

Interactive comment on **“Stratosphere-troposphere exchange in the vicinity of a tropopause fold” by Christiane Hofmann et al.**

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This modelling study of an STE event is, in general, relatively well written and quite clear. While I do think that several sections could be improved by more accurate and more detailed analysis (see points below), overall the analysis methods are solid and the combination of Eulerian and Lagrangian analyses of some interest. So there is nothing really wrong with this paper. However, my main point of criticism is that it simply does not add much to our general understanding of stratospheric intrusions. Similar studies of similar events have been published in great number and I can't find any novel results of this study in particular. Neither are the methods applied particularly novel (certainly good tools have been used) nor are the results showing something

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that has not been shown before. Partly this is also related to the fact that this is a pure modeling study that does not involve any in-depth analysis of measurement data (the passing mentioning and use of ozone time series of surface stations does not really provide much extra insight). Often it is the combined use of models and measurement data that can reveal interesting aspects, whereas it is difficult to show something definitive if only a model is used. Overall, I think it is an editorial decision whether such a type of paper is of sufficient novelty to justify publication in ACP, or not. In my overall ranking, I suggested that the paper should be reconsidered after major revisions, as a compromise. But I think the decision is really an editorial one between acceptance with relatively minor revisions, or rejection because of lack of novelty. More specific comments about the paper are given below.

The episode chosen does not appear to be one with major surface ozone enhancements. There exist much stronger episodes with increases due to stratospheric intrusions. Why was this particular one chosen for a case study? Is it just because simulations already existed (and have been published already)?

Section 4.2.2 is not very clear and rather vague. It is touched upon which processes are responsible for STT, but they are not isolated and quantitatively discussed. In a pure model study, it should be possible to say which exact model process(es) is (are) causing the STT. There seems to be also some lack of a clear understanding about processes that can lead to STE. For example when it is written on page 11, 117, that your results indicate “that particularly non-conservative processes enable air parcels to cross the tropopause”. Given the definition of your tropopause in PV units, only diabatic and frictional processes can actually lead to such an exchange, and this is by definition so, not because your results indicate that. If they would indicate otherwise, something would be wrong.

Section 5: For my taste, there are too many relative values here, making the whole section somewhat difficult to read, especially when relative values of relative values are given. Furthermore, it would be interesting to know what the actual mass ex-

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change rates (or air and of ozone) are, as they could be compared to other cases documented in the literature. The relative frequencies do not suffice in that regard. Surprisingly, then, the last sentence of your paper says that “coarse resolution models might underestimate STE mass and ozone fluxes” – so why was this not shown in the first place, especially since you have done your study with one coarse- and one high-resolution model. With accurate analysis, this statement could be quantitative rather than speculative (at least for the event studied).

Further specific points:

To my (admittedly, personal) taste, the use of names for individual modules or subroutines is inflationary. This creates chains of acronyms that may sound funny but are actually difficult to remember and unnecessarily extend the text. This concerns many things in this paper but one example is the “submodel” PTRACINI that seems to be nothing more than a subroutine in a computer code that does some tracer initializations. Does it really need its own name, suggesting that this is something of greater significance?

Somewhat related to the above point, is it necessary to explain MECO(n) in a general way if anyway only one nesting level is used, as explained on page 4?

Pg 2, lines 30-31: There are a few strange statements about problems with Lagrangian and Eulerian approaches. For instance, numerical diffusion is highlighted as a problem for Lagrangian models, whereas it is exactly the (near) absence of it that makes Lagrangian models valuable tools. For Eulerian models, interpolation is highlighted as a problem, which also is not true.

Pg 5, 17: Where is it actually shown that this zone is baroclinic? General synoptic understanding suggests that but it is not really shown, is it?

Fig. 2: It appears that EMAC SLP agrees better with ECMWF analysis than COSMO SLP. Any good explanation for that?

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Pg 6, line 2: Be concrete and accurate: Which front? You mean the cold front but this should be said explicitly.

Pg 6, l6: Jungfraujoch is described as a remote site but it is located in the heart of Europe and also sees regular pollution episodes, especially in summer, e.g., from the Po Valley.

Pg 6, l13: “raising processes”: What shall that be? Do you mean convection? Or slantwise frontal ascent? Again, language not very precise.

Figure 7g-h: I understand that the normalized scale may make these panels more compact, but it also hides a lot of information. For example, what is the actual change in potential temperature? This is referred to e.g. on page 9, l13, but cannot really be judged well from these figures. It should be possible to plot this also with absolute scales without inflating the panels too much.

Language issues:

Pg 1, l12: vicinity -> vicinity

Pg 2, l14: use of word “current” not correct here

Pg 3, l7: twice global within a few words

Pg 3, l9: occuring -> occurring

Pg3, l13: correct superfluous, as there is also the word “correctly” in same sentence.

Pg 6, l17: increaseS

Pg 7, l1: showS

Pg 7, l4: observationS

Pg 7, l9: “produced by photochemical production” - > produced photochemically

Pg 8, l12: “initially isentropic along the tilted isentropes” -> initially following the tilted

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isentropes”.

Pg 8, l18: ApproximatEly

Pg 10, l21: the the (remove on “the”)

Pg 10, l24: two thirdS

Pg 10, l24: amount -> fraction

Pg 11, l6-7: Sentence starting with “By now, ...” is unclear. Not sure what you want to say here.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-949, 2016.

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