The Cryosphere Discuss.,
doi:10.5194/tc-2017-60-RC1, 2017

The Cryosphere
Discussions
Open Access

Interactive comment on "Arctic sea ice drift-strength feedback modelled by NEMO-LIM3.6" *by* David Docquier et al.

Anonymous Referee #1

Received and published: 23 May 2017

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.

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.

p. 3 l. 29-32:

* You use P* = 20 kN/m^2. 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.

* 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.

* 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.

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.

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.

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.

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.

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!

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.

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

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?

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.

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

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

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.

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.

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

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

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

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.

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.

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

p. 11 l. 28: I don’t know what you mean by “large-scale effects”

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

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.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2017-60, 2017.