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The Cryosphere Discussions

Interactive comment on "A lead-width distribution for Antarctic sea ice: a case study for the Weddell Sea with high resolution Sentinel-2 images" *by* Marek Muchow et al.

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Dear Mr. Muchow, Dr. Schmitt and Lars,

I have taken over the editing process for your paper from Yvegenny Aksenov. Thank you for your contribution. Measuring lead widths and understanding their distribution is important for improving model representation of ocean-atmosphere fluxes. Studies like yours are valuable, and I hope you can see from the comments on the paper that your work might be a valuable contribution, providing high-resolution data where it has not been previously available. However there are some significant issues in the methodology and reporting that I hope you are able to address.

Two reviewers have provided a critique of the paper, and I invite you to respond to their reviews. There are specific points made that must be addressed. The paper could do with some rewriting to improve clarity. I agree that there are places the language can be improved for accuracy, so that your methodology and results are clear. I provide some examples of where significant language improvements are required, as do the reviewers. I would highly recommend asking an English speaking colleague to proof read the paper and point out where your language is either overly technical (and therefore obtuse to the layreader) or where you could clarify your point.

The first paragraph of the paper demonstrates some of the language that could be tightened and "Leads are created by dynamic motions of the sea ice (Miles and Barry, 1998) and thus follow a linear-like shape" Please rephrase, this is not correct.

"For parametrization of the lead width lead-width distributions are used." This could be clarified somewhat.

"Shear and divergence rates, estimated from models and satellite observations, follow a power law for the Arctic sea ice (e.g. Girard et al., 2009; Marsan et al., 2004), which suggests that a power law can also be used for the statistical parametrization of the lead width." Perhaps this paragraph would read more clearly if the second part was before this sentence. The mixing of observation types and models then makes the opening to the next paragraph confusing. There you want to clarify you are only talking about lead width observations, rather than "these studies".

Specify the wavelength of band 4.

"For the simplification of the problem only cloud-free Level-1C products were used". Can you even use your method for cloudy images. I think the statement "For the simplification" is not the correct phrase here.

"The goal of the classification is to get thresholds for different surface types". I think "get" is not the correct word to use here. What parameters are you identifying thresh-

olds in? First state that the method is classification of surface types using x and y data. Then point out that you look for thresholds in band 4 reflectance that distingous different the surface types.

"Therefore, nine out of 20 later used Sentinel-2". Therefore is not the correct word to use here. There is no follow on from the previous sentence.

I agree with the reviewers that the methods section would benefit from some additional figures to demonstrate the procedure and how thresholds are identified. Some particular comments: Where do the values in Table 1 come from? Is your Gaussian fit appropriate? Are the thresholds consistent across images with different sun and look angles? I also agree that you can clarify the methods description. For example you could describe what the apparent width is succintly, and how this differs from the true width.

Figures 4 and 5 lead me to wonder if the apparent lead width can be accurately measured down to 10m. There is an arc is the data points, with the gradient becoming less steep towards smaller lead widths, which could be indicative of white noise. I echo the reviewers interest in a more detailed discussion of the errors introduced in the method.

Can you describe the reasons for the differences in LF and MF fit based on their weighting of the data? It might be more interesting to consider if the fit is statistically significant.

"Based on our current knowledge, previous studies (Table 2) focused on different regions in the Arctic and not on Antarctic regions." Here "Based on our current knowledge" is not needed in the sentence.

When comparing to other studies in the Arctic it should be clarified that you are not comparing like method for like. In particular the different sensors have different spatial resolution, which will constrain the range over which the power law fit is made. If you consider how there appears to be a decrease in exponent at smaller scales in your

data, would this explain any of the differences to previous studies with lower resolution sensors?

I think your study highlights the need for more studies like yours, providing more data on lead width. It is confusing that there is so much spread in power law exponents between these studies, and whether this is related to differences in lead detection method and the physical processes controlling lead width is hard to determine.

Finally, it is my understanding that The Cryosphere accepts short contributions. However, unlike one reviewer, I believe your paper could be strengthen by including a little more content in the methodology, to ensure clarity of your approach and reproducibility. Including a consideration of errors and the robustness of your results and whether the fits are significant would greatly improve the paper. You could also provide more guidence in the discussion to address the reviewers concerns regarding the scientific contribution you make.

At this stage I believe the paper requires major revisions, however the revisions might change the main findings which would require a second review of the paper and further consideration. Please let me know if you need any clarity on this. And I look forward to your response to the reviews.

With very best regards. Jenny

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