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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Forecasted deep stratospheric intrusions over Central Europe: case studies and climatologies" *by* T. Trickl et al.

**Anonymous Referee #1**

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This manuscript estimates the frequency of stratospheric deep intrusion cases forecasted with a Lagrangian model and verified by nearby measurements of ozone with a lidar or by in-situ data recorded at the Zugspitze altitude station in Germany. A method is proposed to classify the source regions of the intrusions and to determine stratospheric intrusions in the observations with filtering criteria. The main result is that the frequency of stratospheric intrusions would exceed that established in previous studies by more than a factor of two.

This is an important topic, the contribution of stratosphere-troposphere transport in the tropospheric ozone budget is still an open question. Efforts made in the present work to validate a methodology only based on measurements in order to assess this component of the budget, as well as its long-term trend, are welcome and would certainly

be of interest to ACP readers. However, the paper is far from being well written and is much too long. The rationale of the junction between section 3, that takes too many case studies, and section 4, that establishes the classification method, is indefinite. I cannot recommend publication of this study in ACP without major revisions, which must include a new design of the classification method.

General comments

1)Wordy section 3.

The difficulty of this section is that stratospheric intrusions travelling over many days in the troposphere may have a complicate history. It is shown by the inextricable hanks of spaghetti drawn by the trajectory forecasts (Fig. 3). The result is that it is often very difficult to understand the interpretations made by the authors on Figs. 2 and 3, for each case study. I recommend to make this section much more comprehensible by only displaying the most coherent and relevant airstreams on Fig. 3, and by shortening it. Case studies in this section are too numerous and a more rigorous selection, based on a modified classification method with lesser types (see below), would increase the value of the paper. Finally, although table 1 shows a classification of case studies in the different types, no explanation about the identification of the type is given, except for a very few cases.

2)Classification of the intrusions

The authors define a classification method based on the determination of the source regions with some consideration on the type of advection between the source and receptor regions. Such a method leads to too many intrusions types that can not all be studied. Indeed, only 4 types off 8 are further on characterized with Figs. 13 to 16. A more meaningful classification method, still combining the two pieces of information, would be based on the identification of the North Atlantic weather regime in which case studies are occurring. The case studies may be classified among the 4 classical North Atlantic weather regimes, like the typical European blocking dipole, or the enhanced



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zonal flow, or the positive anomaly over Greenland, or the ridge over the eastern Atlantic Ocean (Vautard, 1990 ; Kimoto and Ghil, 1993). I feel that if this is addressed the authors will make the rationale of the junction between sections 3 and 4 more coherent and the conclusions sufficiently substantial and considerably more meaningful.

3)Hemispheric CTM

EURAD simulations are not useful in the present paper. A separate paper may be written for the validation of this model, using some of the case studies picked up in section 3.

Specific comments :

Figure 1 is not a result from the present paper, it is rather an extension of the work by Stohl et al. (2000). Deprived of more detailed explanations of how such a result is obtained, it makes the interpretation of the figure difficult. Please explain how to read the y-coordinate. Is it relative or absolute values of ozone volume mixing ratio ? Stating that about 4.2ppbv of ozone is originating from the stratosphere at the Zugspitze summit (page 2230 lines 2-4) imposes to read the figure with absolute values. But then, does it hypothesise that stratospheric intrusions of different tropospheric ages simultaneously exist and add their contributions at the monthly scale ? And does it suppose that the older is a stratospheric intrusion the stronger is the ozone mixing ratio ?

Because readers at not all familiar with the geography of the south Germany, it will be nice to add some pieces of information on the respective locations of the different site experiments used in the study.

Page 2227 lines 21-23: explain why such criteria are favourable for the occurrence of stratospheric intrusions.

The paragraph spanning pages 2232 and 2233 is not very clear. Please, simplify and rewrite.

Page 2235, line 13: Please, explain the sentence: coinciding with import from North



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America to Europe.

Page 2236, lines 19-22: What do the authors mean here ? This sentence is not comprehensible. It mixes comments on ground-based measurements and lidar soundings. The end of the sentence about the role of low-lying clouds is unclear. Please, clarify.

Page 2238, lines 6-7: What do the authors call direct intrusion system ? Please, explain.

Minor comments :

Figure 2: Please, explain what is the time scale.

Figure 3: Add in the caption that the red point corresponds to the location of the Zugspitze summit.

References:

Vautard, R., 1990: Multiple Weather Regimes over the North Atlantic: Analysis of Precursors and Successors. Mon. Wea. Rev., 118, 2056-2081.

Kimoto M., and Ghil M., 1993, Multiple Flow Regimes in the Northern Hemisphere Winter. Part I: Methodology and Hemispheric Regimes, Journal of the Atmospheric Sciences Volume 50, Issue 16, pp. 2625-2644

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