Reply to Referee #2

Interactions between Arctic sea ice drift and strength modelled by NEMO-LIM3.6

Docquier *et al.* (2017), tc-2017-60

We would like to thank Referee #2 for his/her very constructive feedback, which has helped us improve the paper quality. Below we present our detailed responses to the comments and suggestions proposed by the reviewer in **red**. The corresponding corrections are in **red** in the revised manuscript.

1. Summary

The authors conducted ocean-sea ice model NEMO-LIM3.6 numerical experiments to investigate interaction and/or feedback mechanisms between sea ice drift speed and ice strength, focusing on the Arctic Ocean. As measures of ice strength, the authors employ ice concentration and thickness, and then examined relation between ice drift speed and them. In order to assess the model performance, sea ice observation data derived from satellite and ice-tethered buoys (ice concentration, ice drift speed and ice thickness) were exploited. The authors introduced a systematic model validation method based on sea ice diagnostics (ice extent, ice concentration, ice thickness and ice drift speed) as well as process-based diagnostics (relation between different ice properties, e.g., ice drift speed and concentration). They also introduced metrics which quantify modeled or observed ice dynamics as scalar variables. Using these diagnostic and metric approaches, the author assessed model performance, and then conducted sensitivity experiments varying ice strength parameter to see the effect of ice strength change on the relation between drift speed and ice concentration (or ice thickness) simulated in the model.

2. General comments

The authors provided a good review regarding relation between ice drift speed and ice concentration (and thickness) in the Arctic Ocean, mainly focusing on observational studies. The diagnostics and metrics used to validate model performance and the thorough assessments on the simulated ice properties are very helpful to assess the capability and limitation of the model, although I have an additional suggestion regarding ice thickness evaluation (see comments below). The results of the sensitivity experiment, in which ice strength parameterization is changed, are interesting to me (and probably informative to many sea ice modellers), while the whole strategy used to assess the feedback between ice drift and other ice properties seems to me not always suitable nor convincing for the purpose of the study. As far as I know, "feedback" is a term originally introduced to describe an electric circuit which has a recursive input, and is widely used in other study area to explain similar concept. Also in geo-scientific studies, the term "feedback" is widely used to explain a recursive interaction between different processes or subsequent chain of phenomena, sometimes without clear definition nor quantification. Although the authors invoked the term "feedback", the strategy does not seem to be designed so as to extract a feedback mechanism from a complicated system. For this reason, I would recommend to change the focus of the study to more specific issues (e.g., sensitivity of ice strength parameterization to ice drift, concentration and thickness) or to reorganize experiment design so as to quantify the feedback between them. Even without feedback issue, the manuscript contains interesting model results.

We thank the reviewer for his comments. Since his point related to the feedback methodology is related to the first major point below, we provide our answer there.

3. Major points

1) The strategy used to extract (and quantify) the drift-strength feedback is not always suitable nor convincing. I think more consideration is necessary about how to quantify a "feedback". Since ice drift, concentration and thickness are described by a set of simultaneous partial differential equations in numerical models, it is a matter of course that there is some sort of 'feedback' between them. An important point is how to extract and quantify a feedback in a simple formula so as to abstract the essence of the complicated system. Mathematically, a feedback between two variables (most simple case) can be described by a set of equations,

$$\frac{d\overline{|V|}}{dt} = F(\overline{T}) \qquad (1)$$

$$\frac{d\overline{T}}{dt} = G(\overline{|V|}) \qquad (2)$$

where |V| and T are respectively mean ice drift velocity and ice strength (or thickness) in the present case (e.g., $|V| = 1/TS \iint |V| dxdydt$, T: one month or one year period, S: SCICEX box). F and G are the functions describing a feedback. The equations mean that the temporal evolution of mean ice drift is controlled (or affected) by mean ice strength, while at the same time, the evolution of ice strength is also controlled by drift speed. The most simple solution is an exponential formula, $Ce^{\alpha t}$, and then temporal evolution of |V| and T depends on α. The system has a positive feedback if $\alpha > 0$, while a negative feedback if $\alpha < 0$. If the authors intend to clarify and quantify the feedback mechanism between ice drift and thickness, a definite formula (not necessarily means complicated formula) in such a simple theoretical framework should be provided. Otherwise, we cannot learn anything about the quantitative features of the feedback between ice drift and thickness (or concentration) at all. Note that the system also needs a higher order stabilizing term to prevent exponential growth of a positive feedback. (At least discussion for such a damping mechanism is necessary.)

We agree with the reviewer that our study probably lacks a robust mathematical framework to quantify the sea ice drift-strength feedback. Due to the complexity of this feedback (see sensitivity experiments) and the lack of observations confirming the existence of this feedback in real life (see main reservation of Referee #1), we decided to change the focus of this study by analysing the interactions between sea ice drift and strength (concentration and thickness) rather than the feedback itself. The main results stay similar but the wording is now slightly different in the revised manuscript: we talk more about interactions between drift speed and strength and about the impact of changes in strength parameterisation on the resulting drift speed and thickness, rather than about the feedback itself. We also changed the article title accordingly.

2) The design of the sensitivity experiment seems to me not suitable for examining the effect of ice strength change on ice drift, ice thickness and concentration. Since the authors change the ice strength, P, by changing exponent of ice thickness h, the associated change of ice strength has an opposite sign between h > 1 and h < 1. This is easily confirmed by calculating P value for different λ used in the sensitivity experiment: if h > 1, P λ=2 is larger than P λ=1 , while P λ=2 is smaller than P λ=1 if h < 1 (this is also confirmed by closely looking Fig. 2). Due to this experiment design, we cannot directly relate the change of parameter λ to the change of ice strength and therefore, to changes of the ice thickness and ice concentration, which makes interpretation of the results complicated and difficult (There are many misinterpretation in sec. 3.3 due to this fact, see the specific points below). Generally speaking, changing exponent of h in the ice strength equation is not equivalent to changing ice strength, but to changing sensitivity of ice strength to h. My recommendation is to use more simple formula for the sensitivity experiment (e.g., $P = P^* \lambda h \exp[-C (1 - A)]$, which is equivalent to change P*), or to redo analyses based on the actual ice strength P used in the model (i.e., calculate $P= 1/TS \iint P(x , y , t)\, dxdydt$ and examine relation between P and modeled ice properties; ice drift, ice concentration and thickness).

We agree with the reviewer that an increase (decrease) in λ for thickness h < 1 m leads to smaller (higher) ice strength P. Due to this problem and the remarks of both reviewers regarding the design of our sensitivity experiments, we performed new sensitivity experiments in which we change the P* parameter according to values found in the literature. The chosen P* values are described in our response to Referee #1 (see his minor comment p. 3 l. 29-32) and a detailed explanation of the P* experiments has been added to the manuscript (Section 2.1). Results are presented in Section 3.3 and lead to the same conclusions as for λ experiments, i.e. a higher (smaller) initial ice strength, caused by higher (smaller) P*, leads to lower (higher) sea ice thickness.

In the revised manuscript, we decided not to show the results from λ experiments but we discuss them in Section 4.2 as they are still interesting for several reasons:

a. **Some sea ice models use P scaling as $h^{1.5}$ (e.g. Lipscomb et al., 2007) since it is more physically realistic than P scaling as h (but less numerically stable); therefore it makes sense to test the sensitivity of the model to different λ values.**
b. **Even if it is true that P decreases with increasing λ for h < 1 m, the differences in P between the four different experiments for h < 1 m are very small; these differences become more important for h > 2 m as shown in Fig. 2 in the previous version of the manuscript; moreover, the mean ice thickness averaged over the SCICEX box (the quantity we use in our study) is always higher than 1 m, even in September when ice is the thinnest (Fig. 10a in the previous version of the manuscript).**
c. **These experiments are original (most modelling studies change P*) and this is the first time that four λ values are used (the existing studies only look at λ = 1 and λ = 2).**
d. **The λ experiments are more complex than the P* experiments (due to their exponential nature), but they confirm the results obtained with the latter experiments.**
e. **Leppäranta (2011) states that the value of λ is an open question.**

3) I admire the systematic approach used in this study to assess the model performance using observational data, whereas I have a concern about the use of ice drift and thickness data derived from PIOMAS, instead of direct observations. The authors did not utilize two important dataset for ice drift and thickness, which have sufficient spatial and temporal coverage for the present study. One is ice drift data provided from Colorado University group (Tschudi et al., 2016), the other is the long-term ice thickness estimate by Lindsay and Schweiger (2016). Both estimates provide error of the estimates as well. Since these data were derived from in-situ and satellite measurements while PIOMAS did not assimilate ice drift and thickness, I recommend to use these two data instead of PIOMAS simulation. Note that the bias (or error) of ice drift field in Tschudi et al. (2016) reported by Szanyi et al. (2016) is a crucial issue, only if the data is used for divergence/convergence calculation. Since the divergence/convergence features reported by Szanyi et al. (2016) always appear as a divergence/convergence pair around buoy-data merged location, a spatial averaging (e.g., SCICEX box) can eliminate the error.

Following the remarks related to PIOMAS from both reviewers, we now use the multiple regression model from Rothrock et al. (2008) based on ULS submarine ice draft measurements to derive sea ice thickness. These data constitute the longest record in terms of sea ice thickness (1975-2000), they cover the SCICEX box that we mainly use in our study and they are integrated into the Lindsay and Schweiger (2015) dataset.

For the observed drift speed, we now include the NSIDC merged dataset from Tschudi et al. (2016), which includes IABP buoy data. We call this dataset 'NSIDC' throughout the paper. However, we are a bit skeptical about the mean seasonal cycle derived from these data (Fig. 3d in the revised manuscript), which gives drift speed values up to 4 km/d lower than the IABP buoy drift speed values. The amplitude of the seasonal cycle of the merged product is also much lower than the one from the buoy data. These features were already noticed by Olason and Notz (2014) who used the previous version of the NSIDC merged product (Fowler et al., 2013) (see their Fig. 10).

Please note that we already use IABP buoy data from the Tschudi et al. (2016) dataset as well as OSI SAF data for drift speed, and ICESat for ice thickness. We decided to remove PIOMAS drift speed from our analysis due to the poor results associated with this reanalysis for drift speed as well as the scaling problem (factor 2).

4. Specific points / additional comments

- Page 2, line 1-8: I would suggest the author to use the term 'feedback' more carefully, particularly when mentioning 'positive feedback'. Since a positive feedback has an exponential growth feature by its definition, it always needs damping or stabilizing mechanism in higher order, when the concept is used to explain things occurring in the nature.

Please see our response to the first major point above.

- Page 2, line 9-22: I appreciate the good summary of the former studies here, which is useful to survey the current status of our understanding on this issue. On the other hand, I am a little bit skeptical to provide conceptual illustration like Fig. 1, since such an illustration sometimes goes out of authors' control if once published, even if each arrow in the figure is not really examined.

We removed the diagram (Fig. 1 in the previous version of the manuscript) due to our decision not to focus on the feedback but rather on interactions (see our response to the first major point).

- Page 3, line 29-30: Why the authors conducted the sensitivity experiment by changing exponent of h in the ice strength equation, instead of changing P* or C? If the authors intend to increase ice strength in the entire h range, changing P* seems to be more suitable approach (as many modellers do). I request more explanation.

As mentioned in our response to the second major point, we ran new P* experiments and we decided not to show results from λ experiments (but we discuss them in Section 4.2). Finally, we had already carried out experiments by varying C between 16 and 22, but we found negligible differences in sea ice thickness and drift speed, so we decided not to include them in our study.

- Page 4, line 1-7: Since we cannot directly relate the increase of λ to increase of ice strength in the present experiment design, more careful explanation is needed. For example, ice thickness during the summer season in the SCICEX box may thinner than 1 m, at least part of the area. In this case, an increase of λ leads to decrease of ice strength.

Please see our response to the second major point above.

- Page 4, line 26-34: Why the authors did apply ice drift data provided from PIOMAS instead of satellite- and buoy-based data as a long-term ice drift observation? Polar Pathfinder Daily 25 km EASE-Grid Sea Ice Motion Vectors (Tschudi et al., 2016, now version 3 is available via NSIDC website) provides Arctic-wide long time series from 1979 to present. Although PIOMAS reasonably reproduced sea ice extent and ice concentration field as a result of ice concentration assimilation, there is no reason to believe that ice drift and thickness from PIOMAS are consistent with observation, since these variables are not assimilated. I think the ice drift data from PIOMAS should be dealt with a great care (Since PIOMAS applies strong constraint on ice concentration by an optimal interpolation, the simulated ice field never breaks down, even if ice drift is totally unrealistic). Use of Polar Pathfinder data is much more reliable approach, since the uncertainty of the estimates are also provided. The problem of the Polar Pathfinder data reported by Szanyi et al. (2016) is not a critical issue for the present application (see my major point as well).

We removed PIOMAS drift speed from our analysis and now use the NSIDC dataset from Tschudi et al. (2016).

- Page 5, line 20-24: For long-term ice thickness estimates in the Arctic Ocean, I strongly recommend to use the estimate presented in Lindsay and Schweiger [2015]. They provided an empirical function describing the spatially and temporally varying ice thickness field over the Arctic Ocean, by exploiting all available in-situ and satellite measurements of ice thickness. As far as I know this is the most reliable long-term estimate of ice thickness field based on measurement for the time being (One can calculate seasonally varying long-term ice thickness field by the description in Lindsay and Schweiger).

We are aware of the sea ice thickness dataset from Lindsay and Schweiger (2015). However, we decided to use only one source of data, namely ULS submarines, due to the fact that these data cover the SCICEX box (which we use as a region of interest in our study) with sufficient accuracy.

- Page 6, line 1-4: How did the authors define the daily ice drift in Eq. (2)?

$$u_d = \frac{1}{T_{day}} \int_{t_1}^{t_2} v(t)\,dt$$ (i.e., temporal average of instant velocity in x and y direction)

or $$u_d = \frac{|x_{t_2} - x_{t_1}|}{T_{day}}$$ (zonal or meridional displacement for 24 hours)?

The definition of satellite- or buoy-derived ice drift is the latter. I don't think the difference between the two definitions is large, but it would be nice if the authors clarify their definition for comparison with other model results.

For our model, daily mean ice drift speed is defined by Eq. (2), where u_d and v_d are daily mean ice velocity components. We articulate this in the revised manuscript. u_d and v_d are computed in the model by averaging values over the 8 model time steps covering each day, i.e. following the first equation mentioned by the reviewer above.

- Page 7, line 27-30, Page 8, line 1-5: I think the comparison with PIOMAS thickness data provides useful insights, while I am skeptical to regard PIOMAS thickness data as substitution for observation, since we cannot distinguish bias or error of the estimates, which are always provided for observational data.

We added observations from ULS submarines. However, we also think that PIOMAS is useful since its thickness is realistic compared to observations (Schweiger et al., 2011) and due to high uncertainties related to ice thickness observations. Please also see our response to the third major point above as well as our response to the comment 'p. 4 l. 29' of Referee #1.

- Page 8, line 1: Do the authors have an explanation why the peak shifts?

Modelled sea ice thickness averaged over the SCICEX box and over the domain north of 50°N is shown below (Fig. S1 below). As shown, the SCICEX maximum occurs in May while the 50°N maximum occurs in June with a second peak in August. We do not have an explanation for this shift. However, the 50°N is a very large domain that takes into account coastal areas, so we are a bit skeptical of the representativeness of the 50°N seasonal cycle.

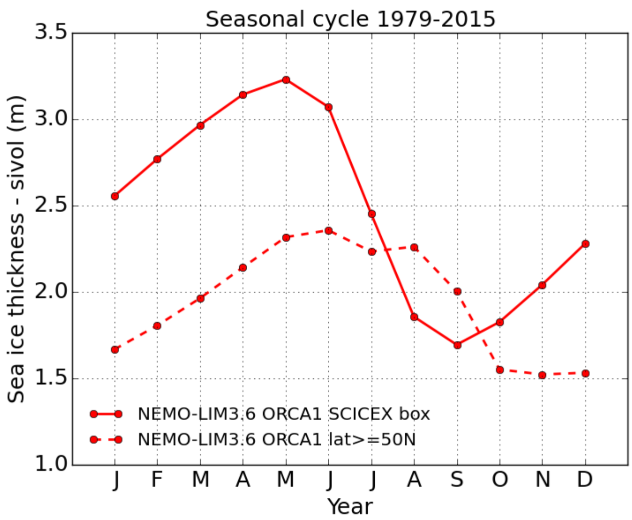

Fig. S1: Modelled (NEMO-LIM3.6) monthly mean seasonal cycle of Arctic sea ice thickness (sea ice volume per area) temporally averaged over the period 1979-2013 for two different domains (solid line: SCICEX box; dashed line: north of 50°N with A >= 0.15).

- Page 8, line 23-24: How the IABP ice drift data were processed to calculate spatial (and temporal) average over the SCICEX box? Since the spatial coverage of the buoy data is not sufficient, one needs to interpolate/extrapolate the data. How did the author define influential radius of the buoy data? How much uncertainty should we expect from the interpolation/extrapolation process? Since the IABP data averaged over the SCICEX box is an important measure to validate the model, a description is necessary.

For IABP drift speed data, we first computed daily spatial means by taking all buoys within the SCICEX box. Then, we computed monthly means from these results. By doing so, we do not need to interpolate / extrapolate and there is no error due to the spatial sampling. The spatial coverage of buoy data within the SCICEX box is fairly good (Fig. S2 below, coming from Rampal et al. [2016], Fig. 2). However, we acknowledge that comparing buoy and model drift speed has to be done with caution, as buoys measure the drift speed at one particular location while the model is meant to give the grid-cell average. This information has been added in the manuscript (Section 2.3).

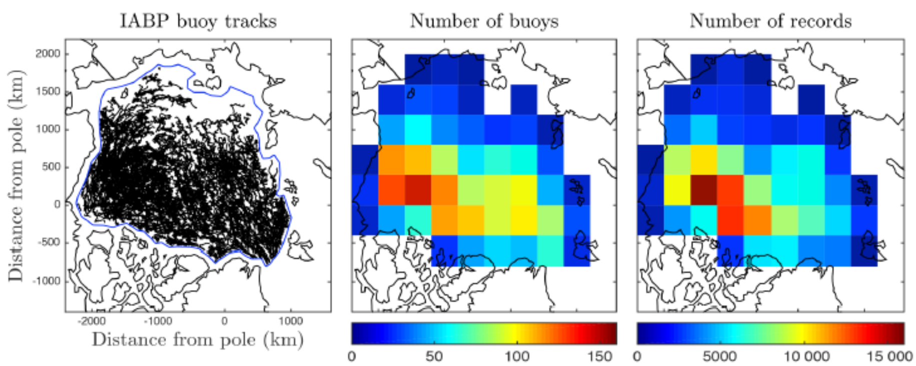

Fig. S2: Buoy tracks from the IABP dataset for the winter periods 1979–2011 (left panel) and the corresponding number of buoys (middle panel) and records (right panel). Source: Fig. 2 from Rampal et al. (2016).

- Page 9, line 2-3: Why the authors show the relationships in terms of mean seasonal cycle, not by scatter plots based on the relations in each month? I mean, for example, a scatter plot for drift-concentration relation in each month can more clearly show the validity of the regression line.

Our aim is to show how changes in drift speed are linked to changes in concentration and thickness for each month of the year (mean seasonal cycle) for both the model and the observations as in Olason and Notz (2014). We agree with the reviewer that it is also interesting to show these relationships for each month of every year but this produces a noisier picture (especially for the thickness loop). Please see Fig. S3 below, where we plot scatter plots for every month of each year. They show that drift-concentration and drift-thickness slopes are similar to the ones using the mean seasonal cycle (Fig. 8a-b in the revised manuscript), with slightly lower model performance. This information has been added in the revised manuscript.

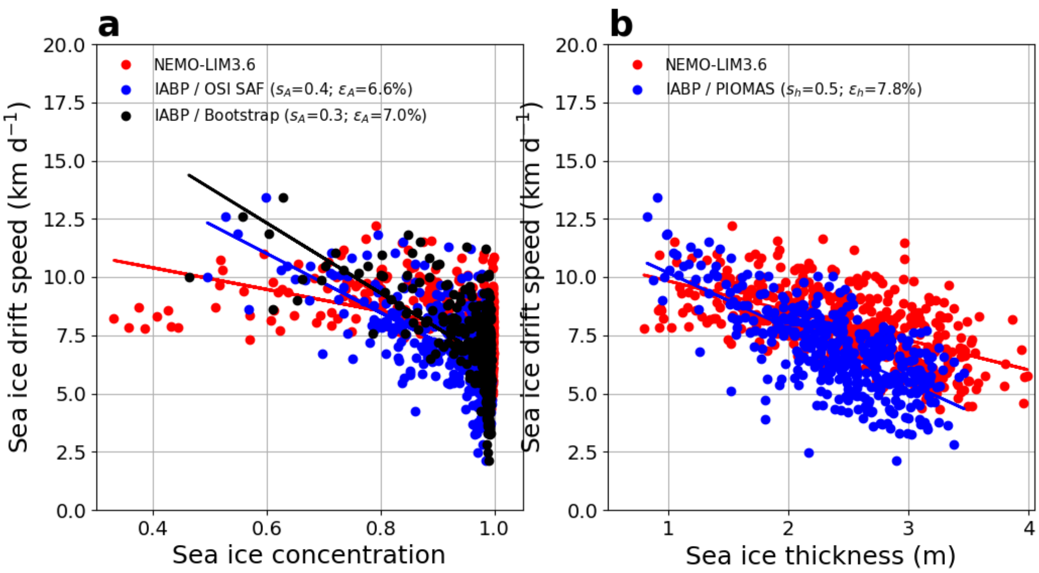

Fig. S3: Scatter plots of modelled (NEMO-LIM3.6) and observed monthly mean sea ice drift speed against (a) concentration and (b) thickness spatially averaged over the SCICEX box for each month of the period 1979-2013. Slope ratios and normalised distances between NEMO-LIM3.6 and the different observation/reanalysis datasets are shown in brackets in the legends.

- Page 9, line 16-17: Why the authors did not apply normalization by wind stress? Since the wind stress may differ between each month, it is difficult to derive a general relation between drift speed and other variables. If there is a reasonable explanation for not to apply normalization, please describe in the text.

For normalising drift speed by wind friction speed, we use the same methodology as in Olason and Notz (2014), except that we use wind speed from DFS5.2 since this is the atmospheric forcing of our model. We also use air density of 1.225 kg/m^3 and drag coefficient of 0.0015. As shown in Fig. S4 below, the normalised drift-concentration and normalised drift-thickness relationships look very similar to drift-concentration and drift-thickness relationships without normalisation (compare to Fig. 8a-b in the revised manuscript). Therefore, we decided not to perform the normalisation, which facilitates the interpretation of data. Please see Section 2.3 of the manuscript: 'A key difference with Olason and Notz (2014) is that we do not normalise drift speed by wind friction

speed since our findings were not sensitive to such normalisation'. Please also see our response to Referee #1 (p. 6 l. 29).

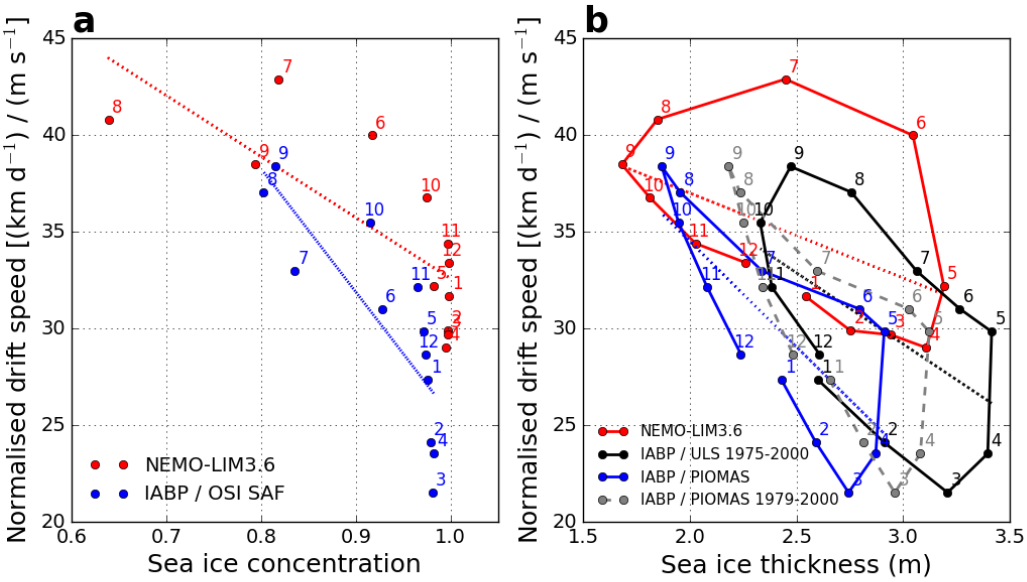

Fig. S4: Scatter plots of modelled (NEMO-LIM3.6) and observed monthly mean normalised sea ice drift speed against (a) concentration and (b) thickness spatially averaged over the SCICEX box and temporally averaged over the period 1979-2013. Drift speed is normalised by wind friction speed (derived from DFS5.2) as in Olason and Notz (2014).

- Page 9, line 30-31: I think the use of 'feedback' in this sentence is not appropriate. The result in this section (sec. 3.2) shows that there is a (linear) relationship between drift speed and strength (thickness or concentration) as equilibrium states (A feedback system does not reach an equilibrium state unless α = 0, or having oscillating solution. see my major point). I don't mean the analysis in this section is meaningless, but is not appropriate to show the existence of feedback (see also major point).

We changed the term 'feedback' into 'interactions' as we think it is more appropriate. Please also see our response to the first major point above.

- Page 10, line 9-20: There are a number of incorrect sentences here. Please keep in mind that higher λ does not directly correspond to larger ice strength, due to the current formulation, Eq. (1). Particularly, it is not true, when discussing relation between ice drift and thickness (or concentration) in summer season. The thickness may thinner than 1 m at least part of the SCICEX box.

This part has been re-written due to the inclusion of new P* experiments and in response to the second major point of the reviewer.

The mean seasonal cycle of ice strength P for the different λ values is shown in Fig. S5a below, where we see that ice strength is higher for higher λ most of the time, with very small differences in summer compared to winter. The zoom in summer months (Fig. S5b below) reveals that this relationship still holds (higher strength for higher λ), except for the λ = 2 curve in summer, which is located between λ = 1 and λ = 1.5 in July, and between λ = 0.5 and λ = 1 in August and September.

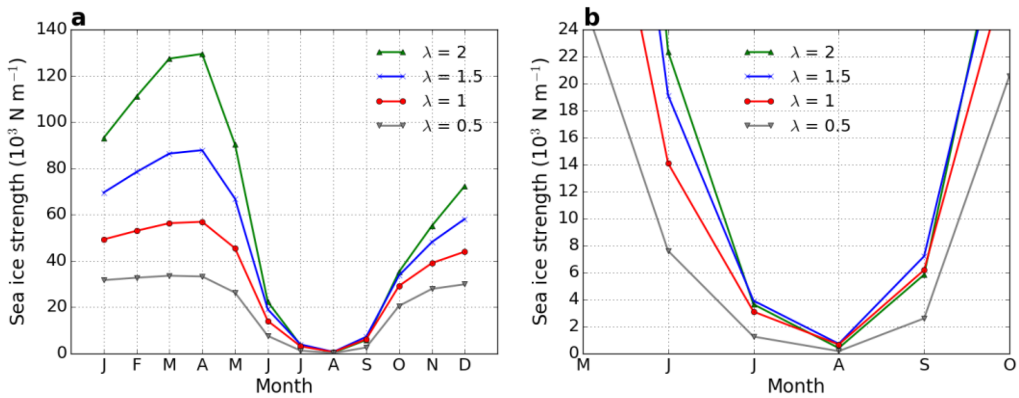

Fig. S5: (a) Modelled (NEMO-LIM3.6) monthly mean seasonal cycles of sea ice strength temporally averaged over the period 1979-2013 and spatially averaged over the SCICEX box for four different λ values (see Eq. (1)). (b) Snapshot of (a) for June-September.

- Page 10, line 16-20: I think this interpretation is wrong, probably due to the fact which I described in major point. Since the ice thickness during summer season is close to (or even smaller than 1 m), the ice strength for larger λ becomes smaller than that for smaller λ. Therefore, the larger drift speed for larger λ can be simply the result of smaller ice strength.

As demonstrated in our response to the previous comment and in Fig. S5, higher λ leads to higher strength when averaging over the SCICEX box, except for the λ = 2 curve from July to September. Therefore, the larger drift speed for larger λ in summer in Fig. 10b in the previous version of the manuscript is not driven by lower ice strength P (except maybe for λ = 2). Furthermore, a similar behaviour is observed with P* experiments: higher P* leads to higher drift speed in summer (except P* = 100kN/m²) (Fig. 9b in the revised manuscript).

- Page 10, line 32-33: I would say the result is not counter-intuitive. Note that increase of λ leads to smaller ice strength in h < 1 area, it means ice can be easily deformed or compressed in h < 1 area, compared to small λ.

We rephrased this following comments from both reviewers and due to the use of new P* experiments.

- Page 10, line 4 - page 11, line 8: Section 3.3 needs additional figure showing the relation between λ and mean ice strength in SCICEX box (a seasonal cycle for each λ should be shown), otherwise, we cannot relate ice strength with ice thickness (and concentration) nor examine the relation between ice strength and ice drift.

Please see our response to the comment 'Page 10, line 9-20' and Fig. S5. However, the main focus is now on P* experiments, which are simpler to interpret. We only discuss λ experiments in Section 4.2.

- Page 10, line 33 - page 11 line 6: I think the reason for the increase of modal thickness for larger λ (Fig. 13) can be simply explained. The experiment with increased λ has larger ice strength for h > 1 (winter season), which prevents ice thickening due to ridging, while it has smaller ice strength for h < 1 (summer season), which enhance ice thickening due to ridging. As a result, larger λ leads to larger peak at modal thickness.

We are not sure about the validity of this hypothesis since we obtain similar results with the new P* experiments (in which larger P* always lead to larger ice strength P), i.e. decrease of modal thickness and decrease of ice thickness heterogeneity for larger P* (Fig. 12 in the revised manuscript).

- Page 11, line 14-15: I would say that this study also did not 'quantify' the magnitude of the drift-strength feedback. To quantify a feedback, one should present growth rate of the feedback.

We rephrased this paragraph since we focus now on the interactions (and not on the feedback) between drift speed, concentration and thickness. Please see our response to the first major point above.

- Page 11, line 23-24: Due to the analyses without normalization by wind stress, it is difficult to distinguish the reason for the hysteresis loop shown in Fig. 9b and Fig. 11b. Can the hysteresis loop be observed if the ice drift speed is normalized by wind stress?

As shown in Fig. S4b, a hysteresis loop is also present when normalising drift speed by wind friction speed.

- Page 12, line 14 - page 13, line 15: As pointed in major point, I think the basic strategy for the sensitivity experiment and analyses are not suitable for quantifying 'feedback'. If the authors intend to quantify 'feedback', the entire framework should be reconsidered.

Please see our response to the first major point above.

- Page 14, line 16-18: Why such hysteresis loops are observed in the modeled sea ice? Since observation (IABP) does not show such a feature, I guess this is an artifact coming from insufficient modeled physics.

As shown in Fig. 8b in the revised manuscript and in Fig. 4b of Olason and Notz (2014), observed drift-thickness relationships are marked by a hysteresis loop: for a given thickness, two different drift speed values exist depending on the season.

- Page 14, line 21-28: The summary provided here should be reconsidered, by taking the second paragraph of the major point into account. Although the authors generally provided descriptive result of their analyses on ice drift - thickness relations, they did not show any results on thermodynamic analyses. Therefore I cannot follow nor rely on the arguments associated with thermodynamic effects.

We re-wrote this part of the text since a new set of P* experiments have been performed. Concerning the thermodynamic analyses, we refer the reviewer to our response to the first minor comment of Referee #1 (p. 1 l. 11).

5. References

- Lindsay, R. and A. Schweiger, 2015: Arctic sea ice thickness loss determined using subsurface, aircraft, and satellite observations, The Cryosphere, 9, 269-283, doi:10.5194/tc-9-269-2015.

- Szanyi, S., J. V. Lukovich, D. G. Barber, and G. Haller, 2016: Persistent artifacts in the NSIDC ice motion data set and their implications for analysis, Geophys. Res. Lett., pp. 10800-10807, doi:10.1002/2016GL069799.

- Tschudi, M., C. Fowler, J. Maslanik, J. S. Stewart, and W. Meier, 2016: Polar Pathfinder Daily 25 km EASE-Grid Sea Ice Motion Vectors. Version 3. Boulder, Colorado, USA. (https://nsidc.org/data/docs/daac/nsidc0116_icemotion.gd.html).

Additional references included in this reply that are not in the revised manuscript:

- **Fowler, C., W. Emery, and M. Tschudi, 2013: Polar Pathfinder Daily 25 km EASE-Grid Sea Ice Motion Vectors, version 2, NSIDC, Boulder, Colorado.**