Author Comments to Referee Comments by Peters on RECCAP FFCO2 manuscript.

We appreciate the constructive comments of the reviewer (italicized below). We respond to each of his comments below.

The paper provides a comprehensive and valuable synthesis of fossil-fuel emission inventories. The authors cover most aspects of fossil fuel emission inventories at various spatial and temporal scales. They also describe the main differences between independent data sets, without performing an extensive intercomparison. The text is of good quality and I recommend the paper for publication. I do, however, have many minor comments. Most of my comments are directed at sections 1-5, and in particularly section 3. While I do not have any problems with the content, I have many suggestions for minor improvements, reordering, modification of some figures, etc. Most of these comments are suggestions which the authors may like to take up, or not!

Minor comments and suggestions

1. The title states ". . .emissions from fossil fuel combustion", but the first paragraph states ". . .emissions from fossil fuel combustion and cement production". Perhaps later gas flaring and other process emissions are considered? If so, consider changing the title to something like ". . .emissions from fossil fuel combustion and process emissions"?

The suggested title change brings up an old discussion in the community. The majority of what is reported on is CO_2 from fossil fuel combustion. Most data sets (see Table 1) consider other CO_2 sources as well (e.g., cement production, gas flaring, ...). These other sources have not always been given title space because individually as they are a minor component (<10%) of the overall emissions. Additionally, by not making the title a grocery list, the title becomes shorter and more tractable.

The title has remained unchanged as not all of the data sets discussed contain process emissions.

2. The last sentence of the abstract repeats some earlier text (line 6+). Line 6+ explains that the paper covers various issues, but the abstract focuses on very narrow aspects (total emissions, uncertainty). Consider removing the last sentence and including some other findings in the paper, such as on spatial and temporal issues, etc, etc, . . .

We agree the last sentence is redundant with the preceding paragraph; it has been removed from the text to be published in BG. We replaced that sentence with:

This manuscript concludes that carbon dioxide emissions from fossil-fuel combustion continue to increase with time and that while much is known about the overall characteristics of these emissions, much is still to be learned about the detailed characteristics of these emissions.

3. The use of FFCO2 throughout, when sometimes process emissions are included. A short hand is okay, but it would be good to somehow acknowledge that this is more than just FF.

This comes back to the issue raised in #1 above: completeness of description versus a tractable solution. The majority of what is discussed are fossil-fuel related emissions. Other emissions are sometimes included as is discussed in the paragraph which spans pages 1302-1303 where cement, gas flaring, and other industrial processes are specifically discussed. Figure 1a specifically lists and shows a cement contribution to FFCO2. We think the intent of the acronym is clear; we welcome specific suggestions for an alternative acronym.

4. Page 1302, line 25. And some inventories may include other, albeit more minor, process emissions. As you mention later.

We specifically mentioned these two (gas flaring and cement) because they are later shown in Fig. 1a and are commonly included in some inventories. Other, minor processes are mentioned later because they are not commonly included in inventories (with EDGAR and UNFCCC being the major exceptions), but they are still CO_2 sources.

5. Page 1303, line 1-2: Is the venting included in CO2 only in CDIAC, or is this a standard assumption across all inventories?

It is a standard assumption across most inventories (EDGAR 4.2 is an exception). For most inventories, gas flaring is then actually the sum of flaring and venting. Perhaps this another instance of where the tractability of flaring is being used for the lengthier, but more complete, flaring and venting. The text to be published in BG is slightly modified to reflect this EDGAR 4.2 exception.

6. For all figures. The figures are plotted with lines and symbols. I suggest removing the symbols as it makes it easier to compare the curves.

We have maintained the symbol and line approach where it is used to emphasize the density of our data. Lines generally imply continuity in the data. Symbols generally imply the discreteness of the data. As the manuscript hopefully conveys, FFCO2 data is discrete in many dimensions (including space, time, fuels, ...). So, perhaps we should do away with the lines altogether? For now, we have maintained the lines to aid the viewer in seeing the trends, realizing that the trends emphasized are a function of the x-axis unit scale (e.g., an annual x-axis shows annual trends, but hides monthly trends/variability).

7. Page 1303, line 18: Perhaps say a few words about what the figure shows?

Some description of “what the figure shows” is in the figure caption itself. This description was not included in the main body of the text to maintain the flow of ideas in the main body without a digression into the details of this particular data set. We think it is obvious from Fig. 1a that emissions are generally increasing with time; this is later specifically discussed in the text.

8. Page 1304, line 8, equation 1. Is this really done for cement and gas flaring? By definition, the emissions are allocated to where they occur, and for cement and flaring the emissions will *occur at the point of production?*

Yes, but from cement and gas flaring the terms are equal to zero for all but production. While in the case of cement and gas flaring the zeroing of terms may seem extreme, it is similar to the case of solid fuels where the non-fuel uses are also set to zero.

9. Page 1304, line 8: This is not strictly true. It is true for energy statistics, and most emission statistics. However, emission statistics that are consistent with the system of national accounts should, by definition, include the emissions from bunker fuels allocated to the country where the operator of the ship is resident. This is reported to Eurostat by EU countries for example. E.g. Peters, G.P., Hertwich, E.G., 2008. Post-Kyoto Greenhouse Gas Inventories: Production versus Consumption. Climatic Change 86, 51-66.; Peters, G.P., Marland, G., Hertwich, E.G., Saikku, L., Rautiainen, A., Kauppi, P.E., 2009. Trade, Transport, and Sinks Extend the Carbon Dioxide Responsibility of Countries. Climatic Change 97, 379-388.

We wrote here, as you stated, the common reporting of bunker fuels in energy and emission statistics: not attributable to any one country. We also write on pp. 1309-1310 how individual data sources (i.e., CDIAC, IEA, EIA, EDGAR, and the UNFCC) treat bunker fuels. Table 1 also includes a bunker fuel footnote. We agree bunker fuels are not uniformly reported on an equal basis; that is one of the reasons for disagreements between different data sets as noted in Fig. 2.

You mention one scheme for a uniform treatment of bunker fuels. It has not been uniformly adopted by all nations and has not been uniformly adopted by the suppliers of energy data which form the basis of the FFCO2 statistics presented in this manuscript.

This is not an easy problem to solve and the increasing politicalization of FFCO2 may make a solution even more difficult to achieve. Let me illustrate the accounting issues with a real world example that hopefully illustrates some of the political issues involved: An Air France jet leaves a New York airport bound for a Paris airport. Onboard are 122 passengers of 13 different nationalities. The plane was fueled with aviation gasoline stored in New Jersey and piped to the airport. To whom should the resulting FFCO2 emissions from the flight be credited? To France as this was an Air France jet? To the U.S. as this was where the flight originated? To a multiple country allocation in proportion to the nationality of the passengers onboard? To New Jersey (as opposed to New York) as this was the selling point of the fuel (important if one wishes to focus spatial resolution to states/regions within nations)? All of these accounting options have been proposed. None of them have been universally adopted (although the 2006 IPCC reporting guidelines provide more clear guidance on this issue, these guidelines do not comply with traditional classification schemes used by ICAO for distinguishing between domestic and international flights).

10. Page 1305, lines 5-13. I assume this is a CDIAC specific result?

You are correct, the four reasons listed here are CDIAC-specific and are given because the data in Fig. 1b are CDIAC data. This figure is presented for two reasons of a structural nature for the manuscript: 1) it helps illustrate the level of detail inherent to FFCO2 inventories, and 2) it helps to set the stage for uncertainty discussions. While most FFCO2 inventories do not follow the CDIAC method of reporting emissions on a production or a consumption basis, they do work with these four issues in their accounting schemes.

11. Page 1305, lines 5-13. Is it possible to put what share each of the different factors play in the difference?

I (RJA) have started this calculation in the past, but never completed it. I pulled up the old results in response to your question. For the period 1950 to 2008, bunker fuels account for 93% of the difference between the two curves shown in Fig. 1b. That is where I stopped the calculation since bunker fuels were such a dominant part of the answer. However, that overall value hides a lot of variability in individual years. The full answer is also not a simple one. In some years, the amount of bunker fuels consumed exceeds the difference between the two curves. Since this is a zero sum calculation, that means that some combination of the other three factors makes up the difference - a likely candidate is changes in stocks. Since this is a synthesis manuscript and the full answer to your question is a separate detailed study, the text has not been changed in response to your question.

12. Figure 1b. This figure would be better as an absolute or percentage difference (or both, one on each axis). The difference is too small relative to the difference for the current plot to be useful.

We disagree about the utility of the current plot. The current plot effectively shows that the two curves are similar, but not exact. That is the point: production is similar to consumption, but not exactly equal. There are offsets for various reasons as discussed in the text. But in the big scheme, these offsets are not so significant. However, depending on the nature of a specific inquiry of the data (especially in regards to specific spatial or temporal scales), the offsets may be significant.

Your suggestion of an absolute or percentage difference plot could be a useful addition. The text already gives some statistics in absolute terms (i.e., 400 Tg C and 24 Tg C). In relative terms, the 400 Tg C is only 5% of the production value in that given year. Other years will see even a smaller percentage contribution. These specific data are available from CDIAC, but over the years I (RJA) can only recall less than five inquiries about their specific values. Given the apparent lack of interest in the general community for these values, that the nature of this manuscript is a synthesis, and the length of the present manuscript, we are not inclined to add the absolute or percentage difference plots you suggest.

13. A general comment, many of the results and discussions seem to be CDIAC specific. When this is the case, this should be mentioned. You could outline the issue you want to discuss, and then say "In the case of the CDIAC database, . . .". This is done in many cases, but just check it is done in all cases.

We have reread the manuscript in an attempt to comply with this suggestion. We agree that we have used CDIAC data to illustrate many of the topics discussed in the manuscript. As stated on page 1311, "[u]se of IEA, EIA, or EDGAR data would give similar results and/or conclusions", but maybe not the same exact quantitative results. However, the general qualitative conclusions should remain the same, regardless of what data set were used to derive the result. We have attempted to attribute all of the specific data used in the text. We include in the author list some of the principal contributors to the major data sets discussed. We welcome specific suggestions where we have overlooked and not complied with appropriate attribution of results or conclusions.

14. Page 1305, lines 14+. This seems one good justification why FFCO2 inventories are needed. Perhaps this paragraph could be shifted up towards the start of the introduction.

An earlier version of the manuscript did have this paragraph closer to the beginning of the text. As the manuscript unfolded it was gradually pushed deeper into the introduction and then intentionally placed near the end of the introduction to reinforce why the rest of the manuscript follows.

15. Page 1307, lines 4-5. What confidence interval does the error refer to? These percentages are also different to what was mentioned in the abstract?

A confidence interval is not specifically imposed here. Instead, only a general comparison is implied by directly comparing two numbers generated by different authors.

The percentages specifically mentioned here are in reference to national totals. These percentages discuss all of the outliers. In the abstract, we tried to give the full range of values. While the few percent is seen on page 1307 (lines 4-5), the full abstract statement comes from page 1338 (lines 11-15).

16. Page 1307, lines 17. Also, Myhre, Gunnar, Kari Alterskjær and David Lowe, 2009. A fast method to update global fossil fuel carbon dioxide emissions. Environmental Research Letters, 4: pp. 034012. I also think EDGAR might do this for their updates as well, but check?

Many people have used the BP data because it has published estimates for a given year prior to the more complete data sets compiled and published by UNSO or the IEA for that same given year. We can not guarantee that all users of the BP data follow the same procedures in its use. Here, we cite one author who has published using the BP data. We do not cite here all authors who have published using the BP data.

17. Page 1307, line 13. I am not sure exactly what is being said here. It is often assumed that the CO2 inventories include the C that ultimately ends up as CO2, even if it goes via CH4 or CO? But perhaps this is not the case for some of the global datasets? Or the amounts are so small it does not affect the inventory? Or is there some inconsistency between CO2, CH4, CO, BC, ash, . . . inventories as they should all add to the C in the fuel? I think this is an important point and *could be explored a little more. This paper is also relevant in this context, Boucher, O., Friedlingstein, P., Collins, B., Shine, K.P., 2009. The indirect global warming potential and global temperature change potential due to methane oxidation. Environmental Research Letters 4, 044007.*

The FFCO2 inventories discussed here report released carbon as if the fraction oxidized has all been oxidized immediately to CO_2. However, that is not truly the case. Incomplete combustion can lead to less-oxidized forms of carbon also being released. The fraction oxidized term accounts for carbon left in the combustor as ash or some other residual product.

One use of these inventories is in atmospheric circulation and chemistry models. There is a lot of variety in these models in how they treat carbon. Some have various inputs and outputs from the atmosphere in which carbon is added or subtracted in various chemical states. Some have active chemistry in which carbon is transformed (including oxidation and reduction reactions), others do not. One of the goals of these models is to simulate using FFCO2 inventory data, as well as other data, the concentration of carbon in the atmosphere. The degree of agreement between model simulated concentrations and actual measured concentrations is one metric of model quality. Obviously, the sources and sinks of carbon used in a specific model as well as the timing of chemical reactions affect how well simulated and measured concentrations compare.

The text to be published in BG has been slightly modified in accordance with your suggestion:

The fuel oxidation term is important as it assumes immediate oxidation to FFCO2. This ignores kinetic and other chemical effects and becomes important when measured atmospheric carbon concentration data is compared to model output (see Enting et al., 2012 and references therein; Boucher et al., 2009).

18. Table 1, EDGAR, global total on a common basis seems to be missing?

It is missing. We have not been able to reconcile EDGAR to this common basis.

19. Figure 2. This is a nice figure, but I am not sure why it is reported by GDP per cap? I think for emission inventories it would be better to sort by size of emissions. This would be approx. a cone shape with the big emitters at the top (presumably more certain, give or take a China, India, Russia, etc) and the small emitters at the bottom with larger uncertainty. Also, it might be worth cutting Gibraltar and mention it in the caption to give more detail on the x-axis.

The figure to be published in BG has been replaced with one sorted along the y-axis by mean carbon dioxide emissions (of reported values).

20. Page 1309, line 5 writes "Figure 2 and Table 1 summarize the published comparisons", but the Table or Figure do not have such information. Or did I miss something?

Figure 2 graphically compares emissions from four different data sets for given countries. Table 1, common basis row, numerically compares three data sets.

21. Table 1 on the UNFCCC and 191 countries, it is probably with a footnote that says that x countries are updated annually (Annex I countries I assume), and the remainder are irregular.

The text to be published in BG has been modified in accordance with your suggestion:

[a]Annex I countries are to report annually, non-Annex I countries have less stringent reporting requirements

This information is also given in the main body of the text on page 1310, lines 23-26.

22. Page 1309, line 11. Put UNSO in words the first time.

It was, page 1307.

23. Page 1309. For the IEA description, detail is given on how the energy data is converted to CO2 (using IPCC approaches). There is no such description for CDIAC, it does not mention how the energy is converted to CO2. Perhaps mention this for all datasets to make consistent?

For CDIAC, the cited references give the full details of the calculations. For this syntheses, the authors do not think more details are necessary. You have already stated that the IEA has a description. The EIA is CDIAC-like as discussed in the text. EDGAR has the same level of detail given as the EIA. The UNFCC uses the IPCC guidelines similar to IEA and EDGAR.

24. Page 1311, line 9. But also not remarkable. Many of the data ultimately start at the same place, but undergo different processing. The energy statistics, emission factors, etc, will be different in each data set, but sometimes one data set will be an overestimate and other times an underestimate. The law of many numbers would then indicate that many of the differences will cancel. What would be more interesting, and time-consuming, would be to see what causes the differences between data sets. For example, go through a set of stepwise calculations were 1) use the same energy statistics in each data set and compare results, 2) use the same emission factors in each data set and compare, 3) . . . In other words, what is the cause of the differences? I am not sure if anyone has done this, and I am not really suggesting you do it (it would be another paper), but I think the use of the work "remarkable" could be expanded on a little bit. As I mentioned at the start, you could also argue it is non-remarkable.

Full data sets comparisons at the level you suggest have not been completed. But, they have been completed for selected subsets of the full data sets. We have kept the word "remarkable" because while some of the input data is the same, it is not all the same. Likewise, while the different data sets undergo similar procedures, each tuned to different assumptions and coded to different emphases, they also are not identical. We think it is "remarkable" that the global results are well within the uncertainty estimates of the various data sets given the slightly different initial data and slightly different data processing. Obviously, mathematically, the differences are quantitatively small. But since the data and processes were derived relatively independently and not with the goal of similarity, we think this highlights a strength of science: different investigators using different means achieving the same result.

25. Page 1311, line 12. This is a little misleading. At the global level, yes, but not at the country level (as in Figure 2). Also, from my experience, the datasets can also vary considerably over time (for example, two data sets can be very similar for some years, but then there may be a period where one data sets shows a drop in emissions and another doesn't). I think it is okay to use CDIAC through the paper, but I think it should be emphasised that there is a lot more variation in the datasets then the global total indicates.

As you note, Figure 2 is specifically constructed to show the differences between the data sets. Many of the rest of the figures convey global (not national) properties of the data sets. For these figures we have selected to use CDIAC data, as discussed already in the text. Use of the other data sets would not significantly change the figures or the text. We could have opted to show all the data sets for each figure, but given their similarity at these global scales, we did not think that was a good use of publication space or reader's attention. We could also have chosen to use different data sets for different figures, but we did not as we wanted to avoid potential criticism that we were picking specific data sets to illustrate specific points.

26. Page 1312, line 1: E.g., Peters, G.P., Marland, G., Quéré, C.L., Boden, T.A., Canadell, J.G., Raupach, M.R., 2012. Rapid growth in CO2 emissions after the 2008–2009 global financial crisis. Nature Climate Change 2, 2-4.

Yes, this reference describes another example of the variability of individual annual FFCO2 emissions. It has not been included in the list of types of individual year deviations from smooth functions.

27. Fig 3a. This could be dropped as it has the same data as Figure 1a (though, see my earlier comment on this).

Figure 1a shows fives sources of FFCO2. Fig. 3a shows the total of the five lines on Fig. 1a. The two have not been combined into one figure so that five individual lines of Fig. 1a could be more easily distinguished. If combined into one figure, the five lines would occupy about the bottom 1/3 of Fig. 3a.

28. Page 1312, line 8: It is not "evident" from the figures, but evident from the work of Raupach et al.

The sentence has been deleted and that paragraph to be published in BG now begins with:

Figures 1a and 3a show that FFCO2 from each of the major fuel sources has grown over time.

29. Page 1312, line 20: While biofuels are strictly not included in FFCO2, that does not they are unimportant. How big are the biogenic CO2 emissions in relation to FFCO2? Maybe EDGAR has this data (short cycle CO2). Is this share growing over time? While it is generally assumed that biogenic is CO2 (flux) neutral, it does not have a neutral impact on climate (Cherubini, F., Peters, G.P., Berntsen, T., Strømman, A.H., Hertwich, E., 2011. CO2 emissions from biomass combustion for bioenergy: atmospheric decay and contribution to global warming. GCB Bioenergy 3, 413-426.). Also, are biogenic CO2 emissions not important for modelling the global carbon cycle? In any case, it would be good to add a few more words on the biogenic CO2 emissions, even if they are not FFCO2.

You raise lots of important issues in relationship to biofuels. However, most of them are not pertinent to an FFCO2 synthesis. We mention biofuels here only in brief because of the interest in them of late.

The text already gives one measure of modern biofuels relative to road transport fuels (e.g., oil). We have not extended this measure to total FFCO2 as it is less than the 3% already in the text and as Fig. 3d shows road transport is near the middle in terms of sectoral consumption. Besides biofuels used for transportation, biofuels are also used elsewhere (e.g., fuelwood, charcoal, wastewood, ...), especially in the residential and industrial sectors. EDGAR 4.2 provides this type of data. These uses are well outside the scope of this synthesis.

In absolute terms, the use of biofuels for transportation is growing over time as not too long ago the global production was small and it is now growing in response to various market and government policy forces.

We have not claimed a zero impact on climate from biofuels. This synthesis about FFCO2 emissions. FFCO2 effect on climate is one reason to study FFCO2 emissions, but the climatic effects are not the focus of this manuscript.

Biogenic CO2 emissions are important to the global carbon cycle. Biofuels are just one component of these emissions.

We have not expanded the text on biofuels in response to this comment. The suggested changes are outside the scope of this manuscript. If we expand the scope to include these comments, then we should also include similar comments on other non-FFCO2 energy sources that have similar scope such as solar, nuclear, wind, ...

30. Just a general comment on all the CDIAC related figures. Why not show the results up until 2010, the most recent value available?

Two main reasons for this:
1.) the long time lag to publication of a 15 author manuscript (when the writing of this manuscript began, the 2007 data were the latest available), and
2) the later data (to year 2010) are based upon BP data. These data are fundamentally different

in their collection and utilization than the more detailed UNSO data used. We believe that inclusion of these later year data would at best misleading without an extensive discussion of their utility in some graphs and discussion s presented in the text and at worst factually wrong as the more detailed UNSO data are obtained and processed, potentially leading to significant revisions in graphs and discussions presented in the text.

31. Fig 3b/3c. It would be interesting to see this figure as percentage growth rates as well. Figure 3c is remarkably uninteresting (given it just integrates Fig 1c). I think the percentage growth rates would be more interesting than 3c? See my later comments on Fig 3h.

We had thought about a percentage growth rate plot, but decided against it as it can be easily calculated from the data already given in the manuscript. Additionally, we believe percentage growth rates are more interesting when put into a temporal context (e.g., over annual, 5-year intervals, 10-year intervals, ...). The context for these intervals is study dependent and while one study is interesting to one reader, it is not so interesting to another. The cumulative plot has its own interest: from size of fossil fuel reservoir emitted to the atmosphere to issues of emissions equality to issues related to the atmospheric airborne fraction and resulting disequilibrium and radiative effects in the atmosphere.

32. Page 1313, line 9: "Figure 3c also highlights . . . that more than 50% of FFCO2 has been emitted since 1980." I could not detect that by looking at the figure?

The curve maxes out at about 350000 Tg C. One half of this is about 175000 Tg C. If one visually inspects the curve at 175000 Tg C, then one sees this occurs at about 1980. The revised figure to be published in BG will have dotted lines to explicitly show the 50% point.

33. Page 1313, line 3-10: The airbourne fraction discussion could do with a little more elaboration. For example, the work of Knorr, Sarmiento, etc that critique some of the cited literature.

We agree that there is disagreement about the magnitude of the airborne fraction. However, the airborne fraction is not a central idea of this manuscript and we do not utilize the airborne fraction magnitude further in the discussion of the manuscript. We have decided not to devote more manuscript space to an idea not central to this synthesis.

34. Table 2a/2b. I think this needs a bit better explanation. The n refers to the number of countries, but are the values in the table the growth rates at the country level or the global level? I guess at the country level with (2007-19XX)/19XX, but then the word "annual growth factor" is used? Are the statistics based on finding all the growth rates, then doing a min, max, med, avg? An alternative could be to do a regression (making the end point less important also). And some of the values are remarkably high, which makes me think this is done at the country level and we are looking at statistical noise for the max. It seems that the method chosen is skewed by outliers? Another way to approach this would be a little in the spirit of Figure 2. For example, have a scatter plot with mean(emissions) on the x-axis and mean(growth rate) on the y-axis. The *different time periods could be different colours? The mean would be calculated as regression/meanvalue. It may be possible to label some of the most relevant countries. This might be an easier way to portray the results in the table.*

The values in the table are for individual countries.

We do not understand the confusion between "annual growth factors" and "global" factors. One is dealing with temporal (i.e., annual) units and the other spatial (i.e., national/global) units.

Yes, the statistics (i.e., min, med, avg, max) are from all the available growth rates in each category (i.e., specific begin and end year dates and/or Annex B status).

We wanted to show statistics that are sensitive to end points, thus the min and max. These indicate the extremes of the growth rates. We also wanted to give an idea of the central tendency of the growth rates, thus med and avg.

We did think about using a scatter plot similar to what you described. However, we chose to use a table instead because we wanted the reader to focus more on the global properties of these countries rather than the values for each individual country. If the reader is interested in the value for each individual country, the data are freely available and can be plotted up in a manner best suited for the reader's interest.

35. Page 1314, section 3.2. At the start, you could give more detail on the results. There is only really one sentence, but there are some other interesting features that could be pointed out. Since you mention it would be interesting to show a developed/developing split, why not do it? Also, for the last paragraph perhaps backing up with references (Baiocchi, G., Minx, J.C., 2010. Understanding changes in the UK's CO2 emissions - A global perspective. Environmental Science and Technology 44, 1177-1184.; Peters, G.P., Minx, J.C., Weber, C.L., Edenhofer, O., 2011. Growth in emission transfers via international trade from 1990 to 2008. Proceedings of the National Academy of Sciences 108, 8903-8908.)

There has been some debate amongst the coauthors of this manuscript about how much sectoral data to include. There are many studies, including the two you mention, which delve deeper into sectoral emissions at both global and national levels. A synthesis of sectoral studies alone could easily reach the same length as the present manuscript. However, sectoral aspects are just one aspect of FFCO2 emissions. We have not explored any of these aspects in great detail (e.g., more could be written about which sectors are driving trends in fossil fuel consumption), but rather have tried to synthesize the pertinent details of many aspects.

We did not do a developed/developing split as we described it briefly in the text and to that level another figure was not needed.

36. Fig 3g, it might be more interesting to show this as a share of total cumulative emissions? This would presumably show that Annex I has dominated, but that dominance is decreasing (in *both annual and cumulative emissions).*

We have kept the two curves as they are the basis for some arguments on how historical emissions should be considered in future emission reduction schemes.

37. Section 3.3. This is a much better description of the data. It might be worth mentioning early on when discussing Fig 3e, that the drop in 1990 is primarily caused by the former Soviet. You mention the UK and Germany later on, but I think the former Soviet issue needs to be emphasised. The last two paragraphs seem a little misplaced though, but they should be mentioned somewhere. The last paragraph should cite Raupach et al 2007, and it would be even better if there was a plot of CO2 intensity as a function of time in B and non B (e.g., Caldeira, K., Davis, S.J., 2011. Accounting for carbon dioxide emissions: A matter of time. Proceedings of the National Academy of Sciences 108, 8533-8534.)

There are many inflection points in the figures presented in the manuscript. We do not discuss or even mention all of them due primarily to space reasons and that discussion would cloud the larger issues of the synthesis. We have not specifically mentioned the economic collapse of the Soviet Union and its satellite countries, but have mentioned it in general.

The last two paragraphs are mentioned as a transition to the next section. Focused geographic and temporal trends (as well as other terms such as carbon intensity) should not be used to hide the fact of rising FFCO2 emissions. While emissions are local in both space and time, the effects are globally dispersed and local effects are not proportional to local emissions.

Raupach et al. (2007) has been added to the text to be published in BG.

We have not included a carbon intensity plot as it detracts from the central message that emissions are rising.

38. Page 1318, line 15: While I have no problems with the cited literature, I have discovered recently it can be misleading. Allen et al, for example, seek a relationship with peak T and cumulative emissions for all time (that is, the integration continues past the peak)! I find this a little bizarre, but I am not an expert on such things. Matthews et al look at T(t) and cumulative emissions as a function of time, so that the T and cumulative emissions are compared at the same time.

Both works you cite have made contributions to the ideas discussed in the text. While we may not agree with all of their methodology and conclusions, they did make contributions to the evolution of this idea.

39. Page 1318, line 25: Suggest to include Peters, G.P., Marland, G., Quéré, C.L., Boden, T.A., Canadell, J.G., Raupach, M.R., 2012. Rapid growth in CO2 emissions after the 2008–2009 global financial crisis. Nature Climate Change 2, 2-4.

Peters et al. (2011) has been added to the text to be published in BG.

40. Fig 3c, could be dropped given Figure 3h.

Figure 3c is the global cumulative FFCO2 emissions curve for years 1751-2007. It gives one basic description of the FFCO2 data.

Figure 3h does include a cumulative emissions curve, but it is for a subset of years. Figure 3h is used in a different discussion in the text.

41. Page 1318, section 3.4. I am not convinced that this is the correct section heading. I think perhaps "The importance of cumulative emissions" or similar. If so, then I think that the cumulative emissions material from earlier could be moved into this section.

The coauthors thought that we needed to add a section on why we monitor FFCO2 emissions. We did not want to reproduce the latest IPCC Impacts volume. So, a section on global emissions and one global impact: temperature, was chosen. Other impacts could also have been chosen, but temperature has one of the simpler messages.

42. Section 3 and Figs 3. Overall. I think there is a lot of useful information here, though I think there might also be a little bit of redundancy. I think some figures could be combined, some pieces of text reordered. On occasion, the same information could probably be portrayed in the same figure (ie, combine some figures and make use of the left and right axis). For example, the figures on cumulative emissions could probably all put together side by side. If the Kyoto figure was added as an addition subplot in Figure 3h, then all the cumulative figures would be in the same row of subplots. Likewise, Figs on growth rates could be put side to side to have the global and Kyoto figures next to each other. There is also redundancy with Figure 1b and Figure 3a. Before I mentioned the global total could be put on one axis and the different on the other axis. The sector figure and the fuel type figure (1a and 3d may go well together next to each other). Essentially, I think all this information is useful and needs to be there, but I think it may be combined a little better to reduce the number of figures and put relevant information together and not split between figures.

We, too, were concerned about the number of figures. We did eliminate some during the composition stage of the manuscript as well as resisted review comments to add more figures.

We also tried to match the figures to the text as this is a synthesis manuscript and not all of the manuscript readers will be as familiar with FFCO2 data as the coauthors. Thus, figures were tailored to where they were initially discussed in the manuscript to illustrate specific ideas. While some figures may be able to be combined or co-located, we did not pursue this as the manuscript discussion had yet to unfold to that point. Instead, we added text to figure captions to show the flow of data (e.g., Fig. 3a, " This figure was created from the sum of national production values (see section 1) for 15,830 country-year pairs.").

As for the cumulative figures specifically mentioned, we do not think it makes sense to combine them. Figure 3c is the global cumulative plot from 1751-2007. Figure 3g gives a breakdown of this by Annex B status. As Annex B had yet to be introduced in the manuscript, it does not make sense to include the Annex B curves on the Fig. 3c plot. Figure 3h versus Fig. 3c/3g has already been discussed in #40 above.

Figure 3b is the global growth rate plot from 1752 to 2007. Figure 3f gives a breakdown of this by Annex B status. As Annex B had yet to be introduced in the manuscript, it does not make sense to include the Annex B curves on the Fig. 3b plot.

Figure 1b is used to illustrate the differences and similarities between global FFCO2 totals as calculated by production and consumption data. Figure 3a is a repeat of the production data but is displayed separately because in Fig. 3a there is no distraction caused by the sum of countries curve.

We see no advantage to placing Fig 1a (five fuels) and Fig. 3d (sectoral consumption) next to each other. While the strongest link between the two may be liquid fuels and the transportation sector or solid fuels and the power generation sector, those are not 1:1 relationships. Additionally, the earlier figure is introduced in the manuscript when basic fuels that comprise FFCO2 are being discussed. The latter figure occurs several pages later when how those fuels are consumed is discussed.

43. Page 1319, line 10. I am not sure "Discretizing" is the right word?

Discretizing conveys what we are doing here: breaking up a continuous global data set in separate regional parts. We welcome suggestions for an alternate word.

44. Page 1320, lines 26+ and Fig 5. I am not sure why this discussion is here? Fig 5 has 5 hand-picked countries and thus the discussion is irrelevant? Fig 5, I see that Libya and Grenada were chosen as the min and max. However, it may be more relevant to include 5 politically relevant countries? If doing the max min, it may be worth including all the countries in the figure, but put the remaining 117 in a light grey line plot or something? This would demonstrate the point more clearly. Following this idea further, all countries could be put in light grey colour and then 5 politically relevant countries selected for highlight. This would show the bounds and allow you flexibility to include more interesting countries. Just an idea.

We have tried many variations on this plot and thought this was the best. Aspects we thought important to include in the plot were: 1) min and max, 2) average, and 3) some important emitters. The number of curves shown is a compromise between these goals and clarity in the plot. As all curves fall between the min and max, adding more curves tends to muddle the presentation as opposed to clarifying it.

The discussion is here to give the reader a sense of relative growth factors among the various nations in the FFCO2 data sets. This is another dimension to growth than that portrayed by the global growth curves.

45. Page 1322, section 5.1. There was no mention of the EDGAR gridded data (perhaps this is to come later) and perhaps more city based inventory could be mentioned (e.g. Greenhouse Gas Emission Baselines for Global Cities and Metropolitan Regions, C. A. Kennedy, A. Ramaswami, S. Carney, and S. Dhakal, in Cities and Climate Change World Bank). Perhaps contact one of those authors for some more references on city level inventories. It may also be worth mentioning that there are methodologies for corporate level and city level inventories, though non-standard (WRI and WBCSD, 2004. The Greenhouse Gas Protocol - A Corporate Accounting and Reporting Standard. World Resources Institute (WRI) and World Business Council on Sustainable Development (WBCSD)., ICLEI, 2009. International Local Government GHG Emissions Analysis Protocol. ICLEI - Local Governments for Sustainability, and probably more). I don't think this has to be an extensive review or reference list, but mentioning the main references to allow the reader to research themselves would be useful.

The EDGAR maps are discussed in section 6 where their mapping methodology is the focus. EDGAR does not contain sub-national data, but rather maps national data at a sub-national scale. Sub-national data is the focus of section 5.1.

Since this is mainly a global synthesis, not a local synthesis. We did not focus on city-level inventories which are few in number, of limited geographical extent, and composed by many different methodologies which have various levels of completeness.

46. Page 1325, section 6.1. A reference on the importance of altitude would be useful.

Altitude is discussed later.

47. Page 1328. I know you mention the ordering, but I would consider putting 6.2.2 first. My thinking is that the ordering would make sense from less detailed to most detailed?

Section 6.2.1 was placed first as it contains the first approach to this mapping (e.g., Andres et al., 1996). Subsequent approaches looked to improve upon this first effort.

48. Page 1331, section 6.2.4. But can other emissions be measured and correlated with CO2? E.g., Akimoto, H., Ohara, T., Kurokawa, J.-i., Horii, N., 2006. Verification of energy consumption in China during 1996-2003 by using satellite observational data. Atmospheric Environment 40, 7663–7667.

Yes, there are other satellite approaches but these are all based upon subsequent models, assumptions, and ancillary data to make CO_2 maps. These are included in section 6.2.3 Satellite-based proxy approaches.

49. Page 1331, section 6.3. I guess there are too issues here. First, emissions can be assumed to *occur at the surface elevation using digital elevation models (ie, meters above sea level). This can be done with any gridded data set (each grid is just moved vertically up according to the DEM). Second, in addition to this, emissions can be located at where they occur (e.g., smoke stacks, aircraft, etc). The discussion seems to imply the second. Is the first also important, should they both be included, etc? It would be worth mentioning this differentiation and explain what is required.*

Your first point is already routinely incorporated into models via DEMs as is already mentioned in the text: “not solely at the surface as is common today”. This section concentrates its discussion on your second point: emissions above the surface of the Earth.

50. Fig 7a. It is useful to show the spread in uncertainty. Though, this could be shown in Fig 3 and remove fig 7a? In particular, it would be interesting to show the 95% band on the global and the Kyoto figures as it might visually show the effect of the uncertainty in non-Annex I countries.

The series of Fig. 3a-h specifically does not include uncertainties as for many of these figures, they have not been specifically calculated. One uncertainty estimate for the global total has been shown in Fig. 7a. This may be of surprise in the larger global carbon cycle community where FFCO2 emissions are often assumed to have zero uncertainty associated with them.

51. Page 1341, line 14. I did not see this spline in the figure?

The two curves shown in Fig. 6b are the splines.

52. Page 1343, line 2 and Fig 7b. What is IER?

IER is the Institute of Energy Economics and the Rational Use of Energy. The full name is now written out the first time IER is used in the manuscript to be published in BG.

53. Page 1344+, Conclusion. The conclusion drifts a little of topic, for example, discussing weather and climate models and impacts on agriculture.

The conclusions section consists of three paragraphs. The first two contain the primary and secondary conclusions of the manuscript and flow directly from the proceeding text. The third paragraph is expansive in that it points to a potential future for FFCO2 emissions research. As funding for FFCO2 emissions research is limited, we have focused on potential synergies that may allow new frontiers in FFCO2 research to be reached.

54. Figures. I suggest to remove the symbols and just have lines.

We have addressed this in #6 above.