

Interactive comment on “What can tracer observations in the continental boundary layer tell us about surface-atmosphere fluxes?” by C. Gerbig et al.

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This paper describes the information available using the combination of atmospheric measurements, a transport model, a simple model of biospheric flux and various assumptions about the prior uncertainty of the unknown parameters. Methodologically it is an application to regional space scales and higher time resolution of the approach described in [Kaminski et al.(2002)]. The use of pseudodata, while disallowing any physical interpretation, does allow more commentary on the observing system than in [Kaminski et al.(2002)]. The authors put this to good use commenting not only on

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the information content (embodied in the reduction of error) but also in the dispersion of a set of estimates using different realizations of the first guess or prior estimate. Normally this would be only a numerical demonstration of Bayes Theorem but much of the paper studies cases where the prior or background uncertainty assumed for the unknown parameters is different from that generating the realizations of the prior estimate. This is a problem we will face in real cases where, of course, we do not know the structure of the prior uncertainty.

The authors show that correct choice of the structure of prior uncertainty (in this case embodied in a correlation length) is important for good retrievals. In principle this is not big news. It has been well-known, for example, in the numerical weather prediction community for many years that correct specification of the background error covariance (their term for the prior uncertainty) is critical to a good assimilation and also something of a black art. Just how important it is, and what one can reasonably do about it, is problem specific however and it is very much worth learning for the types of applications described in this paper. The authors make a real contribution to this problem. I therefore am happy to recommend the paper be published almost unmodified although I would make a couple of suggestions for reorganizing parts and also a couple of questions on the paper.

I would like to see the whole discussion of discretization brought together. The authors raise the concept of “aggregation error” in three places in the paper. First they refer to the choice of spatial grid. This is the classical sense in which we first used the term in [Kaminski et al.(2001)]. Second they acknowledge that the choice of scaling parameters multiplying fixed functional forms (radiation and temperature) represents a temporal aggregation error. Thirdly they comment that the reduction of effective degrees of freedom inherent in large correlation lengths is also a form of aggregation error. These are all correct and important; the second especially so I think. Aggregation errors occur when inhomogeneous sampling covaries with fluxes that cannot occur within the statistical model. Note that both elements are important. As a temporal example consider

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the impact of water-stress on plants. This is not included in the biosphere model in the paper. Further the passage of a rain-bearing front that changes the water-stress conditions also reorganizes the large-scale flow so that different parts of the domain are observed from the tower. This is quite likely to bias estimates of the model parameters. I also think this is different from the aliasing of unmodelled processes. To quote an example I am more familiar with: in [Rayner et al.(2005)] we calculated a sensitivity of respiration to temperature that almost certainly tried to account for the higher biomass burning sources one may see in a hot dry year. The mistake here is interpretive, but aggregation error, at least as originally defined, has not occurred. I believe the authors understand all this very well but I believe they could do the community a service by bringing the threads together in the discussion section.

My other question concerns the biosphere model itself. I am surprised to find only one parameter in the respiration relationship. This seems to suggest that all the respiration-temperature curves pass through (0,0). This seems unlikely to correctly represent the AmeriFlux data. Normally models include something like a base rate of respiration. One can think of it playing the role of an intercept in a regression. This value will have a large impact on the source-sink status of a gridcell. We could only remove it in [Kaminski et al.(2002)] by assuming an annually averaged biosphere. One might argue it does not matter with the high time-resolution of the data used in this study but I believe the advection of mean structures over the observing site contributes substantially to the variability in measured concentration Misinterpreting this mean structure as commenting on the temperature sensitivity seems a problem. If so, I would mention this in the text, otherwise explain it in the response.

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