Answer to RC4 (Bruno Tremblay and Amelie Bouchat) for the manuscript "Sea Ice Deformation in a Coupled Ocean-Sea Ice Model and in Satellite Remote Sensing Data" by G. Spreen, R. Kwok, D. Menemenlis, and A.T. Nguyen

Dear Bruno, dear Amelie,

Thank you very much for your detailed and very helpful review of our manuscript. Find below your comments in blue and our answers to them in black. We will follow them closely and think that we can address almost all of them in a revised version.

In this paper, the authors present: 1- a sensitivity study of the simulated sea ice mass balance on the sea ice strength parameterization and 2- a sensitivity study of the simulated sea ice deformation (divergence, shear, vorticity) on the spatial resolution of the model. The model is the coupled ice-ocean MITgcm with a two-category ice thickness model and a viscous plastic sea-ice rheology. The pressure term in this model is the standard parameterization of Hibler (1979) with a linear dependence on h and exponential dependence on sea ice concentration.

The authors show that a lower ice strength parameter leads to a reduced net annual ice export through Fram Strait and an overall reduced ice production in the simulations after 8 years of integration. They show that the reduced ice export is the dominant mechanism explaining an increase in ice volume in their runs with reduced ice strength. They conclude that the ice mass balance in coupled ice-ocean models is very sensitive to the value used for the ice strength parameter.

In the second part of the paper, they compare their simulated deformation fields (divergence, shear and vorticity) at different spatial resolutions with the Radarsat Geophysical Processor System (RGPS) satellite observations on the basis of their spatial patterns, power law scaling and probability density functions (PDFs). They find that the simulated deformations with the highest spatial resolution (4.5 km) agree best with observations on all metrics tested. However, they show that the model does not capture the enhanced deformations (magnitude and spatial density) in the seasonal ice zone at any spatial resolution and that it has a mean total deformation rate that is about 50% lower than observations. The authors attribute this shortcoming to the ice strength formulation being linearly proportional to the ice thickness. On the other hand, they are able to reproduce the power law scaling of the total deformation rate with the spatial resolution as observed in RGPS observations and the PDFs also agrees with those of RGPS – but are in contradiction with results from Girard et al 2009.

The paper is generally well written – despite some awkward sentence structures and typos (see specific comments below). It presents a long-awaited (re) analysis of the scaling law for sea ice deformations simulated by viscous plastic sea ice models – with results that are contrary to what was published in Girard et al. but that are in accord with several other modeling groups that have done similar analysis. This paper constitutes a welcomed clarification. The results on the effect of the sea ice strength parameterization on the sea ice mass balance are also insightful. Given that the Arctic is transitioning to a seasonal ice cover, and that current rheological models do not simulate the correct deformation characteristics of the seasonal pack ice (as reported here) is interesting.

The tone of the paper should be less a little less defensive and/or more assertive. The paper presents very interesting results. Those new results need to be prominent. For instance, negative results are presented first followed by positive results. The particular is presented before the general. The results that cannot be compared with observations are presented first followed by the results that can be compared with observations. All of this makes the key findings of the paper more difficult to find and appreciate. More specifically, a key finding of the paper (one that is buried deep in the paper) is that the simulated sea-ice deformation simulated by a viscous-plastic model follows a power law - contrary to what was presented in Girard et al 2009. The results presented in Girard et al. 2009 cannot be reproduced by the authors nor by any other modeling group in the community, yet it has become common (accepted) knowledge that VP rheologies do not follow a power law. This must really be stated early on and clearly. More suggestions regarding this issue are listed below.

Thank you for this comment. We will restructure the paper following these lines. We will change the order of sections 3 and 4 and start with the model to data comparison first, as both reviews agree that this is the most important part of the paper. Also the order of four sub-sections will be changed to allow a better flow of the results. The power law result will already be mentioned in the abstract.

We recommend that the paper be accepted for publication after having addressed the comments below carefully.

Amelie Bouchat, PhD candidate
Bruno Tremblay

**Major Points**:

1. Page 6: general comment: Since the ice export depends on ice thickness in the central Arctic. I would discuss the change in ice thickness in the Arctic with changing P* first. Then I would discuss the change in ice export. I understand that it is a chicken and egg situation, but still ice will thicken in the Arctic irrespective of lower export because of weaker ice. The lower export is a positive feedback of the increase in ice thickness – i.e. the increase in ice thickness does not compensate for the reduction in sea ice velocity. Now we are reading the paper about the export changes without knowing all a-priori knowledge.

We will change the order of sections 3.2 export and 3.3 ice production/melting and now first discuss ice thickness changes.

2. A discussion of the ice thickness distribution should be included in the manuscript. The fact that the deformations in the model are generally too low in magnitude and too sparse maybe due to the fact that the ice is too

thick. This may also explain why the deformations in the seasonal ice zone are too weak.

Figure 1 shows the spatial distribution of ice thickness for the 9 km model solution. A sub-figure showing the ice thickness distribution for the three model solutions will be added. For a detailed comparison of the model ice thickness to measurements see Nguyen et al. (2011), who use the same 9 km model solution as presented here.

3. We disagree with the interpretation from the authors that the discrepancy between RGPS and the simulated deformation in the seasonal ice zone is necessarily due to the linear relationship between P and h. A map of the simulated ice thickness for March and September for different ice strength would be useful to better understand this issue.

We reformulate this statement to become more a hypothesis. We did some test with changed relationships, which support the hypothesis but this would be a different study and don't think it is necessary to discuss this in detail here.

4. Page 11, line 23: I am not sure we can blame all of this on the linear h dependence of P*. The ellipse results in equally large viscous coefficients (eta and zeta) for the same divergence (in absolute value) and for a given shear. In reality, sea ice would interact little with other ice floes when we have divergent sea ice motion. I would think that in the seasonal ice zone, where there is more space for the pack ice to expand (in regions of coastal polynya, etc), an elliptical yield curve and normal flow rule that gives unrealistically large viscous coefficient in divergence, would lead to reduced deformation as you see here. This is jus another possibility. The point is that I do not think that this can simply be related to the linear dependence of P on h as discussed here.

We will change these sentences to:
"This discrepancy between seasonal and perennial ice hints to a shortcoming of the sea ice rheology used in the simulations. To first order the main difference between seasonal and perennial sea ice is the ice thickness. The model sea ice strength P , as defined in Equation 2, depends linearly on ice thickness h. This is the typical P formulation for a VP or EVP sea ice rheology with two ice classes and might not be the best representation of the P to h relationship. Models with more ice thickness classes often use a $P \propto h^{3/2}$ formulation (Rothrock, 1975; Lipscomb et al., 2007), which can be considered more realistic. There are, however, also other differences between the seasonal and perenial ice zone than the ice thickness. The proximity to open water, for example, will allow more cases of ice divergence at the ice margins than in the ice pack, which might be less well represented by the VP rheology."

5. Page 15, line 20: Start your discussion here where you analyze the results for the same geographical region as that of the RGPS. Then you discuss the caveat associated with including points close to coastlines. I.e. you go from

General to specific. The way it is presented is a little defensive (i.e. you show the problems first and then show what works well). These are very nice results, one that is in conflict with that of Girard et al. but in accord with results from all other sea ice modeling groups. The authors need to make this point more prominent. I would say this point is one of the highlight of your paper and finally clarifies this situation.

We changed the order of Sections 4.4 and 4.5 and first show that the PDFs of the model solutions in general follow a power law comparable to the RGPS observations. This is then followed by the section where we use a power law to make the deformation rates of the model solutions with different grid spacing comparable. Parts of these sections were reformulated to make the findings more prominent.

6. In section 4.4, I would discuss the case where you compute the scaling exponent with same domain as RGPS first, since this is what you are interested in to compare with observations. Then when you know you are doing fine, you can go and discuss the fact that this scaling exponent depends on ice concentration and thickness. Also, 3-day means should be used instead of daily means of deformation to have data as similar to RGPS as possible for the comparison.

In section 4.4 we use the power law dependence of deformation rates to make deformation rates of model solutions with different grid spacing comparable. This approach cannot directly be compared to the RGPS data as also other factors than the model grid spacing will influence the deformation rate between the three model solutions.
We therefore changed the order of sections 4.4. and 4.5 (see last comment) and now start with the power law behavior of the probability density functions, which can be directly compared with the RGPS data.

**Minor Points - A**:

Page 5, line 2: define shear and divergence. They are defined but only much later in section 4.2.

Added reference to strain rate definitions

Page 6, line 18: It is not clear what the authors are referring to by “anisotropic behavior of sea ice”. The authors are using the standard Hibler rheology which is isotropic. This should be clarified.

Yes, there is no sub-grid scale anisotropy and “anisotropic” probably was not the best word here. We were referring to the irregular distribution of ice stress causing e.g. LKFs and ice arches. Changed in manuscript.

Page 6, Line 19: Type-O. “the the”

corrected

Page 6, line 19: These are important sentences. They must be expanded. Describe the ice arching. Show example in a figure? "Leads to change in the sea ice circulation". This is vague. What kind of changes? How are they link with ice export? The paper is about P* and ice export. These must be documented.

We did not explore changes in ice circulation within the Arctic Basin in detail. This was already done by Steele et al. (1997) who find an acceleration of the Beaufort Gyre and a stronger piling up of ice at the coast of North America for reduced P*. We agree that this manuscript should focus on ice export. We will expand the discussion on this. However, the main part of the paper is about the model to RGPS data comparison. For a more detailed discussion about changes in the force balance, Arctic Basin thickness, and circulation we can only refer to Steele et al. (1997). They, however, do not consider changes in ice export, which we show to be one of the main contributors to the observed changes within the Arctic Basin (which we will not explore in detail here).

Page 6, line 20: Again vague statement. What fraction is due to arching, and what fraction is due to changes in the sea-ice circulation. This must be quantified.

See last answer.

Page 6, line 30: Add space before 0.3 P*.

corrected

Page 6, line 30: Is it really interesting to quote the total (sum over years) difference in ice export? I would prefer to see the new equilibrium numbers in km/yr.

We assume by this you mean the mean difference in export from e.g. 2000 onwards. We will add these numbers.

Page 7, line 2. No it should be discussed first. The fact that the change in export cant totally be discussed at this stage suggest that the order should be changed.

Order of sections 3.2 and 3.3 will be changed

Page 7, line 5: "…sea ice export (E^bar)…"

added

Page 7, line 15: I am guessing the export must increase since the ice strength is lower and that the ridging more than compensate for this in the first 5 years. You need to discuss the ice export variation in this part of the paper.

The ice export is not changing significantly between the three experiments during the first two years. Note that the export E is removed for the calculation of the ice production B. We will add the sentence: "This causes the ice production B to increase compared to the baseline. B is corrected for the influence of ice export E, which, however, does not change much from the baseline integration during the first two years (not shown).

Page 7, Line 23: This is counter-intuitive. I would have expected an increase in the ice volume export. Again, two opposing effects are at play: increase ice thickness and reduced ice velocity. A few additional words should be included to clarify this.

Yes, the reduction in ice export is not directly intuitive. Therefore we describe the different effects leading to it in a separate section 3.2 and only reference to it here.

Page 8, line 5: Give many examples or kill "e.g."

Removed "e.g."

Page 8, line 9: The best value for P* is traditionally found minimizing the error between the simulated drift and the observed drift using models where the wind forcing is specified as observed. Of course biases in the thickness field will impact the optimal P*. But in principle, a model that assimilates sea ice concentrations, and ice thickness from satellite and forced with reanalysis data could be used to find an optimal value for P*.

We used a method similar what you describe to find the optimal P* value for our baseline integration. Will add that information to the text.

Page 8, line 12: give references.

added

Page 8, line 28: This should read "from the simulated ice motion dataset…"?

No, we are still talking about the observed RGPS SAR ice motion here. Clarified in text.

Page 9, line 6:"…since November 1996 until 2008…"

done

Page 10, line 5: Why are they removed? Please clarify.

Deformation rates higher than 1 are considered outliers. Clarified in text.

Page 11, line 29: Define the periods here as well (not just in the Table)

Maybe we misunderstand what you mean. Table 3 lists 20 periods. To include them all in the text would be hard to read.

Page 11, line 33: "… on the sea-ice deformation rate"

done

Page 12, line 13: "…slightly differs from this general behavior…"

done

Page 12, line 12: This sentence is not English. "… shows a weak minimum in March in contrast with the RGPS data…"

done

Page 12, line 23: Is the model iterated to convergence? We see much better defined LKFs in a model that was iterated to convergence compared with one that was not, see for instance Lemieux Tremblay (JGR). I am curious if this has an impact on your simulation results.

We did not perform explicit tests regarding the convergence of the model. We, however, discussed the Lemieux Tremblay (JGR) paper and concluded that our iterations should be sufficient.

Page 12, line 24: "… is calculated as:,… where Di are …"

done

Page 13, line 14: say which summer months.

Information added.

Page 14, line 2: missing word or one word too many. "…find an in magnitude…"

corrected

Page 14, line 18: When we do best linear fit in log-log scale the error for large D will be underestimated. I.e. you best fit will preferentially minimize the error for the small D. Can you comment on the impact of doing this?

We are not completely sure we understand the question. For large D the number of observations gets very small and therefore the scatter large. We therefore stop our fit at 0.8. We do not aim to minimize the error for larger D
If you are talking about doing the fit in log space and therefore having a non-linear distribution of D than you are right, the fit will preferentially minimize the error for small D. We cannot comment on the impact on that because that would depend on the question. One has to keep in mind that the probability is also scaled logarithmic and therefore there are many more observations with small deformation rates, which one could argue therefore should have a higher impact on the fit.

Page 14, line 22: typo. Missing dot in -0.54.

corrected

Page 14, line 18: You have already said above that there is a constant b value in the winter and a higher b value in the summer. I.e. we cannot just use a constant value.

Why test the constant b case if this is so? Eliminate this part? Or say why you still want to look at it.

Yes, we agree one cannot use a single scaling exponent b to make detailed comparisons between strain rates from models with different grid spacing. As Fig 10 a and b demonstrates using a power law with constant b is still useful to compare mean (complete domain, yearly) strain rates of models with different resolution. While the reproduced details in sea ice deformation are very different between the three solutions, Fig 10b demonstrates that the mean deformation rate of all solutions is quite comparable if one takes the different grid scales into account. Will add a sentence about that to the manuscript.

Page 14, line 20: "...approaches zero linearly..." instead? "...for 100% ice-covered ... the deformation rate decreases exponentially". The part of the sentence "but in a more exponential way" is colloquial English.

Reformulated

Page 15, line 8: It is not clear why A=1 would prevent the power law to exist. The exponential dependence of P on A is a continuous function. Why are we loosing it only for A=1?

As said in the manuscript we do not have a clear answer to that. From theory one should expect the power law scaling to also exist for 100% ice cover. We are not saying that we are loosing the power law scaling just for 100% ice the bower law scaling exponent is converging to zero for high ice concentrations. We can only speculate that this has to do with the exponential dependence on the ice concentration in the model implementation. We remove this discussion from the manuscript as it is not conclusive at the moment.

Page 15, line 12: "geographic location" is not a physical parameter. I think you mean, that the power law exponent depends on the "mean internal ice stress" which is higher when we are in the proximity of continents.

Yes, will be reformulated

Page 17, paragraph starting at line 24: The authors need to discuss what works first and then discuss what does not work. It is the same content, just the order that needs to be changed.

Yes, agreed. The order will be changed.

Page 18, line 5: Again the order should be reversed. The authors need to discuss the results using the same domain as the RGPS and then the one where they include the regions close to the coastlines.

Yes, agreed. The order will be changed.

**Minor points - B**

Suggestion: "sea ice deformation" should read "sea-ice deformations" in most places in the text. "Sea ice" takes a hyphen when used as a compound adjective.

done

-- PAGE 1 --

Line 8-9 : Replace "All three model simulations can reproduce the large-scale ice deformation patterns but ..." with: "All three model simulations can reproduce the large-scale ice deformation patterns, but small scale sea-ice deformations and linear kinematic features are not adequately reproduced." Then go with "The overall sea ice..." followed by "A decrease in ...".

done

Line 10: Replace "The overall sea ice deformation" with "The mean sea-ice total deformation rate"

done

Line 16-17: "Either way, this study..." Delete sentence.

We prefer to keep this sentence.

-- PAGE 2 --

Line 4-5: Suggestion: Change "or if new sea ice rheologies like the one..." for "or if new sea-ice rheologies (Girard et al. 2011, Sulsky et al. 2007, etc.) have to be used."

Followed your suggestion

Line 6: "(2) brine rejection into the ocean, (3)..." Add "(2) brine rejection in the ocean due to freezing in open water areas, (3)..."

done

Line 13: "were" should be "are"

done

Line 13-15: Suggestion: Change to "The model sensitivity to the model ice strength parameterization is assessed by comparing the model solutions with different ice strength parameters to the RGPS satellite observations spatially and temporally. These comparisons also allow us to study the model uncertainties regarding the sea-ice deformation representation in the current formulation of VP models."

Will follow your suggestion

Line 18: "into a mean and fluctuating field" change to "into mean and fluctuating fields"

done

Line 19: "to evaluate models with first order..." change to "to evaluate models on the basis of their first order mean velocity field and it can be correctly predicted even by simple sea ice models..."

Reformulated along the lines of your suggestion.

Line 20: "Second order sea ice deformation fields..." change to "The second order sea-ice velocity field, represented by the sea ice deformation fields (strain rates), has to be used for comparison to take into account the high frequency fluctuations of the sea-ice velocity field and to assess the quality of the sea-ice rheology formulation."

done

Line 24: "For RGPS deformation rates" should be "For RGPS total deformation rates"

done

Line 25: "a scale dependence" should be "a spatial scale dependence"

done

Line 34: Replace "for example they show" with "showing"

done

Line 35: Replace "Some improvement in modeling sea ice deformation" with "Improvements in the modeled sea-ice deformation"

done

-- PAGE 3 --
Line 4-6: "A recent example...." Delete sentence.

Why? The Tsamados et al. (2013) study should be mentioned. We kept the sentence.

Line 11: Replace "We reconstruct the observed sea ice deformation..." with "Using the VP model, we construct simulated deformation fields on the same spatial and temporal scales as in the RGPS observations."

done

Line 12: Replace "In addition we also compare..." with "We then compare the power law scaling properties of the modeled and observed deformation rates (section 4.4) and we perform a sensitivity study of the deformation fields properties to the model ice strength parameter (section ??)"

Reformulated sentence

Line 13-14: Delete "sea" and "and thereby ice deformation"

done

Line 16: Delete "as a consequence also" and replace "can effect the Atlantic Ocean circulation" with "can also affect the modeled Atlantic Ocean circulation"

done

Line 16-18: "Ultimately, we would like..." Reformulate. Maybe write: "Ultimately, we would like to highlight why the sea-ice strength representation and the sea-ice rheology should receive more attention in models."

done

We looked through the comments in the following and think that we can address almost all of them in a revised version.

-- PAGE 4 --
Line 15: "fit to available satellite and in-situ data..." Data of what? Ice velocity? Ice thickness? Please specify.

Line 22: "As a consequence these higher-resolution simulations exhibit somewhat larger model drifts relative to observations than the 18-km simulation." Does that mean that therefore you would need to increase P* with increasing resolution to slow down the pack? Please state so if it is the case.

Line 27: Replace "thus the local ice thickness distribution" with "thus modifies the ice thickness distribution" and change "Furthermore, changes in the model ice strength alter the sea-ice drift speed..."

Line 28: Replace "changes in sea ice deformation therefore..." with "these changes can alter the equilibrium sea ice volume in the Arctic."

Line 29: Replace "a set of sensitivity experiments" with "a set of experiments" and replace "changes in sea ice deformation to motivate the importance of sea ice deformation" with "changes in ice strength parameter to highlight the importance of using accurate rheological models and sea-ice deformation fields"

Line 31: Replace "start" with "are done"

Line 32: Replace "The sea ice deformation rate" with "The total sea-ice deformation rate"

-- PAGE 5 --

Line 1-3: Rewrite as: ", where nabla_dot is the divergence rate and tau_dot is the shear rate, is used as a measure for the overall sea-ice deformation occurring at a certain point in space (e.g. Stern and Lindsay 2009). The magnitude of both the divergence and shear rates are to some extent controlled by the strength of the sea ice. In our model configuration, we use the typical ice pressure formulation P (or strength) of Hibler 1979:"

Line 13: Maybe it would be worth noting that the differences in the values of P* that are used in different models come in part because of the need to calibrate the parameters of one's model depending on the forcing used (ocean + atm.) and drag formulations. There is also the need to recalibrate this P* parameter depending on the spatial resolution used in the model.

Line 13: What is the time step used for simulations?

Line 18: Add "For any given month, the monthly deformation rate D_bar increases..."

Line 20: Replace "deformation rates" with "simulations"

Line 22: Replace "of these sea ice deformation" with "of changes in the deformation rates and ice velocity on..."

Line 25: Delete "will" and "for a discussion of geophysical sea ice volume change over time, see Nguyen et al. (2011)."

Line 28: Replace "starts immediately to" with "rapidly" and delete sentence "A similar sensitivity...". Instead, add "Hence, after 8 years of integration, the sea ice volume has increased by 7%..." and continue with sentence from line 30-31.

Line 29: Maybe add a sentence here to clearly state that you do have thicker ice in agreement with Steele et al, but what controls the ice volume change in your simulations are the changes in ice export and ice production and melt.

Line 33: Replace "quickly diverges from the baseline. The divergence gets..." with "diverges from the baseline at a much faster rate than for the solution with 0.7P*_0. The rate of increase of the ice volume gets smaller after 1999, but the volume keeps increasing until 2005."

-- PAGE 6 --
Line 1: Why does the volume start decreasing after 2005 in both runs? And there seems to be much more variability in the case P*=0.3P*0. than with P* = 0.7P*0. Can you comment?

Line 4-5: Put this sentence in previous section, and maybe add something like "both these mechanisms are explored in the following sections".

Line 5: Delete "also" and add it on line 6 between "experiments" and "diverges"

Line 8: Add "Even more pronounced is the change" Delete "however".

Line 11: Rewrite: "...(blue shaded area), and during winter, E_bar is lower than..."

Line 12: Delete "however" and "large" and replace "overall" with "the net annual"

Line 13: Add "nearly balance in the course of one year and this results in a net annual decrease in... "

Line 13: Can the very enhanced seasonal cycle of run with P*=0.30P*0 explain the high variability seen in Fig. 1a of sea ice export compared to run with P* = 0.7P*0?

(See comment for p.6 line 1 above.) If it is the case, then I would suggest moving this section before section 3.1 for clarity.

Line 15 : "Intuitively one might expect an increase of ice export for weaker ice since the ice speed increases." Add "Intuitively one might expect an increase of ice export for weaker ice even during winter since the ice speed increases."

Line 15-16: Change "The ice area export (not shown), however, is smaller for both “weak” experiments during the complete year." for "However, during both summer and winter, the ice area export (not shown) is smaller for both "weak" experiments."

Line 17: "The increase in ice thickness..." This isn't shown in the paper. It would benefit the reader to see maps of mean thickness for your runs and could help you explain better the differences in ice volume, export and even later for your deformation fields.

Line 18-20: I am confused here. You are using an isotropic VP model, yet you are talking about the anisotropic behavior of P. It is also not very clear why the export is less during the winter when the ice strength is weaker. Please expand this paragraph with further explanations.

-- PAGE 7 --

Line 11: Please specify in text what a positive/negative delta_B means. Does a positive delta_B means that there is more ice production and negative delta_B means that there is more ice melting?

Line 25-26: Delete "and also small compared to the volume differences caused by the reduced sea ice export (Figure 3b)." In the run with P*=0.3P*0, it is approximately a third of the changes in the ice volume. It is not small.

Line 27-28: "The results suggest that..." Maybe state that up front in section 3.1 when talking about the sea ice volume changes and say that you explain this in the next sections. Or again, move this section before section 3.1

Line 29: Replace "deformation" with "strength"

-- Page 8 --

Line 28: Why not use the "Lagrangian ice deformation" product directly? Or even the Eulerian ice deformation product?

-- Page 9 --
Line 19: Why using triangles and not a square grid? If I am not mistaken, RGPS uses a square grid to calculate these integrals. Also, the error associated with the estimates of deformation are greater when using triangles than with squares. See Thorndike, Kinematics of Sea Ice, Chapter 7 in The Geophysics of Sea Ice, NATO ASI Series, vol 146, 1986. In particular: section 5.4.5 - Errors in Estimating the Large Scale Deformation.

Equations (3) : Do you compute these integrals assuming u/v vary linearly between each corner? Please specify.

Page 10
---------------
Line 4: In what sense do you associate a total deformation of 1 day^-1 to a deformation of 100%? What ratio are you taking to find a percentage?

Line 17-18: Put this sentence before the last one? It is really referring to the fact that you are putting everything on the same grid, not that some runs are under-sampled or oversampled.

Last paragraph: Maybe differences in ice thickness could explain this? If the ice is too thick in the model, it will be stronger and you will have less deformations. It would be nice to see the thickness fields.

Line 30: Replace "...and model shear is worst." with "...and model shear is the worst."

Line 31: Replace "...and model is best." with "...and model is the best."

Page 11
---------------
Line 3: Delete sentence "The picture changes when..."

Line 5: Delete ": divergence, shear and vorticity."

Line 9: "...its deformation distribution is most consistent with RGPS observations." On what basis? PDFs? Spatial Patterns?

Line 16-17: Delete sentence: "The representation of large-scale sea ice deformation..."

Line 18: What is the black contour? How do you define seasonal ice? Please mention in your text.

Line 22-23: "The model sea ice strength P, as defined in Equation 2, depends linearly on ice thickness h. Clearly the linear relationship between P and h is not suitable to realistically model sea ice deformation." As mentioned earlier, the problem here could be instead that the model has too thick ice in the seasonal ice zone....

Line 24: "Models with more ice thickness classes often use a P ~ h^(3/2) formulation (Rothrock, 1975; Lipscomb et al., 2007)" Doesn't this mean that you make ice more stiff? This will not fix the problem that you do not have enough deformations in the seasonal ice zone... it will in fact make it deform even less.

What I see is that the problem here is that your seasonal ice (supposed to be thinner) may be too and not deforming enough... Can you show a map of sea ice thickness? Increasing the dependence of P on h will not help this problem, since stronger ice deforms less and leads overall to an ice pack that is thinner (see Steele et al. 1997 for example).

Line 31: "for visual clarity the period means... " Not clear... Does this apply to figure 8a only? If so, then maybe write something like :
"Figure 8 shows (a) the period-averaged sea-ice deformation rate D_dot, and (b) the monthly-mean seasonal cycle of D_dot (both computed with all 20 RGPS periods available)."

-- PAGE 12 --
Line 1: Are these numbers the total mean? Please specify.

Line 5: Again, I would check the differences in the thickness field to see if it can explain the differences between your runs. Also, the fact that your model seems 50% too low in deformation could again be linked to the fact that the ice in your model is generally too thick, too strong...

Line 11: March instead of May?

Line 12: Replace "and shows a small but, compared to RGPS data, not very pronounced minimum during March." with "and shows a small but not very pronounced March minimum compared to RGPS data."

Line 13: Delete sentence "That is, the 4.5km solution..."

Line 17: Delete sentence "Again the 4.5km solution..."

-- PAGE 13 --
The discussion on Q could maybe be combined with section 4.3.1?

Line 12: Can you give more details about the implications of having an enhanced seasonal cycle of Q in the model?

Line 27: Here do you compute the deformation rates D_dot from the triangulation of the RGPS positions? Or do you use the Eulerian grid of the model? Please clarify.

-- PAGE 14 --
Line 3: Replace "find an in magnitude about 50% lower scaling exponent (i.e. b ~ −0.12 during winter) for the deformation rate." with "find the magnitude of the scaling exponent to be about 50% lower (ie, b approx -0.12 during winter) for the deformation rate."

Line 8: "...the mean sea ice deformation rate" Monthly means?

Line 10-12: As you can see here with your mean deformation rates, you have much higher values than in figure 8 because you are considering regions of very high strain rates (probably near the coast and in the region of the transpolar drift)... If you are to compare those number with RGPS, you have to bring everything on the same domain covered by RGPS only.

Line 13-14: "Some years, e.g., 1997–1999, have clearly reduced summer deformation rates in comparison to, e.g., the beginning of the 1990s or 2007 and 2008." This is not very clear to see on the figure... Maybe plot winter average and summer average on Fig 10 (a) and (b) instead of monthly means?

Line 14-15: Delete sentence "The deformation rate during 2008..."

Line 19: "daily mean", Maybe use a 3-day period to be as close as possible to RGPS?

Line 20: "the power law scaling exponent b is estimated to be −0.54." Maybe you should show the graph with all the daily mean deformation rates as a function of L and plot the regression line you find. It would make it more clear as to where that number comes from.

Line 20-21: "Figure 10b shows the deformation rate time series for the three model solutions normalized to a length scale of L = 10 km, using the estimated scaling exponent b = −0.54" How do you do this normalization to a different length scale?

Line 23-24: "If looked in detail, however, there remain some quite large differences." This is really not clear on figure. Maybe, as suggested earlier, if you present season means in the graph it would be more clear and we could see better the differences.

-- PAGE 15 --

Line 2-3: "The scaling exponent b gets more negative for weaker sea ice and approaches zero for very strong sea ice, i.e., thick ice and 100% ice concentration" Maybe you need to explain clearly what is the relation between b and Fig.10 b and c. It is the spacing between the curves, ie the larger the space, the larger the slope?

Line 6: Replace "even at 100% ice-cover a cell should show power-law scaling behavior." with "a cell should show power law scaling behavior even with a 100% concentration."

Line 7-8: Why is that? So then, can we really expect to find a power-law scaling in winter, when concentration is almost 1 everywhere?

Line 9: Replace "free ice drift" with "free-drift ice"

Line 15: Replace "the b values of" with "the values of b of" and replace "b values between" with "the values of b between"

Line 17-18: Why not start the section with this? And then say that the value of b is dependent on the ice concentration and thickness, so that if you consider different regions in the Arctic you end up with different b's. And then present your results when considering the whole Arctic domain.

Line 30-31: "model output was bin-averaged to the same spatial scale, L = 12.5 km," What does that mean that the data is bin-averaged? Please explain method.

-- PAGE 16 --

Line 5: "A linear regression was applied to the PDFs in log-log space 5 for the deformation rate range 0.03–0.8 day−1, shown as dashed lines in Figure 11." Not very visible on the graph. Could be removed or offset.

Line 25: Girard et al. 2009

Page 17

---------------

Line 5: Replace "(ice growth equals ice export)" with "(ie, when ice growth equals ice export)"

Line 10: Ocean sensitivity was never really mentioned in the paper... Delete this sentence?

Line 11: Replace "more deformation" with "more deformations"

Line 11: "the ocean mixed layer depth increases during winter time." This was not shown.

Line 14: Add "Deformations in Arctic ocean and sea ice simulations..."

Line 20-21: "The largest difference occurs for the magnitude of divergence, which is 67% to 79% too low (Table 4)." I do not recall seeing this clearly stated in the discussion. Please add.

Line 26-27: "This suggests a shortcoming of the ice rheology, for example, the linear dependence between ice strength and ice thickness." Not necessarily... Again, you have to check the ice thickness first. It could be due to the fact that your seasonal ice is too thick.