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Discussions

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

Anonymous Referee #1

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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.

Major Comments:

The purpose of the approach is to apply it to zonal-mean atmospheric tracer data. The corresponding continuity and tracer continuity Eq's are supposedly those arising from the zonally averaged 3-d Eq's, 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 it's 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 Tung's 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 are pressure coordinates (or log-pressure coordinates) because they implicitly include mass/density-weighting.

I think these issues should be discussed in the appendix. I recommend focusing on the conceptual points (approximations needed to make eddy tensor independent of tracer; interpretation of eddy tensor and symmetric / antisymmetric components as additional mixing / mean advection); the detailed mathematical treatment is not necessary I think or at least can be significantly condensed.

Related to the above, I think the main description in section 3.1 needs more physical interpretation. The meanings of "effective transport velocity" and "eddy diffusion" are not unique, but depend on the kinds of approximations and treatment of perturbations outlined in the appendix. A different zonal mean / eddy treatment and different approximations would result in different effective transport velocities and eddy diffusivities. In fact, I would argue that the only unique definition of "effective transport velocity" would be one that sets the eddy diffusivities to zero (ad hoc), thereby absorbing all resolved transport into zonal-mean advection. After all, "eddy diffusion" is a parametrization of eddy advection and almost all transport is in reality advective (although, of course, some small-scale / sub-grid scale diffusion is still necessary to close the system). I am sure the authors have thought about the physical interpretation of their deduced transport contributions and I would like to see some of those thoughts discussed somewhere in the paper. The way it's currently written makes it sound too much like the inferred transport characteristics are unique.

Minor Comments, incl. editorial:

page 1, line 15: "are under debate" feels out-of-date: as suggested by the introduction and by the other reviewer comments, a lot of what seemed to be a debate at first has become a more detailed and nuanced description

page 1, line 21: "is not that one-dimensional": somewhat confusing what this refers to (I suppose it refers to the spatial variability in age trends - see following sentence in text)

page 1, line 24: I think you either need to spell out the "ad hoc assumptions" or remove this remark

page 2, line 6/7: I think most models in practice use the Earth's surface as a reference (see e.g. CCMVal reports)

page 3, line 11: suggest "source and sink terms" for clarity

page 4, line 2: "shallowness approximation": it will help some readers to spell out the approximation (i.e. z much smaller than r_E)

page 7, line 28: differentiate

page 10, line 4: I'd prefer "identity matrix"

page 12, line 4: equal to an integer multiple . . .

page 12, line 12: I suggest "real atmosphere"

page 19, line 22/23: velocities should be v and w, subscript for K_ϕ (also Fig. caption of Fig. 3)

page 20, line 13: "roughly consistent" should be specified/quantified more - in what way are they consistent (list a few examples, such as overall latitudinal and vertical structures etc)?

page 20, line 14/15: "shows many detailed features demanding scientific investigation in their own right": which detailed features and why to they demand investigation? I think this should be discussed in the text.

page 20, line 17: should be "mixing coefficients"

page 20, line 25: weekly

page 20, line 33: view _of_ the same problem

Fig. 1: right-hand column misses a color bar; labels are hard to read - please increase font size

Fig. 2 caption: "a detail of this" - I suggest "a zoomed-in view"

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