Dear Charlotte Hoppe,

The revised version of your manuscript has been sent back to the referees and in the meantime both referees submitted their final recommendations. While one referee suggests ‘publish as is’, the other referee recommends further revision of your manuscript. I carefully read the referees’ comments, your reply as well as the revised version of your manuscript. You and your co-authors were indeed responsive to both reviews, and the referees also credited your efforts in improving the manuscript. Nevertheless, I agree with referee #1 that your paper needs further revision before publication.

Your paper describes the implementation of the Lagrangian transport scheme CLaMS into the EMAC model, and compares simulated stratospheric distributions of long-lived trace species and the age of air to results with a flux-form semi-Lagrangian advection scheme (FFSL). As mentioned in the Introduction, there are two main differences between both advection schemes: 1) numerical diffusion, and 2) the representation of vertical velocities. Although both advection schemes were run in the same climate simulation, the different representation of vertical velocities leads to different dynamics (wind fields) which drive the tracer transport. Therefore, I do not fully agree with your statement that ‘the underlying dynamics were identical’ (p10, l8). So the question is how would the results look like if both schemes were driven by the same vertical velocities? Your conclusions suggest that the differences in the shown tracer distributions are related to the superior numerical properties of the Lagrangian approach, but it is important to clearly distinguish between numerics and dynamics. Your reference to an upcoming paper (p10) dealing with the impact of the different vertical velocities on the model results indicates that this is an important point and needs to be addressed.

Please find attached the comments of referee #1 on your revised version. I kindly ask you to revise your paper taking into account the comments about the Figure 2 discussion, the Figure 6 and 8 discussions, as well as impact of the vertical velocities, or respond accordingly. Concerning the comment about temperatures (‘I read Referee 2’s comment about temperature …’), I think this is a misunderstanding. As far as I understand both transport schemes, FFSL and CLaMS, were applied simultaneously in a single simulation the EMAC model, and not the FFSL driven by EMAC dynamics and CLaMS driven by ERA-Interim, as stated here by the referee. However, it is also not absolutely clear to me whether both advection schemes were run in a one-way-coupling mode or whether either the FFSL or the CLaMS tracer set serves as input for the EMAC radiation scheme. So some clarification in the text might be helpful.

Best regards,
Andrea Stenke

Comments by Referee 1 on the revisions to Hoppe et al.

The authors have introduced additional comparisons that I suggested (i.e., age of air, MLS N2O pdfs), which improve the usefulness of this manuscript a great deal. The new Figure 2 that compares with age of air determined from observations provides a lot of diagnostic information about transport in the two models. However, there are several significant mistakes in the authors' interpretations of the age of air and vortex pdf comparisons that must be addressed because they are leading the authors to wrong conclusions about their simulated transport. In addition, not all of the issues raised by Referee 2 regarding temperature and dynamics were adequately address but need to be. This paper requires significant revision.

<u>Figure 2 Discussion</u>

The main problem is the interpretation of Fig. 2c which shows the tropical midlatitude mean age gradient. This is a diagnostic of ascent rate, not subtropical barrier strength. Please see Neu and Plumb (1999) as well as the discussion of this comparison in Ch. 5 of the CCMVal report. This diagnostic is actually <u>*insensitive*</u> to barrier strength: an increase in mixing between the tropical and midlatitude lower stratosphere increases mean age in <u>*both*</u> places, so their difference (the gradient) does <u>*not*</u> change. This gradient depends on tropical ascent rate. When the simulated gradient is less than observed (as it is in both simulations below 20 hPa), this means ascent is too fast. Clams has the slower and more realistic ascent rate of the two models. Fig. 2a, tropical mean age, and Fig. 2c, tropical ascent rate, tell even more about tropical transport when used together. The Clams tropical mean age is younger than FFSL above 50 hPa, yet Fig. 2c shows that Clams actually has a slower ascent rate! To get this result means that Clams is slower yet has even less mixing of midlatitude (i.e., older) air into the tropics than FFSL.

You've also stated in this same paragraph (on page 12) that the tropical mean age shows that CLAMS has stronger upwelling because its mean age is younger. Not true! Mean age is a function of ascent rate <u>and</u> mixing – you can't draw any conclusion about ascent from tropical mean age alone. (That's why the combination of Fig. 2a and Fig. 2c is essential for interpretation.) See also Strahan et al. (2009) on tropical recirculation. This explains about these contributions in the tropics.

Now I realize that you have drawn the conclusion that Clams has a stronger subtropical barrier and the analysis I did above shows that this is true. But you've arrived at this conclusion fortuitously and not for the right reasons. These two new paragraphs discussing Figure 2 need to be completely revised with a correct analysis. I urge the authors to read parts of Chapter 5 of the CCMVal report, Neu and Plumb (1999), and Strahan et al. (2009).

I don't know if you can take a look at the residual circulation in these two simulations. How does w* compare in the tropics? I think the FFSL w* should have the large velocity.

p. 12, line 12. 'reasonable'. I do not agree that these young mean age are 'reasonable'. I suggest a more neutral approach and simply call it as it is: both models produce a similar age of air distribution which lies in most cases at <u>or below</u> the boundary of one sigma….

Figure 6 and 8 Discussion

Fig. 6: You cannot draw the conclusion about FFSL that the higher mixing ratios in the vortex peak indicate that downwelling is too weak. The mixing ratios could be too high because, even though there was enough downwelling, there was too much mixing with extravortex air during descent. That would give the same result as too little descent. Given that the FFSL pdfs show less separation and a higher minimum between the peaks, too much mixing during descent is a likely possibility.

You'll have to change the statement about faster upwelling in the Clams model as per the discussion of Figure 2 above. Also, though, given that this pdf examines 50-80S, it's not reasonable to conclude anything about tropical transport from it. Isentropic mixing in the surf zone (connecting the tropics and high latitudes) plays a role in the midlatitude pdf peak.

Once again I'm going to suggest that if you can get w* from each simulation's met fields, you might be able to say more about transport differences.

MLS polar plots: please zero out the 'data' between 82^{o} and the pole. There are no observations there.

Regarding Figure 8 discussion, does EMAC-FFSL have a lot of sudden stratospheric warmings in the NH? If so, then averaging the 10 Arctic winters in the simulation could explain the smaller separation of the pdfs. The dynamics of this model are worth considering before you draw conclusions about the quality of the transport.

Comments on the response to Referee 2

I read Referee 2's comment about temperature and the authors' response. The referee is not concerned about radiative effects of a few trace gases in the radiation scheme. The issue is the temperature differences in the simulations. Temperature does not come from the transport scheme in the chemistry transport model. It comes from the input meteorological fields – in this case from ERA Interim and from the EMAC general circulation model. Temperature is most certainly not the same in these met products, and is thus not the same in the two simulations! Please return to the original referee's comments and address them.

I also agree with Referee 2's point that the reader should get a clear idea of transport and dynamics of both models. I think the authors' are not understanding the difference between transport and dynamics. This is a very important point in this analysis. Dynamics (i.e., waves, their propagation, their interactions with other waves, and where they break) is fundamentally what drives transport. But simulated transport is also affected by the advection scheme, and this is what this paper intends to compare, i.e., FFSL vs. Lagrangian schemes. But without a clear idea of the dynamical differences between the (meteorological) models, this paper cannot hope to present a clear analysis of transport differences *that are due to advection scheme differences*. The authors have not adequately addressed this comment, and as it is an important point I believe they need to try again.