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Interactive comment on "TNO_CAMS high resolution European emission inventory 2000–2014 for anthropogenic CO_2 and future years following two different pathways" *by* Hugo A. C. Denier van der Gon et al.

T. Oda (Referee)

tomohiro.oda@nasa.gov

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Dear Dr. Daniel van der Gon et al.

This manuscript presents the CO2 version of the TNO emission dataset (historic 2000-2014 and future 2018-2050). TNO_CAMS high resolution European CO2 emission inventory (hereafter, TNO-CO2) is based on disaggregation of the UNFCCC fuel-based emissions. The emission estimates are supplemented by the IIASA's GAINS model and the JRC/EDGAR emission dataset to cover the emission categories of the inter-

est. The authors also developed an emission dataset with the CIRCE projected future scenarios.

First of all, I must say this manuscript failed to deliver details of their dataset development. Also, the authors do not present any evaluation of the dataset, especially sectoral and spatial disaggregation. ESSD is a very unique journal. I believe this manuscript needs to help future data users to answer a simple question like "Which dataset should I use for my European CO2 simulations: TNO or EDGAR?". Future data users would not choose the TNO-CO2 just because of the higher spatial resolution. Future data users would need a good scientific justification. Emission datasets are very difficult to evaluate. I share this difficulty. What we often could do is just to compare emission datasets each other in terms of emission estimates and/or spatial distributions. Well... we all know such comparison would not tell you what is good or bad in an absolute sense. It is also very challenging to do a clean comparison as emission datasets are constructed differently. But characterizing inter-emission data differences would greatly help future data users to familiarize themselves with the TNO-CO2 to feel confident for their use. I believe it would be a very important piece of information in an article like this describing an emission dataset. I would also add that this manuscript is not self-explanatory enough. I had to go to other publications and the data website to understand the TNO-CO2 dataset. It is pretty normal for us to do so, but probably not for articles in ESSD, given the nature of ESSD. I must acknowledge that I took me really long to understand how the authors constructed the dataset, while (I thought) I have a fair amount of knowledge and experience in emission dataset development. I imagine the audience of ESSD would experience the same with the manuscript in current form. Adding more details of the dataset development and dataset, and evaluations would make this manuscript more suitable for publication in ESSD. Hope my comments are useful to improve the manuscript.

Apart from the first point, I am having a hard time to understand the scientific significances of this emission dataset/study. It seems to me that the dataset was not constructed in a way to support the authors' claims. Especially the words such as "compatible" and "consistency" do not seem to be a right word at many places in this manuscript. One thing I thought the authors have incorrectly (to support their claims, I mean) done in the development is that the use of the UNFCCC fuel-based emissions. The fuel-based emissions are calculated by the reference method (see IPCC, 2006). The estimates can be different from estimates from the sectoral approach which we often use as an official emission estimate. In the reference approach, complete combustion is often assumed. Thus, the resulting emission estimates include some emissions that should come out as carbon monoxide (CO) and some others. So when combined with other CO emission estimates, one will have to do a correction to subtract the CO portion of CO2 emissions in order to avoid a double count. This has been a known issue (e.g. Nassar et al. 2010 GMD). On top of that, unlike the EDGAR, CO2 emissions in the TNO-CO2 and air pollutants emissions in Kueren et al. (2014) are not constructed in a systematic way. Those two datasets do not share the activity data and emission calculation methodology (including sectoral emission disaggregation). I must say the TNO-CO2 seems to have some internal inconsistencies. It would be a problem since we think we should be able to calculate CO2 emissions from fossil fuel combustion really well and those estimates are used to derive policy implications. Especially, fossil fuel CO2 emissions are a key reference information in carbon cycle studies and people like us devote time to update emission information regularly (e.g. Oda et al. 2017 or 2018 in ESSD). The point source data are certainly shared by the TNO-CO2 and Kueren et al. (2014), but the point source emissions are roughly 50% or smaller according to this study. Meaning the rest of the emissions (non-point source emissions) are distributed in the unique way in this study. The 7km spatial resolution is also certainly compatible. But that is not very scientifically significant especially without other compatibilities claimed by this study. If what I pointed out here are fair, I would suggest the authors to reconsider how to message their claims.

I carefully went through the manuscript at my best and listed my questions, comments/suggestions and concerns. At this point, I am not happy to recommend this

manuscript for publication in ESSD. However, the authors might convince me (or just point out my misunderstandings) in their response.

1. Paper presentation

I would suggest to add more details of how the authors developed the TNO-CO2 dataset. I found that the TNO-CO2 is based on many different data sources, different data preprocessing and disaggregation procedures. A data source summary table and a schematic figure of the TNO-CO2 emission data production would be extremely helpful for the audience of ESSD. A data source table would help the data users to quickly get a rough idea of differences from other existing CO2 emission datasets. The manuscript also lacks details such as spatial and temporal resolutions in the data used in the sectoral and spatial disaggregation (for example, transportation data). The authors often cite published works to omit explanations. That would be totally fair for articles in other journals, but probably not in ESSD. In fact, I found that the TNO-CO2 data production is very different from Kueren et al. (2014) excepting the point source emission part (< 50% in 2014). It is important to add more effort to explain the TNO-CO2 data development in details, especially to support the claims made in this manuscript. I would also suggest to improve the data section that is required by ESSD. The TNO-CO2 dataset is distributed in two data formats (CSV and netCDF). The netCDF version of the TNO-CO2 dataset has a complex, unique data structure (compared to the EDGAR or other datasets, I mean). Although the netCDF file states that the netCDF version of the TNO-CO2 dataset was derived from the CVS version of it. But to me, it seems to me it was opposite (netCDF -> CSV?). Also, I would imagine the data users would like to know how often the TNO-CO2 dataset will be updated. Some of the information I mentioned in this section are presented in the website. Improving the data section thus should be a very easy task for the authors. I believe my suggestions here will improve the quality of the manuscript as an ESSD article.

2. Lack of evaluation – total emissions and sectoral disaggregation

It was quite a surprise that this manuscript does not present any evaluation of their emission estimates, sectoral and spatial disaggregation. As pointed out earlier, the TNO-CO2 is based on the fuel-based UNFCCC country estimates. Those estimates are obtained with the reference approach which theoretically gives us the upper bound of the energy related CO2 emissions (see IPCC, 2006). Those estimates are often supplementary compared to the estimates obtained using the sectoral approach and less policy relevant due to the lack of the sectoral information. This needs to be clarified as the authors mentioned the policy applications of this dataset at many places in the manuscript. I also think the authors should show the differences from the sectoral approach-based UNFCCC emission estimates, although we often don't expect huge differences in most of European countries. Because of the differences in the calculation methods, it is very challenging to do a clean comparison (probably only for the energy sector emissions). But it is important for the data users to know the differences before using the TNO-CO2. The data users would be also interested in the country total emissions differences between the TNO-CO2 and other datasets like EDGAR. This should be easy as EDGAR does provide sectoral emissions. As we all know, these comparisons are not very satisfying. But characterizing differences among emission inventories is often the only way to evaluate emission datasets. The same comment goes to the sectoral and spatial disaggregation. Due to the use of the fuel-based UNFCCC emission estimates, the authors estimated the sectoral emission shares using the IIASA's GAINS model. The authors should have shown the validity of this unique approach by comparing their sectoral share estimates to the reported sectoral emissions. My understanding was that the GAINS model's output is 5-year increment (maybe the authors used a different version of the model?). If so, the sectoral shares do not vary annually. Even if the GAINS model-based sectoral disaggregation is fair, I got an impression that the TNO-CO2 (2000-2014) was not ideally built to contribute to policy applications mainly due to the lack of the reported sectoral information. This comment does not apply to the future version of the TNO-CO2 as we have to rely on models like the GAINS model for future calculations anyway. The TNO-CO2 his-

toric and future are certainly compatible (constructed in almost the same way), but the compatibility was achieved by lacking the reported sectoral emissions in the historic TNO-CO2. I think this is a huge limitation in the TNO-CO2 and probably something the authors need to fix in the future version of the TNO-CO2 to be truly policy-relevant. Describing future plans is fair. But before doing so, it is important to document current limitations that the TNO-CO2 has. One more comment on the sectoral disaggregation: It is still not clear to me how cleanly the sectoral disaggregation was done, because the UNFCCC emission estimates only indicate the emissions from the energy sector. Especially, I am still confused how cement production emissions (or point source information of cement production emissions) are treated in the TNO-CO2. The authors stated that the fuel type information are kept in the TNO-CO2, but I could not find any fuel type information in the data provided from the website. This was another source of confusion. Last of all, I would suggest to provide uncertainty estimates where necessary. The TNO-CO2 is essentially based on their own sectoral emission estimates (CO2_ff) and own total CO2_bf estimates. It is difficult to estimate uncertainty estimates for the emission estimates, but the evaluations I proposed in this review would place the authors to a better position to discuss about the uncertainties associated with the TNO-CO2.

3. Lack of evaluation – spatial disaggregation

Spatial disaggregation is a tough topic, so I will discuss in this separate section. Although the disaggregation of the point source emissions was shared with Kueren et al. (2014), the rest of emissions are disaggregated by methods unique to this study. As mentioned earlier, characterizing emission datasets by comparing to other emission datasets is a very important in my opinion. There are existing emission inventories such EDGAR and IER emissions. The data users probably would like to know how the resulting TNO-CO2 emission fields are different from other emission datasets, given the data development in this manuscript. I think it is a necessary piece of information to guide the audience of ESSD. A comparison at the native 7km resolution cannot be done cleanly. A comparison at a common aggregated (coarser) spatial resolution would be still doable and come out very useful, as you would expect many data users run their models at a coarser resolution. In my opinion, even a comparison to an outdated emission dataset (such as IER?) would be useful. We can still loosely characterize the differences from disaggregation methods and inform the data users what happen they use an outdated emission dataset instead of the TNO-CO2. I don't think the authors have to compare the TNO-CO2 to many multiple others existing emission datasets. I think the comparison to EDGAR or IER emissions would be a good one to guide the data users, as some European simulation studies have used those emission datasets (e.g. Vogel et al. 2013; CarbonTracker-EU).

4. City-scale emissions - spatial resolution and granularity

I would like to discuss this especially as the authors mentioned a potential application to city scale emission studies. Before getting into the discussion, I would like to mention that I think a 7 km emission dataset might be a high resolution dataset for air pollutants, but probably not for CO2. There are many CO2 emissions datasets provided at < 7 km spatial resolution and those datasets have been intensively used in the published works (e.g. Vogel et al. 2013; Feng et al. 2016; Lauvaux et al. 2016). Please note this is just a comment. I assume the word high-resolution came from Kueren et al. (2014). If the authors think the title is appropriate, I would not object to it. Arguing the definition of high-resolution CO2 datasets is not the main point here. One thing I would like to really clarify is that the 7km spatial resolution is probably not good enough for city-scale studies. At a 7km, spatial emissions patterns (even major ones) within cities are aggregated. A recent paper by Gately and Hutyla (2017) did a nice job to compare their ACES 1x1km bottom up emission dataset (here you can imagine an emission data like the Vulcan dataset) to existing global emission inventories. Another good study by Feng et al. (2016) showed differences in modeled CO2 concentrations over Los Angeles (LA) using the Vulcan 10km emission data (Gurney et al. 2009) and Hestia fine-grained emission dataset (http://hestia.project.asu.edu/). Also, as the

authors have cited, Ciais et al. (2015) report's recommended the development of a 1km hourly emission dataset. Also, I should add that CO2 observing satellites such as NASA's OCO-2 has been collecting high-resolution (few km$\sim$) scale CO2 information (e.g. Schwandner et al. 2017). The more serious issue is the granularity of the emission data. I would like to point out that spatial resolution does matters in modeling, but high spatial resolution datasets without local information are not policy relevant. In the TNO-CO2, gird point emissions are obtained via sectotal and spatial disaggregation. The emissions spatial patterns look reasonable (high at highly populated areas, point sources and line sources), but those emissions patterns are essentially modeled. I agree with the authors that disaggregation-based emission datasets can be useful for city-level studies by adding local information. But I could not see a clear path of how the authors make a future TNO-CO2 to be useful for city-scale emission studies. I do not see a logical pathway to include locally-reported emissions information while keeping the consistency with national level. For example, the ACES is based on emission disaggregation using information at census block level. My 1x1km emission dataset (ODIAC) showed a reasonable agreement with the ACES 1x1km dataset. But it does not mean that ODIAC is as useful as the ACES dataset for guiding city-level emission mitigation. The TNO-CO2 is already based on lots of different data even at national level (before disaggregation). It seems to me that the use of the UNFCCC fuel-based emissions and sectoral disaggregation using the GAINS model posed a huge limitation on the TNO-CO2 to derive city policy implications.

5. TNO-CO2 as a CO2 emission dataset

As the authors pointed out, CO2 is very different from air pollutants. In my opinion, carbon cycle scientists are pickier than the authors assumed in terms of how they specify CO2 emissions. With some clarifications, the authors can help those picky carbon cycle scientists to better understand the TNO-CO2. First thing I would suggest is to clearly define CO2_ff. For example, Pinty et al. (2017) defined CO2_ff as CO2 emissions from fossil fuel combustion and cement production. I expected this manuscript shared the same definition, but the TNO-CO2 apparently does not have emissions from cement production given the use of the UNFCCC fuel-based emissions. But I did see the cement production sector in the SNAP sectoral conversion…. so my understanding is some of point source emissions are incorrectly distributed using cement production point source information. It would be very useful to document what emission are included and what are not, especially in contrast to the popular fossil fuel CO2 emissions definition. Then the data users could supplement missing emissions using other emission datasets. The second point is about the country total emissions. After clarifying the definition of CO2_ff, the authors should evaluate the country totals (as I also have suggested earlier). This is a regional emission data, so the data users often need to get large scale simulations to get reasonable boundary conditions. The data users then need to know the compatibility of the TNO-CO2 with the emission dataset for the simulations for the boundary conditions. Among Fossil fuel CO2 emission datasets, we often see a good agreement on global and country totals. For example, we often believe that the global total emissions are well known within 10% (e.g. Andres et al. 2014; Le Quere et al. 2017). Tiny differences among emission estimates could be a serious issue.

6. Consistency and compatibility Given my lengthy comments above (I am sorry!), the authors probably have a good idea of why I was not comfortable with the word “consistency” and “compatibility”. Here I will just list my concerns that I have been discussed earlier:

a. Total and sectoral emissions calculations are not shared with Pinty et al. (2014) (e.g. UNFCCC and activity data),

b. Only point source disaggregation is done in the same way as Pinty et al. (2014), but the rest of emissions are distributed in a unique way (improved way?),

c. Possible double count of carbon monoxide emissions, and

d. If just disaggregated to the same spatial resolution as Kueren et al. (2014), the

science significance is not so high (the data granularity matters.)

7. Some results are already published in Pinty et al. (2017)?

Apparently, some of results are also presented in the Pinty et al (2017) report in a different presentation. I am not 100% sure plots are made from the same emission data/model output. From the captions, I could not differentiate those plots. If those plots are based on the same results, it would be better to clarify that.

8. Main text

P1, L18: Cities need fine-grained emissions (not an issue of spatial resolution, but data granularity). My understanding is that cities are still struggling with compiling accurate emission inventories on regular basis and thus not in the stage of demanding high-resolution gridded datasets (excepting some pilot research cities). I think the need for high-resolution gridded emission datasets is now mostly driven by the research community. Do you get a different impression from European cities?

P20, L20: Please clearly define CO2_ff. The text says “CO2 from fossil fuels”, but apparently point source emissions from cement production are also considered in the modified SNAP categorization. If the authors expect the carbon cycle community to use the TNO-CO2 emission dataset, it would be good to let the data users know what are the differences from the common emission definition such as fossil fuel CO2 emissions (fossil fuel combustion, cement production and gas flaring).

L1, L20: It would be better to include a detailed definition of short-cycle. How different from the EDGAR short-cycle?

L1, L23: What do you mean by “complete coverage”? What are missing in the UNFCCC emissions? UNFCCC emissions have their own definition of “complete” coverage. To me, the word “complete” here sounds subjective to what the authors would like to achieve in this study.

L1, L27: What are the data (year) gap between the historical data and future data?

L1, L29: If the TNO-CO2 is to be used in designing future observational systems, rigorous error and uncertainty analysis need to be done for emission datasets as well.

L2, L21: I thought it was simply because subnational and seasonal emissions changes were beyond the Kyoto Protocol Framework, plus we did not have a well-established method to accurately produce emission estimates at such spatial and temporal scales.

L2, L24: Downscaled emissions won't help much to understand the dynamics of CO2 emissions. I'd suggest to rephrase something like "the natural side of carbon cycle processes".

L2, L30: Please note that the Ciais 2015 report recommended a 1km hourly dataset.

L3, L1: That is the beauty of the EDGAR database (and the reason why I think EDGAR approach is suitable for global monitoring purpose). Several studies have pointed out regional inaccuracies in the EDGAR database, but those inaccuracies are (in my opinion) mostly originated from the EDGAR's global consistency. Logically, we expect adding local or regional data would improve emission estimates, but it is depending on the emission dataset structure and data granularity the emission dataset holds.

P3, L8: Yes, but only if subnational emission distributions are accurately created. For example, can we tell total emission differences between cities from a country (for example, Utrecht and Amsterdam)? City scale emissions are based on the disaggregation and the accuracy of the estimates is subject to the accuracy of the spatial disaggregation

P3, L10: The word "consistency" here only means if you sum up the all the emissions in the individual countries domain, then your sum is the same as the UNFFCC emissions. It does not mean you can estimate city-level or regional emissions that are consistent with the IPCC-approach based emission estimates. This needs to be clarified.

P3, L20: I am confused with the author's mentioning "mitigation". With the current set up of the TNO-CO2 emission disaggregation, how can you accurately include the

mitigation effects in the resulting emission fields? (lack of sectoral-national emissions relationships)

P3, L25: Spatial disaggregation might have been done in a similar way, but the emission estimates are not consistent. The point source emissions are disaggregated in the same way as Kueren et al. (2014), but the rest of the emissions seem to be disaggregated in a different way (improved way). Regional sectoral share information (emission factor x activity data) are estimate by the GAINS model. The authors should provide more information to support this claim.

P4, L13: Please define NOR, CHE and NMS here.

P4, L16: This paragraph is just to mention the emission categorization, not data. To me, this does not seem to be parallel to other paragraphs that follow. Maybe better to reorganize this section?

P4, L16: This was the source of confusion. The authors mentioned the official UNFCC emissions earlier, but the TNO-CO2 is actually based on fuel-based emission estimates. And the sectoral information was estimated by the GAINS model.

P4, L17: Spell out CRF here rather than later.

P4, L25: "The motivation for..." This is unclear to me. This seems to be solely for the TNO disaggregation. The differences among the definitions used by parties are less significant than the TNO-CO2 is distributing non-cement production emissions using the SNAP cement production point source information. Also, the TNO-CO2 emissions are based on the UNFCCC fuel-based emissions and the GAINS model-based sectoral disaggregation. So the country total and sectoral emissions are already difficult to compare in a systematic way.

P4, L28: Not just some cases, but most of the cases, excepting the point source. Correct? As non-point source emissions do not have any sectoral information as the emission estimates were fuel-based. This process seems to be very important in order to support the authors claim (consistency with reported emissions), but this process is never evaluated.

P5, L4: The GAINS model plays a very important role in this study. But no evaluation of the model performance is presented. The authors should mention here that the GAINS model is also used for sectoral disaggregation of the fuel-based emissions.

P5, P24: The UNFCCC emissions used in this study are obtained with the reference approach, not the sectoral approach. Then the authors disaggregated the fuel-based emissions to sectors using the GAINS model. This is a big assumption. The authors should check the country emission totals in relative to the official UNFCCC estimates and evaluate the sectoral disaggregation by comparing to the actual UNFCCC sectoral emissions. The accuracy of the TNO-CO2 highly depends the GAINS model's performance. Any references to support the accuracy of GAINS model for this particular case? Also, the TNO-CO2 is based on the UNFCCC and TNO-MACC_II is based on the Convention for Long-Range Transboundary Air Pollution (CLARTAP). The emissions estimates are not consistent at the activity data level (even before the disaggregation). This is an issue when doing multi species analysis.

P5, L30: The unique spatial distributions are estimated/determined via disaggregation. This study never evaluated the resulting emission fields.

P5, L32: So this is not reported emissions, but modeled emissions. Even before doing disaggregation, the TNO-CO2 approach has introduced errors and uncertainties in their dataset.

L6, L5: Bias low – Reference?

L6, L9: These biomass burning estimates are from this work. The authors should have shown some comparison just to make sure the numbers are reasonable compare to the other estimates (or characterize differences).

L6, L12: Consistent... CO2_bf emissions over European countries are calculated in

a consistent way as the authors defined (Akagi EF x GAINS activity data). But here the consistency is a good thing? (ex. the use of same Akagi EF). I would suggest to characterize differences from other CO2_bf estimates (or something similar).

L7, L1: I would suggest to document what was the original spatial and temporal resolutions of the proxy and location data and elaborate how to achieve the 0.125 deg x 0.0625 deg resulting emission fields. It would help the data user to understand the quality of spatial disaggregation.

L7, L5: Which is which? It would be better to have a summary table to show data, data source and references.

P7, L13: Emissions are only for fossil fuel burning (given the use of the UNFCCC fuel-based estimates), but the emissions were distributed using non-fossil burning point source information. Correct?

P7, L29: I would suggest the author to elaborate the advanced techniques. What differences did it make compared to the EDGAR for example?

P8, L11: Such as?

P8, L14: I would imagine the use of fuel-based UNFCCC would be an issue because the scenario analysis would need to take sectoral information into account.

P9, L23-: Lack of the evaluation.

P10, L10: The depiction of city emissions (which are due to disaggregation) is not an important point especially at a 7km resolution. Please see my comment in the section titled "City-scale emissions".

P10, L25: The audience of ESSD (especially future data users) would be curious how different from EDGAR (emission total and spatial distributions) which have been for CO2 transport model simulations.

P10 L16: The rest of 50% emissions are distributed via the proxy data-based disaggregation. This is unique to this work and I think the disaggregation needs to be evaluated.

P11, L3-: It is fair to document the future plans. But before doing so, it would be important to document current limitations for the data users. Also, what is described in this section is very general ideas and I do not see much implications for future TNO-CO2. I mostly agree with this text, but I am not clear these ideas are really applicable to the TNO-CO2. To me, this discussion showed how difficult to use the TNO-CO2 emissions for city-level studies.

P14, L5: If the spatial pattern changes are not closely connected to the change in emission scenario, I do not see much significance in this plot as this is almost just a scaled picture.

P14, L13: It would be better to contrast with bigger countries with possibility. . .

P14, L20: Conclusion. The text is mostly dedicated to mention future perspectives. But before doing so, it would be helpful for the audience of ESSD if the authors summarize what this study has been done and what are not. I also a bit worried the TNO data is not ideally designed to achieve future plans described here.

P14, L25 – P15, P04: The same comment as above. Is the TNO-CO2 ideally designed for this purpose? I think the only significance of this dataset is the fact that the dataset is provided at the same spatial resolution as Kueren et al. (2014). It is very convenient for modeling, but the scientific significance is not high as emission fields at the spatial resolution could be achieved by adjusting other datasets. The emission estimates and disaggregation methods do not seem to be consistent. Then I started questioning like “So why don’t we stick to the established EDGAR approach/dataset?” and “Wouldn’t it be more reasonable to disaggregate UNFCC sectoral emissions using the EGAR approach?” I would have been in a better position to review this manuscript if the authors had provided more evaluation of the TNO-CO2.

P15, L12: To design future observation systems, the uncertainties in emissions need to

be quantified. The TNO-CO2 system, like any other datasets, is certainly adding disaggregation errors (sectotal and spatial in this case). From this view point, an evaluation of the TNO-CO2 is essential.

P15, L20: This data section can be improved. The authors have put a lot of useful information on their website (https://zenodo.org/record/112889#.Wj6P_lQ-cWp). It would be a good idea to incorporate the information to this section. Also, some more data description would be useful. The CSV and netCDF files do not have the same information. The data structure for the netCDF file is sort of complicated.

P21, Table 2: Uncertainty estimates?

P22, Figure 1: Uncertainty estimates? Is this figure already published in Pinty et al. (2017)?

P23, Figure 2: Uncertainty estimates? (the left) it looks like the totals are within the uncertainty range?

P24, Figure 3: So those sectoral shares are estimated by the GAINS model. Correct? If so, it would make sense to compare the sectoral UNFCCC for the evaluation of the GAINS model performance. Another evaluation in relative to EDGAR would be also helpful for the data users.

P25, Figure 4: Is this the same results as Figure 2b in the Pinty et al. (2017)? From Pinty et al. (2017), the EDGAR and the TNO-CO2 are very similar. It would be good to characterize differences as the plots do not tell much.

P26, Figure 5: Compared to other fuel-based emission inventories? Such as PKU-Co2 (http://inventory.pku.edu.cn/).

P28, Figure 7: Uncertainty estimates?

P29, Figure 8: Error estimates?

P30, Figure 9: If uncertainties are considered, I wonder how robust these changes are.

Are these same as Figure 5ca in Pinty et al. (2017) figure?

9. Supplementary material

P31, Table S3: It would be better to make it clear that this study used the UNFCCC data as activity data for the calculation of CO2_bf, as opposed to using the UNFCCC (emissions) as emission estimates for CO2_ff.

P33, Table S4: This confuses me… Apparently this study is using the fuel-based UNFCCC estimates (reference approach-based estimates) that is theoretically indicating the upper bound of the Energy sector. Did you use the emission estimates from SNAP? OR did you use those emissions to disaggregate? Depending on which, you will have some errors.

P38, Figure S10: I thought it be nice to add a plot for big counties from Pinty et al. (2017) to compare.

10. References

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