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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Daily CO_2 flux estimates over Europe from continuous atmospheric measurements: 1, inverse methodology" *by* P. Peylin et al.

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GENERAL COMMENTS:

The authors present a method for estimating fine-scale trace gas sources and sinks in a more computationally-efficient manner than traditional approaches. They use it to estimate daily fluxes at a mean resolution of 50x50 km across Europe and the adjacent part of the Atlantic for the month of November 1998. The initial concentration field at the beginning of the month is also optimized. Their approach of using fine-scale regional corrections to coarse-scale results from a global inversion has been used before (Law,

et al., 2002), as has their approach of using backwards model integrations to more efficiently compute the coupling of fluxes to measurements (Kaminski, et al., 1999a,b; Rodenbeck et al, 2003). What is new in our CO_2 inversion field is their use of a flux and covariance calculation requiring the inversion of a matrix of the dimension of the measurement vector, rather than the much longer state vector containing the fluxes and initial condition. This is done in a single batch inversion step using equations analogous to the measurement update step of the traditional Kalman filter, an approach that also permits the computation of the full-rank covariance of the estimation error.

The computational savings of this approach are greatest when the number of measurements is small compared to the number of fluxes estimated. When the number of measurements approaches the number of fluxes to be estimated, the advantage over the traditional synthesis inversions is largely lost. Because of this, the method is most useful when applied to the current CO_2 observing network, and to its in situ extensions for the coming decade. It would be less applicable to processing data from satellite-based instruments; these will produce such high volumes of data that this method would be overwhelmed if data averaging were not used extensively. Other approximate inversion techniques (variational data assimilation, ensemble filtering) would then have to be used, at the cost of obtaining a full-rank covariance estimate.

Two other aspects of the method are largely new in our field: the estimation of the 3-D initial concentration field at the start of the span, and the use of an approximate adjoint (the "retro-plume" approach). The authors use an efficient approach to solve for the initial concentration field at a reduced resolution to minimize the dimension of the state. The impact of using the "retro-plume" approach on the accuracy of the inversion is unfortunately not explored in detail here.

The authors are careful to point out that their main goal is the presentation of the method and their discussion of the key issues associated with it (efficient treatment of the initial conditions, specification of the error correlations, etc.), rather than the application of the method to the regional European flux estimation problem. It is well

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that they do, because the results they obtain using their limited set of six continuous sites improve upon their prior estimate only slightly. If the authors would have used simulated data instead of the sparse real data available, then several issues might have been examined that would have been of great interest to those who will later do similar inversions with more data:

1. How well do the error estimates represent the actual estimation errors (obtained from the difference of the estimate and the known truth) in the problem?

2. What is the minimum correlation length at which the flux corrections are spread well across Europe while not degrading the fit to the measurements significantly?

3. What are the actual errors in the estimate due to the transport model and to the approximate adjoint of the "retro-plume" approach?

We do not expect the authors to re-do their results with simulated data to address these questions, but we do feel this was a missed opportunity and hope that such issues will be addressed soon in a similar study. Without such a study, however, the results presented here are most useful for the questions they raise: What correlation length scale would give the most useful results? How may the length of the span and the size of the regional domain best be chosen to reduce the impact of the initial conditions and the boundary fluxes? What are appropriate a priori flux error correlations and magnitudes to use? and How useful are the error estimates obtained? The manuscript is worth publishing for these questions and for the outline of the method. The presentation of the document (especially the English spelling and punctuation) is quite poor at the moment, however, and significant revisions will be required before it is publishable.

REFERENCES:

Kaminski, T., M. Heimann, and R. Giering, A coarse grid three-dimensional global inverse model of the atmospheric transport, 1: Adjoint model and Jacobian matrix, *J. Geophys. Res.*, **104**, 18535-18553, 1999a.

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Kaminski, T., M. Heimann, and R. Giering, A coarse grid three-dimensional global inverse model of the atmospheric transport, 2: Inversion of the transport of in the 1980s, *J. Geophys. Res.*, **104**, 18555-18581, 1999b.

Law, R.M., Rayner, P.J., Steele, L.P., and Enting, I.G., Using high temporal frequency data for CO_2 inversions, *Global Biogeochem. Cycles*, **16**, 1053, doi:10.1029/2001GB001593, 2002.

Rödenbeck, C., S. Houweling, M. Gloor, and M. Heimann (2003), Time-dependent atmospheric CO_2 inversions based on interannually varying tracer transport, *Tellus*, *55B*, 488–497.

SPECIFIC COMMENTS:

page 2, column 1, paragraph 1, lines 6-7: This really affects the ability to constrain the fluxes *inside of* the continents.

p2 col1 par1 ln7-9: If you are going to make the point that the 40-day smoothing removes much of the signal, you ought to give a time scale (i.e. removes the detail at scales less than 40 days – those used in this study).

p2 par2 last 2 sentences: The resolving power of the continuous data is greater only if some assumption is made about the time variability across time scales equal to or longer than the synoptic sampling frequency. In other words, if the back trajectory from the measurement site flops across a flux region once every 5 days, then some assumption about the flux variability on time scales about 5 days long or longer must be made in order to get extra information out of the continuous time series. Otherwise, you have just increased the number of parameters to be estimated in step with the number of measurements, and no greater observability is obtained. To indicate this, perhaps you could reword the last sentence by replacing "predictable" with "assumed" or "specified".

p2 par5 ln2: "synthesis inversion" has not been defined before – define it here rather

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than later.

p2 col2 par3 ln10-14: You should note somewhere that errors in your assumed diurnal cycle must be considered somewhere (probably in the data error).

p3 col1 par 2: The authors use the "retro-transport" approach of Hourdin and collaborators. We were not able to find the Hourdin, et al 2004a,b references; they have apparently not been published yet in the QJRMS (as indicated in the reference list), neither in 2004 nor so far in 2005. The Hourdin and Issartel (2000) reference given here merely presents the analytical form of the transport adjoint equations, but gives no details on the numerical implementation. As a result, it is not clear to the reader how this "retro-transport" approach is implemented, and in particular how precise the numerically-implemented version is to the exact adjoint of the forward tangent linear model. It would be useful for the reader to know what impact on the flux estimates errors in this "retro-transport" approach would cause. If a reference cannot be found for this, some indication ought to be given here, if only a rough average number.

p3 col2 par2 ln1-5: Say explicitly that the 6 sites used here were not included in the 64 used in the global inversion, if this is true.

p3 col2 par3 ln13: The standard deviation of the hourly flux values, not hourly errors, correct?

p4 col1: Again, this reviewer could not locate the cited Hourdin, et al 2004a reference: please check the citation in the References to be sure this is correct. It is important for the reader to have access to this reference to be sure the method is correct.

p4 col2 par1 ln6-7: Again, it is very important that this paper be available to the reader – otherwise a number describing how good the agreement is should be added here.

p4 col2 par3 ln5-7: For clarity, you should note that each pulse is run back separately from the others.

p4 col2 par3 ln8: Maybe say "we averaged all hourly data across the full 24 hours" to

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be very clear that the average is across the full diurnal cycle.

p5 col1 equation (10): The transpose symbol is in the wrong place. Should be = V^T Cov(C|$_{t0}$) V

p6 col1 last paragraph: It is not clear what the block form of the matrix is due to: the separation between the initial concentration field and the fluxes for each day? It seems as though the most important part of the cost savings is not due to the block form of P_b but rather to the ability to project to the full spatial dimension line by line.

p6 col2 par4 ln4: For clarity, you should refer to "high-altitude" stations instead of "altitude" stations. Would suggest changing this throughout the manuscript.

Subtitle 3.2 Would suggest changing this to: "Contributions to the Model Concentration Output"

p7 col1 par3 ln8-15: The fact that the estimated daily fluxes depend strongly on the decay of the initial conditions and the invasion of the fluxes from the coarser regions into the domain seems like it should be one of the main conclusions. If the fluxes depend on the set up of the problem (time span considered, the nesting of the fine grid into the coarse grid) then this dependence must be thoroughly examined before presenting the flux results (for example, with simulated data).

p 7 col2 par3 ln6-7 from end: The Dargaville et al reference is not given on your reference list: either add it on the list, or delete it here.

p8 par1: Given that the shape (as opposed to the magnitude) of both the flux corrections and the reduction in error are quite similar in both the 500 km and 2000 km correlation length cases, it seems like it should be difficult to know which correlation length would be more appropriate given the results presented here. Rather than being "one clear result", it seems a bit of a stretch to then say that "the use of large regions in previous inversion studies had greatly overestimated the power of isolated concentration measurements to constrain regional fluxes", especially since these studies attempted

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to estimate monthly-scale fluxes rather than the daily-scale ones examined here.

p8 col1 par2 ln3: Contrary to what is stated, the reduction-of-error statistic does not directly measure the information added by the measurements. Since it depends strongly on the a priori errors assumed, it only indirectly measures the contribution of the measurements. If the a priori error is doubled, for example, the reduction of error will then become twice as strong, percentage-wise, in an area mainly constrained by the measurements rather than the prior. This statistic has been used in several papers recently, but it is really a poor choice for displaying the measurement constraint. A better option would be to use the measurement information content, given by $H^T R^{-1} H$, which would be positive near the measurement sites and would approach zero in areas not constrained by the data. A second better possibility would be the inverse of this quantity, $(H^T R^{-1} H)^{-1}$, which would give the covariance of the estimate when constrained only by the measurements (without the contribution of the prior).

Figure 3: The different lines on these figures are not labeled well. Also, it is not immediately obvious that "pixels" refers to the small LMDZ regions, and something more descriptive than "big regions" could be used to describe the results of the global inversion. What are the dashed lines in this figure?

Figure 4: Are these maps displaying the average quantities across the full month, or a snapshot for some particular day? This should be stated in the caption.

TECHNICAL CORRECTIONS:

Title: Either say "A daily CO_2 flux estimate..." or "Daily CO_2 flux estimates..."

Abstract, ln 9: move the comma from after "condition" to after "20 days"

p1 col1 paragraph 1 of Intro, ln5: "small-scale flux estimates"

p1 col2 par2 ln8: "data points"

p1 col2 par3 ln5: Maybe use "approach chosen" instead of "solution chosen"

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p2 col2 par4 ln5: "biweekly" suggests once every two weeks. Most flask sites take data once per week. Maybe change this to "weekly"?

p2 col2 par4 ln 4: "Denning et al (1996)" should be in brackets (?).

p3 col2 par1 ln12: use "monthly values" instead of "monthly statistics" to describe the fossil emissions.

p3 col2 par2 ln 5: "in meters" rather than "in meter" ?

p3 col2 par2 ln13: "hourly averages" not "hourly average"

p3 col2 par2 last line: "a following paper" rather than "the following paper"

p3 col2 par3 ln3: delete first "a"

p3 col2 par3 ln5: "used" instead of "use"

p3 col2 par3 last line: "needs" instead of "need"

p4 col1 par2 ln5: no parentheses around "Figure 1"

p4 col1 last paragraph, ln2: "as is the case"

p4 col1 last paragraph, ln8: "These properties"

p4 col2 par1 ln3: "differs"

p5 ln1: "With our short study period, we..."

p5 col2 par2 ln 4: "November 1st"

p5 col2 par2 ln15: "with some algebra"

p5 col2 par2 ln24: "each element in the initial condition"

p5 col2 par3 ln5: do not capitalize "under-constrained"

p6 col2 par3 ln6: fix the chi-square statistic symbol typesetting p6 col2 par3 ln19: ditto

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p6 col2 par4 ln9: "that usually last"

p7 col1 par1 ln7: "LMDZ"

p7 col1 par2 item 3: "toward a non-zero mean value"

p7 col1 par3 ln3: "North Atlantic": change it here and elsewhere in manuscript

p7 col1 par3 ln4: "high-altitude": change it here and elsewhere in manuscript

p7 col2 par3 ln8: fix the meters-squared part of the units here

p8 col1 beginning of par3: Capitalize "Western" and "Central"? Use "outlines" or "boundaries" instead of "contours" to describe the regions in figure 1.

p8 col2 par2 last line: "oversimplified"

Fig 2 caption: "dashed lines" "their uncertainties"

Fig 3 caption: "two regions"

Fig 5 caption: "North+Central" and "North Atlantic"

SPELLING ERRORS:

Besides the problems found above, the authors have done an poor job of proofreading their manuscript. I would have hoped that they would have at least run a spell-checking program through it before submission. The state of the manuscript is such that the editors could reject it on those grounds alone, should they be looking for an excuse... I will leave this decision up to them and merely suggest major revisions. While I do not appreciate being put in the position of copy editor as a reviewer, I might as well provide the following spelling corrections that I came across and suggest that a more thorough editing of the document be made to improve the punctuation and grammar:

p2 col1 par1 ln5: "interpreting"

p2 col2 par1 ln7: "pointed out in Chevillard"

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p2 col2 par3 ln 9: add closing quotes on "well-known"

p3 col1 par1 ln2: "such an effect"

p3 col1 par1 ln13: "easily"

p4 col1 par2 ln4: "stretched"

p4 col1 par2 ln7: "relax"

p4 col1 last paragraph, 2nd to last line: "symmetry"

p6 col2 par2 ln6: remove extra space after "inversion" before the comma

p7 col1 par1 ln2: "great care"

p7 col1 par1 ln10: "prerequisite"

p7 col1 par3 2nd-to-last line: "too-low model concentrations"

p7 col2 par1 ln3 "another"

p7 col2 par3 ln13: "ventilation"

p8 col1 par3 lines 3 5 from end: "length" not "lenght"

Fig. 4 caption: "length" not "lenght"

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