Discussions of the 15th International Workshop on Water Waves and Floating Bodies

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Contents

1 Agnon, Y.[1] and Bingham, H.B.[2]. *A Fourier-Boussinesq method for non-linear wave propagation on a variable depth fluid*

[1] *Technion, Haifa, Israel.*
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[1] *agnon@technion.ac.il,* [2] *hbb@imm.dtu.dk* **8**

1.1 Discusser L.J. Doctors, l.doctors@unsw.edu.au 8

2 Akylas, T. *Supercritical wakes in stratified flows*

Department of Mechanical Engineering,Massachusetts Institute of Technology, Cambridge, MA 02139, USA.
trakylas@mit.edu **9**

2.1 Discusser J. Grue, johng@math.uio.no 9
2.2 Discusser W.W. Schultz, schultz@engin.umich.edu 9

3 Borthwick, A.G.L., Turnbull, M.S. and Taylor, R.E. *Nonlinear wave loading using sigma-transformed and unstructured finite element meshing*

Department of Engineering Science, Oxford University, Parks Road, Oxford OX1, 3PJ, U.K.
alistair.borthwick@eng.ox.ac.uk **10**

3.1 Discusser H. van Brummelen, Harald.van.Brummelen@cwi.nl 10
3.2 Discusser J. Chaplin, J.R.Chaplin@soton.ac.uk 10
3.3 Discusser J.N. Newman, jnn@mit.edu 10
3.4 Discusser W.W. Schultz, schultz@engin.umich.edu 11

4 Brummelen H. van[1] and Raven, H.[2] *Numerical solution of steady state free surface Navier-Stokes flow*

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Harald.van.Brummelen@cwi.nl **12**

4.1 Discusser K-H. Mori, kmori@ipc.hiroshima-u.ac.jp 12
4.2 Discusser E.O. Tuck, etuck@maths.adelaide.edu.au 12
4.3 Discusser R.W. Yeung, rwyeung@socrates.berkeley.edu 12

5 Chen, X-B. *Peculiar properties of ship-motion Green functions in water of finite depth*

Bureau Veritas, DTA, 17bis, Place des Reflets, 92400 Courbevoi, France
xiao-bo.chen@bureauveritas.com **14**

5.1 Discusser S.D. Sharma, sharma@nav.uni-duisburg.de 14

6 Clammond, G.[1] and Grue, J.[2] *Dynamics of the transient leading part of a wave train*

Mechanics Division, Department of mathematics, University of Oslo, Norway
[1] *didier@math.uio.no,* [2] *johng@math.uio.no* **15**

6.1 Discusser M.W. Dingemans, maarten.dingemans@wldelft.nl 15

6.2 Discusser R.C.T. Rainey, rctrainey@wsatkins.co.uk 15

7 Daalen, E.F.G. van[1], Gerrits, J.[2], Loots, G.E.[2] and Veldman, A.E.P.[2] *Pressure surface anti-roll tank simulations with a volume of fluid based Navier-Stokes solver*

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[1] *e.f.g.van.daalen@marin.nd* **16**

7.1 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk 16

8 Doctors, L.J.[1] and Day, A.H.[2] *The squat of a vessel with a transom stern*

The University of South Wales, Sydney, NSW 2052, Australia

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[1] *l.doctors@unsw.edu.au* **18**

8.1 Discusser E.O. Tuck, etuck@maths.adelaide.edu.au 18

8.2 Discusser S.D. Sharma, Sharma@nav.uni-duisburg.de 18

9 Drimer, N.,[1] Glozman, M., Stiassnie, M. and G. Zilman. *Forecasting the motion of berthed ships in harbour*

CAMERI-Coastal and Marine Eng. Res. Inst., Technion City, Haifa 3200, Israel.

nitay@cameri2.technion.ac.il **20**

9.1 Discusser H.B. Bingham, hbb@imm.dtu.dk 20

9.2 Discusser M.W. Dingemans, maarten.dingemans@wldelft.nl 20

10 Evans, D.V. and Shipway, B.B. *A continuum model for multi-column structures in waves*

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D.V.Evans@bris.ac.uk, B.Shipway@bristol.ac.uk **21**

10.1 Discusser C.M. Linton, C.M.Linton@lboro.ac.uk 21

10.2 Discusser T. Miloh , miloh@eng.tau.ac.il 21

10.3 Discusser B. Molin, molin@esim.imt-mrs.fr 21

10.4 Discusser J.N. Newman, jnn@mit.edu 21

11 Greco, M.[1], Faltinsen, O.[1] and Landrini, M.[2] *An investigation of water on deck phenomena*

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[2] *maulan@waves.insean.it* **22**

11.1 Discusser J. Grue, johng@math.uio.no 22

12 Grue, J.[1], Jensen, A.[2], Rusås, P-O and Sveen, J.K.[3] *Solitary waves in stratified fluid: modelling and experiments*
Mechanics Division, Department of Mathematics, University of Oslo, Norway.
[1]*johng@math.uio.no,* [2]*atlej@math.uio.no,* [3]*jks@math.uio.no* **23**
12.1 Discusser T. Miloh, miloh@eng.tau.ac.il 23
12.2 Discusser M.P Tulin, mpt@vortex.ucsb.edu 23

13 Hamilton, M.[1] and Yeung, R.W.[2] *Non-linear motion of a submerged body in waves*
Department of Mechanical Engineering, University of California at Berkeley, Berkeley, CA 94720, USA.
[1]*rwyeung@socrates.berkeley.edu,* [2]*andy@stokes.oe.berkeley.edu* **24**
13.1 Discusser J.N. Newman, jnn@mit.edu 24

14 Huseby, M.,[1] Jensen, A.[2] and Grue, J.[3] *An experimental investigation of ringing loads on a vertical cylinder in transient waves*
Mechanical Division, Department of Mathematics, University of Oslo, Norway.
[1]*mhuseby@math.uio.no,* [2]*aytlej@math.uio.no,* [3]*johng@math.uio.no* **25**
14.1 Discusser J. Chaplin, J.R.Chaplin@soton.ac.uk 25
14.2 Discusser B. Molin, molin@esim.imt-mrs.fr 25
14.3 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk 25

15 Iafrati, A.[1] and Korobkin, A.A.[2] *Liquid flow close to intersection point*
[1]*INSEAN-Italian Ship Model Basin–Rome, Italy.*
[2]*Lavrentyev Institute of Hydrodynamics–Novosibirsk, Russia.*
[1]*a.iafrati@insean.it,* [2]*KAA@hydro.nsc.ru* **26**
15.1 Discusser E. Fontaine, emmanuel@engineering.ucsb.edu 26

16 Iwashita, H. *On unsteady waves generated by a blunt ship with forward speed*
Engineering Systems, Hiroshima University, 1-4-1 Kagamiyama, Higashi-Hiroshoma, 739-8527, Japan.
iwashita@naoe.hiroshima-u.ac.jp **29**
16.1 Discusser W.W. Schultz, schultz@engin.umich.edu 29
16.2 Discusser R. W. Yeung, rwyeung@socrates.berkeley.edu 29

17 Jiang, T.[1] and Henn, R.[2] *Nonlinear waves generated by a surface piercing body using a unified shallow-water theory*
Institute of Ship Technology, Mercator University, Germany.
[1]*jiang@unidui.uni-duisburg.de,* [2]*rupert@unidui.uni-duisburg.de* **30**
17.1 Discusser H.B. Bingham, hbb@imm.dtu.dk 30
17.2 Discusser M.W. Dingemans, maarten.dingemans@wldelft.nl 30
17.3 Discusser R.W. Yeung, rwyeung@socrates.berkeley.edu 30

18 Kashiwagi, M. ***Wave interactions with a multitude of floating cylinders***
Research Institute for Applied Mechanics, Kyushu University, 6-1 Kasuga-koen, Kasuga-city, Fukuoka 816-8580, Japan.
kashi@riam.kyushu-u.ac.jp **32**
18.1 Discusser R.F. Beck, rbeck@engin.umich.edu 32
18.2 Discusser D.V. Evans, D.V.Evans@bris.ac.uk 32
18.3 Discusser J.N. Newman, jnn@mit.edu 33

19 Lin, H.J. and Perlin, M. ***The velocity and vorticity fields beneath gravity -capillary waves exhibiting parasitic ripples***
Department of Naval Architecture and Marine Engineering, University of Michigan, Ann Arbor, MI 48109-2125, USA.
perlin@umich.edu **34**
19.1 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk 34

20 Molin, B. ***On the sloshing modes in moonpools, or the dispersion equation for progressive waves in a channel through the ice sheet***
Ecole Superieure d'Ingenieurs de Marseille, 13451 Marseille cedex 20, France.
molin@esim.imt-mrs.fr **35**
20.1 Discusser D.V. Evans, D.V.Evans@bris.ac.uk 35
20.2 Discusser J.N. Newman, jnn@mit.edu 35

21 Mori, K. and Nagaya, S. ***Wave making resistance of a submerged hydrofoil with downward force***
Hiroshima University, 1-4-1 Kagamiyama, Higashi–Hiroshima 739-8527, Japan.
kmori@ipc.hiroshima-u.ac.jp **36**
21.1 Discusser E.O. Tuck, etuck@maths.adelaide.edu.au 36
21.2 Discusser M.P. Tulin, mpt@vortex.ucsb.edu 36

22 Newman, J.N. ***Diffraction of water waves by an air chamber***
MIT, Cambridge, MA 02139, USA.
jnn@mit.edu **37**
22.1 Discusser C.N. Linton, C.M.Linton@lboro.ac.uk 37
22.2 Discusser S. Malenica, sime.malenica@bureauveritas.com 37
22.3 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk 38
22.4 Discusser R.C.T. Rainey, rctrainey@wsatkins.co.uk 38

23 Ohkusu, M. ***Analysis of wave force on a large and thin floating platform***
Research Institute for Applied Mechanics, Kyushu University, 6-1 Kasuga-ko25000i70.00010TDeny,-24000Kasugacsity,-24

[1] *Laboratory of Hydrophysics and Nonlinear Acoustics, Institute of Applied Physics, 46 Ulianov Str., 603600 Nizhny Novgorod, Russia.*
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24.1 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk 40

25 Retzler, C.H.[1], Chaplin, J.R.[2] and Rainey, R.C.T.[3] *Transient motion of a vertical cylinder: measurements and computation of the free surface*
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[1] *c.h.retzler@soton.ac.uk,* [2] *J.R.Chaplin@soton.ac.uk,* [3] *rctrainey@wsatkins.co.uk* **41**
25.1 Discusser J. Grue, johng@math.uio.no 41
25.2 Discusser B. Molin, molin@esim.imt-mrs.fr 41

26 Rognebakke O.F.[1] and Faltinsen, O.M.[2] *Damping of sloshing waves due to tank roof impact*
Department of Marine Hydrodynamics, Norwegian Institute of Science and Technology, N-7491 Trondheim, Norway.
[1] *olarogn@marin.ntnu.no,* [2] *oddfal@marin.ntnu.no* **42**
26.1 Discusser E.O. Tuck, etuck@maths.adelaide.edu.au 42

27 Shemer, L., Jiao, L.H. and Kit, E. *Nonlinear wave group evolution in deep and intermediate -depth water: experimental and numerical simulations*
Department of Fluid Mechanics, Faculty of Engineering, Tel-Aviv University, Tel-Aviv 69978, Israel.
shemer@eng.tau.ac.il, kit@eng.tau.ac.il **43**
27.1 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk 43

28 Subramani, A.K. and Beck, R.F. *Suppression of wave breaking in non-linear water wave computations including forward speed*
University of Michigan, Ann Arbor, Michigan 48109, USA
aks@umich.edu, rbeck@engin.umich.edu, **44**
28.1 Discusser H. van Brummelen, Harald.van.Brummelen@cwi.nl 44
28.2 Discusser M. W. Dingemans, maarten.dingemans@wldelft.nl 44
28.3 Discusser J.-M. Vanden-Broeck, J.Vanden-broeck@uea.ac.uk 44

29 E.O. Tuck. *Numerical solution for unsteady two-dimensional free surface flows*
Department of Applied Mathematics, The University of Adelaide, Australia 5005.
etuck@maths.adelaide.edu.au **46**
29.1 Discusser R.F. Beck, rbeck@engin.umich.edu 46
29.2 Discusser W.W. Schultz, schultz@engin.umich.edu 46
29.3 Discusser S.D. Sharma, sharma@nav.uni-duisburg.de 46
29.4 Discusser J.-M. Vanden-Broeck, J.Vanden-broeck@uea.ac.uk 47

30 Simon, M.J.[1] and Kuznetsov, N.G.[2] *On the uniqueness of water-wave problems for submerged cylinders in oblique waves*

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30.1 Discusser C.M. Linton, C.M.Linton@lboro.ac.uk 48

30.2 Discusser S.D. Sharma, sharma@nav.uni-duisburg.de 48

31 Ursell, F. *The metacentre in stability of ships. Some difficulties*

Department of Mathematics, Manchester University, M13 9PL, UK.

fritz@fs2.ma.man.ac.uk **49**

31.1 Discusser J.N. Newman, jnn@mit.edu 49

31.2 Discusser H. Nowacki, Horst.Nowacki@ism.tu-berlin.de 49

31.3 Discusser R.C.T. Rainey, rctrainey@wsatkins.co.uk 50

31.4 Discusser W.W. Schultz, schultz@engin.umich.edu 51

32 Vanden-Broeck, J.-M.[1] and Spivak, B.[2] *Free-surface damping due to viscosity and surfactants*

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32.1 Discusser L.J. Doctors, l.doctors@unsw.edu.au 52

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32.4 R.W. Yeung, rwyeung@socrates.berkeley.edu 52

1 Agnon, Y.[1] and Bingham, H.B.[2]. *A Fourier-Boussinesq method for nonlinear wave propagation on a variable depth fluid*

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Question/commenta

2 Akylas, T. *Supercritical wakes in stratified flows*

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2.1 Discusser J. Grue, johng@math.uio.no

Question/comment. Can you model support upstream solitary waves, for slightly supercritical flow?

Author's reply. While the present investigation has focussed on steady flows, the model can be extended to study upstream solitary waves near critical conditions in a laterally bounded flow.

2.2 Discusser W.W. Schultz, schultz@engin.umich.edu

Question/comment. What is the aspect ratio of the island in your photo and how important is it that it be small in interpreting the wave features?

Author's reply. In developing the 3-D nonlinear theory, it is assumed that the topography aspect ratio (stream-wise length-scale divided by span-wise length-scale) is small. The geometry of the induced wave pattern in the far field turns out to be independent of the aspect ratio, however. This allows to make comparison with observed wave patterns, such as the one induced by the island of Jan Mayer, without restriction on the aspect ratio. On the other hand, the amplitudes of the wave patterns depend on the topography aspect ratio.

3 Borthwick, A.G.L., Turnbull, M.S. and Taylor, R.E. *Nonlinear wave loading using sigma-transformed and unstructured finite element meshing*

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3.1 Discusser H. van Brummelen, Harald.van.Brummelen@cwi.nl

Question/comment

1. How does the bandwidth of the matrix grow with the number of mesh points?
2. Remark. In order to improve the efficiency of the method with decreasing mesh width, the Gauss-elimination should be replaced by an iterative solver, e.g., GMRES and multi-grid.

Author's reply.

1. For the unstructured finite element meshes, the matrix bandwidth is limited by nodal reordering. It should be noted that Wu and Eatock Taylor considered matrix bandwidth versus total number of nodes, and found their direct solver to be more efficient than the corresponding boundary element one.
2. The authors agree. In a separate study Ma (1999) developed a matrix solver for the structured-mesh finite element model based on the preconditioned conjugate gradient technique. It is intended to utilize this type of iterative solver in future applications of the present model.

Reference. Ma, Q.W. (1999), Ph.D. Thesis. *Dept. of Mech. Engng. University College*, London, UK.

3.2 Discusser J. Chaplin, J.R.Chaplin@soton.ac.uk

Comment. It is worth adding that the problem of a horizontal cylinder beneath waves is one that is surprisingly influenced by viscosity. In some cases comparisons with some features of measurements ought not to produce good agreement.

3.3 Discusser J.N. Newman, jnn@mit.edu

Question/comment. Please elaborate on how the grid refinement was required (or time steps decreased) to analyze the submerged cylinder?

Author's reply. For the submerged horizontal cylinder cases, the finite element meshes had 32, 64 and 128 nodes equally distributed on the cylinder boundary. Mesh convergence tests were undertaken, and the magnitude of the first-order force and the mean second-order force found to converge to within two significant figures. Poor mesh convergence was evident regarding the magnitude of the second-order oscillating force. This is being investigated using even finer meshes.

3.4 Discusser W.W. Schultz, schultz@engin.umich.edu

Question/comment. In your comparison to Ogilvie's first-order solution, do you know whether your computations are more or less accurate?

Author's reply. The computations give reasonably similar results to those of Ogilvie's theory for the first-order force magnitude, and are accurate up to two significant figures (for the cases considered). The authors do not believe that mesh convergence has been fully underway using finer meshes (with at least 128 nodes located on the cylinder surface).

4 Brummelen H. van[1] and Raven, H.[2] *Numerical solution of steady state free surface Navier-Stokes flow*

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4.1 Discusser K-H. Mori, kmori@ipc.hiroshima-u.ac.jp

Question/comment. You have taken into account the tangential component on the free-surface condition which is expected to play an important role. Could you show us how much has the consideration of the tangential component changed the final elevation ?

Author's reply. One can show that a change in the tangential dynamic conditions due to a disturbance in the free surface elevation depends on the gradient of the disturbance only. Hence, the tangential dynamics conditions essentially do not affect "smooth" paths of the wave.

The tangential conditions can have some effect on the elevations in the neighborhood of acute wave-crests if they are locally non-smooth.

In any case, to determine the effect of the tangential dynamics conditions one would have to make a comparison with the inviscid solution, i.e. with the solution of the associated Euler problem.

4.2 Discusser E.O. Tuck, etuck@maths.adelaide.edu.au

Question/comment. How do you justify neglecting viscous contributions to the normal stress on the free surface?

Author's reply. If the viscous contributions were included, the normal dynamic condition would read $p = p^a + \bar{n} \cdot \bar{\tau} \cdot \bar{n}$, with p the pressure, p^a the atmospheric pressure (both non-dimensionalized) $\bar{n}$ the unit normal vector to the free surface and $\bar{\tau}$ the viscous stress tensor with components $\tau_{ij} = Re^{-1}(\partial_j u^i + \partial_i u^j)$. For the applications in mind, ships-stern flows, the length scale is very large and $\mathrm{Re} = O(10^8)$. It is disregarded in comparison with p^a. However, the assumption is violated if $(\partial_j u^i + \partial_i u^j)$ becomes large, i.e., if the velocity field in the neighborhood of the free-surface becomes unsmooth. It is not yet clear how this relates to the large velocity gradients in the boundary layer near no-slip boundaries.

4.3 Discusser R.W. Yeung, rwyeung@socrates.berkeley.edu

Question/comment. You mentioned at the outset that you want only a steady-state solution. Unfortunately, in most applications of interest, such as flow about a ship, the solution around the bow may not be steady because of bow-wave oscillations (Grosenbaugh and Yeung, ONR Symposium, 1988 Den Haag) and generation of spray or splash (Yeung and

Ananthakrishnan, J. Engng. Math. 1998). Presumably, if one goes through all the trouble of solving Navier-Stokes equation, one would want to capture the correct flow physics. The steady-state flow solution assumption may be inappropriate.

Author's reply. Thank you for pointing this out. Currently our focus is the solution of ship-stern waves, because here viscous effects are most prominent. Hence, the unsteady phenomena at the bow are of no concern yet. In that case, the problems that you mention need to be addressed. It may be necessary to introduce time averaging, to arrive at a well-posed steady problem. Time averaging is already commonly applied in viscous flow computations due to the introduction of turbulence models.

5 Chen, X-B. *Peculiar properties of ship-motion Green functions in water of finite depth*

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5.1 Discusser S.D. Sharma, sharma@nav.uni-duisburg.de

Question/comment. Once again the author has demonstrated his super fast pace of research by extending his 1999 study of Green function in deep water to finite water depth within just six months. My next suggestion would be to include surface tension in the dynamic free-surface condition. It will be found that gravity waves of zero wavelength along with the associated singular behavior of the Green function are then cut off.

Author's reply. Thank you for your encouraging words and valuable suggestions. Indeed, if one includes the effect of surface tension in the dynamic free-surface condition, the dispersion relation is modified such that the open dispersion curves (existing when neglecting surface tension) become *closed* at large wave numbers. Associated far-field waves are no longer singular and high oscillations of small-wavelength waves are damped out. It should be interesting to look at it in detail and see how much advantage one may take in the numerical evaluation of source potential.

6 Clammond, G.[1] and Grue, J.[2] *Dynamics of the transient leading part of a wave train*

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6.1 Discusser M.W. Dingemans, maarten.dingemans@wldelft.nl

Question/comment. I noticed that you did not use higher-order wave-board steering. This has a consequence that the generated long wave in the plume is not corrected for. Can you estimate the influence of this long wave on your results?

Author's reply. We use a pneumatic wave-maker where parameters are tuned to maximize the energy transferred from the wave-maker to the wave. Hence, no long wave is generated. Of course, if arbitrary values of parameters are set, a long wave is generated and an amplified standing wave appears below the wave-maker.

6.2 Discusser R.C.T. Rainey, rctrainey@wsatkins.co.uk

Question/comment. Can you compute the particle trajectory for a surface particle, as the leading part of the wave train passes? Perhaps the particle motion is larger than in the latter waves, which would support the "flow separation theory" of the secondary loading cycle during "ringing".

Author's reply Yes. The scheme being fully nonlinear, all quantities are computed "exactly". To compute particle trajectories, it is sufficient to solve $u = dx/dt$, $v = dy/dt$. It can be implemented easily in the scheme.

7 Daalen, E.F.G. van[1], Gerrits, J.[2], Loots, G.E.[2] and Veldman, A.E.P.[2] *Pressure surface anti-roll tank simulations with a volume of fluid based Navier-Stokes solver*

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7.1 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk

Question/comments.

1. How was the turbulence and the surface disturbance it caused modeled?
2. How did the results compare with those obtained by using the shallow water model?

Author's reply

1. Turbulence is not modeled explicitly in the computer simulations. For this type of anti-roll tank, the fluid dynamics seems to be dominated by inertia forces and viscous effects are expected to be of minor importance. This may not be true for free surface anti-roll tanks with different geometries, such as internal vertical plates with holes. Also, in the U-tube type anti-roll tank, viscous effects seem to be important, considering the flow in the narrow duct connecting the two wing tanks and the flow around the duct openings.
2. We assume that you refer to the analysis presented by Verhagen and Van Wijngaarden [1]. Their analytical approach towards this problem is based on a first order perturbation of the nonlinear shallow water equations, where the perturbation parameter involves the tank width, the roll amplitude and the water depth. We compared the analytical predictions for the roll moment amplitude and phase angle with the experimental and numerical results presented at the workshop, for a wide variety of tank and motion parameters [2]. The (maybe not so surprising) conclusion is that the analytical approach gives rather good results for nearly all parameter combinations. The agreement seems to be somewhat less when the roll amplitude increases and/or when the water depth decreases, which can be understood very well from the definition of the perturbation parameter [1]. Clearly, the analytical approach fails for tank geometries which differ significantly from the rectangular open container (e.g. tanks with internal obstructions, such as bottom bars, partially impermeable vertical plates etc.).

Nevertheless, for a first estimate (and within the limitations of shallow water theory, of course) the analytical approach is a good alternative to the computer simulations and experiments.

References

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8 Doctors, L.J.[1] and Day, A.H.[2] *The squat of a vessel with a transom stern*

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8.1 Discusser E.O. Tuck, etuck@maths.adelaide.edu.au

Question/comment. There is a paradox here, in that you say that it is *bad* to use experimental squat for that purpose. However, you get reasonable agreement between computed and experimentally observed squat, so perhaps there should be no difference between these two methods? Incidentally, there is also a "consistency" issue here. Formally squat is of second order in thinness of the ship and its effect "should" be small. If it is not small, it may still be acceptable to include it, but perhaps then there might be other second-order effects which should also be included.

Author's reply. The suggested paradox that sinkage and trim should be accounted for if they are computed, but not if they are measured, may be due to the fact that the resistance is very sensitive to the sinkage and trim. This was observed when examining the convergence process when setting the vessel in equilibrium. Often the resistance after first iteration (only) was worse than that based on no iteration at all.

The matter of consistency is understood by the author. Perhaps this can be explained by the fact that a thin-ship approach is being used to solve the problem for a vessel that is also shallow (or flat). It is suspected that including sinkage and trim for a thin ship in which the draft is of the same order as the length will indeed result in a small correction. On the other hand, for the current, slender vessel, the sinkage and trim represent a large change relative to the draft of the vessel.

8.2 Discusser S.D. Sharma, Sharma@nav.uni-duisburg.de

Question. Would you please clarify whether in what you call the "physical" approach the pressure integration was carried out only over the physical hull surface or also over the fictitious extension behind the transom?
Comments.

1. The reason why the use of measured trim and sinkage in calculating the flow by Michell's theory is counterproductive may be that in model experiments the sinkage is measured relative to the carriage, not relative to the surrounding water surface. In reality, most of the so-called sinkage is simply due to the mean local lowering of the free water surface around the ship. It does not really affect the wave-waking potency of the hull.

2. Of course, theory can generate also vertical forces on the ship without resort to vortices or doublets. By Lagally's theorem a source would experience a vertical force if there is a vertical perturbation velocity at its location.

Author's reply. In the so-called physical approach, the pressure integration is carried out only on the wetted surface of the hull, which changes as vessel's sinkage and trim are iterated. One might argue that the integration could also be carried out over surface of the hull and its fictitious extension, since the pressure over the latter is supposed to be zero. However, the existence of the singularity at the trailing edge, where the "rooster tail" occurs, is likely to cause a problem.

The comment about the use of measured sinkage and trim being counterproductive are accepted entirely. No doubt it would make more sense to also measure the free-surface profile and then compute the effective (relative) sinkage and trim for use in the calculations.

The second note about the generation of lift without the use of vortices or doublets is interesting. There still appears to be a difference between a fully-submerged body and a surface-piercing body in this regard.

9 Drimer, N.,[1] Glozman, M., Stiassnie, M. and G. Zilman. *Forecasting the motion of berthed ships in harbour*

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9.1 Discusser H.B. Bingham, hbb@imm.dtu.dk

Question/comment.

1. If you are only interested in forces and motions you can use the Haskind relations for the diffraction forcing.

2. I have used a similar technique for this problem and found that surge, sway and yaw motions are substantially over-predicted because wave radiation damping is very small at these low frequencies. Can you comment?

Author's reply. We estimated several mechanisms of surge damping including wave radiation damping, viscous damping, and fender friction. To our understanding, based on theoretical study and field observations the viscous damping is of the order of wave radiation damping. However, fenders friction is significant and may be the dominating mechanism of damping, especially, if wind (or wave drift) pushes the ship against the fenders. Apparently, any kind of damping may be accounted for in a time domain approach, probably using empirical coefficients. It is also possible to account for external damping by introducing equivalent damping coefficients to the linear (frequency domain) solution, as we currently apply in our system Sea-21.

9.2 Discusser M.W. Dingemans, maarten.dingemans@wldelft.nl

Question/comment. I understand that the amount of long waves at the harbour mouth, computed with the nonlinear model, is treated as to consist of free waves only for treatment in the harbour. Part of this long-wave portion consists of bound waves, giving different agitation in the harbour. I recognize that it is difficult to estimate the amount of bound or free wave energy in the total long-wave energy.

Author's reply. In the case of shoaling up to the harbour mouth, on variable depth, the usual notion of bound and free waves is not easily applied, since the nonlinear boundary value problem does not have a simple solution in terms of free and bound waves. The model we use takes into account the resonant interaction among a triad of free waves, and the bottom variation. This way, say, two short waves, generate a free wave at their difference frequency, which then excites waves in the harbour. The agitation inside the harbour due to the bound waves outside, is considered to be a bound wave itself. Since the short waves in the harbour are small, so is the bound wave associated with them.

10 Evans, D.V. and Shipway, B.B. *A continuum model for multi-column structures in waves*

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10.1 Discusser C.M. Linton, C.M.Linton@lboro.ac.uk

Question/comment. The homogenization theory is based on the approximation $ka \ll 1$. Have you checked the results (for either the trapped-mode or scattering problem) against calculations based on full linear theory to see how significant the restriction on ka is?

Author's reply. No, but Porter (unpublished) has computed trapped modes for an $N \times M$ array of circular cylinders and finds $N \times M$ trapped mode frequencies. It ought to be possible to let M, say, become large and compare the trapped modes with those obtained from the homogenization theory.

10.2 Discusser T. Miloh , miloh@eng.tau.ac.il

Comment. In my discussion to the paper "Water-wave propagation through an infinite array of cylindrical structures" by P.McIver presented at the 14th IWWWFB (see p. 69), I have suggested using homogenization schemes which treat the medium as a continuum. I am happy to see that this idea has been adopted here.

10.3 Discusser B. Molin, molin@esim.imt-mrs.fr

Question/comment. Could the added mass coefficients a_{11}, a_{22} be made complex to introduce some energy dissipation?

Author's reply. I am not sure how it would go through, although it appears a good idea in principle. It is possible the trapped modes would become leaky modes, for instance, by reference to equation 21 of the "Abstract".

10.4 Discusser J.N. Newman, jnn@mit.edu

Question/comment. Is it necessary to assume both k times the radius and spacing are small, or only the former?

Author's reply. I think as a reflection it is probably sufficient to assume $ka \ll 1$ only.

11 Greco, M.[1], Faltinsen, O.[1] and Landrini, M.[2] *An investigation of water on deck phenomena*

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11.1 Discusser J. Grue, johng@math.uio.no

Question/comment. Does a shallow water formulation of the flow on the deck seem relevant? Is it feasible to match a shallow water formulation to the flow conditions at the bow of the ship? If not, where are the obstacles or shortcomings?

Author's reply. In the case of dam-breaking like water on deck the shallow water model is proved to be efficient and accurate to capture the flow evolution along the deck. We have performed some numerical tests, where a shallow water type of computation has been initialized by BEM data. In particular, we saw that both the initial time of the shallow water simulation and the distance of the shallow water region have to be carefully selected to get a good agreement with the BEM solution, and these data depend on the particular incoming wave and on its interaction with the ship. These elements strongly effect the water shipping characteristics and severity and require the simulation of the exterior (deep water) flow.

These observations limit the practical use of the SWA and suggest that a good compromise for design purposes may be a method obtained by matching the shallow water approximation for the flow evolving along the deck, with an 'exact' method for the exterior flow. We have not studied this matching solution but we think it is a sensible thing to try, mostly for three-dimensional problems.

12 Grue, J.[1], Jensen, A.[2], Rusås, P-O and Sveen, J.K.[3] *Solitary waves in stratified fluid: modelling and experiments*

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12.1 Discusser T. Miloh, miloh@eng.tau.ac.il

Question/comment. The weakly non-linear integro-differential equation which you presented is very similar to the Joseph-Kobuta equation. Can you please comment on this similarity?

Author's reply. Yes, this is the well-known weakly non-linear, so-called finite-depth theory, developed by Kubota, Ho and Dobbs (1978), *J. Hydronautics*. For $h \to \infty$ this reduces to the Benjamin-Ono equation. For $kh \ll 1$, the KdV equation is recovered.

12.2 Discusser M.P Tulin, mpt@vortex.ucsb.edu

Question/comment. I notice that in Fig. 1, for suitably large wave amplitude, that the local velocity profile has an inflation point ($u'' = 0$). This would normally lead to instability in a homogeneous fluid. Have you investigated the possibility that this is the source of what you call "breaking"?

Author's reply. In the experimental results where $u'' = 0$ in the fluid, breaking is also observed. At an early time of the experiment, we observe that u'' is not zero, and that the flow induced by the wave is laminar. Repeated experiments, where the conditions are varied, suggest that the local breaking depends on the boundary conditions at the free surface, and that the effect of a surface film may be the cause of the breaking. This is true for moderate waves. For large waves, intense breaking always occurs.

13 Hamilton, M.[1] and Yeung, R.W.[2] *Non-linear motion of a submerged body in waves*

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13.1 Discusser J.N. Newman, jnn@mit.edu

Question/comment. It is relatively easy to evaluate this Green function without performing numerical integration, e.g. using methods which I described in the Ursell Symposium Proceedings. Are you integrating over the cylinder to make the numerical integration easier (which seems unnecessary) or to invoke a Galerkin approach in solving the integral equation?

Author's reply. The Green function itself has rapid oscillations in time for large time when the source and field point are on the free surface even though an expression for its evaluation is available. These oscillations become all the more problematic if a panel element pierces the free surface. An integration of the unsteady Green function with respect to the spatial variables will help to make the resulting kernel of the integral-equation less oscillatory in time and easier to convolve. In our formulation, we evaluate these kernels on a fixed (matching) surface once for all and use the resulting "shell coefficients" for any body shapes in the internal domain. The integral-equation is solved by collocation, but over a high-order panel. We do not use a Galerkin formulation to solve the integral equation.

14 Huseby, M.,[1] Jensen, A.[2] and Grue, J.[3] *An experimental investigation of ringing loads on a vertical cylinder in transient waves*

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14.1 Discusser J. Chaplin, J.R.Chaplin@soton.ac.uk

Comment. There seemed to be a suggestion that the secondary load cycle was responsible for ringing excitation. In measurements (Chaplin, Rainey, Yemm, 1997) the response started earlier than the secondary load cycle, which in some cases led to a sudden reduction in the cylinder's motion.

14.2 Discusser B. Molin, molin@esim.imt-mrs.fr

Question/comment
1. What is your definition of ringing?
2. I am puzzled that you hint that ringing occurs for a wave crest elevation larger than the radius. This corresponds with the onset of vortex shedding. Viscous loads are not usually held as responsible for ringing.

Author's reply.
1. We don't aim to make a definition of "ringing" here. The events we discuss are those where a secondary loading occurs in the force.
2. Yes, we observe the secondary loading when the ratio of maximal wave elevation to the cylinder radius is larger than 1.

14.3 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk

Question. What was the horizontal displacement of water particles compared with the cylinder radius?

Authors reply. The horizontal displacement of water particles was not measured. The maximal elevation, ζ_{max}, is in the range 0.5–4.5 times of the cylinder radius r. The secondary force cycle is obtained for some of the intermediate values of ζ_{max}/r, but not for all.

15 Iafrati, A.[1] and Korobkin, A.A.[2] *Liquid flow close to intersection point*

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15.1 Discusser E. Fontaine, emmanuel@engineering.ucsb.edu

Question/comment. Numerical simulations for the wedge entry problem have been performed successfully using cut-off techniques for deadrise angles ranging from 4 to 81 degrees [1,2]. The reliability of these models seems therefore not to dependent on the limiting value of the jet angle, nor to depend on the experience of a researcher as claimed in the introduction. These models for the flow within the jet are in fact based on physical considerations rather than from a systematic expansion of the solution near the intersection point. As a matter of fact, these models are able to handle the transient regime prior to the self-similar one without the need of using a small time solution to start with. Following these comments, here are two questions:

1. Could the small time analysis be applied to shapes other than wedges ?
2. How are the effects of gravity accounted for in the expansion of the solution near the intersection point ?

Author's reply. The reliability of the approaches suggested in [1,2] depend on the limiting value of the jet angle even in the problem of wedge entry. In order to understand this point, we need to know the dependence of the jet angle β on the wedge deadrise angle γ. The function $\beta(\gamma)$ was depicted by Dobrovol'skaya (JFM'69, Figure 13) and was reported in [1] (see Table 1). The angle between the tangential to the free surface and that to the body contour at a distance r from the jet tip (see Fig. 1) will be denoted here by $B(r,\gamma)$, $B(0,\gamma) = \beta(\gamma)$. Using the known behavior of the similarity solution, we may suppose that $B(r,\gamma) \geq \beta(\gamma)$ in the jet region and even in the jet root region. If we are going to simulate the similarity solution with the cut-off technique described in [1], we need to choose a cut-off angle $B(r_*,\gamma)$ which at least satisfies the inequality $B(r_*,\gamma) > \beta(\gamma)$ and to be sure that r_* is small enough so that we are in the jet region but not in the jet root region. Let us check these conditions for $\gamma = 81^o$. In [1] it was taken $B(r_*, 81^o) = \pi/15 = 12^o$ with $\beta(81^0) = 0.07153\pi \approx 12.7755^o$ (see Table 1 from [1]).

It is seen that the inequality $B(r_*, 81^o) > \beta(81^o)$ is not satisfied, which means that this choice of the cut-off angle for $\gamma = 81^o$ does not correspond to the basic assumptions which lay in the heart of the cut-off technique. The effect of this disagreement is clear in Figure 6(k) from [1]. As to deadrise angles smaller than 81^o, the situation is not better. The similarity solutions depicted in Figures 6(a-k) from[1] show that the jet is wedge shaped. This means that $B(r,\gamma) \approx \beta(\gamma)$ in the jet region and $B(r,\gamma)$ changes abruptly in the jet root region, where the pressures are very high. This conclusion is consistent with the shallow-water solution by Howison *et al* (JFM'91) and the asymptotic analysis of the jet flows in both 2-D and 3-D impact problems by Korobkin (see "Liquid-Solid Impact", 1997, Chapter IV). This makes it possible to assume that, in order to employ the cut-off technique in the way described in [1], one has to take the cut-off angle being equal to $\beta(\gamma)$ - which comes from the paper by Dobrovol'skaya - and choose properly the distance of the cut from the jet tip position which again comes from the similarity solution. If we take the cut-off angle $B(r_*, \gamma)$ less than $\beta(\gamma)$, we will get nothing. If we take the cut-off angle $B(r_*, \gamma)$ greater than $\beta(\gamma)$, we will be forced to cut the jet in the jet root region, where the applicability of the zero pressure condition along the cut is questionable. A possible explanation why the cut-off technique works can be derived from the recently performed generalization of the Wagner's approach (see, for example, Mei *et al.* Applied Ocean Research, 1999, 21). It was shown that, in order to obtain numerical results which are reasonable good, one does not need to account for the real shape of the free surface and to satisfy accurately the non-linear free surface conditions. Much more important points are to account for the real shape of the entering body and to satisfy the original boundary conditions on the body surface.

It is clear that the BEM together with the cut-off technique described in[1] is more accurate than the generalized Wagner's approach and may provide results which are very close to the similarity solution. On the other hand, in order to achieve a "good" solution, a researcher has to be very experienced and has to know forehand many details about the liquid flows caused by impact.

The cut-off technique presented in [2] looks more robust. The jet is truncated normally to the body boundary as soon as its thickness becomes of the order of magnitude of the segment of the grid. The fluid tangential velocity on the cut is assumed to be constant, equal to its value on the body boundary. This technique requires a proper choice of the grid size. In particular, the grid size cannot be greater than the jet thickness which is unknown in advance. On the other hand, it cannot be too small. We think that the optimal grid size in the jet region is up to the experience of the researcher. We are not familiar with details in which way "these models are able to handle the transient regime prior to the self-similar one". We could not find formulations of the initial conditions and details of the start of numerical calculations in [1,2]. The description of the impact force in the initial transient seems not reliable, which comes from [1], where the slamming force is plotted only after the first truncation is performed but not before. The small time analysis is important, in particular, to understand better the behavior of the impact force during the initial transient stage.

In our paper we proposed a different model to cut the jet. The model itself does not require the similarity solution to start calculations. The suggested approach is weakly dependent on body shape. The main idea of the approach is to construct the local solution which satisfies the motion equations and the boundary conditions up to a prescribed order, and to use this solution together with numerical simulations of the flow. The local solution contain several undefined constants which have to be found by matching the local solution with the numerical one. The idea is helpful for any time, almost any shape and can be extended to account for gravity and surface tension.

The second part of the report is about the uniformly valid initial asymptotic of the flow. Nonlinear inner problem had been derived and we tried to solve it numerically. We found that more information about the solution is required to perform accurate calculations. We expect that the approach described in this report should work for shapes other than wedge but we have not tried this generalization yet.

At this stage the gravity effects are not accounted for in the jet solution. But it is possible to include gravity into the analysis.

16 Iwashita, H. *On unsteady waves generated by a blunt ship with forward speed*

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16.1 Discusser W.W. Schultz, schultz@engin.umich.edu

Question/comment. You mention larger modelling discrepancies due to a viscous bow wave. Could this be due to breaking or is it just viscous effects on the hull?

Author's reply. I think that the breaking can be considered as the most possible reason since we can observe waves with plenty of dimples at the bow part. But there is no confidence at this moment.

16.2 Discusser R. W. Yeung, rwyeung@socrates.berkeley.edu

Question/comment. I wonder if you can detail how the wave surfaces of the unsteady (sinusoidal) motion were obtained. Was that a process involving some surface mapping technique?

Author's reply. Any surface mapping technique are not used. The experimental method used here is a multi-fold method developed by Prof. Ohkusu in 1977. Waves are measured along a longitudinal line parallel to the x-axis by using 6 wave probes set along longitudinal line with a certain distance. Therefore they measure 6 waves of different time at the same x point on body-fixed coordinate and unsteady wave (cos and sin components). By changing the location of y, the same measurement is repeated provided that the same physical phenomenon appears again. One experiment for one y value needs 1 hour waiting for the water surface to become calm again. This time since we measured for 20 different y values, it took about three days to get one perspective view of heave radiation wave on x-y plane.

17 Jiang, T.[1] and Henn, R.[2] *Nonlinear waves generated by a surface piercing body using a unified shallow-water theory*

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17.1 Discusser H.B. Bingham, hbb@imm.dtu.dk

Question/comment. You did not show any force calculations. Low-order Boussinesq methods typically get only the first evanescent mode correct, which has severe consequences for the prediction of added-mass. Does your method improve this state of affairs, and if so, how?

Author's Reply. We thank Dr. Bingham for his question. Our preliminary study shows a good agreement of the calculated force amplitude with that from panel methods in the frequency domain for small and moderate motion amplitudes. However, there exists a phase shift, apparently caused by the initial conditions in our time domain calculation.

17.2 Discusser M.W. Dingemans, maarten.dingemans@wldelft.nl

Question/comment. You take u expressed in $\bar{u}$ including z^2 terms, while the vertical velocity w is linear in z. As a consequence, z^2 terms remain in $u_x + w_z$ and the continuity equation is not fulfilled. What is the influence of the fact that your velocity field (u, w) does not fulfill the continuity equation?

Author's Reply. We thank also Dr. Dingemans for his comment. In general, the shallow-water wave equations of Boussinesq type guarantee the exact satisfaction of the vertically integrated continuity equation, i.e., horizontal mass conservation. However, for reconstructing the velocity field using $\bar{u}$, formulations of different orders can be given. In our oral presentation the equations for the velocity field components were used just for the visualization of the velocity field. This formulation does not fulfill the continuity equation exactly. The higher-order formulation which satisfies the continuity equation exactly at each field point is indeed different.

17.3 Discusser R.W. Yeung, rwyeung@socrates.berkeley.edu

Question/comment. Thank you for a nice presentation. Because of the depth-wise approximation of the flow, these modified Boussinesq's equations would nevertheless be unable to model geometric changes (free surface and body juncture lines) that are not smooth. The simulations you showed seem to suggest a discontinuous tangential (vertical) velocity under the bottom corners of the circular cylinder (or rectangular cylinder). This could mean only that the pressure and flux are matched in a certain weighted-average sense, but not the detailed flow structure. Therefore, quantities such as added mass may not be well predicted, but wave damping could be adequately predicted. Perhaps a good test is to check the added-mass and damping of your cylinder over a wide range of frequencies against known solutions, subject to some nonlinear effects, of course.

Author's Reply. We appreciate Prof. Yeung's comments. It is true that no shallow-water equations are valid directly on the line of intersection of the free surface with the body surface. In the unified shallow-water theory proposed here, this intersection is just the dividing line between the domains of the two different sets of Boussinesq type equations, namely, the outer and the inner fluid field, respectively. The basic difficulty is thus to find suitable coupling conditions at this intersection line (the instantaneous waterline). In our preliminary study we have just implemented the first-order interfacial coupling conditions which led to realistic wave elevations as well as wave forces. However, for the detailed flow structure, especially around the corners, higher-order coupling conditions can be theoretically derived. But they are numerically hard to implement. We will follow Prof. Yeung's suggestion to compare our results with known solutions in the frequency domain, including computed and measured hydrodynamic response forces in the time domain, particularly at large motion-amplitudes.

18 Kashiwagi, M. *Wave interactions with a multitude of floating cylinders*

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18.1 Discusser R.F. Beck, rbeck@engin.umich.edu

Question/comment. Have you [illegible] used. Of course there is no fundamental difficulties in numerical computations for higher frequencies.

18.3 Discusser J.N. Newman, jnn@mit.edu

Question/comment.Your experiments show remarkable qualitative agreement with linear theory, contrary to the comparison last year by Kagemoto and his colleague. What is the difference between your respective experiments?

Author's reply. In the experiments of Kagemoto *et al*, many cylinders were placed in a single straight line and attention was paid to the wave elevation at just the middle points between adjacent cylinders. Furthermore, measurements were carried out at some frequencies, not enough to show complicate variation with respect to Ks. The experiments in this study are conducted using 64 cylinders arranged in 4 rows and 16 columns and concerned with spatial continuous variation along the centerline of the structure The results show rapid and large variation near the critical frequency ($Ks = 1.26$): the wave elevation is relatively small at just the side of the cylinders but becomes very large between adjacent cylinders. In comparison with computed results, measured wave elevations between cylinders are much smaller than computed ones especially near the critical frequency, which is actually consistent with the conclusion of Kagemoto *et al*. In addition, this paper demonstrates also continuous variation of waves and forces with respect to Ks at some fixed positions, and overall agreement between experiments and computations is shown to be very good except for a limited region near the critical frequency of trapped mode.

19 Lin, H.J. and Perlin, M. *The velocity and vorticity fields beneath gravity -capillary waves exhibiting parasitic ripples*

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19.1 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk

Comment. With regard to eddy motion at the wave crest Van Dorn and Pagan (1975, JOEL Rep. No. 71, *Scripps Inset. Oceanog.* Ref No. 75-21) measured an eddy in the crest as waves were about to break that was counter clockwise, for rightward propagating waves, and which could be attributed to viscous effects of the air passing over the moving wave crest. These were 0.66 Hz waves on deep water.

20 Molin, B. *On the sloshing modes in moonpools, or the dispersion equation for progressive waves in a channel through the ice sheet*

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20.1 Discusser D.V. Evans, D.V.Evans@bris.ac.uk

Comment. There has been a good deal of work done on the eigenvalues in a channel through an ice-field using powerful variational methods, where it is possible to get accurate complementary bounds for the eigenvalues.

20.2 Discusser J.N. Newman, jnn@mit.edu

Question/comment. The change in resonance period you observed is surprising to me too. We have analyzed similar problems and found that the first sloshing mode period was as expected, close to the point where the half-wavelength equals the long dimension of the moonpool, but this was for platforms of greater depth.

Author's reply. For the period to be modified both the draft and the width of the moonpool need to be small as compared to its length. Also the beam and length of the barge need to be sufficiently larger than the width and length of the moonpool.

21 Mori, K. and Nagaya, S. *Wave making resistance of a submerged hydrofoil with downward force*

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21.1 Discusser E.O. Tuck, etuck@maths.adelaide.edu.au

Question/comment

1. Was the second part of the talk about the use of wave-free singularities 2-D or 3-D?
2. I presume that as usual in aerodynamics the induced drag reduces as the aspect ratio increases ?

Author's reply

1. 2-D singularities are used. But numerical simulation is carried out 3-dimensionally.
2. It's true, but in the present case it happens. More studies may be necessary.

21.2 Discusser M.P. Tulin, mpt@vortex.ucsb.edu

Question/comment. The Type I wave free singularities when applied to slender bodies, can not produce a net upward sustaining force (which you need for ships). How then can you use them to produce "ships" of minimum wave resistance? *Recommendation*: Study Type II singularities (see the paper by Tulin & Oshri in ONR Symposium, 1994, Santa Barbara).

Author's reply. Thank you for your suggestion. Our primary purpose is to make clear the phenomenon anyway. Although the body itself cannot be applied for ships with no pay-load, there are still some applications such as foils attached to the hull to control the ship motion which has less resistance.

22 Newman, J.N. *Diffraction of water waves by an air chamber*

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22.1 Discusser C.N. Linton, C.M.Linton@lboro.ac.uk

Question/comment. The structures you are considering are very long. In practical situations do you think that finite depth effects would be important?

Author's reply. This will depend on the locations where these structures are used, with some deep and others shallow. In practical design work I could expect 3-D radiation/diffraction codes would be used, and it would be straightforward to account for the actual depth.

22.2 Discusser S. Malenica, sime.malenica@bureauveritas.com

Question/comment. In the context of very large floating structures you may also have "problems" of structural (elastic) resonances. Would it be possible with your method to couple all three phenomena: water waves, acoustics and elasticity ?

Author's reply. In our analyses of more realistic three-dimensional structures we have included the effects of elasticity, but not with an air cushion. Relevant references are listed below. The analysis of the 3D air-cushion structure in the VLFS '99 paper (referenced in the abstract) could be extended to include elastic radiation modes of the body, in a straightforward manner. We generally include these effects by extending the number of radiation modes and corresponding potentials to include a sufficient number of Fourier elastic modes to provide a convergent representation of the elastic deflections.

The present study, with an idealized 2D structure, could be extended in a similar manner. However this extension may not be as appropriate, since the structure is simplified and has no mass.

Reference. Lee, C.-H. Wave interaction with huge floating structure, *BOSS'97*.

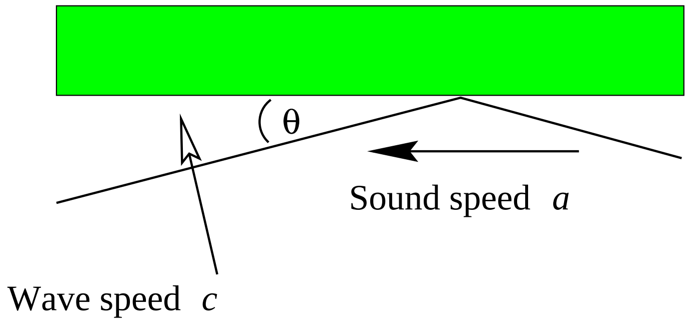

Figure 22.3.1: Interesting effects should occur when $\sin\theta = c/a$

22.3 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk

Question/comment. Your motivation was for a long thin structure. My suspicion is that the most interesting acoustic effects would be for waves that were incident, near normal to the long side such that their phase velocity of encounter with the side of the structure was at the speed of the sound.

Author's reply. I agree that this would be a very interesting case to study.

22.4 Discusser R.C.T. Rainey, rctrainey@wsatkins.co.uk

Comment. A near-broadside incident water wave will have a long wavelength along the floating airport will it not, so that it might match the very long wavelength of the internal acoustic wave. There was an analogous problem during the surface-towing of the Heidrun TLP tethers to site. The worst case there was obliquely incident waves, whose length along the tethers matched the wavelength of lateral tether vibrations of the same period.

23 Ohkusu, M. *Analysis of wave force on a large and thin floating platform*

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23.1 Discusser W.W. Schultz, schultz@engin.umich.edu

Question/comment. One of your boundary conditions imposes a square root singularity. How do you know what premultiplying constant is appropriate. Should that be part of the solution?

Authors reply. Description on the issue raised in the discussion is too brief in my paper. The following is more elaborated one. A solution of the boundary value problem of equation (8) is actually equation (9) plus a square root singularity with arbitrary constant C as the discusser points out. For example it will be on (-1$\leq x \leq 1$, $y = 0$)

$$\frac{\partial \phi_R}{\partial x} = -\frac{1}{\pi\sqrt{1-x^2}} \int_{-1}^{1} \frac{\sqrt{1-\xi^2}}{\xi - x} \sum_n A_n U_n(\xi) d\xi + \frac{C}{\sqrt{1-x^2}}. \tag{1}$$

Integration of this equation with x gives

$$\phi_R(x,0) = \int_{-1}^{x} \frac{i\omega}{\sqrt{1-x^2}} \sum_n A_n T_{n+1}(x) dx + \int_{-1}^{x} \frac{C}{\sqrt{1-x^2}} dx \tag{2}$$

In the derivation I utilized $\phi_R(-1,0) = 0$ resulting from the condition that the velocity potential is continuous and the condition (6) , and

$$\frac{1}{\pi} \int_{-1}^{1} \frac{\sqrt{1-\xi^2}}{\xi - x} U_n(\xi) d\xi = -T_{n+1}(x) \tag{3}$$

where $T_n(x)$ is the Chebyshev polynomial of the first kind. In view of the condition of continuity $\phi_R(1,0) = 0$ and

$$\int_{-1}^{1} \frac{T_{n+1}(x)}{\sqrt{1-x^2}} dx = 0 \tag{4}$$

we conclude that $C = 0$.

24 Pelinovsky, E.[1] and Kharif, C.[2] *Simplified model of the freak wave formation from the random wave field*

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24.1 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk

Question/comment. The solutions presented are extremely special and very unlikely to occur in practice. The work of Henderson, Peregrine and Dold (1999, Wave Motion, Vol.29, 341-361.) has recently been extended to initial conditions with a spectral distribution in 2D. When comparisons between linear theory and fully nonlinear potential flow computations are made it is seen that the nonlinear focusing effects are dominant in creating the highest waves. Work has commenced on studying 3D waves.

Authors reply. Our calculations in the framework of linear theory and nonlinear shallow-water theory confirm that the wave focusing is the dominant mechanism for explanation of the freak wave phenomenon. The first results were published in our paper in Proc. Int. Conf. PACON'99 (Moscow, Russia, 1999, p.241). The main result of our new work is the analytical method to find such possible forms of the wavetrains that their evolution leads to the formation of the freak wave.

26 Rognebakke O.F.[1] and Faltinsen, O.M.[2] *Damping of sloshing waves due to tank roof impact*

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26.1 Discusser E.O. Tuck, etuck@maths.adelaide.edu.au

Question/comment. How can the computations continue after the waves have broken? What do you do with the mass of neglected "raindrops"?

Author's reply. There are two methods that describe the flow. The first method describes nonlinear sloshing with infinite tank roof length. It assumes implicitly that no overturning waves occur. When water impact occurs, the second method (slamming model) is introduced to satisfy boundary conditions on the tank roof. The slamming model describes overturning of the free surface and jet flow. When the impact is over, the first method describes the flow. The effect of the impact of the first method is in terms of a damping coefficient. The first method satisfies automatically continuity of fluid mass. Even if the mass in the "rainfall" is small, it could have caused an important accumulated effect. Since the natural period of the sloshing is dependent on fluid mass, the satisfaction of continuity of fluid mass is important.

27 Shemer, L., Jiao, L.H. and Kit, E. *Nonlinear wave group evolution in deep and intermediate -depth water: experimental and numerical simulations*

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27.1 Discusser D.H. Peregrine, D.H.Peregrine@bristol.ac.uk

Comment. With modelling of wave groups using Davey-Stewartson equations, and full potential flow in shallower water, $kh < 1.36$, we find even stronger effects of phase changes at an amplitude zero than those found in the experiments reported in this talk (C.Bird, 1998 Ph.D. thesis). This is related to the existence of dark solitons in the shallower water.

Author's reply. We agree that in shallow water many aspects of wave group evolution along the tank are quite different from that in deeper water. The corresponding problem in shallow water was studied recently both experimentally and numerically by applying the Korteweg-deVries equation (Kit *et al*, to appear in *J. Waterway, Port, Coastal and Ocean Eng.*) The thesis by C. Bird is cited in this paper.

28 Subramani, A.K. and Beck, R.F. *Suppression of wave breaking in nonlinear water wave computations including forward speed*

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28.1 Discusser H. van Brummelen, Harald.van.Brummelen@cwi.nl

Question/comment. How does your procedure affect convergence to steady state? How do you measure convergence? How do you impose the dynamic condition after fairing?

Authors reply. In our method, the boundary value problem at a given time step is solved exactly, not iteratively. So, the question of an iterative convergence (including measuring such convergence) does not arise. It may be pertinent to draw attention again to the fact that we do a fairing at the end of each mini-time step, in the 4th order Runge-Kutta time-stepping scheme. Presumably, an iterative solver would, likewise, necessitate a check for wave breaking and a possible "fairing" at the end of each iteration. Note that we implement the fairing after using the kinematic and dynamic free-surface boundary conditions to update (time-step) the wave elevation η and the free-surface potential ϕ. It is important to note also that we fair through both, η and ϕ, thereby ensuring consistency. We conjecture that for numerical methods in which fluid pressure is solved for and updated, our technique may find equivalence in the simultaneous fairing of η and pressure.

28.2 Discusser M. W. Dingemans, maarten.dingemans@wldelft.nl

Question/comment. The wave breaking criterion you use is based on experience. I would like to point out that an exact wave breaking criterion is given for a Hamiltonian wave model in Dingemans and Radder (1991), Bristol and Otta, Dingemans and Radder (1996), ICCE. When the wave curvature is too high, the criterion is violated. The criterion gives guidance on how much energy should be substracted.

Authors reply. We thank you for calling our attention again to the cited works. You may recall our having a similar discussion at the 13th International Workshop, where we first proposed our criterion. For nonlinear water waves, which is our interest, we do not believe (as is the consensus) that there exists a single, exact criterion for wave breaking.

28.3 Discusser J.-M. Vanden-Broeck, J.Vanden-broeck@uea.ac.uk

Question/comment. For the 2-D calculations, steady solutions exist only for values of draft-based Froude number greater than a critical value, F^*? To which value of F do your computations correspond?

Authors reply. We are well aware of the paper (Vanden-Broeck, 1980) in which you reported a minimum Froude number (of 2.26) below which the downstream waves would exceed the theoretical breaking wave steepness limit $H/\lambda = 1/7$. As stated in our paper, our computations correspond to a Froude number based on transom depth of 2.3, a Froude number at which we encountered the numerical wave breaking shown in figure 4 (dashed line).

Reference. Vanden-Broeck, J.M., "Nonlinear Stern Waves", *Journal of Fluid Mechanics*, Vol. 96, part 3, 1980, pp. 603–611.

29 E.O. Tuck. *Numerical solution for unsteady two-dimensional free surface flows*

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etuck@maths.adelaide.edu.au

29.1 Discusser R.F. Beck, rbeck@engin.umich.edu

Question/comment. Do you have a node at the intersection of the water surface and the wall (i.e,. on the plane of symmetry)?

Author's reply. We have tried both ways, i.e., either placing a point on the wall or not. Both seem to work OK, and we have standardized on placing a point on the wall, really for aesthetic reasons. In principle, there should then be one less variable, since such a point $x = 0$ for all time but we have not found it necessary to impose such a constraint. Some of our computations (e.g. the 2:1 ellipse) give sharp spikes at $x = 0$ and this needs care, but in principle and in general there is nothing special about the symmetry plane $x = 0$, and our program works as well there as anywhere else.

29.2 Discusser W.W. Schultz, schultz@engin.umich.edu

Question/comment. Your first results on the speed of Stokes wave implied that the larger the desingularities, the better. Is this always true? Our similar computations show an optimum near one grid space away? Have you checked conservation of energy (or momentum or mass)?

Author's reply. I have not found such an optimum, although I certainly do not want to use too large a ratio between offset distance and grid spacing. I have not had any trouble up to ratios of 4; beyond that, there may be ill-conditioning difficulties. Conservation of mass has been checked, e.g. when no total source, strength constraint is used, we find that nevertheless the sum of source strengths is zero to machine tolerance. The other conservation laws should be true automatically by the assumption of irrotational potential flow.

29.3 Discusser S.D. Sharma, sharma@nav.uni-duisburg.de

Question/comment. Would you please repeat your empirical rule for choosing the local magnitude of "offset", i.e., distance of source singularity form the free surface? Should it now be explicitly related to the local curvature of the free surface?

Author's reply. We use an offset distance of about 3 times the local grid spacing. Indeed it is true that one might expect that the offset distance should be reduced where the radius of curvature is small, and perhaps that distance should also be not greater than some small multiple of the radius of curvature. However, we have taken the attitude that there is no need to build in such a requirement. Rather, it is our responsibility in designing the initial spacing of the grid to ensure that the local grid spacing remains adequately fine near regions that are (or will become) of high curvature. Having done that, our criterion of an offset of about 3 times the local spacing can be applied universally.

29.4 Discusser J.-M. Vanden-Broeck, J.Vanden-broeck@uea.ac.uk

Question/comment. Do you need to smooth the solution when marching in time?

Author's reply No. The output seems to be inherently smooth.

30 Simon, M.J.[1] and Kuznetsov, N.G.[2] *On the uniqueness of water-wave problems for submerged cylinders in oblique waves*

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30.1 Discusser C.M. Linton, C.M.Linton@lboro.ac.uk

Question/comment. Do you think it would be possible to extend your proof method to the case of two-layer fluids?

Author's reply. I am hoping to consider the case of a two-layer fluid next.

30.2 Discusser S.D. Sharma, sharma@nav.uni-duisburg.de

Question/comment. What does the non-uniqueness of the solution mean physically?

Author's reply. It is known that local non-radiating solutions can exist in problems of water-waves interacting with bodies; there are many examples of such trapped modes now. Consequently a wavelike solution of a suitable frequency can be non-unique because of the addition of such a trapped mode. However, the fact that the proof described here fails to demonstrate uniqueness in a certain case does not necessarily indicate the presence of such a non-uniqueness.

31 Ursell, F. *The metacentre in stability of ships. Some difficulties*

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31.1 Discusser J.N. Newman, jnn@mit.edu

Question/Comment. Regarding the "feeling" that the static couple and (initial) fluid couple must be in the same direction, it should be sufficient to prove that the generalized added mass coefficient is positive. This would seem to be a straightforward extension of classical added-mass theory for a (non-rigid) body in an infinite fluid.

Author's reply. When we are given the an expression for the energy in terms of coordinates, we then hope to be able to deduce the corresponding force components. For example, in particle mechanics, if T is the kinetic energy, the corresponding force (or impulse) for the q-coordinate is

$$\frac{d}{dt}\frac{\partial T}{\partial \dot{q}} - \frac{\partial T}{\partial q}$$

In fluid mechanics the corresponding argument is much more elaborate (since the position of the fluid particles is no longer specified by coordinates), and is given for a rigid body in an unbounded fluid by Lamb, ch.6, art.135, and in more detail by Garrett Birkhoff in his little book on *Hydrodynamics*. In our problem we are concerned with the corresponding theory for a deformable body of a simple type. This would be very interesting, presumably additional coordinates would be needed. In the end it might be easier to try to calculate the impulsive forces directly.

31.2 Discusser H. Nowacki, Horst.Nowacki@ism.tu-berlin.de

Comment. I am submitting this written discussion in response to the author's abstract posted on the conference website because I was not able to participate in the meeting. My remarks are based on a recent thorough scrutiny of Pierre Bouguer's work when he introduced the metacentre as well as that of his precursors (Archimedes, Huygens) and contemporaries (mainly Euler). I believe it is important to do justice to the original purpose of and justification for the metacentre as presented by Bouguer as its inventor, which are still widely understood in the same spirit today, thereby avoiding misunderstandings possibly derived from modern textbooks.

In Bouguer's Traiteé du Navire (1746) the concept of the metacentre is introduced for assessing the hydrostatic stability of a ship in the upright condition. He also alludes to the hydrostatic stability at finite angles of heel by introducing the metacentric curve (without much further discussion), but he does not claim to deal with ship motions or dynamic stability at all. One may in practice distinguish three distinct cases of ship stability assessment:

1. Hydrostatic transverse stability in the upright condition (initial stability): The ship is floating in equilibrium at zero heel when an infinitesimal heel angle distortion is applied. The equilibrium is stable if after removing the cause of the distortion a positive restoring moment prevails. As Bouguer derived, two infinitesimally neighboring buoyancy directions intersect in the metacentre M, and the restoring moment is positive if M lies above the center of gravity G. This is a necessary condition for stable equilibrium in the upright condition.

2. Hydrostatic transverse stability at a finite angle of heel: Given a static equilibrium condition at some finite heel angle, produced by some internal weight shift or some external heeling moment. The issue now is, when some distortion by an incremental infinitesimal heel angle is applied, whether the ship will return to its initial heeled equilibrium condition once the cause of the distortion is removed, while the given heeling moment remains in action. Bouguer showed, at least he correctly inferred, that the metacentric curve, i.e., the evolute of the buoyancy curve, furnishes a stability criterion in this case. He pointed out that the evolute, i.e., the locus of the centers of curvature of the buoyancy curve, and the curve in which infinitesimally neighboring buoyancy directions intersect are identical and the curve in which infinitesimally neighboring buoyancy directions intersect are identical by definition. It was known from Huygens how to construct the evolute. This is a necessary stability condition for a ship floating in a statically heeled condition.

3. Dynamic transverse stability: Clearly a criterion for dynamic stability of rolling motions requires a fully dynamic approach, possibly based on the equations of motion or energy principles, which are often used. This seems to be the type of stability the paper is addressing. This case certainly merits further investigation as another operationally most relevant case.

In conclusion I welcome the ideas expressed in Fritz Ursell's paper, especially also those on the differential geometry of metacentric surfaces in the context of ship motions. These thoughts deserve to be pursued further without detracting anything from the value of Bouguer's metacentre for static ship stability.

Author's reply. I was very pleased to learn at the Workshop that Nowacki shares my interest in the metacentre. Nowacki has done a service in studying the old literature, and bringing it to our attention, and in particular in reminding us of Bouguer's contribution which relates to the hydrostatic couple on a ship held at rest in the inclined position. Nowacki has sent me a longer and more detailed version of his historical studies on the metacentre, and I very much hope that he will soon publish this.

31.3 Discusser R.C.T. Rainey, rctrainey@wsatkins.co.uk

Prof. Newman has just observed that if your 6x6 "added mass" tensor M (i.e. the linear map from your 6-vector a of initial linear/angular acceleration, to the fluid force/moment

Ma felt on the hull) is positive definite, then you will be a long way towards proving that the initial motion of the body will be well-behaved.

But can one not argue in the style of Lamb's *Hydrodynamics* (1932) Arts. 44 and 121, that if the linear/angular velocity of the hull is the 6-vector v, then the total kinetic energy of the fluid in the upper and lower half-spaces is $[v^T M v + (-v)^T M(-v)]/2$? This would establish that M is positive definite, would it not, because the kinetic energy is necessarily positive?

Author's reply. My observations to Newman's comments are applicable here. We wish to use the energy to find the force (or impulse). Also, in the present case the motion of the hull is not described by a 6-vector, since the hull's motion is not a rigid-body motion.

31.4 Discusser W.W. Schultz, schultz@engin.umich.edu

Question/comment. It seems like one must carefully define what is meant by "stability". Normally, I think of stability as "linear stability theory" determining the short time behavior for small disturbances. You seem to emphasize energy stability theory or "asymptotic stability in the mean" as presumably those on board ship would eventually like to see the ship upright. This second approach is considerably more difficult to determine and normally includes nonlinear terms. Is the metacenter sufficient for the former definition?

Author's reply. My conclusion is that the metacenter enters into the description of the hydrostatic couple, but not into the description of any other component.

32 Vanden-Broeck, J.-M.[1] and Spivak, B.[2] *Free-surface damping due to viscosity and surfactants*

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32.1 Discusser L.J. Doctors, l.doctors@unsw.edu.au

Question/comment. In your first graph showing the free-surface elevation for relatively low Reynolds number, you noted that the curve is asymmetric fore and aft-due to the influence of viscosity. You commented that this curve would be symmetric without viscosity. Could you kindly clarify this, in view of the fact that in the classic inviscid case of a traveling pressure distribution the free-surface elevation is asymmetric.

Authors reply. In the absence of viscosity the free-surface is symmetric if the velocity at which the pressure distribution travels is small enough.

32.2 E. Pelinovsky, enpeli@appl.sci-nnov.ru

Question/comment. The quasi-potential approximation is valid for weak viscosity. Surfactants lead to significant increasing of wave damping at large elasticity. Do you think that quasi-potential theory is valid for weak viscosity fluid with high surfactant film elasticity ?

Author's reply. Our results are only valid for moderate values of the Boussinesq and Marangoni numbers.

32.3 Discusser M.P. Tulin, mpt@vortex.ucsb.edu

Question/comment. What is the limitation of the quasi-potential approximation ? I ask that because of the evidence from observations of wind waves (Toba) and mechanical waves (Duncan) that vorticity is strongly generated from through of high curvature, and that this vorticity can strongly effect the wave. This possibility has been recently addressed by Longuet-Higgins.

Authors reply. The quasi-potential approximation is only valid for high values of the Reynolds number.

32.4 R.W. Yeung, rwyeung@socrates.berkeley.edu

Question/comment. Your viscous effect consists of surface vorticity generated on the free surface. Once generated, this vorticity will diffuse into the fluid, especially if you are seeking a "steady-state" solution. The use of a completely potential-flow model under the surface vorticity layer needs further justification or assumption. The authors may want to comment on this approximation and its restriction.

Author's reply. Our theory is based on the papers of Ruvinsky and Friedman who analyze slightly viscous capillary-gravity ripples riding on the front of a gravity wave. Therefore our results are only valid for high values of the Reynolds number.