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Interactive comment on "Observing Entrainment Mixing, Photochemical Ozone Production, and Regional Methane Emissions by Aircraft Using a Simple Mixed-Layer Model" *by* Justin F. Trousdell et al.

Anonymous Referee #1

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The paper by Trousdell describes new aircraft measurements, which, combined with the mixed layer budget equation, attempts to constrain entrainment, advection, and the emission/production of ozone, methane and water. The dataset and analysis could be suitable for ACP, but as it stands the paper tries to address too many disparate issues: entrainment, the ozone budget, the methane budget, surface heat fluxes and the water cycle. In my opinion the paper needs to be significantly modified before publication.

As a consequence the findings are often not discussed in depth and put into context of uncertainties. A major uncertainty, that needs more evaluation, is the fusion

of in-situ observations with large scale reanalysis data. What are the uncertainties of this approach? E.g. when extracting mean vertical wind speed or surface fluxes from NARR, and plugging these data into eqs. (4),(6), etc., to extract small residuals of the observed quantities. Generally the paper lacks a consistent analysis of error propagation, which makes it hard to follow the uncertainty of the complex method of extracting tracer budgets.

Section 3.2.1: The ozone budget has to be corrected and time-shifted due to rapid photochemistry. Is this done arbitrarily to minimize residuals? Ozone production: methane is used as a VOC tracer to demonstrate that P(O3) is NOx-limited. Yet methane is not a very good tracer, because it has quite different sources compared to VOCs emitted from transport and combustion processes (e.g. aromatics). In addition biogenic VOCs are not considered at all by this approach. Methane is a fugitive emission and therefore does not represent the variation of VOC reactivity properly. To make a more convincing point the authors should use data from the parallel SEACRS mission or ground based observations in combination with a photochemical model to show what fraction of OH reactivity is due to methane (likely very small) and whether methane significantly co-varies with the local VOC reactivity.

In the following section (3.2.2) methane emissions are discussed, but given the uncertainty of the local methane budget (e.g. 100 +/-100Gg/yr), one wonders about the significance of the results. Again, without proper error propagation it makes it hard to follow the validity of the approach, especially uncertainties originating from the model-data fusion. The reader is left with the impression that the approach relies on luck and a fair wind.

Section 3.2.3: Surface latent heat flux: In my opinion this part of the paper presents the most interesting aspects, as it shows a significant bias of surface fluxes obtained from re-analysis data. Why do the authors not present a more in-depth analysis of this finding? Section 4: Rather arbitrarily 5 lines of error analysis are presented here, but only address a very small part that would be necessary for the entire paper.

Generally, in my opinion the paper tries to address too many disparate issues and therefore lacks in depth analysis of the individual pieces. For a focus on ozone, the authors should definitely combine their results with a more comprehensive set of chemistry observations, which seem to be available. For a focus on entrainment and PBL dynamics, a PBL model should be used in conjunction with the budget equation. The paper would also greatly benefit from a more thorough discussion of the associated uncertainties when closing the PBL budget. Perhaps a useful resource to better constrain the thermodynamical and dynamical properties of the PBL during the research flights, and address the propagation of errors and uncertainties, can be found here: http://classmodel.github.io/

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