

Answer to the comments of Referee #2

We would like to thank Referee #2 for his/her suggestions for improving our paper. All the comments have been addressed and point by point response is provided below each comment. The reviewer comments are written in black, our answer in blue, and the corrections in the paper are highlighted in red. The line numbers which are used in the answers correspond to the new version of the manuscript (PDF file) unless otherwise indicated.

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.

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 or if it points to new findings. Especially when tuning of the form drag is postponed to a later paper.

The paper by Dorn et al. (2019) is cited as reference to the model description and to the long-term simulation setup (BASE run). The study in this paper evaluates sea-ice drift and its dependency on wind speed and sea-ice conditions, which was not addressed by Dorn et al. (2019). Therefore, this study is not a follow-up paper to Dorn et al. (2019), but presents exclusively new findings.

In addition, section 4 presents a sensitivity test for a new parameterization that added the sea-ice form drag from sea-ice edges. The focus is to investigate the sensitivity of simulated sea-ice drift to the new parameterization. We also added a new sub-section 4.2 and new figures to find whether the new parameterization improved the model simulations or not. Fine-tuning of the new parameterization is postponed, since it requires a lot of model simulations. Nevertheless, we added a new sub-section 4.3 that describes the ideas of which parameters could be tuned in the follow-up study to improve the model simulation.

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.

We agree that there should be more discussion on our results. We followed the Referee's suggestions and added the uncertainty of KIMURA dataset at line 202:

“The uncertainty of the KIMURA data over the Arctic is from 1.12 to 1.47 km d⁻¹ in summer and depends on the drift speed (Sumata et al., 2015a). In winter, the uncertainty is at least 50% smaller than in summer and depends on the drift speed too (Sumata et al., 2015b)”

We also included the discussion about the KIMURA sea-ice drift speed uncertainty and compared it with the model bias at line 276.

“Compared with the uncertainty in the KIMURA sea-ice drift speed provided by Sumata et al. (2015a, b), the model bias in summer is close to or slightly smaller than the uncertainty of the KIMURA data. This indicates that the sign of the model bias in summer SID is uncertain. In winter, however, the model clearly overestimates the SID over the central Arctic and north of the Canada Archipelago and Greenland, even if considering the uncertainty of the KIMURA data.”

We added a new paragraph to discuss the reason for wintertime SID overestimation at line 300:

“The overestimation of winter SID could be related to the underestimation of winter SIT (Figure S2). Besides, the prescribed values of the ice-ocean drag coefficient and the ice strength parameter could also play a role. The ice-ocean drag coefficient in the base configuration of HIRHAM-NAOSIM (5.5×10^{-3}) is comparable to other CMIP5 models, even though a few models use 3-4 times higher coefficients (see Tandon et al., 2018). Higher ice-ocean drag might damp the SID and its strong dependency on the wind speed. Docquier et al., (2017) show that the higher the ice strength parameter, the lower the winter SID and the higher the winter SIT. The ice strength parameter in the base configuration of HIRHAM-NAOSIM ($30,000 \text{ N m}^{-2}$) is already slightly higher than in all CMIP5 models (see Tandon et al., 2018). This value has been established for stand-alone ocean-ice simulations with daily wind forcing, but might still be too low considering the hourly wind forcing from the interactively coupled atmosphere.”

References:

Sumata, H., Kwok, R., Gerdes, R., Kauker, F., and Karcher, M.: Uncertainty of Arctic summer ice drift assessed by high-resolution SAR data, *Journal of Geophysical Research: Oceans*, 120, 5285-5301, 2015a.

Sumata, H., Gerdes, R., Kauker, F., and Karcher, M.: Empirical error functions for monthly mean Arctic sea-ice drift, *Journal of Geophysical Research: Oceans*, 120, 7450-7475, 10.1002/2015jc011151, 2015b.

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.

A seasonal cycle in the simulated SID is present, even if its amplitude is much weaker than in the KIMURA data due to the model's overestimation of winter SID. We agree that one may argue that it is irrelevant to discuss the minimum as long as the model systematically overestimates the SID just in the season where the observed minimum occurs. On the other hand, the simulated (bad) timing of the minimum is certainly not a random feature, since it appears in all ensemble members and in many other coupled climate models as well. Therefore, we think that it is relevant to point to this model deficit when discussing the seasonal cycle.

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.

We agree that the previous introduction of the validation method was insufficient and should be located in section 2. We added the following description about the validation method to Section 2.3 at line 233:

“Following Olason & Notz (2014) and Docquier et al. (2017), we use scatter plots showing Arctic basin wide and multi-year averaged monthly mean sea-ice drift speed against sea-ice conditions (sea-ice concentration and thickness) to evaluate the relationships between sea-ice drift speed and sea-ice conditions. The linear fit-lines are added in the scatter plots to assist the comparison of the relationship in the model simulations and in the observation/reanalysis.”

Further, we modified the sentence at line 52:

“We first evaluate the simulated Arctic basin-wide monthly mean drift, then we evaluate the relationship between sea-ice drift speed and sea-ice conditions/wind speed both on Arctic basin-wide, multi-year monthly mean scale and on daily grid scale.”

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 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 model ocean-sea ice-atm model. Are there features that could be described by this?

We agree that the comparison with PIOMAS sea-ice thickness only tells us something about the current state of the model. In order to clarify this, we added one sentence in section 2.2.3 at line 222:

“Therefore, the comparison with PIOMAS informs us rather about the current state of

the model than about a direct model bias.”

We also agree that a discussion of the difference between PIOMAS, a forced ocean-sea ice model, and HIRHAM-NAOSIM, a fully coupled atmosphere-ocean-sea ice model, and the possible contribution of this difference to the sea-ice thickness differences between PIOMAS and HIRHAM-NAOSIM should be provided. Therefore, we added the following discussion at line 281:

“The SIT differences between HIRHAM-NAOSIM and PIOMAS could be partly caused by the differences between a fully coupled atmosphere-ocean-sea ice model and a forced ocean-sea ice model. Only the former includes the feedback of the atmosphere to the sea-ice and ocean component.”

In addition to PIOMAS, we have considered to use Cryosat2 product for sea-ice thickness, but Cryosat2 is only available from 2010 onwards and does not cover the whole period 2003-2014. Nevertheless, we decided to add the comparison of sea-ice thickness from Cryosat2 and from the model simulations during winter 2010-2014 in supplementary Figure S2. It shows that the sea-ice thickness difference between Cryosat2 and the model is qualitatively similar to the difference between PIOMAS and the model. Therefore, we added the following discussion at line 284:

“Analysis of the SIT differences between HIRHAM-NAOSIM and CryoSat2 during winter 2010-2014 (Figure S2) confirms that HIRHAM-NAOSIM underestimates the SIT over the central Arctic and north of the Canada Archipelago and Greenland, at least in winter.”

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).

We agree that the CMIP3 results from Rampal et al. (2011) are not up to date anymore. Therefore, we removed the corresponding statement in Section 1.

Line 50: Please don't start the section with Thus. For instance change to: This paper/manuscript has two aims.

We follow your suggestion, and deleted “Thus” at the beginning of that paragraph.

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.

We agree and removed the phrase: “Since both model evaluations and observational studies based on the daily grid scale are rare”

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

We changed the text as suggested.

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

We understand the concern of the Referee about the model domain. However, as the model domain and the study area of interest (shown by the purple line) are already shown in Figure 1, we think there is no need to provide an additional figure. The domain of the ocean-sea ice component is exactly the domain shown in Figures 1, 3, 8, and 11. In the revised version, we added corresponding information to the respective figure captions. Further, we added the following sentence at the end of the model description section 2.1.1 (line 111):

“The ocean-sea ice domain corresponds to the domain shown in Figure 1.”

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.

We agree that the expression ‘upgrade’ is confusing. Therefore, we deleted the phrase “and represents an upgrade of the original Hibler (1979) model” in line 118 of the discussion paper and keep the sea-ice model reference of Harder et al. (1998). Compared to Hibler (1979), the sea-ice dynamics include an upstream advection scheme (to avoid negative ice thicknesses), no explicit diffusion, and drag coefficients optimized by comparison with observed buoy drift (Harder and Lemke, 1994; Fischer, 1995; Drinkwater et al., 1995; Harder, 1996; Kreyscher et al., 1997). As these improvements were already mentioned by Harder et al. (1998), we abstained from repeating it again in this paper.

References:

Drinkwater, M. R., Fischer, H., Kreyscher, M., & Harder, M. (1995, July). Comparison of seasonal sea-ice model results with satellite microwave data in the Weddell Sea. In 1995 International Geoscience and Remote Sensing Symposium, IGARSS'95. Quantitative Remote Sensing for Science and Applications (Vol. 1, pp. 357-359). IEEE.

Fischer, H. (1995). Vergleichende Untersuchungen eines optimierten dynamisch-thermodynamischen Meereismodells mit Beobachtungen im Weddellmeer= Comparison of an optimized dynamic-thermodynamic sea ice model with observations in the Weddell Sea. Berichte zur Polarforschung (Reports on Polar Research), 166.

Harder, M. (1994). Erweiterung eines dynamisch-thermodynamischen Meereismodells zur Erfassung deformierten Eises. Berichte aus dem Fachbereich Physik, Report, 50.

Harder, M. (1996). Dynamik, Rauigkeit und Alter des Meereises in der Arktis= numerische Untersuchungen mit einem großskaligen Modell= Dynamics, roughness, and age of Arctic sea ice-numerical investigations with a large-scale model (Doctoral dissertation, Universität Bremen).

Kreyscher, M., Harder, M., & Lemke, P. (1997). First results of the Sea-Ice Model

Intercomparison Project (SIMIP). *Annals of Glaciology*, 25, 8-11.

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?

The BASE ensemble simulations were already carried out for the study by Dorn et al. (2019). The design of the preceding spin-up simulation was described in detail by Dorn et al. (2019): “initial ocean and sea-ice fields were taken from the Januaries 1991 to 2000 of a preceding coupled spin-up run for the period 1979–2000. The coupled spin-up run already reached a quasi-stationary seasonal-cyclic state of equilibrium for the mid-1980s. Consequently, all ensemble members were initialized with ocean and sea-ice fields that represent the diversity of ocean–ice conditions within the steady state of the specific model configuration”.

To better emphasize that the BASE ensemble simulations, including the preceding spin-up simulation, were carried out for the study by Dorn et al. (2019), we reformulated the beginning of the paragraph at line 125:

“A 10-member ensemble of multi-decadal climate simulations for the period 1979–2016 were carried out by Dorn et al. (2019) with the base configuration of HIRHAM-NAOSIM 2.0. These multi-decadal ensemble simulations represent the basis for the present study and are referred to as BASE hereafter. The individual BASE ensemble members used the same atmospheric initialization, but applied different ice-ocean initial conditions, which were taken from January 1 of the last 10 years of a preceding 22-year-long coupled spin-up run (see Dorn et al., 2019, for more details).”

We also modified the description of the initial condition for CTRL and SENS simulations at line 192:

“The ice-ocean initial conditions for CTRL and SENS were produced in exactly the same way as for BASE.”

The Levitus climatology is only used at the open boundary in the northern North Atlantic (at approx. 50°N). Since this boundary is far away from the area of interest (our study domain in this paper), the Levitus data are not an issue for the present study. Even though there are no externally forced variations at the lateral ocean boundary, there are variations between the ensemble members as well as year-to-year variations. Variations between the ensemble members are by definition a result of internally generated variability in the model. This comprises both atmosphere and ice-ocean variability. In contrast, year-to-year variations in the ensemble mean can be attributed to the external forcing at the lateral atmospheric boundaries (and the surface boundary conditions outside the coupling domain).

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.

It is beyond of the scope of our study to evaluate KIMURA drift data against buoy and SAR drift data. Intercomparison studies of Arctic ice drift exist in literature. For example, Sumata et al. (2014) intercompared four remotely sensed ice-drift products (incl. KIMURA) and compared them also with available buoy data. Also, there is a whole international activity to validate sea-ice drift products (<http://esa-cci.nerisc.no/>, http://esa-cci.nerisc.no/?q=webfm_send/195). It is also beyond the scope of our study to evaluate the model with other data sets such as buoys and SAR data. We have justified in section 2.2.1 why we have selected the KIMURA data set. It is because it has a much wider spatial and temporal coverage than buoys data and is therefore appropriate for regional model evaluation (Sumata et al., 2015a). Another advantage of the KIMURA product is that it provides ice drift data both in winter and summer. More details are given by Kimura et al. (2013) and Sumata et al. (2015a).

Sumata, H., Lavergne, T., Girard-Ardhuin, F., Kimura, N., Tschudi, M. A., Kauker, F., Karcher, M., and Gerdes, R. (2014), An intercomparison of Arctic ice drift products to deduce uncertainty estimates, *J. Geophys. Res. Oceans*, 119, 4887– 4921, doi:10.1002/2013JC009724

According to the Referee's suggestion, we include information about the uncertainty of the KIMURA product and link that with the identified model bias.

We included at line 202:

“The uncertainty of the KIMURA data over the Arctic is from 1.12 to 1.47 km d-1 in summer and depends on the drift speed (Sumata et al., 2015a). In winter, the uncertainty is at least 50% smaller than in summer and depends on the drift speed too (Sumata et al., 2015b).”

And, we included at line 276:

“Compared with the uncertainty in KIMURA sea-ice drift speed provided by Sumata et al. (2015a, b), the model bias in summer is close to or slightly smaller than the uncertainty of the KIMURA data. This indicates that the sign of the model bias in summer SID is uncertain. In winter, however, the model clearly overestimates the SID over the central Arctic and north of the Canada Archipelago and Greenland, even if considering the uncertainty of the KIMURA data.”

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

We followed the Referee's suggestions and added the comparison of sea-ice thickness from the model and from Cryosat2 during 2010-2014 by extending supplementary

Figure 2 and Section 3.1 (line 284):

“Analysis of the SIT differences between HIRHAM-NAOSIM and CryoSat2 during 2010-2014 winter (Figure S2) confirms that HIRHAM-NAOSIM underestimates the SIT over the central Arctic and north of the Canada Archipelago and Greenland, at least in winter.”

The modified supplementary Figure 2 is as follow:

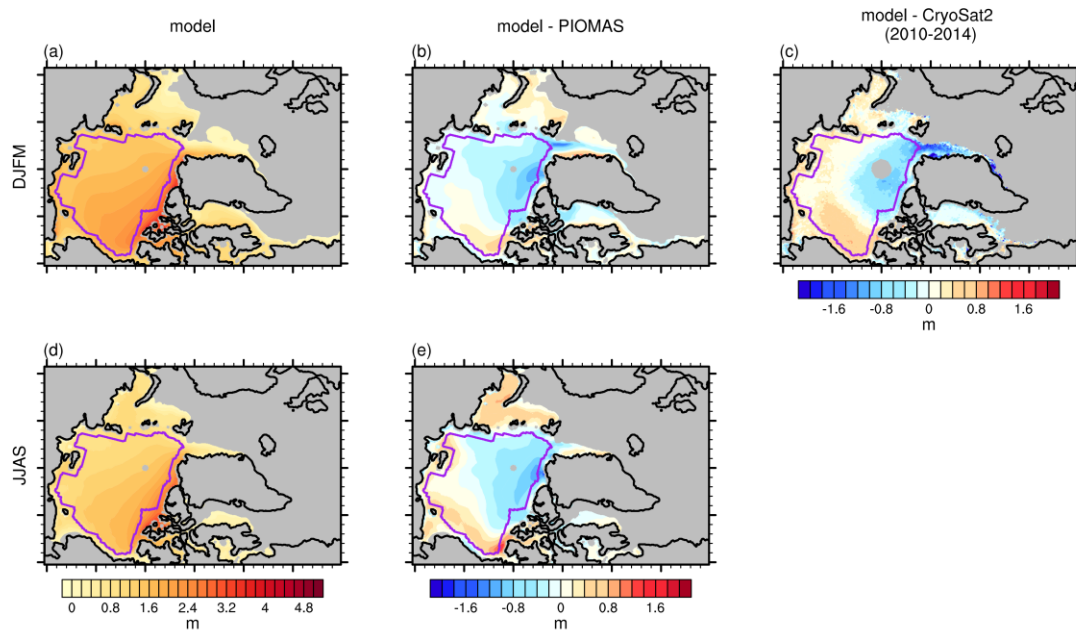


Figure S2: Mean spatial pattern of sea-ice thickness [m] in the model (ensemble mean) during 2003-2014 (a) winter (DJFM) and (d) summer (JJAS). (b) and (e) are the model differences to the PIOMAS (“Model - PIOMAS”) during winter and summer respectively. (c) are the model differences to the CryoSat2 sea-ice thickness in winter during 2010-2014. The purple line in each panel indicates the study domain used for the basin-wide analysis.

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

The reason for not showing the spring and fall is that we focused our study on the extreme seasons to emphasize the contrast between warm and cold conditions. Figures for spring and fall show intermediate results and do not provide additional insights.

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).

We agree that the previous description was unnecessarily long. We shorted the sentences at line 266 as follows:

“SID is mainly governed by near-surface wind, sea-ice conditions, and ocean currents.”

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.

We agree that the previous wording was misleading. We reworded the paragraph (lines 312) as follows:

“As shown in previous studies (Docquier et al., 2017; Kushner et al., 2018; Olason & Notz, 2014; Tandon et al., 2018), the observed distinct mean seasonal cycle of SID (maximum in autumn, minimum in spring) is obviously not solely controlled by the wind speed, which is strongest in winter and weakest in summer (Figure 2). The phase lag between the seasonal cycle of SID and WS is about 3-4 months in observations/reanalysis (KIMURA ice drift/ERA-I wind). The modeled seasonal cycle and magnitude of the WS agrees well with the ERA-I reanalysis. According to the delayed SID minimum (section 3.1), the phase lag between the simulated seasonal cycle of SID and WS is reduced to about one month, like in many CMIP3 and CMIP5 models (Rampal et al., 2011; Tandon et al., 2018), leading to a higher correlation between SID and WS. This indicates that the modeled SID is much stronger controlled by the wind speed than the observed SID.”

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.

We added a discussion of potential causes for the model bias to Section 3.1 and refer at the end of this paragraph to this discussion:

“As mentioned in Section 3.1, too high sensitivity of the SID to the wind in winter may be related to the underestimated SIT and model parameters governing the sea-ice dynamics.”

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

From a model perspective, ocean and sea ice are coupled throughout the model domain. Here we refer to a specific physical coupling process between ocean and sea ice. We agree that more information is helpful and modified the sentences at line 336:

“This could be the result of a dynamical coupling between sea ice and the coastal ocean as suggested by Nakayama et al. (2012): In a coastal ocean covered with sea ice, wind-forced sea-ice drift excites coastal trapped waves and generates fluctuating ocean currents. These ocean currents can enhance the sea-ice drift when the current direction is the same as the wind-driven drift direction.”

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

introduction

A description of the method was added to Section 2.3 at line 248 (see above). In this paragraph, we removed the citations and the introduction of the method. The remaining text at line 378 reads:

“Figure 6a shows the relationship between SID and SIC in terms of the mean seasonal cycle.”

Line 350 Abrupt end to line.

Thank you very much! The corrected sentence reads now:

“As pointed out by Olason & Notz (2014), the inverse correlation between drift speed and thickness in winter, when the ice concentration is high, is physically plausible, but the inverse correlation in summer, when the ice concentration is lower, is probably only of statistical nature.”

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

We agree that Figures 4 and 5 are hard to read. We enhanced the visibility of the two figures by rearrange the panels and increased the font size as suggested. Besides, we changed the wind class bin size to 2 m/s and the sea-ice fraction classes to (0,0.1], (0.1,0.3], (0.3,0.5], (0.5,0.7], (0.7,0.9], (0.9,1.0]. The modified Figure 4 and 5 are as follows:

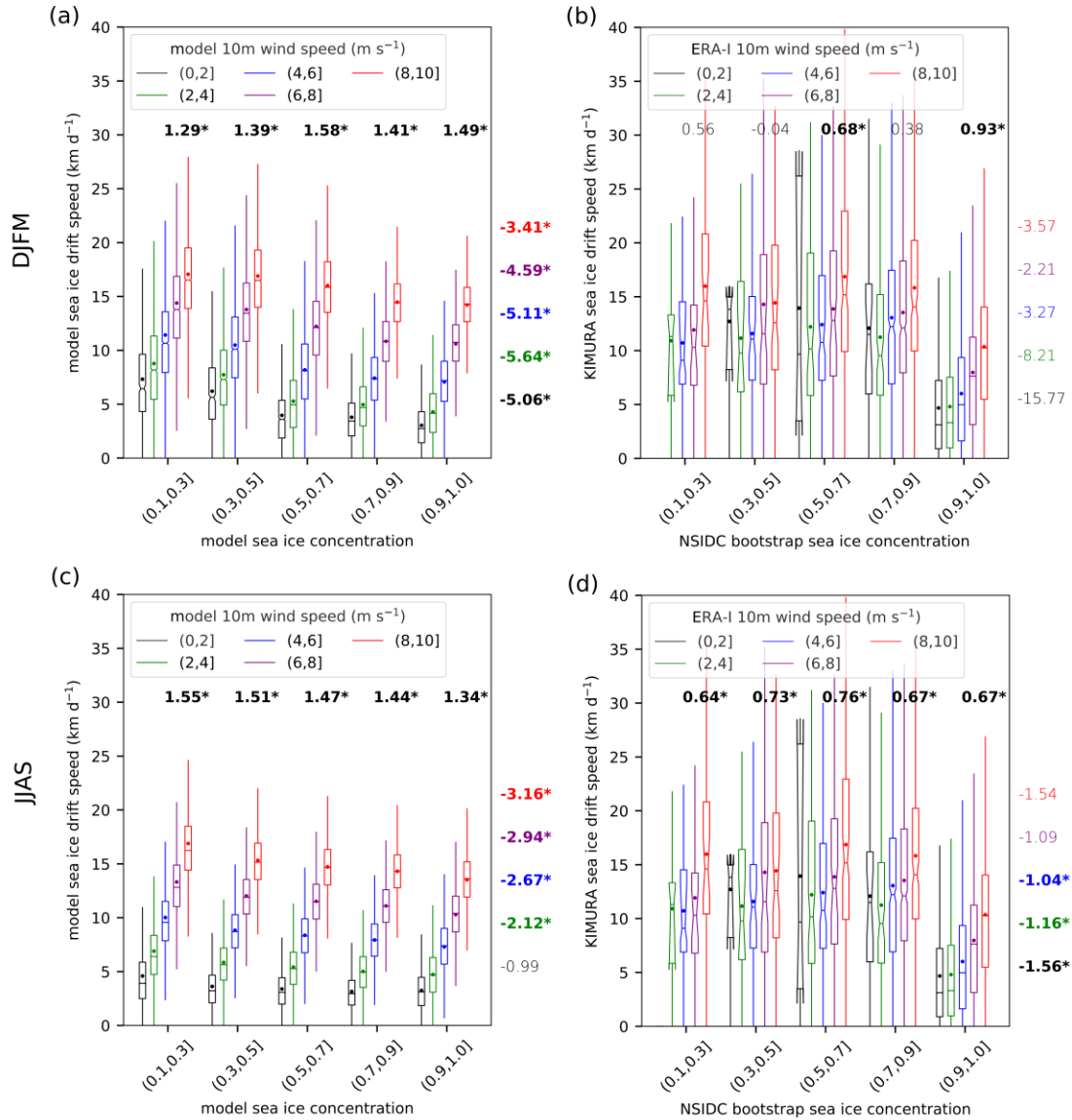


Figure 4: Box-whisker plots of the relationship between sea-ice drift speed and sea-ice concentration for different near-surface wind speed classes (different colors) for 2003-2014, in the model for (a) winter (DJFM) and (b) summer (JJAS), and in observation/reaanalysis data for (c) winter and (d) summer. For the model, all 10 ensemble members are included. The plot is based on daily data and on all grid points within the study domain indicated in Figure 1. The horizontal bar represents the median, the notch represents the 95% confidence interval of the median, the dot represents the mean, the top and bottom of the box represent the 75th and 25th percentiles, the upper/lower whiskers represent the maximum/minimum value within 1.5 times interquartile range (IQR) to 75/25 percentiles. The numbers above the boxplots represent the slopes of near-surface wind and sea-ice drift speed fit lines (unit: km d⁻¹ per 1 m s⁻¹ wind speed change; font colors as for the wind speed classes). The numbers right of the boxplots represent the slopes of sea-ice concentration and sea-ice drift speed fit lines (unit: km d⁻¹ per 10% sea-ice concentration change). A bold and asterisked number indicates that the slope of the fit line is significant at the 95 % level. In the labels of different sea-ice concentration and 10-m wind speed classes, “(” means exclusive and “]” means inclusive. The sample size of each boxplot is shown in Table 1.

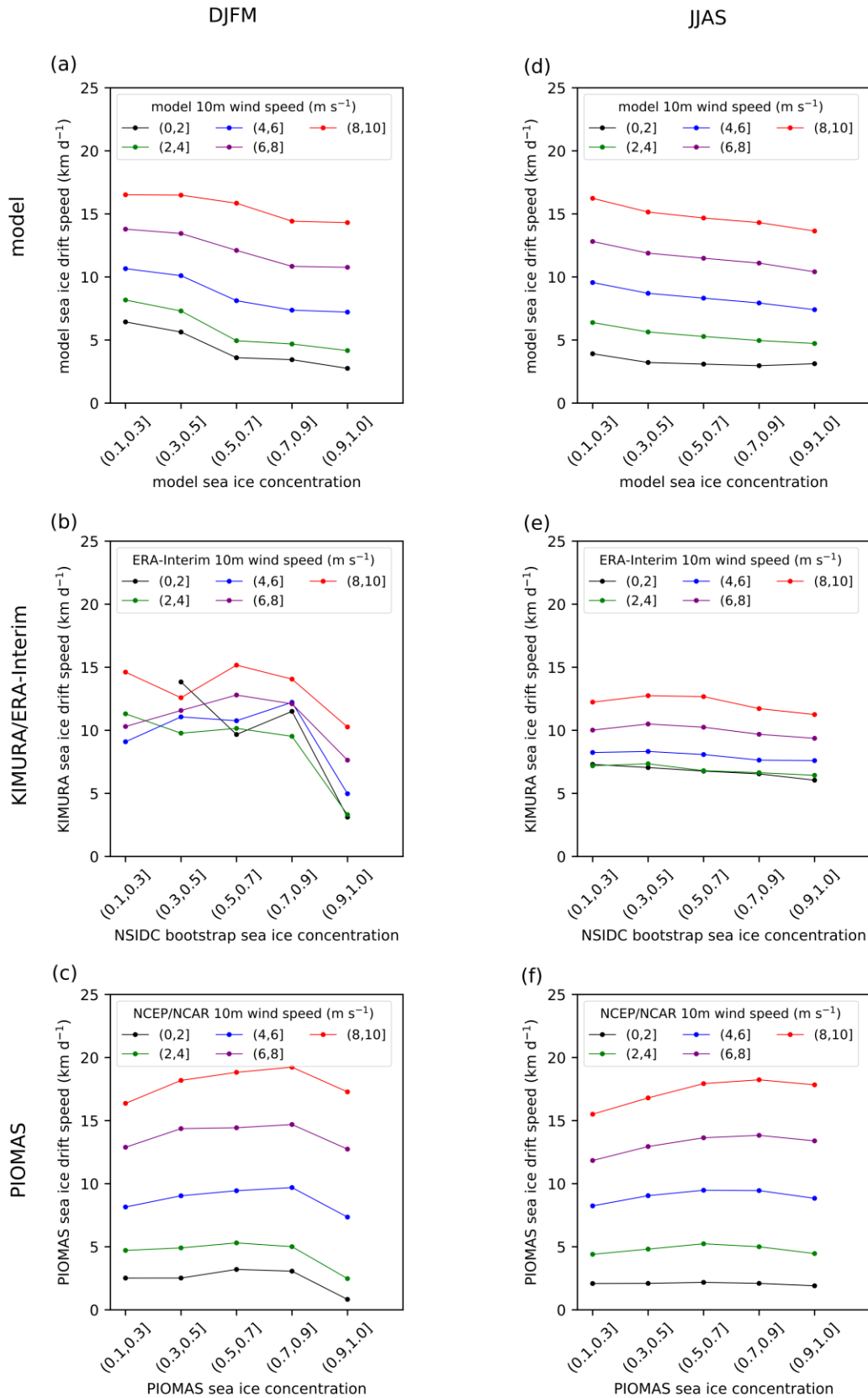


Figure 5: Relationship between sea-ice drift speed and sea-ice concentration for different near-surface wind speed classes (different colors) in the model during 2003-2014 (a) winter (DJFM) and (d) summer (JJAS). (b) and (e) are based on observation/reanalysis data. (c) and (f) are based on PIOMAS data. The points in the plot are the median value of all the daily data and on all grid points for certain wind speed and sea-ice concentration, within the study domain indicated in Figure 1.