

Comment by L. Golston

We would like to thank L. Golston for the additional comments. In the following we carefully address the comments point by point. First we repeat the comment (C#...) and then give our response (A).

C#1: The authors describe two methods for quantifying point source emissions, one based on in-situ mass balancing and one on remote sensing coupled with an inverse Gaussian plume model, along with field data comparing the two methods. I would like to add three comments on the description of the in-situ method, which I hope will help strengthen that section.

It was surprising that Gordon et al. 2015 (AMT 8:3745-3765) and Cambaliza et al. 2014 (ACP 14:9029-9050) are not discussed or even cited, given that they both investigate in detail the uncertainties of the in-situ aircraft mass balance methodology.

Gordon et al. 2015 discusses issues of interpolation, extrapolation, turbulent fluxes, and issues related to determining the background concentration in the context of determining emissions from an elevated source as was done here, while Cambaliza et al. 2014 also considers interpolation, boundary layer entrainment, and other effects. On page 4 transparency of interpolations and extrapolations is claimed as a benefit here, but seem less well developed than in either of those papers. The description of both on Page 8, is actually specifically not transparent and neither seems to be included in the error budget of Table 4. Part of the reason why the in-situ method is discussed in such detail seems to be because a variation on the mass balance method is presented, however the differences and benefits are not clearly distinguished in Section 4.1 or the results.

A: Two papers where Cambaliza was a co-author were referenced. However, the additional references from Cambaliza et al. (2014), and Gordon et al. (2015) were studied now in detail and were helpful. In the revised manuscript, we discuss especially the link to points that Gordon et al. have mentioned.

C#2: I also recalled that Figs 3, 4, and part of 2 are identical to Figure 3 in Hacker et al. 2016 (Animal Production Science 56:190-203), who cited the report from the authors of the current paper, Bovensmann et al. (2014). Since the figures are now also in Hacker et al., I think that the original Bovensmann et al. (2014) should be referenced here to avoid confusion.

A: The new Figures 2 and 3 for the measurements and 4 to 8 for the gridding are much clearer now.

C#3: Finally, it would be helpful to know whether the turbulent (5 Hz) could be resolved as indicated in the caption for Figure 3 or if there was attenuation, and how the inclusion of the turbulent flux compares to not including it.

A: This is now explicitly done by providing our standard fluxes (averages plus inter- and extrapolations of local mass x wind) plus 'flux 2', which was calculated after the averaging of the mass- and wind-field by our method, and by Kriging.

Since a complete revision was performed, and the separation of more individual sources was possible, the results as displayed in table 3 and Figure 18 were updated. The details are presented in a separate supplement.