Review of: *Small emission sources disproportionately account for a large majority of total methane emissions from the US oil and gas sector*

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Summary:
This work uses emission factors from ~20 published studies across ~9 regions to estimate national methane emissions from active mid- and up-stream oil/gas production facilities for 2021. Using infrastructure inventories (Enverus, OGIM database), regional emission rates were modelled and validated with airborne surveys.

The manuscript is well written and the subject is of suitable content for EGUsphere. The subject is timely as there is active discussion regarding how mitigation funding can most effectively be used to reduce fugitive emissions from O&G. The figures are well designed and informative.

I have two main hesitations that together question the novelty of this work and the contributions that it provides. First, the chosen methodology, which is complicated and I am not convinced contributes to the authors results, discussion, or conclusions (see general comment 1). Second, the close similarity of this work prior work from this group (see Omara et al. 2022; 2024) questions the novelty of this manuscript. Specifically, the aggregation of emission factors is already published in Omara et al. 2018, 2022, & 2024. Scaling from emission factors to national budgets using Enverus is repeated from Omara et al. 2022 & 2024. Lastly, comparison of national/regional/basin-level emissions to airborne studies was previously done in Omara et al. 2022 & 2024.

Given the incremental differences between this manuscript and others that this group has published, I recommend declining publication.

General Comments:

1. Methodology: What is the benefit of using a bootstrapping approach? Is the bootstrapping solely to provide confidence intervals, or is there an additional benefit?

   My criticism is that many of the same results and conclusions are achieved without this analysis or less complex approach. Conclusions 1 and 2 can be drawn solely from the prior EF distributions. Conclusions 3 and 4 require knowing the number of facility types and production rates (taken from Enverus, OGIM database) but also do not require the monte carlo bootstrapping. Same critique for sections 3.1, 3.2, and 3.3.

As an example, the main conclusion of the authors (Conclusion 1, L712) is that 72% (70% as stated in abstract, L22) of total emissions are from facilities that emit less than 100 kg/hr. This is in fact buried in the last table of the supplement, which states the prior emission distribution and shows that 72.7% emissions facilities are from these "small" emitters. The posterior is unchanged from the prior, which is good since MC bootstrapping in this approach shouldn't change the center value.

A MC bootstrapping technique may be more interesting if applied to randomly select which EF studies to include. For example, if 6 of the 11 studies of facility category "Well Sites" listed in Table S1 were randomly selected for each simulation, then we might assess dependence based on regional dependence of studies, sampling/analytical methodology, etc. Indeed, regional differences are maybe observed, e.g. loss rates of 0.90% for Appalachian and Greater Green River regions (Omara et al, 2018) compared to >4.5% for San Joaquin and San Juan regions, but the variance within the regional populations precludes saying these loss rates are different (based on a Tukey test). Could the Tukey test be run on the $log_{10}(loss\ \%)$, given that these appear to be lognormally distributed in Figure 1?

2. What is the 95% CI for the total national CH4 emissions?
3. Data Availability: Data should be made available in a publically accessible, reliable repository and linked, preferably, through a DOI per EGUsphere instructions.

   Ideally, I would also prefer to see a table or reference section in the supplementary that has direct links, references, etc to the data from other studies used in this manuscript. This would be the data references in Table S1, plus Lan et al. 2015.

Specific Comments:

Line	
#	Comments
62	Would be useful to state what the LOD of Bridger GML is here.
106	"1,898 facility-level…" I am a bit confused since Table S1 only sums to 1866 observations.
127	"high-emitting intermittent are included" → "high-emitting intermittent sources are included"
Fig. 1	There appears to be a linearly decreasing relationship between the loss % and production rates for well-sites (facility category 5-9). Is this real? Is there a reason to include this in the facility-level model?

226	"… gas flared for 2021 by Elvidge et al. (2016)… efficiencies from Plant et al. (2022)" Are these the correct references? It seems unlikely that Elvidge et al (2016) published gas flaring for 2021.
254	"…production well sites that we use in this work generally do not show significant…" → "… basin-to-basin, production well sites in …"
352	"… Ravikumar et al. (2019) From …" → "…Ravikumar et al. (2019). From…"
Fig 4	What do the error bars represent? 95% CI?
534	"our results show the essentiality of expanding beyond solely on super-emitter mitigation". Some sort of grammatical correction needed.
538-540	It would be nice to provide the sample size of these studies.
Table S1	Appears to be missing a reference to Lan et el. 2015. There are several other references used by Omara 2018 not included in this study. (Goetz et al 2015, ERG 2018)
Table S2	The total number of well sites for the Barnett basin is 32 wells less than the sum of the bins. I assume this is the 32 wells measured by Lan et al. (2015) that was not included in Table S1.