Review of Sea-ice deformation in a coupled ocean-sea ice model and in satellite remote sensing data

Dear authors,

Thank you for your revised version of the paper. It is clear that you have put in substantial work to address the shortcomings pointed out by the reviewers and I appreciate this. I would also like to reiterate what both I and the other reviewers have said that this paper has the potential to be an important part of the literature and so it is equally important that it be correct and accurate. In line with that I have some major comments, which I would like to ask the authors should address before I can recommend publication.

Major comments:

I still don't think the two parts (sections 3 and 4 of the revised manuscript) are closely related enough to belong in the same paper. The link between the two parts is weak, as mentioned previously, and I believe that only through a substantial effort can this link be strengthened. An effort the authors don't seem to be willing to make, and neither would I, as a matter of fact. Section 4 is also weak scientifically. It gives little new information. The results are qualitatively already known or obvious for members of the community and the quantitative results are of little use as they are specific to the model and set-up. Besides, the model set-up itself is suspect since you don't initialize the ocean properly (see “other comments” for l.9 p.4). But most importantly the interesting science is in section 3 and you don't need a justification to do it! So from there on out section 4 becomes a distraction, or even an annoyance. I earnestly believe that removing section 4 would only strengthen your paper. As such it is the easiest thing you can do to improve your paper!

You still don't filter the results like Bouillon and Rampal suggest. I really think this is regrettable and encourage you to reconsider this choice. You should note also that the noise Bouillon and Rampal discuss is inherent to the method used to calculate the deformation quantities and not inherent to the RGPS data, per se. This means that using the same method on the model results, as you do, also introduces noise there (although it's probably less in your case, since you use triangles, but Kwok uses rectangles). You can therefore not say that there's noise in the observations and not the model and that this could explain some of the discrepencies between model and observations. If you don't filter, both are noisy. Even worse, since you don't filter then the real spatial scaling of the model is substantially weaker than the one reported in the paper.

I am also still not happy with section 3.1.1. Yes, I accept that the averages were properly calculated, although you still don't say it explicitly. Should be done in the first line of section 3.1.1. However, LKFs form on a much shorter time scale than one month. So what we're seeing in the figures is a superimposition of multiple events, something that is not discussed in the text. This is important, because we don't know if the LKFs that form in the model do so repeatedly in the same place or if we have few strong events. In reality they form in multiple locations. This is, potentially (and in my opinion very probably) a fundamental difference between the model and observations that the current figures and discussion gloss over.

Also, w.r.t. section 3.1.1 I would really recommend considdering a month when the Arctic is full of ice, like March or April. If you do that you will avoid the problem of influences of the open boundary. You will have more pack ice, more LKFs and less MIZ areas. This will help you see the influence of ice thickness, rather than that of concentration, boundary conditions, and ice state (MIZ vs. pack).

In section 3.2.1 you calculate the scaling exponent, *b*, which is a most welcome addition. You do so, however, for the absolute divergence, which is surpriseing to me. We already know that the model has the most difficutlies in simulating the shear rate, so why not calculate the scaling for it? If this gives a poor result and the absoulte divergence is much better, then that is an interesting point for disucssion. In addition it is expected that Hibler's model will have more problems with shear rather than divergence and shear is also the dominant form of deformation in the Arctic. There is therefore plenty of reason to prefer shear over divergence. Even to the point of only showing divergence may look suspicious. The same holds for the PDF calculated in sectino 3.2.2. Also, why don't you do the multi-fractal analysis as well? You have all the ingredients right there and it would tell us more about the distribution of shear rate than the localisation does (section 3.1.3).

Finally, in section 3.2.3 you make a jump from *D* to *b*, which leaves me completely behind. You don't show any dependence of *b* on concentration or thickness, only of *D*. I'm not saying it's not there, but you don't show it. This leaves the conclusions to be drawn from this section unfounded. Also, you note that *b* for the entire area is -0.54, but you previously showed it to be -0.1 for winter and -0.15 for summer. This difference shows you already that you can't compare models of different resolutions using a constant *b*.

I really do feel quite strongly that these points must be addressed adiquetly before this paper can be published.

Other comments:

l.18 p.1: There are no new quantitative metrics here. Everything you do has been done before, but not at such a high resolution VP model - that's what's new here.

l.23 p.2: A *linear* viscous rheology is not enough to reproduce the basic ice state

l.28 p.2: Should be 1,000 km, not 10,000 km

Table 1: You don't say it when introducing the table, but you are using the setup of Nguyen et al (2011), right? These are some interesting parameters you have there. The ice albedos are high (although not impossibly so), but the wet albedo is higher than the dry albedo. This cannot be right. The ocean albedo is also 0.16,

but I'd expect it to be more like 0.06 or maybe 0.10 at the highest in the Arctic. The air-ice drag coefficient is $1.1x10^{-3}$, which is suitable for daily averaged geostrophic winds. I assume you're using 10 m winds every 6 hours. In this case the drag coefficent should probably be double what you have. I've never used JRA-25 myself, but it is very much off if you really need such a low drag coefficent. P^* is also small and H_0 large. Not impossibly so, but suspiciously. I'm not sure what to make of all this, but it does not confer confidence in your setup.

l.9 p.4: Why don't you use ECCO2 as initial conditions for your model? If this was an ocean modelling paper then this would be grounds for rejection. You're giving your system a nasty shock by initializing with WOA, but forcing at the boundaries with ECCO2. Also, initializing with WOA requires you to spin up your system for a long (long) time. Fortunately for you then we're only interested in the ice and this probably won't affect the results of section 3 too much. Section 4 however is a different matter since the ocean state is important for the changes you're looking at there. At any rate you should re-run with propper initialisation.

l.19 p.4: The 4.5 km resolution solution does show more structure, but you can hardly call it leads when they are full with about 1 m thick ice.

l.19 p.4: You shouldn't refer to Menemenlis et al here but rather Nguyen et al (2011). Menemenlis et al doesn't even include the Arctic!

l.23 p4. Here's the Nguyen reference. It should come earlier.

l.29 p.4: “modest increase” instead of “modes increase”? Also you mean to compare the 4.5 and 9 km runs to the 18 km one, right?

l.7 p.5: You need to define “winter” and “summer”. Are the seasons consistently used throught the paper?

l.21 p.5: “We are bilinearly interpolating” is not propper english. “We interpolate … using a bilinear interpolation” could be one way to go.

l.24 p.5: You use daily output for your calculations, but unfortunately that's not high enough temporal resolution for the 4.5 km grid, since the daily displacement is (often) larger than this. It means that you will transport your Lagrangian points over more than one Eulerian grid cell each time you move them and this causes errors. You should re-run with higher output frequency for the velocities (6 hourly should be ok, I guess), or at the very least discuss the issue.

l.2 p.7: Griard et al also use reconstructed Lagrangian trajectories, like you do. How is what you did different?

l.15 p.7: You look quite closely at November 1999, but there's no mention of any other months or years. Do they look similar? Can we see seasonal differences or inter-annual differences. It's fine to show just one month, but we need to know it's representative.

l.26 p.7: It is to be expected that the vorticity is the best because this is mostly inherited from the atmosphere (and ocean). This should be noted.

l.5 p.8: Ok, here you say it's the same for all 97 months - did you visually inspect them all?

l.17 p.8: The main difference between the seasonal and perennial sea ice is not neccessarily the ice thickness. This is the case for Hibler's model, but in reality floe size and the level of fragmentation and fracturing of the ice is probably more important, or at least as important. This should be noted.

l.22. p.8: Here you outline why it's a bad idea to choose November. You should pick a different month.

l.33 p.8: This part should contain a discussion on why the model is better in summer than winter. Or at least some ideas.

l.24 p.9: This was done first by Marsan et al in 2004, then by Girard et al and Stern and Lindsey in 2009. Marsan et al should be cited.

l.30 p.10: It's hardly a controversy. Controversy requires quite lively debate beween oposing viewpoints, but you're the first person to try to reproduce what Girard et al did (or at least to try and publish it), so there has been no debate in the literature.

l.3. p.11: We need to know the shear rate scaling, as mentioned above.

l.1 p.12: As noted above you need to be carefull when interpreting the noise. There is also noise in our model results.

l.13 p.12: Please show shear rate, not divergence. Also, why use L=20 km, why not 10?

l.16 p.12: What kind of linear regression - least mean squares?

l.18 p.12: Again, be careful with the noise

l.1 p.13: It's only the slopes of the tailes that are in good agreement

l.15 p.13: Now you use D again, why are you switching back and forth?

l.19 p.14: For free drift b gets less negative, not more.

l.24 p.14: This paragraph tells you well why November is a bad choice for section 3.1.1

l.35 p.14: Be sure to stress that it is only in the model that the scaling depends on ice strength, and it is only in the model that the strength depends on concentration and thickness. In reality there more things that come in to play (floe size and level of fragmentation and fracturing for instance).

l.4 p.21: Again, “the slope of the tails of the PDF”, not the PDF it self.

l.14 p.21: Ice conentration and thickenss is not the same as the ice internal stress!