

## ***Interactive comment on “Evaluation of Arctic sea-ice drift and its dependency on near-surface wind and sea-ice concentration and thickness in the coupled regional climate model HIRHAM-NAOSIM” by Xiaoyong Yu et al.***

### **Anonymous Referee #2**

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General description The Manuscript validates a fully coupled ocean, sea ice and atmospheric circulation model (HIRHAM-NAOSIM). Main focus is on the correlation between the sea-ice drift, sea-ice conditions and the near surface wind speed. The model is validated towards remotely sensed data and another model (PIOMAS). The latter is more a comparison than a validation.

Comparisons has been made at two time scales. First the seasonal variation has been compared, then the daily variations and correlations between mainly ice drift and wind speed in different sea ice regimes.

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Results are in general at the level of other model systems, however a few pointers are provided to places where HIRHAM-NAOSIM performs better than PIOMAS

Introduction of sea-ice form drag influences the drift speed but this does not improve the overall performance.

#### Mayor revisions/concerns

A paper that validates a model is of relevance, however it seems like there are many references to the 2019 paper by Dorn et al. Without having read this I am a little puzzled whether this manuscript is more of the same of if it points to new findings. Especially when tuning of the form drag is postponed to a later paper.

My main concern with this manuscript is that it presents many numbers and correlations but there is a lack of introduction, perspective and discussion. A few lines is mentioned in the end of section 4 where ocean forcing is mentioned. I think that this should be the start of a discussion that discuss the reasons why for instance the seasonal cycle is poorly represented. How well is the internal ice pressure described? Are observations always the truth? For instance what are the uncertainties/biases of the KIMURA dataset.

The article points to the lack of a seasonal cycle and a bad timing of the minimum. My opinion would be that the minimum is more a matter of lack of a seasonal cycle and that this is a random minimum that is irrelevant as long as the seasonal cycle is not present.

From line 52 and the next few lines a method for validation is mentioned. I would recommend to move this into section 2 and describe what this validation method do.

Some of the findings are close related. Higher ice drift will lead to lower ice thickness and again higher ice drift. Therefore a comparison with for instance PIOMAS tells you more about the current state of the model than a direct bias (at least that would be my opinion). The comparisons are valid but I will be hesitant to say that for instance the

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internal strength of the model is too weak. A relevant discussion related to PIOMAS would be to discuss the difference between a forced ocean-sea ice model and a fully coupled ocean-sea ice-atm model. Are there features that could be described by this?

Minor corrections Line 44: In my opinion the comparison of CMIP 3 models is outdated. The reference provided afterwards is more relevant (Tandon et al 2018).

Line 50: Please don't start the section with Thus. For instance change to: This paper/manuscript has two aims. Line 54. Stating that an observation is rare seems a bit short and subjective. They do exist (RGPS buoys, SAR drift), however these are not present for the entire period. Choosing not to use them is valid but again a few more lines on why would be nice.

Line 75: Replace with: The organization of this paper is as follows: Section 2.

Line 84 to 95: A map of the domain and the where the boundaries extend to would improve the understanding of the model domain.

Line 92 reference a dynamic-thermodynamic model described by Harder is an upgrade? What is upgraded. Dynamics are referenced to 1979 and thermodynamics to 1976. Maybe "update" should be removed or explicitly explained what is the update.

Line 104: How is the spinup designed? Running one year 22 times? Has the model bin spun up properly or is the ensemble a representation of the spinup? A bit more elaboration of the choices would be nice. Is Levitus data near the area of interest good enough? Does this imply that the variations seen only originates from the atmosphere?

Line 157: Validation towards AMSRE. Is the ice drift It would be interesting to see how the model performed vs RGPS buoys and Sentinel 1 SAR icedrift data. Alternatively an evaluation of the uncertainty of the chosen drift product versus the bias/uncertainty of the model results.

Line 162: As partly mentioned the comparison with PIOMAS just shows whether

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NAOSIM provides the same as PIOMAS. Why not use Icesat as mentioned in the discussion about PIOMAS. Admitted there are relatively high uncertainties on ice thickness products like IceSat, however reference a model and motivate this choice by its skill vs another product seems weird. Other data sets that can be used are operation ice bridge and Cryosat. They do not cover the full period and domain but they can do as Ground Truth.

Line 187 - 189. Is there a reason for excluding spring and fall?.

Line 211-216 Not sure why it is required to include such a long description of why sea ice drift is influenced by thickness, concentration and wind speed. This is stated in several articles. Just state that the drift is governed mainly by ice conditions, wind speed and ocean currents (less important).

Line 240 - Small variation of wind don't explain variation of ice drift. The modelled ice drift seems to be controlled mostly by the wind, however this is in contrast to obs.

245 - 250 Again too high correlated wind and ice drift speed in winter. Other factors/forcing of the dynamics of the sea-ice must impact. A discussion of these would be relevant in a discussion section.

Line 260. I thought that there is a dynamical forcing between ocean and sea ice everywhere. This should be more specific.

Line 300 what is the method? Short description please. Same reference is made in introduction

Line 350 Abrupt end to line.

Figure 4 and 5 are hard to read. Please increase font size

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-183>, 2019.

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