

Interactive comment on “Evaluation of anthropogenic CH₄ emissions over China using bottom-up inventories” by Xiaohui Lin et al.

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Reviewer 1#

GENERAL COMMENT The authors present a comparison of different bottom-up estimates of methane emissions over China. They discuss the differences in the spatial distributions and temporal changes in methane emissions for total anthropogenic emissions and some sectoral emissions. They use both global and regional data set to feed the discussion. They conclude on methane emission changes in China over the past decade and on the uncertainties on estimating methane emissions. The manuscript is readable and intelligible. However English polishing would be welcome (use of past tense during the discussion of the results seems odd). The figures are hardly legible

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(small font, pale colours) and need to be revised. Also, the study is lacking more detailed information on the data set used, and on the differences between inventories explaining discrepancies in methane amount or changes. Some information is provided in the supplementary but never mentioned or used in the main text. Response: We thank the reviewer for understanding the merits of our work. We also appreciate the reviewer for her/his constructive comments, which has significantly improved the quality of the paper. We addressed these comments point-by-point as follows. According to the reviewer's comments, in the revised paper, the language and editing has been polished by a native editor, and we have changed the past tense to present tense for the discussion part (e.g. in Lines 212, 213, 298, 306, 307, 331). Larger fonts and brighter colors have been adopted in producing the figures, which makes these figures more readable. Moreover, we added a detailed Table S1 summarizing the data sets used in this study, and we also included the changes of sectoral emissions by introducing Figures S2 and S3 to explain source of difference. More descriptions on explaining discrepancies in methane amount and changes among inventories (Lines 157-162) have also been included in the revision. In this revision, all the supplementary files are correctly referred in the main text. Some data sets are not introduced properly (sources of data/methods). The title could suggest that the BU estimates are evaluated, which is not the case. They are only (poorly) compared to each other. The reader would expect to gain insights on which data set provide the best estimates (depending on the sector maybe). Are some data sets out-dated for some reason (REAS?)? Why? Response: Good point! In the revision, we have included more descriptions/details on these inventories in the Methods section (Lines 110-131). Firstly, we changed “Evaluation” to “Comparison” in the title. Besides, we have added a detailed Table S1 to summarize all the data sets used in our study. Moreover, we have added more evaluating descriptions, e.g. in Lines 203-206 and Lines 216-226. For magnitude estimates, the NCCC could be considered as a reference, those have larger differences may need further check and should be cautiously used. We have clarified this in the revised manuscript (Lines 310-313). Moreover, we have added the kappa statistics in

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sect 3.2 to quantitatively identify the difference among inventories. Among the datasets used in this study, REAS2.1 is an old version since the newest version (REAS3.2) stopped CH₄ update, and the reasons may be this inventory concentrated more on polluted gases (<http://www.nies.go.jp/REAS/index.html#data%20sets>) (Kurokawa and Ohara, 2020). Other data sets such as Yue et al., (2012) did not update too, which may be due to lack of human or funding resources. What kind of activity data / emissions factors are the best to be used to properly represent Chinese emissions? Response: Since CH₄ emissions has large spatial-temporal variations and also the complexity of the processes included. To reduce uncertainties in emission estimates, detailed local to regional specific emission factors and temporal dynamic activity data are properly to produce more accurate inventory. We added discussions on this topic in lines 177 for coal emissions, 310-313 for rice emissions, 364-365 for localized emission factors (EF) (Table S6-S7). Using coal as an example, we recommended provincial EF and activity data from Sheng et al (2019). At the end of the manuscript, the reader has learned a bit on Chinese emissions (sector contribution, trends, some uncertainties) – assuming he did not know anything, but he doesn't know what to do with these different data sets, nor what is the best suited for any specific sector. Response: Thank the reviewer for pointing out this. As described for the above question, we added recommendations for the use of these inventories. We added more suggestions on the use of these inventories, e.g. in Lines around 310-313 for rice cultivation emissions, and 359-365 for waste emissions. In total amount estimates, EDGAR and FAO are close to NCCC, but FAO's close in total is resulted from higher estimates in energy and lower estimate in agriculture. Overall, we suggest the use of sector specific inventories with provincial activity data and EFs for improvements of national-data-based inventories. Specifically, for rice cultivation, we recommend Zhang et al., (2017) for the detailed regional water management data and provincial organic matter application rates, which is a Tier 3 method used in NCCC as part of national inventory reported to UNFCCC (Lines 310-313). For livestock fermentation, the regional temperature-dependent and species specific EFs from PKU are better than the default ones. For coal mines emissions, we

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suggest that estimates from Sheng et al., (2019) have more reliable emission patterns by using more than 10000 coal mines and the provincial activity data in coal productions. For waste treatment, emissions estimate from PKU which are based on the provincial statistics are more reliable, and show a minor difference with the reference values from NCCC. And if for atmospheric transport or inversion studies, total emissions covering full time-series is more important and PKU is recommended. We have thoroughly addressed this issue in the revised MS (Lines 310-313, 364-365), we hope this can satisfy the reviewer's concerns. However, such inventories comparison could be useful to the community, especially to highlight existing regional/sector specific data sets, under the conditions that thorough presentation, analysis and evaluation of the inventories are performed. Lastly, evaluation of inventories could be done using regional modelling of methane concentrations over China. For the above reason, I would recommend publication in ESSD but only after major revisions addressing the general and specific comments highlighted in this review. Response: We thank reviewer for her/his positive comments of our study. Following her/his suggestion, we have added more discussions on the evaluation of inventories using modelling methods.

Below are some comments to help improving the manuscript and study, but more comments could have been risen. Response: Thank you for your careful review and editing, we have addressed all your concerns and details are shown below.

SPECIFIC COMMENTS Section 1 – Introduction Line 36. Might be worse specifying that the value is a global mean average over remote marine stations (I guess...)
Response: Revised accordingly in Lines 39-40. Line 48-49. The reasons are not well explained here. Please detail. There are numerous estimates of methane emissions (as used in this study), the (small?) number of estimates is not the reason of the challenge of methane compared to CO₂. The challenge comes from the processes leading to methane emissions (leaks, biogenic emissions, ...) that are far much uncertain and difficult to estimate than emissions from combustion of fossil fuels – LULUCF standing aside. Response: Thank you and we stand with you on the

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comment that the large uncertainty in CH₄ comes more from the processes. We have revised accordingly in the revision (Lines 52-56). Line 53. This sentence is omitting regional inversions at higher resolution (for example, Thompson et al., 2015, <https://doi.org/10.1002/2014JD022394>). If inversions are discussed here broader and appropriate literature needs to be added. Response: Modified accordingly (Lines 61-63).

Line 58. “the quality” of activity data. The sentence seems to say that the activity data are of good quality – positive; while they are known to be quite uncertain; the whole sentence is too long and lacks of specific criticism against activity data and or emissions factors. Are they both uncertain in space and time? Split the sentence and rephrase to explain better why “the use of BU is challenging “ (I guess “challenging in modelling studies”: : : not specified). Response: Modified accordingly (Lines 70-73).

Line 62. I do not agree. Many studies have discussed Chinese methane emissions. Many of them are cited in this ms, and probably many other exist. Most of the global methane studies do mention Chinese emissions, their amount, spatial distribution, changes over time, and even uncertainties on emissions factor, specifically related to coal. This sentence is not above China, but more about the attention given to methane compared to carbon dioxide. Please rephrase or delete or move up. Response: Deleted accordingly (Line 75-77).

Line 66. Sort sectors by order of importance: energy is first with 45%! Response: Revised accordingly (Line 79).

Line 67-68. “large part of the variability”. Variability of what? Response: It means the difference among inventories. We have revised (Line 81).

Line 72. What could be the reasons explaining these differences? Paddy areas? Parametrization? Cultivation practices hypothesis? Response: Thank you for asking. On average, parametrization and model imperfection contributed 56.6% of the uncertainty, while errors and the scarcity of input data (irrigation and organic matter

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input amount) contributed the rest. We have clarified this in the revision (Lines 85-87).

Line 74. Meaning that there is none on solid waste? Response: Thank you for pointing out this. We have included those on solid waste in the revision (Lines 90-91).

Line 78. What is meant by “systematic”? is it regularly? Or with common procedure across sectors? Response: Here we mean comprehensive, not regularly, but in a systematic way (common data process procedures) for all inventories and sectors. We have clarified these accordingly in Lines 94-95.

Line 79 “Further mitigate climate warming”: not clear, please rephrase Response: Modified accordingly in Line 96.

Lines 80-81: The author state that they collect most of the existing data sets. This means that some are missing? And could have been included as well? It is stated here that 12 global and regional inventories have been gathered? Do they all covered all sectors? Table 1 lists only 4 global inventories. Response: Thank you for your helpful suggestion; following which, CEDS has been introduced in the revision. We have included all the datasets available to our best knowledge and ability in this study (Lines 97-98). We have added the detail information of inventories (8 tabular data) in Table S1. Table 1 listed the gridded data sets used in MS. All the 13 inventories covered all sectors. The sector information is listed in Table S2.

Section 2 – Data and Methods Lines 89-100. Following the previous comment. Table 1 lists only the 4 gridded data sets. No detail is given regarding the 8 so-called “statistical data sets”. What is “statistical datasets”? After stating the number of inventories, sectors are introduced, then we go back to the inventories. Please reorganize the section. The PKU inventory includes all anthropogenic sectors apparently and is not just a “fuel combustion inventory” The Global Methane Budget is based on EDGARv432-EPA- ECLIPSE v6 –FAO (agriculture) – CEDS - / so there is some overlap with the other datasets used here. The reader has no idea about the “published literature Yue, Huang, Zhang and Chen and Zhang2016 Zhang2018. A table and/or text are needed

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to explain these data sets: sectors/methods/ specificity. Why are this specific dataset included? What is their added-value? How do the reader access to them? (it is mandatory to have this information) Response: We added detailed information of inventories (8 tabular data) in Table S1 and Table S2 including activity data, sector and sources. Thank you for the reorganization suggestion. We moved the sectors introduction after the 13 inventories descriptions. We removed the “fuel combustion inventory” for PKU, and indicated that GMB included some data used in this study. We introduced other published literatures in Table S1 and acknowledged the data availability in the main text (in Data and Methods part). The added value of published literatures is cross-check of inventories using independent studies to include potential uncertainties and enhance the confidence of estimates. Some of the 8 tabular published literatures used provincial activity data and published in high-impact journals (e.g. Zhang and Chen, 2014).

Line 102. Why 3 and not gridded data sets here? this sentence is true for any inventory, gridded or country based. Response: We add the general information for explaining the classification of two groups in Lines around 135.

Line 104. What is the point stating that some inventories are used in inversion? It's really depending on the group doing the inversion (global..?). Response: Here, we meant to mention that one of the functions of inventory is used as prior for atmospheric inversion. And which inventory is used in the inversion model depend on the international fame, suitability (spatial-temporal coverage) of inventories for individual studies. CEDS, EDGAR and GAINS are more well-known data, and used more widely in atmosphere inversion.

Section 3 Results and discussions. Line 111-112. This is not really surprising as the Global Methane Budget is based-approximately on the same data set. Response: We agree with the reviewer and added more descriptions on annual changes of used datasets and also differences with GMB accordingly in Lines 155-156. Section 3.1 is entitled “temporal variations of anthropogenic emission” but half of it compares the magnitude of the inventories emissions or the relative contribution of the sectors.

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Please organize the discussion. Response: Thank you for these comments, and we added more descriptions on temporal variations and deleted sectoral descriptions in Lines 157-163. Line 115 – Energy sectors dominates (27-60%) but then agriculture activities contribute to 27-50%... is the relative contribution varying across inventories? Over time? Does it mean that the splitting is much different from one inventory to another? a stacked bar plot could be more explicit. Fig 1 – revised the colors and font size of the fig. FAO data are shown for all sectors. The authors have to acknowledge the source of FAO data for other sectors than agriculture. FAO produces only agriculture emissions; other sectors are from third party? After digging a bit, it comes from PRIMAP hist. dataset v2.1 (Gutshow 2016) using EDGARv42 for the recent period. No information is given regarding this dataset. Response: Yes, as the reviewer suggested: sectoral relative contribution varies across inventories and over time. We revised the sector contribution to temporal variation of methane emissions in Line 157. Figure S2 added for stacked bar plot of sectoral contributions over time. We revised the colors and font size accordingly in Figure 1. Thank you. We added FAO data description in Table S1. Line 119-121: what is the link between REAS and GOME data? Here again lack of description of the data set prevent the reader to understand the suggested conclusion. Response: We revised in Lines 169-170. The link between REAS and GOME data is that GOME trend served as an independent verification for activity data. After verification with higher GOME trend (increased 50% from 1996-2002) than provincial statistical trend (25%) and IEA trend (15%), statistical data on coal consumption were modified to higher values in the China Statistical Yearbook.

Line 123. “these results may be due ..” such a study need to be more persuasive and provide better and thorough explanations on the differences or discrepancies between inventory? Response: Indeed, this was mainly caused by higher EF for EDGAR than NCCC. We revised in Lines . For coal mining, EDGAR EF is 10.0 m³/t, while NCCC is 8.89 m³/t (Table S4); For rice cultivation, EDGAR EF is 0.1-1.4 g/m²/d, while NCCC is 0.005-0.21 g/m²/d (Table S4).

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Line 124/125. What is NCCC? Is there any ref to cite “proving” what is stated about lower emissions factors? Response: NCCC is the National Communication on Climate Change (NCCC) of the People’s Republic of China, which is considered as official data for China’s GHG emissions. We added the EF of NCCC in Table S4, and added the reference. Line 128. Here and elsewhere, when discussing EDGAR please specify the version, the trend will differ from one version to another. FAO other sector being based apparently on EDGARv42? EPA is EPA2012 (projection from 2005?) this need to be specified somewhere Response: Thank you for this good suggestion. We specified version information for EDGAR. and FAO other sectors are taken from the third-party PRIMAP-hist dataset v2.1 (Gütschow et al., 2016; Gütschow et al., 2019), and we listed them in Table S1. EPA 2012 is removed due to the reasons of projection from 2005, and detailed information provided in Table S1. Line 131: “may be caused by”. See previous comment, the readers need to be more confident in the results. Data/publication that could strengthen this suggestion/conclusion? Response: Indeed, we checked the publication and data, and drew conclusions with more confidence. Line 132 : subsectors are discussed but no plot are shown in the main text nor in the supplementary, how the reader can check the findings? Response: Thank you, we added subsector plot in Fig. S3. Line 147: The authors state that the gridded emissions are limited. But some are missing here. CEDS (based on EDGARv42) is missing. There are other version of EDGAR that could have been integrated (V432) for comparison. Response: Thank you for this suggestion. We added CEDS dataset in Figure and more discussions on EDGAR older versions, but to keep simplicity, we used the newest EDGAR version in the main text. Line 148-150. Again relative contribution of sector at national scale is discussed here. . . reorganize please. (Are the number in agreement with the previous ones?) Response: Thank you. We kept the relative contribution of sector at national scale here and deleted the previous ones. Yes, this is consistent with previous ones, this is one year (2010) and falls into the previous larger ranges. Line 150 and following. Discussing province differences could be difficult to follow for non-specialist of Chinese province. Please add a map (in the supplementary) of the provinces. Response: Thank

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you, and we added the province map of Fig. S1 in the supplementary.

Line 164. Emissions factors control more the magnitude than the spatial distribution, which is more related to activity data used to spatialized the emissions. Response: Thank you for pointing out this, we revised in Line 230-231.

Line 169 : What is NBS? Response: NBS is National Bureau of Statistics of China, and we added full name for abbreviation in Table 1 and Data and Methods in Line 140. Line 170 and below: Here the reader is waiting for information on the sources of emission factors, activity data and specialization hypotheses that may differ between inventories explain the discrepancies in different sectors. Could the different resolution of the inventory different induce visual spatial differences? Response: Thank you, we revised in Line 240-244. We changed the total emissions per grid cell to emission intensity (g CH₄ km⁻²) and thus the different resolutions of the inventories would not induce visual spatial differences.

Fig2 and 3. The figures are hardly legible. Color scale/ names of the inventories too small. Fig 2 right panels: the reader cannot read this. When is this useful? What is the grid resolution of these plots? EDGAR spatial distribution (and trend in Fig3) for energy is really different from the other inventories. Is such a distribution realistic? Response: Thank you, we revised Fig. 2 and 4, enlarged the color scale and names. Font size in Fig 2 right panels were enlarged and used in Lines 236-239 and 244 (Fig. 2v) to discuss the differences of spatial distribution (e.g. EDGAR distribution features). The grid resolution of these plots was original datasets resolutions in 0.1° or 0.25° (Added in figure caption). EDGAR originally uses 328 coal mines with locations for China from world coal association (<https://eerscmap.usgs.gov/wocqi>) as point emissions to disaggregate the amount of national emissions (Greet et al., 2019), and then update by Liu et al. (2015). However, emissions from coal mining estimated by EDGAR still have notable bias toward Shanxi province (Fig. 5f), while PKU and Sheng et al., 2019 used independent sources of 4264 and over 10,000 coal mines. As a result, EDGAR put small coal mines emissions to large coal mines.

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Fig. 1 The spatial location of coal mines in China from world coal association (a), spatial distribution of methane emissions from coal mines in EDGAR (b).

Line 190-195. Trend in agriculture emissions. Why EDGAR is so different from the other? Are these conclusions in agreement with FAO? with more specific agricultural? Response: We added the temporal variation of emissions from rice production and livestock (Fig. S3). The decreasing trend in southern China for EDGAR is consistent with GAINS and some provinces in PKU (Fig. 4 in the main text), while FAO only provides tabular data and no gridded maps, the trend comparison with FAO is not possible. For livestock, EDGAR and FAO show a similar trend (Fig. S3). For rice production, EDGAR is consistent with FAO (and other 4 datasets) with two dips in 2003 and 2007 (Fig. 2).

Fig. 2 The temporal variation of emissions from rice cultivation

Line 225-230. Here the authors state that rice cultivation areas (emissions have increased); while previously (line ca 190), a decrease in emission from rice cultivation was presented. How is that consistent? Response: The previously statement of decreasing emissions are mainly due to adopt the practice of draining paddy fields in the middle of the rice-growing season (Qiu 2009). The combined effects of the decrease in irrigation management and area expansion are decreasing emissions.

Line 230. Livestock includes enteric fermentation+ manure management? There is a recent paper on enteric fermentation that could be useful (Chang et al., 2019, <https://www.nature.com/articles/s41467-019-11066-3>) Response: Yes. We added this reference in the discussion (Lines 314). Livestock emissions include enteric fermentation and manure management. According to Chang et al., (2019), enteric fermentation from ruminants dominates the livestock emissions, and manure management has a smaller contribution.

Line 232; "REAS underestimates emissions" compared to what? Is there a reference/preferred inventory taken as reference? Response: Here, it means lower esti-

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mate than all the other inventories. Revised in Line 317. NCCC is generally considered as a reference and Lin et al., (2011) is the closest one.

Line 235(around). Here a discussion is needed on the sources of activity data used in the different inventories. Most of them use FAO, aren't they? So why such differences? Response: Thank you, we added it in the discussion for rice production in Lines 310-313. The spatial distribution is strongly correlated with the proxy data. National and sub-national FAO livestock statistical data are made into gridded maps using machine learning method (Gilbert et al., 2018), and PKU used province-level annual census data from agriculture statistics yearbooks (China Agricultural Statistical Yearbook, 1980–2010). We communicated with Gilbert and point out this large difference in Beijing, he explained that this could be resulted from the proxy method. Add the information in Line 321. Line 247. This could be easily avoided by plotting the maps on the same resolution. See also a previous comment. Response: The gridded maps of REAS and Sheng et al., (2019) are based on a spatial resolution of 0.25 degree, while others are in 0.1 degree, to keep the higher resolution and make them comparable, emission intensity are calculated and showed. Emissions from coal mining in EDGAR v5.0 are not clear in 0.1 degree, but the bar chart is calculated using its original spatial resolution to obtain the original value, and thus would not change the result.

Fig 4. Same comments as for other figures. What are the horizontal resolutions of the maps? Do they differ from one inventory to another? Response: Revised accordingly. The horizontal resolutions of the maps are shown within Fig.2 (under inventory name). The emissions intensity has considered the area of grid cells and thus is comparable. Line 253 and following. "considering the comparability of different inventory"; This is not clear. REAS seems to have been excluded from the mean calculation. Is there a reason? EPA is included while this seems to be EPA2012 data set, which is a projection from 2005 onward. How such a data set is valid? These mean/SD calculations seem to be the concluding numbers of this study, though they use a subset of the dataset discussed. What are the selection criteria? Is that the "best guess for Chinese

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methane emissions”? are the considered inventories the best ones for all categories?
Response: Thank you, here “comparability” means the same year (2010), completeness of all same subsectors (added in Lines 340), REAS was excluded due to lack of 2010 data. We excluded the EPA2012 data. The selection criteria for total emissions is the same year (2010), and completeness of all same subsectors. While for subsectors we included studies focused on only subsectors. Since we included estimates as much as possible and are likely to eliminate systematical errors, which make it a potential “best guess”. The considered inventories are well known ones representing the state of the art estimates. Line 268-272. Two groups of inventories are formed based on waste emissions. Which group is the most realistic? Can we conclude on this?
Response: Thank you for this question and we are tended to think group 1 is more realistic due to the detailed provincial activity data and localized EF they used while group 2 used national activity data and IPCC default EF. Section 4 Conclusions. The end of the conclusion mentions the used of default emissions factor instead of province specific emissions factors. This is the first time this point is mentioned and more discussion is needed on that throughout the paper. Are inventories using default emissions factor as reliable as others? More constructive criticism of the methodology used in the different inventories is needed to assess which inventory fits the best the reality.
Response: Thank you, we added in Lines around 177-178, 310-313, 363-365. Peng et al., (2016) had compared their results (PKU) with the estimates using same method but IPCC default emission factors (IPCC-EF). Estimates from PKU are consistent with IPCC-EF, but ~30% lower after 2000. We suggest the use of the best available information (e.g. regional-specific activity data and emission factors for each source sector, which is an important way forward in improving accuracy of CH₄ emission inventories.

TECHNICAL COMMENTS Line 37. Remove “However” Response: Revised accordingly.

Line 48 Change to “contribute the most to global...” Response: Thank you. Revised accordingly.

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Line61 Change “area” to “country” Response: Revised accordingly in Line 74.

Line 135: substantial variability. Do you mean increase? Response: Revised accordingly in Line 186.

Reference Andrew, R. M.: A comparison of estimates of global carbon dioxide emissions from fossil carbon sources, *Earth System Science Data*, 12, 1437-1465, 2020.
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Please also note the supplement to this comment:
<https://essd.copernicus.org/preprints/essd-2020-210/essd-2020-210-AC1-supplement.pdf>

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2020-210>, 2020.