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Interactive comment on "Prediction of monthly Arctic sea ice concentration using satellite and reanalysis data based on convolutional neural networks" *by* Young Jun Kim et al.

Anonymous Referee #1

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Main comments:

(1) On the choices of SIC predictors, I think more justification should be provided regarding why all of these predictors are needed. I would argue that they are not.

From my understanding of the model and intuition of the problem, the SIC information from the year before should only be contributing information to the statistical model regarding the trend in observed SIC – i.e. there's no physical explanation why these interannual variations would contribute to skill one year in advance. However, sic_1y and ano_1y both contain interannual variability information as well. Those fluctuations are just noise in this case and very likely resulting in over-fitting. I would suggest

replacing both of these predictors simply by the observed trend, and see how this affects the model results. If they are kept, these issues should be addressed.

Second, what is the reasoning behind using both sic_1m and ano_1m? Apart from the baseline used to compute anom_1m, they should be perfectly correlated with one another, which introduces the problem of multicollinearity. Have the authors tried only using anom_1m and not sic_1m? Again, if both are kept these issues should be addressed.

(2) On the simple statistical model as reference:

The use of a yearly trend extrapolation as a reference forecast is a bit conservative for the one-month lead time forecast, since there is typically high autocorrelation at a lag of one month for SIC (even after accounting for the long-term trend). A more robust reference, and what is commonly used, is an anomaly persistence forecast, or a damped anomaly persistence forecast (e.g. see Wang et al 2016). Ideally this would be done by persisting anom_1m one month ahead and adding it to the observed trend calculated for that month (as opposed to climatology as done in Wang et al 2016).

(3) Fig. 2 - it would be helpful to split these up for the melt and freeze seasons. Showing the annual mean makes it difficult to interpret the figure.

(4) Why are only error/bias metrics considered? Include a figure analogous to Fig. 2 showing maps of anomaly correlation for the de-trended predictions relative to de-trended obs would provide a more comprehensive analysis.

(5) The paper needs to be reviewed carefully for proper use of the english language. There are several spelling and grammatical errors.

Minor comments: L35-40: "Arctic sea ice has been rapidly declining, which impacts not only the Arctic climate, but also mid-latitudes (Yu et al., 2017)" should say "... but also possibly the mid-latitudes". There is not yet a consensus on this matter.

L40: Usually we say "projecting climate change"; "forecasting" refers to an initial-value problem.

L 40-45: Should reference Drobot et al, 2003, 2006; Lindsay et al. 2008; and Wang et al. 2016 for statistical predictions of sea ice concentration.

L55-60: “Studies regarding short-term sea ice forecasting have received relatively little attention (Grumbine, 1998; Preller and Posey, 1989).” Specify that you’re referring to machine learning predictions; studies have considered this with classic statistics models (e.g. references on previous comment).

L60: “SIC describes the amount of the sea that is covered by ice”. ’The sea’ is poorly defined; better to say “SIC describes the fraction of a specified area (typically a grid cell) covered by sea ice”.

L185: eqs 1-4; please expand on how these error metrics are computed with respect to space and time. I would think that the spatial averaging would have to be done before any temporal averaging in order for the mask (based on the previous nine years w.r.t the forecast month) to be applied effectively. . . Is this correct?

L250: “The model did not catch well the decreasing trends of sea ice due to global warming.” The model is a linear trend, so that’s exactly what it should be doing unless the actual trend is accelerating. I would think It’s more likely that the trend forecast is showing 100% SIC in the central Arctic in years like 2007 and 2012 when sea ice retreated further north than the marginal ice zone predicted by the trend. However, the fact that high-SIC values that are less than 100% do not show this bias in Fig. 3a is highly suspect. Are the authors sure an error hasn’t been made either making the figure or in calculating the reference forecast?

Fig. 5: Was the impulse noise (i.e. setting to zero) used on the predictors over the training sample and the testing (i.e. forecast) sample? Are the predictions for 2007 and 2012 with impulses on the SIC variables (panels b and c) just that there is no sea ice (all very negative values)? If the impulses are applied over the training data, it’s

hard to imagine why the remaining variables wouldn’t be capable of “creating ice” in the model, even if it’s just a climatology.

References: Drobot, S. (2003). Long-range statistical forecasting of ice severity in the Beaufort–Chukchi Sea. Weather and Forecasting, 18(6), 1161-1176.

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Lindsay, R. W., Zhang, J., Schweiger, A. J., & Steele, M. A. (2008). Seasonal predictions of ice extent in the Arctic Ocean. Journal of Geophysical Research: Oceans, 113(C2).

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