The Cryosphere Discuss.,
doi:10.5194/tc-2016-13-RC2, 2016

The Cryosphere
Discussions

Interactive comment on "Sea Ice Deformation in a Coupled Ocean-Sea Ice Model and in Satellite Remote Sensing Data" *by* Gunnar Spreen et al.

Anonymous Referee #1

Received and published: 4 April 2016

1 General comments

This paper is essentially split into two parts. The first part discusses how modifications of the model parameter P^* affect modelled sea-ice volume, export, and production and melt. The second part analyses the modelled sea-ice deformation in differently resolved model runs, comparing it to observations using the RGPS set of observations. The two parts are poorly linked, even though the authors do point out that such a link is possible. The splitting of the paper into two parts like this is cause for concern. An immediately obvious way to link the two would be to analyse the deformation patterns of the two low P^* runs used in part one in part two as well. With the current set-up I would recommend spliting the paper in two and expanding on each part. As it stands I will review the two parts of the paper independently, since this makes the most sense

to me.

2 Specific comments

2.1 Part one

In part one the authors consider the effects of decreasing P^* on modelled sea-ice volume, export, and production and melt "to motivate the importance of sea ice deformation for the Arctic sea ice mass balance". I'm not sure the second part really needs this motivation. To me, seeing if we're modelling the deformation correctly is motivation enough. The question of to what extent modelled deformation affects the sea-ice mass balance is also interesting enough on its own. I am, however, not convinced by the approach taken by the authors. There is no comparison to observations or estimates so I don't know whether the normal P^* is even giving a reasonable deformation rate or mass balance, or how changing P^* affects the deformation, other than the deformation rate. We can't compare figures 2b and 8b either, since the d eformation is calculated over different areas for the two. So while we can see that changing P^* does affect the deformation rate we don't know how it affects various other properties of the deformation. It is therefore also not clear (to me at least) that P^* is an appropriate tuning knob to get the deformation rate right. It is a possible one, but more work is needed to show that it is an appropriate one. Also, what happens to the deformation rate once the model has spun up properly after changing P^*? This is not clear, since figure 2b shows the deformation rate for 1992–2009, which is arguably a period of transient response as discussed below.

This leaves us with nothing much to judge the results of this experiment. It doesn't help that what we're looking at is essentially the model's transient response to a large change in its internal mechanics. Normally one would spin the model up to see the effect of a lower P^* on a model in equilibrium, but this is not done here. The authors claim that the model has reached a new equilibrium after about 8 years, but the difference in "sea-ice production/melting" is still changing rapidly at the end of the model run (figure 3c). If we knew how the deformation rate changes from 1992 to 2009 and that the model does not capture that, and that tuning P^* correctly would give the right deformation rate, then we could say something about how simulating the wrong deformation rate gives the wrong mass balance, but the manuscript gives none of those building blocks.

In terms of analysis of the low P^* runs the authors also miss what must be in my opinion the most obvious cause for increase in volume, and that is thickness increase due to excessive convergence. This is also pointed out by Steele et al . (1997), who performed a similar experiment. When the ice is artificially weakened (which is what we should consider is happening when using 30% of P^*) it can be expected to ridge excessively and pile up at the north-Greenland and Canadian coasts. This effect is completely ignored by the authors, even though Steele et al. (1997) discuss it quite nicely and the authors cite that paper. In particular, the authors state that "[o]verall, the decrease in ice export $\bar{E}$ for both "weak ice" experiments explains most of the sea ice volume increase in the Arctic Basin shown in Section 3.1" —- a statement which seems to contradict the results of Steele et al. (1997) without giving due consideration to the piling up of ice. The pile-up of ice is, in my opinion clearly what causes the increased "sea-ice production" that the authors note in section 3.3. From the text it seems clear that the authors consider the sea-ice production(/melt) to be thermodynamic production, but there is no reason to assume that this is the case. Without considering the ice pile-up the analysis of the difference in "sea-ice production/melting" is deeply flawed.

2.2 Part two

Part two of the paper has, in my opinion much more potential than part one. It is really what I was hoping to see when I read the title and agreed to review the paper. In my opinion the title belongs to part two and part one should be releg ated to a different paper. In part two the authors compare the results of differently resolved model runs to the RGPS observations. This is a worthy goal and I would be very interested in a more detailed and thorough analysis of the high resolution MITgcm model. This could function as a continuation of the work done by Girard et al. (2009,2011), and a contrast to that done by Bouillon and Rampal (2015b) and Rampal et al. (2015). I know there are a number of people within the sea-ice modelling community who hope and believe that running an (E)VP model at a higher resolution than Girard et al (2009) did will give better results than what they got. It is, therefore particularly interesting to know whether the results of Girard et al. (2009) hold for the 4.5 km resolution and to get an independent verification, or contradiction of the results for lower resolutions, as well as an indication of the resolution dependence.

Unfortunately the current analysis is inferior to that performed by Girard et al. (2009,2011), Bouillon and Rampal (2015b), and Rampal et al. (2015). The authors of the current work mainly base their conclusions on monthly averaged deformation, which is inappropriate, and on visual and qualitative inspection of the simulated and observed deformation fields. They should instead use the quantitative statistical tools and metrics previous authors have used. This would have made for much more solid conclusions and results that are quantitatively comparable to observations (e.g. Marsan et al., 2004 or Stern and Lindsey, 2009) and the model analysis mentioned above.

I want to stress, in particular that using monthly averages when studying deformation is inappropriate, since nearly all of the deformation happens at a much shorter time scale. This is a major problem with section 4.3.1. If the authors want to consider long-term differences in deformation then figure 7 is a more appropriate approach than figures 4, 5, and 6. I would even recommend taking a multi-month or seasonal average instead of only one month, in that case. It is interesting how large the difference in deformation rate is between the seasonal and multi-year ice is.

I'm also left wondering if the deformation rates used in section 4.5 are monthly averages or not. Using monthly averages there would be inappropriate for the same reason as before, although it is not immediately clear how large an error we get using monthly averages in this case. Should the results in section 4.5 hold then they are a very interesting contradiction of the results of Girard et al. (2009). It does seem strange though, that the authors choose not to remove the noise of the RGPS data as prescribed by Bouillon and Rampal (2015a). They need to either remove the noise or justify not removing it.

It is also inappropriate to consider the percentage of area containing 80% of the deformation as a measure of localisation (section 4.3.3). It should be the largest 15% of the deformation, like Stern and Lindsay (2009) use. Using 80% of the deformation you essentially include all the deformation so this is no longer a measure of the localisation of deformation. The way it stands the metric is essentially meaningless.

The authors also do the power law scaling of deformation rate incorrectly (section 4.4). They use different model realisations (i.e. 4.5, 9, and 18 km resolutions) to determine the scaling, but the correct thing to do is to use a coarse graining method (like the authors named above) and calculate the scaling based on it. The authors of this manuscript argue that the high resolution model gives better results than the low resolution ones, but they then combine all three to calculate the scaling. This makes no sense.

3 Conclusions

I am sorry to say that I will be recommending that this paper be rejected publication in The Cryosphere. The reasons for this decision are the poor structure of the paper, it being split into two unrelated parts, and the substantial shortcomings of both parts. This is quite disappointing since I believe that the comparison of the MITgcm results with RGPS data could be very interesting indeed. My recommendation to the authors is to thoroughly review Girard et al (2009) and the related literature, and then to revisit part two of the manuscript with the aim to refute or support the conclusions of Girard et al (2009) in the case of the 4.5 km resolution simulation, give an indication of the resolution dependence, and to provide contrast with the results of Bouillon and Rampal (2015b) and Rampal et al (2015). If this is properly done then that would make for an interesting paper and one that would be important for further evaluation and development of dynamical sea-ice models.

4 References

All the papers I refer to here are already cited in the paper, with the exception of Rampal et al (2015), which is still under review at The Cryosphere Discussions: http://www.the-cryosphere-discuss.net/tc-2015-127/

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