

Interactive
Comment

Interactive comment on “Cyclone impact on sea ice in the central Arctic Ocean: a statistical study” by A. Kriegsmann and B. Brümmer

O. Persson (Editor)

ola.persson@colorado.edu

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These comments were received from anonymous referee # 3 on 6/26/2013 after comments were closed, but before authors have responded to previous comments. These new comments could be of use to reviewers in preparing their response and manuscript modification to comments received prior to closing. O. Persson, handling editor

Comments from anonymous referee #3

Kriegsmann – sea ice and cyclones

This paper investigates the impact of cyclones on Arctic sea ice primarily through modeling and statistical analysis. The statistical compilations are novel and interesting. The approach could be a valuable contribution. The main difficulty is in evaluating the

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quality of the modeling results and assessing the significance of the statistical measures. The model is used to simulate ice motion, thickness and concentration, with the primary emphasis in this study on ice motion and concentration. A thorough evaluation of the model simulations is necessary.

Let's start with the ice motion. An abundant data set of ice velocity is available from the International Arctic Buoy Program. I caution however that there are bad data in this archive, particularly in 2008. Compute the annual correlation of the buoy velocities (away from the coasts) with the simulated velocities. . . correlations that are very bad. . . much worse than the bulk of the buoys. . . may represent bad data. I suspect a timing error in the buoy data. You might ask the administrator about it if you still find these errors. But the fact remains that the seasonal speed and turning bias of the model as well as the vector correlation need to be reported through comparisons with observations. Perhaps you have already done this.

Ice concentration errors also need to be evaluated more systematically. You have a plot of the differences with AMSR-E ice concentrations but attribute the errors to the observations, not the model. Why? What are the correlations of the simulated ice concentration with the observed concentration by season? Where are the AMSR-E ice concentrations obtained from? Finally, the changes in ice concentration are reported in percent to two decimal places. . . how do justify this number of significant figures?

Finally, the model provides estimates of the ice deformation that is at the root of any changes in ice concentration. I would recommend showing composite maps of the mean ice divergence and mean shear magnitude instead of the turning angles and wind factors.

Other comments:

Page 1142, Line 16: see the recent paper Zhang et al. 2013: The impact of an intense summer cyclone on 2012 Arctic sea ice retreat. Geophys. Res. Lett., 40, , doi: 10.1002/grl.50190.

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Page 1145, Line 10: State that the model has no ice thickness distribution. (I think that is right.). This is important for interpreting the results. In particular, how quickly can thin ice form in leads and the ice concentration increase if there is only one thickness class? If there is a thickness distribution, how is it defined?

Page 1146, Line 13: what kind of weighting?

Line 15: Is the intensity the average of the Laplacian or the maximum?

Line 18: 7987 “distinct” cyclones

Page 1149, Line 1: Explain what parameters in the model establish this angle and the wind factor. How are these parameters chosen? Consider replacing Fig. 4 with a composite of the divergence and another of the shear.

Line 6: This is a flawed argument. In steady state free drift the water stress equals the air stress, so $U_{ice} / U_{10} = \sqrt{\rho_{air} C_{air} / \rho_{water} C_{water}}$ and the ratio is nearly constant.

Line 27: area – > location ?, radius at the detection time? I don’t think Fig 5 and this discussion adds anything useful to the paper. You might consider dropping it.

Page 1152, Line 15: What are these angles based on?

Page 1153, Line 2: You might give the same statistics for the region as a whole so we can see the relative magnitude of the seasonal trends.

Page 1154, Line 1: What is the implication here of using a 2-layer model?

Page 1157, Line 27: again, see Zhang et al (2013) for another point of view.

Table 1: Add “distinct” cyclones

Table 2: Add intensity to the season and region parts of the table.

Fig. 1: Rotate the map so it is aligned with the other figures (positive x to the right).

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Fig. 2b: Add ± 0.1 lines to show the data excluded by the filter.

Fig. 3: I can't see the vectors very well. Subsample the vectors and make them much larger. The ice motion is not really needed, just say it is approximately proportional to the wind.

Fig. 4: It would be much more informative to see figures of the ice divergence and maybe the shear magnitude. These are the processes that control changes in the ice concentration and ridging. Same for the seasonal and regional figures.

Fig. 11: Nice figure, but the seasonal trends should be removed in the bottom figure to make it clearer what the impact of the cyclones are apart from the seasonal trends.

Fig. 12: Add "for different regions" in the caption. I'm not sure you need the median values written out.

Interactive comment on The Cryosphere Discuss., 7, 1141, 2013.

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