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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.

S. Newman et al.

sally@gps.caltech.edu

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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. We have changed the wording and added Andres et al. (2012) as a reference.

Pg 29595 lines 1-3. Are there examples outside of the US? Airparif? There are no examples of such detailed emissions products outside the US of which we are aware.

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. Since all of our samples were aggregated, we have no way to know the optimal number. We see the difference compared with sparser, individual sampling reported by Affek et al. (2006). This is a good study to do in the future, pending funding.

Pg 29597 line 18. Typo – CCAMS. Good catch, thank you! This has been changed.

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. A comment has been added, explaining that the dominant source of error is from the Δ14C analyses.

Pg 29598 line 1. How were the 14C errors determined? Is this described in the Xu 2007 paper? Please reference or describe this. Overall precision is 2‰ which is based on long-term reproducibility of secondary standards. It is described in both Xu et al. 2007 and Xu et al. 2010 papers. Also see the two inter-lab comparisons in the response to Reviewer #1 above.

Pg 29598 lines 23-24. It is a pity there is no more recent La Jolla 14C data. We agree completely and eagerly hope for funding to continue the analyses.

Pg 29600, lines 9-10. I take it that the nuclear contribution is therefore ignored? Yes, we have added a comment to this effect.

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. This has been described more thoroughly in the text, in Section 2.3.3. Thank you for raising this important issue. The discussion is as follows: This value represents data from temperate northern latitudes (28 – 55 °N), dominated by C3 plants with some C4 grasses present (Bakwin, et al., 1998). Indeed, grasses in southern California are mostly C3 ryes, fescue, and bluegrass, with some C4 grasses such as St. Augustine (www.cropsreview.com/c3-plants.html, last accessed January 25, 2016).

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? We have added to the text: The magnitude of this correction is 0.5 – 1.2 ppm, averaging 0.84 ppm, which represents approximately a quarter of the Cbio, but the latter is very small, averaging 3 – 4 ppm and the correction does not affect our results with respect to Cpet and Cng. 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. We have added a description of the algorithm to Section 2.3.4.

Pg 29603 line 23. Bottom-up data products, not inventories – they are based on inventories but are much more complex than that. We have added "and data products" for clarification.

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. We have added a reference to the later 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? We have added a few sentences of discussion, saying that more influence by C4 plants would raise the Cng curve relative to Cpet, because C4 plants discriminate less strongly against the heavy isotope of carbon.

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. We have added two sentences of discussion to Section 3.2. We do not try to interpret the biosphere signal here, because it is very small relative to the uncertainties.

Pg 29608 line 2. r2 should be lower case. Changed!

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. In the winter season, photosynthesis is largely reduced. The peak for gross primary production is relatively flat in winter. However, there are still CO2 emitted to the atmosphere by respirations from the biosphere in winter, which has a relatively sharp peak compared with the photosynthesis term. The combination of gross primary production and respiration leads to the double peaks in each year in the net ecosystem production, which contributes to the semi-annual oscillation in CO2 [Jiang et al., 2012]. We have revised this is Section 3.3.

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. We have added a comment saying that we suggest that artificial irrigation removes the seasonality that might be expected for wet versus dry seasons.

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, 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? The goal of this analysis is to get at the longer-term trend, which requires removing high-frequency noise and signals for which we understand the governing process. We understand the annual cycle and wish to isolate the longer-term signal. A description of the process for calculating the modes is given in Section 2.3.4. The full set of IMFs is given in Figure A3.

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. This standard deviation comes from the raw data set, since the set without the annual cycle is simply an arithmetic manipulation of the original data. Figure 10f is the sum of the final trend (only one maximum or minimum) plus IMF 6 for the data set that has had the average seasonal cycle removed. It has not had any other trend, such as linear, removed. Therefore is contains information about variations that are inter-annual.

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. We have changed the plots of the modified data set, seasonal cycle removed, to include the average of the raw data. This leads to deleting the confusing parenthetical remarks in the discussion. Thank you for pointing this out. We have included revised plots in the Supplement to Referee #1's comments.

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. We have deleted this discussion, since it does over interpret the data.

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? A comment has been added to address this: There is a difference in timing between the data presented here (2010) and those from the CARB inventory (2011-2012). The difference may be due to uncertainties in the data or to the different domains covered by the two data sets.

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? As mentioned above, more influence from C4 plants would raise the Cng curve relative to the Cpet curve. We suggest that the different domains of our data and the CARB and EIA inventories are the dominant reason for the difference in seasonality. The statement regarding lag between purchase and combustion has been deleted.

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. We have changed this figure to use the annual averages from the Pasadena data as compared with the annual CARB inventory. The revised plots are included as the Supplement to Referee #1's comments.

The revised text, showing tracked changes, is in the Supplement to this comment.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C13054/2016/acpd-15-C13054-2016-supplement.pdf

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