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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new real-time Lagrangian diagnostic system for stratosphere-troposphere exchange: evaluation during a balloon sonde campaign in eastern Canada" *by* M. S. Bourqui et al.

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Bourqui et al present in their manuscript a new real-time diagnostic for stratosphere-troposphere exchange (STE). Several aspects are noteworthy in this study: (a) the Lagrangian STE diagnostic is based on an operational high-resolution weather forecasts, (b) a careful discussion of uncertainties in the methodology is offered, and (c) the model-based STE events are validated against observations at three different stations. The presentation of the method and the results is clear, and there is a good balance between text and figures: all essential pieces of information are adequately

shown in figures.

One major point concerning the overall structure of the manuscript might be the following: Section 4.4 is a rather detailed discussion of the internal structure of the stratospheric intrusions and how well it is represented in the Lagrangian STE diagnostic. It was a little 'too detailed' for fluent reading! I wonder whether this part can be somewhat shortened, and possibly be combined with section 4.3. On the other hand, section 5 is a rather interesting and, I think, enthusiastic discussion of STE forecast errors. I like this section, and also like very much the cluster analysis of missed events! It would deserve a more prominent place in the manuscript - I would 'vote' for a place before the much more 'technical' section 4.3. But of course, there are also good reasons for the present structure...

In summary, considering the already well-written state of the manuscript I suggest that it can be published with minor revisions, which are enlisted below. Most of them are requests to clarify certain statements.

MINOR COMMENTS:

L37: The listing of physical processes related to STE ends with a numeric process, numerical diffusion. This is somewhat unhappy, because numerical aspects should be separated from true physics.

L40-43: The term 'chemical gradients' is readily understood, but not very fortunate. More correctly, it should be 'the gradient of the chemical constituents'. Furthermore, I think the same statement (L40-43) would also be true if the gradients are not so large.

L45: Is a resolution of 0.5 deg sufficient to resolve all of the above mentionend physical processes (L36-37). Probably not, but the text suggests that this is the case.

L52: It might be helpful for the readers not familiar with 'residence time' to explain it already at this place with one sentence.

L67-69: Here it is suggested that Wernli and Bourqui (2002) and Sprenger and Wernli

(2003) significantly underestimate the frequency of deep STE events. But in these studies a different residence time (96 h) was applied compared to the present study, which enforces a 12 h threshold. I guess that the discrepancy can be explained by this different choice of residence times. Please comment!

L104: 'within five succesive 24 h time windows with a 12 h residence time' -> at first reading the meaning of the sentence is somewhat difficult to grasp. Please reformulate!

L114: What is the 'spatial density of initial trajectories'? More precisely, it would be the spatial density of the initial trajectory points. To put it otherwise: by definition, a trajectory is a whole path in space and time, and cannot be used to refer to a single point/time step along the trajectory.

L145: Low-level PV anomalies with PV > 2 PVU might mimic a stratosphere, but are actually of tropospheric, diabatic origin. How are such tropospheric PV anomalies handled? Furthermore, the definition of the of STE needs a clarification. What if both, the 2-PVU and the 380-K isosurface, are crossed? Obviously, what is meant is that the lower crossing counts. Right?

L160, 172-177: Some reference is made here to the T->S calculations. But the whole study relies only on the S->T trajectories. Indeed, the restriction is motivated in L174-176, and is perfectly ok. I simply wonder whether any reference to non-used products of the methodology must already be described here. I would prefer to skip these parts.

L236-243: Here, the objective STE identification is described. I wonder a little why ozone gradients are used in the criterion, but absolute ozone concentrations are not. Is there a specific reason for omitting [O3]?

L251: At this place place it becomes not clear why the comparison with the Lagrangian STE data is simplified by the choice of 50-hPa bins

Figure 1: The thermal tropopause is shown, most likely because it can be derived on the measured temperature alone. On the other hand, the Lagrangian STE diagnosis

uses the 2 PVU/380 K tropopause. Would it make sense to include this dynamical tropopause also in Figure 1?!

Figure 2: The upper-most intrusion might more reasonably be called 'the stratosphere'! How is this handled in the validation of the method? Is the highest high-PV reservoir taken as an intrusion, or is it excluded from the validation because it is the lower part of the stratosphere and hence deserves special treatment?

L182-283: The number is about two orders of magnitude larger than estimates in Sprenger and Wernli (2003). But note that the latter study is climatological in nature, and hence considerably lower values must be expected. It would be interesting to compare the value in the manuscript with Sprenger and Wernli (2003) on an event basis. Furthermore, note again the different residence times which are applied. In short, a comparison with Sprenger and Wernli (2003) is difficult, and it should be stated so.

Figure 3: This is an important figure of the manuscript; and it presents two quite different pieces of information - if I correctly grasp it: (i) the frequency of bins in the different categories; and (ii) the distribution of RH, Q and O3 within the categories. Aspect (i) is clearly discernible, but could easily be shown in an extra row - note that the frequency is the same for all three rows. On the other hand, aspect (ii) is partly rather dificult to see; e.g. RH in the intermediate (top) bin. As a remedy: If (i) is shown in an extra row, (ii) could be shown in the next three lines, but now each bar would be equally high and represent 100 %.

L323-324: Here the initial grid of trajectory starting points (55 km, 5 hPa, 24 h) is compared to the mapping grid of STE mass fluxes (2x2 deg, 50 hPa, 24 h). Are the numbers for the 'mapping grid' subjective, based on your experience, or are there some objective criteria which define the mapping grid in terms of the initial grid?

L371: 'In general, the PC score artificially increases in cases of low event frequency.' -> A eference might be made to the pitfalls of skill scores based on contincency tables. See for instance, "The Finley affair: A signal event in the history of forecast verification.

Murphy - Weather and Forecasting, 1996"

Figure 4, L405-406: Within several time periods the thermal tropopause is rather high (e.g. 15 July)? I wonder where the dynamical tropopause based on 2 PVU/380 K is situated for these periods. My guess is that the dynamical tropopause behaves quite differently, i.e. that it is even found at lower-than-average heights during these periods. If so, I wonder whether it is even worthwhile to show the thermal tropopause? Please comment!

L417: '(3, left column)' -> confusing! Most likely, you mean (Table 3, left column)?

Figure 6: Above 300 hPa (dark blue), the number of occurrences drops from 24 at 48 h quite dramatically to 7 at 60 h. Is this sharp drop due to the small sample size, or is there a good physical reason for it?

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