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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "A very high-resolution global fossil fuel $CO_2$ emission inventory derived using a point source database and satellite observations of nighttime lights, 1980–2007" *by* T. Oda and S. Maksyutov

**Anonymous Referee #2**

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This manuscript explores a mathematical method to distribute an existing carbon inventory on a global grid. It does this through a two step process. First, point source emissions are quantified and located. Second, non-point source emissions are distributed via a night lights proxy which provides both quantification and location.

The scientific significance of this manuscript is that it provides an alternative distribution methodology for carbon emissions. Unfortunately, independent measurements of the true value of the variable being distributed are lacking. Thus, evaluation of the distribution methodology is difficult due to lack of an absolute comparator. Overall, the

significance is rated as good.

The scientific quality of the work is fair. The quality suffers for two main reasons: 1. Extrapolation errors associated with the CARMA database, and 2. Errors in the methodology are not quantified. Thus, comparisons with existing distribution methodologies are hampered.

The presentation quality of the manuscript is good. Suggestions for improvement are included below.

Detailed comments keyed to the manuscript:

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 CO2 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 CO2 emissions in 2007.

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 CO2 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.

page 16312, lines 18-20. Why are gas flaring emissions specifically excluded? You state you can locate them with night lights and you note the NOAA reference giving



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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 CO2 emissions in 2007.

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

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.

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.

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.

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



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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.

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.

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, ...).

Page 16318, lines 11-12. Three discussion points. First, this procedure assumes that



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radiance linearly scales with non-point source CO2 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-CO2 emissions relationship seen in arid Libya is the same as seen in tropical Guinea (an African example) or the same radiance-CO2 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?).

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 CO2 and the non-point source CO2.

Page 16318, line 25 - page 16319, line 2. Was point source and non-point CO2 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-CO2 emissions relationship used for a specific country/region.

I assume CO2 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



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in the manuscript?

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.

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)).

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.

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



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

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

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

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.

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

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.

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.



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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.

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).

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."

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

Page 16336, Table 2. (Mt CO2/yr) in the table heading is redundant with the table caption. It can be removed here and thus save a line of publishing space.

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.

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 inventory. 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



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between these other distributions and ODIAC. As a reader of an ODIAC manuscript, I do not care how FFDAS compares to Vulcan, I care about how FFDAS compares to ODIAC.

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.

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.

Page 16344, line 1. ear -> year

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