Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-322-AC2, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Direct Inversion of Circulation and Mixing from Tracer Measurements: I. Method" *by* Thomas von Clarmann and Udo Grabowski

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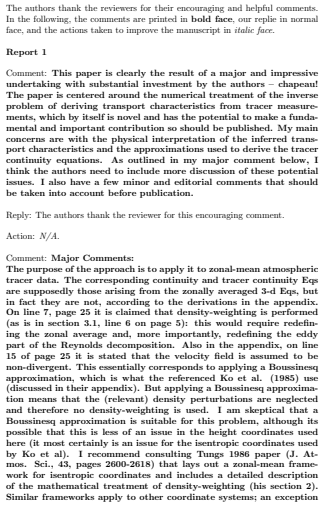
Received and published: 7 October 2016

The reply is found in the supplement.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2016-322/acp-2016-322-AC2-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-322, 2016.



The authors thank the reviewers for their encouraging and helpful comments. In the following, the comments are printed in **bold face**, our replie in normal face, and the actions taken to improve the manuscript in *italic face*.

Report 1

Comment: **This paper is clearly the result of a major and impressive undertaking with substantial investment by the authors – chapeau! The paper is centered around the numerical treatment of the inverse problem of deriving transport characteristics from tracer measurements, which by itself is novel and has the potential to make a fundamental and important contribution so should be published. My main concerns are with the physical interpretation of the inferred transport characteristics and the approximations used to derive the tracer continuity equations. As outlined in my major comment below, I think the authors need to include more discussion of these potential issues. I also have a few minor and editorial comments that should be taken into account before publication.**

Reply: The authors thank the reviewer for this encouraging comment.

Action: *N/A.*

Comment: **Major Comments:**
The purpose of the approach is to apply it to zonal-mean atmospheric tracer data. The corresponding continuity and tracer continuity Eqs are supposedly those arising from the zonally averaged 3-d Eqs, but in fact they are not, according to the derivations in the appendix. On line 7, page 25 it is claimed that density-weighting is performed (as is in section 3.1, line 6 on page 5): this would require redefining the zonal average and, more importantly, redefining the eddy part of the Reynolds decomposition. Also in the appendix, on line 15 of page 25 it is stated that the velocity field is assumed to be non-divergent. This essentially corresponds to applying a Boussinesq approximation, which is what the referenced Ko et al. (1985) use (discussed in their appendix). But applying a Boussinesq approximation means that the (relevant) density perturbations are neglected and therefore no density-weighting is used. I am skeptical that a Boussinesq approximation is suitable for this problem, although its possible that this is less of an issue in the height coordinates used here (it most certainly is an issue for the isentropic coordinates used by Ko et al). I recommend consulting Tungs 1986 paper (J. Atmos. Sci., 43, pages 2600-2618) that lays out a zonal-mean framework for isentropic coordinates and includes a detailed description of the mathematical treatment of density-weighting (his section 2). Similar frameworks apply to other coordinate systems; an exception

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Fig. 1.