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

**Anonymous Referee #1**

Received and published: 14 September 2012

Review of "Imbalance of energy and momentum source terms of the sea wave transfer equation for fully developed seas" a discussion paper by G. V. Caudal

all line numbers correspond to those in the file obtained as: os-2012-71-manuscript-version1.pdf

Comments which are not suggestions for revisions:

I found the concept of checking for conservation of energy and momentum to be an interesting idea.

Take a hypothetical case of a numerical model which is run until it is fully stationary, partial/partial  $t = 0$ . The  $E(f, \theta)$  at this stage... call it  $E_{\text{num}}(f, \theta)$ . Energy and momentum are conserved. Then the same source functions are applied to an em-

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pirical spectrum, call it  $E_{\text{emp}}(f, \theta)$ . Energy and momentum are not conserved, as demonstrated in the paper. The non-conservation is caused by the differences between  $E_{\text{num}}(f, \theta)$  and  $E_{\text{emp}}(f, \theta)$ .

Wave model source terms, such as the ones cited and used here, are developed for use in numerical wave models. In particular, the whitecapping term  $S_{\text{ds}}$  is calibrated to act as a closure term to achieve a desired result, either for a growth curve, or a global model simulation, etc. The author here applies the source terms with two empirical parametric wave models, referred to in the paper as "two-dimensional empirical sea wave spectral models". I agree with the author that the empirical wave models are more reliable than the source terms. Similarly, wave spectra from the numerical models are less realistic than spectra from the better empirical wave models. This is especially true in the HF tail. We can expect that the source terms (again,  $S_{\text{ds}}$  in particular) are highly sensitive the differences between the less realistic numerical model spectra (which they are developed for) vs. the more realistic empirical model spectra (which they are not). So when we apply the source terms to the empirical spectra, it is not representative of what goes on when the numerical models are applied. The former is a useful exercise, but results should be interpreted with a grain of salt. For example, most  $S_{\text{ds}}$  terms other than those from Hasselmann 1973 are highly nonlinear. We should not be surprised if the integrated dissipation that we get applying these  $S_{\text{ds}}$  terms to empirical models differ by a factor of 3 or 5 vs. the integrated dissipation that we get from applying these  $S_{\text{ds}}$  terms within a numerical model.

I like the discussion of modulation instability. However, I found it to be a bit of a way of "dressing up" what the author wants to do, which is to add an energy flux from high-to-low frequencies, and "this explanation is as good as any". The text is honest enough about this, so it's not a problem for the paper.

Lines 70-80. Numerical models do not always reach a steady state in such experiments. In fact, perhaps only a minority of them do. Those that don't will never be in balance, of course. The text does not contradict this comment, so it is probably OK.

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Generally, I was very pleased with the clarity of the paper. I missed some key points the first time through, but more careful reading cleared this up.

Comments which are suggestions for revisions:

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 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.

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

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.

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

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  $\gamma$ ?

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?

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

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bility...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.

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.

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.

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).

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.

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.

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

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