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Comment on essd-2021-153

Anonymous Referee #1

Referee comment on "Daily CO2 emission for China's provinces in 2019 and 2020" by Duo Cui et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-153-RC1, 2021

Review of "Daily CO2 emission for China’s provinces in 2019 and 2020" (essd-2021-153)

The authors state in their introduction that "(current) estimates of China's CO2 emissions carry significant uncertainties" and "having timely and accurate CO2 emission estimates ... is therefore a fundamental prerequisite to designing evidence-based policies for reducing China's CO2 emissions." But the paper does not demonstrate that the new estimates are of any lower uncertainty than those already found. Indeed, the methododology used introduces very significant uncertainties with numerous assumptions and proxies. Can the authors argue that this new dataset has lower uncertainty than previous estimates at the provincial level?

On line 148, the authors write "The provincial dataset constructed in this study includes daily CO2 emission data from the three main polluting sectors (power, industry and ground transport) in China, which together account for more than 90% of the total emissions." However, the data used to estimate these are for the power sector, cement production, and road transport, and together these accounted for only 55% of China's CO2 emissions in 2018 according to EDGAR v6.0. The way this sentence is worded gives the strong impression that the data used cover the majority of the total and implies robustness from this. However, it is not 90% but 55%. Please reword to reflect the true situation.

Looking at the method used for road transport emissions, the national-level estimates for China in CarbonMonitor (the starting point of this study) are based on traffic congestion data tuned to traffic flows in one city: Paris. This method has been shown to have high and variable uncertainty when applied to other cities and countries (Gensheimer et al., 2021, who provide strong warnings that such methods should be used with caution). Further, the correlation used is with traffic flows (vehicles per hour), which in turn is assumed to correlate well with total transport emissions.
Starting with an uncertain estimate of daily transport emissions in China, the method in this paper then further disaggregates that by using vehicle ownership data by province

(annual, for the year 2018), on the assumption that the distance travelled each day in one province is very likely to be close to the share of that province's ownership of vehicles in China's total ownership. I find no discussion of how the age of the vehicle fleet might vary by province, how the share of diesel, petrol, electric might vary by province, and how driving behaviour might vary by province (for example according to wealth or urban density), to take just three examples. Nor do I find any discussion of how much passenger transport makes up of total ground transport in each province, when we know that goods transport is a large source of emissions.
This method leaves me wondering whether the resulting estimates are of any use at all, but the paper takes it as given that they are.
Since the details are not provided, I have looked at the data files and compared with NBS data and find that the authors have used "Possession of Civil Vehicles" (the English title), which appears to include all non-military vehicles. Please add sufficient details so that others might better understand the methods used.

Looking at the power sector, CarbonMonitor made use of daily power generation data to estimate daily CO2 emissions at the national level. However, daily data were only available for six power companies so had to combine with monthly national thermal generation data. Further, that method used a constant emission factor across all thermal generation in China, despite significant swings in total power generation caused by the pandemic early in 2020. While 'normal' changes in total production might not have much effect on the emission factor of total thermal generation over the country from month to month (and this is already debateable), large drops lead to the very clear possibility that high-cost production drops out first, potentially meaning inefficient power stations with higher emission factors. Whether this is the case or not in China has not been discussed.
In this article, only monthly data were available at the provincial level, so the national CO2 emissions from power generation on a particular day are scaled based on the province's share of thermal generation in that month.
Again, this leaves me wondering how accurate the resulting daily estimates will be, given that the entire day-to-day variation comes directly from the national estimates. I find no discussion of these issues in the article.

For the industry sector, CarbonMonitor used monthly data and then disaggregated these to daily data using the power sector emissions estimates. As that article puts it "assumes a linear relationship between daily electricity generation for industry and daily industry production data to compute daily industry production." Already a very strong assumption, with no discussion of how reasonable it might be. Here these very approximate national daily estimates are then disaggregated to provincial level using only monthly cement production data. Again, it's very difficult to imagine that the resulting estimates of daily industry emissions at provincial level are of high quality.

The article says that "at a provincial scale, only the data on cement production was available, with other indicators from the steel, chemical and other industries were missing." Does this refer to monthly data, or are indeed also no annual provincial data for these other sectors to be found? If annual provincial data for production in other industrial sectors are available, why are those not incorporated into the method to disaggregate the national industry emissions? Doing so would better constrain provincial estimates in this important sector compared to just using cement production.

Just before the concluding section, there is a short section discussing uncertainty. No quantification of the uncertainty is presented here. Only a very small number of the sources of uncertainty are briefly noted, and Monte Carlo anslysis is mentioned, without any results being presented. This is far from sufficient.

Overall, my sense is that this paper takes the approach that if a method can be developed then the numbers should be shown, regardless of how heroic assumptions might be. If I had a method with numerous untested assumptions to estimate some number that allowed me to produce that number with 10 significant figures, and then used a further method with numerous untested assumptions to extend that to 15 significant figures, should I then report that number at 15 significant figures? The analogy here is to both temporal and spatial scales: while the methods developed allow the calculation of daily provincial emissions, this is equivalent to my example of calculating something to 15 significant figures. Having a method is not the same as having a reasonable method. The authors started the article arguing that more accurate and more timely estimates were needed for provincial emissions, but the method only appears to address the latter (timeliness) and ignores the former (accuracy).

Much of the results section analyses patterns over the year, which does not require daily estimates, which is good. If the paper is rewritten to be an estimate of monthly province-level emissions, this reviewer would be more satisfied of its contribution.

Line 57: "However, these studies are based on provincial energy statistics". The authors need to state clearly why this statement warrants 'however', with the implication that this is a poor approach to estimating emissions.

On line 296 the authors write that a result is consistent with three previous studies, but since all three used the same underlying dataset (CarbonMonitor for China), this is to be expected and the three studies cannot be used to support the result. Exploring consistency with other studies relies on use of independent datasets and/or methods.

It is very good to see that Tibet is included, for completeness, although obviously that's achieved by way of very approximate proxies since energy data are not available. I would like to see it mentioned that Tibet's emissions are the lowest of all provinces. However, I see that CEADS has Tibetan emissions in 2014, so this article is not the first to publish Tibet's emissions. Please rephrase.

In the section "Trends in emissions from industry sector" the authors are effectively comparing the ratio of monthly provincial cement emissions to national total industrial emissions, because of the way provincial industrial emissions are calculated, and all trends discussed are of that ratio. Given this, do the authors believe that their interpretations in this section are useful? I do not.

The results presented in the section "Trends in emissions from ground transport" are entirely national, which is to be expected given the method uses no additional information on sub-annual emissions at the provincial level over what the national-level data already provide. This should be made clear to avoid readers misunderstanding.

In the analysis of the effects of holidays, results are presented in the text only for how the reduced emissions during holidays compare to the annual total, which is of minor interest. That the most significant holiday in 2019 reduced emissions by 0.33% isn't very interesting in itself. That this holiday was 7 days in duration and nevertheless reduced emissions by little more than one day's normal emissions (1/365=0.27%) is perhaps more interesting, and also how much these holidays reduce daily emissions compared to the normal for that time of year would be of interest. Also of interest would be some discussion of what sorts of activities (e.g. cement and steel factories) continue regardless of holidays, and which activities decline (e.g. road passenger transport?), citing any literature on the subject.

In the light of the very large uncertainty of the numbers, it is not appropriate to present results such as "91350.96 thousand tons". Please reduce the precision. Further, I presume these are tonnes (called 'metric tons' in the US) rather than tons, but if you do mean tons then you should make that clear.

Line 414: "changed little in those two years." Which two years? The two years of this analysis, i.e. 2019 and 2020? If so, then your statement is that because there was little change in proportions between those two years, you assume that there was little difference between 2018 and 2019. This could be written more clearly rather than leaving the reader to interpolate. Further, CEADS has provincial emissions from 1997 and the authors should use the information therein to support their assumption that provincial shares change little from year to year.
I think the authors should also provide the basic information for why this comparison is being made: Is CEADS the only other dataset to provide provincial emissions for China? Is it widely used? Is it considered a standard by which to compare?

The final sentence of the conclusions states "more work is still required in order to improve the provincial daily CO2 emission estimates from the lower emitting sectors, such as the residential, aviation and shipping sectors". I'm greatly concerned that the authors do not think that any further work is required to improve the sectors that are covered by the paper, given their very approximate nature.

References

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