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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Refinements in the use of equivalent latitude for assimilating sporadic inhomogeneous stratospheric tracer observations, 1: Detecting transport of Pinatubo aerosol across a strong vortex edge" *by* P. Good and J. Pyle

Anonymous Referee #2

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General Comments:

In this paper, a presumed near-tracer quantity, derived from six stations' lidar aerosol measurements, is used to characterize the uncertainty in equivalent latitude, as calculated from analyzed potential vorticity transported by an Eulerian model. Instead of a tight (piecewise) correlation between the tracer and equivalent latitude, some scatter is introduced by random error of the measurements, and some scatter is introduced by measurements being assigned to wrong values of equivalent latitude. Near the polar

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vortex edge, where the random scatter in the measurements is much less than the difference in the measurements across the edge, the scatter seen tells us mainly about the error in the equivalent latitude. By constructing PDFs of the tracer measurements as a function of equivalent latitude, the authors determine the spread of the calculated equivalent latitude values about their true values, and estimate the true equivalent latitudes themselves.

This work is very interesting and, I think, represents a valuable and original contribution to the literature. However, the paper does not always communicate clearly, and some points need clarification. I urge the authors to make certain (relatively minor, I think) revisions before the article is accepted for publication.

Specific comments:

There are only a couple of science issues that I believe need to be addressed.

First, data are used from only six stations whose latitudes vary from 44.5 N to 78.9 N. The small number of stations would seem to make the analysis vulnerable to variations in lidar instrument or technique masquerading as latitude variations. That is, even though the random error in R is estimated at 20 to 30 %, might there not be systematic errors in one or more stations which would skew the PDFs when all of the measurements are counted together? Are some of the apparent changes in time actually shifts in the number of measurements from one station compared with another? How difficult would it be to rule out this possibility, say by comparing different stations at similar equivalent latitudes?

Second, in Figure 6, the authors show that the equivalent latitude of the aerosol tracer shifts in time over the month of January. However, to show that cross-vortex transport has taken place, we need to be able to see the movement of the vortex edge itself. The reader can sort of see this in the spacing of the lines in Figure 6, but as the authors point out the width of the edge region obtained from these lines is terribly wide. It would perhaps be helpful to overlay (as an extra, gray line in the figure) the equivalent latitude

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of the vortex edge as obtained from the meteorological analyses.

Third, it is not clear that the error in equivalent latitude, evaluated near the edge of the polar vortex, is necessarily the same as the error poleward or equatorward of the edge. After all, as different dynamics come into play, it is plausible that the PV values from which the equivalent latitudes are calculated might have different error statistics.

The paper is organized well, and the presentation of the data and the analysis technique are reasonably clear in most places.

Some parts, however, could be made a little clearer for the general reader. I would suggest augmenting Figure 1 with one or two small histograms of measurements vs. equivalent latitude, for small bins of aerosol tracer. Then explain how the cumulative probabilities are more statistically reliable, and identify peaks in the Gaussian-like histograms with the edges in the PDFs of Figure 2. This adds no new substance to the discussion, but it would improve the flow of thought a little. (This is only a suggestion.)

The relationship between sections 3.1 and 4.1 is somewhat unclear. It appears that in 3.1, estimates of the true equivalent latitude obtained in 4.1 are used to shift all the PDFs to the same equivalent latitude, so that the width of the presumed Gaussian can be estimated. But one of the two methods in section 4.1 is a curve-fitting procedure that estimates the true equivalent latitude using a given value for the width of the Gaussian. Is this an iterative procedure? A sentence in the second paragraph in section 5 seems to indicate this, but a little more specific discussion here would be helpful.

The discussion from the last paragraph of section 4.1.3, through section 4.2, is very confusing and unclear, and would benefit from rewriting. In 4.1.3, of what quantity is "an appropriate time mean" to be taken? And what exactly is meant by "chosen to go with?" In Section 4.2, how are the assumed "6 completely independent comparisons" related to the four time periods listed in this paragraph? Or do the six include the 0-30 and 10-40 periods not listed? The next paragraph refers to a point in the PDF at equivalent latitude 71.25 as dominating the fit for days 0-30. But casual inspection

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of Figure 2 gives no obvious indication of anything special about that point. Now, I believe that I do understand what the authors are trying to say here, and why the point at 71.25 is throwing off the curve fit. But it really ought to be explained more clearly and explicitly.

In last paragraph of section 4.3, after the paper presents the vortex edge gradient from the aerosol tracer as 11 degrees wide, I find the rest of the paragraph confusing. Are the authors saying that there is no difference between the 11 degrees and the much tighter gradient seen in the PV analyses? Or are they saying that the difference exists, but that it is not statistically significant? (Such a statement would seem to call for a little more explanation and discussion.) Or are the authors saying that there is a difference, and that difference is caused by mixing across the edge and perhaps also by sedimentation?

Technical corrections:

p. 1, column 2, line 15: "ground base tracer observations" should be "ground-based tracer observations"

p. 1, column 2, line 27: "cooridinate" should be "coordinate"

p. 3, column 1, line 18, and elsewhere: single quotes are used where double quotes are preferred. Also, the opening quote mark is backward; if the authors are using TeX (this is a common mistake in TeX), remember to use the correct quote marks.

p. 4, column 1, line 35: Do we really have to use the term "Julian day" to refer to "day of the year"? They are not the same.

p. 5, column 2, line 20: "having gained an understanding of the error". I think a more accurate phrase might be "having characterized the error". I'm not convinced that we truly understand the errors any better than before. But we do have a way of estimating how large they are.

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Interactive comment on Atmos. Chem. Phys. Discuss., 4, 635, 2004.

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