

## **Review for Stratosphere-troposphere exchange in the vicinity of a tropopause fold by Hofmann et al.**

### **Synopsis:**

Hofmann et al. look at a tropopause fold event and study the mass exchange (STE) in its vicinity. The topic is of interest to the readership of ACP. Of course, this is not the first study considering STE and trying to understand the mechanisms leading to the crossing of the dynamical tropopause. Nevertheless, I think that the problem is by far not yet solved, and additional careful case studies can shed further light on the relevant processes. Given this, I would recommend the study for publication in ACP. However, I see some space for improvement, which I will list below.

### **Major Concerns:**

- In Figs. 3,4 and 5 the EMAC and COSMO simulations are shown. However, I am not convinced that we can learn too much about the processes leading to STE by looking at the EMAC simulations. The horizontal resolution of the model is simply too coarse (as the authors themselves state in the conclusions) and therefore the relevant physical processes are not adequately represented. To me, only the high-resolution COSMO simulation is of interest and really allows to study the processes. In short, I would recommend not to compare the two models explicitly, and simply mention the EMAC simulation as an intermediate step. No EMAC figures need to be shown and the text related to the comparison can be shortened. Note that from section 4.2 on, the authors in fact discuss only the COSMO results!

- Table 3 and its discussion 'confused' me - less would be more. In fact, I see the benefit from distinguishing different source and target regions, but the contents in Table 3 is simply too much. For instance, I wonder what can be learned from only looking at the region 40 N (first column)? Or if there is a good reason to have the 2-4 PVU criterion (last column), why also looking at the strictly-2 PVU case (middle column)? Maybe, what I am missing is a clear motivation for this whole detailed analysis. I think it would help a lot if from beginning some clear hypotheses could be stated, and Table 3 is then the data supporting (or rejecting) exactly these hypotheses. Now, while reading the text I had a little the impression that many of the entries/calculations in the table are simply there because they could easily be done. Of course, the authors might see it differently! Then, I only request a more concise discussion!

- At some places, the text reads too technical. I managed to keep track until section 2.2, but then came 'ESCIMo simulation RC1SD-base-10a', which I do not know. Furthermore, there is definitely no need to mention 'INT2COSMO', and the whole Appendix A 'PTRACINI' is rather technical (with namelists,...) which in my view is not relevant for the reader, or should be discussed in a non-technical way. In the same line: On P9,L25-26 the calculation of trajectories is described: First, 48-h trajectories forward in time; then - if still in the domain - 72-h backward trajectories from the end points; resulting in information of the initial trajectories 24-h before they are in the fold. Why not simply write that 24-h backward trajectories are also calculated from the initial position in the fold and then combined with the 48-h forward ones. This sound less complicated to me? Or do I not really understand the reason for the strategy presented in the paper?!

- Personally, I would restructure the study! First, in section 3 I do not see a benefit in discussing the EMAC simulation; then section 4.1 is rather long. It compares the simulations with observations. Its main goal is concisely summarized in the last sentence of the section: "Since this evaluation has shown that only the COSMO instance is capable to capture the descent of stratospheric air masses into the troposphere in accordance to measurements, for the analyses hereafter, only the results of COSMO are considered." Note that the aim of the study, as stated in the introduction, is "to quantify the stratospheric ozone contribution and to identify responsible processes leading to STT". In fact, I think we do not really come closer to these aims with the rather detailed comparison with the observations. I would suggest to shorten this section 4.1 and then to combine it even with section 3. The new section would then define the synoptic and observational basis for the following process-related analysis. Then, I would suggest to split the new section 4 (with a potential title "Detailed analysis of the exchange process") more strictly into three parts: the first looks at the origin (near the jet) of the STE trajectories, the second handles with the STE parcels within the tropopause fold (which is rather interesting), and the third addresses explicitly the crossing of the dynamical tropopause. If the authors agree, this structure would more clearly follow the air parcel on their way from the stratosphere to the troposphere. But I leave it to the authors to decide whether they want to accept it or not.

- A remarkable result is the strong concentration of the stratospheric tracers behind the fronts (see Fig. 4). In the text the authors discuss the importance of fronts as 'barriers' for the tracers (P6,L3-4). Hence, they see the key impact of the fronts on the surface impact of the stratospheric tracers. But the discussion of the front-associated circulation, e.g., vertical winds is missing completely. Because fronts are identified as an important feature, I would appreciate a more detailed discussion. For instance, does the surface tracer imprint coincide with the frontal vertical winds?