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Atmospheric Measurement Techniques Discussions

Interactive comment on "Areal-averaged trace gas emission rates from long-range open-path measurements in stable boundary layer conditions" *by* K. Schäfer et al.

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We answer on the basis of the Referee Comment of the Anonymous Referee # 2 which is repeated in the following. At first we give the statement that it seems to us that this Referee Comment is based on the first manuscript and not the revised one which is available for the Open Discussion online. If so, then we are not sure where to go with it since we did a major revision already - on the basis of referee report 1 in the quick review phase. Nevertheless we are working at the answers to the Referee Comment of the Anonymous Referee # 2 to provide a revised manuscript after the discussion phase. "The manuscript addresses the important question how to quantify emission

rates of trace gases like NH3 or N2O as spatial averages over areas of one ha or more. The authors state that this can be more readily done using spatial-integrating micrometeorological methods than the widely-utilized small chamber measurements. This statement, for which they do not come up with a justification from their own original research, forms their motivation to evaluate several micro-meteorological flux-gradient methods utilizing non-intrusive path-averaging measurement methods for determining land-surface emission rates of trace gases under stable boundary layers." <We describe our own chamber measurements in the manuscript (see also the further Referee Comment below).> "The authors correctly state that successful application of a flux-gradient method requires confidence in the gradients of trace gas concentration and wind and in the applicability of boundary-layer turbulence theory, which is especially challenging for stable stratification. The study is based on two experiments in Indiana (USA) and Fuhrberg (Germany). Concentration differences of N2O were measured during the Fuhrberg experiment by two bi-static open-path FTIR spectrometers applying a correction of the bias between the FTIR spectrometers. 3-D sonic anemometers were installed at the same heights as the FTIR open paths. Concentration gradients of NH3 were determined during the Indiana experiment from scanning TDLAS measurements. 3-D sonic anemometers were installed at 2.5 m, 4.4 m and 16.2 m height. The authors employ two flux-gradient methods based on Monin-Obukhov similarity theory (MOST), which assume a loglinear profile of the wind speed and concentration gradient. In addition they use a more generic flux-gradient method, which they call 'similarity' method, using turbulent diffusivity as exchange coefficient. Finally, they utilize a so-called integrated horizontal flux (IHF) method based on the product of an interpolated mass concentration distribution and interpolated wind speed normal to the measurement plane. The authors assume that the flux determined from the IHF method was closest to the actual flux. The micro-meteorological measurements are complemented by small chamber measurements. From their quality-assured measurements the authors conclude that applying flux-gradient methods based on MOST results in incorrect vertical profiles and thus fluxes in the stable boundary layer. In general, the manuscript

should be shortened where the topics are well described in the scientific literature." <This is done on the basis of the referee report 1: see the Author Comment amtd-5-C411-2012.> "The manuscript is partly confusing since the two experiments are rather different with respect to scientific objectives, study designs, instrumentations and environmental boundary conditions. I have the impression that the way in which the two experiments have been put together has to be optimized such that the reader is able to see how the authors come to their general conclusions, and which of the results are depending on the specific site. My major concern is, however, that the manuscript is not able to prove which of the methods is most appropriate to quantify the actual fluxes. It mostly shows the similarities and dissimilarities of the different methods, and it argues on the reasons behind the findings using general statements from the scientific literature, which may probably be true, but this has not been tested by their own analyses." <All this is done on the basis of the referee report 1: see the Author Comment amtd-5-C411-2012.> "I therefore recommend to completely revise the manuscript not only to make it more compact and readable but to come up with scientific analyses that are, at least, clearly showing the strengths and weaknesses of the different methods." <We did a major revision during the quick review phase already on the basis of the referee report 1. Further, on 16 March 2012 the Anonymous Referee # 1 submitted a Referee Comment (amtd-5-C255-2012) which is identical to its review from the quick review phase. In agreement with the associate editor we posted a reply to the Referee Comment of the Anonymous Referee # 1 even by repeating the content of our original response to referee report 1 during the quick review phase (see Author Comment amtd-5-C411-2012). So, the documents 'Referee Comment amtd-5-C255-2012' and 'Author Comment amtd-5-C411-2012' do not refer to the revised manuscript which is available for the Open Discussion online but to the original manuscript. It could be that this Referee Comment of the Anonymous Referee # 2 is based on the original manuscript also. This conclusion seems possible because the Anonymous Referee # 2 is repeating a lot of phrases of the Referee Comment of the Anonymous Referee # 1 like: First topic: Referee 1: "While I think the topic of the paper of the paper is impor-

tant and fits well with the journal's scope, I find the MS too long and too discursive in its present form. I feel its length could be reduced by a considerable amount, maybe 40 or 50% with the heaviest reductions in Section 1, the Introduction, and 3, Methods. Those Sections contain interesting information, but much of it is unnecessary." Referee 2: "In general the manuscript should be shortened where the topics are well described in the scientific literature." Second topic: Referee 1: "The telling of the story seems to dart off in different directions unexpectedly in these 2 Sections and I suggest that their structures be tightened to more concise and logical forms I am concerned that there is no really convincing demonstration either in the paper or in the references that flux-gradient relationships developed for measurements in the vertical plane apply over long horizontal paths of around 100 m." Referee 2: "The manuscript is partly confusing since the two experiments are rather different with respect to scientific objectives, study designs, instrumentations and environmental boundary conditions. I have the impression that the way in which the two experiments have been put together has to be optimized such that the reader is able to see how the authors come to their general conclusions, and which of the results are depending on the specific site." Third topic: Referee 1: "The authors consider the problem, but seem convinced that their quality assurance measures will overcome it. To me, something more convincing seems necessary, although I must admit that I can't say what it is; perhaps more chambers or anemometers or scintillation measurements or better still some demonstration either through measurement or modelling that time integrated means will iron out local perturbations." Referee 2: "My major concern is, however, that the manuscript is not able to prove which of the methods is most appropriate to quantify the actual fluxes. It mostly shows the similarities and dissimilarities of the different methods, and it argues on the reasons behind the findings using general statements from the scientific literature, which may probably be true, but this has not been tested by their own analyses." Specific comments Since I recommend a major revision I will not go into the details of the manuscript in its current form. Nevertheless, the authors should take the following points into consideration when revising the manuscript: a) The English language

needs improvement. <This is done on the basis of the referee report 1: see the Author Comment amtd-5-C411-2012.> b) The layout of the manuscript should be modified such that related information is not spread over different sections. <See second topic above: This is done on the basis of the referee report 1: see the Author Comment amtd-5-C411-2012.> c) The figures are partly incomplete and difficult to read. <This is done on the basis of the referee report 1: see the Author Comment amtd-5-C411-2012 to Figure 8, 9, 10, and 11.>

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 1459, 2012.