

Interactive comment on “A Bayesian inversion estimate of N₂O emissions for western and central Europe and the assessment of aggregation errors” by R. L. Thompson et al.

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We thank the reviewer for his/her helpful comments.

General comments

"A few of the synthetic experiments in this paper lead to conclusions that do not go beyond the general conclusions on limitations of Bayesian inversion systems and the influence of choices in error correlation structures, I would like to see more specific conclusions towards this specific case for inversion of N₂O fluxes using one (!) receptor point and what this tells us for the direction to go to improve the inversion system and the observation (network)."

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There are a number of important and novel conclusions in this work, firstly the importance of temporal aggregation error in the inversion framework and that these errors may be accounted for using the Kaminski algorithm (as far as the authors are aware, this study is the first to investigate the influence of temporal aggregation in atmospheric inversions and the first to test this algorithm), and secondly, that (at least in the case of perfect transport) averaging the observations deteriorates the retrieval of the fluxes. These two conclusions were investigated using a single observation site, and we agree that as an important next step these need to be investigated with multiple sites and in the case where the transport is not perfect. The decision to use one observation site was made in order to avoid problems of scale/calibration offsets, which are very important for measurements of N₂O and to simplify the interpretation of the results of aggregation errors. Furthermore, N₂O observations from the sites included in the study of Corraza et al. ACPD (2010) were only very recently made available and after this study was started.

p26076, I6: the precisions (mostly reported) and accuracies (most often not directly reported but may be estimated from inter-comparisons) differ for all the sites and is not very relevant here. We, however, report the precision and accuracy for Ochsenkopf (p26078, I8-10).

p26079, I9: the column height for the calculation of mixing ratio (i.e. $0.5 \cdot H$) is based on previous studies using STILT (Gerbig et al., 2003). We have modified the text accordingly.

p26082, I18: We agree that the temporal and spatial resolutions should ideally be determined on the basis of the actual flux variability. However, in the case of N₂O the actual scale of flux variability is not well known, and even if it were, there are still practical limitations on the number of state variables that can be solved for, these are both computation as well as the limited resolving power of the inversion. While these problems may be resolved by using a smaller domain, and thereby reducing the number of variables, other problems arise, such as the increased importance of errors

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at the domain boundary.

p26092, l1: The reasons for only using one site were to i) avoid problems of scale/calibration offsets, which are very important for measurements of N₂O and ii) to simplify the interpretation of the results of aggregation errors (as discussed above).

p26094, l6: This is a very interesting point for a number of reasons. It is true that N₂O emissions from soils are strongly influence by management (e.g. fertilizer application) and the timing of this with respect to environmental factors such as rainfall. However, even state-of-the-art flux models e.g. Orchidee O-CN (Zaehle and Friend, GBC, 2010) and CERES (Rolland et al., Environ. Pollution, 2010) have difficulty reproducing the sporadic N₂O emissions. Furthermore, these events can be very short-lived, lasting only a few days. First, it is not clear, whether an inadequate representation of these emissions in the prior would improve the inversion above a smoother prior in which these events were not represented. Second, the temporal and spatial resolution of such an inversion framework would need to be considerably higher than current frameworks in order to resolve these.

Minor comments

We have taken note of the minor comments but we feel it is not necessary to reply to each one individually in our response here.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/10/C14718/2011/acpd-10-C14718-2011-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 26073, 2010.

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