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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Historical (1850–2000) gridded anthropogenic and biomass burning emissions of reactive gases and aerosols: methodology and application" *by* J.-F. Lamarque et al.

Anonymous Referee #1

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I do not think this paper does an adequate job of documenting what will prove to be a very valuable emissions product. It is my opinion that it needs to be revised in a major way.

I would like to set some context for my review. The publishing of a unified set of historical emission estimates from 1850 to 2000 is a major milestone in the emission field. I have no doubts that this dataset will be jumped upon by chemical transport modelers and climate modelers for historical reconstructions. In fact, I have already seen several references to this paper, even though it has not yet passed the review stage.

Researchers desperately want to be able to reconstruct the atmosphere of the 20th century; climate modelers will eagerly take up this dataset and use it in temperature reconstructions and thence in justifications of man-made influences on climate. Do the authors of this paper realize the gravity of their undertaking? Any researcher in these fields now understands the extra demands placed on researchers to fully document their work if it is likely to be used for important climate-change applications. The furor that has erupted around the IPCC products in the past year is ample evidence of the demands now placed on the compilers of datasets that will be used for historical climate reconstructions. It is no longer adequate to respond that the dataset was only intended for a limited modeling study or for a single project. It will get out there and it will be used and it will be scrutinized.

I therefore ask: Does this paper do an adequate job of documenting the emission inventory? Will it stand up to future scrutiny as a foundation of historical atmospheric and climate studies? My answers are that it does not and it will not. One has only to set it alongside classic emission inventory papers like Bond et al 2004 to reveal its inadequacies. It has no emission values in it. It has no emission factors in it. It has no uncertainty estimates. It has no comparisons with other emission estimates. How can we then judge if it is any good or not? In my opinion it is not sufficient to say that this is just a conglomeration of other, published datasets: A community effort. That is a cop-out. The authors need to do a much better job of telling the reader what sources are and are not included, what are the assumptions about emission factors and activity levels over time, what are the emission values for each species in modern times, and how well do they agree with other estimates (e.g., national estimates like the U.S. NEI). We need to be convinced that this inventory is robust enough for the critical analyses to which it will be applied. Why do we not at least have a table of the 2000 emission values for all species and for key world regions that anchor the extrapolations back in time? Then we can judge for ourselves if the trends are biased.

My major recommendation is that this paper be split into two papers: one that does a proper job of documenting the emission inventory and one that applies the inventory in a modeling exercise.

My comments are restricted to the emission inventory parts of the paper.

Some examples of the paper's deficiencies at the detailed level:

(1) Page 4967, lines 24-26, are an unacceptable dismissal of the value of seasonal emission estimates. (Doesn't line 5 on the same page say that the target of the work is "to provide monthly emissions…"? Confusing.) Some of these species have large monthly variations in their emissions (BC, CO for sure), and these need to be taken into account if any comparison is to be done with observations; they will add to the seasonal variations in observations that result from transport variability. "no enough information of past emissions available" is not good English.

(2) The Introduction does not mention the approach to gridding that was taken.

(3) Page 4968, lines 6-10, why is Bond et al 2007 not mentioned here or Junker and Liousse? There are more historical emissions papers than are mentioned here. Perhaps this part is just not well written. What is the meaning of "…, or at least." (line 10)?

(4) Page 4968, line 19, should be "East Asia" and possibly "South Asia", but not "Southeast Asia". I think a rather poor case is made for choosing the base year of 2000, rather than 2005, say. There are ample emissions data in all parts of the world now to do 2005… The point is that emissions changed A LOT between 2000 and 2005.

(5) I think the argument made on Page 4969, lines 14-24, is flat out wrong, and it would have made a big difference if some of these other inventories had been included or at least compared. There should be no inference that these other inventories are somehow inadequateâĂŤin fact, a number of them are arguably better than what was chosen.

(6) Section 1.3, it is not clear to me if the methodology accounts for changing emission factors over the time period. Undoubtedly emission factors have changed radically for

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almost all source types, and are uniformly lower today than they once were. It seems to me that unless such changes were included in the original work, they are not included in this compendium. Yet, on page 4971, line 22-23, the text says changes in emission factors are captured. If so, how is this done? For all species? Can some examples be provided of emission factors assumed for key source types during the historical period?

(7) Page 4976, line 13, "(see Sect. 2)" cannot be correct, as this statement occurs in Section 2 itself. There is very little discussion of biofuel use, which is hugely important in the period 1850-1920 or so. How was biofuel use over time quantified and its emissions calculated? We need to see more data. . .

(8) Page 4987, lines 23 onward. The dismissal of uncertainty in this cursory way is disturbing. There have been many attempts to quantify uncertaintyâĂŤeven in the studies that are used hereâĂŤand it is a shame to see the topic given such limited attention. Many current estimates are much better than a factor of two and some are much worse. How do the authors think the uncertainty increases as we go back in time? So many unanswered questions! By the way, Smith et al. 2010, is unpublished and not a good citation here.

(9) Table 1. Can we identify the years of these cited studies (e.g., Bond et al, 2007)?

(10) Table 2. I find the term "Asia-Stan" objectionable.

(11) Table 3. Natural emissions are included? I am surprised, as I don't recall seeing any mention of natural emissions in the text.

(12) Fig 1 is confusing to me. First, are there no EDGAR data after 1980 on the left panel? Why is that? Second, the green EDGAR line appears to stretch to 1990 on the right panelâĂŤis that correct? Third, is the point on the right graph supposed to be the U.S. NEI value? This definitely should be explained on the graph. Fourth, avoid use of the term "actual" emissions. None of these values are actual emissions! Absolute values, perhaps. I don't feel very comfortable with this illustration. EDGAR

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and RETRO are quite dissimilar, especially in the recent past. They appear to be trending in radically opposite directions during 1970-2000. RETRO in particular seems to be extremely different in 2000 to what I assume is the NEI value and the starting point for the historical reconstruction. Perhaps some time should have been taken to probe the reasons for the differences in the datasets used. The more I look at this figure, the more uncomfortable I become. If you check EPA's emission trends reports for the transportation sector, you will find that CO emissions in 2000 are similar to what is shown on this graph, about 83 Tg. But the corresponding EPA value for 1980 (as far back as my version goes) is 146 Tg, far above any of the values in the right hand panel. Ouch.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 4963, 2010.