**Ocean Science
Discussions**

Interactive
Comment

Interactive comment on "Imbalance of energy and momentum source terms of the sea wave transfer equation for fully developed seas" *by* G. V. Caudal

G. V. CAUDAL

gerard.caudal@latmos.ipsl.fr

Thank you for your detailed comments on the paper. I answer here below to the comments which are suggestions for revisions (answers labelled (A)). I have also prepared a new version of the paper taking into account your recommendations (see attached file). In this proposed new version of the paper, the corrections are written in red in order to visualize more easily the modifications.

Abstract: Energy/momentum "balance" and energy/momentum "budget" are used, e.g. "fulfilled the ...balance", and it is clear that the paper is using fully developed sea states, but the connection is not clear in the abstract. It becomes clear at lines 30-40: in the concept of full development, source terms are in balance with respect to total energy and momentum. The author already says something about this, but I would emphasize

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

further: that the concept of full development is really just that: a convenient vehicle for a mental experiment but not particularly "real" since winds are always nonstationary and nonuniform.

(A). The connection between fully developed situation and overall balance of energy and momentum is essential for the whole paper, and I agree that the abstrasct must state this point very clearly, and this was presumably not the case. I propose to add a sentence at the beginning of the abstract to make this clear (lines 3-5). In the introduction I stress the point regarding the nonstationarity of the sea wave spectrum in real situations, implying that the concept of full development should be regarded as a mental experiment rather than describing the real world (lines 38-40).

line 59. Equation (1) is not the transport equation, since there are no propagation terms here.

(A). The propagation term has been added in the left hand side of equation (1) (now equation (1a), and in the following discussion.

eq 4, 5a, 5b. Reading this part, I was confused re: the significant of LHS vs. RHS, as it would seem logical to group these like terms and have zero on the RHS.

(A). Modifications done

line 280-290. example plots of the two spectra would help here.

(A). I have added a new figure displaying an example of the comparison between Elfouhaily et al.'s (1997) and Kudryavtsev et al.'s (2003) model functions, in the high wavenumber domain where they are differentiated ($k \geq 25kp$). (see new Figure 1, and text lines 296-299).

eq 23. Beta_br and Beta_s are discussed before and after but are not part of the equations. This is confusing. Why is P in the equation but not Beta or gamma?

(A). It is more convenient to give expressions (23a,b) as functions of P rather than βbr

and βS because in the paper each wave breaking model has been defined through its function P (see equations (14b), (16b), or Appendix B). Gamma (=γ1 or γ2) is indeed in the left hand sides of equations (23ab) (=equations (26ab) in the new version), but will be eliminated thereafter by stating γ1=γ2. In the paper I had omitted to recall here the generic relation βbr=-γP (see equations (14a, (16a) and (17)), which is however essential to obtain equations (23ab) (=equations (26ab) in the new version). I have now corrected the text carefully to give those precisions and avoid any confusion (see lines 450-464).

Line 540-550. So does the cost function exercise tell us that the approach of the paper (to include a downshifting breaking term) is not worthwhile?

(A). Actually, the situation depends upon the wave breaking and wind source term models chosen : The approach of this paper gives better results (i. e., lower cost function) than the classical approach if one uses the WAMDI(1988) and Komen (1984) models (the ones which led to the smallest energy-momentum imbalance in Tables 1 and 2). On the contrary, if one uses Ardhuin et al. (2010), and JBA models, my approach gives a cost function similar to the classical approach if Kudryavtsev et al.'s (2003) HF spectrum is used, but slightly worse results if Elfouhaily et al.'s (1997) spectral model is used. Also the comparison with laboratory experiments given in section 3.3 tend to indicate that the values of μ (between 3 and 6) obtained using the Ardhuin et al.(2010) and JMB models were overestimated. Values of μ obtained by using WAMDI(1988) and Komen(1984) models ($\mu \approx 2.5$) are more consistent with Tulin and Waseda's (1999) experimental values. Therefore I agree that using my approach is more useful if the WAMDI and Komen (1984) models are used, rather that when the Ardhuin et al (2010) and JBA models are used, even though in that latter case the energy/momentum imbalance should be addressed somehow. I have introduced a discussion about that point in the paper (lines 599-606), and for those reasons the examples given in the figures now display the results obtained using WAMDI(1988) and Komen (1984) models.

Line 473: Subjective comment: I found the cost function exercise to be less interesting

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

than the rest of the paper. This issue of "addressing the efficiency with which different source... terms cancel each other..." : I did not find this motivation very compelling, and my general sense is that due to the underlying uncertainties with the source terms, the assumptions of infinite fetch and duration, the assumption re: modulation instability...the link to reality is too tenuous at this point to justify investigating the details through the use of cost functions. Also, I wasn't clear re: what was accomplished from this.

(A). In the new version of the paper this cost function is now used to indicate that my approach is more or less useful depending upon which wind and wave breaking source terms are used (see comment above). I agree that, due to the underlyind uncertainties, details about K are probably superfluous, and thus my former Figure 3 (displaying delta(K) versus parameter ν), which did not bring much to the paper, has therefore been suppressed.

Line 610-620 : "Since the momentum of waves is quite sensitive to the directional spectrum at high wavenumbers"... Sounds reasonable, but then..."further improvements of the model for directional spectrum..." Is this author implying that this is the most urgent issue to resolve now? I tend to think that a more urgent issue is to solve the uncertainties in the source function themselves, especially in the context of frequency/directional distribution, than to improve the empirical models that they are applied to. This is in fact more consistent with what the author said earlier in the paper.

(A). My point here was that, since the momentum of waves is particularly sensitive to the directional spectrum at high wavenumbers, any improvement of the description of the spectrum would be particularly useful in the high wavenumber domain to address that question. However, the most important issue raised by the paper is that, when defining and tuning the source functions, the requirement of balance of both energy and momentum budgets should be taken into account. The end of the conclusion has been modified accordingly (lines 691-698).

pg 17. Since it is such a quick thing to calculate source terms on a parametric spectrum, and there is no strong connection to operational numerical model, I can see no reason why DIA should be used instead of exact-NL. This may have a huge impact on the fluxes, judging from figures in Hasselmann et al. 1985 (the DIA paper). This would in turn have a huge impact on how strong the "modulation instability" source function needs to be to get a momentum balance of zero.

(A). Under your recommandation, in the whole paper I have replaced the DIA by the exact-NL. The computation is performed by using the method described by Van Vledder (2006). All the results displayed now take into account this modification.

Conclusion section: What is the message to take away? I guess the main message is on lines 585-586. Perhaps it would help to come back to the main point in the closing statement(s).

(A). I have modified the end of the conclusion, and added a final statement synthetizing this main message.

Line 220-230. I'm curious: have these equations ever been used in a numerical model? If so, please give a reference. As far as one can tell from this, it is just an idea.

(A). The model function proposed by Phillips(1985) was based upon theoretical considerations. As far as I know this model has not been used in operational models, although the idea of βbr proportional to the spectral density raised to some power has been retained by Donelan and Pierson (1987) for the high wavenumber domain (but with a different power than Phillips (1985)).

Minor comments:

To help the "careless reader", the words budget and balance should be explicitly associated with an equation early on. Like the text on line 74, but more explicit.

(A). New equation (1b) added.

Description of DIA on pg 17 can be removed, assuming that this is pretty standard stuff.

(A). Description removed (and DIA replaced by exact-NL, see above).

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/9/C1167/2012/osd-9-C1167-2012-supplement.pdf

———————————————————

Interactive comment on Ocean Sci. Discuss., 9, 2581, 2012.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper