Reply to Referee #1

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 #1 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 **blue**. The corresponding corrections are in **blue** in the revised manuscript.

1. Overview and major comments

In this paper, the authors analyse the results from the ice-ocean model NEMO-LIM3.6, forced with atmospheric reanalysis, in order to better understand the drift-strength feedback in the Arctic. Based on previous work the authors propose new metrics and use those, as well as other metrics and diagnostics to evaluate their model against observations and results from the PIOMAS model. They then discuss how their evaluation relates to the drift-strength feedback and do a sensitivity experiment to evaluate how ice strength in their model affects the modelled drift-strength feedback.

It's always nice to see modellers evaluate their model results against data and the authors should be commended for making the effort here. It was also nice to see an evaluation that goes beyond considering just the concentration and extent and I enjoyed seeing that the authors are trying to push for new methods of analysing their model.

My main reservation, though, regarding the paper is the premise of the drift-strength feedback, as presented here. In particular, the authors state that larger sea-ice drift leads to larger exports, but this does not seem to be the case. It is well established that the drift speed of ice in the Arctic is increasing, but at the same time there seems to be no clear increase in (Fram Strait) export. Some studies do find an increase, while others find no increase or a decline in the export. The authors themselves choose (very rightly I think) to cite Döscher et al., which say that there is no significant long-term trend in the area export and a slight decrease in the volume export (p. 2, l. 20 of the manuscript). Thus, we have established increase in the drift speed and no increase in export and we therefore cannot connect the "Drift" and "Export out of Arctic Basin" boxes in figure 1. This puts in question the premise of the paper and some of its contents (though not nearly all).

The reason we don't see an increase in export even if the drift speed increases is that the increase in drift speed is in the synoptic-scale back-and-forth movement of the ice, not the long-term, large-scale drift. This is highlighted by Olason and Notz when concentration is low, but it also seems to be the case when concentration is high. I consider this a major shortcoming of the paper and recommend that the authors re-think and re-structure its contents. There is good material here which, with some re-structuring and extra work can be made into a good paper.

We agree with the reviewer that the drift-strength feedback that we present in our study has not been formally demonstrated by any previous study. Rampal et al. (2011) suggest it might be an important feedback but its existence has never been proved formally. Due to the lack of observational evidence to confirm this feedback, the results we obtain with our sensitivity experiments and the remarks from both reviewers, we decided to change the focus of our article. We now concentrate more on the interactions between sea ice drift speed and strength (concentration and thickness) rather than on the feedback itself. Please see also our response to the first major point of Referee #2.

2. Minor comments

- p. 1 l. 11: You say "We demonstrate that ... leading to lower heat conduction fluxes ...", but there is no analysis of the fluxes provided. As it is you don't "demonstrate", but "suggest" or "speculate". An actual demonstration of this would be very interesting to see, especially since I don't think this is what's happening. I would think that higher ice strength results in less ridging which then results in less volume. This is the result of Steele et al. (1997), as well as Flato and Hibler (1995) and I tend to think this is what you get as well.

We do not have the model outputs of heat conduction flux for the sensitivity experiments with varying λ. However, we performed new sensitivity experiments in which we vary P* based on the comments from both reviewers, and for these experiments we made sure to compute heat conduction fluxes. The main results are:

- **lower P* leads to higher average ice thickness (Fig. 9a in the revised manuscript) and higher ice thickness heterogeneity in space (Fig. 12 in the revised manuscript), which is in agreement with our previous λ experiments**
- **lower P* leads to lower heat conduction fluxes and lower thermodynamic ice production (Fig. R1 below), which is not in agreement with our initial hypothesis.**

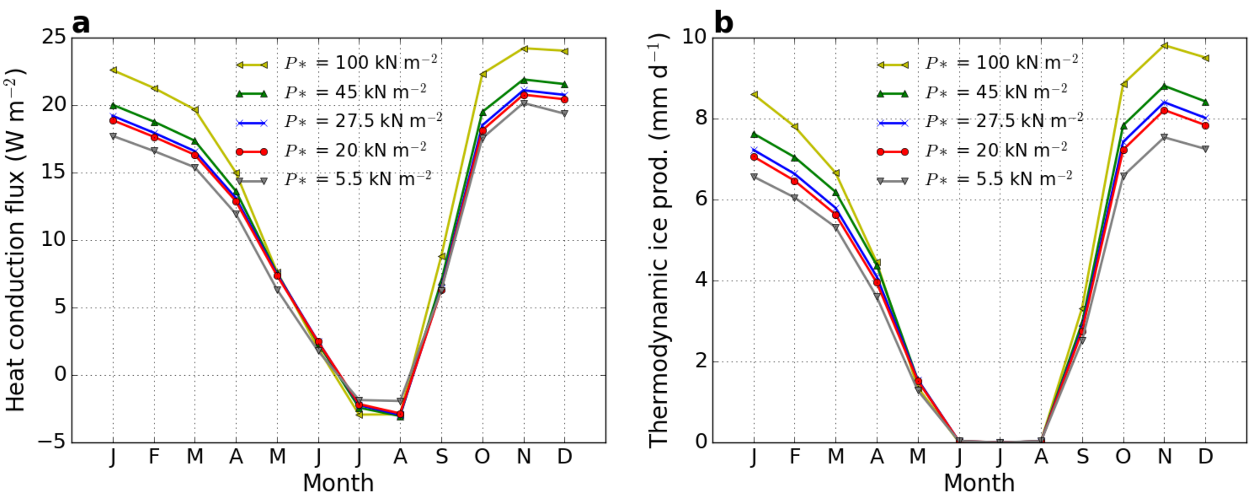

Fig. R1: Modelled (NEMO-LIM3.6) monthly mean seasonal cycles of (a) heat conduction flux at the ice bottom (positive from the ocean to the atmosphere) and (b) thermodynamic ice production temporally averaged over the period 1979-2013 and spatially averaged over the SCICEX box for five different P* values.

Therefore, our initial hypothesis (lower initial ice strength leads to higher sea ice thickness heterogeneity, which results in higher heat conduction flux and higher ice production, hence larger ice thickness) is not the right reasoning that explains why ice thickness is higher with lower initial ice strength. We agree with the reviewer that the process is simpler: lower ice strength leads to higher deformation and more ice piling up (Fig. 11 and Table 2 in the revised manuscript), which results in higher ice thickness. We adapted the manuscript accordingly.

It is important to note that the results obtained by Steele et al. (1997) and Flato and Hibler (1995) arise from a different experimental setup than the one used in our study and do not exactly support the hypothesis that higher ice strength leads to less ridging and less volume, as we discuss now.

In the former study (Steele et al., 1997), the standard value P* = 27.5 kN/m² is used in a sea ice model based on Hibler (1979) with a grid resolution of 40 km. This standard value is decreased and increased by a factor of 5 respectively. In the case of P* decreased by a factor of 5, the mean ice motion is faster (the ice is nearly in free drift) and the mean wintertime ice thickness is 35% higher than in the standard case. Increasing P* by a factor of 5 locks sea ice and motion of sea ice ceases, which produces an ice thickness that is essentially determined by equilibrium thermodynamics. In this case, the mean wintertime ice thickness is also slightly higher than in the standard case (by about 6%). A range of further sensitivity experiments performed by Steele et al. (1997) shows that the dependence of mean ice thickness h on P* is nonlinear (see their Fig. 16), with a sharp decrease of h with increasing P* for P* <= 27.5 kN/m² and then a slight increase of h with increasing P* for P* > 55 kN/m². Furthermore, ridging is not mentioned in their study. Therefore, not only the experimental setup of Steele et al. (1997) is different from ours, but also the results show a nonlinearity that we do not find (maybe because our λ values and P* values were not increased enough). The comparison of these results with our results is now discussed in our revised manuscript (Section 4.1).

In the second study, Flato and Hibler (1995) use a sea ice model based on the thickness distribution theory of Thorndike et al. (1975), which has a resolution of 160 km (which is much lower than the one used in our study), and perform sensitivity experiments by varying different ridging parameters. Since these experiments use a different model and a different experimental setup, we think it is difficult to perform a detailed comparison of their results with ours. But the results of Flato and Hibler (1995) do generally support the idea that more sea ice ridging leads to greater sea ice thickness.

- p. 3 l. 29-32:

* You use P* = 20 kN/mˆ2. This is quite small. The “canonical” value of Hibler and Walsh is 27.3 and that was using daily forcing. What is the temporal resolution of your forcing? If it’s something like every 6 hours then you should be using a larger value than Hibler and Walsh, not smaller. You need a reference for this value.

We agree with the reviewer that the value of P* = 20 kN/m² used here is smaller than the value of P*= 27.5 kN/m² found in Hibler and Walsh (1982), which provides the best agreement between their model and observations in terms of mean drift rates. However, P* = 20 kN/m² is the commonly used value in the NEMO-LIM model and has been chosen via a tuning of mean sea ice

thickness and mean Fram strait ice export (Vancoppenolle, personal communication). It is also the value used in the viscous-plastic models of the Sea Ice Model Intercomparison Project (SIMIP) (Kreyscher et al., 1997) as well as in other modelling studies (Lipscomb et al., 2007; Juricke et al., 2013). Tremblay and Hakakian (2006) find that the most likely value of P* lies in the range 30-45 kN/m² based on satellite observations. Therefore, a wide range of P* values is used in the literature and not a single value is considered as a reference (Feltham, 2008). Since our new sensitivity experiments consider different values of P*, the precise choice of P* is no longer a concern. This information has been added to the revised manuscript (Section 2.1). The temporal resolution of our forcing (DFS5.2) is 6 hours.

* You give no justification for the lambda parameter in equation (1). This is non-standard and requires at least a reference to back it up.

Based on the comments from both reviewers, we performed new sensitivity experiments in which P* is varied. We decided not to show the results of the λ experiments anymore. We only discuss them in Section 4.2.

The main goal of introducing a λ parameter was to test the impact of a change in the strength parameterisation on sea ice drift speed and thickness. Previous studies have performed similar tests by using a square dependence of ice strength on thickness (Overland and Pease, 1988; Häkkinen and Mellor, 1992). Moreover, in the CICE model the ice strength P increases as a proportion of $h^{1.5}$ instead of h in order to improve the physical realism (Lipscomb et al., 2007). According to Leppäranta (2011), the value of λ is an open question (just as P*). Other reasons for choosing λ experiments are also given in response to the second major point of Referee #2.

* Why don't you try different values of P* instead of changing lambda? It is well known to be an extremely uncertain parameter and I'm already suspicious of the value you use.

As said earlier, we performed new sensitivity experiments in which we vary P* based on values found in the literature. We describe these experiments in the revised manuscript (Section 2.1). The chosen values provide an ice strength P range that is comparable to the λ experiments:

- **P* = 5.5 kN/m²: lowest value used by Steele et al. (1997) in their model sensitivity study, corresponding to 27.5 kN/m² divided by 5; this experiment is comparable to λ = 0.5 in terms of strength-thickness dependence**
- **P* = 20 kN/m²: value commonly used in NEMO-LIM3.6 and other modelling studies; experiment comparable to λ = 1**
- **P* = 27.5 kN/m²: reference value found by Hibler and Walsh (1982)**
- **P* = 45 kN/m²: highest value of the likely range found by Tremblay and Hakakian (2006) based on satellite sea ice drift observations; experiment comparable to λ = 1.5**
- **P* = 100 kN/m²: value providing ice strength comparable to λ = 2 and close to the highest value of Steele et al. (1997), i.e. 27.5 x 5 = 137.5 kN/m².**

The main result of these experiments is similar to our λ experiments, i.e. ice thickness increases with decreasing P* (Fig. 9a in the revised manuscript, to compare to Fig. 10a in the previous version of the manuscript).

- p. 4 l. 28: You should really calculate the model speed the same way the observation speed is calculated, not calculate a one-day average from a two-day observation. But the effect here is probably very small.

We also computed the modelled sea ice drift speed the same way OSI SAF observations provide drift speed (i.e. two-day average) but did not find any significant difference compared to our method. Moreover, since we mainly compare our modelled drift speed to IABP buoy data, for which the temporal coverage and resolution are higher than OSI SAF, we prefer keeping the daily temporal resolution. This information has been added in the revised manuscript.

- p. 4 l. 29: PIOMAS is a model, not observations, and I would like to ask you to please not treat it as observations. It has plenty of shortcomings and uncertainties all on its own.

We agree with the reviewer that PIOMAS is not observations and we do not aim to treat it as such. The title of Section 2.2 ('Observations') is probably misleading since we include a brief description of PIOMAS into this section: the goal is more to put all data against which we evaluate our model in the same section rather than providing observations strictly speaking. We renamed Section 2.2 as 'Reference products'.

We also agree that PIOMAS has shortcomings and uncertainties. We now also use ULS submarine observations for sea ice thickness (the multiple regression model of Rothrock et al. [2008]) as well as the merged product of Tschudi et al. (2016) for sea ice drift speed. The manuscript has been revised accordingly.

However, observations also have uncertainties and suffer from sparse temporal and spatial coverage, especially for sea ice thickness (Stroeve et al., 2014; Zygmuntowska et al., 2014; Lindsay and Schweiger, 2015). Upward-looking sonar (ULS) measurements cover the period 1979-2005 but have incomplete spatial coverage and limited records for each year. Airborne (e.g. IceBridge) and satellite (e.g. ICESat, CryoSat) measurements only cover the recent period with very short temporal coverage for ICESat and limited spatial coverage for IceBridge. Lindsay and Schweiger (2015) conclude that 'more research to understand, characterize, and correct these errors [in sea ice thickness measurements] is clearly required before we can homogenize the observational ice thickness record'.

Furthermore, Schweiger et al. (2011) find that PIOMAS ice thickness estimates agree well with ICESat observations in the area for which submarine data are available, i.e. the SCICEX box. Therefore, we think PIOMAS still represents a valuable tool against which we can compare our modelled sea ice thickness since we use the SCICEX box in our study and due to the high spatial and temporal coverage of PIOMAS. We have however tempered our statements to reflect the uncertainty of PIOMAS.

Given the uncertainties of both observational products and PIOMAS, using all of the products together allows us to obtain a range of 'reference values' that is more reliable than the range based on observational products alone.

For drift speed, since we now include the merged product from Tschudi et al. (2016) and due to the temporal resolution of PIOMAS sea ice velocity vectors (monthly), we decided to remove PIOMAS drift speed from our analysis.

- p. 5 l. 20: I guess the paragraph on p. 4 l. 29 belongs here. Just keep in mind that even though Schweiger et al. (2011) is a very nice paper, then PIOMAS is not the truth. I would ask you to reduce considerably your reliance on PIOMAS in this study and try to compare to actual observations instead, as flawed as they may be. You also haven't considered the Rothrock et al. (2008) multiple regression model, which is well worth taking into account here.

Based on the comments from both reviewers regarding PIOMAS, we now use ULS submarine ice thickness observations as well as the drift speed merged product of Tschudi et al. (2016) in our study, and we removed PIOMAS drift speed. Please see our response to the previous comment related to the criticism of PIOMAS.

- p. 6 l. 6: This is not the right reasoning for choosing daily time scales. With daily time scales you capture synoptic-scale variability, but with monthly time scales you average these out and capture the longer-term, large-scale drift.

We rephrased according to the reviewer's suggestion.

- p. 6 l. 8: Given my comment above it should be clear that you cannot use the monthly values from PIOMAS in this way. They contain different physics and you can't just scale with factor two!

We decided to remove PIOMAS drift speed from our analysis due to the error linked to this scaling and the poor results obtained with this reanalysis in terms of drift speed.

According to our results with NEMO-LIM3.6, the scaling between monthly sea ice drift speed derived from daily components of velocity and the one derived from monthly components is two (Fig. 2 in the revised manuscript). A recent study focussing on Arctic sea ice drift speed using 22 CMIP5 models shows that this factor 2 is valid for all the models (Tandon et al., submitted). Therefore, we think that it is a good approximation, even if we agree that there is an uncertainty linked to this scaling.

- p. 6 l. 21: From here on out this section becomes increasingly hard to understand. I had to re-read and then re-read again to completely understand which metrics and diagnostics you use. It's all there, but you're making your reader work way too hard to get the point. Please rewrite and try to make it clearer and better organised.

We re-organised this part of the text to make it clearer.

- p. 6 l. 28: The novelty here is really that you use this method as a way to evaluate your model.

We removed this sentence after the re-organisation made in response to the previous comment.

- p. 6 l. 29: You don’t normalise with wind friction speed, but Olason and Notz (2014) say they do this to take atmospheric stability into account. It is interesting that you find that this is not necessary, it is not what they find. However, I don’t understand why you don’t normalise with the 10 m wind speed at least, since we know there should be a close correlation between drift speed and wind speed. My main concern, however, is that your figure 9b gives a completely different shape for the curve than figure 6 from Olason and Notz (2014). Why is that?

When normalising sea ice drift speed by wind friction speed (Fig. R2 below), we obtain very similar drift-concentration and drift-thickness relationships compared to these relationships without normalisation (compare Fig. R2 below to Fig. 8a-b in the revised manuscript). That is why we decide not to normalise. We think it is easier to interpret (physically speaking) direct data instead of normalised data. Please see also our response to the specific comment of Referee #2 (page 9, line 16-17).

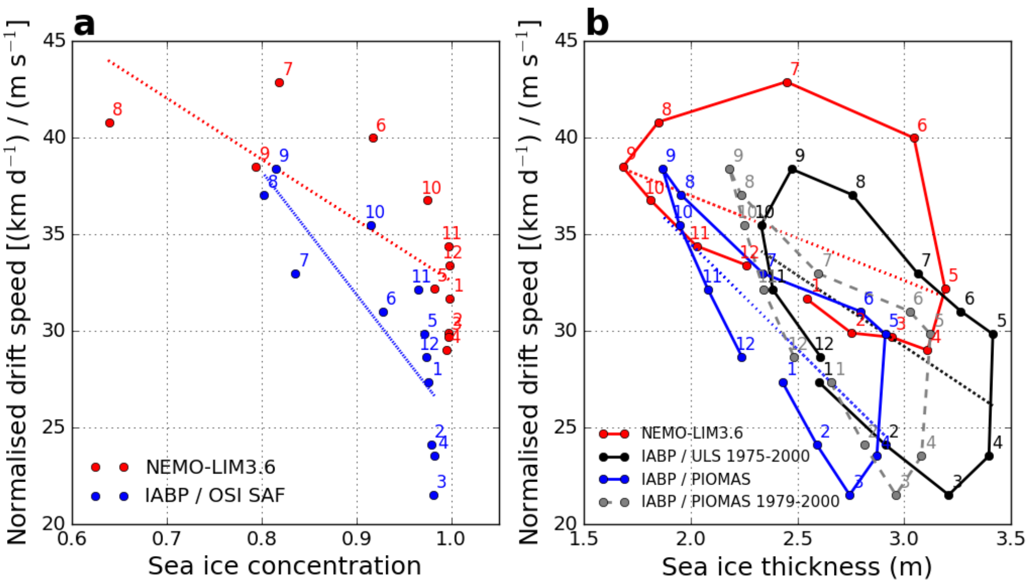

Fig. R2: Scatter plots of modelled (NEMO-LIM3.6) and observed monthly mean normalised sea ice drift speed against (a) concentration and (b) thickness spatially averaged over the SCICEX box and temporally averaged over the period 1979-2013 (except when stipulated in the legend). Drift speed is normalised by wind friction speed (derived from DFS5.2) as in Olason and Notz (2014).

We do not think we obtain a completely different shape for the observed drift-thickness relationship compared to Olason and Notz (2014). Please see Fig. R2b above, where our black curve (IABP/ULS) is very similar to the blue curve of Fig. 6b in Olason and Notz (2014). Our blue curve (IABP/PIOMAS) in Fig. R2b shows thinner ice compared to the red curve of Fig. 6b in Olason and Notz (2014) but we think that is mainly due to the period used, i.e. 1979-2013 in our study and probably 1979-2000 in Olason and Notz (2014) in order to compare to ULS observations. We demonstrate this effect of the period by also plotting sea ice thickness from PIOMAS averaged over 1979-2000 (see gray dashed curve in Fig. R2b): the resulting curve is much closer to the red curve of Fig. 6b in Olason and Notz (2014). The remaining differences probably arise from the slightly different period: Olason and Notz (2014) do not say over which period they compute the mean seasonal cycle in their Fig. 6b.

- p. 7 l. 9: These are probably good metrics you've developed, but you don't use them enough and you don't discuss them enough to make me want to use them too.

We make use of these metrics in Sections 3.2 and 3.3 as well as in Figs. 8 and 13 in the revised manuscript. We also discuss the use of process-based metrics in the framework of a model intercomparison in Section 4.4.

- p. 7 l. 15: Mention (again) the period you average over.

Done.

- p. 7 l. 16: What are the (main) differences between your set up and Rousset's et al. (2015)? If it's just the resolution then remind the reader which resolution you use.

The resolution is different (1° for our study and 2° for Rousset et al. [2015]) as well as the atmospheric forcing (DFS5.2 for our study and CORE normal year for Rousset et al. [2015]). We added this detail in the text.

- p. 7 l. 27 (all paragraph): I'm concerned that you rely too much on comparison with PIOMAS. Again, it's only a model so you should try hard(er) to compare to observations before resorting to comparing with PIOMAS.

We considered the remarks from both reviewers by using other observational datasets. Please see our response to the comment 'p. 4 l. 29'. Furthermore, we also compare the modelled sea ice thickness to ICESat in this paragraph. However, as explained before, the temporal coverage of the latter dataset is very limited (2003-2008) and the measurements suffer from uncertainties.

- p. 8 l. 6 (the paragraph and this section in general) You jump a lot between the SCICEX box and your "wider domain" and I'm having trouble keeping up. Try to decide which is more important, stick to it and mention the other one only when necessary.

We removed all results related to the wider domain from Section 3 (Results) and synthesised this information in Section 4.3 (Impact of domain choice) to make it less confusing.

- p.8 l. 26: What conclusion should I draw from this paragraph? Is the trend significant or a post-processing glitch?

We rephrased to make it clearer. The main conclusions are that modelled trends are good for sea ice concentration and thickness, are less good for sea ice extent (model underestimation, especially in winter) and do not capture the observed positive summer trends in drift speed provided by IABP buoys.

- p. 10 l. 6: You need a justification for using lambda and not tuning P*

We now use P* experiments. Please see our response to a previous comment (p. 3 l. 29-32).

- p. 10 l. 14: It's not counter-intuitive to me, as I mentioned earlier when commenting on the abstract.

We removed this sentence following our results explained above (comment p. 1 l. 11).

- p. 10 l. 25: It's not really a hysteresis loop. Physically the drift speed depends on ice thickness only when the concentration is high, so the change in drift speed only relates to the change in thickness in winter.

We think it is a hysteresis loop in the sense that for a given sea ice thickness, two different drift speed values are found for a given thickness depending on the season (summer vs. winter). As we show in Fig. 8b in the revised manuscript, drift speed does not only depend on thickness when concentration is high (October to March) but also when concentration is low (May to September).

- p. 11 l. 2: Your heat-flux theory contradicts the results of Steel et al. and Flato and Hibler. You need to show that it's true by actually showing the ocean-atmosphere heat flux and analysing that.

We found that our 'heat-flux theory' was not confirmed by the results obtained with the P* experiments (see our response to comment p. 1 l. 11). The manuscript has been revised accordingly.

- p. 11 l. 22: It's only a physical correlation in winter. See my comment for p. 10 l. 25

We do not have enough arguments to make such a statement (only physical correlation in winter). What we observe when we plot drift speed against thickness is an anti-correlation between both variables in both summer and winter. This does not infer causality.

- p. 11 l. 28: I don't know what you mean by "large-scale effects"

This has been removed.

- p. 13 l. 16: I can draw no concrete conclusions from this sub-section

This has been revised. Please see also our response to a previous comment (p. 8 l. 6).

- Figures 5, 6, 8, and 12: These figures are very small and hard to read. It would be better if they focused on the central Arctic. I don't think you would lose much information doing that. Figure 6 is particularly hard to read and I can't see the directions of the vectors at all. It would also be nicer to have the magnitude and direction of the difference, rather than the way it's done now.

Done.