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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.

T. Trickl et al.

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General Remarks

We thank the two reviewers for their very careful reading the manuscript and tried to incorporate the suggestions made whereever possible. We could not follow all suggestions, in particular when the recommendations are conflicting.

The purpose of the paper has been not to write "just another case study". The principal advantage of measurements based on forecasts is that one really obtains information on the full complexity and case-to-case variabiliy of intrusions. These rather different cases also yield a good basis for model validation under different conditions, and this an explicit goal of this study (not consuming much space).

We also have problems with a significant reduction of the number types. For instance,

if one omits the less meaningful (but interesting) types 7 and 8 one would not know their fraction, and I do not want to drop this information. These cases are not discussed further and, thus, do not require additional space in the paper. The other types are now grouped into two principal classes tentatively assigned to two different main advection patterns, but are still retained because they make sense (and will be used further) since the source regions differ by thousands of kilometres. Any advanced meteorological classification, though interesting, is beyond the scope of this paper (the entire statistical effort for this paper took many months, a more elaborate anaysis would require even more time). Although I think that the approach suggested could be important I fear that the modified classification could lead to more ambiguity. In 2007 we have started more systematic long-term measurements, and we are going to keep an eye on these issues.

The classification, so far just needed for the analysis of the filtering criteria, now precedes the case studies which facilitates their understanding. The different types are now explained.

Specific replies:

Review 1:

Wordy section 3:

I assume that not the length of this section is alluded to. In the printed version the length of this section would be just about 2.5 pages (now: less than two pages). As a matter of fact, I tried to make the text for the individual cases compact, which is justified by the extensive existing knowledge on intrusions. Also the way of presenting and interpreting both the lidar panels, the station data and the forecast plots is rather compact (introductions are given in the preceding chapters). The individual subsections briefly describe the observational material (lidar and station) and then the model results.

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The following was done:

1. The individual descriptions were, again, modified for more clearness.
2. Due to the recommendations of both reviewers I removed two cases. This results in a limitation of Figs. 2 and 3 (new Figs. 3 and 4) to two pages each and just six lidar cases. I am not really happy about this shortening because this cuts the full, unique variability revealed by the material.
3. The classification of the intrusions (initially defined only for the final part) now precedes the case studies. This allows us to illustrate the assignment of the types.
4. The intrusion types are now marked in Fig. 3 (new Fig. 4) for intrusions relevant for the respective case study. This helps in finding the relevant trajectory bundles much quicker.
5. More information on the motivation and more conclusions are given. This also helps to create links of the different main sections.

"*I recommend to make the section much more comprehensible by only displaying the most coherent and relevant airstreams ...*": This is not a good idea because one important result of this study is the ubiquity of STT events over the North Atlantic during certain periods. I added a few sentences in different sections to make this clear. However, I added labels to the figures and, in the text, gave a link to the different types for more clearness. As mentioned not all types are discussed (1 and 2 for the first group, 5 and 6 for the second group). Thus, we do not present material on an excessive number of types at all!

"....*no explanation about the identification of the type is given, ...*": This is now done!

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Classification of the intrusions

"*...too many intrusion types that can not all be studied*": The first step is to identify all intrusions found and to determine how important they are. This leads go the information in Table2. It turns out that the types 7 and 8 are not that significant. However, the quantification is is a result of this study. The low number found does not mean that these cases can be thrown away. It also does not make sense to group these types as "other types" since they are clearly different. As to the other types there are similarities, but also differences (see seasonal cycles for Types 1 and 2). The types 4, 5, and 6 are frequently related to similar, rather zonal (cyclonic) air streams, but not always. The source regions differ by thousands of kilometres and I strongly hesitate to condense these types into a single one or two. In particular, the relative humidity for these case exhibits differences. What I did now is to assign the types 1-3 to more northerly advection and the types 5-6 to more zonal advection frequently observed during cyclonic situations when the main trough is located more to the west (which not always the case).

"*A more meaningful classification method ..... would be based on the identification of the North Atlantic weather regime ...*": I appreciate this suggestion, and I also thank you for the two citations added. An assignment to these principal patterns could be interesting. But, as mentioned, this would be completely new project (thesis). The formation and propagation of intrusions under these different meteorological conditions must be identified which may be rather difficult. Much more experimental material is needed, in particular for the intrusions over Canada, in relation to the weather situation. For the time being I prefer to stay with the grouping introduced in the revised text.

Hemispheric CTM

The EURAD simulations, made possible by a considerable effort to extend the model, mean a reasonable progress with respect to what has been published by

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Roelofs et al. (2003). The paper explicitly lists validation of this effort as one of the three goals since this benefits from both the forecasts and the resulting measurements. The validation paragraphs are really not excessive. Publishing this effort elsewhere would require describing the respective cases in detail again, which does not make sense to me. Furthermore, a different community would be addressed. By contrast, Reviewer 2 names the inclusion of the different models a strength of this study.

Reply to specific questions:

Figure 1: The y co-ordinate is now explained in the caption; I also added remarks on the model domain, and on the full description in (Stohl et al., 2000). I also made a few changes in the text. The other two questions concern details beyond the level of an introduction. I have problems in understanding them anyway. The colour coding looks clear to me.

The distance between IFU and the summit station is 8 km, this is now specified. Since this information was given in the companion paper I am astonished that it was forgotten here!

Page 2227, lines 21-23: These criteria were derived from extensive analyses of intrusions in the seventies (this comment is now added). I fear that these criteria mostly predict Type-1 intrusions.

Pages 2232-2233: I think that the first two paragraphs (ETH forecasts) are rather clear and limited improvements to the remaining part of this section. I think that the new description is substantially clearer.

Page 2235, line 13: changed to ".....coinciding with the observation of ozone import...."

Page 2236, lines 19-22: "lidar" was added for clarification; the remark about the clouds was eliminated since this is not that relevant. The clouds just delay the beginning of the measurements.

Page 2238, lines 6-7: "system" was removed since this could be confusing. The intru-

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sion seems to exhibit several vertical layers that can only be seen when expanding the scale of the plot.

Minor comments: All changes suggested were made.

Review 2

General remarks:

We appreciate these rather positive remarks.

Scientific question/issues

Models

Agreement/difference between the different models....: As mentioned I compared selected HYSPLIT and ETH results and found reasonable agreement. However, this is very difficult to present without blowing up the manuscript that focusses more on the outcome of all these efforts. The pathways for the different cases are really different, as can be seen from the examples shown. However, there are cases for which a dictinction has been rather difficult. For instance, sometimes intrusions over Labrador procede on a northerly route getting close to that of a Type-1 intrusion. Then, the source region is the only way to distinguish the types. However, these Type-5 intrusions mostly arrive on a rather zonal pathway. Unfortunately, we omitted these cases in the lidar sounding of 2001. They have been an almost persistent feature this summer, and we have obtained nice results (too late to be included).

I added a few lines on the most typical pathways to Central Europe (cyclonic/anti-cyclonic) to the classification, which now precedes the case studies. The types 1-3 and 4-6 form subgroups, qualitatively assigned to these two principal pathways. The types 7 and 8 are special and just listed for completeness. As a matter of fact: We do not overload the paper with plots and discussions on all classified types. Why is there a problem?

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FLEXPART: For coherent air streams FLEXPART yields astonishingly good results, as, e.g., verified by comparison with satellite data. There are numerous papers documenting this. Of course, there are limitations for a 20-day run that are hard to specify. However, the information needed is not exclusively obtained from day -20! Mostly, rather short backward times are sufficient for the analysis. I added a few remarks in the model description and the first case study where one component with really long travel time exists (revised interpretation). As mentioned more examples for 20-day simulations can be found in the companion paper (Trickl et a., 2009a).

As to tropospheric residence times: The operational 20-day FLEXPART runs do not allow one to distinguish the tropospheric age of each air parcel as done in the version used for Fig. 1. However, as mentioned in the March-2001 case, one is able to distinguish days with and without stratopheric contact by looking at the STT fraction for the individual days.

EURAD: The focus here is on the validation of the extended model rather than on contributing to the interpretation. However, it is important to see that even if the model is not operated with its full resolution the experimental findings are reasonably well reproduced. After the less satisfactory results of Roelofs et al. (2003) it was good to see that Eulerian models can do a much better job. I made some changes to clarify the purpose of the EURAD contribution..

Case studies:

I added a few lines on the general purpose of this section and some conclusions, also on the intrusion type. The “classified archetypes” are now assigned in both the text and Fig 2 (new Fig. 3). Indeed, the formation of intrusions has been extensively described before. We, therefore, focus more on the previously less document complexity and its potential influence on the long-term observations at the Alpine summit stations. This is mentioned more explicitly now.

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Mixing:

I feel that, as far as mixing is concerned, some wind shear must be involved. In my view stirring is the large-scale motion eventually causing the wind shear, but also leading to Lagrangian inflow of air. I added “inflow” in one of the sentences. By contrast, we have seen very low concentration changes in layers presumably travelling with the same speed as the air masses above and below over thousands of kilometres. This indicates that mixing in the free troposphere far away from frontal systems should be extremely small. We plan to make focussed lidar measurements (O_3, H_2O) to study specific layers in more detail.

Representativity and transferability

A few sentences were added, also concerning the high intrusion frequency.

Zugspitze measurements:

I made some adjustments, but did not go into details. The missing anti-correlation between CO and O_3 has been the subject of numerous discussions here. In the ATMOFAST final report Dr. Scheel even showed positive CO trends in intrusions. However, this topic will be discussed in a subsequent publication.

Orography:

A quantification of this effect is possibly rather difficult. We also do not know what the model really does. As to ECMWF: The FLEXPART or FLEXTRA results look clearly better (e.g., Fig. 5 = new Fig. 6), but there are sometimes also (smaller) vertical offsets. I do not want to discuss this here in order not to interrupt the text. However, a few improvements were made.

Technical issues:

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The model data for HYSPLIT are now specified.

We would have expected a higher stratospheric fraction in intrusions reaching our site within two days. To our surprise Type 6 (longest advection path) seems to provide very dry layers too.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2223, 2009.

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