Notes: Line numbers refer to the original manuscript, and may have changed in formatting.

The answers to the questions raised by the referee #2 by T. Oda and S. Maksyutov

We would like to thank the referee #2 for the comments and questions to our manuscript. Prior to the answers for the specific comments, we would like to address several key issues here.

The focus of this study: In this paper, we presented a method to disaggregate national and regional emissions we prepared using the BP statistical review (discussed more later), and then compared our resulting gridded inventory with existing ones which were constructed using other disaggregation methods. In the comparison (shown in Table 3, discussed later), we show an improvement in disaggregation of national emissions when compared to others.
As pointed out, our resulting gridded inventory lacks several points which are usually considered in a good global inventory such as CDIAC. This is mainly because we used the BP statistical review as a single data source for preparing emission estimates to be distributed (will be discussed more later). Those lacking (e.g. cement production, gas flaring and international bunker) could be supplemented in the ways partly described in the manuscript (see P16312, L21).
Regardless of these shortcomings, we think however, that the comparison clearly showed our improvement. We have compared gridded inventories, but intended to primarily compare methodologies, which is our focus in this study. The comparison fairly showed our better agreement with Vulcan and suggested our method would be beneficial for constructing a gridded emission inventory for regional inversions. It is difficult to fully quantify the errors in extending CARMA and associated with our methodology due to the data availability, we think however the inventory constructed via disaggregation, which is as an approximate of true CO_2 emission distribution (though not indicating exact "true" distribution), would be useful for atmospheric modeling purpose (e.g. P16308, L3; P16310, L5).

CO2 emissions: We prepared estimates of national and regional CO_2 emissions using 2008 year edition of the BP statistical review (In the manuscript, the BP statistical review was cited as BP (2007), but it was actually BP (2008). This will be amended in the revised manuscript). The emission numbers we used in this paper were calculated according to the procedure described in the section 2, and are not the ones cited from the original BP statistical review. We would like to mention that CO_2 emission numbers were not presented in the version we used, though current BP statistical review includes CO_2 emissions. Unfortunately the link to the data to the current statistics is the same as that of previous one and we do not have access to the 2008 version statistics anymore. Also, we would like to mention that we do not intend to put too much emphasis on emission numbers we used because the main focus of this paper is the method to distribute national and regional emissions.
In the revised manuscript, we will add more text to clarify how the numbers we used are obtained, and put emphasis on the fact that emission estimates are based on the BP statistical review (not like just "our"). But we still think that emission numbers we used should be kept in the manuscript and it would be of interest for the audience of this paper, as they cannot find those numbers in original BP statistical review. We also think giving a quantitative measure via comparison would be also useful to our audience (especially, for users of other inventories such as CDIAC), as we used national and regional emission numbers and trends for our development. Because of these

reasons, we think the comparisons of global and national emissions should be kept with modifications (will be discussed later).

The use of BP statistics: The use of the BP statistical review (as a source for emission estimates) posed some limitations to this study. First, we cannot follow common definition of fossil fuel CO_2 emissions (as you can see P16312, P13). The BP statistical review includes fuel consumption from international bunkers and biofuels, which are not included in the land emissions, for instance. Second, we cannot put error bars to our BP-based estimates because uncertainties in the BP statistics are not provided.
In the revised manuscript, we will add more text to describe those limitations according to your suggestions. In the comparison of global and national emissions, we will simply compare our BP-based emission numbers with CDIAC using CDIAC error bounds taken from literatures. EDGAR and IEA will be removed from the comparison and associated sentences in the manuscript.

Comparison with other existing inventories using Vulcan: Our conclusion supported by the comparison shown in Table 3. As described earlier, in the comparison, we evaluated our methodology by comparing the resulting inventory among exiting ones. In this comparison, we need a reference inventory that is not based on the disaggregation of national and regional emissions to conduct a fair comparison of disaggregation methods. As far as the authors' are concerned, Vulcan is one of the most ideal bottom-up inventories, though it covers only US and North America for the year 2002. Vulcan is based on 10 km x 10 km collected statistics and usually such a detailed inventory is not available globally. Thus we defined the Vulcan as a true distribution (reference) in this comparison (the authors are aware that there are not measurements to verify true emission distribution). We think scaling in emission magnitude and spatial scale are also needed for a fair comparison. By scaling emission magnitude, we can sorely compare the spatial distributions regardless of differences in emission estimates. To compare our inventory with Brenkert et al. (1998), which is a common fossil fuel CO_2 inventory for inversions, we need to implement scaling in space as it is 0.5 degree resolution. We agree that it would be of interest to characterize the differences among nightlights-based inventories, however it would be difficult to draw conclusions which we would like to present in this paper (improved method for disaggregation and useful for inversions). See also the comment to referee #1.

Answers to specific comments are listed below:

Page 16312, line 15. *Emissions from cement production are specifically excluded. This is puzzling since good international statistics are available for such emissions and thus other CO_2 inventories commonly include cement. Why did you exclude it? Exclusion makes comparison with other inventories more difficult, as cement is approximately 4.5% of the global total fossil fuel CO_2 emissions in 2007.*

Good statistics for cement production such as USGS (2007) are available, however the data for their spatial distribution are not available. Emissions from cement production are often distributed together with other fossil fuel CO_2 emissions using population (e.g. Andres et al. 1999) and in the absence of the spatial data on cement production, in this study, we have to keep emissions from cement production in the aggregated non-point source land emissions, which are distributed using

nightlight proxy. The emissions could be supplemented simply using statistics such as USGS (2007), depending on the purpose of the use of the resulting inventory. However to distribute them more appropriately, appropriate data for the spatial distributions (something similar like CARMA database) are expected. In the comparison of global and national emissions, we excluded emissions from cement production (see P16320, L27).
The change we have made here will be shown as changes for *page 16312, lines 13-24.*

Page 16312, lines 15-18*. Bunker fuels are included in the non-point source land emissions estimates. Why? National emission estimates usually specifically exclude bunker fuels due to international agreement. Here, you reverse that agreement and put these sources over land masses (which may be appropriate in the case of some air travel, but certainly is not appropriate for sea travel). Inclusion makes comparison with other inventories more difficult as bunker fuels are approximately 3.2% of the global total fossil fuel* CO_2 *emissions in 2007. Inclusion of bunker fuels are also problematic as to their location using your methodology. They are not CARMA point sources so their emissions are distributed by the night light algorithm. Night lights are not related to aircraft flight routes. Thus, emissions from bunker fuels (both air and sea) are incorrectly apportioned to human settlement locations.*

Bunker fuels are included in the non-point land emission because fuel consumption of international bunker is included in the BP total fuel consumption (P16313, L18). The emissions could be supplemented using existing inventory such as EDGAR (see P16312, L15). In the revised manuscript, we will explicitly list the inclusion of bunker emissions in land emissions as a possible source of deviations from other emission estimates in our current resulting inventory.
The change we have made here will be shown as changes for *page 16312, lines 13-24.*

Page 16312, lines 18-20*. Why are gas flaring emissions specifically excluded? You state you can locate them with nightlights and you note the NOAA reference giving their magnitude. How are these different than emissions located and sized via the CARMA database? Exclusion makes comparison with other inventories more difficult as gas flaring is more than 0.5% of the global total fossil fuel* CO_2 *emissions in 2007.*

We excluded emissions from gas flaring because the BP statistical review do not include statistics of gas flaring. The emissions could be supplemented using existing NOAA gas flaring estimates that indicate emissions and distributions. We will include emissions from gas flaring in the future development, but we think the exclusion would not affect on the conclusion in this study.

We have changed "(p.16312, L13-L24) Herein, the terms fossil fuel (and anthropogenic) CO_2 emissions refer to emissions over land, which are attributable to the combustion of fossil fuels (coal, oil, and natural gas). Non-land fossil fuel CO_2 emissions from sources such as international bunkers (marine and aviation) and fisheries, which are usually not correlated with the nightlight distribution, were included in the land emissions estimates. Although gas flare emissions could be pinpointed using the nightlight data, they were incorporated into the land emissions because it was not appropriate to distribute those distinct emissions together with other aggregated land emissions. The emissions that were not considered herein may be introduced using supplemental existing inventories, such as EDGAR v4.0 (EC-JRC/PBL, 2009) for international bunkers and global gas flare estimates by the U.S. National Oceanic and Atmosphere Administration (NOAA) (http://www.ngdc.noaa.gov/dmsp/interest/gas flares.html)."

to:

"Herein, the terms fossil fuel (and anthropogenic) CO_2 emissions refer to emissions over land, which are attributable to the combustion of fossil fuels (coal, oil, and natural gas). Due to the data availability, the use of an energy statistics as a data source for estimation of emissions and limitations of our method, the definition of fossil fuel CO_2 emissions in this study differs from a common definition in several ways (e.g. cement production, international bunkers and gas flares). Emissions from cement production, which are commonly included in fossil fuel CO_2 emissions, are not considered in this study. This is because data for the global spatial distributions of the emissions are not available while good emission estimates such as USGS (2007) are available. Non-land fossil fuel CO_2 emissions from sources such as international bunkers (marine and aviation) and fisheries and were included in the land emission estimates, as individual fuel consumption statistics are not available in BP (2008) which is the energy statistics we used for estimating emissions. Thus, we simply kept those emissions in aggregated land emissions. Emissions from gas flares are not included because the BP statistical review do not provide the mount of gas loss attributable to gas flares. The emissions that were not included herein could be introduced using supplemental existing inventories, such as USGS (2007) for cement production, EDGAR v4.0 (EC-JRC/PBL, 2009) for international bunkers and global gas flare estimates by the U.S. National Oceanic and Atmosphere Administration (NOAA) (http://www.ngdc.noaa.gov/dmsp/interest/gas flares.html) in the future development."

We have also changed "(p.16330 L5) Other fossil fuel emissions that were not considered in this study, such as gas flares, international bunkers, and fisheries, may need to be accounted for by including such inventories in a full description of fossil fuel CO_2 emissions."

to

"Other fossil fuel emissions that were not considered in this study, such as cement production, gas flares, international bunkers, and fisheries, may need to be accounted for by including such inventories in a full description of fossil fuel CO_2 emissions."

Page 16313, lines 11 and 15*. Table 2 is referred to before Table 1. Reverse table numbering here and in the tables themselves.*

This has been amended.

Page 16313, line 16*. 65 nations and regions -> 65 nations and region There is only one region (i.e., Hong Kong) in the table of 65 geographic entities.*

This has been amended.

Page 16313, lines 19-20. *Inclusion of fuel ethanol and biodiesel makes comparison with other inventories more difficult. Since the magnitude of these inclusions is not given it is difficult to assess their importance, presumably it is more important in some countries than in others. It is odd that these emissions are included in a fossil fuel inventory.*

Individual consumption statistics for fuel ethanol and biodiesel are not included in the BP statistical review. In the revised manuscript, we will mention that individual emissions from biofuels cannot be quantify in our study and the inclusion of biofuel in land emissions are one of sources of errors in our BP-based estimates of fossil fuel CO_2 emissions.

We have changed "(p.16313, L19) Consumption of fuel ethanol and biodiesel were also included."

To:

"Consumption of fuel ethanol and biodiesel were also included in the oil statistics. As the individual consumption of fuel ethanol and biodiesel were not provided in BP(2008), emission estimates in this study include emissions from the combustion of fuel ethanol and biodiesel, which are not included in common definition of fossil fuel CO_2 emissions. The inclusion may cause overestimation in national emission estimates compared to other studies over some countries and regions"

Page 16314, lines 25-29. *The discussion of pre-1985 emissions from countries of the former Soviet Union is puzzling. The breakup was not until 1991 and thus emissions from these countries (in an annual accounting scheme) do not occur until 1992. Please clarify what your methodology is here.*

We extended emissions of Former Soviet Union countries even before 1991 to keep the consistency in the global emission estimates, which are sum of 65 national and regional emissions. The derivation of those national emissions are presented in P16313, L25. We will mention the assumption in the revised manuscript.

We have changed "(p.16314, L25) The consumption statistics for the eight former Soviet Union countries (Azerbaijan, Belarus, Kazakhstan, Lithuania, Russian Federation, Turkmenistan, Ukraine, Uzbekistan) prior to 1985 were unavailable, and, therefore, were extrapolated by scaling to the annual total consumption for the full former Soviet Union provided in BP (2007)."

to

"In the estimation of annual emissions in this study, we extended emissions of the eight former Soviet Union countries (Azerbaijan, Belarus, Kazakhstan, Lithuania, Russian Federation, Turkmenistan, Ukraine, Uzbekistan) prior to the year 1991 to keep the consistency in the global emission estimates, which are the sum of 65 national and regional emissions. The consumption statistics for the eight former Soviet Union countries prior to 1985 were unavailable, and, therefore, were extrapolated by scaling to the annual total consumption for the full former Soviet Union which were provided in BP (2008)."

Page 16315, lines 13-16. *The quality of the CARMA database can be debated, but I will not pursue that issue further here. My concern is that the 2007 CARMA data is assumed to be valid for the entire 1980-2007 time period. The advantage of using CARMA is that it gives point source locations in 2007. CARMA details nothing about point source locations in other years. This is a very large assumption as plant commissioning and decommissioning are completely ignored. This is not just a problem in developing countries where power plants are being constructed at a fast rate, but it is also a problem in developed countries where power plants are being constructed and deconstructed.*

The second major defect in relying on CARMA and the methodology presented here is that CARMA quantifies emissions for one year only. These emissions are then scaled by the annual BP data. This approach does not account for the addition or loss of boilers (e.g., at a power plant) or for boiler maintenance/repair/retrofitting which can involve considerable downtime of six months to a year or more. Thus, the national emissions scaling approach (based on annual BP data) can lead to very unfavorable results at the 1 km resolution of the resulting ODIAC inventory. Assuming no cross-national-border power sharing (which is not a good assumption for many/most large power consuming nations (e.g., North America, Europe, ...)) and even or increasing power consumption
(a good assumption for most regions of the world), then the ODIAC-apportioned emissions for a plant with an inoperative boiler are still apportioned to that boiler location when in reality those emissions are being produced by a boiler located somewhere else. With grid technology, that boiler could be tens to hundreds or more kilometers away. This is a major problem for an inventory aiming for one kilometer spatial resolution.

More text needs to be devoted to these deficiencies in the stated approach: 1. Incorrect point source locations, and 2. Incorrect point source emission magnitudes. Both of these deficiencies could be remedied by a more thorough methodological approach, assuming the data exist on point source locations and pont source emission magnitudes. From the submitted text, the authors have not pursued this level of detail.

As you correctly pointed out, we cannot correctly account for changes in locations and emission intensities for intervening years with the assumption. Authors are aware of the ramifications caused by the assumptions presented in this paper (see P16315, P14), and that is a current limitation of our method due to the data availability. As far as the authors are concerned, CARMA database is the only available power plant database that has both global coverage and is freely available. There is no other global power plant database which could supplement CARMA with data for other years, which are not included in current CARMA (e.g. *boiler maintenance/repair/retrofiting and construction/destruction*). This is a reason why we extended CARMA to prepare point source data for other years. In the revised manuscript, we will describe the possible errors posed by our assumption as pointed out, and will work on improvement when suitable data (which could supplement the assumption) is made available.

As mentioned earlier, this study presents a disaggregation method. And the authors are aware that the resulting inventory would be an approximate distribution of true CO_2 emission distribution. It is difficult to construct such a very high-resolution inventory without approximation because of the data availability, and that leads us to need for the disaggregation method. Regardless of the ramifications, we think that scaled CARMA is still useful to construct inventories for other years and the good agreement with Vulcan shown in Table 3 supports it. Also, the resulting inventory would be useful for atmospheric modeling (e.g. inverse modeling of CO_2) regardless of the ramifications mentioned above. We agree that we should work on improvement of resulting inventory, assuming the dataset users understand possible quality deterioration towards earlier part of inventory period.

We have changed earlier part of section 2.2 "(p.16315,L14-L26) The 17,668 power plants were assumed to be operational during this period, and their annual emission levels were simply scaled by the national (or regional) emission trends obtained from BP (2007)."

To

"The 17,668 power plants were assumed to be operational during this period, and their annual emission levels were simply scaled by the national (or regional) emission trends obtained from BP (2008). This assumption was taken because no other database to supplement power plant information such as location, intensities, commission years, operational situation (operation/maintenance and etc) for other years, as authors concerns."

Some of the suggested changes have mede in section 3.3. The changes we have made for section 3.3 are shown in the response to the referee #1 together with other changes (see the answer to referee #1 addressing section 3.3).

Page 16316, lines 1-3. *The spatial distribution is accurate for 2007 only. See above comment regarding other years. This sentence is misleading in regards to the entire 1980-2007 time period about which this manuscript is written.*

We will add "for the year 2007" to the sentences as:

"By making use of the CARMA database, the spatial features of the power plant distribution ***for the year 2007*** were directly included in our inventory."

Page 16317, lines 25-27. *For rural areas, eliminating gas flare pixels is reasonable and likely leads to small error for other emissions occurring in the same 1 km pixel. However, in urban areas, where gas flaring occurs, this elimination of pixels can quickly lead to large errors as other local sources are ignored (e.g., Los Angeles, Baku, ...).*

We thank the reviewer for bringing this to our attention. DMSP satellites have difficulties in distinguishing surface lights attributable to gas flaring over lit areas (e.g. cities) according to NOAA gas flaring estimates, and thus shapefiles for gas flaring, which we used for masking out gas flare pixels, do not include gas flare pixels in lit areas.

Page 16318, lines 11-12. *Three discussion points. First, this procedure assumes that radiance linearly scales with non-point source CO_2 emissions. The authors give no assurance/data that promotes this assumption. Second, the authors expand this correlation from one country (where one may correctly or incorrectly assume a single economy and climate) to multiple countries via the use of regions. Thus, for example, the authors assume that the same radiance- CO_2 emissions relationship seen in arid Libya is the same as seen in tropical Guinea (an African example) or the same radiance-CO_2 emissions relationship seen in Nordic, former Soviet Union Estonia is the same as seen in more southern, mountainous Albania (a European example). Third, the regional approach assumes the same point source-non point source division of energy consumption across a region. The authors acknowledge this later in the manuscript on page 16320, lines 8-10. The authors do not address if this is a good assumption (e.g., do Estonia and Albania have the same point source - non point source percentage?).*

Point 1: Population is a good proxy for human activities (hence CO_2 emissions) (see 16310, L9) and nightlights correlate with population (e.g. P16316, L28). Thus, the nightlight data could be expected to work as a proxy for CO_2 emissions like population.

We have added sentences describing our assumption and its expansion and its limitations to p.16317, L17 as

"In this study, we utilized the correlation between the calibrated radiance data and population, which is a common surrogate as mentioned earlier. Correlation between population and the calibrated radiance data have observed in the developed country (Elvidge et al., 1999) and we extended the relationship to whole world. The correlation however would be different over different countries and regions as previous study suggested (Raupach et al. 2009)."

Point 2: A previous work suggested that correlation would not be uniform across different countries (see P16311, L9, p.16328, L17-). The assumption we used would be useful for developed countries (high correlation areas), it however would be weak over developing countries and countries in economic transition (low correlation areas) as described in the manuscript. Quantitative assessment of the nightlight- CO_2 emissions over different countries will be conducted in future study when appropriate gridded inventories (ideally, based on bottom-up procedure) for comparison become available. As far as authors concern, fine-grained bottom-up inventories are currently available over industrialized countries where our assumption would work reasonably (e.g. US, Europe and Japan).

Point 3: This is obviously a weak approach and this is due to the use of BP statistics review. The portions accounted by point sources and non-point sources would not be the same across countries included in the geographical regions, and thus regional approach would cause errors in the resulting inventory. We will add text to mention the limitation posed by our regional approach in the revised manuscript.

We have added

"(p.16320, L10 after "non-point sources".) This assumption is obviously weak coming from the use of BP (2008) for preparing emission estimates. Regardless of the weak assumption, the fraction of total emissions from point sources appeared to be smaller than the fraction of emissions from point sources in 61 countries and regions. "

Page 16318, lines 12-16. *How did you resolve conflicts between the national point source data and the 5 km political unit database you used. This will cause problems where a power plant of one nation is sited in the land area of another due to the mismatch in databases used. Example potential problem areas include El Paso, Texas, USA (next to Mexico); Buda/Pest of the former Czechoslovakia; Elat, Israel (where the power plant could be in Israel, Jordan, Egypt, or the Red Sea). A CARMA power plant of one country placed in the grid cell of another country (by GPWv3) will have ramifications for both the point source CO_2 and the non-point source CO_2.*

We placed CARMA power plants to exact points (see P16315, L11) and we haven't used national boundary data for locating point source emissions. National boundary data were only used for normalizing nightight intensities for each grid by total sum of nightlight numbers for each country and region. We think situations you mentioned would not be a critical problem as long as the national and regional total emissions are conserved in this study (assuming locations in CARMA are correct.), considering our focus in this study (create an approximation of true CO_2 emission distribution). As you pointed out, we might not be able to properly consider emissions from grid systems and we agree that that is a source of error in this method.

Page 16318, line 25 - page 16319, line 2. *Was point source and non-point CO_2 apportionment done on the one km data and the results then aggregated to five kilometer or was the apportionment done on five kilometer data? I suspect it was the former, but the text is unclear on this point. The order of apportionment and aggregation has an effect on the radiance-CO_2 emissions relationship used for a specific country/region. I assume CO_2 emitted from a point source located in a water grid cell was kept in the database and the grid cell changed to land ownership of the appropriate country. Is this correct? Or were these point sources deleted as bad locations as indicated elsewhere in the manuscript?*

The 5 km x 5 km gridded inventory was constructed using 5 km x 5 km resolution nightlight data according to the method presented in this study (see p.16318, L28). We did not aggregate 1 km x 1 km data to 5 km x 5 km.

We changed "(p.16318, L25) For convenience in data handling, we developed a 2.5 arc minute (5 km) low-resolution inventory. The analysis described in this paper was obtained from the low-resolution inventory dataset unless stated otherwise. The low-resolution inventory was developed using 2.5 arc degree nightlight data reduced from the original 30 arc second (1 km) dataset."

to

“For convenience in data handling, we developed a 2.5 arc minute (5 km) low-resolution inventory. The low-resolution inventory was also developed according to the method presented in this paper using 2.5 arc degree nightlight data reduced from the original 30 arc second (1 km) dataset. The analysis described in this paper was obtained from the low-resolution inventory dataset unless stated otherwise.”

As you correctly mentioned, CARMA power plants in water grid cells are kept and their emissions are included in non-point emissions. And we found that we did not devote much text to describe the selection criteria for CARMA power plants (see P16315, L10) and problems we found in their location (P16328, L6). We will describe the procedure mentioned here in the revised manuscript.

We changed “(p.16315, L9) Data for the fossil fuel-fired power plants with valid location information (n = 17,668) were selected from the database, and the values for the national total emissions from such power plants were calculated.”

to:

“(p.16315, L9) Data for the fossil fuel-fired power plants (emission > 0) with valid location information (n = 17,668) were selected from the database, and the values for the national total emissions from such power plants were calculated. CARMA power plant emissions located in water grid cells were reviewed using Google Earth (http://ear th.google.com/) and if the locations could not be confirmed, the emissions were included in non-point emissions if they’re invalid.

p.16328, L9 URL of Google Earth were removed.

Elsewhere in the manuscript, you compare geographic entities from different inventory compilers. In aggregating from one kilometer to five kilometers you had to make decisions about borders. One decision would be about how to handle the border between two or more countries that share the same five kilometer grid cell. Was the resulting five kilometer grid cell apportioned proportionately to each nation (e.g., 20% nation a, 30% nation b, and 50% nation c)? Was the resulting five kilometer grid cell tallied to one nation according to dominant land area or some other criteria (thus incorporating the carbon from one nation into another)? Another decision would be regarding five kilometer grid cells containing carbon-emitting-land and water. Was carbon conserved and the resulting five kilometer grid cell allocated to a given nation? Was carbon not conserved? What if only one cell of the 25 cells (five one-km cells on a side) was land and it emitted carbon? Was carbon conserved or not conserved? These border and conservation questions need to be considered when making comparisons across different inventories.

We did not aggregate 1 km x 1 km to 5 km x 5 km as mentioned before.

Page 16319, lines 10-11. *You cite the Gregg et al. (2007) best estimate, but ignore the error bars Gregg et al. provided. Considering errors, the Gregg et al. (2007) estimate overlaps the ODIAC* *estimate and the two are indistinguishable (without even considering ODIAC error bars (which are not provided in the manuscript)).*

We have removed the sentences p.16319, L10-L14 as the emission estimates are not the focus of this study.
We will add error bounds to CDIAC estimates in the comparison (see the comment to *Page 16341, Figure 4*).

Page 16319, lines 5-17. *I do not understand why this paragraph is included in this manuscript. The numbers cited are not from new ODIAC calculations, but rather are based on BP estimates. At the global level, all ODIAC has done is taken the BP estimates, subdivided them into point and non-point sources, and plotted the resulting data. The global totals are unaffected by the ODIAC procedures. This paragraph, as presented, can be deleted.*

We would like to keep this sentences. see “the use of BP statistical review” in the beginning.

Page 16319, line 21. *There is no “our” global total - see comment above. Replace “our” with “BP’.*

We will use “our BP-based”. Also, see “the use of BP statistical review” in the beginning.

Page 16319, line 22*. What makes CARMA data “invalid”? A listing would be useful (e.g., incorrect latitude-longitude coordinates, zero emissions, ...).*

We exclude CARMA power plants with incorrect coordinate information (latitude, longitude) or/and zero emissions. We will list the criteria mentioned above in the revised manuscript.

We changed “(p.16319, L21) Here, we note that the emissions from all power plant data available in CARMA were not summed, because invalid power plant data were eliminated at an earlier stage of the analysis.”

to:

“Here, we note that the emissions from all power plant data available in CARMA were not summed, because invalid power plant data (e.g. zero emissions and incorrect latitude-longitude coordinate information) were eliminated at an earlier stage of the analysis.”

Page 16319, line 26*. Australia is 55.6% by Table 2.*

This has been amended.

Page 16320, line 4. *for determining the spatial emission patterns -> for determining part of the spatial emission patterns*

This has been amended.

page 16320, line 17. *our -> BP (also page 16321, line 6). But who created the data is not really the issue here. This manuscript is about the ODIAC methodology which is an emissions distribution methodology not an emissions magnitude methodology. Comparison of BP and other inventories at global or national scales is not a central concern. Comparison of ODIAC and other inventory distributions at the grid cell level is of central concern. This paragraph would support such grid cell level analysis. It does not support country level analysis (e.g., Figure 4) and if that is all it is used for, it should be deleted.*

We will remove descriptions about IEA and EDGAR as we will not use them for the comparisons presented in the revised manuscript. We will keep text describing about CDIAC and we will use CDIAC for the comparison, which will be mentioned later. The change we have made here will be shown as changes for p.16320, L16 – p.16322, L17 later.

Page 16320, line 19. *Figure 4 is mentioned in the manuscript before Figure 3. Renumber here and in the figure captions themselves.*

This has been amended.

Page 16321, line 17-28. *At a global scale, this study is really BP estimates. There is no 'this study", thus words like "this study" and "our" should be replaced with BP or something similar. Again, I think the authors are missing the point. ODIAC is a distribution methodology. Comparisons at a global level eliminate the need for ODIAC altogether as there is no distribution of emissions. This paragraph can be deleted.*

We will put emphasis on BP. Also, see "the use of BP statistical review" in the beginning.

Page 16321, line 29 - page 16322, line 17. *Due to anomalies introduced by borders, apportionment, and aggregation, "This study" may in fact be different than BP data. However, again, national totals are not the concern of ODIAC. Apart, from the border issues, ODIAC is about individual grid cells or even perhaps multiple grid cells. But, the multiples are at a scale less than national. At national, we are back to BP versus other inventories. This paragraph, as written, can be deleted. Of the last three paragraphs, the details of the various inventories mentioned will be important at grid cell or multiple grid cell scales. These details should be kept in the manuscript, just presented alongside grid cell (not national or global) information.*

As presented above, we have removed descriptions about IEA and EDGAR as we will not use them for the comparisons presented in the revised manuscript. The change we have made here will be shown as changes for p.16320, L16 – p.16322, L17 later.

Page 16325, lines 13-14. *See below comment for Page 16337, Table 3. ODIAC should be the comparator, not Vulcan. Various sentences in this paragraph need to be rewritten to reflect the suggested changes (e.g., scaling of emissions and absolute difference definition).*

We will include the comparison in the revised manuscript, because of the reason mentioned in "Comparison with other existing inventories using Vulcan" in the beginning of this response.

Page 16326, line 7. *I suggest adding sentences to this paragraph something akin to "Although ODIAC and Vulcan showed the highest correlation, this should not be construed that the true distribution is similar to either of these distributions. Both are products of particular distribution algorithms and are not products of independent measurements. The authors are aware of no independent measurements to verify any of the inventories mentioned here."*

Vulcan is a bottom-up type inventory and not a result of disaggregation of national emissions.
We will add *as:*

"Although ODIAC showed better agreement with Vulcan among participant inventories, this should not be construed to mean that the true distribution is similar to either of these distributions. The authors are aware of no independent measurements to verify any of the inventories mentioned here."

Page 16329, lines 15-19. *Again, emphasize ODIAC, not Vulcan.*

We think the comparison using Vulcan as a reference is fair considering our focus in this paper, because of the reason mentioned in "Comparison with other existing inventories using Vulcan" in the beginning.

Page 16336, Table 2. *(Mt CO_2/yr) in the table heading is redundant with the table caption. It can be removed here and thus save a line of publishing space.*

This has been amended.

Code GBR should have the country name Great Britain, not just Great. Likewise Saudi Arabia, not just Saudi; New Zealand, not just New; and Czech Republic, not just Czech.

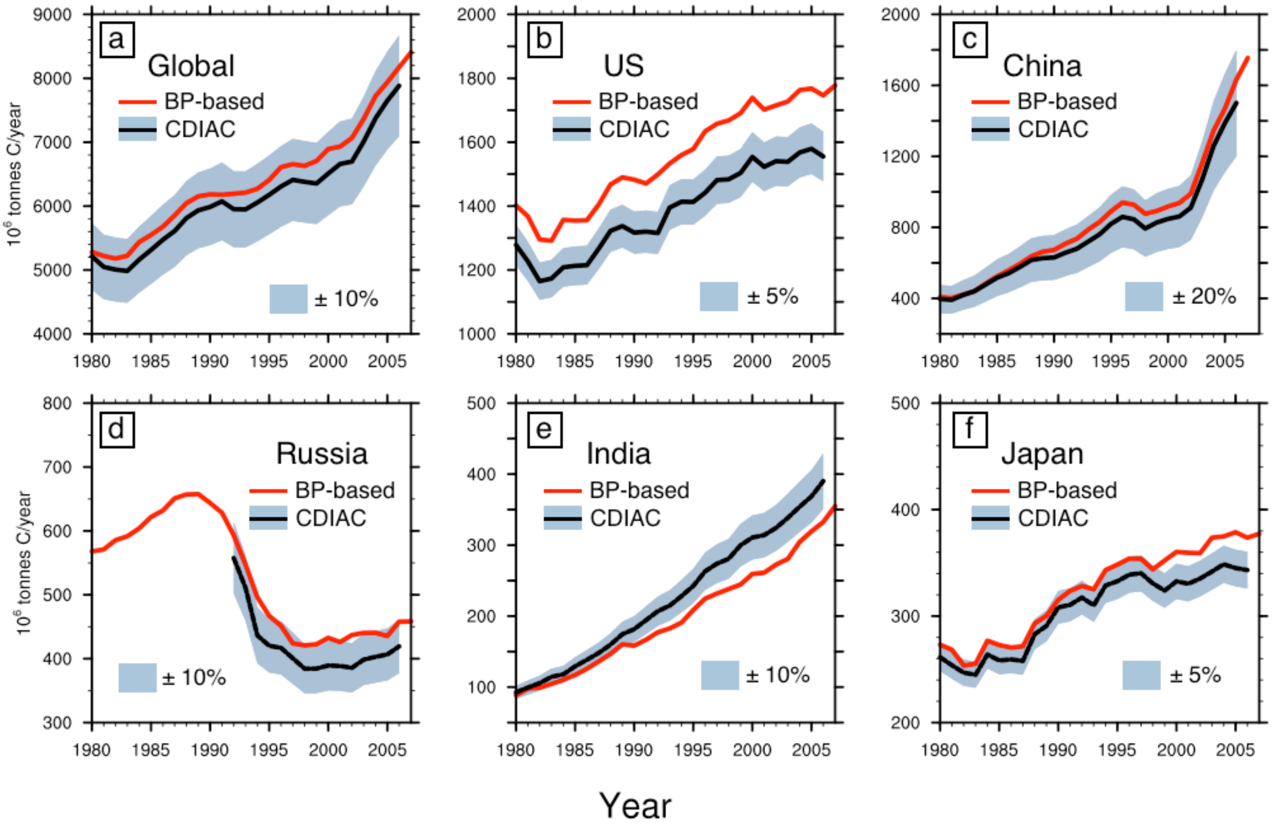
This has been amended.

Page 16337, Table 3. *I think it is a mistake to make these comparisons relative to Vulcan. Using Vulcan as the common comparisons has two disadvantages: 1. It confers, intentionally or unintentionally, upon Vulcan a sense of status as being the correct distribution and inventor y. This is far from proven in either magnitude or distribution. Additionally, Vulcan is limited in spatial extent (continental U.S. only) and temporal extent (one year only). ODIAC is much more expansive in spatial (i.e., global) and temporal (i.e., 1980-2007) extent. 2. It detracts from the ODIAC product being discussed in this manuscript. This manuscript is about a new distribution. Comparisons should be made between these other distributions and ODIAC. As a reader of an ODIAC manuscript, I do not ODIAC.*

We would like to keep the comparison in the revised manuscript, because of the reason mentioned in "Comparison with other existing inventories using Vulcan" in the beginning.

Page 16340, Figure 3. *The accumulation of global totals between "This study" and the other inventories/distributions is not very meaningful for two reasons: 1. This study is really BP data or a close modification of it subject to the gridding of the ODIAC procedure (see comments above regarding borders, aggregation, and apportionment). 2. No error bars are provided on any of the data sets which does not allow for an assessment if the data are statistically significant from each other.*

We think comparison of emissions should be kept in the manuscript and would be useful information for audience, because we used emission numbers we obtained and scaled historical emissions using the numbers (also, see "CO_2 emissions" in the beginning). We agree that comparisons with some exiting inventories is difficult because our emission estimates lack several components. In the revised manuscript, we will exclude EDGAR and IEA and compare our BP-based emission numbers with CDIAC to give a measure to our emission numbers. We will add error bounds to CDIAC using literature values. We will also remove sentences describing comparisons with IEA and EDGAR, and keep ones describing comparison with CDIAC. We will mention that our mean values could be within error bounds of CDIAC estimates for global. As for national numbers, we will keep the same sentences describing the sources of errors.
The revised figure 3 is shown below. The size of error bound was taken from literature values and we would like to note that they are not always ones estimated for CDIAC and typical values in general. We assigned 10 % for global (Marland and Rotty, 1984; Marland, 2008), 5 % for US (EPA, 2010) and for Japan, assuming it is a country with good statistic collection system, 20 % for China (Gregg et al 2007), and 10 % for Russia and India, assuming them as the same level of OECD countries (Olivier and Peters, 2002). We took largest numbers we found in the literatures. The change we have made here will be shown as changes for p.16320, L16 – p.16322, L17 later.



Revised Figure 3.

Page 16341, Figure 4. *The accumulation of national totals between "This study" and the other inventories/distributions is not very meaningful for two reasons: 1. This study is really BP data or a close modification of it subject to the gridding of the ODIAC procedure (see comments above regarding borders, aggregation, and apportionment). 2. No error bars are provided on any of the data sets which does not allow for an assessment if the data are statistically significant from each other.*

Comparisons shown in Fig 4 will be included in Fig 3 with modifications, as mentioned above.

We have changed p.16320, L16 – p.16322, L17:

"To give a measure to the BP-based emission estimates in this study, we compared our global and national estimates with CDIAC estimates (Boden et al., 2009) (Figure 3). The data, calculation methods, and CEFs we used differed from those in Boden et al., (2009), and thus deviations from CDIAC estimates were expected. Briefly, national total emissions from CDIAC were estimated using the apparent consumption distribution and were based on energy statistics published by the United Nations (U.N., 2008) (e.g. Marland and Rotty, 1984). On the other hand, global total emissions from CDIAC were estimated using production statistics based on U.N. (2008) (e.g. Marland and Rotty, 1984). CDIAC estimates for the comparison are the summation of emissions from the combustion of fuels (gas, liquid, and solid) and gas flaring; emissions from cement production are not included. The size of error bound was taken from literature values and we would like to note that they are not always ones estimated for CDIAC emission estimates and typical values in general. We assigned 10 % for global (Marland and Rotty, 1984; Marland, 2008), 5 % for

US (EPA, 2010) and for Japan, assuming it is a country with good statistic collection system, and 20 % for China (Gregg et al 2007). As for Russia and India, we assigned 10 %, which is an error estimate for OECD countries (Olivier and Peters, 2002), assuming the two countries have the good statistic collecting system as OECD countries. We took largest numbers we found in the literatures. As shown in Figure 3a, the global total emissions in this study overestimated CDIAC estimates. Our estimates were on average 4% higher than the CDIAC estimates in mean value, but agreed in error bounds. Comparisons of historical emission estimates for the top five emitting countries (the U.S., China, Russia, India, and Japan) are also shown in Figure 3b-f. The annual national emission trends in this study agreed well with those of CDIAC, although quantitative differences between the annual emissions were present. At the national level, in particular, the deviations from CDIAC may be more apparent than at the global emission level, because the data, calculation methods, and emission factors used for the CDIAC estimates were usually country-specific. The calculation method in the CDIAC estimate was based on apparent consumption, as described, whereas the national emissions in this study were derived from the annual total fuel consumption. Thus, deviations from the CDIAC estimate at the national level may be explained by, for example, the omission of adjustments for import/export and stock change and also the inclusion of international bunker emissions."

Page 16344, line 1*. ear -> year*

This has been amended.

Additional changes

In addtion to the changes shown above, we would like to add changes listed below:

The reference of Rayner et al. (2010) has updated as

Rayner, P. J., Raupach, M. R., Paget, M., Peylin, P., and Koffi, E.: A new global gridded data set of CO_2 emissions from fossil fuel combustion: Methodology and evaluation, *J. Geophys. Res.*, 115, D19306, doi:10.1029/2009JD013439, 2010.

We would like to add a citation for satellie-observed CO_2 data as:

"(p,16310, L1) CO_2 concentration data are available from the Atmospheric Infrared Sounder (AIRS) satellite (e.g. Strow and Hannon, 2008), *SCIAMACHY* (SCanning Imaging Absorption spectroMeter for Atmospheric CHartographY) *onboard the Environmental satellite (Envisat) (e.g. Schneising et al., 2008)* and the Japanese Greenhouse Gases Observing SATellite (GOSAT) (e.g. Yokota et al., 2009).

References (not listed in the manuscript)

EPA (2010) *Inventory of United States Greenhouse Gas Emissions and Sinks: 1990-2008*, EPA 430-R-10-006, Annex 7. http://www.epa.gov/climatechange/emissions/downloads10/US-GHG-Inventory-2010-Annex-7-Uncertainty.pdf

Marland, G. (2008), Uncertainties in Accounting for CO_2 From Fossil Fuels. Journal of Industrial Ecology, 12: 136–139. doi: 10.1111/j.1530-9290.2008.00014.x

Olivier, J. G. J. and J. A. H. W. Peters. 2002 Uncertainties in global, regional, and national emissions inventories. In Non-CO_2 greenhouse gases: Scientific understanding, control options and policy aspects, edited by J. Van Ham, A. P. M. Baede, R. Guicherit, and J. F. G. M. WIlliams-Jacobse, New York: Springer

Schneising, O., Buchwitz, M., Burrows, J. P., Bovensmann, H., Reuter, M., Notholt, J., Macatangay, R., and Warneke, T., Three years of greenhouse gas column-averaged dry air mole fractions retrieved from satellite - Part 1: Carbon dioxide, Atmos. Chem. Phys., 8, 3827-3853, 2008.

USGS (2010) *Minerals Yearbook - Cement* H.G. van Oss (Ed.), U.S. Department of the Interior, U.S. Geological Survey, Reston, Virginia. http://minerals.usgs.gov/minerals/pubs/commodity/cement/index.html