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The Cryosphere Discussions

Interactive comment on "Variability of sea ice deformation rates in the Arctic and their relationship with basin-scale wind forcing" *by* A. Herman and O. Glowacki

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Overall I am very glad to see that the analysis in this paper has been done. The trends and variability in deformation rate you document compliment results found independently by Pierre Rampal. The seasonal cycle in deformation has been documented by others, I see you mention Kwok's, Lindsay's and Stern's work, and I believe Thorndike's work should referenced with regard to this. The correlation of deformation variance to wind stress which is changing in time is very interesting and a result worth publishing.

Your results are dependent upon the spatial and temporal scales you average over. At small spatial scales, I find that deformation becomes less coherent with surrounding ice

and wind. On larger scales, as you approach the synoptic scale coherence appears to be an emergent property of the ice pack deformation (see for example Hutchings et al. 2011, Hutchings et al. 2012). I think you should make it clear in your abstract what spatial and temporal scales your analysis is performed on. The fact that you are performing analysis on basin averaged deformation and wind stress should be clear up-front.

The force balance model in the paper is overly simplistic. Though this is your point, as you want to describe how much of the observed behavior can be described with this simple model. It basically states that deformation rate decreases linearly with increasing ice strength, and wind forces the system. Of course this model is going to represent the data if you consider the data to be part trend (ice strengthening over winter) and variance due to wind forcing. I feel your discussion could be improved by outlining how this simple model is equivalent to the first order behaviour of more complicated full force balance models of the ice pack. For example, do we expect an appropriate rheological model would be linear? Is this a reasonable first order simplification? (Incidentally, I believe it is for your needs - you just need to justify this). How much of the observed trend can be explained by this model?

If you consider the Hibler's VP model rather than Girard's EB model, you will find that to first order the internal ice stress term is not proportional to total strain rate, but has a stronger dependence on divergence. Divergence, shear and total deformation do not have the same scaling and statistical properties, so you might find a different result if you consider divergence rather than total strain rate.

Can you also interpret your results in terms of how much deformation signal does the wind stress variance explain, and how much is explained by the seasonal trend?

Overall, your model describes what is commonly understood about the sea ice momentum balance. Hence I believe that a more exhaustive literature survey on the topic of the magnitude of forces in the ice pack and models would help you justify this model.

I am generally in agreement with you that most of the variance in divergence, at the large scales you consider, and as it is explained by wind variance and a seasonal trend, can be describe by a model similar to equation (7). I do feel however that more detailed justification for this choice of model would help a reader less familier with sea ice modelling.

One small point I would caution you on is that coastal impact on the sea ice stress state, and hence deformation, can be large. Do you see improved correlation between deformation and wind if you reduce your domain to a region in the central Arctic?

Another point you might bring up in your discussion is that your results indicate that the ice pack is moving faster under the same wind stress in recent years. Though your results are not statistically significant, they do still indicate an increasing wind factor, which together with the observation of increasing inertial motion indicates a weaker ice pack.

Specific Comments:

Regarding paragraph 15: I would disagree that our knowledge underlying mechanisms governing sea ice deformation remains far from satisfactory. This statement ignores the understanding that has come from the work of Bazant, Schulson, Weiss and others. The questions are a little more subtle than this statement suggests.

Interactive comment on The Cryosphere Discuss., 6, 3349, 2012.