

Interactive comment on “Sixty years of radiocarbon dioxide measurements at Wellington, New Zealand 1954 – 2014” by Jocelyn C. Turnbull et al.

Anonymous Referee #2

Received and published: 13 February 2017

The atmospheric radiocarbon measurements conducted at Wellington are a very important record and the authors' efforts to maintain and evaluate the observations are valuable to the community.

However, there are some major revisions needed before publication of this manuscript. Much of the paper is used on re-reporting trends and gradients that have already been shown in other work. The authors also make unsupported claims about the mechanisms driving the interhemispheric gradient and seasonal cycles of D14C.

The paper postulates a sensitivity to Southern Ocean air-sea exchanges that is misleading and unsupported. It gives the impression that the Southern Ocean only began influencing the interhemispheric D14C gradient in 2002, whereas the Southern Ocean

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has always been a primary influence on the interhemispheric D14C gradient, via gross, not net, carbon exchange. Levin et al. 2010 and Randerson 2002 clearly show that the observed trend in the interhemispheric D14C gradient is consistent with a long-term change in the oceanic influence, dominated by the long-term decrease in atmospheric D14C and the change in D14C disequilibrium over the Southern Ocean, which is further supported by the Graven 2012 papers. A change in upwelling is interesting to consider as a secondary effect, but the authors do not include quantitative models or estimates of how large the effect could be, nor any specifics on how it influences D14C. Furthermore, the Wellington data from 1995-2005 are shown to have serious issues, which would complicate identification of a signal originating in the early 2000s. And there is no discussion about the period in the 1990s when upwelling was increasing.

The authors similarly make statements about the influences on the seasonal cycle of D14C at Wellington that aren't well-supported.

The paper should be shortened to minimize the re-reporting of previous observations, reduce repetition, clarify the long-term trend in the Southern Ocean influence on the interhemispheric D14C gradient, and remove unsupported statements. As the main contribution is to revise the Wellington data, i.e. no new modeling or other evidence is given to help interpret the data, the paper might be better suited to a journal like Radiocarbon or Atmospheric Measurement Techniques.

Specific Comments.

Section 3.5.3 appears to show major problems in the measurements for the 1995-2005 period, with large scatter and a high bias. I don't agree that the questionable data should be retained, as the authors have done - "in the absence of better data, we retain both the original and remeasured NaOH sample results in the full record." This conflicts with the aim of the paper to evaluate and refine the previously reported measurements and, presumably, to prevent the interpretation of measurement problems as real atmospheric variability.

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The code WLG is already used by NOAA for Mt Waliguan, China – perhaps another code would be better.

L15 Earliest direct atmospheric

L98 Revisiting key findings can be placed in the introduction for brevity.

L104-108 Unsupported. See above comment.

L234 Please quote a value for precision

L306 Why would this result in higher D14CO₂?

L378 More detail needed. Where is this used?

L384 How do 4-day back trajectories address the seasonal cycle? The panels in the figure all look the same. This is not very useful. A panel should be shown with the differences if there is a difference to highlight.

L413 Since 2005 or earlier?

Section 4.1 seems out of place and repetitive. Should move to introduction and focus on new results here.

L435 Turnbull 2009 only includes simulations from the 2000s, so they do not show the Sues Effect became the dominant driver in the 1990s.

L454 Do you mean when mixing with lower-D14C air from the stratosphere was the strongest? Are there Southern Hemisphere stratospheric observations from the bomb period supporting the idea that tropospheric D14C was higher than stratospheric D14C? Are you saying that tropospheric D14C was higher than stratospheric D14C in the Southern Hemisphere until the late 1970s? Bomb 14C would have also entered the SH stratosphere through the tropical tropopause, while at the same time tropospheric D14C was declining, so this seems unlikely. Note Northern Hemisphere sites also showed minima in spring in the early bomb period. Levin 2010 simulate recent

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seasonal influences on D14C and should be cited here. Oceanic influences on the seasonal cycle should also be mentioned.

L468 See Brenninkmeijer, C. A. M., Lowe, D. C., Manning, M. R., Sparks, R. J., & van Velthoven, P. F. J. (1995). The 13C, 14C, and 18O isotopic composition of CO, CH₄, and CO₂ in the higher southern latitudes lower stratosphere. *Journal of Geophysical Research: Atmospheres*, 100(D12), 26163-26172. doi:10.1029/95JD02528

L494 This is the time of maximum in the NH so this phasing is unexpected. Is there an explanation for the double-peaked shape of the cycle? This section relies on dismissing the Cape Grim data, which is not entirely convincing. Are other Southern Hemisphere observations relevant here?

L517 It would be useful to include a plot of the difference between the Wellington and Cape Grim data.

L521 Delete the word signal. Is it possible to say something more quantitative than "homogeneous"?

L527 What is the basis for the new estimate of the interhemispheric exchange time? How was this calculated? Without any supporting information this paragraph should be deleted.

L544 Need to cite Levin 2010, and Graven 2012

L561 Also shown in Randerson 2002 and Levin 2010

L565 This paragraph is misleading. See main comment above.

L575 This is the gross carbon flux not the net carbon flux. Atmospheric D14C has been highly sensitive to Southern Ocean upwelling not only since the 1980s but since the preindustrial period and throughout the bomb peak period – see Randerson 2002 and Levin 2010

L593 “Although the changing Southern Ocean carbon sink is the most likely explana-

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tion," Atmospheric D14C is not directly affected by the Southern Ocean carbon sink. What is the justification for this statement? See main comment above.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1110, 2016.