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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial distribution of $\triangle^{14}CO_2$ across Eurasia: measurements from the TROICA-8 expedition" *by* J. C. Turnbull et al.

J. C. Turnbull et al.

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Reply to reviewer comments on manuscript acpd-2008-0240

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Thanks to Nir Krakauer and an anonymous reviewer for the helpful comments and suggestions on our paper. We address specific comments below.

1. 15209:14-26 The initially very large disequilibrium between the oceans (and to a lesser extent, the terrestrial biosphere) dominated the secular decrease (and seasonal change) in D14CO2 until the mid-1980s, causing annual decreases in D14CO2 of up to 100 permil - There is probably a better way to word this. The hemispheric-scale decline rate in atmospheric D14CO2 actually reached a maximum of 80 permil per year

in 1964/5 and was less than 10 permil per year by the mid-1980s. Further, Randerson et al. (GBC, 2002, cf. Figures 5 and 6 there) suggest that exchange with the stratosphere was initially the main driver of the seasonal cycle in tropospheric D14CO2, far outweighing air-sea exchange or land respiration.

This is correct, and has been revised.

2. 15214:12 net oceanic fluxes derived from delta pCO2 (Takahashi et al., 2002) - With which assumptions about the gas exchange rate?

We use the formulation of Wanninkhof (1992), with 10m winds. We also note that for the analysis presented in this paper, where we only examine the D14CO2 over the Northern Hemisphere land, the choice of ocean CO2 and 14CO2 fluxes does not have a large effect. This has been clarified in the paper.

3. 15214:18-19 extrapolating the emission pattern up to 2004 - Is this a linear extrapolation of the 1995-2000 trend, or holding constant the 2000 pattern?

The 2004 pattern is obtained by extrapolating the trend in the 1995-2000 spatial pattern, with the global annual total determined separately by extrapolation of the Marland et al annual global totals. The text of the paper has been revised to clarify this.

4. 15214:22 Where did the value of 14CO2_bg in Equation 1 come from?

We set the model initial 14CO2_bg value at the start of our model run in 2000 to match the Northern Hemisphere D14CO2 measurements from Levin and Kromer (2004). This has been clarified in the paper.

5. 15214:24 What is a <14CO2 value>? Did you transport 14CO2 mixing ratio, D14CO2 anomalies, or something else?

14CO2 mixing ratios were transported separately, and D14CO2 values were calculated by convolving with the transported CO2 mixing ratios, and clarified in the paper.

6. 15216:25-27 Net 14CO2 fluxes into and out of the biosphere and ocean, and au-

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totrophic respiration are not included because D14CO2 accounts for natural fractionation (Stuiver and Polach, 1977) and these fluxes are therefore all necessarily zero. - This is not quite the right reason. For example, the main reason for neglecting the influence of autotrophic respiration flux on atmospheric D14CO2 is the assumption that almost all the respired carbon was fixed in the past few months, and therefore has essentially the same D14C level as the current atmosphere.

In fact both reasons are correct: since D14CO2 is specifically formulated to account for natural fractionation (unlike delta_13CO2, where the natural fractionation dominates and is of most interest), we only need to account for disequilibrium terms. Uptake into oceans and biosphere necessarily must have no disequilibrium, and the disequilibrium from autotrophic respiration is (as noted) small.

7. Please include a data table similar to that in Turnbull et al., JGR, 2007 with the sampling time and location, measured D14CO2, flags, and any other relevant information.

This data is permanently archived at NOAA/ESRL and is available for download at: ftp://ftp.cmdl.noaa.gov/ccg/co2c14.

8. 15218:5 predominantly producing 14C as 14CH4 - Add something like: <which becomes well-mixed in the atmosphere by the time it oxidizes to 14CO2>

Done.

9. 15218:16 These flagged samples are excluded from further analysis. - Can you include the exact criteria (e.g. proximity of back-trajectory to a nuclear power plant by a certain time) for excluding samples as unrepresentative on this basis?

The criterion for excluding these samples was that the HYSplit 2-day back trajectory passed within 200 km of the sampling location. We selected this wide band around the HYSplit mean back trajectory, to approximate a more realistic footprint or region of influence.

10. 15221:11ff The observations suggest... - How is this inferred? Be more specific.

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The CO values vary most in the west, close to the source.

11. 15222:11 but is heavily influenced by a single outlier - What is the influence? Is the correlation significant without the outlier?

We apologize, this was a typo from an earlier version of the document. From figure 6, it can be seen that in fact the weak correlation is due to the scatter in the western part of the transect, with PCE values varying dramatically, likely due to the point source nature of the PCE emissions.

12. 15223:8 The westernmost sample... - How far from Moscow is this sample actually?

The sample was taken 40km from central Moscow.

13. 15223:26-27 Increasing the global fossil fuel CO2 emissions by 10% increased the modeled west-east gradient by only 0.2 permil in each model scenario. - I don’t see how this can be right, given that the modeled east-west gradient attributable to fossil-fuel burning is 8 permil (Figure 7, or 5 permil just across Siberia) and that the gradient should scale linearly with emissions (assuming that the distribution is held constant)

Thank you for pointing this out. Closer investigation showed that the reason for the weak dependence was actually a transcription error in the value for a single modeled datapoint (at 120.6°E). After correcting this error, the gradient scales approximately linearly with the increase in fossil fuel emissions. Although it does change the modeled gradient very slightly, it does not change any of our interpretations.

Reply to anonymous reviewer #1

1. Although it is demonstrated in the paper that the gradient of fossil fuel CO2 across Eurasia can be inferred from the observations of 14CO2, it still remains open whether reductions (or increases) in fossil fuel emissions could be identified from this type of measurements (if they would be repeated regularly), given the small size of the signal.

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It would be helpful if the authors could comment on this in their conclusions.

We have added a comment to this effect.

2. Page 15208, line 4: ...can be used to constrain fossil fuel emissions... might be more logical: to constrain fossil fuel emission estimates

Revised.

3. Page 15209, line 24: <disequilibrium between the oceans (and to a lesser...) dominated> please insert <and the atmosphere> before <dominate> to make this sentence clearer.

Revised.

4. Page 15214, line 12: Please specify whether you use the CO2 fluxes provided by Takahashi et al. or the pCO2 values. In the latter case please specify which gas transfer formulation was used.

See revised paper.

5. Page 15214, line23: How is the 14CO2 background in Eq. 1 determined? Is it an arbitrary offset value or is the model integrated over a very long time or...? Please specify this.

See revised paper.

6. Page 15215, line 20: Do you use the same gas transfer formulation for 14CO2 as for CO2?

Yes.

7. Page 15216, line 25: This is difficult to understand, please clarify why net 14CO2 fluxes are necessarily zero.

See revised paper.

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8. Page 15223, line 27: Actually one would expect that the gradient should scale linearly with the emissions if there is a linear relation between fossil fuel CO2 and delta14CO2. Please try to explain why this is not the case.

Please see response and discussion to the first review above.

9. Technical corrections:

Have been revised in the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 15207, 2008.