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Ocean Science Discussions

Interactive comment on "Results from the implementation of the elastic viscous plastic sea ice rheology in HadCM3" *by* W. Connolley et al.

Anonymous Referee #2

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Review of 'Results from the implementation of the Elastic Viscous Plastic sea ice rheology in HadCM3' by W. Connolley, A. Keen and A. McLaren

General Comments: It is interesting to see the progress in the model community towards more detailed and physically more correct sea ice models is going on. The paper provides an interesting view on how changing the sea ice model towards a more correct one can change the climatology of the model and has a subject that fits well into the scope of Ocean Sciences. The discussion becomes even more interesting as the well-known and widely used HadCM3-model is the subject of the change and results have been booked. Although the future transfer to the HadGEM-model diminishes somewhat its importance, the paper is very interesting and should be published, if attention has been paid to the following specific and technical comments (especially

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to comments S3, S9, S10, S11 and S12):

Specific comments: S1) pg 780 line 6 'But there are obvious deficiencies to the scheme, especially near land, where the wind stress can be directly offshore (and around Antarctica, frequently is) whereas the ocean currents generally flow parallel to the coast.' I do agree with this sentence, but does the model is capable of reproducing the Antarctic katabatic winds, I mean, even if the sea-ice model is good, if the atmospheric model fails to reproduce these winds, no effect will be seen. Can you comment on that?

S2) pg 780 line 19 Please explain that the HadAM3 is the atmospheric model (explain the abbreviation) so that the readers don't have, as I had, to search for it in the joint reference.

S3) pg 782 line 11 'For example, in September in the Southern Hemisphere (SH), Bootstrap and NASA Team agree that the ice extent is 2.0x10ˆ{13} mˆ2.' First of all, please make from this sentence: '... the mean ice extent ...' if that is what has been presented by the number. If it is something else, please make that clear. Secondly, this number doesn't concur with the findings of Zwally et al. 2002 (Inset in their Figure 2) (Reference: Zwally H.J, J.C. Comiso, C.L. Parkinson, D.J. Cavalieri and P. Gloersen, Variability of Antarctic sea ice 1979-1998, Journal of Geophysical Research, 107, C5, 10.1029/2000JC000733, 2002), who give a SH september mean sea ice extent of clearly less than 20x10ˆ6 kmˆ2. Finally, the value given here does not correspond with the figures of your paper (black line on figures 2, 5, 6, 7) that give a maximum extent of about 16x10ˆ6 kmˆ2 in September in the Southern Hemisphere.

S4) pg 782 line 17 Could you please make a table of the different experiments in order to make it more clear later on when viewing a description abbreviation of the run (these abbreviations are rather complex) an easy reference to the table can be given.

S5) pg 783 line 13 and 16 '... are somewhat disappointing ...' and '... is slightly too big ...' In Figure 2 it can be seen that instead of the maximum observations of 16x10ˆ6

OSD
3, S146–S151, 2006

km^2 you have in the standard EVP-experiment a maximum extent of 23.5x10^6 km^2 which is 7.5x10^6 km^2 too much or almost half of the total extent too much. Furthermore, the ancient version of the model had 19.5x10^6 km^2 as maximum extent which is much closer to the observations. I don't think the word 'slightly' is at its place here. And although the form of the curve is better, I would drop the word 'somewhat' out of the first quote too. With the adjustments later on, I agree that the comparison with the observations gets better but the initial comparison is more than somewhat disappointing.

S6) pg 783 line 16 '... but there are regions with large differences, especially around 90E and the Antarctic Penisula ...' If I look correctly on your Figure 1, the largest differences are indeed around 90E but only if you take a very broad region around 90E (it is, if you take into account the eastern Indian Sector and the complete Western Pacific Sector, i.e., from 40E to 160E). The differences around the Peninsula are more West of the Peninsula than really around, i.e. in the Bellingshausen Sector and even a little bit in the Amundsen Sector. Please, adjust your sentence as to be more accurate.

S7) pg 783 line 21 'EVP improves the simulation in some respects around Antarctica: the ice in the Amundsen-Bellingshausen sea is now more concentrated." First of all, it is the Amundsen-Bellingshausen Seas. Secondly, I agree with this sentence, but is this an improvement? As can be seen on Figure 1, the sea ice concentration in these Seas is indeed higher in observations than in the HadCM3 model but only in a rather small region against the Antarctic Peninsula, whereas everywhere else the concentration is already too high in the previous version. An increase in concentration in the new scheme is only making the problem worse in my opinion. So, the word 'improves' is, in my opinion, not correct at all in this sentence.

S8) pg 783 line 23 'The lack of ice there in the standard HadCM3 run hinders interpretation of climate change in the Antarctic peninsula, which is closely linked to the sea ice'. Can you put a reference in here to demonstrate the last part of your sentence? Furthermore, it's 'Peninsula', not 'peninsula'.

OSD

3, S146–S151, 2006

S9) pg 784 lines 16-18 The ice thickness shown in Figure 4 (HadCM3+EVP) is clearly too low for the Northern Hemisphere. At least you should comment on that and discuss this figure more extensive than it has been done now.

S10) pg 786 line 14 '... we choose a value of 5.10ˆ3 for P*.' This value does seems a little bit low. As you point out earlier, normally, a mere 5-fold of this value is used. Can it be that it would be better if you took a higher value with adjustments of the thermodynamic parameters. A problem here is that in the zero-layer Semtner model which is known to be not really adequate to describe the thermodynamics, the representation of the ice thermodynamics and the ice thickness distribution is largely simplified. Also in the Semtner model, the melt rate is known to be underestimated. In your paper, there is also no description how the model handles the leads in the ice. Can the leads stay open, i.e. can they continue to produce sea-ice even with quite a high concentration of sea-ice? There are also other parameters in the thermodynamic model (like the conductivity of snow, ...) that can be adjusted to get some more realistic parameter estimations. All these points should in my opinion be worked out more in your paper.

S11) pg 786 lines 15-20 In the description of results shown in Figure 8, I do not agree with the author that the P* has to be so low. The peaks of the multiyear Arctic sea ice lie around 2.3 m in the P*_27k run and around 1.7m in the P*_5k run. The 2.3m is much closer to the observations for multiyear ice at the end of the 20th century. As there is no explicit mention of the simulated period (is it 20th century, pre-industrial, future?), I assume that it is end 20th century (this should absolutely be mentioned in Chapter 4). As the thickness distribution in the Arctic seems much more plausible in the P*_27k run, this points towards problems with the thermodynamic run (see also point made above).

S12) pg 787 line 8 Glad to hear that the HadGEM1 distribution of sea ice is much improved. I hope that it will help us to better predict the future climate. My comments on this paragraph: 1. You've missed the full stop at the end of the last sentence. 2. As you want to show that this is the best run, it is very important that you show also figures

OSD

3, S146–S151, 2006

of summer concentration and winter ice thickness so that the reader can compare with your figures.

Technical comments:

T1) pg 778 line 21 than instead of that.

T2) pg 779 line 25 Turner (2005) is in the reference Turner (2006). Please correct one of them.

T3) pg 780 line 12 I was not able to find Hibler, 1979 in the reference list.

T4) pg 780 line 22 I can't find Connolley et al., 2004 in the reference list.

T5) pg 780 line 24 I was not able to find Kreyscher et al., 2000 in the reference list.

T6) pg 782 line 13 '... whilst Bootstrap gives 1.6x10ˆ{13}.' Please mention the unit also in this sentence.

T7) pg 783 line 20 'In the northern hemisphere ...' It is 'Northern Hemipshere'.

T8) pg 784 line 9 'Arctic Basin' instead of 'Arctic basin'.

T9) pg 784 line 9 'Bering Strait' instead of 'Bering strait'.

T10) pg 784 line 10 'Fram Strait' instead of 'Fram strait'.

T11) pg 785 line 5 'However, whilst changes do have an effect there is a trade-off between summer and winter sea-ice;' I think the sentence would be much more clear if you would put a comma between effect and trade.

T12) pg 785 line 20 '... figure 6 also shows both schemes combined as HadCM3+P+M_10 which results is a further slight reduction in the ice extent'. Two problems: 1. In Figure 6, you're talking about P+M_5 whereas in the text it is P+M_10. Which one is it now? 2. or you have to put '... which results in ...' or '... which results are ...' but not '... which results is ...'.

OSD

3, S146–S151, 2006

T13) pg 786 line 5 '... HadCM3+M_10+P ...' For the sake of clarity and continuity, please keep the same order in your abbreviations (compare with my previous point).

T14) pg 787 line 14 Is figure 10 ice concentration weighted? If so, please mention it. If not, why are figures a and d not similar (most differences between 70E and 120E)?

T15) Figure 1: caption Please change 'Sea ice' in 'Sea ice concentration'.

T16) Figures 2, 5, 6 and 7 Please change the 'light blue' into another colour to avoid confusion.

T17) Figure 2 Is it possible to put a little key in the corner of the figure like you did in Figures 5, 6 and 7?

T18) Figure 3 The lines are not clear. Please, or delete them, or get them more clear (with clear labels).

T19) Figure 4: caption Is this a figure for the winter or for the annual mean? Please state this clearly.

T20) Figure 5: caption 'HadCM3+M-10' should be in my opinion 'HadCM3+M_10'.

T21) Figure 12 A scale for the geostrophic wind anomalies is missing.

Interactive comment on Ocean Sci. Discuss., 3, 777, 2006.