Atmos. Chem. Phys. Discuss., 11, C1940–C1945, 2011
www.atmos-chem-phys-discuss.net/11/C1940/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The CO₂ release and Oxygen uptake from Fossil Fuel Emission Estimate (COFFEE) dataset: effects from varying oxidative ratios" *by* J. Steinbach et al.

Anonymous Referee #3

Received and published: 17 April 2011

General comments:

This paper constructs a new emissions data product for fossil fuel oxygen uptake allowing for the oxidative ratio due to fossil fuel to be constructed for use in carbon cycle studies. By combining high res CO2 emissions and country-level fuel/category data, this data product is built, made available and analyzed. The analysis focuses on comparing simulated oxidative ratios to measurements at a single (influential) station and by performing a series of pseudo inversions which test the impact that variations in the emissions data product might have in interpreting the estimated oceanic contribution to APO.

This is a solid contribution to the literature on this subject and the authors have done a good job at both creating a new dataset (thanks are deserved for making is public) and performing some analysis with it. It should be published after some modifications, which are noted in both the scientific and technical comments. Much of this hinges on a better description in places of how the dataset was created and how some of the analysis was performed as it was difficult to follow the description in its present form.

Specific scientific comments:

I remain mostly skeptical of the seasonal interpretation. This goes back to the way that sub-annual structure was included in the COFFEE data product from the outset. This appears to be based on the time structure in the Netherlands with some modification. This seems bordering on being a bit ad hoc. Firstly, how the time structure of the Netherlands was constructed should be included in the paper given the important of this to the results. Furthermore, there are high resolution sub-annual data to which this could be compared (European inventory, Blasing et al Gurney et al). Finally, the modification that was performed for the rest of the world needs a better description.

I question the validity of figure 3a. It seems that much of the long-term time variations were based on country-level extension in time. How can a pixel-level trend be determined? I also am a bit confused by figure 3b or more specifically the discussion regarding uncertainty associated with figure 3b. I am not sure if the three lines are statistically different given what manning and keeling suggest as absolute uncertainty. But, since I had trouble seeing the lines in the figure and the discussion on this point was not clear to me, I may be misunderstanding.

Technical comments:

Page 2, lines 8-10: grammar is a problem in this sentence

Page 2, line 13: should you not have a delta symbol in the denominator of this expression?

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Page 2, line 14: ". . .whereas it is αF=1.4 for fossil fuel. . ."

Page 3, first para: I find this discussion a bit confusing as you seem to be suggesting these ratios are now inferred ratios from atmospheric measurements? A bit more clarification would help here. The last sentence suggests that these are calculated from CDIAC values. So, I assume that the OR is based on measurements that when combine with emission estimates from CDIAC result in the calculated OR? Again, I think just some clarification would help.

Page 3, line 9: I think you need to close a parentheses.

Page 3, line 13: ". . .global maps of the oxygen uptake. . ."

Page 3, line 28: ". . .with oxidative ratios at the national level.."

Page 3 line 29: Start the sentence with "high resolution CO2 emissions. . ." this will make it clear that this is tied to the phrase in the previous intro sentence.

Page 4, line 6: ". . .cement production data at the national level.."

Page 4, 1st para: pretty heavy extrapolation. . . any worries about this? It is worth a comment, I think.

Page 4, line 13: instead of "sorts" use "categories".

Page 5, line 15: ". . .uptake at the gridcell level. . ."

Page 5, line 16: "..year were multiplied by the obtained. . ."

Page 5: an equation might be helpful here to show the relationship between the high res CO2 and the country/fuel derived OR to get the O2 uptake.

Page 5, line 19 and forward: The temporal structure appears to be very ad hoc. Sounds like using the Netherlands as the template with some adjustment. On line 24, suppression in lower latitudes is referred to and done where "reasonable". This is vague and warrants some further description as the time structure is critical to such a dataset.

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Page 5, line 30/31: I would not the download website here as well as where you have it in the conclusions portion of the paper.

Page 6, line 14: strike “expectedly”

Page 6, line 18: comma needed after “India”

Page 6, line 21: strike “in the plot”.

Page 7 line 8: strike “merely” and “only”.

Page 7, lines 4-26: since this is all based on the time structure of a small country, the Netherlands, I recommend caution in interpreting the time structure from your global map. It is somewhat tautological. For example, the depressed seasonality in the tropics is nothing more than a reflection of what you imposed. Also, how might this compare to high res country treatments such as Gurney et al or Blasing et al for the US? You can note all of the time structure but you must reiterate that this is nothing more than the Dutch time structure. This also places emphasis on explaining how that time structure was created (in EDGAR).

Page 7, line 27-31: But, if the pixel-level data were built from 2001 on with a country-level time series, how can you honestly calculate a pixel-level trend? You don’t have the resolution in time at the pixel level (it is country-level time extension). This is a bit misleading it seems to me.

Page 8. Line 24-28: I wonder if the lines in 3b are actually statistically different from each other. CDIAC states 10% uncertainties... this would imply that there isn’t much difference between these. Error bars would help to convince the reader that these are actually different. My suspicion is that they aren’t. If you take the manning and keeling uncertainty yuou quote in the next para these don’t appear to be different. Add in the uncertainty in the CO2 emissions and I think you can’t separate them at all.

Page 8, line 32: I think you want to note that this is the most recent sink estimate that incorporates APO. There are many estimate of carbon uptake and many of these vary

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from the manning and keeling estimates.

Page 9, 1st para: this clearly relates to my comment above.... But, this puzzles me further. It seems to me that you are using the IAV as a form of uncertainty? You seem to agree that the uncertainty that manning and keeling identify is the dominant but go on to state that this cannot be quantified for coffee. But, it seems that the 0.04 value could be used and worked into figure 3. You appear to have done that but I cannot make out any other lines on the plot as the color is very weak.

Page 10, lines 1-13: you use "perceived" in 3 places in this para (and in figure 4, figure 5). I am not sure what is intended by this word. Do you mean "simulated" perhaps? Or maybe "calculated"??

Page 10, lines 6-8: I cannot understand what regression is being performed. It seems like perhaps a regression over time is being performed on the oxidative ratio as derived from the simulation? And this time regression is of 5 days duration? So, 5 points? The purpose of this is to capture the OR coming from a surface gridcell rather than a mixture of sources as might be expected from an ambient measurement? This section needs a lot more explanation as it appears critical to the subsequent discussion and conclusions. It is hard to interpret figure 5 without a better explanation of this.

Page 15, line 7: the word should be "assess" not "access".

Page 17, line 21: "...emission ratios from COFFEE results from differences in the ..."

Page 19, line 7: recommend citing the european high res inventory and/or the US-vulcan inventory here.

Page 16, line 23: I have to question the utility of this section given that the time structure of the underlying CO2 emissions was produced from a very limited and nearly ad hoc application. It seems to me that interpretation of the seasonality is directly dependent upon the approximations and assumptions used to construct the sub-annual time structure in COFFEE.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 6187, 2011.

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