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Climate
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Discussions

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

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Response to Reviewers

Here we present our responses to the points raised by the three reviewers and the editors. We begin by offerering some general comments that touch on the points highlighted by the editor and then proceed to the more specific points raised by the individual reviewers.

The first point that is clear from the reviewer responses is that the manuscript needs a more comprehensive introductory section to better convey the scientific context within which this study is to be interpreted. Addressing this concern requires both a clearer exposition of the existing controversies involving climate variations of the last millenium and why radiocarbon may be expected to contribute to resolving some of these issues.

In order that the Introductory section provide a broader context, we will describe in more detail the current state of knowledge regarding the equatorial dominance of the interannual variability in air-sea CO2 fluxes and why this motivated the study of Turney and Palmer (2007) to focus on this region in their study on the interhemispheric gradient. In describing this, we will also discuss how the mechanisms driving variability in atmospheric CO2 differ from the mechanisms driving variability in atmospheric Δ14C, such that variability from the Equatrial Pacific tends to play only a minor role in variability in atmospheric Δ14C.

Following the description of the role proposed for an equatorial ocean control on the interhemispheric gradient in atmospheric Δ14C, we will then provide a more detailed description of some of the controversies involving climate variability over the Last Millenium (850-1850AD). In particular, we will point out that the coupled modeling study of Jungclaus et al. (2010) has demonstrated that the current generation of coupled models may not be able to account for the variability in atmospheric CO2 concentrations measured in ice cores over the same period. This is signficant, as the study of Jungclaus et al. (2010) used a state-of-the-art coupled model that shoud in principal account for both natural variability and the response of the earth system to external forcing (volcanoes and solar variations).

We will close the revised Introductory section by clearly stating our intention that radiocarbon be considered as a tracer that may help to resolve some of the outstanding uncertainties about the dynamical controls on variability over the last millenium highighted by the study of Jungclaus et al. (2010). In particular, we wish to explore the potential of atmospheric Δ14C, and in particular the interhemispheric gradient in Δ14C, as a proxy record that integrates over large scales to reveal past variability in the Southern Ocean winds. Jungclaus et al. (2010) emphasized that their earth system model is not able to reproduce the variability in ice core CO2 concentrations over the last millenium revealed by ice core records. However, they are not able to identify what the "missing processes" are that limit their simulations. Our intention in the manuscript is to introduce ∆14C as a potentially powerful new constraint that can help to identify and understand the missing processes. Important to the idea presented in the paper is that ∆14C serves as a proxy that is not local, but rather representative of large-scale changes over the Southern Ocean. The introductory section will reflect this larger context. In other words, our intention is to demonstrate that ∆14C does not suffer from the shortcomings that have been identified for a number of local proxies in the publication of Wunsch (2010).

The second point that will be presented more completely is a description of the modeling tools that are used in this study. After reiterating the point that the Jungclaus et al. (2010) study, as well as other coupled modeling studies to date that have considered the last millenium, does not reproduce the observed amplitude of variability in atmospheric CO2 over the last millenium, we will point out that a test-of-concept exercise with a simpified model configuration is appropriate to consider the question of whether the problem may be an underestimate of the natural variability of atmospheric winds over the Southern Ocean. This will be followed by a more detailed description of the modeling configuration used here. We agree with the reviewers and the editor that this more extensively developed "context" will be very beneficial to the general reader.

General Responses to Anonymous Referee #1

The first reviewer has raised the question of the "restoring" or "nudging" of temperatures and salinities for the ocean model simulations. This question is important, as this type of "restoring" could be expected to dampen the response of the model configuration to perturbations. If this were the case, then the implied amplitude of the past variations in Southern Ocean winds would be larger than what is inferred in Figure 6 of the manuscript.

Importantly, the robustness of the response shown in Figure 6 (the sensitivity of the Interhemispheric Gradient to wind speed perturbations) is supported by the amplitude of the disequilbrium fluxes in response to wind perturbations found in the coupled modeling study of Galbraith et al. (2011), accepted for publication in Journal of Climate (http://journals.ametsoc.org/doi/pdf/10.1175/2011JCLI3919.1). This is the first fully coupled ocean/atmosphere model to our knowledge that includes oceanic radiocarbon as a tracer. Although this model does not use any surface restoring for temperature and salinity, the relationship between wind perturbations and disequilbrium flux perturbations over the Southern Ocean is very nearly equal to what we find in this study.

In fact, this may be an important piece of information regarding the "missing proceses" mention above in the context of the Jungclaus et al. (2010) study. It raises the possibility that the current generation of models underestimates decadal- to centennial-scale variations in wind variations over the Southern Ocean. If the natural variability in the real-world winds were to be significantly larger than what is suggested by models, this would have a number of implications. This possibility should provide justification for including ∆14C as an earth system tracer in coupled earth system models, since it adds an additional important constraint. In the revised version of the manuscript, we will be very clear about this point in the Discussion section.

A separate comment by the reviewer raised the question of whether the high bias in modeled sea surface ∆14C (the comparison with GLODAP in Figure 4) may undermine the argument presented for the relationship between wind speed and the Interhemispheric Gradient in ∆14C. This is an important question and will be addressed in the revised manuscript by explicitly making two points. We will explain how the GLODAP estimate of pre-bomb ∆14C builds on the potential alkalinity method of separation of bomb- and pre-bomb ∆14C using the method of Rubin and Key (2002) and that the inherent uncertainty in this method for the surface ocean should be expected to be as large as the difference between the modeled ∆14C and the GLODAP reconstruction agree to within the uncertainty associated with the GLODAP pre-bomb reconstruction.

General Responses to Anonymous Referee #2

We agree with the reviewer that it is important to understand the dependence of our model results on the way in which the gas exchange parameterization is treated, given that gas exchange over the Southern Ocean is emphasized in the Conclusion Section as being key to interpreting the Interhemispheric Gradient in Δ14C. The coefficient we use is 0.339, which is 14% smaller than the Wanninkhof (1992) value of 0.39. So in fact we are more than halfway to the full reduction of 25% that has been suggested by the studies referenced in our manuscript. It will certainly be part of our future sensitivity work with a higher-resolution ocean model to examine this sensitivity to gas exchange exhaustively. In addition to testing the sensitivity to the amplitude of the coefficient used here for the quadratic gas exchange relationship, we will also test the sensitivity to the application of linear and quadratic gas exchange formulations.

However, there is an additional point that we wish to emphasize in the text which is equally important. The implied natural variability in Southern Ocean winds inferred from Figure 6 is approximately three times as large as the background natural variability in Southern Ocean winds found in the coupled model of Galbraith et al. (2011), with the same being true for the higher resolution version of the same GFDL model (CM2.1). This factor of 300% is much larger than the 11% (25%-14%=11%) overestimate that we have used for the gas exchange coefficient. For this reason, our interpretation that the current generation of coupled models may be underestimating the natural variability of the winds over the Southern Ocean is not impacted by the amplitude of the gas exchange coefficient that we have used. We fully intend to make this point clear in the text, as it will strengthen the manuscript.

The reviewer also raises a question of whether we believe that the mechanism we have emphasized (disequilbrium fluxes and gas exchange) can account for longer-timescale changes in atmospheric Δ14C. In fact we simply have not tested this, as the duration of our wind perturbation experiments was multi-decadal. So we do not have any means of addressing glacial timescales. For glacial timescales, one must

assme that the dynamical effect of changing winds should have an impact on the ocean interior distribution of density, and thereby the perturbations of the type presented in our manuscript are not appropriate. We will be clear about this point in our revised manuscript.

General Responses to Anonymous Referee #3

We appreciate the reviewer's comment that the paper needs to be reorganized to improve clarity. As is noted above under the heading "Response to Reviewers", we will introduce significant changes to the Introductory section, providing a broader view of the outstanding controversies and questions inolving current research on climate variations over the Last Millenium (850AD – 1850AD).

The reviewer emphasizes an important question about variablity in the Equatorial Pacific. Given that there is quite convincing evidence from models and observations that for the modern climate the Equatorial Pacific is the regions of largest amplitude interannual variability in air-sea CO2 fluxes, why do we in our manuscript instead emphasize the Southern Ocean as being the region of control of the interhemispheric gradient in atmospheric Δ14C? The importance of the equatorial Pacific for air-sea CO2 fluxes was certainly the main motivation for the equatorial focus in the study of Turney and Palmer (2007) on the interhemispheric gradient in Δ14C.

In order to achieve maximum clarity in the text, we will present our reasoning in more detail in the revised manuscript. In addition to raising this issue explicitly in the interoduction, including references to the papers of Le Quéré et al. (2000) and Obata and Kitamura (2003), we will return as well to this point in the interpretation presented in the Discussion section. There we will reiterate the importance of the disequilibrium flux distribution map shown in Figure 2c for the cyclo-stationary unperturbed state of the ocean model, and emphasize the contrast in that figure between the Equatorial Pacific and the Southern Ocean. Figure 4 reveals that it is the region where old and decay-depleted waters are brought to the surface and the sea surface Δ14C is most negative.

This explains why the disequilbrium flux variability is dominated by the Southern Ocean even though the Equatorial Pacific plays a first order rolle in the variability of CO2. Although water that upwells in the Equatorial Pacific does have a signature of moderate depletion of sea surface ∆14C, that signal is simply much more pronounced over the Southern Ocean.

General Responses to Editor

Our response to the comments of the Editor is provided above under the heading "Response to Reviewers".

In summary, we will rewrite the Introduction to better represent the "state-of-the-art" in the current understanding and controversies surrounding both the use of ∆14C as a proxy of past change, as well as the more general topic of large-scale variability over the Last Millenium. Additionally, we will provide a more detailed account of the model configuration chosen here, and a clear explanation of why that model configuration is appropriate to the science question of the controls on the Interhemispheric Gradient.

Detailed Responses to Anonymous Referee #1

P. 349 Line 18

The reviewer has said that they don't see any association between weakening of the winds over the Southern Ocean and the MCA-LIA transition ca 1375, and that we should change our language. In order to address this point, we will modify the text to make it clear that we aren't claiming that the Southern Ocean winds have been definitely identified as the mechanism explaining the robust change point in 1375 in the interhemispheric gradient of ∆14C. Rather, we are arguing from our sensitivity experiment that this mechanism is likely to be a major player in accounting for this shift. We are currently conducting a series of independent process-focused sensitivity tests to evaluate the amplitude of a potential contribution from the ITCZ, which may be expected make a minor contribution. The text in the resubmitted manuscript will be

absolutely clear about this point.

P. 354, line 23

The reviewer has asked how the value of 7% was calculated for the relationship between the terrestrial and oceanic disequilbrium fluxes for the pre-anthropogenic climate state. According to IPCC 2007, the terrestrial biomass was 2300 PgC and the carbon mass in the ocean 38000 PgC. If one assumes that the average ∆14C of the terrestrial biosphere is close to 0 per mil (Normalized 14C activity = 1) (at an average age of 30 years), and if one takes an average oceanic value in the ocean of -150 per mil (Normalized 14C activity = 1)), then given that the fluxes from the atmosphere to these reservoirs must supply the radiocarbon decay, the ratio of natural radiocarbon fluxes into the ocean relative to the terrestrial biosphere is (38000*0.85)/2300 = 14. By inverting this ratio, we get 1/14=0.07, or 7%. The uncertainty here should be relatively small, since the value will change very little if the age or turnover time for terrestrial carbon and radiocarbon is 60 rather than 30 years.

P. 356

The reviewer has asked us to clarify the units in which disequilbrium fluxes are expressed. We agree with the reviewer that the GtC units presented in the text are problematic, so we will follow the sugestion of the reviewer and express these as GtMCE (gigatons of Modern Carbon Equivalent), and we will state the reasoning clearly in the Appendix.

P. 357 lines 22-25

The text here will be changed to "more than twice as large", as suggested by the reviewer.

P. 362 line 15

The scaling behind this calculatiion is as follows: First we assume a simple two-box model for the troposphere, with the lateral boundary being at the equator, the upper boundary being the tropopause, and the lower boundary being the ocean surface. The ocean in the mean absorbs approximately 2.7 Gt equivalent for the Southern box and 0.9 Gt equivalent in the north. If the flux across the top of the boxes is 1.8 Gt equivalent in either hemisphere (following the calculations of Levin (2009)) then this implies an interhemispheric (southward exchange) of 0.9 Gt equivalent. If we assume that the amplitude of the flux from above into the northern hemisphere tropospheric box (1.8 Gt equivalent in the mean) has a seasonal amplitude of 0.9 Gt/year equivalents, then the inventory variability is 0.9 Gt/(2*pi) = 0.14 Gt C in scaled units, or 0.58 per mil.

P. 366 line 21

The spelling will be corrected so as to say “distributed”

Detailed Responses to Anonymous Referee #2

Sec 2 Model configuration

The author has pointed out some issues that were unclear about the model configuration that need to be clarified in the revision process. The reviewer is particularly concerned about the looping procedure used to drive the atmospheric transport model. Although it is common practice to follow the method we used for looping through a 10 year stretch of reanalysis atmospheric fields, which was done for example in the intermodel comparison study of Naegler et al. (2007), we did in fact test the consequences on the interhemispheric gradient of Δ14C of looping over 10 years versus cycling through individual years, and then constructing a climatology from the output of those different runs. By using winds in the transport model that include interannual variability, we are folding in variability in the Intertropical Convergence Zone (ITCZ), and thereby accounting for variations of the type emphasized by Turney and Palmer (2007) in their tropical mechanism. It is not our interpretation that our analysis rules out a role of the ITCZ, but rather that it demonstrates that the Southern Ocean winds can offer a first-order contribution without necessarily excluding other mechanisms. Exploring a potential contribution from the ITCZ is left as a subject for future investigation,

and in fact we have already begun working on this complementary science problem. In the main text, we will expand the text to include a more detailed description of the forcing fields used for both the ocean model and the Atmospheric Transport Model. Additionally, we will explain that the model was driven with 6-hour reanalysis fields from the atmosphere, and linear interpolation between these fields was used to get to the model timestep.

P. 354 line 18

The reviewer has asked about the residence time of carbon in the terrestrial biosphere that is used to calculate a relative disequilbrium flux for the terrestrial biosphere. As has been stated above in the detailed responses to Reviewer #1, the value of 7% was taken from a scaling that were obtained from the IPCC AR4 report. Also as stated above, the CASA model was only used to represent the spatial pattern of these fluxes, with the total value of 7% taken from the scaling argument. This will be made absolutely clear in the revised version of the manuscript.

P. 361 line 25

The reviewer has asked about CO2 fluxes and their impact on atmospheric Δ14C. It’s important to emphasize here that with our modeling work that we specifically chose a somewhat simplified modeling configuration that allowed us to (a) run the model to steady state for the initial condition, and (b) to perform a number of perturbation sensitivity studies. This “test of concept” would not have been possible with the higher resolution model components that are for example used for IPCC-class ocean model configuations with significantly higher resolution. Additionally, we used the abiotic OCMIP-2 formulation of radiocarbon, which is at least an order of magnitude cheaper computationally than the IPCC-class biogeochemistry/food-web models. For that reason, the Δ14C fields in our modeling configuration are deemed to be appropriate for research purposes, but the CO2 fluxes or 14CO2 fluxes themselves should be interpreted with a great degree of caution.

Detailed Responses to Anonymous Referee #3

Abstract

The Abstract will be modified to reflect that the analysis is based on modeling results

Abstract Line 7

The reference fo Stuiver and Quay (1980) will be fixed.

Introduction

The Introduction is being changed in order to better represent the “state-of-the-art”, following the deas described above under the heading “Response to Reviewers”.

P. 350 line 9

The Interhemispheric Gradient in Δ14C is calculated by taking the difference between the successive 5-year means of atmospheric Δ14C for each hemisphere (the curves shown in Figure 1). The difference that the reviewer suggests showing (the difference between the two hemispheres) is already shown in Figure 1b. The references that are relevant here are INTCAL04 (Reimer et al., 2004) and SHCAL04 (McCormac et al., 2004). We agree with the reviewer that the legend of Figure 1 should be made more precise, in order to convey the information in the Figure. We will make it clear that the uncertaintly bars are from the INTCAL04 and SHCAL04 datasets themselves. For the uncertainty bars in the difference plot, the uncertainties shown are calculated as the square root of the sum of the squares of the components.

P. 350 line 16

We will clarify more precisely what we mean by “small” with reference to the study of Levin et al. (2009). The GRACE model used by Levin et al. (2009) suggests that the difference between the hemispheres is less than 10%.

P. 361 line 4

The reviewer has asked us to be more precise than simply saying, “Our goal in this study is to use models to test…” We agree that this is a good idea, and as was stated above in the section “Response to Reviewers”, we will provide a more detailed account of the models use here, and the specific reasons for why we think that the modeling configuration is appropriate to the science questions outined in the Introduction.

P. 351 line 7

We agree with the reviewer that the reference to Skinner et al. at the end of the Introduction does not really fit. Skinner et al. considered a very different timescale that we did not address in our study. For that reason, we will close the Introduction section with more detailed informatin about the tools that we will apply to the science questions described earlier in the Introduction.

Model configuration

We will be clearer in the details of the models, as described above.

P. 352 lin 18

“Watermasses” now reads “water masses”

P. 352 line 25

In order to clarify this point, we will take a sentence that was in Appendix A for the submitted manuscript (“as long as one assumes that delta-c13 is not affected by wind-driven changes in ocean dynamics and gas exchange, the variabiltiy of atmospheric Δ14C can be identified approximately with changes in the 14C- to 12C ratio in the model atmosphere”) and include this in the main text.

P. 353 line 7

“etc” will be deleted

P. 354 line 22

As stated above in our detailed response to Referee #1, we have assumed that “short” is of order 30 years. The scaling analysis we used here will be presented in the text, and we will emphasize that the result for the disequilbrium flux over land will only have very small differences if the residence time of carbon in the terrestrial biosphere is 100 years, or 10 years, rather than 30 years. The uncertainty introduced with this assumption is very small, and has little impact on the arguments presented in the manuscript.

P. 355 line 2

In the text we have stated that the decay of 14CO2 in the atmosphere is relatively negligible, so that we have ignored this, and the reviewer has asked us to clarify how small this is. For the duration of the experiments considered here, decay is small relative to the signal of interest. In fact, the decay would not only be small, but even the small amount of decay should be almost exactly equal in the two hemispheres. In our future work with Δ14C in an Earth System Model (the extended version of the model presented by Galbraith et al, 2011) we will certainly account for decay of 14CO2 in the atmosphere.

P. 356 We repeat here the scaling arguments given above for the seasonal variability question, in order to illustrate the scaling for the passage in question. First we assume a simple two-box model for the troposphere, with the lateral boundary being at the equator, the upper boundary being the tropopause, and the lower boundary being the ocean surface. The ocean in the mean absorbs approximately 2.7 Gt equivalent for the Southern box and 0.9 Gt equivalent in the north. If the flux across the top of the boxes is 1.8 Gt equivalent in either hemisphere (following the calculations of Levin (2009)) then this implies an interhemispheric (southward exchange) of 0.9 Gt equivalent. The total value of 3.6 Gt equivalent into the troposphere from the stratosphere is simply taken as a balancing of the decay rate in the ocean interior. When we say “within 20%”, we mean that the value of 4.9 per mil is within 20% of the mean background gradient in evidence from the tree ring proxy record in Figure 1.

P. 357 line 14

The unit for the number “3” is permil – we will clarify this in the text.

P. 357 line 15

We will add in the text the mean value for the gradient (in permil units) from the data proxy record.

Figure 3

The reviewer points out that the legend for Figure 3 is incomplete, and that we need to specify what precisely is shown in the light blue curve. In order to address this concern, we will include the following text in the revised version of the manuscript: “The Δ14C for each of these components is calculated as the 14CO2 from the specific process being considered and the total CO2 from all processes. The background component reflects a globally uniform initial value of 14C plus the seasonal rectification in 14C from the terrestrial biosphere described in Section 2.2, divided by the spatially varying CO2 from all processes. This background value is necessary in order to ensure that the curves sum to the black curve”

P. 358 line 2

The text will be changed so as to read “perturbation”

P. 358 line 10

Following the suggestion of the reviewer, we will omit the description of the third case.

P. 358 line 13

The mean value from the data proxy record will be included in the text.

P. 358 line 18

The reviewer has asked us to quantify the expected degree of uncertainty in the model in reproducing the observed interhemispheric gradient in atmospheric Δ14C. In recognition of the importance of this question, we will specifically investigate this question in the next stage of our work, where we will use a higher-resolution ocean model that includes a biotic representation of the oceanic carbon cycle (the model configuration used in the study of Galbraith et al. (2011)). There we will test the systematic uncertainty related to the choice of air-sea CO2 flux parameterization (quadratic versus cubic) as well as ocean mixing parameter settings and choice of surface forcing fields.

Discussion

Following on the comments presented above for the changes we will make to the Introducory section to better contextualize the main science results, we will make corresponding changes to the Discussion section to return to these points.

P. 360 line 15

The reviewer has asked us to clarify which tracers are impacted in their interhemispheric mixing timescale by El Nino and La Nina. The argument presented in Lintner et al. (2004) applies generally to atmospheric gases.

P. 360 line 20

We will add the mean exchange time of Keller, as suggested by the reviewer. The range 6-12% was calculated by taking the difference stated for El Niño and La Niña years (0.05-0.10 years) and dividing this by the 0.86 year value.

P. 361 line 22

This spelling change will be made in the revised text.

P. 363 line 22

This spelling change will also be made in the revised text.

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