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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of global inventories of monthly CO emissions derived from remotely sensed data" *by* D. Stroppiana et al.

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We have revised the manuscript following the comments of anonymous referee#2; we acknowledge the referee for his/her work on the manuscript. Below we address the general and specific comments.

Referee #2-general comment. The paper by Stroppiana et al. is a good paper that attempts to shed light of the accuracy of global inventories of monthly CO emisisons through a comparison of different products that are used to drive the emission inventories. It is a complex task because there are any differences in the methods used, the data sources utilised and the uncertainty associated with these data sources. The

results look encouraging, but a key limiting factor is that GFEDv2 is used as well as reference to a land cover data set that is not considered to be state of the art (GLC2000). The paper still does not address whether the derived results are correct as validated from an independent method. This is stated in the last sentence of the abstract. So what have we learned with this paper, it is not clear to me.

AUTHORS- The objective of our comparison is to provide to the users information on the differences and similarities among the inventories of CO emissions from biomass burning. As suggested we addressed more in depth the causes of the differences observed. A proper accuracy assessment is beyond the scope of our work. We would rather highlight the difference which can in turn influence the variability of the results of models of atmospheric composition. Following the comments of the referees we decided to process GFEDv3 to update our results. For what concerns GLC2000, we are aware that it might not be the state of the art for land cover distribution but we deem it suitable given the fact that our reference year is 2003. The Globcover land cover map and its updated version (Globcoverv2) were derived from MERIS images acquired later than our reference year (end of 2004 to 2006) (Arino et al., 2007). Despite the finer spatial resolution of the Globcover product (300 m), we chose GLC2000 because most of the inventories compared in our study were built with 1 km resolution satellite data. Finally, the parameters used to derived the VGT, ATSR and MODIS inventories were associated to the GLC2000 classes as described by Mieville et al. (2010) and as agreed by the partner of the INTERMEDE exercise in which framework this work took place (Liousse et al., 2008). In the text we clarified the reasons that led us to choose GLC2000 rather than Globcover although we are aware of the fact that it might be a better quality product for most applications (Page 5, lines 17-20).

Referee #2-1. The data and methods are not strictly independent of each other. For example MOPITT is couple with an active fire data set. How coupled?

AUTHORS- In our work we aim to provide the users with information on the impact of choosing one dataset over the others since there is no dataset widely accepted as the

most accurate one. Since this work is not intended as a rigorous accuracy assessment, we accepted to include also datasets which were not independent because the correlation does not influence the achievement of our goal. The inventories, which use the same source of fire information (i.e. ATSR active fires) are built with different methodologies and therefore lead to different temporal and spatial patterns of CO emissions. Moreover, with the inclusion of the GFED3 inventory, which is mainly derived from the 500-m daily burned area product (Giglio et al., 2009) and not from the MODIS active fires, we avoid the correlation which was existing before between GFED2 and MODIS. Since we agree with the referee that correlation is an important issue, we address it in the manuscript (Page 19, lines 22-26). Finally, we revised the description of the MOPITT inventory with specific attention to the use of fire information (Pages 7-8).

Referee #2-2. There are strict conditions of use for L3JRC (wrt seasonality). Has this been observed?

AUTHORS- It is true that the use of the L3JRC fire product should take into account fire seasonality. In the paper by Tansey et al. (2008) we read: "Furthermore, the validation does not tell us about the performance of the algorithm at time periods outside of the validation, for example, at high northern latitudes during the winter period. Corrections for the bias observed in the detection of burnt area with the low resolution instrument to burnt area estimates should be made within specified fire seasons and not applied across a full year. Furthermore, corrections for bias should be made with consideration of the land cover distribution." The same authors clarify that no validation has been done outside the fire season and corrections should be done only within the fire season. The duration of the fire season could be defined, separately for each region of the globe, to mask out those periods of the year when fires do not occur (e.g. winter periods at the northern latitude when the surface is covered by snow). However, this would not be suitable for the anomalous peaks of CO emissions observed from the VGT inventory in the Northern Hemisphere because the other inventories show low but not null emissions for the same period; hence we could not mask these months

out. Moreover, as far as we know, no correction factors are provided by Tansey et al. (2008) or by later publications; they rather seem to suggest a careful use of the product. We discuss this topic in the revised manuscript (Page 15, lines 7-9). The validation of the L3JRC dataset of burned areas highlighted that over two land cover classes in Africa the burned area estimates are systematically lower than estimates derived from Landsat TM images (Pereira and Barros, 2007). Therefore, Liousse et al. (2009) applied a correction factor of 2 and 1.67, respectively to the deciduous broadleaved tree (GLC03) and the deciduous shrub cover (GLC12) land cover classes of the GLC2000 (Page 6, lines 4-8). Finally, we also added to the conclusions that, given all the above reasons, VGT estimates should be used carefully (Page 20, lines 25-27).

Referee #2-3. Many beleive that the ATSR inventory is flawed as it collects its data at night when there are much fewer fires. What assumptions have been made? Is the data set reliable? I understand the WFA has recently been re-processed. Are the authors aware of this?

AUTHORS- We agree that the distribution of active fires given by the WFA is biased due to the fact that this product relies on night-time satellite images. We deepen the discuss on this topic throughout the manuscript. Note, however, the ATSR inventory does not use directly the active fire counts but it is build from the intercalibration of CO emissions between GBA2000 and WFA (Mieville et al., 2010). Our work aims to highlight any bias induced by the use of night-time active fires but we do not think that this product should be discarded because it is invalidated by the use of this source of fire information. We are not aware of a new operational WFA product. Some work has been done by ESA to improve the algorithm for hop spot detection (Casadio S., Arino O., ATSR-WFA New Algorithms for Hot Spot Detection, 2nd MERIS-AATSR workshop, September 22-26, ESRIN, ESA Publication, http://dup.esrin.esa.it/ionia/wfa/references/285casad.pdf) but the accuracy of the algorithms tested is not suitable for an operational use yet. Moreover, these new algorithms have been developed with the major objective of extending the WFA back to 1991,

therefore these changes might not affect our analyses which are carried out for 2003.

Referee #2-4. I'm not clear what the reference time period for the intercomparison? Is the time period sufficient?

AUTHORS- The comparison has been carried out for the year 2003 since it has shown an important fire activity in most regions of the globe. The referee comment is pertinent because this time period could be too short and does not highlight inter-annual differences between the inventories. However, at this stage of our work we thought that the use of a multi-year dataset would add complexity to the comparison exercise which is already performed in both space and time. We plan to extend our research in the future by repeating the comparison exercise for other years.

Referee #2-5. Are not MODIS and GFEDv2 based on very much the same data set? I it therefore no surprise that the results are similar and agreement is high?

AUTHORS- We agree with the referee, but we clarified that the correlation does affect the goal of our work. Moreover, the new GFED3 dataset is derived mainly from the MODIS daily burned area maps (Giglio et al., 2009; Giglio et al., 2010) and not from the active fire counts as the GFED2.

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