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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Very high stratospheric influence observed in the free troposphere over the Northern Alps – just a local phenomenon?" *by* Thomas Trickl et al.

Anonymous Referee #1

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The paper deals with the analysis of a 9-year lidar data base (ozone UV DIAL and water vapour) to discuss the statistical occurrence of stratospheric intrusion above Southern Germany. The objectives are to extend previous estimate of the fraction of intrusion days derived from mountain top observation at 2962 m. Indeed the lidar data base appears well suited for such a task. Interesting results are discussed in section 3.4 and Fig. 13 and 14, and they deserved to be published. However the paper is not very well written with many digressions not necessary to discuss the results of the statistical analysis. The section 3.1 which is critical to understand the methodology, is not very clear and a table with the list of criteria to identify stratospheric intrusions must be provided. It is hard to see if the water vapour lidar is actually used for the

relative humidity assessment. Section 3.2 and 3.3 are either very qualitative or out of scope and could be easily shortened or removed. Overall the paper must be published but with a major revision of the presentation. There are also many self citation to previous works which are not always necessary. Finally it is a pity a similar approach is not applied to the Hohenpeissenberg ozonesonde data base to alleviate error due to ozone/humidity layer mismatch and to obtain a complementary statistical study of intrusions in the same region.

Specific comments

p1 l37: Tarasick et al. 2019 (accepted for publication in Elementa) recently show that the 10-ppb figure from early ozone observations is strongly underestimated by a factor of 2. This will increase the estimate of transport from the stratosphere when using such a number. Please acknowledge this likely underestimate in the paper

p2 l10: add Ebel et al. for regional model studies https://doi.org/10.1016/S1352-2310(97)00063-0

p2 l29Âă: Colette et al. 2006 https://doi.org/10.1029/2006GL025793 also analyzed the lifetime and mixing of laminae in the free troposphere. Add citation

p3 l5: add Kowol et al. 2000 https://doi.org/10.1029/2000GL011369d where ozone exchange near the STJ is quantified.

P3.l32: O3 precusor did not decline everywhere. Ozone still increases in Eastern Asia (see Parrish et al. ACP 2012)

p3.l22: Better to show a figure with the Zugspitze data up to 2013 for a better comparison with the lidar time period 2007-2016. Considering the rise of tropospheric ozone in Fig. 1 (30→46ppb), the direct ozone intrusion is actually decreasing from 1978 to 2004 and only the indirect contribution is increasing form 1/3 to $\frac{1}{2}$.

P4.L18-22: After a nice overview of stratospheric ozone transport at different latitude and altitude, one would expect a better description of the actual goals of this work in

the introduction and how the authors will fill the gaps identified in the introduction.

p.6 l13-20 Only half of the lidar record corresponds to the in-situ data set. How do the authors deal with this ?

p.6 l33to37 Do you mean that RH from radiosonde are used in conjunction with the O3 lidar to identify intrusion ? What is the uncertainty due to layer mismatch or radiosonde poor sensitivity in very dry layers ? Why are the Hohenpeissenberg ozonesonde not used in the analysis of your case studies ?

p.7 l25-30 Is a paragraph advertising for availability of the “intrusion hit tables” really necessary in this paper ? Better to discuss here Fig. 3 which is a good example of a LAGRANTO output.

P8. l15-18 The author cannot claim the re-analysis are better than operational analysis without a thorough discussion about the methodology to compare the two meteorological fields. The following sentence about validation of backtrajectories with observations of ozone is good enough to justify the use of HYSPLIT in this paper. Is the diabatic air mass vertical motion in the UTLS properly accounted for when using the analyzed vertical wind in 15-day trajectory calculation ?

p.9 This section is quite important because it is the backbone of the statistical analysis. It deserves a summary table to list the different criteria to identify a stratospheric intrusion: ozone and RH thresholds, from which instruments, need for forward of backward trajectories. The discussion about the humidity values within stratospheric intrusion is hard to follow. I understand a 10% RH threshold is taken. But the following discussion focuses on a list of case studies with smaller RH values. Please rewrite the paragraph l24 to 34 (emphasize that 10 % is good enough and avoid multiple references to your past studies). It is not clear if the aerosol lidar data are actually used and how. If not discussion about the aerosol lidar in section 2.1 is unnecessary and the lengthy presentation of Saharan dust or volcanic/fire aerosol transport events is not needed in section 3.2.

p.9 l.1 Better to emphasize the number of ozone measurements days (585) which is a better proxy to assess how meaningful is the statistical study. For example in Fig. 10 on May 28 there are only two clearly independent profiles while 7 are reported. Give also the number of water vapour lidar observations coincident with the ozone data.

p.9.L13 What do you mean by ozone as a secondary indicator ?

P9.L15 A 10% ozone increase seems a very low threshold considering first the measurement accuracy (5%-10%) and second the ozone variability due other transport processes (horizontal differential advection, convection).

Section 3.2 and 3.3 These two sections are way too long and is of little value for the analysis developed in section 3.4. I recommend to keep only quantitative information needed for the statistical study e.g. two examples of ozone vertical profiles: a thin and thick intrusion and the ancillary information to identify the stratospheric origin. Quantitative information of some features discussion section 3.2 can be indeed developed: - very intense intrusion are rare (define and give occurrence fraction) - thin layers are frequent (same recomm.) - slow descent are frequent above 5 km (same recomm.) - intrusion does not penetrates in PBL (how many cases ?) Discussions about the dust cases or the volcanic episode are out of scope and do not add a new perspective in the analysis. I get the feeling the authors wish to emphasize the lidar capabilities of monitoring both aerosol and ozone without making an actual use of this capability for a better assessment of the stratospheric intrusion frequency in Southern Germany. P.14 l.5-10 Do you mean that the water vapour lidar is not used ? Please clarify how you truly derive the RH needed for the stratospheric intrusion analysis. This is a critical parameter and large bias can occur if the two measurements are not co-localized.

p.14 l16-20 Please explain what you are doing here. Why do you focus on data taken on monday and thursday ? I do not see the usefulness of decreasing the number of measurements by a factor of two.

p.14 l.21-24 The uncertainty analysis of the fraction given in this part of the paper is

missing, e.g. the sensitivity to a different threshold in the ozone increase (20% instead of 10%) or to a missing criteria (trajectories or low humidity)

p.16 l.15. It is not true to say that ozonesonde yield a substantial lower temporal coverage. For a ten year period, weekly launches (several stations in Europe are doing more than 1 launch per week) already provide as many independent profiles (520 profiles) as the IFU ozone lidar from 2007 to 2016 (585 measurement days). In fact I think the same analysis could be made for the european ozonesonde stations. Indeed it is recognized in the discussion p.17 l.13.

p.17 l.5 In fact direct intrusion is even decreasing at low altitude according to Fig. 1.

p.17 l.16-24 Other mechanisms must be included in the analysis like lower frequency of fast vertical transport of low level boundary layer ozone and longer lifetime of free tropospheric layers (see Colette et al. GRL 2006)

p.17 l.25-33 I do not undertand the meaning of this paragraph. It sounds like if the authors doubt about the methodology based on the ozone/RH joint analysis which is the backbone of this work.

Table 1: Specify that this table is for the number of O3 lidar data.

Fig. 3 Dark blue dots (start points in stratosphere are difficult to read). Black dots are for September 29 while the actual lidar data are shown on October 1st in Fig. 2. Why not providing the map for t0+6days ?

Fig.11 Caption says trajectories are calculated on June 28th while ozone lidar data are on May 28th, please clarify.

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