Answer to the comments of Referee #1

We would like to thank Referee #1 for his/her suggestions to improve our paper. All comments have been addressed and a 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 comments

This article contains two pieces of work, the first being an assessment of the simulation of sea-ice (in particular sea-ice drift, SID) in a recent ensemble of 10 control members from a coupled regional climate model of the Arctic. The second part is then a sensitivity test, where the parameterization of surface exchange of momentum and heat between the ice and atmosphere is improved based on recent parameterization development documented in other papers. The research is state-of-the-art and an important step forward in the broader aim of trying to improve the fidelity of coupled climate models. The representation of Arctic sea ice is a long-standing known weakness in these models and improving the surface exchange parameterization is tackling one important weakness. The results of this study are mixed. Overall the model CTRL ensemble performs reasonably well compared to observational data sets (although these themselves have deficiencies). The new parameterization acts in a physically realistic way and leads to significant changes in surface variables. However, the authors state that it does not (yet) provide an improved simulation of sea ice, because no model tuning has been carried out yet, and they reserve this for future work.

Overall this is a very commendable study and an important piece of work, so I would like to see it published. I have a number of comments that would improve the manuscript that I'd like to see the authors take on board – some on the presentation that would greatly help new readers and make the work more accessible. The results of the second part of the study seem to end before the punch line! Normally upon introducing a new parameterization, authors invariably tend to find that their new parameterization improves the model. Here we seem to stop short of a full investigation of whether this is the case or not, because some tuning is required. I think this is reasonable, because the paper is already quite long at this point, and I am aware that such tuning is time consuming and opaque. But it does make the paper feel a little unfinished. Have the authors considered making this part 1 of a two-part paper, or at least spelling out in more detail the implied follow up study?

We added a new paragraph to describe the ideas for the implied follow-up study (line 514):

“Although the new parameterization does not improve the simulated SID dependency on WS and sea-ice conditions compared to observations/reanalysis, the sensitivity study clearly shows that the new parameterization does increase the SID due to the added form drag. In a follow-up study, we are going to put efforts therefore on several aspects to improve the simulations. First, tunable parameters of the new parameterization, such as z_0, z_t, $Ce_{10,i}$, $Ce_{10,k}$ and β represent an opportunity to better adapt the form drag parameterization itself to the observations. A first step could be the use of values found by Elvidge et al. (2016). A large effect can be expected by a modification of the skin drag coefficient, since a large region would be affected, and large variations in the drag due to pressure ridges allow a wide range of values. Second, model parameters outside the new parameterization, which have direct impact on SID, like ice strength and ocean-ice drag coefficient, need to be harmonized with the new parameterization, since their values were chosen empirically in terms of adequately balanced performance of the model. A key is probably the oceanic form drag. Its effect is accounted for in the present study only indirectly via the constant oceanic drag coefficient. Such a parametrization is probably too simple, especially when atmospheric form drag is included (see also Tsamados et al., 2014). Birnbaum (2002) as well as Lüpkes et al. (2012b) found in a mesoscale modelling study that oceanic form drag can have a strong decelerating effect on SID especially when the sea ice concentration is low so that the discussed drawbacks for small sea ice fraction would be reduced or even removed. This effect of form drag on SID was discussed also by Steele et al. (1989). The parametrizations are evidently not balanced anymore after improving one key process of the SID-related atmosphere-ocean-ice interaction. A previous study on the surface-albedo feedback by Dorn et al. (2009) showed that an improved simulation can only be achieved by a harmonized combination of more sophisticated parameterizations of the related sub-processes. It can be assumed that this holds true for the SID-related sub-processes.”

Birnbaum, G., and Lüpkes, C.: A new parameterization of surface drag in the marginal sea ice zone, Tellus A: Dynamic Meteorology and Oceanography, 54, 107-123, 10.3402/tellusa.v54i1.12121, 2002.

Steele M., Morison, J.H., Untersteiner N. (1989) The partition of air-ice ocean momedntum exchange as a function of sea ice concentration, floe size, and draft. J. Geophys. Res. 94: 12739-12750.

We also revised the discussion of the new parametrization at line 572:

“The inclusion of the melt pond effect on form drag in the model might be beneficial. In the current version, form drag was only considered at the edges of ice floes, mainly in the marginal sea-ice zone, but not on top of the ice, where melt ponds cause form drag also during summer (Andreas et al., 2010; Lüpkes et al., 2012a). Additional form drag at the ice-ocean interface may further improve the simulated SID-WS relation, because the oceanic form drag has normally the opposite effect on the ice motion as the atmospheric form drag (Steele et al., 1989; Lüpkes et al., 2012b). Systematic biases in the reanalysis used for the calculation of the 'observed' wind factor cannot be excluded, since form drag is not taken into account in the underlying atmospheric model. Therefore, the increased deviation of the simulated SID-WS relation from the observations/reanalysis does not necessarily mean that the implemented new parameterization worsens the SID-WS relation."

Specific Comments

1. The paper's title is long and a bit clumsy (three "ands"). I'd maybe try to reword.

We agree and modified the title to

"Evaluation of Arctic sea-ice drift and its dependency on near-surface wind and sea-ice conditions in the coupled regional climate model HIRHAM-NAOSIM"

2. In the Introduction I would recommend a short discussion on the quality of the surface exchange parameterization you've introduced. Around L65 you point out that parameterizations without a form drag element for momentum exchange are "poorly constrained" and that a recent observations-based form-drag parameterization has been implemented in a model by Renfrew et al. Here I think you need a few sentences pointing out that the mathematical parameterizations by Lüpkes et al. 2012 and Lüpkes and Gryanik 2015 were constrained by summertime observations over the sea-ice pack (from Andreas et al. 2010) and by limited aircraft observations over the MIZ (marginal ice zone). Then more comprehensively validated and tuned over the MIZ by a larger set of aircraft observations in Elvidge et al. 2016 [Note, this paper is not in the reference list, but there is a citation for Elvidge et al. 2018 in the manuscript, but no reference, so I think you mean the 2016 paper]. Importantly, I think you also need to point out that most of the validation and tuning has been done for momentum exchange (i.e. C_{DNi}), very little validation has been done for heat or moisture exchange (i.e. C_{HNi}). The validation and tuning for scalar fluxes is, I think, still something of an open question.

We corrected the citation and added a short discussion on the quality of the surface exchange parameterization following the Referee's suggestions (line 85):

"The mathematical parameterizations proposed by Lüpkes & Gryanik (2015) were constrained by summertime observations over the sea-ice pack and by aircraft observations over the MIZ (marginal ice zone) during winterly conditions. Later the parameterizations were once more validated using a larger and independent set of aircraft data obtained from campaigns during different seasons (Elvidge et al., 2016). This validation work concerned the momentum transport, but the assumptions of Lüpkes & Gryanik (2015) about heat and moisture flux over the MIZ could not yet be evaluated by measurements. Thus, further research is necessary on this issue."

3. Page 4 contains a mathematical description of the new surface exchange parameterization for over sea ice, based on Lüpkes et al. 2012 and Lüpkes and Gryanik 2015. I am familiar with these two papers and I think you are right to leave most of the mathematical details out of this article and refer the reader to these previous articles for details. However, what is tricky is that both of these previous articles are long and technical, with more than 60 and 70 equations in them respectively, and both contain several sets of parameterizations in a hierarchy of complexity. This makes checking the summary you have here difficult, especially as the notation used here is slightly different to the previous papers. I think you need to be more specific and say which equations from the two above papers are implemented and try to use notation that is as close as possible to what is already published (I appreciate this can be difficult). To give one example, equations (3), (4) & (7) all have a '+1', in "$z_{0,i}$+1" – this isn't explained and I don't know what it means. Also equation (8) does not seem to match equation (63) in Lüpkes and Gryanik 2015 – should it? Finally there are a number of parameters set on page 4: C_{e10}, $z_{o,f}$, b, then later on a and $z_{0,i}$. It is not clear where the values for these parameters have come from and I found it difficult to relate them to parameters in the previous studies or in Elvidge et al. 2016. I think this section (2.1.3) could be vastly improved without much additional length or detail. Finally, you don't comment on exchange of moisture, is this changed?

We added the source of equations (1) to (4) and the explanation of adding '+1' in equations (3) and (4) at line 158:

"Equations (1) to (4) are common descriptions of air-ice momentum and heat transfer coefficients except that '+1' was added to both $z_L/z_{0,i}$ and $z_L/z_{t,i}$ in equations (3) and (4). This is done in the model to avoid that the argument of the logarithm can go to zero, for which $C_{d,i}$ ($C_{h,i}$) would go to infinity (see also Giorgetta et al., 2013)."

Giorgetta, M. A., Roeckner, E., Mauritsen, T., Bader, J., Crueger, T., Esch, M., Rast, S., Kornblueh, L., Schmidt, H., and Kinne, S.: The atmospheric general circulation model ECHAM6-model description, 2013.

The source of equations (5) to (7) were added at line 169:

"Equations (5) is obtained by combining the equation (6), (52) and (70) of Lüpkes & Gryanik (2015). Equations (6) is obtained by combining the equation (9), (64) and (74) of Lüpkes & Gryanik (2015). After adding '+1' both to $10/z_0$ and z_L/z_0 and replacing z_0 with $z_{0,f}$ in equation (65) of Lüpkes and Gryanik (2015), $C_{dn,f}$ is calculated as

$$C_{dn,f} = C_{e10}\left[\frac{\ln(10/z_{0,f}+1)}{\ln(z_L/z_{0,f}+1)}\right]^2 A(1-A)^{\beta} \qquad (7)"$$

Equation (8) represents a simple algebraic transformation of equation (60) by Lüpkes and Gryanik (2015) making use of their equations (59) and (61). We added one sentence to clarify how we got the equation (8) at line 184:

"Equation (8) represents a simple algebraic transformation of equation (60) by Lüpkes and Gryanik (2015) making use of their equations (59) and (61) with $\alpha_f = \alpha$."

We added the source of the values for C_{e10} , $z_{0,f}$ and β at line 176:

"The value of C_{e10} is the average given in equations (48) and (49) by Lüpkes and Gryanik (2015). The value of $z_{0,f}$ is an average resulting from measured roughness lengths by various campaigns considered by Andreas et al. (2010), Lüpkes et al. (2012a) and Castellani et al. (2014). Note that this value is not critical for the parametrization. The value of β comes from equation (59) by Lüpkes et al. (2012a)."

The exchange of moisture is treated in the same way as the exchange of heat, meaning that the same coefficients are used.

4. In the summary (L440) you state that the SENS simulation is not any better than the CTRL simulations, in terms of sea-ice drift etc. However, you don't really provide evidence for this statement. I think there is evidence in your paper, but you need to discuss it and demonstrate this is the case. Consequently, I would recommend adding another paragraph or two to Section 4, where you discuss the quality of the SENS and CTRL simulations. For example, is it possible to compare the gradients in Fig 6a to Fig 10 and demonstrate whether the CTRL or SENS is better? Could you add some observational data to Fig 10 to show this fact? I appreciate that 'not any better yet' is a bit of a negative result and could be changed by tuning the model, so perhaps you don't want to spend too much time and effort on this aspect. But I think you need to provide a small amount of evidence for this statement.

We agree that it is helpful to support the statement that SENS is not better than CTRL in term of SID and SID-WS relation. We followed the Referee's suggestions and modified the original Figure 10 to include the boxplot of sea-ice drift speed against different sea-ice concentration and wind speed from observation and reanalysis. We added a new Figure 11 that compares wind factor, 10-m wind speed and 2-m air temperature from observation/reanalysis and from the CTRL and SENS simulations for summer 2007. We also added a new Figure 12 that compares the seasonal cycle of Arctic basin-wide averaged sea-ice drift speed and 10-m wind speed from observation/reanalysis and from CTRL and SENS. The modified and new figures show that the new parameterization both slightly reduces and increases the wind factor and 10-m wind bias over the Arctic dependent on location. The discussions based on these figures are added as a new subsection "4.2 Model versus observation":

"4.2 Model versus observation

The increased SID dependency on WS and SIC in SENS compared to CTRL does not reduce the deviation to observation/reanalysis. In contrast, Figure 10 shows that the overestimation of SID dependency on WS and SIC in SENS is larger than in CTRL.

Figure 11 shows the spatial distribution of the summer 2007 wind factor, WS and near-surface air temperature from observation/reanalysis data, and the deviations of these three variables in CTRL and SENS from observation/reanalysis. It is obvious that both the bias patterns and magnitudes of CTRL and SENS are quite similar. Considering the ensemble mean bias and taking the internal model variability into account, it is hard to detect significant changes in SENS, compared to CTRL, as discussed above (Figure 8).

Although the ensemble mean of Arctic basin-wide mean SID from July to September in SENS is larger than in CTRL, the differences are not statistically significant due to a large ensemble spread (Figure 12). Actually, there are no statistically significant differences in the Arctic basin-wide mean SID between CTRL and SENS in all months. From January to May, the simulated Arctic basin-wide mean SID (both in CTRL and SENS) are higher than that in KIMURA. With respect to the summer months (June to September), the August simulated Arctic basin-wide mean SID in CTRL is lower than in KIMURA, while the July and September simulated Arctic basin-wide mean SID in SENS are higher than in KIMURA. For the Arctic basin-wide mean WS, there is no significant difference between CTRL and SENS as well as between model and reanalysis, except for May, when both model simulations significantly overestimate the WS."

The modified Figure 10 is as follow:

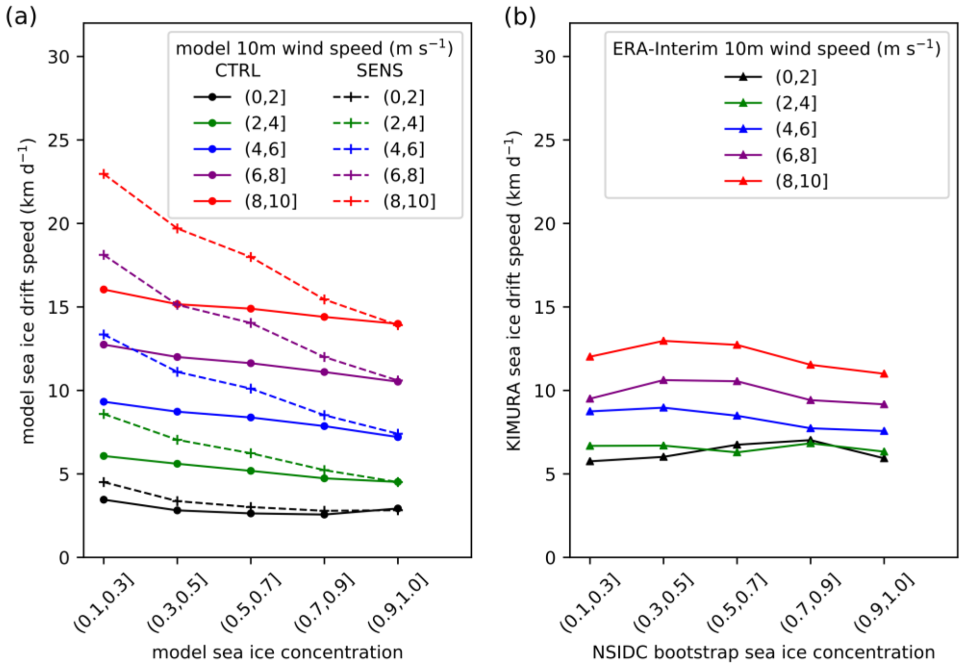

Figure 10: The (a) simulated relationship between sea-ice drift speed and sea-ice concentration for different near-surface wind speed classes (different colors) for 2007 summer (JJAS) for CTRL (circle marker and solid line) and SENS (cross marker and dashed line) experiment. The relationship based on KIMUAR sea-ice drift speed, NSIDC bootstrap sea-ice concentration and ERA-interim 10-m wind speed is shown in (b). The points in the plot is the median value of all the daily data and on all grid points within the study domain indicated in Figure 1 under certain wind speed and sea-ice concentration classes.

The added Figure 11 and 12 are as follows:

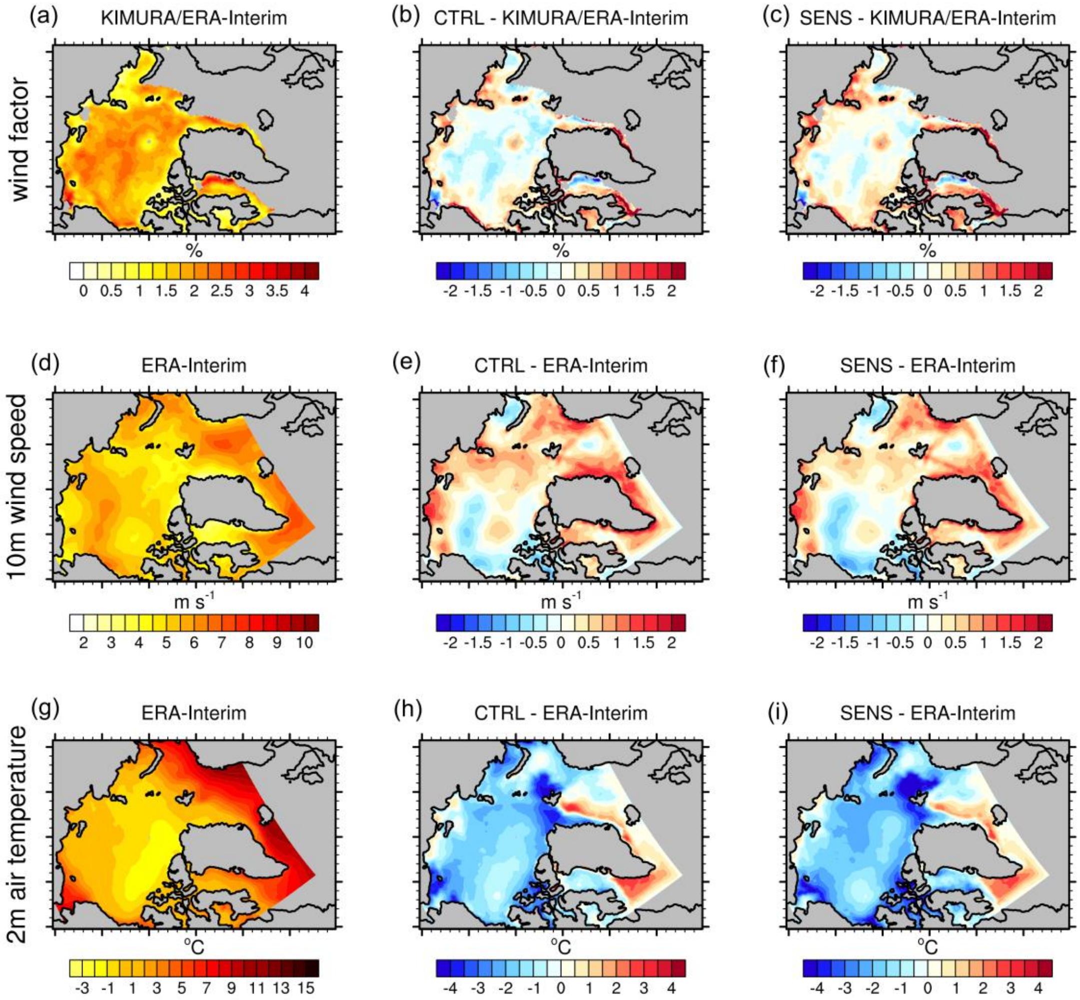

Figure 11: The 2007 summer (JJAS) wind factor, 10-m wind speed and 2-m air temperature from ERA-Interim (KIMURA sea-ice drift is used for wind factor calculation) and the deviations of these three variables in the ensemble mean of CTRL and SENS experiments from ERA-Interim (KIMURA and ERA-interim for wind factor).

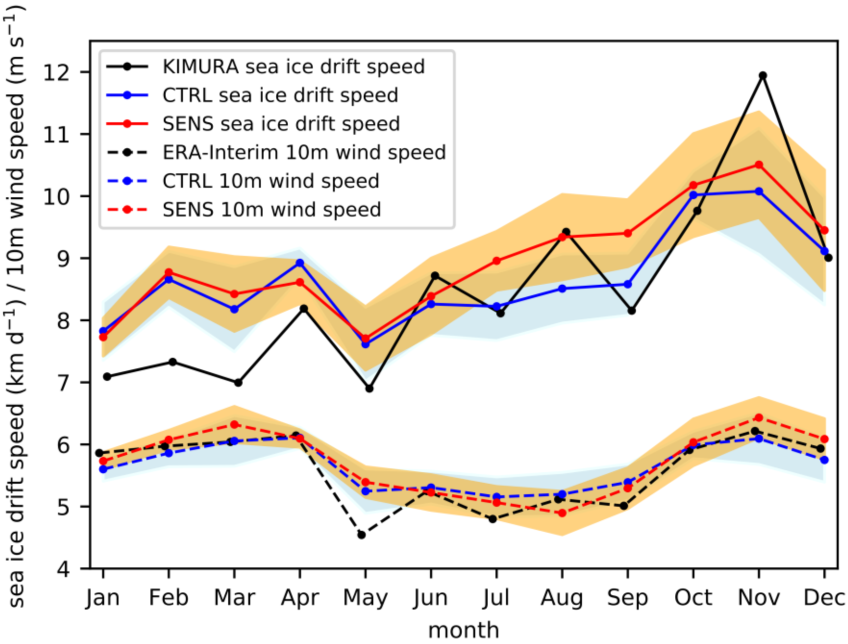

Figure 12: Mean annual cycle of sea-ice drift speed [km d^{-1}] (solid lines) and 10-m wind speed [m s^{-1}] (dashed lines), based on CTRL experiment (ensemble mean; blue lines), SENS experiment (ensemble mean; red lines) and observation/reanalysis (KIMURA ice drift, ERA-I wind; black lines) for 2007 over the study domain (indicated in Figure 1). The across-ensemble scatter (standard deviation) of the simulations is included as shaded area (light blue for CTRL, orange for SENS).

Minor Points

L14 – "…of the Arctic basin" [insert the]

Changed as suggested.

L28 – "sea ice has experienced…"

Changed as suggested.

L66 You categorise Tsamados et al. 2014 as an ice-ocean model, but that study was actually using only a sea-ice model.

We agree and modified the introduction of the study of Tsamados et al. (2014) and other studies that also include sea-ice form drag in the model simulation as follows (line 67):

"Several model studies that include the sea-ice form drag were carried out (Castellani et al., 2018; Renfrew et al., 2019; Tsamados et al., 2014). Tsamados et al. (2014) implemented a complex sea-ice form drag parameterization based on many sea-ice cover properties (e.g. sea-ice concentration, vertical extent and area of ridges, freeboard and floe draft, and the size of floes and melt ponds) into the stand-alone sea-ice model

CICE. Castellani et al. (2018) implemented a simpler sea-ice form drag parameterization that only relies on sea-ice deformation energy and concentration into the coupled ocean-sea ice model MITgcm. Both studies showed improvement in sea-ice drift after the form drag had been included. Recently, Renfrew et al. (2019) implemented an observation based parameterization of atmospheric form drag caused by floe edges based on Lüpkes et al., (2012a), Lüpkes & Gryanik (2015) and Elvidge et al., (2016) into a stand-alone atmosphere model. The simulation results show an improved agreement of mean atmospheric variables and turbulent fluxes with measurements in cold-air outbreak situations over the Fram Strait when form drag is included."

L175 – Have you considered a Cryosat product for sea-ice thickness – probably not worth the effort now, but might be interesting for any follow up studies.

We have considered to use Cryosat2, but Cryosat2 is only available from 2010 onwards and from October to next April. Therefore, Cryosat2 does not cover the whole period of 2003-2014 and does not provide the summer data. 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.

We added according sentence in section 3.1:

"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."

L180 – The description of ERAI resolution is misleading. The resolution of the atmospheric model is T255 equivalent to about 80 km resolution, and you have downloaded it on 0.25 degree grid. So please rephrase.

We added more information to the description of the ERA-Interim data at line 228:

"For the near-surface wind speed (WS), daily 10-m wind speed from ERA-I is used. The ERA-I data were downloaded from the MARS archive at ECMWF and interpolated to the same 0.25° x 0.25° grid as used in the model's atmosphere component HIRHAM5."

L185 – "resolutions, a bilinear…"

Changed as suggested.

L191 – "as the study…"

Changed as suggested.

L250 – Maybe swap order to winter then summer to match the order earlier in the sentence, i.e. rephrase sentence.

The sentence was rephrased as suggested (line 322):

"Averaged over the study domain, the simulated wind factor is 1.77% in winter and 1.87% in summer, which agrees with the observations/reanalysis (KIMURA ice drift/ERA-I wind) in the sense that the averaged wind factor is smaller in winter (1.42%) than in summer (1.96%)."

L258 and L262 – Maybe rephrase to state 'in winter…" and "in summer, …" clearly at the beginning of the statement, rather than hidden in the middle of the sentence.

The sentences were rephrased as suggested (line 333):

"In winter, however, the simulated wind factor is overestimated compared to the KIMURA/ERA-I data almost everywhere over the study domain, with the maximum bias reaching 1% over the thick ice north of the Canadian Archipelago (Figure 3). In summer, the modelled wind factor peaks (~3%) along the marginal ice zone, such as in the coastal Beaufort Sea.

…and (line 340):

In contrast to winter, the modelled wind factor in summer is underestimated over the study domain."

L335-340 – Can you cite some evidence that PIOMAS is wrong here – I think it is incorrect and it is certainly inconsistent with the model.

We don't have a reference yet, but we think one possible explanation that PIOMAS gives a SID-SIC relation inconsistent with the observed relation is a violation of physical consistency in the modeling system by the data assimilation. We added the following sentences in paragraph 3 of section 3.3.2 to elaborate our explanation:

"PIOMAS gives a SID-SIC relation that is inconsistent with the observed relation. Thus, the PIOMAS relation might violate physical consistency due to the used assimilation method as explained in the following. PIOMAS employs the optimal interpolation method to obtain a realistic sea ice field (concentration). This procedure contains addition/subtraction of sea ice into the system at every assimilation time step, when the modeled sea ice concentration differs from the observed one. Due to the addition/subtraction of sea ice (called increment or innovation in the terminology of data assimilation), PIOMAS does not necessarily preserve the physical relations described in the underlying sea ice-ocean model. Such an inconsistency is one of the drawbacks of the optimal interpolation method and therefore relations between assimilated physical properties should be examined with caution."

L345 – You don't discuss Fig 6a at all. Is it needed? Perhaps it should be discussed later.

Actually, we already discuss Figure 6a at the beginning of Section 3.3.1. We agree that it is easy to overlook Figure 6a or 6b because they are not discussed together. We start Section 3.3.1 now with the sentence 'Figure 6a shows…'

Figures

Figures 1, 3, 8, S1, S2, S3, S4

These all use the same colormap which is a blue-white-red (diverging colormap). Such colormaps are ideal for difference plots, e.g. Fig 1b,d, but are an odd choice for non-diverging fields, such as Fig 1a,c. I wonder if you are better changing colormap for the left-hand columns in all of these plots.

We understand the reviewer's concern about using blue-white-red color map for non-diverging fields. Therefore, we replaced the blue-white-red color map in Figures 1, 3, S1, S2, S3 and S4 with a yellow-red color map for all non-diverging fields. The color map in Figure 8 was not changed because there is no non-diverging field.

Figure 4

Unfortunately, this is really hard to read at this size (printed A4). I also think it has too much unnecessary detail in it. You have 10 wind speed classes. Do you really need this many classes? I think you'd get the same result with 2 m/s bins and it would be much clearer. Also do you include winds >10 m/s in the (9,10] bin? Note I have taken (1,2] to mean winds between 1 and 2 (inclusive) m/s – you should explain this in first caption. Secondly you have 9 SIC bins – again this is a lot and there seems very little difference in the results between adjacent bins. I'd perhaps recommend fewer bins, perhaps (0,0.1], (0.1,0.3], (0.3,0.5], (0.5,0.7], (0.7,0.9], (0.9,1.0]. This keeps the 'end' bins separate as these are more interesting. At present this Fig 4 and also 9 has so much detail and numbers, that the main message is a bit hidden. Finally, it may be worth noting how much data is in these bins. Although the bins are the same size (0.2 in ice fraction for example), the distributions mean there could be relatively few points in some bins.

For wind class bin (9,10], the wind speed greater than 10 m/s is not included. We agree that it is helpful to explain that "(" means exclusive and "]" means inclusive. We added this to the caption of Figure 4:

"In the labels of different sea-ice concentration and 10-m wind speed classes, "(" means exclusive and "]" means inclusive."

We agree that the boxplot figures in Figure 4 are complex, now we followed the suggestion of the Referee that change the wind class bin size to 2 m/s and rearranged 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]. Also, we stress that we additionally provide Figures 5, 7, 10, where we display the median values only for an easier visualization of the relationships. The modified Figure 4 is as follow:

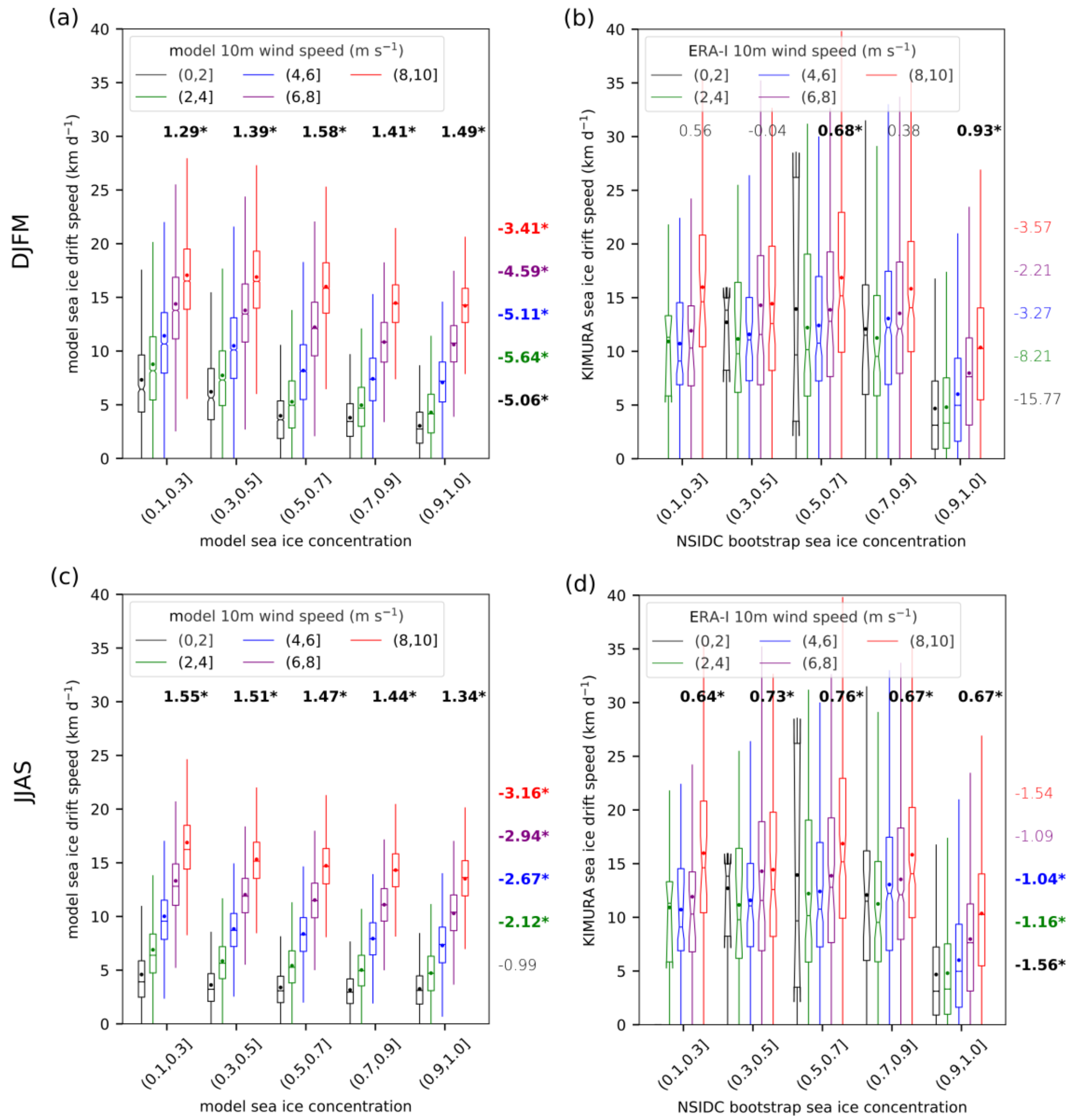

Figure 4: Box-whisker plots of the relationship between sea-ice drift speed and sea-ice concentration for different near-surface wind speed classes (different colors) for 2003-2014, in the model for (a) winter (DJFM) and (b) summer (JJAS), and in observation/reanalysis data for (c) winter and (d) summer. 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.

We understand the Referee's concern about the sample size in each bin. Instead of giving the sample size of each bin directly in Figure 4, we provide therein the 95% confidence range of the median value for each bin (represented by the height of the notch in the boxplot). The confidence range includes the influence of the sample size. We provide the sample size for each bin in the new Table 1 as follow:

Table 1 The sample sizes of sea-ice drift speed data under different 10-m wind speed and sea-ice concentration classes in Figure 4.

Season	**Data source**	**Wind classes**	**Sea-ice concentration classes**				
			(0.1,0.3]	**[0.3,0.5]**	**(0.5,0.7]**	**(0.7,0.9]**	**(0.9,1.0]**
DJFM	model	**(0,2] m/s**	494	864	3328	135516	45562216
		(2,4] m/s	2070	3716	12780	489893	183056161
		(4,6] m/s	3432	5766	17172	618383	239287363
		(6,8] m/s	5804	9402	17215	435238	181532612
		(8,10] m/s	7950	12511	18413	246921	102066710
	KIMURA/ERA-I/NSIDC	**(0,2] m/s**	0	7	7	40	102295
		(2,4] m/s	15	29	50	124	365803
		(4,6] m/s	32	66	117	279	499634
		(6,8] m/s	28	88	137	381	386667
		(8,10] m/s	53	83	145	288	218638
JJAS	model	**(0,2] m/s**	92547	2535519	17432992	25896542	9142130
		(2,4] m/s	322269	8521163	57295068	84426782	30714941
		(4,6] m/s	534300	12186383	81681898	113048227	40015962
		(6,8] m/s	549254	9864436	67161887	84920918	27346273
		(8,10] m/s	356102	4746320	34131702	38211228	11159363
	KIMURA/ERA-I/NSIDC	**(0,2] m/s**	1519	3439	5150	12096	100317
		(2,4] m/s	4727	10504	16204	39911	312814
		(4,6] m/s	6533	14571	22364	55418	385667
		(6,8] m/s	5261	11976	17587	40559	262251
		(8,10] m/s	2514	5249	7276	17272	107988

Figure 5

Same comments as above really and note some of the colours are very faint (7,8) class. These plots are more readable but need to be consistent with Fig 4.

Agree and now we enhanced the visualization of Figure 5 by increasing the font, re-arranged into 3×2 panels, reduced the sea-ice concentration and wind speed classes as discussed before and discarded the faint color that previously used for wind class (7,8] m/s. The new Figure 5 is as follow:

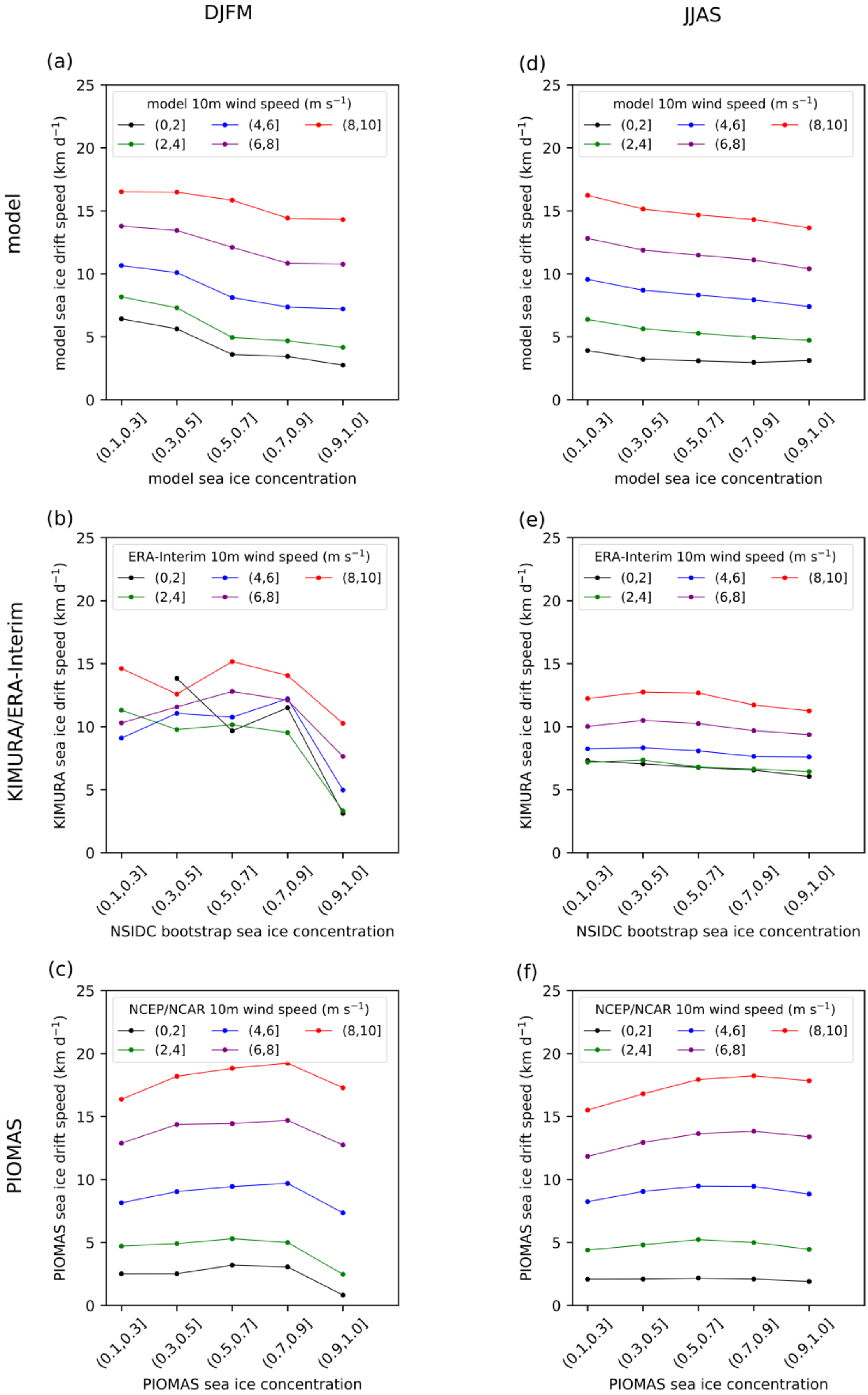

Figure 5: Relationship between sea-ice drift speed and sea-ice concentration for different near-surface wind speed classes (different colors) in the model during 2003-2014 (a) winter (DJFM) and (d) summer (JJAS). (b) and (e) are

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.

Missing Reference:

Elvidge, A.D., I.A. Renfrew, A.I. Weiss, I.M. Brooks, T.A. Lachlan-Cope, and J.C. King 2016:Observations of surface momentum exchange over the marginal-ice-zone and recommendations for its parameterization, *Atmospheric Chemistry and Physics,* **16,** 1545-1563. doi:10.5194/acp-16-1545-2016

We added the missing reference.