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Discussions
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# *Interactive comment on* "Going with the floe: tracking CESM Large Ensemble sea ice in the Arctic provides context for ship-based observations" *by* Alice K. DuVivier et al.

**Anonymous Referee #2**

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— Summary

DuVivier et al. present results that shed light on possible trajectories and corresponding ice conditions that the upcoming year-long Arctic drift campaign MOSAiC will encounter. Ensembles of trajectories are based mainly on CESM model data from different periods corresponding to past (perennial ice) versus present/near-future (seasonal ice) climate states, and to some extent also on satellite-derived drift data from the past few decades. The authors compute probabilities for various scenarios, such as to end up in the Beaufort Gyre, and present some conclusions that are potentially relevant for the planning of the campaign.

The paper is well-written, scientifically solid, and contains appropriate high-quality figures. I have one main remark relating to the fact that the authors have failed to include reference to similar work carried out by MOSAiC planners, which should certainly be taken into account. Apart from that I have only relatively minor suggestions for how the paper can be improved in my view. Overall, I recommend publication in The Cryosphere subject to minor-to-major revisions.

— Main point

The authors should include reference to and relate their work to similar work that has been carried out by the MOSAiC planners. While this work has to my knowledge not been published in a peer-reviewed journal, some of it has been published as "grey literature" in the MOSAiC Implementation Plan, version April 2018 (see link below). In Section 4.2 "Drift Trajectory and Re-supply", results for trajectories based on satellite-derived drift, similar to the satellite approach presented by DuVivier et al., are presented and probabilities of drifting into the Russian EEZ, into the "pole hole", the probability of "melting out", the expected drift distance etc. for different start positions are derived and discussed.

The present study by DuVivier et al. adds additional insight, in particular by extending the methodology to an ensemble of model trajectories and by studying present versus past conditions, so the paper is certainly a valuable addition. Nevertheless I consider it very important - and fair toward the MOSAiC planners - to refer to their work and to relate the present results to those contained in the MOSAiC Implementation Plan.

MOSAiC Implementation Plan: https://www.mosaic-expedition.org/fileadmin/user_upload/MOSAiC/Documents/MOSAiC_Implementation_Plan_April2018.pdf

— Other specific points

* There are some minor language errors distributed over the text, so I recommend a careful language check before final publication.





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Discussion paper

* P1L22: "We find that sea ice predictability emerges rapidly during the autumn freeze-up" (and corresponding parts of the main text related to predictability): By using only the autocorrelation relative to the initial state to quantify this, only the type of predictability related to the persistence of each single quantity is accounted for. However, predictability that is carried into the future through persistence of other quantities, conditions at other locations, and/or any non-persistence type of predictability is neglected. I therefore suspect that the sudden jump in "predictability" from initial states at the beginning versus the middle of October might be an artifact caused by the use of autocorrelation to measure predictability. In other words, I could well imagine that such a jump would be absent or less pronounced if predictability were quantified by means of "perfect-model" ensemble simulations initialised at those times of the year. I am not suggesting to do such additional simulations, but I recommend to mention the limitations of autocorrelation to quantify predictability and to formulate the result of a "rapid emergence of predictability" more cautiously.

* P1L30: Can't the reference to personal communication be replaced by a usual reference? There should be numerous up-to-date sources of the pan-Arctic sea ice extent that can be cited.

* P2L47: "These observations ...": which?

* P3L88-89: "For the satellite-derived drifts, more recent years tend to have longer drift distances (not shown)" (and other statements related to the first versus second half of the satellite period): I suggest to add two more columns to the table to show results for the first and second half of the satellite period.

* Second half of rows of Table 1: It would be more straight forward to compare the numbers if they were given as "percent" instead of number of tracks.

* The authors might consider adding the supplementary figure to the main paper; I think the figure is sufficiently interesting for the main part, and then no supplementary material would be required anymore.

* P4L119-120: "when the field experiment might be observable by satellites": The size of the pole hole is different for different satellites, which might be worthwile mentioning.

* The way they are computed by connecting modal values of spatial probability distributions, how different are the "most likely tracks" from tracks one would get if the ensemble centroids were used? Also, I am wondering how robust those modal-value based tracks are? E.g., in the example shown in Fig. S1-b, there are three distinct maxima, so the location of the modal value might easily jump from one such "hot spot" to another, no?

* Related to the former point, one could relatively easily extend the sample size of the model trajectories by using a few years before and after the single years currently used for "Seasonal" and "Perennial"; the "climate change" occuring over a few years is arguably not too significant. Is there a particular reason why the authors have not extended the sample size in such a way?

* P6-7L182-184: "By calculating the autocorrelation coefficient between the 30 unique initial floe conditions and the subsequent conditions each following month throughout the year, we are able to explore how long there is predictability in the sea ice system based on the initial sea ice state": Here you mention that this measures only predictability "based on the initial sea ice state", but I recommend to state more explicitly that, by capturing the initial state much more completely and considering not only correlation, predictability metrics based on initialised ensembles (e.g. perfect-model type) could well be considerably higher and show a weaker or no jump in predictability from early to mid October initial conditions.

* P7L184-185: "In the CESM, the sea ice model represents subgrid-scale heterogeneity for five thickness categories": I suggest to include this and some other relevant information about the model, in particular the sea-ice component, in the "Data and Methods" section.

* P7L187-194: Could you comment on the significant negative correlations obtained in

particular for CONC2, Seasonal, Oct.15?

* I assume that the probabilities highlighted in the "Discussion and Conclusions" section are quite sensitive to slight model biases, in particular in terms of the Arctic ice drift pattern. I would thus recommend to include a corresponding note of caution and to also provide the corresponding numbers derived from the satellite-based drift (possibly only the second half of the period), which might serve to give a rough idea about uncertainty in these numbers.

* P8L234-235: "the emergence of predictability during the autumn freeze-up is not well understood and was unexpected": see my corresponding remarks above.

* Figures 1,2,3,6,S1: A general recommendation is to use colour-blind friendly colour bars. ColorBrewer is an excellent source for such colour bars.

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