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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.

Anonymous Referee #2

Received and published: 13 December 2011

General Comment: The work by Bourqui et al. (2011) presents a high-resolution real-time Lagrangian system to diagnose global STE. A regional sub-set of STE occurring during summer 2010, have been compared to ozone-sondes measurements over eastern Canada. The new diagnostic system here presented can represent a valid tool in the study of STE (and related impacts on troposphere chemistry, which is well into the ACP scope) and the results are presented in a clear way. The identification algorithm basing on the sounding data and the analysis of STE forecast errors are extremely interesting also for other application related to STE investigations. However, I think that

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the short time duration of the experimental campaign prevents this global system to be fully evaluated and validated. Moreover, as the “core” of the paper is the presentation of the real-time system and the “regional” comparison with observations in the eastern Canada, I’m wondering if the paper can be more profitably presented as a “Technical note”?

Principal remarks

1.The comparison with the measurements only cover about 20 consecutive days during summer season. Thus, no information about the skill of this system are provided for other seasons characterised by different meteorology which can also affect STE features (both in term of frequency and modality of occurrence). Probably, the authors should provide more information about the meteorological patterns observed over East Canada during the measurement campaigns to assess the capacity of their system in “catch” STE under different modality of stratosphere-to-troposphere transport.

2.As also reported in the abstract the authors claimed that “the predictive skill for the overall intrusion depth is excellent for intrusions penetrating down to 300 and 500 hPa”. As reported in the conclusions: “ the statistical bias was found to be slightly positive in the upper troposphere”. Since the authors indicated that 89% (79%) of days showed signature of stratospheric intrusions below 300 (500) hPa, I suspect that the “excellent” predictive skill in the upper troposphere can be simply due to the fact that almost for the entire measurement periods STE signatures were present over East Canada. Please comment on that! Also for better clarifying this point I think that a more extended validation exercise should be done before claiming “excellent” skill” (to my knowledge ozone-sondes are lunched routinely at Egbert with a coarser time frequency, is there any possibility of comparison even only for a specific event during a winter month?) or at least conclusions should be presented in a more and more cautious way. Finally, as reported on page 27974, the evaluation was restricted only to the trajectories started at 00:00 UTC and representing exchange occurring within [12h, 36h[. This should be better stated both in the abstract and in the conclusions.

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3. Is there a possibility that the methodology for identifying intrusions from ozonesondes could overestimate the actual occurrence of events. Did you perform a sensitivity study by changing the threshold values described in the paragraph 3.2 (for instance decreasing the RH threshold value)? Moreover, can you compare your results with pre-existing studies about STE climatology over Canada/North America?

Specific remarks

Abstract, line 28: " A significant low statistical bias. . .is found in the layer..". In respect to what?

Introduction, pag 27969, line 14: numerical diffusion is more a modelling issue that a process of STE.

Pag 27971, line 5: please specify the scenario.

Pag 27972, line 6: Actually is not the first time that a Lagrangian STE data-set on global forecast has been used. See please" Trickl, T., Feldmann, H., Kanter, H.-J., Scheel, H.-E., Sprenger, M., Stohl, A., and Wernli, H.: Forecasted deep stratospheric intrusions over Central Europe: case studies and climatologies, Atmos. Chem. Phys., 10, 499-524, doi:10.5194/acp-10-499-2010, 2010 "

Page 27972, line 11: ". . .within five successive 24h time windows with a 12h residence time." It is not clear to me. Please rephrase.

Page 27973, line 8: please specify "EC"

Page 27973, line 23: it is not clear to me why, along the 6-day trajectory, only the time window [12h, 36h] was analysed for STE occurrence.

Pag 27976, line 15: "three large deep intrusion events"

Pag 27981, line 14: I wouldn't indicate O3 as "passive "tracer"!

Pag 27986, line 5: "Above 300 hPa, the predictive skill is excellent". According to Fig.

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1, basing on the selection methodology, almost all the days presented an intrusion. Thus, are your sure that the forecast system is really excellent or simply it always “see” STE?

Pag 27986, line 26: I would say simply that :” the STE data not provide useful predictive skill.”

Pag 27987, line 23: “. . .which is well captured with about 5% overforecasts”. The forecast system strongly underestimate intrusions in the lower troposphere below 700hPa (which are quite rare events). Thus, this “good” result can be simply related to the high number of non-intrusion day.

Pag 27990, line 27: “it is likely that these errors cancel on climatological averages”. Please explain why.

Pag 27993, line 22: Also for the reasons you explained later in the text (Pag 27994, line 11), I’m not convinced that only using RH and O3 an intrusion can be identified with accuracy. Please, rephrase

Pag 27994, line 17: You should add a comment (and a sentence in the abstract also) about the fact that the evaluation can be influenced by the large occurrence of STE diagnosed by the ozone-sonde algorithm.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27967, 2011.