

## ***Interactive comment on “Inter-comparison of Atmospheric Trace Gas Dispersion Models: Barnett Shale Case Study” by Anna Karion et al.***

### **Anonymous Referee #1**

Received and published: 27 November 2018

Review Comments: Karion et al, 2018.

I enjoyed this paper, and I feel that this manuscript brings together some excellent work highlighting some of the potential pitfalls of emission flux modelling using dispersion models. I recommend publication of this work, subject to some very minor corrections / edits.

The paper is well structured and very readable, with enough information generally being given to satisfy both the observational and modelling communities.

My most significant negative comment regards the current state of the conclusions. I feel that on their own they do not really show off the depth or value of the work presented. Currently, the conclusions hide behind the “we need to more research”

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caveat, whereas I feel the manuscript has the detail within the results and discussion to put something more brave and definitive into the conclusions. I would ideally see this as some sort of recommendations / best practice for selection and set up of model given these circumstances. I'm sure that I'm not alone in reading papers "Abstract-Conclusions-Discussion-Other" and a stronger conclusion will draw more readers into the meat of the manuscript.

\*\*\*\*\* Very minor comments:

Abstract: It would be good to have the models used in the investigation named in the abstract. L31: Can you put a more definitive number rather than "significant" to describe the variability.

Methods:

P3. L3. Have any other models been used to successfully simulate CH<sub>4</sub> observations? From the way it is written it makes it seem that WRF-chem is the only one thought to be capable.

P4. L17. Why use the Texas Railroad definition? Is there not a more appropriate definition from the USGS or similar?

P6. L9. "The footprints. . ." Could this be summarised into an equation form? It feels quite a wordy explanation of the calculation process? The same comment also applies to P7. L13-15.

Note, editorial. Some of the equations do not print properly (e.g. P6. L9), despite it being in pdf format. There may be an issue with the way the equations are encoded, please check as this may be an issue local to my computer or a more general issue.

P7. L6. The CH<sub>4</sub> inventory was provided at 0.1 degree resolution, this reads as if there were options on the resolution? Were there options, and if so why was this chosen?

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P9. L6. Could a small paragraph be added to explain how the errors from the transport model would be expected to influence the data and what magnitude of errors are expected to exist from the transport model?

Results:

P11. Figure 2. I find it very difficult to see the difference in the colours between NAM-HYSPLIT and WRF-HYSPLIT. Could you look into changing the colour scheme to make them all more distinctive (same applies to other figures where same colour scheme used)

Discussion:

P20. L4. Can you please define “very different” with a numerical value

P20. L19-26. This feels like a very important part of the discussion. Is there any recommendation / specific experimental set up that could be recommended which could help resolve this?

P22. L12. This is excellent, and I feel should be given more prominence in the conclusion.

Conclusion:

See comment at start of review.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-736>, 2018.

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