Geoscientific
Model Development
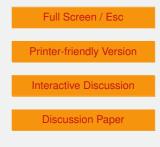Discussions

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**Interactive
Comment**

Interactive comment on "The implementation of the CLaMS Lagrangian transport core into the chemistry climate model EMAC 2.40.1: application on age of air and transport of long-lived trace species" *by* C. M. Hoppe et al.

Anonymous Referee #1

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This manuscript describes a modular coupled model (EMAC) that can be implemented with a choice of two transport cores, one using Lin and Rood Flux-form Semi-Lagrangian (FFSL) transport, the other using a fully Lagrangian forward trajectory method (Clams). The first part of the study describes the Clams trajectory and mixing methods, along with how transport is coupled into the modular framework. The second part of the study focuses on comparison of transported quantities, e.g., age of air and other long-lived trace gases, to evaluate differences in representation of transport processes in these modules. Some comparisons are also made with observations (of

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Interactive Discussion

Discussion Paper

age, from ACE-FTS, and MLS N2O).

The intro and model description section are very good and I have only minor comments.

The comparisons intended to evaluate transport don't do an adequate job of revealing the reasons for the differences. The conclusions drawn are not always supported by the evidence presented. There are numerous, better ways to compare the simulations with each other and with observations that can clearly demonstrate how process representations differ between the two cores. I recommend revisions to Section 3 to include additional diagnostics while removing ones that provide no insight into the cause of differences between the simulations. The authors must revisit their conclusions after assessing the additional diagnostic evaluations; I suspect some conclusions will change. With a more thorough set of diagnostics, this paper will probably be acceptable for publication and could be an excellent example of thoughtful model evaluation.

Section 3.2 Age of Air

p. 1771, lines 15-17. Use simulated mean age relative to the tropical tropopause in Figure 2. It is acknowledged that the observational age is referenced to the tropical tropopause yet the plot shows simulated ages since surface emission. This comparison must be made fairly. Please note the transit time for each model from the surface to the tropical tropopause.

p. 1771, lines 19-22. Figure 2, once simulated age is correctly plotted, will show simulations whose extratropical age is quite young. Twelve of the 15 of the CCMs compared in this same way (SPARC 2010) are in better agreement with the observations than these simulations, so I do not agree this is a 'typical feature . . .which can be much more pronounced than in the example shown here.'

The evaluation of mean age is inadequate but can be easily improved. The SPARC CCMVal report, referenced by the authors, shows four mean age diagnostics that together reveal much more about transport. Please use the 4 diagnostics shown in Fig-

ure 5.5 of this report. For example, the tropical-midlatitude mean age gradient profile is useful for evaluating credibility of the tropical ascent rate. From Figure 2 I calculate this gradient at 50 hPa to be about 1.3 years for both simulations. That indicates much faster than observed circulation, and faster than most CCMVal CCMs. The mean ages derived from observations used in these diagnostics are publically available.

Better still for diagnosing ascent rate, compare directly with the observationally (tape recorder) tropical vertical velocities shown in Fig. 5-6 of the CCMVal report.

p. 1772, lines 1-6. Please report the simulated lifetimes and compare with the SPARC lifetimes report. Here the authors make a qualitative, subjective statement ('compare very well') and reference an unpublished source (Hofmann et al, 2014 – in prep). The recently published SPARC Lifetimes report has new recommended values for CFC11 and CFC12 which should be used in your comparison. Please state the simulated lifetimes too. The lifetimes report is available at http://www.sparc-climate.org/publications/sparc-reports/sparc-report-no6/. The new lifetimes were derived from many observations and models, carefully evaluated and combined to derive the most likely lifetimes and uncertainties. The recommended values and most likely uncertainties are 52 (43-67) years for CFC11 and 102 (88-122) years for CFC12. In Chapter 4 of this report, lifetimes derived from satellite or space-based observations showed a wide range of values with large uncertainties.

p. 1772, last paragraph. It is stated that the simulated horizontal wind (u and v) matters for vortex isolation (agreed), but then you compare only the zonal wind, u – that ignores the v wind which actually matters for meridional transport (i.e., vortex isolation). Please use a better meteorological metric for vortex isolation in the two simulations, perhaps the mean u'v' (horizontal momentum transport) or potential vorticity.

Section 3.3.1 Antarctic Polar Vortex

p. 1774, lines 5-7. A mean trace gas profile, especially a multi-year mean, does not reveal anything about processes (fig. 5). The profile is the net result of two ongoing

processes, descent and mixing, not either one individually. (Those processes differ significantly across the vortex edge. Since these profiles are multi-year means from 70-80S, they may include profiles from outside the vortex although I agree that these are probably mostly vortex profiles.) The differences in September CFC11 profiles can be caused by several things: 1) differences in descent rates during fall and winter, 2) different initial profiles when descent began, 3) differences in vortex isolation at higher altitudes that affected vortex profiles during descent. In addition, the simulated CH4 profiles have small differences, and they are more similar to each other than to the observations.

Even combined with statements about age of air gradients in Fig. 4, this discussion does not unambiguously separate the effects of descent and mixing. I suggest another of the CCMVal transport diagnostics, the Antarctic CH4 pdf profiles shown in Fig. 5-15. The contoured vertical pdfs (68-78S) show vortex isolation as a function of height. As you are trying to make the point about process differences between August and October you could compare PDFs for these months. This could clearly identify down-welling differences (by changes in the polar most probable profile) while at the same time identifying mixing differences (i.e., how well separated the midlatitude and polar most probable profiles are and how their separation changes between August and October). Although Fig. 5-15 used HALOE CH4 data from mid-October to mid-November (HALOE did not sample high latitudes in August), the model-model comparison would reveal process differences between the FFSL and Clams transport. One could take this a step further by using MLS N2O data, available daily during all months over the past 10 years, for the contoured PDF calculations. This could be used to evaluate descent and mixing over the entire winter. This would most useful for processes occurring below 700 K.

p. 1775, lines 1-4. Since Figure 3 showed zonal, not horizontal wind (or momentum flux or PV), it is not clear which simulation has a stronger vortex, greater descent, or differences in vortex isolation.

Figure 6 is a nice comparison and would be complementary to contoured vertical pdfs. But what is going on with the MLS data plotted? There are yellow 'stripes' running from the high gradients (from ∼65oS) to the pole. This does not look at all physical. Also, MLS observations are not made poleward of 82o so 'data' should not be plotted all the way to the pole.

The curious yellow stripes are also found on the EMAC/FFSL panel (but not the Clams panel). The Emac/ffsl gradients are noisy with spuriously high values around latitude circles (numerous yellow stripes along meridians in a sea of blue).Is this a plotting problem or is there a mysterious source of polar $N2O$ in the FFSL simulation (i.e., transport implementation error)? I don't understand why this unphysical feature also appears in the MLS panel. I would like to see how the N2O fields compare between MLS and the simulations.

Section 3.3.2 Arctic polar vortex

While the Clams simulation is older and it might be because the vortex barrier is stronger, the steepness of edge gradients that would show this can't be discerned in this figure. (The MLS N2O gradient plot was better in this respect.) This could be better compared using PDFs of age or N2O (say 60-90N) as they would clearly show mixing by the height of the minimum between the mid and high latitude peaks. Comparing PDFs for February and March would show if the degree of mixing in each model were changing during Arctic winter, plus the change in the high latitude peak would indicate whether additional descent were occurring (unless there was a lot of mixing, in which case you could not distinguish the separate effects of descent and mixing).

p. 1776, lines 15-16 claim that greater mixing has taken place in the FFSL, but this isn't something that can be discerned from these maps. PDFs would show this. If Arctic transport were analyzed using N2O instead of age, this would have the advantage of allowing a comparison with MLS N2O. While one of the primary goals of this paper is to show transport differences between the two model versions, it is also of value to show

how realistic they are.

At present I don't agree with line 28, that these results are consistent with the SH analysis given the odd stripes shown in the N2O gradient figure.

Minor comments

p. 1767, line 24. 'There are two ways to use...' Instead of this being the last sentence of a paragraph it should be the first sentence of the next paragraph.

p. 1769, line 7. Change 'a profound discussion' to 'an in-depth discussion' p. 1773, line 5. 'In contrast' instead of 'on the contrary'.

p. 1773, lines 10-12. A problem with the wording, it's downwelling (of air) not downwelling of vertical velocities you're talking about. I think you're meaning is that diabatic vertical velocities are larger in the vortex than the kinematic velocities, yes?

p. 1773, line 14. suggest changing 'downwelling decreases in strength' with 'downwelling weakens'

p. 1773, line 15. It's the relative impact that you're talking about. Just because descent has slowed to about zero in October does not mean mixing across the vortex barrier has increased or is strong, but the way this is worded increased mixing is implied.

p. 1773, line 20. The pattern of trace gases and age of air are both controlled by transport – I think this is your intended meaning. 'Age of air distribution' is not a force and cannot dominate control.

p. 1776, lines 8-9. Older mean age could indicate a strong vortex but not necessarily. Age is a function of both circulation and mixing.

Interactive comment on Geosci. Model Dev. Discuss., 7, 1759, 2014.