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# *Interactive comment on* "CDIAC-FF: Global and National $CO_2$ Emissions from Fossil Fuel Combustion and Cement Manufacture: 1751–2017" *by* Dennis Gilfillan and Gregg Marland

**Anonymous Referee #2**

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Review of the paper "CDIAC-FF: Global and National CO2 Emissions from Fossil Fuel Combustion and Cement Manufacture: 1751-2017" by Gilfillan and Marland. The paper presents a useful update of a widely-used global and national CO2 emissions dataset along with some basic analysis of the results. Some aspects of the paper should be revised before publication.

Overall, the methodology is not defined in detail. While the main parameters are defined in Table 1 and the text, the details are basically a black box. This doesn't really meet current open-source standards. Its actually quite difficult to trace the methodology in detail through all the past CDIAC reports and papers. While I doubt the authors



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can address this completely, it would be useful if they could discuss this general issue.

Additional specific comments are below.

Line 86 - perhaps mention that space-based validation is complicated by large land-use fluxes of CO2.

Section 2.1

The paper should probably also discuss the CEDS inventory (Hoesly et al. 2018), which also extends from 1750 (using some data in common to CDIAC) and was used in the global CMIP6 climate modeling exercise. CEDS also largely a primary CO2 emissions inventory (energy emissions are calculated using energy consumption and emission factors, although sector emissions are provided) while process emissions are from other sources. The CEDS web site indicates that a new version extending to 2019 may be out soon.

Line 170 - EDGAR also includes emissions from some additional processes (such as lime manufacture and fossil fuel fires) that are not included in the other inventories mentioned.

Lines 205 - 210.

It would be useful to better clarify the issues of fuel oxidation here. There are two pathways for non-oxidized fuels, incomplete combustion (e.g. some carbon in the fuel does not exit the smokestack, e.g., is retained in ash or soot within the combustion devise), and fuel used as feedstocks (e.g., "non-energy uses of fossil fuels" in the author's language) that are subsequently not oxidized.

Line 215 - Clarify (I assume?) that the "correction factor" is applied to the FOi in Table 1 for the global estimate? Clarify if this applied equally to all fuels?

It would be useful somewhere to note that these estimates (like all the others mentioned) represent eventual CO2 emissions into the atmosphere, but actually include





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incomplete combustion (BC, OC, CO, NMVOC, CH4) emissions at the point of emission. Implicitly assuming all of these emissions are eventually fully oxidized).

Line 412 - The earlier methodology section does not discuss the data sources for flaring CO2. This should be added. Then at this point, it would be useful to comment on the accuracy of the flaring data from Iran. Is this from remote sensing? I would presume there is significant uncertainty associated with this data? (How does this estimate compare with other sources, given its this source that puts Iran into the top 10?)

Section 3.3 Decomposition The data used for this analysis needs to be better defined. There are many different metrics that can be used for GDP, for example, including different dollar years. Also is MER or PPP-based GDP used, this can make a large difference.

It is not accurate to use the Kaya identity to say that "increasing wealth" has contributed to an increase in CO2 emissions. What has happened in the real world is a complex system where increasing wealth is accompanies by changes in economic structures and applications of new technologies, all of which impact emissions. The Kaya identity is simply one way of decomposing the major driving forces, but they are actually much more entangled than depicted here. Use of the identify is fine, but the discussion needs to be re-written to be more careful about how the results are described. The Kaya identity does not show causation, it is simply a sometimes useful decomposition method.

The authors have not shown that "population growth is the dominant driving force" in Saudi Arabia. What they have shown is that population is the largest factor in the decomposition. It would take an analysis of the structure of emissions growth over this period to show if population growth was actually "the dominant driving force".

(Language in the conclusion section should be similarly edited to be more accurate).

Figure 3 It would be useful perhaps to add a dotted vertical line at +-10%.

The exact versions of each data set used in Figure 3 needs to be specified.

Figure 3, and accompanying discussion, would be useful if there was some attempt to compare like-with-like taking into consideration different system boundaries of the datasets. The authors have not really shown that "We suggest that the differences are not indicative of accuracy but rather an indication of the different system boundaries and a measure of the uncertainty", but this is instead really the conclusion from previous work and should be re-stated as such.

Line 527. I rather doubt that "obligations to regularly report emissions to the UNFCCC" are the reason explaining slow growth in CO2 emissions. (or, at least the authors have now shown this).

Table 2 - It would be useful to extend the table and comparisons out to 2017 given that both the new CDIAC and Andrew exist at this point (and the continuing trends of decreasing clinker ratio). I suggest re-ordering the rows, with ORNL CDIAC first so that CDIAC-FF and Andrew next too each other since they are more similar.

Hoesly, R. M., Smith, S. J., Feng, L., Klimont, Z., Janssens-Maenhout, G., Pitkanen, T., Seibert, J. J., Vu, L., Andres, R. J., Bolt, R. M., Bond, T. C., Dawidowski, L., Kholod, N., Kurokawa, J.-I., Li, M., Liu, L., Lu, Z., Moura, M. C. P., O'Rourke, P. R., and Zhang, Q.: Historical (1750–2014) anthropogenic emissions of reactive gases and aerosols from the Community Emissions Data System (CEDS), Geosci. Model Dev., 11, 369–408, https://doi.org/10.5194/gmd-11-369-2018, 2018

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