How Stratospheric are Deep Stratospheric Intrusions?

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Reply

We thank both reviewers for carefully reading our manuscript and for their thoughtful comments. We made changes in most cases. In our reply we print the reviewers´ comments in italics, our replies normally. The changes are inserted and marked in the returned PDF version of the manuscript, generated by the tracking option of WORD.

Reply to Reviewer 1:

General comments:

In this article, Trickl et al. present simultaneous ozone lidar and water vapor lidar measurements, model simulation of some STE cases, and investigation of the mixing process for stratosphere-to-troposphere transport by using tracers such as water vapour and ozone. This article presents a lot of detailed analysis of both measurements and modeling. This paper also gives an excellent review on the previous work in the introduction section. Overall, this manuscript is well written and original. I recommend this article to be published on ACP with some minor revisions.

Specific comments:

P15465, L7, 'the' references.

We do not agree: not all references are taken into consideration!

P15470, L17, I don't think 'cut' is appropriately used here. Your point is that two receivers extend the detection dynamic range to about 8 orders.

Sorry for this! We use "divide" now.

P15470, L25, can you give the major reason(s) for the uncertainty improvement of the 2012 upgrade, maybe by adding 'due to : : :'?

The reason is that we finally convinced the manufacturer of our transient digitizers to develop a ground-free input. This stage also implied lower-noise preamplifiers. We added an explanation. Thank you for this comment: This improvement represents another breakthrough in lidar technology!

P15471, L22, maybe it would be better to change unit of density to molecules/m^3. Same for other places.

This is the first time this has been suggested to me in 33 years of publishing. I am aware that many authors prefer to add "molecules", but I feel better with a true SI approach. The unit is specified as density anyway.

P15476, L15, 'the two layers : : :.', which two layers?

We added ", L1 and L3".

P15476, L21-23, I can't understand the logic here. How can we tell this air mass came from upper troposphere? This depends on the mixing process. The SI air mixed with moister air results in higher water vapor.

Indeed, it is to early at this point to conclude this strongly. Although we carefully wrote "indicate" we now rephrase to "could indicate".

Figure 4, P15507,Unit of VMR is used in Figure 1 for water vapor, but unit of RH is used in Figure 4. So, it's hard to compare them. Can you provide an approximate conversion between these two units, maybe in title of Figure 4?

We had been aware of this and explicitly converted the density scale to RH units (lines 17 to 19)!

Figure 5, P15508, it's hard for me to tell dark blue and black dots. Also, add an 'approximately' in the last sentence of the title.

I agree! This is one of the reasons why we explicitly asked for a two-column width of the trajectory plots (not visible in the captions as published in ACPD). Adding "approximately" is a good suggestion: inserted.

P15477, L17-21, are the t-zero dots in Figure 5 are initialized at the same pressure level?

No: See Sec. 3.2!!!

P15478, why would the model underestimate the fraction of stratospheric ozone because of its lower resolution than DIAL?

Indeed, 200 m is a resolution that is quite acceptable in this case! I think that the resolution of the underlying ECMWF data also matters. However, the parametrization of mixing is the main issue. At this point it is too early to mention this. The intrusion is too strong anyway! We removed the two sentences.

P15479, L4, add 'Vaisala' ahead of 'RS-92'.

Done.

P15478, L19-24, I'm curious why the fraction of stratospheric ozone has a large increase at 'Time 30' in the FLEXPART simulation in Figure 7?

This question is interesting and most likely related to the filamentation shown. However, we do want to keep this discussion short the focus being on the low water vapour. All we can conclude that this model result qualitatively confirms the ozone measurements.

P15479, L24, may add 'at a single station' after ' be identified'.

We added "due to an insufficient resolution of the model results".

P15502, Table 1, what's the physical meaning of negative water □apour mixing ratio and negative RH?

The negative values are caused by the noise in the retrieved data as specified on p. 16483 (line 7). We added "±" in line 7.

P15487, L10-12, ': : :very little mixing occurs within most of the troposphere: : :'. This conclusion is questionable. The ozone concentration of the air mass with stratospheric source decreases from hundreds of ppbv to less than 100 ppbv within time duration from hours to three days, as the DIAL measurements shown in this manuscript, suggesting considerable or significant mixing within troposphere.

I think Sec. 4.4 is clear enough in this regard and we now refer to this section for more clearness! The topic moderate ozone is discussed later in Sec. 5. The ozone values are, to our opinion, not governed by mixing. They are mostly related to how deep the separating layer extends into the lowermost stratosphere over the source region.

P15487, L21-22, 'free tropospheric mixing is extremely slow' without 'strong wind shear or convective processes'. This statement is related to the above 'little mixing' statement. I think this statement is not precise. For example, Figure 11 shows the ozone concentration of the SI air quickly decreases by irreversibly mixing with the tropospheric air. How can we conclude the mixing is 'extremely slow'? Slow is a relative word. The similar conclusion also appears in the abstract, P15464, L20.

"Slow" is not precise indeed. We now write "much slower than anticipated from earlier work". The lidar ozone series are deceiving: In many cases they show a transverse cross section of the intrusion layer that has travelled to Europe over many days. An explanation exclusively by mixing is not possible. As explained in the introduction to the "Results" section, the low-ozone edge (west side of a typical intrusion layer arriving from Greenland). In these cases the descent seen in the time series is to a major extent related to the west-east drift of the layer. The low western edge (with low O3) can be extremely dry. Thus, mixing cannot fully explain the low ozone values. We plan address this result in more detail in our follow-up paper on the LUAMI campaign!

P15490, L0-17, I'm not familiar with the uncertainty in the in-situ water vapor measurements. It's ok to present the discrepancy between two instruments. But I would be more careful to draw a conclusion without solid evidence, such as the dew-point-mirror instrument has systematic bias.

I do not understand this statement. We do have solid evidence of the bias of the dew-point-mirror instrument from years of Zugspitze data. And we learnt from experts that obviously the cooling of the dew-point mirror instrument is insufficient.

Review 2

The paper presents a careful analysis of stratospheric-intrusion signatures primarily during 3 case-studies from 2008/2009, combining lidar, radiosonde and in-situ observations at high-altitude observatories in Europe. The authors are able to demonstrate convincingly that the observations are consistent with stratospheric intrusion in thin lays with little mixing after initial intrusion, and that the likely source is in the mixing layer immediately above the tropopause. The study represents a valuable follow up on previous work based primarily on in-situ observations at the observatories , in particular highlighting problems with measurement of very low humidity at Zugspitze summit. The authors are further able to argue that 20-year trends in CO in stratospheric intrusions at Zugspitze can have their source in increasing Asian pollution.

Altogether the questions addressed are important ones, the observations and analysis methods state-of-the art and the conclusions well argued. Technical corrections :

p15841 , line 19 'formation to' –> ' formation of '

I think p. 15481 is meant. Thank you for bringing this this obvious misprint to our attention.

p 15483, line 1 It is not clear what is meant by 'insufficient trajectory results of or the absence of ' - maybe ' insufficient relevant trajectory results or the absence of ' ?

We replaced "insufficient" by "unclear"