**Climate
of the Past
Discussions**

Interactive comment on "Interhemispheric gradient of atmospheric radiocarbon reveals natural variability of Southern Ocean winds" *by* K. B. Rodgers et al.

Anonymous Referee #1

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This paper uses an Atmospheric Transport Model and an ocean GCM, run sequentially, to evaluate factors responsible for the interhemispheric gradient in atmospheric $\Delta 14C$, as derived from tree ring records from the two hemispheres. The study shows that changes in air-sea gas exchange in response to wind speed variations can be an important driver for the $\Delta 14C$ differences – more so than accompanying effects on Southern Ocean dynamics due to changes in wind stress (see Fig 6). However, although this is certainly true within the model formalism of the paper, that doesn't prove it is also true in the real world.

There are at least two reasons for this caveat. First, the paper uses an algorithm for air-

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sea gas exchange that probably overestimates the exchange rate by $\sim$25%. Why this was done is not clear: the Naegler (2009) paper cited in the text shows that estimates by several researchers converge on the 75% value, and as far as I know this conclusion has not been challenged. The stated reason – for consistency with previous studies – seems odd, because detailed comparisons between this study and previous work do not form a large part of the present paper. And in a study that is aimed in part at evaluating the relative strengths of the different mechanisms, why use a formulation for one of them that (apparently) is widely believed to be wrong?

A second potential worry is that the ocean GCM uses uses flux corrections to restore to observed sea surface temperatures and salinities, and (at least for water fluxes – see Gnanandesikan et al., 2004) those corrections are particularly large and are opposite in sign to applied fluxes based on atmospheric climatology, for the region south of 50°S. Additionally, some particularly heavy "nudging" (in the form of short restoring times to observed climatology ,and use of subsurface rather than surface salinities) is required at four near-coastal Antarctic grid points to ensure formation of watermasses with the correct properties during the Antarctic winter. These corrections have a major affect on the overall ventilation of the model ocean interior, as discussed in detail in Gnanandesikan et al, (2004).

The difficulty with this approach is that the flux corrections are unchanged in the various model perturbation experiments of this study (at least I can find no statements to the contrary). Given the strong sensitivity of the overall model behavior to these corrections, and since the wind forcing changes must have affected conditions at the Southern Ocean surface, it is not clear how reliably the model can portray any resulting effects on ocean dynamics.

I do not believe these deficiencies are major in the sense that the paper should be drastically modified, but for the benefit of non-modelers like myself, I would like to see some discussion of the resulting scaling issues with respect to Caveat #1 (e.g., can one simply reduce the estimated gas exchange effects by the same 25%?) and possible

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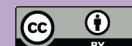
uncertainties related to #2. Apart from work to address these issues and some minor points raised below, the paper requires few changes. It is well written and concise, and highlights the utility of the interhemispheric $\Delta 14C$ gradient as a tracer for the large-scale overturning circulation of the ocean – a subject that has received relatively little attention in the past. The paper is certainly suitable for publication in Climate of the Past.

An additional point is that, based on Figure 4, it appears that the MOM3 ocean has near-surface radiocarbon values that are systematically higher by perhaps as much as 20‰ compared to the GLODAP reconstruction of Key et al. (2004). What are the likely effects on the conclusions of the paper of a more or less constant model marine $\Delta 14C$ shift compared to the real world? – there should be at least a brief discussion.

P 349 line 18: I don't believe any association between weakening of the winds over the Southern Ocean and the MCA-LIA transition ca 1375 has actually been demonstrated – change the language.

P 354 line 23: Actual parameters:for the biosphere mass and turnover time (and a reference – presumably the Siegenthaler 1986 reference from the previous sentence) would be useful for readers wishing to derive the 7% biosphere correction themselves.

P356. Units for disequilibrium fluxes. The scaling applied and the derivation of the 14C disequilibriums flux are a little confusing. In line 3 clarity would be improved if the factor r12/14 was explicitly included inside the brackets ie, (r12/14 = .85*10ˆ12). However, the major issue I have with this section is the units for the disequilibrium fluxes. These are 14C fluxes, not carbon fluxes, and should be treated as such, so the use of GtC as units is confusing. The motivation for quoting the radiocarbon fluxes in units which are equivalent to gigatons of Modern ($\Delta 14C = 0$) carbon is a good one: the fluxes can be very simply applied to atmospheric reservoirs of so many Gt of Modern carbon to directly derive $\Delta 14C$ changes. But these 14C fluxes are EQUIVALENT so many Gt of Modern C (the word equivalent is even used explicitly in line 22) so why not say so?

14C units like GtMCE (gigatons of Modern Carbon Equivalent) or molMCE per mˆ2 may be slightly clumsy but overall I think clarity would be improved if these were used throughout.

P357 lines 22-25. Unless I'm missing something, a Southern Ocean surface Δ14C of –120‰ vs N.Atl –50‰ should result in a 2.4 times air-sea 14C flux, not twice as large – change line 24 to "more than twice as large"?

p362 line 15 Provide a reference (or some other basis) for the flux variability amplitude used.

P366 line 21 "distributed" not "disributed"

Interactive comment on Clim. Past Discuss., 7, 347, 2011.