The Cryosphere Discuss., 6, C2745–C2749, 2013
www.the-cryosphere-discuss.net/6/C2745/2013/

The Cryosphere
Discussions

Interactive comment on "Mechanisms causing reduced Arctic sea ice loss in a coupled climate model" *by* A. E. West et al.

A. E. West et al.

alex.west@metoffice.gov.uk

Received and published: 7 January 2013

Replies to comments by Referee 1

> Review Ogi and Wallace papers on effect of wind forcing sea ice export on September sea ice extent.

They are saying that the wind forcing in both summer and winter can explain a large part of the interannual variability in September sea ice extent, both via increased ice export and via advection of ice away from the Siberian coast, presumably both leading to increased thermodynamic ice loss through the albedo feedback. This makes an interesting comparison to the HadGEM1 runs in question, in which the immediate cause of ice export changes appears to be changes in the ice thickness pattern rather than

changes in the ice velocity. Nevertheless, they appear to provide good evidence for changes in advective ice loss to have a significant impact on September minima. A mention in the discussion would be appropriate.

> Has sea ice extent in the month of October declined at a fast rate?

According to the HadISST dataset (Rayner et al 2003), the month with the greatest rate of change of Arctic sea ice extent is September, with a rate of loss of 810,000 square km / decade. October is indeed a close second, with a rate of 790,000 square km / decade. Will add a reference to HadISST in the text to justify p2654, line 17, along these lines.

> Comment on forcings used in SRES-A1B, in particular the smoothness of the CO2 forcing from 1990-2010, given that the change occurs soon after the switch from historical to A1B forcing.

The CO2 forcing, and all other forcings appear fairly smooth from 1990-2010 (Figure 1); if anything there is a slight acceleration in the rise in CO2 concentrations just before 2010. There are small slowdowns in CH4 and CFC-11 rise around 2000, but these look rather too brief to have had significant effect. Will add a sentence to this effect after p2656, line 16, when the experiments themselves are introduced.

> Verb tense changes to past...

Will fix this.

> Should state that it is reasonable accuracy compared to observations?

Yes.

> The paranthetic statement implies that the surface temperature cannot change for a zero-layer model.

Will reword this sentence: "A very thin 'skin layer' at the top of the ice is given a heat capacity to better simulate the diurnal cycle."

> Residuals confusing.

After p2659, line 5, will replace the paragraph after 'atmospheric heat transport as a residual' by the following.

'Because of the method of calculation of atmospheric heat transport, the atmospheric heat budget balances exactly by design. The ice and ocean heat budgets do not so balance automatically; therefore for these two components residual error terms are calculated and assessed.'

In section 3.2, it will be stated that the ice residual error is negligible, and therefore not shown in figure 5.

Explicit equations will be given in appendix B as suggested to make it clearer, and arrows to represent the ice and ocean residual errors will be added to Figure 4.

> Botmelt is not a good variable name for the conductive heat flux through the ice.

Good point. Will rename botmelt as 'conductive heat flux through ice' in appendix B. It belongs in the atmosphere-to-ice term because: the energy balance at the bottom of the ice consists of ice freeze melt, the oceanic heat flux and the conductive heat flux through the ice. For the second quantity the ocean is the relevant heat source/sink in the model, but for the last it is the atmosphere. The melting / freezing at the bottom of the ice must therefore be partitioned into these two fluxes.

> Suggest giving sign convention of terms in caption to Figure 5.

Good idea, will do.

> Unclear how the changing area of the ice affects the breakdown.

Not directly; as you realised, the fluxes are all given in bulk (TW), not per unit area. For example, the total downwards surface flux from the atmosphere is simply (atmosphere to ice) + (atmosphere to ocean); changing ice area does not necessitate weighting these terms in any way. However, the changing ice area will indeed affect how the

terms are interpreted; clearly the same atmosphere to ice flux in TW will have a much greater effect if concentrated into a smaller area of ice.

Propose adding a clarification to the end of the first paragraph of section 3.2: 'Note that fluxes are given in bulk (TW), so the changing ice area has no direct impact on the plotted quantities.'

> Why does the computation in fig 6b not equal that of fig 6a?

The reason is very close to your guess - daily vs monthly averages. In theory, the exact ice export is given by $\int(\int(h_{ice}v_{ice}.dS).dT)$, an integral over time and the Arctic boundary. The computation in 6b, however, is a spatial integral of a product of monthly means of hice and vice over the Arctic boundary, and therefore represents only the 'mean flow' part of the ice export, not taking into account temporal correlations between hice and vice.

In comparison, the computation in 6a is an integral over the Arctic domain of the annual means of rate of change of ice volume due to advection, a useful diagnostic produced by HadGEM1. With conservation of ice this is exactly equal to the annual mean rate of ice export across the boundary.

Interactive comment on The Cryosphere Discuss., 6, 2653, 2012.

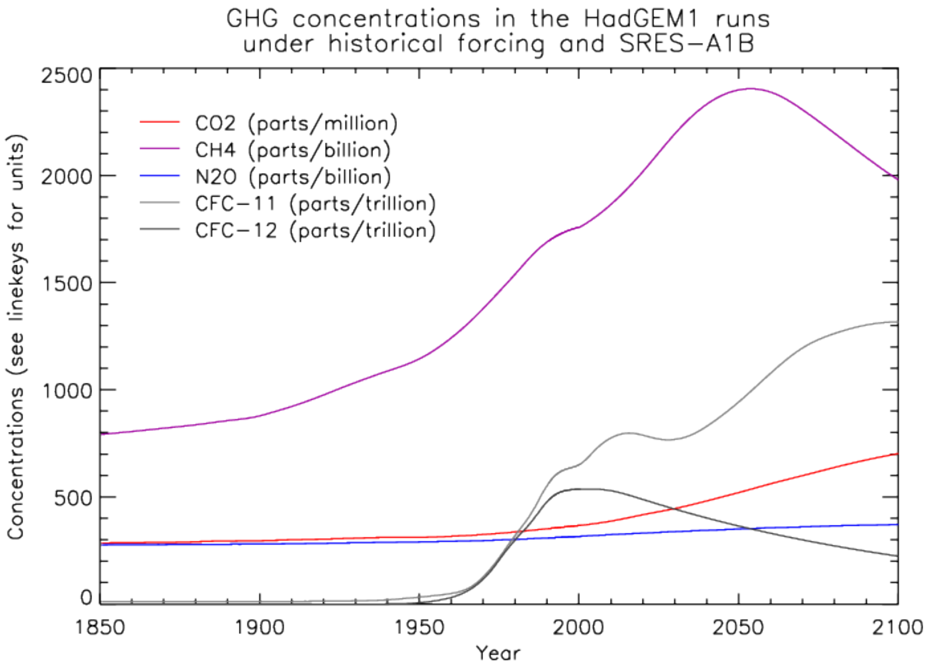
GHG concentrations in the HadGEM1 runs
under historical forcing and SRES–A1B
CO2 (parts/million)
CH4 (parts/billion)
N2O (parts/billion)
CFC–11 (parts/trillion)
CFC–12 (parts/trillion)
Concentrations (see linekeys for units)
2500
2000
1500
1000
500
0
1850
1900
1950
2000
2050
2100
Year

Fig. 1.