Comment on acp-2021-437
Anonymous Referee #1

Referee comment on "Quantification of CH₄ emissions from waste disposal sites near the city of Madrid using ground- and space-based observations of COCCON, TROPOMI and IASI" by Qiansi Tu et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-437-RC1, 2021

This paper uses satellite and ground-based measurements of methane columns to estimate emissions from three landfills in Madrid. It is innovative in its approach and in the data sets that it uses. But I found the paper to be far too long relative to its information content, I was very confused by the presentation, and I found the results unconvincing. I cannot support presentation in present form and would be willing to review a revised manuscript only if it is considerably shorter.

- The Introduction takes a very long time to get to the point, and at the end we still don’t have a clear statement of the problem. Is the paper to quantify emissions from Madrid, or from three landfills, or is this the same thing? Why should we care about Madrid? What is actually new here?
- Section 2.1.2: give reference for the specific TROPOMI product that you are using. TROPOMI has a very low success rate (3% globally), is that an issue here? Or is Madrid sunny enough?
- Section 2.1.3: this combined IASI+TROPOMI TXCH4 product is probably totally dominated by TROPOMI information in the PBL, which is what matters here. So how is it independent from TROPOMI?
- Figure 1 is very difficult to read.
- Section 2.3: this section is very confusing because it is not clear what the authors are trying to optimize. Are the ‘daily plumes’ for the individual landfills? Are they summed over the three landfills? Are the three landfills treated as a single plume? The cone model is surely wrong for instantaneous plumes but is reasonable for time-averaged plumes, which is what is fitted but it takes the paper a while to explain this.
- Line 206: I don’t get the point about seeking an analogy with NO2. The landfills don’t emit NOx, NOx is an area source, and the decay of the NO2 plume is by oxidation rather than dilution into background.
- Wind speed is denoted v in the text, w in Figure 2.
- Equation (9): not clear how you get y_BG
- Section 2.3: there are many uncertainties in the procedure for inferring emissions. How can it be validated? An obvious way would be to use the independent COCCON observations to evaluate the posterior concentrations resulting from the TROPOMI inversion.
- Figure 3: the agreement between TROPOMI and COCCON in that Figure strikes me as
very poor, despite the authors’ claim to the contrary. I’m not surprised by this in view of the known TROPOMI biases, but it undermines confidence in the results of the TROPOMI inversion. The paper goes on about the problems on Sept 25 and Oct 4 but that seems anecdotal and those two days don’t seem any worse than the rest of the population in Figure 3.

- Lines 329-330: how do we know that the `COCCON instruments show a very good ability to detect the source'? No specific results or data from COCCON are shown.
- Figure 8 is cryptic. What domain is shown? What are we learning from it?
- Table 5: I don’t see the relevance of this Table to the paper.