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Atmospheric Measurement Techniques Discussions

Interactive comment on "Assimilating airborne gas and aerosol measurements into HYSPLIT: a visualization tool for simultaneous assessment of air mass history and back trajectory reliability" *by* S. Freitag et al.

Anonymous Referee #2

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1 General remarks

The authors have investigated airborne atmospheric trace substance measurements taken over the tropical Pacific using back trajectories. They are selecting trajectories according to different criteria, and plot them colour-coded. Furthermore, they investigate numerical accuracy by comparing forward and backward trajectories, and wind as used for trajectory calculation and as observed by the research aircraft. There is

extensive discussion of the results.

All this is not really novel, but I think the analyses and discussions are useful, as they may inspire others to use similar methods, especially given that MATLAB source code for facilitating that is provided, and as the paper also contains scientific results. Thus I think it can be published, considering the following remarks.

I want to add that I am neither a MATLAB nor a HYSPLIT user and thus I have not reviewed or tested the MATLAB scripts provided in the supplement.

2 Specific remarks – major issues

1. The title is too long and not appropriate. There is no assimilation taking place. The claim that trajectory reliability can be assessed is not sufficiently justified (see item 4 below). I would thus suggest a title like "Interpretation of airborne gas and aerosol measurements using back trajectory visualization: a case study from the tropical Pacific".

2. The literature discussed in the Introduction is too much focused on North-American authors. It would be good to be a bit broader, even though it is admitted that it makes no sense of trying to be exhaustive as there are too many relevant papers. One example of an important work not referred to would be Lawrence et al. (2003).

3. I think the manuscript, and especially Section 2, could be shortened.

4. Page 5362, last paragraph: The relatively good agreement between forward and backward calculations with respect to the vertical position is attributed "to the fact that vertical motion is generally constrained by potential temperature (Newell et al., 1999)." While it is true that atmospheric motion, as long as it is laminar

and adiabatic, is constrained by potential temperature (i.e., isentropic surfaces are material surfaces), this does not affect kinematic trajectory models. To my knowledge, HYSPLIT in standard set-up uses modelled vertical velocities and not the assumption of constant potential temperature to determine the the vertical displacements. Should the authors have used an isentropic option for HYSPLIT, it would be important to state that clearly. In addition, I don't find any pertinent information in the cited *"Nature"* paper by Newell et al. (1999).

5. Looking at many of the figures, for example at Fig. 6, we see a sudden ascent of air from 1–2 km up to about 4 km. As convection is not considered in HYSPLIT, I am wondering how this is possible. What is the vertical velocity implied, and is it compatible with what the underlying meteorological data can resolve? Some clarification of what is happening here is needed. The wording on page 5370, mentioning "various error sources (see discussion in Sect. 3.2) complicate accurate representation of (backward) transport through deep convective clouds", is not very clear: do the authors want to say that the modelled rise is due to convective clouds? Or do they want to say that the rise will be different in reality where convection is active, whereas it is not in the model?

6. Conclusions (Page 5377) "As an alternative, we suggest replacing these evaluations of trajectory errors with the visualization of in-situ tracers superimposed upon back trajectories. An examination of consistency can be carried out by interpreting the conjunction of these high-frequency trajectory 20 and tracer data sets." While an examination of consistency is certainly possible, and this is also useful and can serve as a way of investigating the reliability of computed trajectories, the conclusion that this qualitative method can replace quantitative error evaluations is too far fetched and not sufficiently supported by the analyses. Also, the value of the paper does not depend on this claim.

3 Specific remarks – minor issues

1. Abstract, line 1: Back trajectories are commonly used as tools for the interpretation of atmospheric measurements in general, not only for airborne observations.

2. Abstract, line 23: inserting "that" after "reveals" would improve clarity.

3. Page 5350, line 19: should "number" be "number concentration? Probably also a few lines below, "surface area" means "surface concentration"?

4. Page 5357/8: "modeled HYSPLIT precipitation", repeats on P. 5359 line 5 and elsewhere. Please not that the precipitation values output by HYSPLIT are GDAS precipitation—HYSPLIT does not calculate its own precipitation. Same for wind etc. It would be more appropriate to refer to "modeled precipitation (GDAS data) along the HYSPLIT trajectory paths" or similar. A similar issue is the the wording "spatial and temporal resolution of the trajectory model is limited" on p. 5349. A trajectory model does not have a grid resolution, it is Lagrangian, only the underlying meteorological fields are gridded.

5. Section 3.2 Discussion of model errors: This discussion mixes effects of trajectory errors and general shortcoming of the mean-wind trajectory approach for atmospheric transport and dispersion modelling. Deformation, turbulence and convection don't affect the accuracy of trajectories as they are per definition movements of idealised, infinitesimally small air parcels caused by the mean wind. They will, however, limit the usefulness of the trajectory approach, and if important, would call for replacement by a Lagrangian particle dispersion model. This latter option should at lest be mentioned. I would even say, one may seriously wish to consider that for applications such as presented here (cf., for example, the Stohl et al., 2002, paper mentioned in the introduction). Please note that the forward/backward test will not give "exact" numerical errors, though it will certainly give useful estimates.

6. Page 5362, Line 16: Transport errors, estimated by the fwd/bwd method, are presented in terms of percent of trace distance. This is an established and useful parameter, however, the related Table 2 does not provide this measure. Please add.

7. The caption to Fig. 4 should explain the content, and not just refer to the text for explanation.

Reference

Lawrence, M. G.; Rasch, P. J.; von Kuhlmann, R.; Williams, J.; Fischer, H.; de Reus, M.; Lelieveld, J.; Crutzen, P. J.; Schultz, Martin; Stier, Philip; Huntrieser, H.; Heland, J.; Stohl, A.; Forster, C.; Elbern, H.; Jakobs, H.; Dickerson, R. R., Global chemical weather forecasts for field campaign planning: Predictions and observations of large-scale features during MINOS, CONTRACE, and INDOEX, Atmos. Chem. Phys., 3, 267-289, 2003.

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