Atmos. Chem. Phys. Discuss., 15, C9778–C9782, 2015
www.atmos-chem-phys-discuss.net/15/C9778/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Toward consistency between bottom-up CO_2 emissions trends and top-down atmospheric measurements in the Los Angeles megacity" *by* S. Newman et al.

J. Turnbull (Referee)

j.turnbull@gns.cri.nz

Received and published: 26 November 2015

This paper describes a study of CO2 emissions from the Los Angeles megacity, focussing on measurements of CO2 and it's isotopes to partition the contributions of different sources of CO2 (biospheric and fossil, with fossil further separated into petroleum and natural gas sources). They compare these top-down observations with bottom-up emission data products and meteorological data to understand the drivers of the changing mole fractions over the long-term and through seasonal cycles. The top-down observations give some support to bottom-up data products which show a decline in fossil CO2 emissions associated with the economic recession from 2008-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

2010.

It is refreshing to read such a clear, well-written paper with such interesting results. This is an excellent study and exactly appropriate for publication in ACP. I have a few minor comments which could improve the paper, particularly around clarification of the time series analysis methodology.

Specific comments: Pg 29594 lines 19-20. Indeed bottom-up reporting may not always be reliable, but this comment should be backed up with references, and perhaps more careful phrasing to avoid the implication of finger pointing at “other” countries.

Pg 29595 lines 1-3. Are there examples outside of the US? Airparif?

Pg 29597 lines 18-22. This is an interesting point – what is the optimal number/time length of samples to combine for measurement to give sensible, useful averages? This could be expanded on here or in the results section.

Pg 29597 line 18. Typo – CCAMS.

Pg 29597 lines 26-27. The CO2 mole fraction error is quite large – I would guess that it is sufficient for this study, but this should be justified.

Pg 29598 line 1. How were the 14C errors determined? Is this described in the Xu 2007 paper? Please reference or describe this.

Pg 29598 lines 23-24. It is a pity there is no more recent La Jolla 14C data.

Pg 29600, lines 9-10. I take it that the nuclear contribution is therefore ignored?

Pg 29602, line 11. How was the biosphere discrimination determined? The value appears to assume C3 plants, but are C4 plants important in Southern California? Is lawn grass in this area typically C3 or C4? And whether C3 or C4, how certain is this value, and how much seasonal variability might there be? A bias (seasonal or general) in delta-bio would dramatically change the proportions of petroleum and gas determined by this method.

ACPD
15, C9778–C9782, 2015

Pg 29603 lines 1-3. This is a reasonably large correction – how large is it relative to the Cbio values themselves? Ie what % of Cbio does it represent?

Section 2.3.4 Time series analysis. The description given here is quite brief, and it is hard to follow the results later. This section could be expanded to clarify what the different IMF categories represent, and how they are determined. See also my comment on the IMF results section.

Pg 29603 line 23. Bottom-up data products, not inventories – they are based on inventories but are much more complex than that.

Pg 29604 lines 23-24. How does the fraction of Cbio change through the seasons? This is discussed in a later section, but you could refer to that section here, since it is an obvious question when reading this section.

Pg 29606. See my previous comment about the delta-bio for C3 vs C4 plants. How would the interpretation here change if delta-bio was strongly influenced by C4 plants?

Pg 29607. Thanks for the nice discussion of the percentages from the biosphere. Does the larger fraction and larger overall magnitude of bio emissions during the cooler months imply a larger biosphere flux during the cooler months? This would be worth a few sentences of discussion.

Pg 29608 line 2. r2 should be lower case.

Pg 29608 lines 9-14. This is hard to follow without thoroughly reading the Jiang paper. Please clarify why the semi-annual oscillation might be driven by NPP and respiration.

Pg 29609 lines 7-8. Why would artificial irrigation reduce the biosphere signal? Intuition would suggest an opposite effect. Please expand and reference to clarify.

Section 3.4.1. The methodological basis for this section is not very clearly explained either in the text or the figure caption. The IMFs are sometimes referred to as “IMF 1”, “IMF 2”, etc., and sometimes by names that reflect what the IMF might represent,

ACPD

15, C9778–C9782, 2015

e.g. “annual cycle”, etc. It is not always obvious which IMF number relates to which cycle. A more detailed introduction to the method would be very helpful, perhaps in the methods section of the paper. How many IMF modes were identified? Why is the trend+IMF6 such an important curve – what is significant about IMF6 versus the other IMF modes?

Pg 29610 lines 7-10. Figure 10f is the detrended signal, and is just showing the deviations from the mean, correct? It is a bit hard to follow where the 7.3 ppm standard deviation comes from when referring to this figure.

Lines 8-13. Again, it is hard to follow how the 9.5% change is determined. Perhaps a version of figure 10f that is adjusted with a mean value matching that of the actual data and the deviations around that mean, rather than just showing the deviations from the mean would help.

Lines 25-30. This is an interesting discussion about how Cff decreases might not follow economic changes perfectly, but I am not convinced that such a detailed comparison is justified by the data presented here. First, there are fairly large error bars on the Cff changes shown in figure 10f, so a decrease of 13% might be consistent with the data. Second, the analysis makes no attempt to account for interannual variability in meteorology, which could potentially drive the observed changes.

Pg 29611 lines 11-15. The shape of the Cff decrease appears to be different between the observations and the CARB inventory. CARB shows a minimum in 2011/2012, whereas the observations as shown in figure 10f appear to show a minimum in 2010. How can these be reconciled?

Pg 29611, lines 23- 30 and onto the following page. Again, how would uncertainty in delta-bio influence these conclusions? A short lag between gasoline purchase and combustion makes sense, but it is hard to believe there is a 3 month lag, given that most people fill up their vehicles every week or two. What other possible explanations are there for this lag?

ACPD
15, C9778–C9782, 2015

Figure 11. The presentation of this figure could be improved. The thick lines (representing the CARB inventory data) draw the eye, and give the impression that they represent some sort of smoothed average of the observational data. Yet no smoothed average of the observational data is actually given on this figure. Perhaps fits to the observational data could be added so that a more direct comparison could be made.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 29591, 2015.