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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical note: an interannual inversion method for continuous CO_2 data" *by* R. M. Law

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This paper presents a next step in exploring the potential value of high frequency data for atmospheric inversions of sources and sinks of CO2 and the development of the numerical tools to deal with this inverse problem. It points out that high frequency measurements have the potential to not only improve multi-year averaged fluxes and seasonal cycles (as indicated by previous studies already) but also to improve the ability of inversions to reproduce inter-annual variability. Furthermore it demonstrates a method that is computationally more efficient, and is actually required to make larger problems computationally feasible.

Overall the presentation is concise and clear. The methods seem scientifically sound. Most of the conclusions follow readily from the results, except for the bias issue (as will

be explained). I see no reason to withhold it from publication provided that it addresses the issues raised below, some of which will hopefully trigger some discussion.

GENERAL COMMENTS

Method

It is unclear how the background fluxes (those that fall outside the 2 year optimization window) are treated. Supposedly they are transported forward using response functions that should then be longer than 12 months. It should have been tested how well the response functions reproduce the forward transport model, and therefore how well the posterior concentrations reproduce the measurements in the forward model (those forward modeled concentrations will differ from the results of a response function multiplication that are used in the optimization). This should be discussed somewhere briefly. It should also be mentioned how well the posterior uncertainties of the sequential inversion match that of a batch inversion. The agreement will, o.a., depend on the treatment of the uncertainty of the background fluxes.

Discussion

It would be useful if links were provided between the current findings and previous work. It would, for example, be worthwhile to mention how the results for the mean flux and seasonal cycle compare with the cyclo-stationary case. Does the present method tell a different story? Was the cyclo-stationary assumption justified for studying 4HR data?

'We can then begin to invert real continuous data'. The question remains what such an inversion would look like. Would it resemble the setup that was used in this study? In my understanding there are only few background sites were CO2 is being monitored continuously. In addition, one would probably want to use flask data simultaneously. To what extent would the findings of this study apply to such an inversion?

A related question is whether this work or other studies indicate that continuous mea-

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surements should receive a higher priority. Law (2003) indicates that the added value of continuous measurements increases for smaller networks. Could we conclude that it is more efficient to add more sites in poorly sampled places and probably some continuous sites on highly heterogeneous sources regions and by doing that, and having that in place there would be no further need for high frequency samples at remote sites?

SPECIFIC COMMENTS

Page 5979 Method

A transport model resolution is missing here. The extent to which high frequency data can be exploited by the inversion will, among different factors, depend on the extent to which synoptic scale variability is resolved by the model. Except for some numbers it would be interesting if some thoughts were given on model-limitations in real world applications and the extent to which the synthetic computations may provide a too optimistic or rather a too pessimistic impression of the benefits of continuous data.

Page 5981 Data

4 hourly data on continental sites suggest that additional constraints may come from observed diurnal cycles. This seems to be missing in this analyzes. It may be for good reason, but even then it would still be worth mentioning this.

sqrt(n)

It is mentioned that data uncertainties are scaled by square root n. The text indicates a multiplication by this factor while the reasoning behind it rather suggests a division. This should be clarified. Related to this is the question of how the 4HR and MON uncertainties relate. Table 2 suggests that there is another factor 2 (...?). Please clarify this.

Page 5982 'The 1981 source estimates are discarded (as unreliable)'

In fact, the same reasoning applies to the last months of 1997, since those fluxes are

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also seen by less data.

page 5983 'Most of the bias is likely to be due to aggregation errors'

Biases that increase with an increasing time resolution of the measurements hint at aggregation errors in time. This would happen, for example, if the temporal evolution of the prior fluxes within a region (=month) differs from the real fluxes. For the presented experiments this cannot be judged since it remains unclear what time evolutions are used (e.g. constant over a month, linear interpolated between months, ...). Furthermore this outcome seems in contrast with Law (2003), where 'The inversion quality, as determined by bias and uncertainty, is degraded when averaging over longer periods'. Finally, the fact that 'larger biases tend to occur for regions without sites nearby' seems to contradict Kaminski et al (2001) who point out that biases result from uneven sampling by heterogeneous networks, this problem should be less for regions that are not well seen by the network. It seems more likely that the poorly sampled regions are just not well enough constrained by the network.

MINOR COMMENTS

page 5979 '... shown in Fig. 1'

It seems more logical to refer to Fig. 6 (if I'm correct there is no reference to this figure)

page 5999 Figure 5

The figures are just a bit too small to see without feeling a starting headache (please enlarge)

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5977, 2003.