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The Cryosphere Discussions

Interactive comment on "The potential of sea ice leads as a predictor for seasonal Arctic sea ice extent prediction" *by* Yuanyuan Zhang et al.

Anonymous Referee #1

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The manuscript investigates a linear regression between the area of Arctic sea-ice leads in January to April and pan-Arctic and regional monthly-mean sea-ice extent in July to September. The sea-ice lead area (SILA) is derived by the authors from MODIS satellite infrared observations and covers the years 2003-2015, and the sea-ice extent (SIE) is provided by satellite passive microwave. The authors find that the January-to-April SILA is significantly correlated with the July pan-Arctic SIE, but not with the August or September SIE. If SILA and SIE are restricted to the sector 15W - 135E, correlations of SILA and SIE are significant for both July and August. The authors claim that these correlations can be exploited to accurately predict summer SIE from late-winter SILA, which they present as the main conclusion of the manuscript.

The manuscript makes some interesting points about the statistical relation between

the area of Arctic sea-ice leads (SILA) observed in winter and the Arctic sea-ice extent observed in the following summer. As a side effect, it also makes statements about interannual variability and trends of SILA that are worth recording. The results are well presented, with clearly structured and text and high-quality figures. I particularly enjoyed reading the introduction which gives a good overview over related literature. However, there are some doubts about the validity of the main conclusions drawn in the manuscript, because (1) there clearly are ambiguities with the calculation of SILA from the infra-red observational data set that are not mentioned at all in the manuscript, (2) only very vague explanations of physical mechanisms are offered that can plausibly explain the correlation between SILA and SIE, and (3) the prediction results (right column of Figure 5) to me seem overly confident. I will detail my concerns (1) - (3) in the general comments below. I would recommend publication of this interesting manuscript after a substantial revision that fully addresses these doubts and convinces me of the validity of the main conclusions.

General comments:

1a) MODIS infrared observations of the surface are only availabe under cloud-free conditions. Therefore, it is potentially misleading to directly calculate the pan-Arctic or regional area of sea-ice leads from the gridded observational product as done by the authors. A brief look at one season of daily gridded maps of the sea-ice lead data product reveals that a large fraction of the sea-ice covered area is obscured by clouds for almost every day, and as expected there are large day-to-day variations in the cloud cover. Therefore, it is not clear at all how SILA as calculated by the authors relates to the area of actually present sea-ice leads. What if the year-to-year variability of SILA shown in Figure 2a is actually dominated by the variability in cloud cover obscuring a constant actual lead area to varying degrees? Varying cloud cover would be an alternative explanation for varying summer ice extent, because winter-time clouds keep the surface warm and inhibit sea-ice growth. The role of clouds needs to be properly discussed before a robust conclusion about the lead area can be drawn.

1b) It is also evident from the gridded maps of the sea-ice lead product that polynyas and the marginal ice zone in the Atlantic sector are wrongly classified as leads. It might well be that year-to-year variability in the area of polynyas and the width of the marginal ice zone in the Atlantic sector is responsible for the year-to-year variability in the SILA calculated by the authors. This would then invalidate their main conclusion as it is specific to sea-ice leads. Please provide some further analysis that quantifies how much of the SILA signal comes from polynyas and the Atlantic marginal ice zone.

2) Related to point (1) above, it would make the author's main conclusions more credible if they were supported by independent observational data, modelling results, or process studies. I would leave it up to the authors to decide what is most appropriate. An idea would be to have a look at observational products of cloud cover on the one hand, and an observational product of winds and sea-ice drift on the other hand. The first is important for thermodynamic ice growth, the second for the creation of leads. From studying the inter-annual variability of clouds, sea-ice drift and winds, some support or additional doubt could be derived regarding the author's main conclusions.

3) I am a bit sceptical about the skill achieved in "prediction mode" as shown in the right-hand column of Figure 5. For example, the forecasts shown in Figure 5e are almost identical to the regressed values shown in Figure 5a. This is surprising given the moderate amount of correlation in the time series used to construct the linear predictor. Could it be that the authors accidentally used the complete time series to construct the linear predictor, rather than only the first 6 years? Can the authors please check their analysis and provide further evidence that the prediction results in Figures 5e-g have been calculated exactly as described in the text?

4) The Data Section needs a more detailed description of the MODIS sea-ice leads data set. This description needs to also discuss the limitations and assumptions of the data set. This comment is related to points 1a) and 1b) above. Furthermore, I would suggest to rename the section to "Data and Methods" and move lines 9-16 of page 4 to that section. The description of how the SILA is calculated needs to include more

details on how clouds and artifacts in the observational data set are treated.

Specific comments:

1) In the title, the last word "prediction" is a duplication of "predictor" and needs to be removed. "seasonal" should be replaced by "summer", because only the months July-September are considered. 2) In the abstract, line 14, the wording "accurately predicted" is subjective and ambiguous. Please provide numbers. 3) The quantity defined on page 7 is not a forecast skill, but rather a potential forecast skill. A forecast skill (score) is always based on comparing the skill of the forecast with the skill of a reference forecast (e.g. climatology, or a linear trend forecast). I would suggest that in this case comparison with a linear-trend would be appropriate, e.g. S = 1 - RMSE(SILA regression) / RMSE(trend). See Jollife and Stephenson (2012) for an introduction into forecast verification.

References:

Jolliffe, I. T., & Stephenson, D. B. (Eds.). (2012). Forecast Verification: A Practicioner's Guide in Atmospheric Science. Wiley-Blackwell.

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