recommend actual use of the theoretical hull form.

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Discussion Record

Session: II

Title: A Realizable Force Feedback-Feedforward Control Loop for A Piston Wave Absorber

Presenter: A. Clément

Discussant: Y. Cao

Question #1:

Did you check the energy balance of the system? In other words, are the energy absorbed by the wave absorber, energy provided by the wavemaker and the wave energy well balanced?

Question #2:

How effective is the wave absorber for the nonlinear incident waves of multiple frequencies?

Answer #1:

We have tested our absorption law by implementing it in an analytical 2D linearized wave basis. The potential and all the derived quantities are then known - as precisely as we want, and are not obtained by an approximation process - such as a BEM. Then in this particular validation study, the energy balance is satisfied by construction, the potential being the exact solution of the boundary integral problem in the rectangular domain.

Answer #2:

This wave absorber has not yet been tested in nonlinear theory. This is one of the tasks we are going to do, from now. In the linear approach, we expect that the superposition of waves of different frequencies will not affect the results of the PUFF mode, but this remains to be confirmed by numerical experiments.

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Session: II

Title: A Realizable Force Feedback-Feedforward Control Loop for a Piston Wave-Absorber

Presenter: A. Clément

Discussant: S. Grilli

Question:

Your method seems to work better for long waves than for short waves. My feeling is this also corresponds to the fact, velocity is more uniform over depth in a long wave than in a short wave.

Do you, then, think there would be any interest in extending your method to multi-segment absorbing plates, in a way that would better fit the velocity diagram?

Answer:

Some years ago (1), I studied wave absorption by a dual-flap system. At that time, I had the same feeling as you express now. But today, I have returned to the conviction that a single mode piston type wave-absorber is a more reasonable solution. In the linear frequency domain model, it was easily shown to be theoretically able to absorb even the shortest waves, at the price of generating more and more near-field waves as the frequency increases. The practical consequence of that lies only in a more complicated force-based control of the absorber due to the increasing phase lag between force and motion in the high frequency range.

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A Realizable Force Feedback-Feedforward Control Loop for a Piston Wave-Absorber

Answer Contd...

Adding a degree of freedom with a segmented wave-absorber would only add complexity to the control problem, but no potentiality regarding the maximum theoretical performance, at least in that linear modelization.

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1. **A. CLÉMENT, E. CHAPUIS:** Un générateur-absorbeur de vagues; à double volets articulés; 1985 - 10th Canadian Congress on Applied Mechanics (CANCAM); University of Western Ontario - London. ISBN 0-920049-01-X

A. CLÉMENT: Simulation et optimisation numérique du fonctionnement d'un générateur-absorbeur de vagues à double volet; 1987-XXII-ème IAHR Congress. Lausanne. ISBN 0-660-53811-3

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Session: II

Title: A Realizable Force Feedback-Feedforward Control Loop for a Piston Wave Absorber

Presenter: A. Clément

Discussant: W. Sulisz

Question:

Your approach is linear. Nonlinear effects generated by a simosoidally moving wavemaker usually significant for waves of low frequency. How do you explain a very good efficiency of your <u>linear</u> absorber for waves of low frequency?

Answer:

Our approach being entirely linear, our results are consistent with our assumptions. In the low frequency range, the Airy waves velocity field approach an oscillating piston mode; then the near-field contribution in the radiated waves vanishes, and the piston absorber tends to be ideally fitted to the incident wave field, with an efficiency of 100% in the (theoretical!) limit $\omega \rightarrow 0$.

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Discussion Record

Session: I

Title: A Multidomain Approach to Free Surface Viscous Flows

Presenter: A. DiMascio

Discussant: K. Mori

Question:

In case of the viscous flow with free-surface, there are two parameters, Rn and Fn. The grid side should be based on Rn in the viscous domain while Fn wave length in the potential domain. Nevertheless, you use the same grid system for both. How about using different grid schemes which may work well in each domain?

Answer:

The mesh spacing in the computations performed in the 3D test cases was chosen to satisfy the constraints related both to Froude and Reynolds number. In more details, the nodes were clustered near the walls to have sufficient resolution in the boundary layer (the thickness of the first cell near the hull was $\delta = \frac{1}{20\sqrt{Re}}$). Moreover, the number of points per wave length was at least 25. For sake of simplicity, in the 3D test cases performed, the inner and the outer grids have the same number of sections in the streamwise direction. This was not a restriction of the numerical algorithm, which can work with different grid systems as well. The possible advantages of a different arrangement of the meshes will be investigated.

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Discussion Record

Session: I

Title: A Multidomain Approach to Free Surface Viscous Flows

Presenter: A. DiMascio

Discussant: W. Sulisz

Question:

Why the velocity potential was split?

Answer:

The Dawson model (3) is one of the most used linearized models in potential ship flow computations. This model, in which the double model flow is the basis flow for linearization, allows a good prediction of the free surface flow also for non-slender ship hulls. For this reason, we preferred the Dawson decomposition with respect to the Kelvin linearization also in the multidomain computation.

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Discussion Record

Session: I

Title: A Multidomain Approach to Free Surface Viscous Flows

Presenter: A. DiMascio

Discussant: M.P. Tulin

Question:

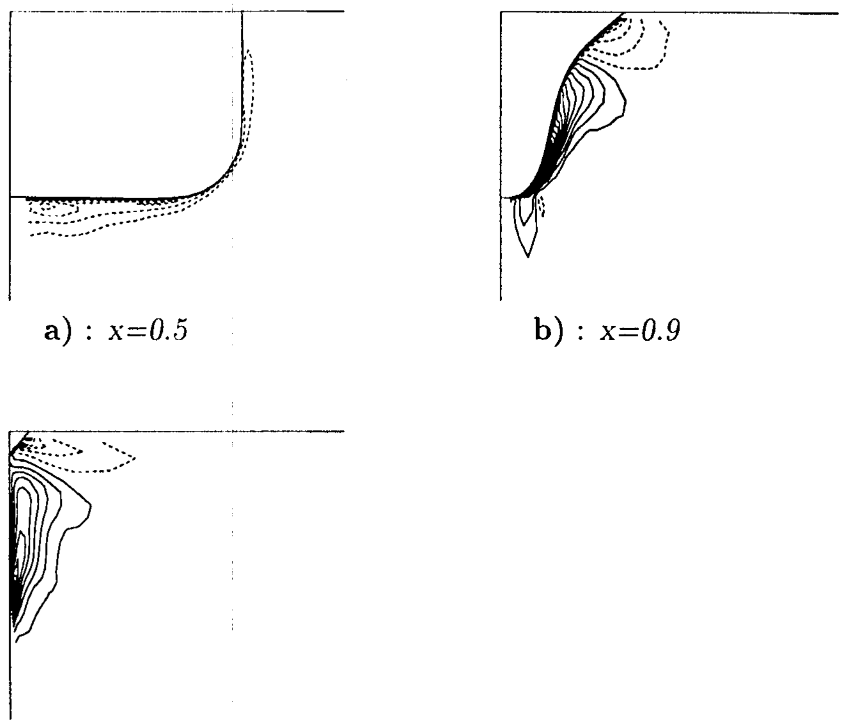
Your work seems very impressive.

Can you resolve vortex structures originating at the bow and stern?

Answer:

continued on next page.

ANSWER



a) : x=*0.5*

b) : x=*0.9*

c) : x=*1.0*

Figure 1: S60 C_b=0.6 - Double model flow at $Re = 4.5 \times 10^6$
x–vorticity countours. Solid lines: positive values; Broken lines: negative values.($\Delta\omega_x = 2.$)

In the above pictures, the x–component of vorticity is plotted on three different planes at x=constant (x is the non–dimensional distance from the bow) for the double model flow past the S60 C_b=0.6 hull. These plots are in good agreement with experimental data reported in *Toda, Y., Stern, F.,Longo, J., IIHR report No.352, Iowa Institute of Hydraulic Research, The University of Iowa 1991*, at least regarding to the qualitative features of the flow field. These pictures show that the proposed algorithm gives a satisfactory prediction also of the vortical structure at the midship and the stern region.

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Discussion Record

Session: I

Title: Computation of Fully Nonlinear Free Surface Flows in Three Dimensions

Presenter: P. Ferrant

Discussant: S. Grilli

Question:

It seems you experienced some of the sawtooth instabilities also observed by Longuette-Higgins and other (LH).

There have been other models developed for nonlinear waves (e.g. Dold & Peregrine) that have proved both more stable and less demanding in terms of time step size.

Is there any reason why you selected LH's model versus a more recent and efficient model?

Answer:

When using Taylor series expansions as a basis for the time marching procedure, one is left with a cascade of boundary value problems for each term of the series (ϕ, ϕ_t,...) with identical kernels.

In 2D simulations, this is not a problem, since the number of unknowns remains moderate, and the direct solvers usually implemented can cope with multiple right hand sides at a negligible CPU time overhead.

The situation is different in 3D, where the number of unknowns prohibits the use of a direct solver. Each solution for the different terms of the series by an iterative method would thus sensibly increase the CPU time, which is critical for much computations.

The multi-domain approach presented by M.P. Tulin, et al in this workshop is of particular interest in that respect, since it applies with the same success to 3D. The resulting systems could reasonably be solved by a direct method, and Dold and Peregrine time marching procedure could be tested in nonlinear 3D cases.

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Discussion Record

Session: I

Title: Computation of Fully Nonlinear Free Surface Flows in Three Dimensions

Presenter: P. Ferrant

Discussant: D.K.P. Yue

Question:

In a recent work (Xu and Yue, 1992, 20th ONR symposium) in which we considered many of the same issues you do, we performed a convergence study for the piecewise linear (flat) panel with <u>constant</u> strength (CPM) as compared to isoparametric quadratic panels (QBEM). Your present table 1 adds a useful data point to the understanding of the solution behaviour near a Dirichlet-Neumann boundary intersection. Since the <u>maximum</u> rather than the average error controls the success of a Lagrangian method - do you have results for the maximum error for the study of table 1?

Answer:

The additional results you request are given below (relative errors):

Unknowns	Panels	Err.Max Neumann	Err. Max Dirichlet	E_N Max/h^2	E_D Max/h
417	768	10^{-3}	7.8 10^{-2}	0.756	1.25
641	1200	6.5 10^{-4}	6.2 10^{-2}	0.260	1.24
1411	2700	2.9 10^{-4}	4.2 10^{-2}	0.760	1.27
2481	4800	1.6 10^{-5}	3.2 10^{-2}	0.756	1.28
2731	5292	1.5 10^{-5}	2.95 10^{-2}	0.764	1.24

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Computation of Fully Nonlinear Free Surface Flows in Three Dimensions

One can see that at Neumann boundaries, max and mean relative errors are strictly $O(h^2)$, with $EN_{max} \sim 2EN_{mean}$, which means that the error is very smoothly varying on the surface. On the contrary, the max error on the Dirichlet boundary is O(h), but is confined at control points situated at the intersection.

I agree that the maximum error is the correct measure of the accuracy of the solver. However, in the present nonlinear computations, Dirichlet and Neumann boundaries do not intersect and the maximum relative error on the Dirichlet boundary (free surface) remains $O(h^2)$.

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Discussion Record

Session: XI

Title: Time Domain Analysis of Wave Exciting Forces Acting on a Floating Body

Presenter: I-Y. Gong

Discussant: A. Clément

Question:

Why do you work with acceleration potential ϕ_t instead of ϕ? Is it to simplify force calculations, or is there another hidden reason?

Answer:

It is partly due to the simpleness when calculating the hydrodynamic forces, however, main reason for that is because the closed form for incident wave potential in time domain was derived as an acceleration potential. Even if the formulations were made in terms of velocity potential, same results may be obtained, but with more computing times because in that case closed form for incident wave velocity potential in time domain is difficult to find, and we should resort to some numerical methods to calculate that.

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Discussion Record

Session: XI

Title: Time Domain Analysis of Wave Exciting Forces Acting on A Floating Body

Presenter: I-Y. Gong

Discussant: P. Ferrant

Question:

You compare your results with frequency domain results, after Fourier transformation. Why don't you compare directly with existing time-domain diffraction results, such as those of King (1987)?

Could you also comment on the difference between your method and King's one?

You present the time domain approach is much more time demanding than the conventional frequency domain approach. This obviously depends on the method used for the evaluation of the Green function. What kind of scheme do you use for this part of the computation?

Answer:

Strictly speaking, the impulsive wave response functions calculated in my research are not unique and they are not characteristic curves of the body, in the sense that the shape may vary depending on the distance between the body and the location where the impulsive wave elevation is supposed to occur. When Fourier-transformed into frequency domain, this mean that phases of exciting force may vary depending on the reference point while its magnitude remains the same. The non-uniqueness of this impulsive wave response function is one of the reasons why I didn't compare my results with those of King. And his results were not available at the time of my computations.

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Time Domain Analysis of Wave Exciting Forces Acting on A Floating Body

The major difference of my formulation with those of King is that I used "impulsive incident wave" concept like an impulse response function in time domain radiation problems. For that, I derived and used incident wave acceleration potential in time domain as closed form, which results in much more efficient computational scheme. And this enables the time domain formulation of exciting force straight forward without any transformations from frequency domain.

In fact, the direct time domain calculation of wave exciting forces need more computing time than the method of inverse Fourier-transforming the frequency domain calculation results. However, I think that current method is more legitimate. The most part of computing times are dedicated to calculating Green's function and convolution integrals. The computing time necessary for Green's function calculation, however, can be reduced significantly by using various approximate formulas such as given by J.N. Newman(1985). For example, the time dependent Green's function used in this research can be approximated by using Dawson's integral, series expansion, and asymptotic expansion depending on the ranges of variables. The method of reducing computing times necessary for convolution integral calculations need to be searched, however, to reduce total computation times.

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Discussion Record

Session:	III
Title:	Impact of Breaking Waves over Emerged and Submerged Coastal Structures
Presenter:	S.T. Grilli
Discussant:	W. Sulisz

Question:

Is an approach based on incompressible fluid and potential flow appropriate to describe impact problem?

Answer:

This problem has been analyzed by Peregrine and others. Using dimenensional analysis, they showed even for very large fluid accelerations (~10,000g) and velocity ($\sim 20\sqrt{gd}$), that compressibility effects are negligible, provided no air is captured by the breaking waves.

Hence, in the present case with wave impact on a vertical wall, when waves break and impact the wall with an almost vertical front face, little air is entrained and incompressible flow applies.

Now, for cases with air bubbles in the fluid, or even with a large size air cushion, experiments show (e.g. Haltoriet et al 1992) the first impact pressure peak, though slightly reduced, is followed by large amplitude high frequency pressure oscillations. This problem has been looked at by Toplins and Peregrine 1992.

Finally, we believe, the important factor for a structure is more the impulse, i.e. force times duration, than the pressure force itself. In particular, the first peak, which is of very short duration in time, should not provide significant impulse, and, hence, influence or jeopardize the global structure stability. This peak might, however, be of importance for the local stability of structural parts (beams, concrete grains).

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Discussion Record

Session: III

Title: Impact of Breaking Waves over Emerged and Submerged Coastal Structures

Presenter: S.T. Grilli

Discussant: M.P. Tulin

Question:

Comment: Professor Grilli's work is most impressive; he has not only developed a sophisticated non-linear numerical wave tank, but he has applied it effectively to provide detailed analyses of engineering problems in the coastal zone, and particularly at the shore line.

Have you considered the small inclination from the vertical of the wall upon the peak loadings experienced when a wave with a flat vertical face impinges upon a vertical wall?

Answer:

First of all, thank you for your nice comments.

About your question; apart from the vertical wall and the curved wall, we have not tried out any other geometry. Experiments with inclined walls - both forward and backward - however, have been reported by Kirkgöz, 1991, and I believe, in these experiments, he also observed a similar pattern for the impact pressures, with a first high peak, and a smaller second peak.

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Session: III

Title: Impact of Breaking Waves over Emerged and Submerged Coastal Structures

Presenter: S.T. Grilli

Discussant: T. Vada

Question:

The experiments indicate a significant energy dissipation as the waves pass the step. It is thus to be expected that the numerical results will overpredict the loads. Have you made any attempts to account for this? A possible (?) solution may be to compare the numerical results with the results from an experimental wave which have the same energy as the numerical wave after it has passed over the step.

Answer:

This is an interesting comment. We indeed believe that the fact the model overpredicts incident wave height by about 10-15% after the step may partly explain the overprediction of impact pressures.

On the other hand, however, earlier results where we compared detailed measured and calculated free surface elevation showed that changes due to dissipation are mostly local at around wave crests, whereas most of the wave profile backward and forward of the crest is almost unperturbed by the dissipation at the step. The total energy dissipation also is only on the order 2-3%. Hence this cannot "per se" explain differences in pressure of 70-80%.

Other factors like wall roughness, and experimental sampling rate, likely play a major role.

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Discussion Record

Session: III

Title: Impact of Breaking Waves over Emerged and Submerged Coastal Structures

Presenter: S.T. Grilli

Discussant: D.K.P. Yue

Question:

I am very interested in your interesting work and we have ourselves looked at a related problem of plunging wave impact with a distinct trapped air pocket (Tanizawa & Yue, 1991, 1992 in these workshops). My question is however a purely practical one - why would one design a wave breaker with a step and a vertical wall?

Answer:

For cases with fairly deep water, it becomes expensive to use trapezoidal breakwaters, and people have preferred using the combination of a rubble berm and a vertical caisson - generally in concrete. This combination, often referred to as a "mixed breakwater", has been extensively used, and studied, for instance, in Japan.

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Discussion Record

Session: V

Title: Second-Order Diffraction & Radiation Solutions on Floating Bodies

Presenter: C.H. Lee

Discussant: M.H. Kim

Question:

In equation (6), the body surface integral that contains 2nd spatial derivatives is replaced by the water line integral through the use of Stoke's theorem. This is quite obvious for simple geometry such as hemisphere. However, in case of bottom-mounted vertical cylinder, do we also have to include the integral along the circle of the bottom? Is the use of Stoke's theorem valid for very complicated, multiply connected geometry such as a TLP?

Answer:

1) It is right that the line integral includes the intersection of the cylinder with sea bottom. However we do not solve the radiation problem for the bottom mounted cylinder.

2) We think the geometry of TLP does not introduce difficulty in applying Stoke's theorem since there is no circulation. Thus the application of the Stokes theorem is valid as long as the first-order potential has continuous second derivative and the body surface is smooth. It may be difficult to have accurate numerical results for the bodies with sharp corners. Even in this situation, we expect that the use of Stokes theorem (assuming small nonzero radius of curvature at the corner) would give better numerical result than the direct integration of the double derivative.

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Discussion Record

Session: VI

Title: Time-Domain Analysis for Floating Bodies in Mild-Slope Waves of Large Amplitude

Presenter: D.W.M. Lin

Discussant: L. Adegeest

Question:

In Lamps, you solved the ship-motion problem for large amplitude waves in the time domain by integration of the pressure over the actual wetted surface. Can you give an indication what % of the nonlinearity in the resulting bending moments was caused by the Froude-Krylov part?

Answer:

I certainly believe that the nonlinear Froude-Krylov forces have important contribution to the overall bending moment. The actual percentage of this contribution may vary from time step to time step depending on the relative position and orientation of the ship and the waves. So far, no attempt has been made to quantify the contribution of the nonlinear Froude-Krylov forces.

Computations will be performed to address this question. The authors will communicate with Mr. Adegeest once the result is available.

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Session: VI

Title: Time-Domain Analysis for Floating Bodies in Mild-Slope Waves of Large Amplitude

Presenter: D.W.M. Lin

Discussant: R.E. Baddour

Question:

You mentioned that it takes a lot of time to compute with the nonlinear version compared to the linear one. But the computation time is dependent on the number of panels and the time step, which are also sensitive to the numerical accuracy.

How did you decide the number of panels and the time interval?

Answer:

Yes, computation time does depend on the number of time steps and the number of panels used. In LAMP calculations, we typically use 30 to 40 time steps per wave or motion period if the wave or ship motion is periodic. It has been tested consistently and the results are shown to be conveyed with 1%. The number of panels required depends on ship geometry. Complicated geometry needs more panels. Typically 250 panels are used on one side of the ship for symmetric case. The following table shows the heave and pitch responses of a series 60. C_B=0.7 ship in a sinusoidal wave with nondimensional frequency 2.0. Two different types of panel distributions and different numbers of panels are used for testing. As can be seen, the results converge with 1%.

CONVERGENCE TEST

PANELS	HEAVE RESPONSE H/A	PITCH RESPONSE θ/ A
180 Constant	0.12587	0.43996
250 Constant	0.12832	0.40857
360 Constant	0.12865	0.40516
180 Cosine	0.12879	0.48146
250 Cosine	0.12951	0.39628
360 Cosine	0.12973	0.40932

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Session: VI

Title: Time-Domain Analysis for Floating Bodies in Mild-Slope Waves of Large Amplitude

Presenter: D.W.M. Lin

Discussant: R.E. Baddour

Question:

Would you elaborate on the CPU * used or needed for your linear and nonlinear models, in particular for the 1800 second nonlinear simulation shown during your simulation?

Answer:

The linear calculation is much faster than the nonlinear calculation because the memory functions do not have to be recomputed every time steps from the beginning. For linear cases, the motion simulation can typically be performed in real time on a CRAY YMP computer. The nonlinear calculation is about 100 times slower.

For the 1800 second time-domain simulation, the linear calculation was performed first. Time instances when the three largest midship bending moments occurred were identified. Three 60 second long wave records before these identified time instances were used for nonlinear calculations. This is a very effective way to identify potential dangerous time instances and to quantity nonlinear effects.

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Discussion Record

Session: IX

Title: Resonant Reflection of Surface Waves Travelling Over Bottom Undulations

Presenter: Y. Liu & D.K.P. Yue

Discussant: D.V. Evans

Question:

How does your method compare to recent work of Davies and O'Hare in Applied Ocean Research who have solved the same problem using a method described by Devillard et. al recently in J. Fluid Mechanics which involves splitting the bottom topography into distinct steps and using an appropriate variational solution due to Miles (J.F. Mechanics 1968)?

Answer:

The present method represents the bottom exactly in terms of a Fourier series and accounts for nonlinear interactions of the bottom and free surface up to an arbitrary high-order in both the bottom and free-surface slopes. The solution converges exponentially fast with respect to the number of unknowns. Furthermore, the extension to three dimensions is straightforward. The method of Davies & O'Hare (1993) approximates in general a smooth bottom by a sequence of steps and the convergence of the solution is thus of low order (algebraic). The method accounts for the nonlinear effect of the bottom but not of the free surface. Significantly, extension to three dimensions is non-trivial. Despite their use of Miles' (1968) approximate solution for a step, the computational effort there (proportional to the number of steps) is in general not less than that of the present approach (proportional to the number of Fourier modes).

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Session: IX

Title: Resonant Reflection of Surface Waves Travelling Over Bottom Undulations

Presenter: Y. Liu & D.K.P. Yue

Discussant: M.H. Kim

Question:

Before going into complicated full three-dimensional problems, it seems to me that it is straightforward to extend your two-dimensional code to the case of oblique incident angles, which is practically very important.

Answer:

The extension of the present work to oblique waves is in principle straightforward and requires the modification of the basis functions (7) and (8). On the other hand, the direct generalization to <u>full</u> three dimensions for this approach is in fact quite simple. Computationally, the oblique wave case would be similar to a three-dimensional calculation with a single transverse mode.

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Discussion Record

Session: IX

Title: Resonant Reflection of Surface Waves Travelling Over Bottom Undulations

Presenter: Y. Liu & D.K.P. Yue

Discussant: P. Martin

Question:

You cite two papers on localization (Belzons et al. 1988 and Devillard et al. 1988). These papers give theory and experiments for a bottom composed of many steps. Will your method work for such a non-smooth geometry? If so, can you reproduce their results.

Answer:

Yes, the method does work for non-smooth geometries. However, the convergence of the solution with respect to the number of spectral modes becomes algebraic (rather than exponential for infinitely differentiable geometries). We are now investigating the phenomenon of localization using the present method and will report the results later.

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Discussion Record

Session: VII

Title: Boundary Integral Equations for Bodies of Small, but Finite, Thickness

Presenter: P. Martin

Discussant: R. Jefferys

Question:

(i) How thin is a thin body? (ii) Is this analysis relevant for thin gaps?

Answer:

(i) Let me quote from the cited paper by Krishnasamy et al.:

> "The difficulties with the basic boundary integral approach for thin bodies are twofold: (1) the BIE's often become nearly degenerate when parts of the surface are separated only by a small distance or thickness; and (2) the task of achieving even ordinary arithmetic accuracy with the BEM, in the face of the near singular integrals which arise in such problems, is very difficult."

Here, we have described two methods for dealing with (1). We can also deal with (2), which is a numerical difficulty; its severity depends on how well you can do the arithmetic. Thus, there is no absolute answer to your question.

(ii) The problem of a thin gap between two bodies is different, but can be analyzed in a similar way.

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Discussion Record

Session: VII

Title: Boundary Integral Equations for Bodies of Small, but Finite, Thickness

Presenter: P. Martin

Discussant: M. McIver

Question:

How do you choose the coupling parameter α in practice?

Answer:

This has been studied in detail by Lee & Sclavounos (1989). Note that there are several papers in the literature on the analogous question in acoustics.

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Discussion Record

Session: IV

Title: Nonlinear Wave Forces on Offshore Structures

Presenter: M. McIver

Discussant: W. Sulisz

Question:

You have mentioned that:

$$\frac{\partial^2}{\partial_x^2}\phi$$

is singular at the intersection of the free surface and a body. Is this applicable to any body geometry?

Answer:

I believe that:

$$\frac{\partial^2}{\partial_x^2}\phi$$

is singular for bodies intersecting the free surface at right angles and having finite curvature there, but not for wall-sided bodies.

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Discussion Record

Session: X

Title: Recovery of Open-Sea Results from Narrow Tank Tests

Presenter: P. McIver

Discussant: R. Jefferys

Question:

Under what circumstances can we believe any uncorrected tank measurements? An added practical complication is the build-up of cross-slop energy in wide tanks while the experiment progresses. I presume that this can affect sensitive phenomena such as ringing/springing as well as drift forces?

Answer:

The drift force is severely affected by the channel walls but many other hydrodynamic quantities (e.g. the first-order exciting force) are little changed for tanks of 'reasonable' width. See results in the cited papers by Linton & Evans and McIver & Bennett and also in Yeung & Sphaier (*J. Eng. Math., 23, 1989*).

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Session: X

Title: Recovery of Open-Sea Results from Narrow Tank Tests

Presenter: P. McIver

Discussant: M. Ohkusu

Question:

It seems to me that this work is based on the idea that provided the wave motion around a body in a channel is as predicted by a linear theory, we can exclude the channel effect from the experimental hydro-dynamic forces on the body. But if so, why don't you predict the forces by the linear theory without relying on the experiment? If you say that results of the experiment are unpredictable, it implies that the channel effect is unpredictable too!

Answer:

I agree that if it were possible to compute wave forces with the linearised theory in a straightforward way for <u>any</u> geometry then the procedure described here would probably not be needed. However, there must be many structural geometries for which the investigation is unable (or unwilling) to carry out accurate computations because of the complexity of the task. In such cases a straightforward procedure for processing tank measurements might be of value.

It might be worth noting that the procedures may be carried out in reverse. That is numerical results for open water can be modified to produce results for a narrow tank. Existing panel codes could then be used to investigate tank-wall effects.

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Discussion Record

Session: III

Title: Ship Generated Internal Waves in Highly Stratified Seas; The Inverse Problem

Presenter: T. Miloh

Discussant: P. Martin

Question:

What are the effects of noise (i.e. errors in your measurements) on your reconstruction of the density profile?

Answer:

The accuracy of the dispersion relationship and the pycnocline variation, clearly depend on the noise level of the measurements (coordinates of the crest lines). The error in the measurements is based on the particular method of remote sensing. However, the error in the slope data can be minimized by using a least squares method and the "best fit" for the dispersion is obtained by employing the "conjugate gradient" method and "golden section". Higher order modified Phillips-Miles spectra have been also used in order to improve the accuracy of the proposed inverse method.

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Discussion Record

Session: III

Title: Ship Generated Internal Waves in Highly Stratified Seas; The Inverse Problem

Presenter: T. Miloh

Discussant: M. McIver

Question:

Is the problem of determining the dispersion relation ill-posed?

Answer:

Generally speaking the problem is indeed ill-posed. However, if one is looking only for the first mode (most important for remote sensing) and using the Phillips-Miles dispersion relationship (sharp thermocline), it can be proven that the inversion is unique. Further discussions on the uniqueness aspects of the inverse problem can be found in "Inverse Storm-Liouville Problems" by B.M. Levitan, Science Press (1987).

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Discussion Record

Session: VIII

Title: Resistance and Lifting Force of A Submerged Body with A Wing Producing A Downward Lifting Force

Presenter: K-h Mori

Discussant: S.W. Song

Question:

Why do the wings reduce the wave-making resistances?

Answer:

It is the point we are studying now. Physically, as seen in Fig. 3, the wave elevation is reduced by the interaction between the waves generated by the main hull and the wing. In our case, the upper side of the wing is the pressure side. This situation may have contributed to reduce the resistance.

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Discussion Record

Session: XI

Title: Deformable Floating Bodies

Presenter: J.N. Newman

Discussant: D.V. Evans

Question:

If one wishes to solve for a body in a channel then one should do just that. Of course some characteristics can be modelled reasonably by a finite number of images but one cannot be sure beforehand which those are. Certainly sway added mass and, according to Phil McIver's contribution, second order drift force, cannot. Currently truncated circular cylinders in channels can be handled extremely efficiently using rapidly convergent multiple potentials which, in addition, model the far-field behaviour exactly unlike any finite combination of images, however large. For general bodies a modified Green's function is needed which is known and, in principle, could be used in an appropriate panel method. The initial investment would of course be considerable but, once completed, would provide an invaluable tool for such problems.

Answer:

From the scientific viewpoint I certainly agree that your multiple potentials are more exact, and elegant. Most importantly, the results based on that approach elucidate the roles of both the cut-off and trapped-mode frequencies, which are not precise in my computations. But if we need to consider general bodies, with panel methods and free-surface Green functions, then the choice is between Professor Kashiwagi's approach and my own.

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Deformable Floating Bodies

You propose an alternative which would combine the best features of both at the expense of extra work, namely to develop fast algorithms for the free-surface Green function in a channel. It is not clear to me how feasible this is, at least in the context of multivariate polynomial approximations, since an extra parameter is involved in the approximations (i.e. the channel width). Yet another possibility (suggested by Professor Tulin in oral discussion) is to use Rankine sources in the relatively confined computational domain of the channel, but far-field closure may be problematic there too in the vicinity of cut-off frequencies.

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Session: XI

Title: Deformable Floating Bodies

Presenter: J.N. Newman

Discussant: M. Kashiwagi

Question:

I have worked on a channel problem by use of the 3-D panel method, a couple of years ago. At that time I used a closed form of the Green function satisfying the side-wall boundary condition instead of worrying about the convergence of infinite number of mirror-image effects. The computation time was not so large at least in the low frequency range. So I think the above-mentioned method still deserves a try for improvement.

Answer:

In our panel code special algorithms are used for the free-surface Green function, as described in "Wave Asymptotics" (proceedings of the Ursell retirement meeting). One can of course use the integral representation of the Green function which satisfies the channel boundary conditions analytically, and evaluate this by numerical integration. So the choice is between fast evaluation of the Green function and extra panels for the images, or a more complicated Green function evaluation with panels only on the central body. We have shown that the first approach works, but it certainly is not the only possibility.

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Discussion Record

Session: XI

Title: Deformable Floating Bodies

Presenter: J.N. Newman

Discussant: W. Sulisz

Question:

Your solution is of current interest to structural engineers. The example for a column is interesting. I am wondering whether you have applied (how to apply?) your method to the structures of more complex geometry (a problem with eigenmodes).

Answer:

So far I have only applied my method to the two cases described in the talk, of a slender barge and a slender vertical column. I do not see any fundamental difficulties for more complex structures, except for the consideration of appropriate modal shape functions.

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Discussion Record

Session: IX

Title: Trapped Modes Above A Submerged Flat Plate

Presenter: N. Parsons

Discussant: D.V. Evans

Question:

Have you tested numerically whether there is agreement with the results of Linton & Evans (1991) when the depth is large?

Answer:

Yes. Comparison of our solution with your solution for large depth shows excellent agreement.

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Discussion Record

Session: IX

Title: Trapped Modes Above A Submerged Flat Plate

Presenter: N. Parsons

Discussant: M. McIver

Question:

Have you used your approximate formula for trapped mode frequencies to help you to locate the exact frequencies numerically?

Answer:

Yes. By fixing the geometry and the wavenumber K, the Wiener-Hopf solution gives an approximation for the longshore wavenumber ℓ. With this approximation as a starting point, the full numerical scheme converges rapidly to give the true value of ℓ. In fact, the approximate value of ℓ was found to be correct to three decimal places in nearly all cases.

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Discussion Record

Session:

Title: Time Domain Calculations of The Second-Order Drift Force on a Floating Object In Current and Waves

Presenter: H.J. Prins

Discussant: E. van Daalen

Question #1:

It is not clear to me why substitution of the boundary conditions into the matrix equation, and then integrating the resulting matrix equation in time, gives better results than "the other way around". Can you be more specific about the instabilities you encountered with the latter method in the case of a current?

Answer #1:

Integrating the free surface in time separately is a bit strange from the physical point of view. One separates the free surface from the rest of the fluid and disregards the interaction during a time step. By substitution the boundary equations into the matrix equation, this interaction is always taken care of. However, a method may look strange physically, but be correct mathematically. This is the reason why it has been applied thus far. The introduction of the forward speed, however, causes the eigen solutions of the boundary condition to be exponentially increasing. So, integrating the free surface separately is mathematically incorrect. Substitution of the condition into the matrix equation changes these eigen solutions into harmonic ones, resulting in an equation which is stable both analytically and numerically.

continued...

Question #2:

Which time-stepping method do you use?

Answer #2:

We used an implicit Euler scheme. Although it is implicit, this method remains rather simple because of the linearity of the equations we considered.

Question #3:

The negative values of the drift force at low frequencies are said to disappear once "the accuracy of the algorithm" is increased. Could you be more specific?

Answer #3:

With "the accuracy of the algorithm" I mean the accuracy of the mesh and the time step used in the calculations. It appeared that the negative values of the drift force were due to inaccurately calculated surge coefficients. This inaccuracy was mostly due to the inaccurate representation of the stagnation point near the body. The mesh consisted of a fixed number of points per wave length, so for long waves, the first element close to the body was too large to represent the stagnation point up to the accuracy required.

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Discussion Record

Session: X

Title: Resonances of The 2-D Neumann-Kelvin Problem

Presenter: J-M Quenez

Discussant: P. Martin

Question:

Have you actually computed any of these resonances? If not, do you intend to?

Answer:

We have not yet numerical results. We intend to obtain numerically that the maxima of the wave resistance be above the resonances due to the zeros of the transmission coefficient. The other resonances coming from the auxiliary problem do not interfere - (that is not yet proved).

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Discussion Record

Session: VIII

Title: On Uniqueness in Linearized Two-Dimensional Water-Wave Problem for Two Surface-piercing bodies

Presenter: M.J. Simon

Discussant: D.V. Evans

Question:

Fitzgerald has shown uniqueness for a class of bottom topographics in two dimensions by using a conformal mapping to map the fluid region into a uniform strip. In the new coordinates the new free surface condition contains the derivatives of the mapping so that the complication of the bottom topography is transferred to the free surface condition.

Presumably the same complication arises here after the transformation using the bipolar coordinates. Does it cause problems?

Comment: It is possible to produce an heuristic argument for non-uniqueness in two-dimensions for the case of two pairs of widely spaced, surface piercing vertical barriers. Evans and Harris (1972, J. Inst. Math. Applic.) showed that there exist particular spacings, depths and frequencies for which incident wave is totally reflected by a single pair of such barriers, so that $|R|=1$. By using a wide spacing approximation for two pairs of identical barriers a distance 2b apart, it is easy to show that the condition for a trapped mode between the barriers is just $R = \exp(2ikb)$ the solution of which gives the trapped mode solutions and correspond to a non-uniqueness of the problem.

It will be of interest (but difficult) to extend the technique of Levine & Rodenich to obtain the explicit solution to the yaw barrier problem to check out whether the above argument holds for the full linearized problem without approximation.

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On Uniqueness in Linearized Two-Dimensional Water-Wave Problem for Two Surface-piercing Bodies

Answer:

The derivative of the mapping occurs in two places:

a)

$$\nu \int_{F_-} |u|^2 \, dx = \nu \int_{-d_-}^{+d_+} |v(-\pi, \tau)|^2 \, \frac{a \, d\tau}{\cosh \tau + 1}$$

which can be bounded by:

$$\frac{1}{2} \nu a \int_{-d_-}^{+d_+} |v(-\pi, \tau)|^2 \, d\tau;$$

also:

b)

$$\nu a \int_{-d_-}^{d+} |v(o, \tau)|^2 dt$$

can be bounded by:

$$\nu (\cosh d_+ -1) \int_{-d_-}^{d_+} |v(o, \tau)|^2 \frac{a d\tau}{\cosh \tau - 1} = \nu (\cosh d_+ - 1) \int_{F_+^e} |u|^2 \, dx$$

In neither case does this present a problem.

See also Fitzgerald (1976) as is in Phil. Trans. Roy. Soc. London A284, 49-89. Further Comment: It would of course be of great interest if there could be demonstrated some example of non-uniqueness in 2-D. I feel that the four barrier problem is a good candidate for this, and is well worth studying.

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Session: VIII

Title: On Uniqueness in Linearized Two-Dimensional Water-Wave Problem for Two Surface-piercing Bodies

Presenter: M.J. Simon

Discussant: P. Martin

Question:

Can you extend your method to three dimensions, where the simplest problem (for which uniqueness has not been proved) would be a floating torus?

Answer:

This method (with toroidal coordinates) would almost certainly give a bound on the frequency of any non-uniqueness. It must be kept in mind however that John's proof is much more powerful in three dimensions, because of a) the connectedness of the free surface for individual bodies and b) the pointwise nature of the bound on the potential energy.

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Discussion Record

Session: VIII

Title: On Uniqueness in Linearized Two-Dimensional Water-Wave Problem for Two Surface-piercing Bodies

Presenter: M.J. Simon

Discussant: W. Sulisz

Question:

Do irregular or singular points effect the uniqueness?

Answer:

By irregular or singular points you mean points in the closure of the fluid domain at which the potential has a singularity. Such points are usually the body/free-surface intersections or the body corners. The method presented here does not seem to be affected by such points.

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Discussion Record

Session: V

Title: Second-Order Solution for Semi-Submerged Horizontal Rectangular Cylinder

Presenter: W. Sulisz

Discussant: S. Grilli

Question:

I have a, maybe naive question about your perturbation expansion. In my mind, higher order terms should be seen as smaller and smaller corrections to lower-order terms, in a converging power series expansion.

You have shown us a case for which second order contributions are on the same order, or even larger than lower-order contributions (i.e. first order).

Does this mean that even higher-order terms - e.g. 3rd order -should be considered?

Can you comment on that?

Answer:

Experimental data indicates that, within the frame of the present approach, higher-order terms can be neglected ($k_1h>>1$).

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Discussion Record

Session: V

Title: The Numerical Simulation of Long Unstable Wave Trains Leading to Group Formation, Wave Deformation and Breaking

Presenter: M. Tulin

Discussant: Y. Cao

Question:

I am very impressed by the performance of the multi-subdomain approach for the waves in the long tank. It seems to me that the multi-subdomain approach may perform better than the multiple expansion approach of O (NlogN). The multi-subdomain approach is of $O(m^2N)$ where m is the code number for the subdomain. However, the multi-subdomain approach is more straight forward and does not require logic IF statements in the computer code, which is of great advantage. It would be interesting to see how the multi-subdomain approach performs as compared to the multiple expansion approach. Did you try or consider the multiple expansion approach? If yes, how the two approaches compare?

Answer:

Thank you for your comments. No, we did not consider the multiple expansion, which would seem more appropriate for a problem containing various scales.

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Discussion Record

Session: V

Title: The Numerical Simulation of Long Unstable Wave Trains Leading to Group Formation, Wave Deformation and Breaking

Presenter: M. Tulin

Discussant: S. Grilli

Question:

I first would like to return you some of your earlier compliments. I think no 2D nonlinear calculations has been pushed as far in terms of number of periods and modes.

Now, how do the group, generated in your numerical wave tank can be related to actual group measured in the ocean?

Have you thought about looking at shallow water cases for which somewhat more numerous data might be available!

When you peel off water from a wave that is close to breaking, how do you select the right kinematics on the modified free surface?

Answer:

Thank you very much for your kind compliment.

We are doing other work on wave groups using the nonlinear evolution equation (cubic Schroedinger Eq.) and we try to relate what we learn in LONGTANK to that analytical work.

Thank you for the shallow water suggestions; we should look into it. Very thin layers are peeled, and the previous kinematics is assumed.

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Session: V

Title: The Numerical Simulation of Long Unstable Wave Trains Leading to Group Formation, Wave Deformation and Breaking

Presenter: M. Tulin

Discussant: J.N. Newman

Question:

Group velocity is a concept which I associate with linear theory. How do you justify its role in connection with these highly nonlinear phenomena?

Answer:

I am only reporting observations in LONGTANK, and for the time being we can choose to ignore the correlations. Let me point out, however, that the amplitude envelope does move with the group velocity (as is clearly shown with Figure 3 of the Abstract), and we have shown analytically that when a wave with $u>c_g$ passes through the peak of the amplitude envelope (into the forward face of the group) the motion ceases to be kinematically compatible with the orbital motions required by the usual solution. This is very curious and may prove eventually to have something to do with our LONGTANK empirical observations.

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Session: V

Title: The Numerical Simulation of Long Unstable Wave Trains Leading to Group Formation, Wave Deformation and Breaking

Presenter: M. Tulin

Discussant: E. van Daalen

Question:

(About interface conditions)

Can you be a little bit more specific about the conditions imposed on the vertical interfaces in your LONGTANK - code?

Answer:

The potential and its normal derivative are required to be continuous across each vertical barrier.

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Discussion Record

Session:

Title: Time-marching schemes for ship motion simulations.

Presenter: T. Vada

Discussant:

Question 1: (T. Miloh)

Can you explain why you chose to include the viscous dissipation effects in your <u>kinematic</u> rather that the dynamic free surface condition? I believe that indirectly you are using Lighthill's method of replacing ϕ by $\phi e^{\mu t}$ and letting $\mu \rightarrow 0$. However, at least to my understanding, it is more natural to incorporate the viscous term in the dynamic boundary condition and interpret it as a pressure jump across an air-water (viscous) interface.

Question 2: (Y. Cao)

You propose to add a damping term to the free surface kinematic condition (rather than the dynamic condition) to reduce the non-physical wave reflection from the truncation boundary. It is known that the kinematic condition states that the fluid particles on the free surface always remain on the free surface. Adding extra terms to the kinematic condition seems to violate the physical law. In other words, the fluid particles on the free surface may leave the surface. Any comments?

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Time-marching Schemes for Ship Motion Simulations

Answer to questions 1 and 2:

The free surface boundary condition is

$$\frac{\partial \eta}{\partial t}+U\frac{\partial \eta}{\partial x}=\frac{\partial \phi}{\partial z}-2\nu\eta+\frac{\nu^2}{g}\phi$$

where

$$\nu(\rho)=3\frac{C_S}{C_w^3}(\rho-\rho_0)^2$$

when $\rho > \rho_0$ and $\nu=0$ when $\rho < \rho_0$. ρ is the distance from the source. ρ_0 is the inner boundary of the "beach".

The design and implementation of the "artificial" absorbing beach was not discussed in the abstract although it is an integral part of the proposed numerical solution algorithm. We thank the discussers for allowing us the opportunity to briefly add some comments on the subject.

The absorbing device is based on the so called Newtonian cooling (cf. Israeli & Orszag, 1981) and its details are discussed in Nakos (1993). Indeed, due to the additional terms in the kinematic free surface condition, there exists a mass flux through the free surface which is directed outside or inside the fluid domain depending on whether the wave elevation is positive or negative. This "spurious" mass flux may be interpreted as a wave-damping mechanism, a fact that is easily verified by the resulting modified dispersion relation.

Exactly the same wave-absorbing effect may be obtained by appropriately modifying the dynamic free surface condition to allow a pressure jump across the air-fluid interface. The overall effect of both damping schemes is identical and may be shown to be equivalent to the well-known Rayleigh viscosity.

The proposed solution algorithm for the time integration of the free surface treats the kinematic condition explicitly, therefore, it is algorithmically more convenient to adopt the Newtonian cooling alternative.

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Time-marching Schemes for Ship Motion Simulations

Question 3: (A. Clément)

A lot of computer simulations of non-linear (or linear) water waves uses Runge-Kutta schemes which allow for time varying steps. This degree of freedom is generally used to relate the value of the time step to some kinematic feature of the flow (e.g. acceleration of the free surface). Did you definitely eliminate this choice in your study of time schemes?

Answer:

An important point in our decisions on which schemes to use was to minimize the number of function evaluations, since such an evaluation involves the solution of the full 3D problem at some given time. We thus decided first to try multistep-formulas. Since we succeeded in finding such formulas with satisfactory properties (at least from what we have seen until now) we decided not to proceed with studies of Runge-Kutta schemes. Our first order scheme will also allow a varying time step, which may indeed be very useful when we go to the non-linear problem.

If we find out at some later stage that our schemes do not work satisfactorily we have the Runge-Kutta schemes as a "backup", but as already mentioned we have not seen any need for this so far.

Question 4: (R. Jeffreys)

Is the filtering applied after the solution is complete or is it part of the integration scheme? If the latter, would inclusion in the analysis of the integration schemes alter your assessment of them?

Answer:

The time filtering needed with the TLF-scheme is a part of the integration scheme and is as such fully integrated in the analysis of the schemes. The effect of the filter is to increase the critical value of β by about 5%. The accuracy of the dispersion relation is in fact slightly improved with the filter included (cf.figure 2).

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Time-marching Schemes for Ship Motion Simulations

Answer Contd...

The space filtering is not included in the analysis. The effect of this is almost certainly small and it would affect both schemes in approximately the same way since it is not related to the time stepping. Thus this filtering will not affect the comparison of the two schemes.

REFERENCE

Israeli, M. & Orszag, S.A.: "Approximation of Radiation Boundary Conditions" J. of Comp. Phys., Vol. 41, 1981

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Discussion Record

Session: II

Title: A Hamiltonian Formulation for Nonlinear Wave-Body Interactions

Presenter: E.F.G. van Daalen

Discussant: T. Miloh

Question:

I want to congratulate you for your presentation of this elegant piece of work which has been proven to be very useful in nonlinear water waves analysis. However, I will urge you to pursue the application of this Hamiltonian formulation for the case of ship motion, in order to get a more concise derivation of the equations of motion in terms of the forward-speed dependent coefficients (added mass & damping). I strongly believe in the Hamiltonian & Lagrangian formulation and would like to see more applications of this powerful method in the fields of naval architecture and ocean engineering.

Answer:

Thank you for your nice comment. Indeed, the next step in this research is to exploit this hamiltonian formulation, firstly to obtain stable and accurate (in an "energy" sense) discretizations for ship-wave interactions, and secondly to derive the equations of motion in terms of added mass and damping coefficients. I would like to refer to your JSR - paper of 1984 (see reference list) and a JFM-paper (1989) by Aranha and Pesce, wherein these hydrodynamic coefficients are expressed as "stationery values of well-defined functionals".

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Discussion Record

Session: IX

Title: Simulation of Nonlinear Irregular Waves by Green-Naghdi Theory

Presenter: Qi Xu

Discussant: J.V. Wehausen

Question (Comment):

I welcome the entry of another group to the small number trying to exploit the approach developed by A.E. Green and P.M. Naghdi for approximation of the equations describing certain water-wave problems. We are so accustomed to using some form of perturbation method to treat these problems, and to being attentive to consistency in doing so, that we overlook other possibilities of approaching such problems. In the GN approach there is no perturbation parameter. The field equations are approximated, but all boundary conditions are satisfied exactly. Moreover, all invariance requirements and all conservation laws, some averaged over depth, are also satisfied. Consequently, at any level of approximation of the field equations, the equations are not just an approximation devised for a particular problem, but are model equations for a particular class of fluid flows, in somewhat the same way that Euler's equations, the Navier-Stokes equations, and equations for non-Newtonian fluids are model equations for fluid motion. In judging the applicability of one of the GN sets of equations, it is not a question of the size of a perturbation parameter but of whether the equations provide a good model for the considered flow. The criteria are really quite different. One consequence of the principles upon which the GN equations are based is that physically unrealistic predictions are hardly likely to occur. Although one would like to think of the various levels in the GN equations as being successive approximations to, say, the Euler equations, no convergence theorems have been proved, as far as I am aware (not unlike the situation with most perturbation series). On the other hand, in several cases numerical evidence does seem to indicate convergence.

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Simulation of Nonlinear Irregular Waves by Green-Naghdi Theory

What happens if one does introduce a perturbation parameter into the GN equations at some level? Ertekin has done this in his dissertation (UC Berkeley, 1984, ch.4) for level 1. He found that terms through order 4 in the parameter were present in the GN equations, not all terms of order 4, but those necessary for satisfying the various requirements of the GN theory. If terms of order 2 and higher are deleted, one obtains the simpler generalized Boussinesq equations of T.Y. Wu, but loses those qualities of the GN equations that justify calling them model equations.

One aspect of GN theory that seems forbidding at first glance is the complication of the higher-level equations. But these can now be worked out with a computer program, as can, of course, higher-order perturbation series. For a discussion of this one should consult the paper of Webster and Shields cited by the authors. This paper also contains a very readable introduction to the GN theory.

Answer:

We appreciate the comments by Professor Wehausen. We consider the GN formulation of numerical solution to the problem of water-wave propagation as very promising from the practical point of view. The work presented here is exploratory. We probe the application potential of the GN model. We intend to continue the work until well developed application algorithms are formulated.

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Discussion Record

Session:	IV
Title:	Unsteady Axisymmetric Flow over a submerged Sink
Presenter:	M. Xue
Discussant:	T. Miloh

Question:

I enjoyed seeing a follow-up of a work presented at the previous workshop. I also wanted to draw your attention that the critical value for the free-surface collapse which was suggested by Lubin (JFM 1867) is around 0.2 which seems to be in perfect agreement, with your corresponding value found from numerical simulations. Lubin's value is supported by some experiments and a very crude application of the Bernoulli theorem, which amazingly enough results in a critical value close to 02. Maybe you could comment on the physical justifications for using this method. Finally, I would like to mention that our paper on the point sink, has just appeared in the Physics of Fluids A.

Answer:

Our simulations of the super-critical case show that the dip forms very rapidly resulting in a dividing streamline closely aligned with the horizontal level at the sink location. These observations support the quasi-steady assumptions of Lubin & Springer (1967). They justified their analysis upon experimental observations that immediately prior to the formation of the dip the draining flow was almost steady. Their crude empirical/heuristic prediction gives a value of the critical Froude number (our definition) of $\mathcal{F} = (2^7/5^5)^{1/2} \cong 0.202$, which, as pointed out by Professor Miloh, is in close agreement to our critical value of $\mathcal{F}_2 \cong 0.193$.

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Discussion Record

Session: IX

Title: The Shell Functions: A Global Method for Computing Time-Dependent Free-Surface Flows

Presenter: R.W. Yeung

Discussions

Discusser: S.T. Grilli

The shell function method is very elegant and efficient way of solving unsteady potential problems. My question is about the way you are solving the interior problem: simple Rankine sources coupled to a time stepping scheme have proved very efficient for linear and nonlinear problems. However, they likely require much smaller time steps than the method using the unsteady Green's function. Have you compared those two methods for solving the interior problem from the computational and accuracy point of view?

Author's reply:

Once a shell method is established as we proved, the only viable solution in the interior domain is a simple (Rankine) source formulation as used here. Of course, one can also use field discretization methods such as finite difference and finite elements. But these methods are not as simple to implement as boundary integral methods.

Discusser: D.K.P.Yue

This idea to treat open boundaries is identical to a matching scheme which has been made popular in the 1970's by a number of investigators including Professor Yeung yourself. For fully-nonlinear free surface interactions in the interior, we have employed this technique with some success beginning with vertically axisymmetric flows (Lin,Newman,Yue, 1984; Dommermuth, Yue, 1987). I would like to share the following observations from this experience:

(1) for fully nonlinear simulations, the computational effort associated with the interior (Rankine) part typically overwhelms the linear free surface 'shell' calculations.

(2) we have not been completely successful in calculating the solution (say the free surface elevation) close to the "shell" (although the matching is otherwise satisfactory). Do you have similar experiences or do you have a special computational technique to overcome this?

Answer:

The requirement of matching of ϕ pressure or tangential velocity) and ϕ_n (normal velocity) between two inviscid flow fields in order to ensure solution continuity is known for a long time (see e.g. Bartholomeusz, Proc. Camb. Phil. Soc., 1958). Nestegard and Sclavounos (1984) generalized such a concept to a control surface with ϕ and ϕ_n related in a matrix rather than point-wise sense. The work of Lin et al. (15th ONR symp., 1984), utilizing a line source distribution, could not be easily extended to non-axisymmetric body geometry. In a discussion of the Lin et al. paper at the time, we had cautioned the possible incompatibility of the wave elevation between nonlinear and linear solutions. This difficulty, as reported now again by the discusser, is still to be resolved.

It is important to recognize that the present formulation is more than matching. An essential element of our so-called "shell function method", as initially described in Yeung (1985), is that the information on the shell can be pre-computed once for all and reused. The potential of this approach is only finally validated in the present work. In nonlinear internal computations such as those used by the discusser himself, W.M. Lin and I.Y Gong in these workshop proceedings, much efforts can be saved if the shell-function idea is adopted.

It is presumably possible to have configurations where the internal flow computation effort would dominate but the point is that the shell computation need to be done only once and this is regardless of the internal geometry one has to investigate. Further, the memory integral computations grow like k^2 (k being the number of time steps) whereas the internal computations grow like k. For long-time computations, memory effect will eventually dominate, unless a pre-computation is done as proposed here.

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Discussion Record

Session:	VIII
Title:	A 3-D Panel Method for Free-Surface Flows around Ships at Small Angles of Yaw
Presenter:	Z.J. Zou
Discussant:	J.N. Newman

Question:

This is an impressive piece of work, with application to ship manoeuvring. However, the forces on a ship in a typical manoeuvre may be affected by viscosity and separation along the hull. Can you comment on the role of potential flow here? A related issue in my mind is the extent to which the Kutta condition is applicable at the stern, particularly for practical ships. Perhaps the Kutta condition should only be imposed over part of the draft, or in some "relaxed" manner?

Answer:

Certainly forces connected with flow separation are very important for a manoeuvring ship. Therefore, to the forces determined in potential flow a cross-flow drag of ship sections is added for practical applications. The latter is estimated by using empirical coefficients.

The lifting potential-flow model described here is applicable to the flows where full or partial separation along the length of hull may occur, the Kutta condition is indeed questionable. In these situations vortex separation model or viscous flow model will be required.