Referee #2

General comments: The paper addresses an extremely important aspect of the timeliness of India's GHG inventory reporting. Several data limitations and inconsistencies are rightly identified and an effort has been made to solve these, e.g. the differences in reporting intervals of coal production. The dataset provided here is extremely useful not just as activity data for CO2 emissions but also for other GHGs. Overall, the author has put in a great deal of effort into the paper and the supplement, which must be appreciated.

Many thanks for your thoughtful and helpful comments.

Specific comments: Attending to some of the concerns below, to the extent possible, might further enhance the usability of this dataset:

1. Page 1, Lines 27-31 note the role of renewable growth towards the stabilizing trend in CO2 emissions. It is also useful to point out here the opinion of some experts from the literature that the shortage in coal production has taken due to a combination of complicated factors (land rights, political issues etc.). The following case study makes an excellent assessment of this, and I recommend 1-2 lines on such factors:

Carl, J. (2015). 4 The causes and implications of India's coal production shortfall. The Global Coal Market: Supplying the Major Fuel for Emerging Economies, 123-163.

I believe the point the reviewer suggests I make is largely already made later, in the section "Deviation from forecasts", where I say "But these faced an array of headwinds constraining growth, including difficulty in acquiring land and environmental permits, local protests, difficulty obtaining finance (CEA, 2019), rail under-capacity, debt, subdued demand, unpredictable monsoon rains, "Coalgate" (illegal government coal block allocations; Gilbert and Chatterjee, 2020), the dramatic fall in renewables prices, and large economic shocks such as 2016's demonetisation, 2017's GST introduction, the shadow bank crisis starting in 2018, and 2020's COVID-19 pandemic."

Nevertheless, I agree that the point is usefully made already in the introduction.

- Changes made: Added sentence "In addition, the difficulty India has faced in ramping up domestic coal production has probably also restrained emissions growth (Carl, 2015)."

2. In Page 1, Lines 38-40, it might be useful to point out (if applicable), that the thirdparty reporting through agencies by IEA might not be open-access and that adds to the utility of this dataset.

- Changes made: Added text ", and not all of these are freely available"

3. Page 3, Lines 3-9: I appreciate the explicitness in mentioning the difference in accounting only for combustion based emissions and overall oxidation. In the same vein, a line could be added here (or later) that future inventories could add additional emissions such as CO2 emissions due to spontaneous emissions from coal mines; see following the reference and the recent 2019 IPCC Refinements:

Carras, J. N., Day, S. J., Saghafi, A., & Williams, D. J. (2009). Greenhouse gas emissions from low-temperature oxidation and spontaneous combustion at open-cut coal mines in Australia. International Journal of Coal Geology, 78(2), 161-168.

Singh, A. K. (2019). Better accounting of greenhouse gas emissions from Indian coal mining activities: A field perspective. Environmental Practice, 21(1), 36-40.

This is very interesting. Certainly, future official inventories should attempt to include estimates of emissions from both low-temperature oxidation and any resulting spontaneous combustion of coal. The Australian paper the reviewer cites concludes that CO_2 emissions from spontaneous combustion in the open-cast mines sampled ranged between 0.01% and 1.34% of the amount of CO_2 from eventual combustion of the mines' produced coal. These are very small amounts.

The IPCC 2019 Refinements provide several default factors. Using the average factor for surface mining, 0.44 m^3CO_2/tonne of coal produced, would result in emissions that are 0.03% of India's total CO_2 emissions. The paper the reviewer cites by Singh reports a higher value, from sampling three Indian mines, but this would still result in emissions less than 0.5% of India's total.

Given the very small magnitude of these emissions sources, it's unlikely that I would include these in future revisions of this dataset, when there are much larger sources of uncertainty in this analysis. But I will add this to the section on uncertainty in the estimates.

- Changes made: Added a sentence in the discussion of sources of uncertainty: "A further missing source is that of low-temperature oxidation and spontaneous combustion of coal at mines, but available evidence suggests these would be significantly less than 1% of India's CO_2 emissions (Day et al., 2010; IPCC, 2019; Singh, 2019)"

4. Page 3, Line 41 of main manuscript and section 6 of the Supplement: The authors note using the 2006 IPCC Guidelines default emission factors. However, Indian experts have developed national emission factors which have been vetted and included in the IPCC Emission Factor Database. I recommend using these emission factors either directly or at least for a sensitivity analysis to look at the difference between default and country-specific emission factor.

Indeed, India's reporting to the UNFCCC is based on national coal energy contents and emission factors. However, the information provided in the BUR is insufficient to make use of these, and there has been considerable change over time, not reported in the BUR, which is only for a single year. For now, I will retain the factors I have used, but these can readily be changed in future revisions of this dataset. I have added some discussion of these factors, in addition to significantly extending the discussion of coal energy content in the Supplement.

- Changes made: Added paragraph to Supplement:

  Emissions from coal in India's Second Biennial Update Report (BUR) are derived using country-specific energy contents and emission factors (GOI, 2018). The Report is unclear as to whether these factors, reported in tables 2.3 and 2.4, are only used for domestic coal, or whether they are averages for total coal supply, including imports. Imported coal is of higher quality than India's domestic coal, and this likely explains why the energy contents provided in table 2.3 for coking and non-coking coal (23.66 and 18.26 MJ/kg, respectively) are somewhat higher than those reported by the IEA for domestic coal (20.50 and 16.69 MJ/kg). The BUR's reported energy content of lignite, which is entirely domestic, is 9.80 MJ/kg, very similar to the IEA's 9.55 MJ/kg, and somewhat lower than the Energy Statistics' value of 11.37 MJ/kg.

5. With respect to Figures 12-14 of the supplement, is it possible to decompose the coal production further into surface- and underground-mined coal (either directly or through % estimates from other sources)? That would make the dataset immediately usable for other applications such as methane estimation studies or life-cycle GHG studies.

Yes, that would be useful. Unfortunately, I've found no data that show this split on a sub-annual basis. The *Coal Directory* has data on the share of open-cast and underground production, available from fiscal year 1999, but I have found no monthly breakdown of these. One could make some assumptions, but I have no information on, for example, whether underground mines are less or more affected by the monsoon than open-cast mines. I note that the IEA in its 2020 energy data edition has introduced a fugitive dataset, including methane emissions from coal mining, reaching 1435 kt in 2018.

- Changes made: none.

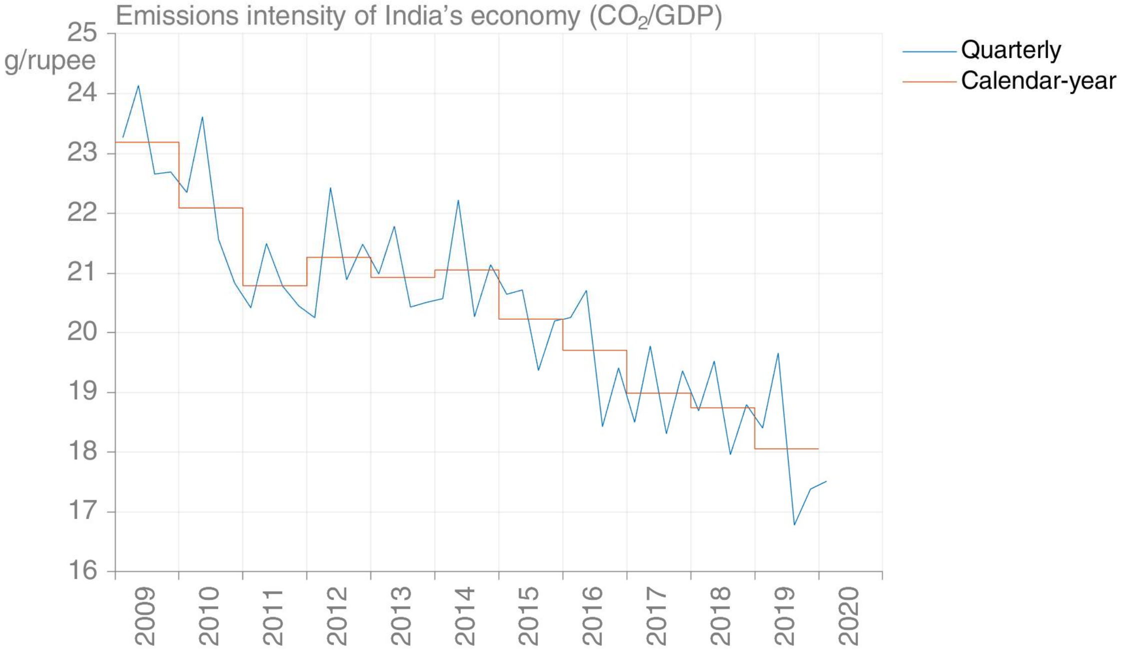
6. In Page 5, line 4: Why does the author apportion the peaking of natural gas emission rise to use as peaking plants? I understand that the use of word "perhaps" conveys uncertainty but I welcome the author to convey the reason for their speculation.

I'm glad you questioned this. Spurred by this comment, I have looked more closely. The monthly data available since 2014 show no indication of any consistent seasonal cycle in natural gas usage for power consumption, so this suggestion of peaking plants was mistaken. However, fertiliser production, which accounts for about a third of natural gas consumption, does exhibit a distinct seasonal cycle, peaking in monsoon months, presumably in response to agricultural demand. I have reworded the text as follows:

- Changes made: Replaced "perhaps being used as peaking plants" with "apparently driven by increased fertilizer production during these months"

7. Page 5, Lines 10-14 make important observations about variation of CO2 emissions per year. The Government of India's INDC mentions its target as reduction of the GHG intensity (or GHG/GDP) by 33-35%. Therefore, in addition to comparing the GHG emissions, it might be useful to compare the CO2 emissions per unit GDP as well to gauge consistency with the above goal.

- Changes made:
  Added the following figure to the Supplement:



  Added a sentence to the manuscript: "Over the same period, the CO_2-emissions intensity of India's GDP has declined from 23.2 g/rupee to 18.1 g/rupee, about 2.2%/yr (Supplement Figure 40)."

8. Page 7, Lines 1-2 note the local peak due to KG-D6 basin. Additionally, do the authors have reason to believe that some emissions in the gas sector might have been due to the increase in coalbed methane production as well?

Data from the Ministry of Petroleum and Natural Gas's Indian Petroleum & Natural Gas Statistics 2018-19 provide CBM production since 2008, and while production has increased from 13 MMSCM to a high so far of 735 MMSCM in 2017/18, the peak proportion of CBM in total natural gas production was only 2.3%, smallest (<0.5%) during the years of growth in offshore production. So no, CBM is not significant here.

- Changes made: none.

9. Page 7, Lines 3-4 mention stranded assets and it might also be useful to mention additional literature discussing potential stranded assets as climate restrictions come into force:

Malik, A., Bertram, C., Després, J., Emmerling, J., Fujimori, S., Garg, A., ... & Shekhar, S. (2020). Reducing stranded assets through early action in the Indian power sector. Environmental Research Letters, https://doi.org/10.1088/1748-9326/ab8033.

This looks like an interesting article, thank you. When I raise the point about stranded assets, it is as a sort of footnote in the context of the substantial rise and fall of natural gas production. The issue of stranded power station assets in general is much larger, and not something I think I should go into in this paper. Unfortunately, Malik et al don't mention the stranding of natural gas fired power stations, otherwise it would have been logical to add this reference.

- Changes made: none.

10. In Page 9, Lines 12-21 where authors point out the COVID-19 effects, it could also be mentioned that this dataset could be used as a correlation to the top-down effects on air pollution reported for Indian studies. This, in my view, further enforces the need for such a dataset.

Yes, I agree.

- Changes made: Added sentence "Furthermore, given the close links between emissions of CO_2 and other air pollutants, studies on changes in air pollution due to India's lockdowns, could be cross-validated with the monthly CO_2 estimates reported here (e.g., Sharma et al., 2020; Mahato et al., 2020)."

11. Page 10, Lines 2-4 make an interesting point about imported urea use. Is it fair to assume that this is another reason why the emissions data in the paper track with the government data as it is also the case for the UNFCCC data reporting practices?

Yes, in this paper I'm trying to include all significant sources of CO_2 emissions in India. In India's reporting to the UNFCCC they are required to do this, and the IPCC Guidelines for inventory construction include such details as calculating emissions from use of urea. The IPCC approach to emissions from use of urea in agriculture is agnostic to where the urea is made, and instead calculates emissions from all urea purchased by agriculture.

- Changes made: none.