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Interactive comment on "How stratospheric are deep stratospheric intrusions? – LUAMI 2008" *by* Thomas Trickl et al.

Anonymous Referee #1

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General comments:

This paper presents a case study of a stratospheric intrusion observed over Germany by a set of instruments during the 2008 LUAMI field experiment. It is, in my opinion, one of most detailed stratospheric intrusion analysis I have ever seen. However, the question raised by the article's title "How stratospheric are deep stratospheric intrusions" is not answered. The paper falls short of giving convincing conclusions on how to proceed to estimate Stratosphere to Troposphere Transport (STT) flux of stratospheric air masses based on the observations used in the paper.

I'm not convinced by what this paper has to offer in terms of new findings or method to estimate STT flux. What knowledge do we gain from this case study compared to the existing literature on the subject, and Trickl et al. (2014) in particular, that is worth

being published in ACP? How can we extrapolate the results of this single event to other STT events in the mid-latitudes in general?

Even though this case study is well presented, I think that a lot of effort is still required to convince me that we may have a way to quantify the STT flux of this case study based on observation alone to make this article publishable in ACP.

Specific comments:

1) In the conclusion, the authors say "the results presented . . . are an important prerequisite on the way to quantify STT based on observational data alone". From the conclusions on the observation and modeling efforts, I don't see how those results show any important prerequisite. Since no STT flux has been quantified, I still have doubt on our capability to estimate STT flux based on observations only. In fact, it is not possible unless the authors have a wind profiler that would provide the wind speed and direction of the stratospheric intrusion. Otherwise, the authors will need to use the 3D wind fields from a global model (e.g. ECMWF).

2) The conclusion on the observations is interesting. However, it doesn't help the reader to estimate how much information do we gain from each measurement techniques to subsequently quantify the STT flux of this case study. What do we need exactly to quantify the STT? Continuous LIDAR measurements from a ground site? LIDAR measurements on board an aircraft? High-resolution radiosondes? all of them? How does it potentially affect our capability to estimate a STT flux if one has access to part of those measurement techniques? A discussion is required.

3) Conclusion based on the modeling work: The trajectories show that the air mass comes from the vicinity of the tropopause. However, the authors didn't explain why the top of the intrusion layer had enhanced ozone and aerosol compared to the lower part. How is it possible that the lower part of the layer, which is supposed to be stratospheric, has no enhancement in ozone and aerosol?

The main hypothesis in the paper is that an air mass with 50ppmv of water vapor has a stratospheric origin. In Vaughan et al. (2005, ACP), it is shown (figure 5, a ozone/water vapor scatter plot) that dry air masses with 50ppmv of water vapor can be associated with low ozone. It means that an upper tropospheric air mass can have very low water vapor concentration in the mid-latitudes.

From the early 2000s (e.g. Cooper et al. 2002 and reference therein), we know that coherent air streams (in the Lagrangian sense) with distinct dynamic and chemical signatures exist in frontal systems. We know that the so called dry aistream, originating from the upper troposphere, is a downward propagating air mass behind the cold front with a dry air signature. The dry air stream is clearly visible in water vapor satellite images. It is, in general, associated with a tropopause fold with a stratospheric signature.

The dry layer studied in this paper, considered to be of stratospheric origin, is probably the dry airstream of a frontal system. In this case, it makes sense that the stratospheric intrusion will be located at the top of the dry airstream as both upper tropospheric and stratospheric air masses will move downward and will follow the same vertical layering.

Therefore, the dry signature of a layer cannot be used to define the stratospheric origin of an air mass in general, which contradicts the main results of this paper. Can the authors provide more evidence that the water vapor concentration in the upper troposphere should be higher than 50ppmv?

It would be good if the authors show water vapor channel images from satellites to see exactly the synoptic situation of this event.

Reference: Cooper, O. R., J. L. Moody, D. D. Parrish, M. Trainer, T. B. Ryerson, J. S. Holloway, G. Hübler, F. C. Fehsenfeld, and M. J. Evans (2002), Trace gas composition of midlatitude cyclones over the western North Atlantic Ocean: A conceptual model, J. Geophys. Res., 107(D7), 4056, doi:10.1029/2001JD000901.

4) The justification of the paper is to provide some elements to estimate a STT flux

based on observations alone. A discussion is missing on the limitation of STT flux estimates. The authors showed that the stratospheric ozone was originating from a layer right above the tropopause, within the mixing layer around the tropopause region. Such an air mass has a short residence time in the stratosphere, and cannot be considered as pure stratospheric. How does that affect a STT flux estimate?

Using 5-day trajectories, such an air mass might be defined as stratospheric. However, a Lagrangian particle dispersion model would show that in fact the chemical composition of the air mass results from the successive mixing of tropospheric and stratospheric air masses in the vicinity of the tropopause. Such a mixing will depend on the duration of the Lagrangian trajectories. How does the author justify the use of 5-day trajectories instead of 10 days? How sensitive the results would be to the definition of the tropopause used?

6) The title of the article is misleading because a single stratospheric intrusion is analyzed in this study. I don't think a case study can help answer such question.

Technical comments:

Page 2, line 29: you mean in stratosphere in the mid-latitudes. Page 8, line 14: "around the middle of 2008": can you be more specific? Page 10, lines 23-29: the CO is reduced by 20ppb in the so-called stratospheric layer. I wouldn't say that "CO did not change much".

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