

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

W. Connolley et al.

Received and published: 17 August 2006

[Comments not explicitly replied to are accepted]

S1 - yes, HadAM3 is capable of reproducing the large-scale structure of the katabatic winds. This paper isn't about the atmospheric model, so we propose to replace "frequently is" with "frequently is, in reality and the model".

S3 - there is a mistake on our part - the values used in figures 2 onwards in the paper are **area** and not **extent**. The value of $20 \cdot 10^6 \text{ km}^2$ is correct, although possibly misleading. That value is for the extent of the ice, averaged over 1979–2004 (ie, $\text{extent}(\text{avg}(\text{ice}))$). This results in a larger value than Zwally et al, because a few high-ice years occur late in the series. This occurs because of the non-linearity of the $\text{extent}(\text{avg}(\text{ice}))$ procedure: a different result (lower) would be obtained from

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

avg(extent(ice)). Since the paper has been reviewed on the basis of the curves in fig 2 onwards, we feel it is best to keep those figures, but to accurately label them as ice area rather than extent. This also requires some changes to section 3: Delete "But where possible..." onwards and replace with "The disagreement between Bootstrap and NASA Team is considerably smaller than between the observations and the model, or different versions of the model. For example in September the SH ice area is $1.5e13$ in NASA team but $1.6e13$ in Bootstrap."

S4 - we will make a table.

S7 - The context for this sentence is the next sentence, about lack of ice hindering interpretation. Therefore we will rephrase the sentence the referee objects to, to: "the ice in the Bellingshausen Sea close to the Antarctic Peninsula is now more extensive and more concentrated."

S8 - add ref to King, 1994

S9 - The ice thickness is indeed too low. We have added a brief comment on this, and a reference to Laxon et al., 2003. However we would rather not discuss this extensively, for the reasons given in the second paragraph of section 5.1.

S10 - pstar values used vary - we have added a reference to Miller et al, J Climate 2005 and a brief comment in section 5.3. The leads in the model continue to produce sea ice unless closed by dynamics or thermodynamics. An improved thermodynamics model, or indeed further experiments with tuning the thermodynamics, could well be desirable, but we believe that is outside the scope of this paper. That we are using the zero-layer model has been noted in section 2.1.

S11 - the runs are pre-industrial (which would also argue for slightly thicker ice). As said in para 2 of section 5.3, the P^* value has been selected based upon the area rather than the thickness. There are trade-offs between area, thickness, and between the northern and southern hemispheres. Based purely on NH ice thickness, a higher

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

P* would have been chosen. But as mentioned elsewhere, the paper focusses on the Antarctic, due to problems with the Arctic model mentioned.

S12 - We will add the extra figures that the referee suggests.

T12 - The caption is correct, the text within the figure should be P+M_5.

T14 - The terms in the figure are those of the momentum equation, weighted as shown in the equation (1). We will add "weighted as in the momentum equation (1)" to the figure caption. Note that the momentum equation itself needs a slight correction: it should have 0 on the left hand side, rather than mdu/dt , since the acceleration term is dropped as being small in the (E)VP method.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper