Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical note: an interannual inversion method for continuous CO$_2$ data" *by* R. M. Law

P. Paul (Referee)

pip@io.harvard.edu

Received and published: 18 December 2003

OVERVIEW

This paper outlines a methodology to use hourly observations of CO2, within an inverse model framework, to determine emissions of CO2. These data have the potential to provide additional information that has been up until now largely ignored, mainly for reasons involving the numerical expediency of large-scale, multi-year inverse model calculations.

This paper is suitable for publishing in Atmospheric Chemistry Physics, after the author has addressed the comments I have outlined below.

GENERAL COMMENTS

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

After reading the paper it is not clear to me whether these hourly data will be more likely to determine the spatial distribution and magnitude of the "missing" sinks of CO_2 than the monthly mean concentration data; this I believe is the ultimate goal of the inverse model analysis that will use these hourly data. The improved temporal resolution of the hourly data provides a more detailed picture of CO_2 in the atmosphere but is the current generation of global forward models (CO_2 source/sink processes) good enough to exploit this information? I appreciate that the paper is a technical note, but I believe it is important to discuss, at least to some extent, the pros and cons of the hourly data.

SPECIFIC COMMENTS

1) Can the four-hourly data be assumed independent or is there some level of autocorrelation associated with such a timeseries? The autocorrelation will reduce the impact of the hourly data.

2) Using the new data will increase the representation error. This is not mentioned in the paper.

3) How well can the 116 regions be retrieved, i.e. how independent are the retrieved state vector elements? Does better temporal resolution data improve the independence of the retrieved state vector?

4) Refer to Table 1 with the Figure showing the region definitions (which is currently the last figure).

5) In step 2 of the sequential synthesis inversion, the covariance between the state vector elements is ignored. Are these covariances significant? This represents important information. I understand that it is difficult to include this information in a large-scale inversion but could it be parameterised?

6) Page 5982. Why are the 1981 source estimates unrealistic? Are they simply initial conditions for 1982?

7) What is the e-folding time for the monthly responses? This would be useful to state

in the text.

8) Is any noise added to the "modeled" sources, e.g. from model error? If it is I can't find any mention of it in the text.

9) The correlation between the estimated and correct residual fluxes is a measure of the "shape" of the interannual variability. I don't understand this point. Needs to be clearer.

10) Figure 3 shows the correlation. Does this refer to r or r2?

11) The fact that the inversion results are insensitive to prior uncertainties implies that the cost function is determined largely by the observations. Is that correct?

12) In Figure 1, would a relative bias be more useful to show?

13) The results and the figure caption of figure 5 are not clear. It would be clearer to show (3 months - truth), (6 months - truth)... I think this would illustrate the degradation better.

TECHNICAL COMMENTS

1) Step 2 in method description. Suggest removing "two years".

2) The values for the level 3 source uncertainties are double those of the monthly mean inversions. This is mentioned on page 5979 but explained on page 5981. Suggest combining sentences.

3) On page 5984, it would be easier to read if the author mentioned that the problem with land/ocean misallocation will be discussed in the next section.

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