

## ***Interactive comment on “A global coupled Eulerian-Lagrangian model and $1 \times 1$ km $\text{CO}_2$ surface flux dataset for high-resolution atmospheric $\text{CO}_2$ transport simulations” by A. Ganshin et al.***

**Anonymous Referee #1**

Received and published: 26 October 2011

The manuscript presents an application of a coupled Eulerian-Lagrangian model for reproducing relatively high sampling rates time series in selected stations on a global scale.

The coupling is based on the concept of backward LPDM, with trajectories going backward in time for a predefined time interval (DT, 2-7 days here). The concentration field at the initial time (T-DT) is given by the Eulerian model while the subsequent emissions are explicitly sampled by the Lagrangian model through the use of higher resolution emissions (surface fluxes well mixed to a height h).

C921

The present work is based on a previously published model (Koyama et al. 2011) but with some modifications allowing the inclusion of higher resolution surface fluxes up to 1km x 1km resolution. Some techniques are proposed that make more efficient the use of 1km x 1 km emissions. This adds technical originality to the current work, beyond the pure application of a previous model just with a higher resolution.

The manuscript is well written and the explanations are sufficient in most of the cases. However, there are few technical details that should be added to make it fully understandable.

The results are clearly discussed and seem to support the usefulness of the proposed approach.

Some detailed comments follow.

1) In the introduction, page 2051, the statement “in the case of Eulerian model... the evolution is described by PDE” is somewhat misleading. In that context, after the explanation of LPDM, it seems that the authors claim that LPDM are not based on PDE, which would be a wrong statement. LPDM are based on probability density function (pdf) transport equations (i.e. PDE, Fokker-Planck equation) both forward and backward in time (see e.g. Thomson 1987, 1990). The Lagrangian particle representation, through stochastic differential equations (SDE) used in LPDM is perfectly equivalent to the PDE for the transport of the pdf. Indeed it can be easily proved (see e.g. Monin and Yaglom 1971) that the PDE for concentration with a K closure often used in Eulerian models is equivalent to an SDE on the particle position (i.e. the particle position is assumed as a Markov process). Also in the same paragraph the authors write that Eulerian PDE model equations are solved numerically using finite differences. This is also somewhat misleading. It would be better to write a more general statement since finite differences methods is only one possibility (for example, grid based discretization methods).

2) Page 2054, the conditional probability  $P(x_r, t_r | x, t)$  with  $t_r > t$  should be rewritten as

C922

$P(x,t|x_r,t_r)$  with  $t_r > t$ . the conditioning event is that the particles pass through the receptor point  $(x_r,t_r)$  and it is customary in probability to write it after the random variable.

3) I would prefer to not use the wording “imaginary particle” but “notional particle”.

4) In section 2.2 more details must be given about the adaptation needed to use the JCDAS data in FLEXPART (e.g. transformation of coordinate, vertical and horizontal interpolation). This will improve the reproducibility of the proposed work.

5) Again in section 2.2, was any interpolation used from the Eulerian grid to the particle position (space and time) ?

6) In section 3.1 page 2058 first line. The authors write that it is possible to use the same approach used for anthropogenic fluxes also for “land fluxes”. Do the authors mean terrestrial biogenic fluxes?

7) In section 3.3 the authors explain how the model VISIT has been used to define the terrestrial biosphere fluxes. However, I’m not sure from the text if the MODIS land cover was used inside the VISIT model (albeit with a redefinition of the biomes and spatial aggregation) or if it was only used afterwards for the interpolation to obtain a 1kmx1km flux map. Could the authors clarify this point?

8) In section 4.1, figure 1, the simulations showing the results obtained by using only the Eulerian model NIES-TM should be added for all the measurement stations.

9) CASA should be explicitly defined.

10) Why a different emission was used for the (terrestrial) biosphere fluxes in the Eulerian simulations? This seems to introduce an un-necessary inconsistency between the Eulerian and Lagrangian models.

11) In the conclusion the authors state that the use of a 1kmx1km resolution has a clear advantage in reproducing high concentration spikes over the 1degreex1degree simulations. This is conceptually true but for these simulations (as the author states

C923

in the previous section 4.3) in some cases there is no clear advantage. Indeed, in the case of Egham (for 2006) there is a slight worsening of the performances and there is no change for Fyodorovskoye. This should be discussed in the conclusions.

---

Interactive comment on Geosci. Model Dev. Discuss., 4, 2047, 2011.

C924