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Interactive comment on “Advection of NH₃ over a pasture field, and its effect on gradient flux measurements” by B. Loubet et al.

B. Loubet et al.

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Reply to Reviewers 1 and 2.

We are grateful for the referee comments which have helped us improving the manuscript. In particular the question of the 2D vs 3D treatment of the dispersion has been analysed and it turns out that the 2D treatment of local advection is justified within certain limits (restricted wind sector), and may underestimate the real advection errors. Moreover, as suggested by the Referee #2 some more theoretical results on advection errors are summarised in the new manuscript which we hope will help the reader to have a broader view on local advection errors magnitudes. In the following we address the points raised by each referee.

Reviewer 1 Reviewer 1: This manuscript presents a novel method for estimated error

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in micrometeorological fluxes due to violation of the assumption of the non-divergence of the vertical flux. Micrometeorological flux techniques provide the most direct measurement of the air surface exchange of trace gases however they are limited to areas that have stationary flow fields and horizontally homogeneous source strengths. Techniques that quantify the error in micrometeorological flux measurements that do not meet the desired fetch requirements are needed by the measurement communities to better understand the air-surface exchange of pollutants and quantify fluxes from areas with heterogeneous source strengths and non-ideal flow fields, i.e. urban settings, small scale agricultural operations, and mixed agricultural operations, etc. Reviewer 1: General comments : I find the term "measured" advection error a bit misleading and is referred as an inferred measurement in section 3.4. The "measured" advection error in the manuscript is a largely deterministic analytical model estimate of the advection error driven by measurements while the modelled advection error is a semi-stochastic model estimate also driven by measurements. The terms "inferred advection error" or "deterministic model advection error" would be more appropriate.

Reply: This is a sound remark. The term “ inferred advection error ” seems indeed more appropriate. It has been changed accordingly in the text.

Reviewer 1: Much of the analysis between the "measured" and modelled flux advection error was for cases where the "measured" values relied on a constant vertical wind speed profile and a linear horizontal concentration gradient from nearby sources and sinks. Alternative wind speed profiles and horizontal concentration gradients were only briefly mentioned.

Reply: This is indeed one point that needs clarification and we are grateful to the referee to have pointed that out.

The integral in equation (2) has been evaluated assuming vertical log-profiles for wind speed and concentration (the details are in the new manuscript). With these assumptions, we obtain a correction factor to equation (2) which is a function of z and z_0 .

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With $z_0 = 40$ mm and $z = 1$ m we found a correction factor equal to 0.57. Using this corrected equation, leads to a better agreement between the inferred local advection errors and the modelled ones. The text and equations have been changed accordingly in the manuscript.

Regarding the horizontal concentration gradient, based on model outputs (e.g. Figure 4) it seems difficult to give a general form for a horizontal concentration profile. However, assuming that the horizontal concentration gradient is logarithmic (taking a reference distance $x = 0$ at the downwind edge of the farm : $c(x) = a \cdot \ln(x) + b$), we could work out a general form for the horizontal concentration gradient $dc/dx = a/x$. Doing that and using Site 1 and Site 2 concentrations, gives an inferred advection error, at Site 1, 16 % smaller than with the linear regression.

Combining the two corrections here above gives a much better agreement between the model and the inference method as shown by the new Figure 6. What is also interesting with the log-regression is that it allows to evaluate the inferred advection error at Site 1 and at Site 2 assuming a logarithmic fit, and this leads to advection at Site 2 being simply $F_{adv}(\text{Site2}) = (x(\text{site1}) / x(\text{site2})) \cdot F_{adv}(\text{Site1}) = 0.74 F_{adv}(\text{Site1})$.

Reviewer 1: The "measured" advection error with horizontal concentration profiles based on a logarithmic decay and a vertically variable wind speed profiles would be more consistent with previously measurements and would be more consistent with the profiles estimated by the FIDES-2-D model. A comparison of the "measured" results with more realistic horizontal concentration and vertical wind speed profiles and modelled results is merited.

Reply: Indeed this is a very sound remark. Making the following general assumptions on $u(z)$ and $c(z)$ profiles being logarithmic (no thermal stratification effects considered):
 $u(z) = u^* / k \ln((z-d)/z_0)$ $ca(z) = c_0 + c^* / k \ln((z-d)/z_0)$

Evaluating the integral of the product $u(z) \cdot c(z)$, after some manipulations, we get a simplified expression: $\int (u \cdot c \, dz) = (u(z) - 2u(z)/[\ln((z-d)/z_0)] \cdot c^* \cdot z + u(z)/(\ln((z-d)/z_0))$

$$d)/z_o)^*z^*[co + 2*(ca(z)-co)/(\ln((z-d)/z_o))] \text{ (E1)}$$

Now the advection error is the integral over z of $u^*(dc/dx)$. Hence assuming that the derivative over x can be taken out of the integral, F_{adv} is simply the derivative of (E1) over x . Assuming that (1) co is not a function of x over the grassland which is reasonable assumption since NH_3 sources are rather constant concentration sources; and (2) u and u^* are not a function of x which is a basic assumption in this study, one gets: $(E1) = u(z) * dc(z)/dx * x^* [1 - 2/\ln(z-d)/z_o) + 2/(\ln((z-d)/z_o))^2]$

Now taking the integral at the limits $z_o \sim 40$ mm and $z_{meas} = 1$, gives the factor under parenthesis ~ 0.57 .

This new approach is exposed and used in the revised manuscript.

Reviewer 1: The last paragraph of page 173 set up two hypotheses, one in which the surface flux from the field is constant and the other where the canopy compensation point is constant. Little is mentioned regarding these hypotheses after this point. The results of these hypotheses may be able to constrain the physical interpretation of the results. Where the tuned surface fluxes and compensation points in agreement with the vast array of field measurements? Did they exhibit a similar temporal trend as the measured surface fluxes?

Reply: The tuned surface fluxes were in agreement with the measured fluxes as is shown in Figure 8. The constant surface concentration and constant surface flux assumptions gave identical surface fluxes ($y=0.97x$ $R^2 = 0.9997$) but consistently different advection errors. The constant source hypothesis gives 1.7 higher advection errors at Site 1 than the constant concentration hypothesis.

The variability of the ratio between the constant surface and the constant concentration source advection error is completely explained by the stability factor z / L (new Figure 8 in the revised manuscript): for unstable conditions, the two methods are consistent (ratio ~ 1.2 - 1.3 , while for moderately stable conditions ($z / L \sim 0.1$) the ratio reaches

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6 and decreases slowly for larger z / L towards 4. This is an expected results, since under unstable conditions, the large mixing leads to a low surface concentration, which stimulates emissions from the constant surface concentration source (and hence we observe a similar behaviour between the two type of sources), while under stable conditions, the surface concentration source will rapidly decrease due to an increased surface concentration (advection and low mixing). This has some consequences on the advection errors expected for different biogenic sources: ammonia is a rather constant-concentration source (NH_3 is lying on the surface and is free to volatilise), while NO , N_2O or CO_2 respiration are rather constant source processes (based on bacterial activity), hence in the later case the advection would be larger.

Reviewer 1 Specific comments Reviewer 1: Page 167 Line 21: "which is what is seek", This is a typo and should be corrected

Reply: This has been changed.

Reviewer 1: Page 168 Equation 3: Why was $u(z)$ assumed to be invariant with height? Clearly this is not the case under normal environmental conditions. The classic log linear profile or the power law function used in section 2.3 could have been easily incorporated into equation 3.

Reply: This has been changed and it clearly modifies the inferred advection error (see answer to previous questions).

Reviewer 1: Page 168 Lines 13 to 15: What micrometeorological technique did you use to estimate z_0 and d ? Did your estimate agree with those presented in the literature for agricultural corps? Before z_0 is introduced in the text it should state that the measurements were taken over a grassland and give the crop height or a range of the crop heights.

Reply: z_0 and d were estimated with a wind speed profile and agree well with other data on grasslands. We have added a short statement about this and make reference

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to Nemitz et al. (2009).

Reviewer 1: Page 171 Lines 3 and 4: Please define "d". I assume that it is the zero plane displacement height. If so, how was it estimated? From micrometeorological measurements or as a function of canopy height, i.e. $2/3 h$? Can you provide the references describing how these measured variables were estimated at 1 m above d?

Reply: See previous answer.

Reviewer 1: Page 172 Lines 8-10: What kind of regression was used, linear, exponential, power, etc.?

Reply: The regression was a linear regression.

Reviewer 1: Page 174 Lines 19-20: Why choose a linear regression to represent the concentration gradient from source to some point downwind? Figure 4 and previous measurements show that ammonia concentrations decrease logarithmically downwind from sources. A logarithmic regression would be a much more appropriate choice and may rectify the overestimation of the local concentration gradients and improve the comparison of "measured" and modelled advection error estimates.

Reply: This is a sound remark and we have evaluated this regression algorithm. The limitation is however to objectively chose an origin. See development above.

Reviewer 2 Reviewer 2: Review of the manuscript, "Advection of NH₃ over a pasture field, and its effect on gradient flux measurements", by B. Loubet, et al. (MS-NR: bgd-2008-0176). This paper describes a study which considers how local advection can lead to errors in estimates of surface deposition near a large emission source. This topic is important and interesting, because of environmental concerns regarding high N-fertilisation rates to the landscape downwind of large ammonia sources. The paper is reasonably well-written, and I have no important disagreements with the "big-picture" conclusions reached by the authors. For example, messages of a "bias towards emission" or "bias toward deposition" are very welcome. For this reason this work is worth

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publication. My criticism of this paper is its complexity. The value of this work is best seen as conceptual, i.e., why micrometeorological measurements of deposition near-downwind of strong sources can be in error. As a conceptual work it suffers from being applied to the imperfect experimental framework of GRAMINAE.

Reviewer 2: GENERAL COMMENTS Reviewer 2: Real World Complexity. Reviewer 2: The GRAMINAE experimental situation examined in this paper is probably a poor choice for illustrating the basic problem of micrometeorological deposition measurements. It's simply too complex a setting: 1) experimental geometry is not suitable for reduction to a two-dimension problem; 2) terrain is very inhomogeneous which will lead to errors in a homogeneous dispersion model treatment; 3) there may well be more NH₃ sources/sinks than assumed here; and 4) there is a need to consider non ideal measurements. Reviewer 2: All of these factors are a distraction for the reader trying to follow the basic concepts. In my opinion it would have been better to look at a simple 2-D simulation of an idealized farm. I think with realistic choices of emission rates, compensation points, etc., one could look at the problem more confidence and less confusion. And I think some realistic results would follow (I think the FIDES-2D model would do very well). A useful calculation for this simple situation would be a graph of the error of a deposition inference with distance from the idealized farm. I don't expect the authors to forgo the GRAMINAE experimental data in this paper at this stage. I simply want to document my confusion, and the source of that confusion in reading the paper.

Reply: We agree that applying a simple model here was a challenge. However, at least two of the mentioned points were controlled: we controlled that there was no other intensive NH₃ source in the surroundings, and the footprint of Site 1 was quite homogeneous. Site 2 indeed is close to a hedge of trees. Anyway, we think it was interesting here to compare a simple model with inferred advection errors based on horizontal concentration measurements. We should have stressed that such measurements on 15 minutes basis are rare and it is a unique occasion here to do such comparison. We

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however follow the referee suggestion and add a more general result and discussion on advection errors: A table has been added to give typical advection errors for several distances, heights, wind speed and stability conditions, and for constant surface and constant concentration sources. Moreover, the paper has been reorganised to improve its clarity: the sections 2.2 and 2.3 have been merged into sections 3.4 and 3.5 of the material and methods section.

Regarding the 2D treatment of the source this is indeed a very sound remark. In the paper Hensen et al. (2009) we have shown that at the upwind edge of the field, the difference between the 2D and the 3D treatment of the source did not change much the estimation of its emissions strength, provided the wind direction was in the range -15 to 15 degrees apart from the direction of the source (this is true because there was little strongly unstable conditions during the experiment). Moreover, since the local advection from the farm becomes an issue only when the plume is in a narrow wind sector, we are confident that the 2D treatment is actually appropriate (for advection from the farm). Moreover, an estimation of the lateral flux divergence has been done and is presented in the following answers.

Reviewer 2: 2-D Treatment of Experiment. The main scientific criticism of this work is the reliance on two-dimensional model simulations. I do not believe that the geometry of the GRAMINAE problem allows a good 2-D treatment. The farm is only about 300 m in cross-wind extent, so sensors 500 m or more downwind of the farm clearly "see" the edge of farm (i.e., sensors are influenced by the lateral dispersion of "fresh" air north and south of the farm). The 3-D geometry creates important consequences for the authors, beginning with the addition of another term to the true conservation equation (Eqn. 1) - the lateral flux divergence, dF_y/dy .

Reply: Using the Huang-3D model Huang (1979), which has the same dispersion module as FIDES but in 3D, we have evaluated the lateral flux divergence and compared it to the horizontal flux divergence at 1 m height for the wind sector [-15 deg to 15 deg]. On average the ratio of $dF_y/dy = (K_y d^2C/dy^2)$ to $(dF_x/dx = U dC/x)$ is lower than 3%

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for Site 1 and the farm source. For the field source (the source is much closer) we find that dF_y/dy is larger (no wind direction condition was applied). However, $|dF_y/dy| < 0.05 * |dF_x/dx|$ during 28% of the time and $|dF_y/dy| < 0.2 * |dF_x/dx|$ for 65% of the time. In this latter case, $|dF_y/dy| / |dF_x/dx|$ is rather log-normally distributed with a median of 0.12. Hence we can deduce that the lateral flux divergence can be neglected far away from the source, but also in the field for a fraction of the time. It may however represent a non negligible fraction of the flux divergence in the field for a number of cases. This problem is acknowledged and discussed in the revised manuscript, but the 2D approach is kept.

Reviewer 2: More importantly, what is the consequence of interpreting the 3-D pattern of downwind concentration through the results of a 2-D dispersion model? In the real 3-D world, crosswind dispersion acts to reduce the concentration downwind of a finite source (i.e., dispersion mixes fresh air laterally). How will a 2-D model, which cannot account for this extra "dilution" account for the more rapid decline in the downwind concentration of real data? One possibility is to attribute the reduction to another factor, such as surface deposition. The possibility of these simulation errors is worrisome when the model is used to estimate deposition rates, or infer the level of errors in the source inferences were. This is a substantial weakness of the paper. While I don't expect the authors to develop a truly 3-D model, I think that there is the need to acknowledge the problem.

Reply: This is a good question. It is true that the lateral dispersion changes the shape of the along wind flux divergence dF_x/dx . Using the same 3D model of Huang (1979), we evaluated the ratio between dF_x/dx evaluated with a 2D model and dF_x/dx evaluated with a 3D model at Site 1 and 1 m height. This ratio ranged from 0.7-1 for a wind direction smaller than 5° to 0.1 for a wind direction of 15° . Hence we can see that for the farm source the advection error evaluated with the 2D model at Site 1 may be greatly underestimated as soon as the wind direction is larger than 5° (away from the farm). This would obviously be a serious drawback of the 2D approach for far sources

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located far away (here 600 m) which does not apply for advection errors due to field emissions / deposition. Since a significant part of the advection is linked with field induced exchange, it may explain why we find a good agreement between the inferred advection errors and the 2D-modelled advection errors. We discuss this issue in the revised manuscript.

Reviewer 2: SPECIFIC COMMENTS Reviewer 2: The experimental map is complex with many labels displayed. Some are mentioned correctly, some are mentioned incorrectly (e.g., there is no labelled Field I), and many labels are not explained or used. I would like to see only the pertinent information displayed.

Reply: The unnecessary explanations have been withdrawn from the text. However, the full map has been kept in order to keep consistency with other manuscript in the same journal issue.

Reviewer 2: Eqn. (3) is a potentially useful quantity when thinking about advection errors. Later in the paper the authors examine the reasonableness of the assumption of a height invariant $u \cdot dC/dx$? Perhaps it might be good to move these ideas forward to accompany Eqn. (3)?

Reply: See answer to Referee 1.

Reviewer 2: Figure 4. Hard to distinguish pre-cut and post-fertilisation lines. At what x distance is the farm located in this figure?

Reply: The farm is located from $x = 0$ to $x = 180$ m. Some text and a rectangle have been added to help understanding this Figure.

References: Huang (1979): Huang, C. H.: A theory of dispersion in turbulent shear flow, Atmos. Environ. 13, 453-463, 1979.

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