# Review of “Interactions between Arctic sea ice drift and strength modelled by NEMO-LIM3.6”

I would like to thank the authors for their responses to my previous remarks. It is clear that they took my remarks under serious consideration. They have, in particular restructured and refocused the article as, in fact both I and the other reviewer felt was necessary. As it stands, the paper is much closer to be acceptable for publication, than was the original submission.

Despite the good progress made I still have two major and some minor remarks that I would like the authors to respond to, before I can recommend publication.

## Major comments

My first major comment is about the drift vs. thickness plots. Reading Olason and Notz, it is my understanding that they view this relationship to be only of relevance during winter, i.e. from November to March. And I agree. The reason for this is that it is only in winter that the ice cover is compact enough so that the ice thickness can play a role in the momentum equation. If the ice cover is very loose the open water part of the cover will “deform”, so to speak, regardless of the thickness of the ice. The straight line between November and March is thus physical, while the straight line between May and September is coincidental.

You can aso see this when looking at the IABP vs PIOMAS curve. I know I said “PIOMAS is not observations” last time, but we know that the shape of the Rothrock et al curve is wrong (they say as much in the paper themselves), and it’s reasonable to assume that PIOMAS is closer to reality in that regard. In IABP vs PIOMAS the straight line in summer is only between July and September; it’s still a straight line, but I would argue that this is coincidental.

Keeping this in mind your metrics should refer only to the November-March period, but it seems the entire curve is taken into account. This is crucial. Also, if you did this you could use your figure 13 b) to see that w.r.t. drift speed P* = 27.5 gives (close to) the right slope, i.e. drift speed-thickness relationship.

My second major comment is that I feel that the structure of the paper can still, relatively easily be improved. I will not stand particularly firm on this, it is more a stylistic comment than a strictly scientific one. But I honestly believe that by improving the structure you can make the paper better focused on the important new methods and results, which will help in making it more widely read, recognised, and cited.

I suggest you really focus on the new metrics you developed. You would then first introduce the metrics and then evaluate NEMO-LIM using them. This second step shows the applicability of the metrics and evaluates the model at the same time. You can then use the

P* experiments to show how you can use the metrics to aid with sensitivity studies. A plot of s_h and epsilon_h against P* w. Finally you show, through the P* experiments, that the drift speed in summer doesn't depend on ice thickness.

This way you put the new metrics and new science in foreground and the evaluation of NEMO-LIM in the background. That would, in my opinion, change the paper from a somewhat utilitarian model evaluation paper to one presenting new metrics and new findings.

## Minor comments

P.2 L.5: Replace "At large scale" with "To first order", or something similar. Changes in ice strength, unrelated to concentration and thickness can have a large scale effect (see Girard et al and the work that follows them).

P.2 L.11: You introduce the phrase "drift-strength feedback", but don't use it again in the revised paper. This sentence is not needed. Also I thought we agreed not to say "feedback" :)

P.2 L.16: I would like you to say "most likely caused by reduced thickness and concentration".

P.2 L.32: change to "... multi-model dataset suggests that in those models thicker and more packed …"

P3. L.18: Change the subsection title to something along the lines of "Model and sensitivity experiment description". Now it sounds like you'll describe the model and perform the sensitivity experiments, but you're in fact describing both.

P6. L.29: You added "sea ice area", but do you really use that? Also, how does that differ from sea ice concentration. One is the true area and the other the fractional area, right?

P7. L.4: You discuss the monthly mean drift speed computed from the daily components of ice velocity at length here. But I don't think it's needed. You don't use this information in the rest of the paper and it's not really relevant to what you're presenting here. Please remove this discussion.

P.7 L.31: When introducing the metrics you don't say which months you use to calculate them. For the concentration related metrics this is probably June-October already(?), but for the thickness it should be November-March, as I mention in the major comments.

P.8 L.12: the part of "which is based on a combination … on the mean 1984-2000 seasonal cycle" is not really necessary here.

P.9 L.10: You should show the OSI SAF drift, even if it doesn't cover summer. There is a huge discrepancy between the two observations you do show, so throwing in the third gives us a better idea of how serious this discrepancy is. Also, the NSIDC data is not really good in summer, last I checked.

P.10 L.5: (Paragraph) See my major point about drift-thickness relationship being coincidental in summer.

P10. L.13: (Paragraph)  I would skip this paragraph and figure. It's not a bad idea, but you need to dedicate much more time/space to discuss the physics and observations, since you have no one to cite on this. For figure 8c: This makes sense, since you'd expect a trend in concentration to coincide with a trend in speed, but the model does the oposite (which is interesting). Why don't you show the NSIDC speeds here? For figure 8d: I can't read anything useful out of this one: A trend in thickness coincides with a trend in drift speed mainly in months where there is no relationship between drift speed and thickness.

P.10 L.23: Skip this last line as it refers to the paragraph I recommend skipping above.

P.11 L.15: (Paragraph) If you just consider the winter months (which is where changing P* really has han effect) then you see that P* = 45 kN/m^2 is actually quite a good fit.

P.11 L.20: (Paragraph) If you consider only November-March for the drift-thickness relationship then P* = 27.5 kN/m^2 gives a reasonably good result. Also, changing P* has little or no effect in summer, so you shouldn't think about the drift-concentration relationship here. Finally: Why don't you plot s_h and epsilon_h against P*? Here's an opportunity to use your new metrics to simplify the analysis!

P.12 L.11: I've already said that the anti-correlation between drift speed and thickness is (mostly) coincidental in summer, but here you can conclusively show that this is the case (at least in your model). In figure 13a you plot drift speed versus ice thickness for different P*. The figure shows that changing P* has much less effect on the drift speed in summer than winter (and no effect at all in July). Since changing P* has the same effect as changing the thickness we can conclude that the speed-thickness relationship in summer is weak to non-existing. This is something new that I'm not aware of anyone conclusively demonstrating.

P.12 .L15: Be careful when comparing to previous studies here, because the frequency (and indeed the source) of the forcing can influence the optimal choice for P*

P.14 L.1 You added a discussion about your lambda experiments, but I don't really see that this is necessary or helpful for the paper.

P.15 L.1: I like your section 4.4

P.15 L.31: Again, I'm not sure that you can't single out a value for P* that is optimal (in some sense at least).

P.16 L.5: What other processes than the drift-strength feedback are important? I'm not sure what you're trying to conclude here or on what it is based.

Figures:

Figure 1: This is not really needed, it's just a linear relationship.

Figure 2: Also not needed since I asked you to remove the discussion related to it.

Figure 5: The difference plots should show the absolute speed difference and the direction of the difference.

Figure 13a is not relevant since changing P* doesn't affect the concentration directly.

In general you have too many figures, some not necessary at all and some just uninteresting (especially if you restructure further). I barely looked at figures 4, 5, 7, 10, 11, and 12, since they are not involved in the most interesting part of the discussion.