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Comment

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

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The manuscript addresses the impact of cyclones on sea ice in the central Arctic. The study is based on an extensive set of material, including ECMWF operational analyses, AMSR-E remote sensing data on sea ice concentration, and experiments applying a coupled sea ice – ocean model. The authors have carried out a remarkable work, and reached important results. I believe that such a statistical approach is among the best ways forward to better understand cyclone effects on sea ice. The manuscript has, however, also weaknesses, specified below, and I recommend that major revisions should be made before it can be accepted for publication in The Cryosphere.

Major comments

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1. The authors have excellent knowledge on Arctic cyclones, but their experience on sea ice dynamics seems not quite as good. This weakens the physical interpretation of the results.

a) In free-drift conditions (no ocean current and no internal resistance of the ice field) the deviation angle between the wind and ice drift vectors depends on the ice thickness. This is not mentioned anywhere, but it might explain some of the variations the authors found in the deviation angle. See also line 15 in Conclusions: the deviation angle does not depend on the cyclone intensity, because it is controlled by the ice thickness.

b) The authors do not pay enough attention on the effect of the ocean current on the wind factor (when the factor is simply calculated as a ratio of the ice drift speed and wind speed). The Transpolar Drift Stream (TDS) probably has a strong effect on the wind factor (Vihma et al., 2012). In particular, during the study period the wind was often aligned with TDS, which favours a large wind factor. In Conclusions (page 1157, lines 7-8) the authors state that the main reason for regional differences in the wind factor is differences in ice thickness. I think that the main reasons are related to the ocean current and ice concentration, the latter being important for the internal ice resistance.

c) The authors' reasoning on the linear dependence of the wind factor on wind speed is incorrect (page 1149, lines 6-12). In stationary free-drift conditions for very thin ice, the air-ice stress balances the ice-water stress and the wind factor simply depends on the square root of the ratio of air-ice and ice-water drag coefficients multiplied by densities (e.g. Vihma and Launiainen, 1993). With thicker ice (coriolis force involved), ocean current, and internal resistance of the ice field, the situation becomes more complicated, but certainly there is not any linear dependence of the wind factor on wind speed.

2. The manuscript is entitled "Cyclone impact on sea ice in the central Arctic Ocean: a statistical study", but the authors only address the dynamic impact of cyclones. Cy-

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clones also have a strong thermodynamic impact on sea ice, as cyclones are responsible for a major part of advection of warm and moist air masses from lower latitudes to the Arctic. The authors should make it clear that cyclone impacts on sea ice are not restricted to the dynamic impacts addressed in this study.

3. Both in the Abstract and Conclusions the authors state that the manuscript is the first statistical study on cyclone impacts on sea ice in the Arctic Ocean. This may be true, but it would be relevant to mention that an extensive statistical study on cyclone impacts on sea ice in the Antarctic has been carried out by Uotila et al. (2011).

4. I am not sure if it is good to merge together results of cyclones of all scales, as the authors do when showing the figures with normalized cyclone radius. Uotila et al. (2011) showed that the interaction between cyclones and sea ice is different for small and large cyclones.

5. To better understand how representative the results are, the study period of 2006–2008 should be described with respect to large-scale circulation (AO, NAO, Arctic Dipole), cyclone activity, and sea ice conditions.

6. High-resolution fields of sea ice drift, based on satellite remote sensing, are available for recent years (see e.g. <http://arctic-roos.org/observations>). I am not sure if such products would have allowed as comprehensive study as the authors made, but this could be briefly discussed in the manuscript.

Minor comments

7. Page 1144, lines 10–13: the results of Screen et al. (2011) appear somewhat contradictory to the results presented in the previous paragraphs. Some more explanation would help a reader.

8. Page 1145, line 13: make clear that you use ECMWF operational analyses instead of reanalysis. The choice is good.

9. Page 1145, line 15–16: in addition to precipitation, also the ECMWF wind stress is

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based on forecasts.

10. Page 1145, line 20: replace “next” by “nearest”, I guess.

11. Page 1146, line 2: two-dimensional field of sea level pressure

12. Page 1150, lines 9-20: The text should be clarified. In two sentences the authors refer to a cyclone that approaches the place of detection. How can we speak about a cyclone approaching some place, if the cyclone is not yet detected?

13. When writing about the highest cyclone activity in summer in the study region, it would be good to mention that further south, around 70N, the highest cyclone activity occurs in winter (e.g. Sorteberg and Walsh, 2008), just to provide a more comprehensive picture of Arctic cyclones.

14. Page 1155, lines 3-4: I don't get this impression from Figure 13 (top panel).

15. Page 1156, line 9, and throughout the manuscript: I suggest replacing “angle difference” by “deviation angle”.

16. End of Conclusions: I fully agree on the importance of the individual storm in August 2012, but I am not convinced about the general importance of summer storms, as they are so rare (although summer cyclones are common). See e.g. Vihma et al. (2008; their Figure 2f) for the diurnal mean summer wind speeds observed at the Russian drifting stations, SHEBA, and Tara.

References

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