Clim. Past Discuss., 10, C1479–C1484, 2014
www.clim-past-discuss.net/10/C1479/2014/

Climate of the Past Discussions

Interactive comment on "Using beryllium-10 to test the validity of past accumulation rate reconstruction from water isotope records in East Antarctic ice cores" *by* A. Cauquoin et al.

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Received and published: 12 September 2014

Review of Cauquoin et al

Summary:

The manuscript presents new reconstructions of accumulation at the Antarctic Dome C site, based on beryllium-10 measurements. The impact of two alternative sets of assumptions on the beryllium-10 accumulation reconstructions are calculated. The difference between these two beryllium reconstructions, alongside three water isotope reconstructions, two of which also use ice flow models (EDC3; and AICC2012), are assessed. The differences between these reconstructions are used to help constrain

past accumulation uncertainty. Reconstruction results are then evaluated alongside results from CMIP5-PMIP3 GCM simulations, and one atmospheric-only GCM.

The authors find some intriguing results. Whilst they show that differences between the reconstructions are mostly small, which is indeed quite reassuring, they also find that accumulation rates during peak warm intervals are underestimated by around $\sim$1 cm-ie-yr-1 when based on water isotope measurements. These low warm period water isotope based accumulation rates are underpinned by the traditional water isotope - temperature conversion.

I think this result is quite noteworthy, since if read alongside the 0.23cm-ie-yr-1K-1 conversion rate, it implies that water isotope based temperature reconstructions of optimum temperature over the MIS 9.3 peak warmth may be underestimated by 3-4K. Given the paucity of other peak interglacial ice core based temperature information, this result seems worth highlighting.

The analysis of the GCM results shows that there is a rather wide spread in the accumulation versus temperature (preindustrial to LGM) relationships within the CMIP5 model dataset. The authors might conclude from this that some current CMIP5 models are not yet accurate at simulating changes in polar precipitation during large climate shifts.

In summary, I believe this is a sound study. The succinct presentation and focused goal of the manuscript are also excellent. However, a set of minor revisions should greatly assist readers in understanding the significance and interest of this work. In particular, the abstract and conclusions would benefit from revision so that they more accurately reflect the study findings. This may also help highlight the significance of this work.

Major comments (but minor revisions):

1. The abstract and conclusions should be substantially revised to bring out interesting conclusions of the work; see summary above.

2. The terminology for the model simulations needs to be corrected. AGCM is used incorrectly in some places in the text. The CMIP5-PMIP3 simulations are not atmospheric-only general circulation models (or AGCMs). There is a similar problem in the use of present day versus preindustrial (control runs).

3. Following on from the point above, it is not clear if the authors are consistently using the present day or a preindustrial average to calculate accumulation and temperature differences, both for the model and the reconstructions. The study would benefit from consistency on this point.

4. It is not clear why the interval of 269-355 kyr BP was selected for the study. It would appear that uncertainties on all aspects of the work would be reduced if the interval was longer, and particularly if it contained the more recent LIG and LGM. Please provide an explanation for the 269-355 kyr BP interval.

Minor comments (minor revisions):

P3423, l16, Explain why 'some decoupling can be expected'

P3423,l18-20 this sentence does make sense; the first part is not a stringent test. However it is true that the simulation of polar accumulation is a weakness for many models, and moreover there are indeed few good sets of accumulation data against which models can be evaluated.

P3423,l25 'present day' or preindustrial?

P3424,l11-16 The existence of magnetically induced Be-10 excursions within the study period does make the selection of this interval seem less defensible. See also point 4. above.

P3424,L20 'inferring information' -> inferences

P3424,L22-26 rewrite / clarify these lines

P3427, l20-24 I think you have to discuss here the Clausius–Clapeyron equation for wa-

ter vapor in atmospheric condition here: the water holding capacity of the atmosphere is exponentially dependent on temperature; and increases by about 7% for every 1°C rise in temperature.

P3428, Implies that the results of the AICC2012 reconstruction of accumulation should also ideally be included in the figures/study.

P3430, l4-5 'preindustrial'?

P3430, L25, Why is sublimation mentioned if it is not used?

P3431,l2? Rewrite sentence 'Then, the IPSL...'

P3431, AGCMs or AOGCMS?

P3431, l10-14 Please be consistent in this calculation. Choice of interpolation versus averaging a larger number of grid boxes may well affect the numbers. Ideally redo either ECHAM5 or the PMIP3 calculations.

P3431-, Section 3, This is particularly interesting.

P3432, l12-19 Could be rewritten to spell out the uncertainties more clearly.

P3433,l6, Please provide number in terms of * cm-ie-yr-1 for the 16% difference between EDC3 and the purely Be-10 conc->flux calculation.

P3433- Section 4, Check all model simulation descriptions. AGCM versus AOGCM (probably better just to use the terminology of 'CMIP5-PMIP3 output'); PD versus PI; and number of models. I think there are 8 CMIP5-PMIP3 models, with multiple simulation realisations for some models.

Section 4 p3433-3434l21-25, This should be rewritten or perhaps removed. Air is not always at saturation point with regards to water vapour. And a mean temperature may not be particularly relevant to the total moisture transport. Thus the comment about maximum moisture comments and mean temperature is misleading. The main

strength of GCMs is that they simulate , in an internally self-consistently manner, transport of water. On the other hand, the Clausius–Clapeyron equation [P depends on exp(T)] is an idealistic calculation, which does not fully reflect the reality of polar/global atmospheric moisture transport.

Section 4, Last two paragraphs. It would be useful if these paragraphs could be carefully reconsidered in terms of Be-10 implications for temperature reconstruction errors. A full joint assessment of accumulation and temperature reconstruction errors based on the Be-10 accumulation reconstruction may be beyond the scope of this present work. However, the authors could usefully rewrite this section, since their results do not appear to support a –10 to +30% uncertainty for water isotope East Antarctic temperature reconstructions during past warm periods.

P3435, l11-13, this is not very well described. Also should refer here to Sime et al 2009 and also Sime et al 2008. (Who also found that the modelled Antarctic dD-temperature gradient can be much lower than a local geographical gradient.) Other references which should be included:

Schmidt, G. A., A. N. LeGrande, and G. Hoffmann (2007), Water isotope expressions of intrinsic and forced variability in a coupled ocean-atmosphere model, J. Geophys. Res., 112, D10103, doi:10.1029/2006JD007781.;

Lee, J.-E., I. Fung, D. J. DePaolo, and B. Otto-Bliesner (2008), Water isotopes during the Last Glacial Maximum: New general circulation model calculations, J. Geophys. Res., 113, D19109, doi:10.1029/2008JD009859.

Section 5, Please rewrite conclusions; see main comment 1.

Table 1. ECHAM5 entry is not very informative.

Figure 3. It is difficult to make out the different colours. Please also use different symbols to help the reader. Caption: 12 different simulations rather than 12 different models.

Interactive comment on Clim. Past Discuss., 10, 3421, 2014.