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Atmospheric Chemistry and Physics Discussions

Interactive comment on "High resolution mapping of combustion processes and implications for CO_2 emissions" *by* R. Wang et al.

KRG Gurney

kevin.gurney@asu.edu

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This is a useful paper on a rapidly evolving sub-portion of carbon cycle research. Better global CO2 emission data products are needed. However, I was troubled by a number of omissions and misunderstandings about some of the input data, methods, and analysis that I believe require correction if this data product is to be published and relied upon by other researchers.

The first concern revolves around the input data and methods. CARMA is not a scientific-level dataset as it has not undergone any peer-review. My analysis of the dataset shows large and significant biases on the location and emissions estimates. For example, the CARMA dataset provides the center of the city nearest to the reported power plant. Hence, it is unclear how the authors of this paper could reliably

check the locations in google earth - many facilities could be located in the vicinity of cities and it is not clear how the correct facility could be identified. This requires more explanation. Datasets that have not undergone peer-review, such as CARMA, must be examined more carefully rather than ingested with little analysis or examination. Furthermore, there is a new CARMA dataset available and the authors should probably be using that data product (with adequate analysis). Since power plants represent such a large portion of global emissions, this is essential.

The authors describe key components of their methodology with inadequate detail. For example, in section 2.3, they refer to "emission proxies following the methods of Gurney et al.". It is not clear what this means. What is an "emission proxy" approach in this context? In the Vulcan effort, the investigators used CO emissions from particular datasets (though not all) and converted CO emissions to fuel. However, the key element is the CO emission factor used. Generic use of emission factor will simply reproduce the error-laden dataset produced by the EPA. One must use the CO emission factors reported by the emitting entities to reproduce fuel consumption. Then, oxidize that to CO2. I am concerned that this was not done. If the authors scaled CO2 with CO, there are a variety of problems with that approach. I think the most useful thing to do, is to carefully describe what exactly was done with CO emissions in those cases where they were used. Similarly, in the next paragraph, the authors refer to an "inner-interpolation approach was applied following the method developed by Gurney to get the data for 64 fuel sub-types". I do not understand what an "inner-interpolation approach" is? I don't believe the Vulcan effort used "inner-interpolation". Again, a more detailed explanation of these methods is required. The current draft has far too many vague descriptions of critical methods and this leaves the draft potentially misrepresenting the Vulcan approach and leaving the reader with a limited understanding of the methods used and potentially not confident of the outcomes.

In the section that compares the PKU to ODIAC and Vulcan (section 3.4) the Vulcan data product is misrepresented. The authors state that "area sources were uniformly allocated within counties in the VULCAN…". This is incorrect. Vulcan did not evenly distribute emissions into counties but rather distributed according to the area of building square footage and did so into building sub-categories. I urge the authors to read the Vulcan methodology and correct the description provided in this manuscript.

In this section, I would also encourage the authors to compare not to ODIAC but to the Rayner et al. data product that was published in 2010. It is surprising that this paper was not cited in general and more surprising that the comparison was not made to Rayner et al. given that Rayner et al. showed better comparison to Vulcan when compared to ODIAC. This would be a far better test of the PKU results.

The comparison to Vulcan requires the identification of the Vulcan version and whether or not the authors used the 10 km or 0.1 degree datasets. Though the comparison to Vulcan shows no overall systematic bias, the scatter in the plot of figure 5 is worth noting. The authors imply that a factor of two difference is "reasonable agreement". I would not agree with that as this seems like a very large amount of disagreement. Would this not support some modification of the uncertainty? Furthermore, I would agree that the differences are due to the traffic issue noted (but as I have said, I disagree with the second reason as a misunderstanding of Vulcan). However, there are a whole variety of other reasons why these data products are different at the gridcell level. In Vulcan, point sources are identified to geocoded locations. That includes most of the industrial sector, $\frac{1}{2}$ of the commercial sector, all of cement, all power plants, all airports, etc. Again, I urge the authors to read the Vulcan methodology so it can be correctly characterized in this manuscript. Finally, a difference plot in figure 5 would be much more illuminating. The side-by side emissions reveal little and don't show what is important – the differences.

In section 2.4, the authors refer to "combustion rates" and provide a series of numerical values. Are they meaning oxidation rates? It is unclear what is meant here. English grammar requires improvement throughout the paper. The tenses are often mixed, plural is mixed with non, etc.

I would encourage this journal to require that the resulting data product and the input datasets be made publicly available as a condition of publication. This is crucial for other investigators to reproduce and analyze these results. If the journal does not require this, I would urge the authors to do so independently.

Citations: No mention is made of the work by Marland et al., which is key to the context of any paper on the this topic, in my opinion.

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