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Geoscientific
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Discussions

Interactive comment on "Simulation of atmospheric carbon dioxide variability with a global coupled Eulerian-Lagrangian transport model" *by* Y. Koyama et al.

Anonymous Referee #2

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The manuscript describes the combination of a Lagrangian model and a Eulerian model for tracer transport. This has been done before by various groups, but here the difference is in the detail of which part of airmass history is simulated with the Eulerian and which with the Lagrangian model. Rather than using a fixed spatial domain, the authors chose a certain duration of the Lagrangian particle dispersion simulation. A number of specific aspects need to be addressed before I can recommend accepting the manuscript for publication.

It should be investigated in more detail what causes the adverse behavior of the coupled model at Samoa, where its performance in terms of the variance ratio is degraded

relative to the Eulerian model, and at Barrow, where its performance in terms of the correlation is degraded. Given that the coupled model works for one out of three sites, there would be a solid method required to decide where it can be applied when extending this to larger observing networks or even space-based observations. The more detailed investigation of model-model differences should attempt to identify the causes, e.g. there could be differences in advection (different wind fields are used), convective redistribution associated with cloud transport, and turbulent mixing / diffusion within the boundary layer, which is certainly different in the two models.

The abstract should contain more quantitative results from the comparison between the two models and the observations. Also the discussion or conclusions should contain some statement on how the coupled model can or will be used for inverse modelling. Will the adjoint of the Eulerian model be coupled with the footprint or influence information from the backward LPDM simulation? Those technical issues should be at least mentioned.

Detailed comments:

P 2052, L 4: add "the" between "by" and "near field"

P 2053, L 22: replace "resolutions" by "resolution"

P 2053, L25: It remains unclear what is meant by "fully realized". A resolution of 10 km might be appropriate or not, but this depends for example on the heterogeneity of the spatial distribution of fluxes in the vicinity of the observational site.

P 2053, L 29: The computational time required for backward LPDMs depends on the number of locations for which mixing ratios are simulated. In case of space based observations a Eulerian approach might well require less computational time. This should be discussed in the paper.

P 2055, L 7: The results of those experiments should be included in a table, as this would indicate how sensitive the coupled model is to the choice of the duration of the

Lagrangian simulation.

P 2055, L 24: what are those transformations? May be rewrite "... transformations as given in the equations below", if this is meant.

P 2056, L 3: both, m(r) and C(r) are time dependent, this should be taken into account in the equations.

P 2057, L24: Again, m(r) should also depend on time.

P 2057, L 10: This of course depends on the details of the "crude representation of C_3-D". So either those are specified here, or the discussion on seasonal variations should be dropped.

P 2057, L 22: Given the strong time dependence of biospheric fluxes on sub-diurnal time scales that influence the observations, it would be better to not limit the interpolation to a simple linear one, but instead use e.g. radiation to interpolate from daily to e.g. hourly fluxes. The implications of limiting the resolution to daily fluxes should at least be discussed.

P 2057, L27: The spatial and temporal resolution of the forcing meteorology should be given.

P 2058, L15: What is meant by "the offset values"? Is there a single number for the global average mixing ratio at the time of the start of the spin-up of the global Eulerian model, or was a site specific offset calculated from the average difference between observation and each model added? What was used as a spin-up time?

P 2059, L3: May be replace "the fewer difference with" with "smaller differences to the". Also: A good statistics would be the standard deviation of model-observation differences for the coupled and the Eulerian model, this could be included either in the text or in the figure.

P 2059, L12: this sentence is unclear, may be the authors mean "... would increase

when restricting the analysis to only the winter season"

P 2059, L18: The authors should state if this difference is statistically significant. Note that a correlation coefficient of 0.5 means that only 25% of the observed variations are explained by the model.

P 2060, L14: Given that daily fluxes have been used for biospheric fluxes, the simulated variations at sub-daily time scales are likely unrealistic. So the statement "the coupled model can resolve concentration variations at an hourly time scale or less" is not really supported.

P 2060, L18-23: This has not been shown in this paper. It strongly depends on the importance of mesoscale circulations (land-sea breeze, mountain-valley circulation) in the vicinity of observing sites. At least a reference needs to be given for such a statement.

Interactive comment on Geosci. Model Dev. Discuss., 3, 2051, 2010.