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Interactive comment on "A global anthropogenic emission inventory of atmospheric pollutants from sector- and fuel-specific sources (1970–2017): An application of the Community Emissions Data System (CEDS)" *by* Erin E. McDuffie et al.

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Review of essd-2020-103 "A global anthropogenic emission inventory of atmospheric pollutants from sector- and fuel-specific sources (1970–2017): An application of the Community Emissions Data System (CEDS)" by McDuffie et al.

The paper describes an interesting new global emission Inventory (CEDSGBD-MAPS)for atmospheric pollutants (1970 – 2017) based on the so-called mosaic approach based on the Community Emissions Data System (CEDS). The paper is generally well-

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written and deserves to be published but I do have several concerns where I would ask for adjustment or further explanation. A problem with emission inventory papers is that one tries to describe a complete set for all pollutants, all source sectors, all countries and many years. It is impossible to write a paper on this that documents, explains & discusses all and is still readable. Choices have to be made. The intention of my review is not to be a dictate. Part of my comments will relate to choices made and I do not demand that all answers to my comments find their way into the paper. If the authors have good reasons for not adjusting something, they can explain themselves.

The mosaic approach is not new and was previously successfully applied for example in the framework of HTAP by Janssens-maenhout et al (2015). This is an often used mosaic inventory. The approach by Janssens-maenhout et al differs from the approach taken in this paper and I think this should be briefly discussed in the introduction. Also to make clear that mosaic inventories are becoming a more frequently followed approach.

A more fundamental problem is the term “calibration inventory” that is coined in the paper. Calibration is the comparison of measurement values delivered by a device under test (or a system) with those of a calibration standard of known accuracy. However, I think it is fair to say that the authors don’t know the accuracy of their calibration inventories. They motivate that regional or national inventories may include more national/regional knowledge and are therefore more accurate. This was also the motivation for e.g. the earlier HTAP_v2.2 mosaic inventory. It may well be true (and this reviewer firmly believes in the usefulness of mosaic inventories) but a) we don’t know for sure if the regional inventory is really better and b) we don’t know how accurate exactly. Good enough for calibration? In my opinion the term calibration adds too much certainty to a more empirical and intuitive solution for an operational problem. It reads well but in reality it is more fitting or scaling than calibrating. In e.g. line 213 is also stated that scaling factors are calculated in the calibration procedure . Apparently scaling is seen as calibrating. Should the authors really think that calibration is still the best

terminology some additional clarification/disclaimer is needed to avoid “whitewashing” of something still uncertain (scaling) by calling it certain (calibrating). [see also the confusion created in line 360-365 between scaling and calibration and the remark that BC / OC are not scaled due to large uncertainties in EDGAR – but how well do you know that other inventories are much less uncertain?]

An advantage of the mosaic approach is the inclusion of more locally / nationally representative inventories in the global emission map. A disadvantage is that the emissions from different regions become apples and oranges. Obviously still the same species but the underlying choices are no longer necessary the same. It would be interesting for some of the more uncertain species like CO, NMVOC or BC to show a plot comparing some implied emission factors for certain source sectors for e.g. Africa, India, China, Former Soviet Union. What is the range in these implied EFs and based on expert judgement of the authors do these ranges seem plausible? This may be used to flag some of the pollutant / source sector / region combinations that may deserve further investigation in the future? It could also be connected to the paragraph starting at line 300.

From the methods section it was not clear to me where the shipping emissions come from. Are these based on AIS data or taken from EDGAR? Or another approach? Like with the regional inventories there may be ways to “scale/calibrate” these in recent years by using AIS based inventories. Was this considered?

The region “Other Asia/Pacific/Middle East region”. This I find non-informative and I invite the authors to think of a solution possibly by breaking it up. The mix of countries (see Table S8 - e.g. Australia, Mongolia, Yemen, Saudi Arabia, Korea, New Zealand, Pakistan, Indonesia etc.) is such that any discussion of the trends for this group in the paper is pointless. Also graphs of such a group in my opinion do not add any information.

Compliments to the authors for all the line plots, they are generally really good to read

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and intercompare and thereby also reveal some issues that appear unlikely to be correct. That does not mean they have to (or even can be) solved in the current paper. There are a few individual cases that draw attention and possibly merit more comments. I like to share them but it is also up to the authors to think about what they feel is justified. I am not advocating to make the paper very anecdotical by discussing every detail. Like the drop in OC emissions for Industry in Figure S6; the CO peak from road transport and SO2 peak for energy in fig S8 (the latter is discussed in the text) - My suspicion is that what such abrupt peaks or drops have in common is most likely a change in legislation or methodology that “on paper” has almost immediate effect but in reality is smeared out over a longer time. For example the car fleet cannot be changed in 1-2 years, cleaner fuels (like low sulphur) generally take years to be completely adopted. NMVOCs from the Energy sector appear a special case (Fig S5) with a very large contribution but little explanation is given other than that these are process emissions. NMVOCs in general draw some attention – e.g. in line 630 there is a discrepancy of possibly missing NMVOC emissions as CEDS has no agricultural NMVOC emission? And, for example Fig3 India NOx emissions – almost a factor 2 difference between 2 CEDS versions. It is commented on in the text but would it also imply it is better not to use the previous CEDS version because of these large deviations? The difference is too large for both to be equally plausible. This also applies to the discussion in line 568 and onward. As both inventories come from the CEDS team it seems logical to express some advice on to what extend you believe the new inventory replaces the old one. (Like the EDGAR team would advise to use v5 and v3 or v4.)

Line 648 – “decreasing uncertainties”: Here I do not by definition agree. If for example the (more uncertain) emissions from Africa and India become dominant and e.g. the more certain emissions from the US & EU go down, than the overall uncertainty might also increase in future years.

A good assessment of uncertainty from a mosaic inventory is very challenging and

simply stating that the uncertainty is similar to the other inventories (e.g. line 655) is an unsatisfactory answer. Moreover, there may also be considerable uncertainty in the spatial distribution. The authors, however, announce that in the near future a more robust uncertainty analysis is planned. And a much longer paper would not be helpful for the community. So separating this is an acceptable solution.

Additional suggestions for final discussion: Recently Huneeus et al. (2020) published an evaluation of emission inventories for South America which included EDGSAR, ECLIPSE and CEDS. It would be interesting to comment on how the new inventory presented here would have an impact on SA estimates and compares to the CEDS version used in that paper? Elguindi et al. (2020) recently published a paper on inter-comparison of bottom-up inventories and top-down emissions. This may well be the way forward to build more confidence in mosaic inventories and justify certain choices.

Small editorial remarks

Line 42 – from "waste" combustion (otherwise strange to have carb aerosol from waste.)

Line 78 – as inputs to solve for? Not clear to me, maybe reformulate slightly?

Line 108 – "emission" reduction of coal-fired etc.

Line 181 – explain the term “working sector”

Line 410 – you mean Section S4.

Line 481 Global emissions of NOx from waste "combustion".

Line 680 I don’t see how satellites will aid in fuel-type recognition.

Line 684 “emissions” – should be “uncertainties”?

Line 786 – but not for the latest years? And these will not be scaled (“calibrated”) so not consistent?

Line 791 – it seems the reference of (McDuffie et al., 2020c) here and in the ref list is redundant because this sis the dataset connected to the present paper? So won't the reference to that data not be simply this paper instead of (McDuffie et al., 2020c)

Line 836 in agricultural "NH3" emissions

Line 867 – what is fuel abatement?

There is an error in Table S8 – Other Asia includes Montenegro and I assume Chinese Taipei is Taiwan?

There is an error in Table S9 – in the column for EDGAR "solvent use" and "waste" are swapped.

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