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The Cryosphere Discussions

Interactive comment on "Sea ice diffusion in the Arctic ice pack: a comparison between observed buoy trajectories and the neXtSIM and TOPAZ-CICE sea ice models" *by* P. Rampal et al.

Anonymous Referee #1

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The manuscript describes a sea-ice drift field analysis of two different sea ice model in the context of buoy drift data. The paper is clearly written (with a few smaller problems that I marked in the annotated PDF, e.g., sometimes the language is a little sloppy and has a colloquial tone not appropriate for a scientific text), but the scientific focus of the paper is not very clear. On the one hand, the "main goal" is to evaluate the models (and there are many figures comparing these models), but on the other hand most of the conclusions section focusses on the "secondary objective" to illustrate how statistics of sea ice drift can be useful. Frankly I find the "secondary objective" more interesting and scientifically better handled (although not much of that is really new) than the "main goal". The model results of the neXtSIM model are remarkable, but the

model comparison is biased and the conclusions that are drawn are likely to be either very specific or misleading. Therefore I recommend a major revision to redefine the focus of the paper properly (or change the weight given to the topics in the conclusions section) and change the nature of the model comparisons. I include an annotated PDF with notes and comments that I made while reading the text. They are meant as well-meaning suggestions (or can be found again below).

Details of my critique:

The model comparison is not very meaningful because the models start from different initial conditions, are driven by different atmospheric forcing and ocean conditions. In fact, all of these aspects tend to favor the neXtSIM simulation (initialization from observations rather than spinup, removed thickness bias, higher resolution forcing data, assimilated surface ocean) with respect to realism, so that I find the conclusion that the neXtSIM model performs better very much confounded by the totally different initial (important for a short integration period) and boundary conditions. At the same time the text seems to try to "sell" neXtSIM, which appears to inappropriate for a scientific paper. E.g. I can clearly see from Fig6+7 that neXtSIM statistics are better that those for TOPAZ, but at least in Fig7, the performance is not as great as the authors are trying to make me think (l382/Fig7: "follow an exponential distribution" may be true below 30cm/s, although it's hard to know if this significant, but clearly it is as bad as TOPAZ above 30cm/s which goes almost unmentioned in the text, or: On page 13, ll388 the authors discuss the initial and boundary value issues and acknowledge them as the weak point in the comparison, but still state that they expect TOPAZ to be a reasonable reference for other models, which is not based on any evidence. From Bouillon et al (2013), we know that standard EVP is not getting it right.

neXtSIM is said to be tuned to fit observations but TOPAZ uses a drag coefficient that leads to too high drift speed? This adds to the non-comparability of the models. The first thing I would have tried is to reduce the TOPAZ drag to 1e-3 to reduce the drift speed bias. Or use the re-analysis/assimilated solution of TOPAZ to begin with.

c_a = 0.0076 is a drag coefficent much higher than “standard” (although I acknowledge that c_a has a tuning range). And c_a has been measured or at least been inferred from observations, e.g., SHEBA observations have a mean of 1.7e-3, a general range is maybe between 1e-3 and 2e-3 (e.g. Nguyen et al 2011), maybe locally values of 5e-3 are OK, but such a high global value smells like a problem in the model (that is compensated by the high drag coefficient). I have had many discussions with meteorologists who work on atmosphere-ice-drag (both observations and atmospheric models) about the functional form and value of the drag coefficients and believe me, they would not accept drag coefficients outside the range of observations. It is surprising that neXtSIM still produces so slow drift. Should be discussed in a few sentences, what is compensated by this value (forcing?) and why nothing is done to adjust the TOPAZ value.

What’s wrong in summer in the model(s) that everything is focussed on winter? Summer is notoriously harder to simulate with “classic” models. Does neXtSim offer new opportunities or new problems? Is this discussed anywhere (in the Rampal et al TCD paper, doi:10.5194/tcd-9-5885-2015 ?)?, otherwise it would be good to say a little about this somewhere in the text.

I suggest to either drop the comparison to TOPAZ, because it is unfairly couched, or repeat the comparison on “equal footings”.

The data analysis and suggestions for using ice drift statistics are interesting and appear useful, but it should be made more explicit, what is new compared to Rampal et al. (2009), and what it reproduced from Rampal et al. (2009). Sometimes, the buoy data analysis start unexpectedly, e.g. l397, and while most aspects are compared to model simulations, the result of Fig8 are not (why?).

Fig8, compared to Fig14 of Rampal et al (2009) has 10 times larger values of r’. Why is that so? As far as I can see, if really r’ =sqrt(<r’^2>) is plotted then, I would expect no larger differences of the geometrical sum than a factor of sqrt(2).

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2015-233/tc-2015-233-RC1-supplement.pdf

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