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Comment on acp-2021-978

Brad Weir (Referee)

Referee comment on "Observational constraints on methane emissions from Polish coal mines using a ground-based remote sensing network" by Andreas Luther et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-978-RC1>, 2022

Luther et al. use remotely-sensed column methane (XCH₄) data to quantify emissions from coal mines. This work is part of the 2018 CoMet campaign to understand carbon and methane emissions and focuses specifically on XCH₄ retrievals from EM27/SUN Fourier transform spectrometers stationed at four temporary sites situated in the four cardinal directions from a collection of Polish coal mines in the Upper Silesian Coal Basin (USCB). The ability to monitor, verify, and estimate methane emissions based on atmospheric data is of great scientific and societal relevance, especially given the role it is expected to play in international efforts to mitigate climate change.

This is a very well written paper with clear relevance to Atmospheric, Chemistry, and Physics. I recommend acceptance and have suggestions for a few minor points of clarification.

Specific points

- A correction after COCCON calibration? I found the correction applied after the COCCON calibration to be confusing/worrying. Isn't one goal of the COCCON calibration to prevent issues like these? Obviously this is more a question for the COCCON group than for this paper, but many of those people are coauthors, so it seems relevant. Could you please clarify here?
- Independent data evaluation. I would've preferred an evaluation against independent data, perhaps say a comparison of prior and posterior CH₄ against the aircraft data from Kostinek et al. (2021). The set-up in this paper is so close to that paper, it really is puzzling how they disagree on greater vs. lesser emissions for the June 6th case. It would be interesting to see how well they agree with each other in CH₄ mixing ratio space and if it has anything to do with the above rescaling. However, the authors can't be expected to do everything, the paper is quite thorough, and the scientific evaluation

in Section 4 and the Conclusions, especially the discussion in the context of Kostinek et al. (2021) is sufficient. Since this is a paper about a campaign and network, I thought it might be nice to have a few more sentences on how to resolve these discrepancies other than adding more and more data until they go away.

- EDGAR version and year. The year 2017 on line 32 must be a typo because EDGAR v4.3.2 ends in 2012. I hate to be this reviewer, but that is a very old version of EDGAR. The current version, v6.0, actually includes the study year of 2018. It also includes a seasonal cycle, which could be relevant to the results in this paper. My understanding is that E-PRTR and not EDGAR is used in the study's prior. In that case, it should be straightforward to include the number cited in the text for the newest version and the study year. If EDGAR was used in the prior, I'd be happy if you just noted both numbers (2012 from v4.3.2 and 2018 from v6.0), no need to redo experiments. Also, it might be nice to list the exact sectoral breakdown that was used.
- Satellites. Campaigns like CoMet and networks like COCCON have another very obvious use in that they are essential in the bias correction and interpretation of retrievals from satellites. There are now several satellites in orbit that observe CH₄, e.g., the GOSATs, TROPOMI, with the expectation of several more (GeoCarb, Merlin) including several from commercial partners. Could you dedicate at least a sentence or two, perhaps in addressing point 2 above, to describe what impact your results might have on those missions and vice-versa?
- The background variability of 0.6 ppb on line 178 seems an order of magnitude too small. Could you provide more support for why this is reasonable? Looking at Table 3 from the Barkley et al. (2017) paper from ACT-America, they have day-to-day variability in background CH₄ over North America of several ppb. I'm not sure that exactly maps to the purposes here, but assuming much of that is passing weather patterns, I'd guess a number of at least 6 ppb would be more appropriate. Maybe this gets picked up in the L-curve fitting, so it isn't of much practical importance? Either way, 0.6 ppb seems low.

Technical points

- Can you give some indication of the correlation lengths/patterns of model background (S_{ϵ})? Correlation lengths affect the interpretation of the values on the diagonal, so this would be helpful putting those numbers in the context of other studies. Since the authors used an ensemble approach, a correlation "length" might not be 100% appropriate, but I'm sure they can come up with some number/figure to indicate how errors decorrelate horizontally, even if it is just one number or a range.
- At least two words about chemistry. Given the short time scale of the experiments in this paper, oxidation probably doesn't play much/any of a role, but it might be worthwhile to say that somewhere?
- "An" averaging kernel instead of "the". I found the use of the averaging kernel very helpful, but it might be better to call it "an" averaging kernel instead of "the" so that the reader does not confuse it with the averaging kernel of the EM27/SUN retrieval. Either that or qualifying adjectives like "the emissions averaging kernel" might help.
- For those of us that haven't dedicated a good chunk of our life to this campaign over the past few years, it might be helpful to add the cardinal directions to the site names in the text as is done in Table 1. I did find myself going back and forth a lot trying to figure out which station was where, but got the hang of it once I finished.