**Atmospheric
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Discussions**

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Interactive comment on "Mechanisms for synoptic transport of atmospheric CO_2 in the midlatitudes and tropics" *by* N. Parazoo et al.

Anonymous Referee #1

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General Comments

This article examines the variability in CO2 concentration at a number of surface monitoring sites around the world, focussing on variations associated with transport. Firstly, composite CO2 time series relative to the passage of cold fronts are created using observed and simulated CO2 concentrations. This illustrates that the typical changes associated with cold fronts vary systematically with monitoring site and are represented by the model in most cases. A local CO2 budget is then used to partition CO2 tendencies by process. Behaviour in the mid-latitudes and tropics is contrasted.

The results of the study are not surprising and the approach is not especially innovative. However, the composites of frontal passage (Figs 3 and 4) are useful for model evalu-

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ation. They indicate clearly that the model is capable of simulating the CO_2 contrasts typical of frontal passage. However, the paper would benefit from better quantification including uncertainty analysis. Similarly, the partition between local and non-local transport at mid-latitude and tropical sites is potentially useful information. However, the numbers presented in Table 2 are not of value unless a measure of variability in the partition by process is given and an indication of the uncertainty associated with model simulation of the total tendency. For example, it is likely that the simulated convective mass fluxes are less certain than the advection by the flow resolved in analyses. How does this uncertainty propagate into the CO_2 budget? Is the tropical budget less certain as a result? In addition, there are potentially major sources of variability such as variations in CO_2 surface fluxes associated with cloud cover that were not considered in the analysis. This paper is missing a trick by not quantifying some of these other terms. If they are small compared with advection, just how small?

Revisions including more rigorous quantification of the simulated CO_2 budget and its comparison with measured CO_2 concentrations are required before publication in ACP.

Specific Comments

1. Although formally it is possible to partition the local tendency into terms as in eqn (1), the processes are interdependent. For example, in the absence of boundary layer turbulence to spread emissions from the ground to the lowest model layers, large-scale advection would have nothing to advect and the convective mass flux would also be impotent. The authors should consider whether it would be better to conduct a sensitivity analysis where parameters such as vertical diffusion coefficient and convective mass flux are varied about their control values, rather than the drastic action of switching fluxes or convection off entirely.

2. It would add value to the paper to consider variations in F_c associated with photosynthetically active radiation and cloud cover, and their impact on the CO_2 tendencies.

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3. Section 3 and the case study figure 6 could be omitted. It is well known that any tracer tends to align with the deformation axis and eqn (2) is not used in this paper other than to make this statement. Indeed, part of the reason for the formation of sharp temperature fronts is that (equivalent) potential temperature is conserved so that its gradient is enhanced by shearing and deformation flow, in a similar way to the passive CO_2 tracer.

4. The other part of the frontogenesis story involves dynamics - in order to maintain thermal wind balance as the temperature gradient increases, a cross-frontal (ageostrophic) circulation is induced that also acts to increase the surface temperature gradient. The discussion in the first paragraph of 3.1 is too loose and detracts from this paper. There are many dynamical arguments along the way to get from differential solar heating to the existence of sharp temperature fronts. It is not necessary to outline them here - just start from the position that sharp temperature fronts exist and there are naturally contrasts in long-lived tracers across them.

5. Similarly the first paragraph of the introduction presents a rather weak discussion of global circulation which could be omitted since it is well known that horizontal advection (particularly of potential temperature) is more important in the mid-latitudes than tropics.

6. What are the main implications of your results? What aspects of the model are worst? Is this a generic problem or specific to your model? How could it be improved? Are similations in the tropics less reliable as a result of convection or other processes?

Technical Corrections

1. p.2: MLO site is not shown in Fig.1.

2. Sec.2.1, l.1: "investigate temporal variations at a point in space"

3. Values averaged 1-5pm local time should be referred to as "afternoon" rather than "midday".

4. The PCTM needs more description. What chemistry is parameterised? What processes are included that could affect global CO2 burden other than emissions?

5. In which years do the events contributing to Figs 2 and 3 occur?

6. What is meant by a "quasi first-order discontinuity"? Also, you state that the frontal passage occurs "at the time at which the second order gradients were at their maxima". This is an odd definition. Figure 4 shows that t=0 occurs where the temperature tendency is greatest which corresponds to zero second derivative with respect to time. Is this what you meant to say?

7. Why is the wind speed comparison in Fig.4 for the SGP station much worse than for the other stations and variables?

8. The experiment with M=0 is referred to as NOCLOUD but it would be better to use NOCONV to separate the effects of convective mass transport and the effects of cloud on radiative transfer.

9. The NOFLUX and NOCLOUD experiments were run over an entire year. Is there a significant change in the global and regional (say North American) CO2 burden relative to the control?

10. Figure 1: The ZEP time series appears to be shifted by six months relative to BRW and ALT (and all the other series) which is rather hard to understand. Image quality is also too low.

11. Figure 2: Why is the model simulation for HUN shifted by 6 hours relative to obs? Is this due to definition of timing of frontal passage and its relation to the model output times?

12. Figure 5: Expand NEE and FF in caption.

13. Figure 6: Omit

14. Table 2: Need numbers quantifying variability as simulated by model and its relation to observed variability in CO2 tendency.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 12197, 2008.

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