The Cryosphere Discussions

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Comment on tc-2022-84

Martin Vancoppenolle (Referee)

Referee comment on "Arctic sea ice and snow from different ice models: A CICE–SI3 intercomparison study" by Imke Sievers et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-84-RC3, 2022

Dear authors, dear editor,

The paper builds upon a nice idea to compare two standard sea ice models used for climate simulation and operational forecasts in a very comparable setup.

However, at this stage, I am sorry to say that I am not convinced that the paper's conclusions are robust/useful enough.

I'm also making suggestions for a revised version that would be more convincing to me.

Best regards,

Martin Vancoppenolle

Evaluation

I think the conclusions are not enough informative / robust. This is because:

A. Analyses are based on static map differences

B. It is quite possible that much of the differences are due to differences in tuning

For A (analysis). Both models provide concentration / mass / momentum budget terms. They could be examined, at at least to some extent, in order to interpret the differences.

Currently, static map analysis makes analysis somehow uninformative, for instance on snow. The conclusions say the two models are different in terms of snow. However this is not expected, since both models are not much different in terms of snow, so it should be easy to find why they differ. Is snow density the same in both models ? Do they lose snow to leads the same way ? If not, why would snow depth at winter maximum would differ ?

Another example is that the authors attribute some of the differences in sea ice distribution to differences in drag coefficients. However, they have not related differences to changes in ice drift, which in my intuition would more likely explain differences in ice edge position between the two models.

For B (tuning), some quite possible differences in ice drift velocity and snow depth could be ruled out by tuning (snow density, loss of mass to leads).

For these reasons, I don’t think the paper progresses state of the art with robust enough findings. I just take that models differ, not much, which was expected, I don’t know why and I suspect most of it to be due to different tuning.

This in essence justifies why I find the paper is not robust / useful enough.

I would also thoroughly check writing. There are a series of spelling mistakes that could easily be spotted and avoided.

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Suggestions.

I think a more interesting paper could be made of a comparable idea, with amended approach, questioning and analysis, which I would encourage the authors to attempt.

I would first recommend to be more strict on tuning and tune the two models on more aspects (thickness, snow depth, drift velocity). I would also set up an objective tuning criterion. As done currently I would tune similar parameters but more of them (not only albedo for thickness, but also thermal conductivity for snow depth, and P* for ice velocity?). I could help the authors to find out which parameters could be used for tuning.

Then I would run the 10 years and analyse things around the questions:

-> Are CICE and SI3 different models ?
-> Can they be tuned to very similar same state ?
-> Is sea ice state in these state-of-the-art model all decided by different forcing and initial conditions ?

When analysing model differences, diagnosing budgets would possibly help. Regarding observations I would also recommend to provide errors from observational products (they are available at least for some of them I think) to better qualify whether model-obs differences are meaningful.

I think the conclusion would probably be that both models are very close when tuned appropriately, except a few small differences.

Some comments on the materials.

Figure 2 does not bring much.

One can guess the essence of it from Fig. 1. I think histograms of frequency would have been more interesting to illustrate things more quantitatively. They would tell whether models capture the low ice concentration and high ice concentration modes the same way. See Notz, TC 2014 for details.

Fig. 4. Could you provide error for CS2SMOS? That could possibly deepen your analysis.

There are no units for ice volume (I know them, but not everyone does so).

Fig. 5 Again histogram of differences would help to push the analysis a bit further. You could add them as inset in each of the four right panels.

Fig. 8. Why snow would be different with same forcing in winter. Is snow density different ?

Fig. 9. Could be clearer. Sign convention for differences should be consistent with previous figures and highlighted in the titles. Capitalization in titles should be paid attention to.

Fig. A1-A12. Hardly useful split on 12 figures. I would either put the panel of differences on a single figure, or provide a more synthetic yet with same message analsyis.