

## ***Interactive comment on “Daily CO<sub>2</sub> flux estimates over Europe from continuous atmospheric measurements: 1, inverse methodology” by P. Peylin et al.***

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### General Comments:

The paper applies a new method for regional synthesis inversion of [CO<sub>2</sub>] data collected continuously from tower sites in Europe. The method is very appealing for two main reasons: (1) the use of the “zoomed” grid LMDZ transport model allows mesoscale resolution without the need to estimate lateral boundary fluxes, and (2) an innovative numerical treatment allows the optimal estimation of initial conditions as well as thousands of daily surface fluxes in a computationally-efficient manner. The paper

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is well-written and clearly explains the new method, demonstrating its potential power.

As Dave Baker has pointed out in his review, the innovative numerical treatment should probably be seen as an intermediate step on the road toward inversion of spatially and temporally dense data by the end of the decade. Daily fluxes and initial concentrations are estimated from daily data at several sites, but the numerical machinery would probably break down given hourly observations at dozens of sites or (much worse!) hundreds of thousands of satellite observations per day. This paper explains a way to extend the life of batch synthesis inversions, but the end is near.

Unfortunately, the authors can't seem to decide if the paper is primarily intended as an exploration of methodological issues or an application to real observations. There are several interesting questions raised about the inverse methods that could have been fruitfully explored in greater depth using synthetic data, and in the final analysis the paper has little to say about the carbon cycle of Europe (as the authors explicitly state). Nevertheless, the use of real observations does allow methodological diagnosis of a different sort, and the paper should certainly be published, though I have some specific recommendations intended to improve the presentation.

#### Specific Comments:

1. The introductory discussion of tradeoffs among (1) resolution (in both time and space) of retrieved fluxes; (2) the loss of information incurred by averaging of the observations; (3) the structure of the transport model; and (4) the treatment of lateral boundary and initial conditions is particularly lucid. These seem to be really key issues that will define the landscape as our science evolves from a study of the "climate" of the carbon cycle to its "weather" over the next several years. A really thorough study of these tradeoffs using synthetic data would seem wise, and is clearly feasible given the tools used in this study, but the authors have chosen to postpone that analysis for another paper. Instead, they use real observations for a single month to explore the feasibility of regional synthesis inversion using a particular choice of problem setup

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(resolution, averaging, etc).

2. The paper would be stronger if the introduction contained a brief but final statement of the study's intended role as either "theoretical/methodological development" or "application to real observations." This kind of "soul searching" appears in too many places in the manuscript. Just as in the inversion setup, there were tradeoffs made and the authors decided to publish results of a real experiment. This reader would be more comfortable with these inevitable compromises if they were laid out in the beginning and the decision was justified once and for all, without looming as subtext throughout.

3. I'm troubled by the potential for serious errors to be incurred by averaging of the diurnal cycle in the observations. This is likely not too serious in the mountain stations, but at lower elevation continental towers one expects the 24-hour mean [CO<sub>2</sub>] to depend to first order on the degree of nighttime buildup under the stable boundary layer. In such conditions, the day-to-day variability which is the information content for the flux estimation might in fact be driven primarily by the strength of the nocturnal inversion rather than by advective effects. If so, then the transport model must be very good at simulating day-to-day variations in static stability at night, with vertical resolution sufficient to represent specific tower heights and local topography. This issue is not discussed adequately in the manuscript. It seems unlikely to me that the 50-km resolution of LMDZ is sufficient to represent local influences, though little is said about the model's vertical structure. Has the model been adequately evaluated for the mountain sites?

4. A related point is the diurnal cycle of the background fluxes. The background fields were calculated using the TURC model. Was the full diurnal cycle of these fluxes retained in the transport calculation, and then averaged in concentrations before comparing to the data? Or were daily mean fluxes from TURC prescribed to LMDZ and then optimized against daily data? I'm not sure which is better or more self-consistent, but this methodological choice should be justified and explained on p 1654. I would guess the structure of the retroplumes would be quite different at night than during the

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day, so this is probably not a trivial issue.

5. “The fossil fuel fluxes are set to fixed monthly estimates” from 1-degree datasets. Is there any information available to the authors about the true variability in Europe in November?

6. The description of the transport calculation (Sec 2.2) should probably include more information about vertical structure (especially resolution near the surface), subgrid-scale transport, and model evaluation that has been previously published for these sites. How are mountaintop sites treated in the 50-km grid? Should we worry about mountain-valley circulations, frontal lifting, convective transport, and stable boundary layers, or have these issues been sufficiently covered by Hourdin et al? Since this paper is still unavailable, it would be nice to briefly summarize the results here.

7. The treatment of the initial conditions is quite elegant and beautifully explained in section 2.3!

8. I can almost but not quite understand the treatment of prior flux uncertainty in lines 20-28 of page 1661 (in the sense that I could almost reproduce the calculation from the information given). Please explain in a little greater detail how the values were derived and what assumptions were made.

9. Bottom of page 1662: The large size of the background covariance matrix and its convenient structure is interesting. How big is it? Would the block approach outlined here be extensible to 50 sites or to hourly observations?

10. The reduced chi-sq diagnostic would seem to indicate that the 500 km length scale matches the data too well (p 1663, line 14). Can you comment on this? Does it help with the selection of L?

11. On page 1665, line 20-30: The discussion of the tradeoffs between accurate specification of the initial conditions and the treatment of their uncertainty in terms of the recovered fluxes seems really important to me. Can you be really explicit in the

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argument that tight priors on initial conditions reduce the possible corrections from this source and that this translates to a requirement for accuracy in the initial concentration field? Could this issue be treated by placing observing stations “upwind” of the region of interest?

12. P 1666, line 1: “cross constraints on the initial field could somehow freeze the updates.” This sentence doesn’t make sense to me. Please reword for clarity.

13. P 1667, lines 5-10: There’s an important alternate interpretation to the “clear result from the difference between these cases.” It may be that the fault should be ascribed to the use of correlation with distance rather than geographic or biogeochemical information as the spatial pattern rather than to the size of the regions. It seems to me that mechanistic hypotheses about fluxes can be tested against other observations more readily than inverse-distance weighting. It’s certainly plausible that the use of realistic spatial patterns prescribed from data actually increase the power of isolated concentration observations, and that this power is reduced by replacing them with agnostic length scales. Again, this is a methodological choice that involves tradeoffs that could be explored in a synthetic data experiment but are not possible given real observations.

Technical Corrections:

P 1662, line 23: “somehow justified” should be “perhaps justified”

P 1656, footnote: Isn’t Geels et al published now?

P 1660, line 19: insert “a” into “(with a little algebra)”?

P 1662, line 23: delete “it” “impossible to store in memory”

P 1663, line 26: Change “It reflects” to “This reflects”

P 1665, line 20 “an other” should be “another”

P 1666, line 21: Dargaville in prep. Is this allowable in ACP?

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P 1668, line 18: “loose” should be “lose”

P 1668, last line: “with monthly data’ should be “and monthly data”

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1647, 2005.

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