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Biogeosciences Discussions

Interactive comment on "Estimation of NH_3 emissions from a naturally ventilated livestock farm using local-scale atmospheric dispersion modelling" *by* A. Hensen et al.

A. Hensen et al.

Received and published: 1 September 2009

Anonymous Referee #1

Received and published: 21 January 2009

Comments on "Estimation of NH3 emissions from a naturally ventilated livestock farm using local-scale atmospheric dispersion modelling", by Hensen et al. This paper examines the use of atmospheric dispersion models and downwind concentration measurements to infer a farm emission rate. This technique has great promise due to its relative simplicity (compared with alternative methods), and the study is a welcome example of this potential. The material is appropriate for the journal, and the manuscript is generally well-written. I have one major scientific concern, and that is the use of the

2-D dispersion model for this problem. I would like the authors to address this concern: either abandon this material or justify.

Reply:

We were conscious about this issue, although we thought the comparison of a 2D and a 3D model was of interest. The referee has however convinced us to use a 3D model. Hence this issue has been addressed in the following way:

- The 3D model of Huang (1979), which uses the same power-law function of the wind-speed and diffusivity profile as the 2D model has been introduced and used instead of the FIDES-2D model to estimate the time course of the farm emissions, assuming a homogeneous surface source. The Gaussian model was used to estimate the source strength of each individual source.
- The FIDES-2D model has been retained in order to evaluate the relative impact of deposition or emission from the nearby fields, since this is the only model able to do this.
- The two models have been compared.

SPECIFIC COMMENTS

1) 2-D modeling

My main scientific concern is the use of a 2-D dispersion model (FIDES-2-D). There are two good reasons to prefer the 2-D model over the alternative Gaussian model: it will more accurately represent vertical dispersion, and the effect of deposition can be considered. However, the geometry of the farm problem limits a 2-D representation. The limited across-wind dimension of the source (~ 300 m) cannot be represented as having infinite crosswind length over the conditions used by the authors.

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The 2-D model is applied to a sensor(s) 230 m from the source, for wind directions from 240 to 300 deg: +/- 30 degrees from directly downwind. I used a 3-D dispersion model (Lagrangian stochastic model) to test the applicability of a 2-D model for the wind directions used (z0 = 0.2 m, surface area source of 300 m x 180m). Below is the 2-D/3-D ratio of predicted concentrations for a sensor at the experimental location for neutral and unstable atmospheric stratification:

——————————- C_2d / C_3d ————————

Wind Dir —– Neutral (L=inf) —– Unstable (L=-10m)

270 ————————- 1.00 ———————— 0.88

280 ————————- 1.00 ———————— 0.84

290 ————————- 0.86 ———————— 0.74

300 ————————- 0.29 ———————— 0.55

By symmetry these results can be transposed to wind directions from 240-270 deg. A ratio of C_2d/C_3d $<$ 1 indicates the crosswind extent of the source impacts downwind concentration (i.e. the sensor "sees" the source edge). These results indicate that the 2-D model was applied to a wider range of wind directions than can be justified for neutral atmospheric stability (+/- 10 degrees would be OK), and that the 2-D approach probably can not be justified for unstable stratification. The authors recognize the potential for this problem in their discussion, as it is mentioned as a possible reason for the difference in emission rates calculated using the Gaussian (3-D) and 2-D models. However, I believe the problem is serious enough to warrant removal of the 2-D simulations to calculate the emission rate (assuming the Gaussian model can be used for this task). There may still be a role for the 2-D model in this paper, but it should be confined to a more general discussion of the impact of surface deposition to a dispersion methodology.

Reply:

We thank to the referee for illustrating this problem. Based on the use of the 3D and 2D models of Huang (1979) (the 2D model being the one used in FIDES-2D), we show the following Figure illustrating that indeed the wind sector is limited to ± 15 degrees. However the number of 30 minutes records when the surface layer was unstably stratified and the wind was in the right sector is very limited (13 case in total) and this last question is not an issue here (as shown by the fact that the points in Figure 1 are all aligned to a single curve).

As a result, although relevant in principle, the concerns turn out in practice to be less important than might have been expected by the referee. As a result, the use of a 3D model gives only slightly different results, while the averages are virtually the same (Figures 5, 6 and 7).

In the case of winds coming from wider angles, Figure 1 shows that the problem is more relevant (underestimation of the source by 60% at ± 30 deg). We have therefore introduced the 3D model in the paper and applied this model to estimate the source strength. We have however kept the 2D model for estimating the effect of deposition /emission from nearby sources.

Interestingly, the use of the 3D model did not however change the global picture and only changed slightly the averaged emission strength. This is because the frequency of wind directions in the sector -15 / +15 deg was high in the overall wind sector selected (-30 / -30 deg)

2) Emphasis on National Inventory Emission Rates

I believe there is too much emphasis on "national inventory" emission rates. There is no reason to believe that these inventory rates apply to this particular farm, for this narrow study period. A rough level of agreement between the rates calculated in this study and those of the national inventory indicate plausibility in the calculations – nothing more. I think the discussion on Pg. 839 which attempts to explain away the difference is not appropriate or needed. I believe the calculated emission rates are reasonable

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(although the stated uncertainties are probably too low).

Reply

The reviewer is right that, an attempt to explain the difference between the results of this method and the inventory should not be overrated. However, in our view, the national inventory emission estimate is a fair starting point for a discussion on a new methodology that aims to quantify a whole farm emission level. The inventory number is the emission level that will be used for the farm. As a result, the discussion on the difference is likely to provide leads for new experiments. On these grounds, we consider it (with suitable caveats) to be rather important to retain the comparison in this paper.

3) Gaussian Plume Dispersion Model

I found the use of a Gaussian dispersion model to be a surprising choice. While relatively simple and easy to use, these models have well-known deficiencies when applied to short-range surface problems. I will not detail these here. I recognize some of the authors have experience with more realistic models. I am curious as to why a more physically realistic model type was not used? I do not think use of a Gaussian model invalidates the study – but it weakens it somewhat. Perhaps the argument is that because the terrain at the site is complex (i.e. inhomogeneous), it is not worth applying more physically accurate models that rely on the assumption of homogeneity. I am sympathetic to this thinking. I would like to see some elaboration of the authors views on this point.

Reply:

This is a good point indeed! The Gaussian model is an easy to understand, simple formula indeed that by no means shows the complex mixing that occurs when the air flows over and in between the animal houses on it's way to the field. There are much nicer CFD models that can compute the flow around buildings. Obtaining the right

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input data for those models and validation of the wind field with extra anemometry was however not possible within this NH_3 project. The outputs of those models are as good as the inputs available which will again lead to uncertainties. (e.g., how convective is the airflow over a 30 degree roof area and is the roof 30 degrees everywhere?)

Furthermore, when taking sufficient distance however between this complex build-up area and the receptors, allowing the air flow to ”calm down” and mix further over a relatively smooth surface, the plumes that are actually observed (for example with the fast response sensor measurements) are really nicely Gaussian shaped again. Any chain is always as weak as the weakest part. Making one piece of the chain extra strong does not necessarily improve over all reliability. So: unless more input data is available (vertical dispersion measurements, more receptor in the plume, detailed activity data on the animals in the housing etc) we probably with the simple Gaussian model remains an effective option.

Comment

I do think use of a Gaussian model adds uncertainty to the emission estimates. When compared with other models there is: i) uncertainty in the link between measured wind properties and the choice of Gaussian model parameters (e.g., the link between measured heat flux and the specification of a stability class, the choice of the height of the wind specification); and ii) uncertainty in the choice of the appropriate sigma curves to use in the Gaussian model. Counter intuitively, the choice of a Gaussian model makes the application more complicated by removing the direct connection between measured wind properties and model parameters that exists for other model types.

Reply:

We thank the reviewer for this sound remark. Indeed our objective was to keep the approach “simple” but most of all “operational” so that it can be used with a simple dataset (typically a met data with a given roughness and some information on the surface layer thermal stratification. However, a sensitivity study was already given

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in the paper (section 5.1) using the Gaussian plume model to stability class. The source estimate varies by 40% when changing the stability classes from D to C or to E (increasing emission with increasing unstability). So this is indeed an important factor.

The results from this uncertainty analysis has been added into Table 6 for clarity.

The effect of the choice of the height of wind speed can be now seen in the comparison between the Huang 3D model and the Gaussian model, since the Huang model takes account of wind and diffusivity profiles. This point is now discussed.

Comment

4. Unneeded Material

The procedure for calculating emissions can be made clearer. It will be hard for readers to follow the variety of calculations being made. Some confusion can be eliminated by not including or describing material not used in this study. For example, why does the reader have to follow a description of location and instrumentation of Site 1 (or Site 2, or Site 4 ...)? As far as I can tell these measurements/locations are not used. This frustrates and confuses the reader.

Reply:

Although all the locations shown in Figure 1 are not used, most of the information is necessary: the Site 1, 3, 5 and 6 are directly used in this study. Only Met data are not strictly speaking used here, however, they allow the reader to refer to the other paper of the special issue using the same referencing of sites which we think is important. We have however withdrawn from the text the description of unnecessary information (concentration measurements at sites 1 and sites 2)

The section on emission calculation procedure has been reorganized (also to include the Huang 3D model description) and clarified.

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Pg. 829, Ln 13. "As a comparison, measurement-based estimates ..."; Not clear what is being compared.

Reply:

The sentence has been clarified.

Pg. 831, Ln 5. "Using a single dispersion model, many inference methods could be used, and on the contrary, with one inference method, many dispersion models could be used"; This statement needs clarification.

Reply:

We meant that several inference methods and several dispersion models could be used. We now have deleted this sentence to improve the readability of the manuscript.

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Interactive Comment

Anonymous Referee #3

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The paper addresses a high relevant and timely scientific question and fits perfectly the scope of Biogeosciences, and presents novel concepts, ideas, tools and data. The reached conclusions and substantial, although a slightly larger experimental setup would have made it possible to performed an even better analysis. The applied methods and assumptions are valid and clearly outlined. Measurements of background levels in different directions from the farm would, however, have improve the study. The descriptions of experiments and calculations are adequate, but there are some central references that should be added to the introduction and these are indicated in this review. The title and abstract of the paper is appropriate, and the paper is well structured in a fluent and precise language.

In the introduction the authors discuss the difficulty of obtaining reliable emission data for naturally ventilated animal houses, which is rightly considered complex, expensive and labour intensive. Here it would be suitable also to mention process based ammonia emission models for farm houses as a potential tool, although these have their significant uncertainties. An example is the FASSET model for which descriptions together with journal references can be obtained at www.fasset.dk. The introduction should refer the recent review of local scale modelling of atmospheric nitrogen deposition (Hertel et al., 2006). For modelling ammonia from single farms, it would be local with reference also to a very recent paper where another Gaussian dispersion model OML-DEP has been evaluated against measurements from single farms (Sommer et al., 2009).

Reply:

We thank the referee for mentioning these papers. The introduction has been modified to account for these new references.

Comment

At the end of the introduction the authors explain the way measurements from the background station are used to subtract inferred emissions from upwind farms. However, they do not state explicitly whether only measurements are selected for the wind sector where the wind is blowing from the background station towards the farm. Assumingly this is the case, but it should be explicitly stated. Here it would have been highly beneficially with more than one background station, but the cost of additional stations is likely the explanation why this has not been used!? The selection of measurements from the appropriate sector is crucial for the results as the surrounding fields and farms must be a significant uncertainty in the analysis.

Reply:

We agree that the location of the background measurement is crucial in determining a source by inverse modelling. However, in this study, we were lucky that the farm was the only large NH_3 source in the surrounding, as this area was at the edge of the city and there were no other agricultural activities in the neighborhood apart from farm fields. Liquid cattle manure (slurry) was spread on Field II (see Figure 4) at $\sim$1700 GMT 22 May. This source of NH_3 led to NH_3 plumes over Kleinkamp during spreading and at $\sim$1800 24 May. Emissions from this source ceased on 25 May, following irrigation (with dirty water).

The main grassland site was fertilized with mineral fertilizer on the 05 June. We however could see no effect of these activities on the background concentrations

Moreover the background concentration was measured at the top of a 42 m tower at about 1600 m away from the farm. Although the background location was downwind from the farm, it was sufficiently far away and high enough to avoid any significant influence of the farm on the concentration.

Comment

It would be useful to present the difference between farm signal and background signal

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to have an indication of the potential error.

Reply:

This is already in Figure 3.

Comment

In section 2.2 it appear that 30 minutes averages are used. Usually dispersion models are evaluated for hourly mean values due to time scale of atmospheric processes.

Reply:

This was actually an error we did not spot from the text: 15 min averages are actually used here. This has been changed in the text. The reviewer is right that common average time for for dispersion models is generally 1 hour or more. The main effect of averaging time is on the horizontal dispersion of the plume.

In general with longer averaging times the plumes are smoothed mainly due to the larger Wd variability affecting the average plume. In the formula (3) the Time averaging correction for the horizontal dispersion coefficient is used to compensate for this effect. From plume measurements with mobile TDL systems we learned that this correction, although originally constructed for corrections between 30 minute, one hour and say six hours averaging, also works fine for smaller average times like 1-3-5 minutes.

A smaller effect can of the averaging time is expected on the vertical dispersion relation.

As for vertical dispersion there is no averaging time dependency included in the Gauss model. The small scale of the setup here, with source-receptor travel times in the order of a minute, turbulent structures with a time scale of 15 minutes or more will have only a small effect on the vertical dispersion. However when using formula (3) the contribution of turbulent structures up to say 1 hour time average is taken into account. So here the reviewer has a point: maybe we are overestimating vertical dispersion and thus overestimating the emission level. We do think the uncertainty linked with this effect is relatively small compare d to the other uncertainties in this simple model. It is clear that, no matter what model is used, measurements that document the vertical dispersion are a valuable (but logistically unattractive), addition to the measurement setup. The revised text now clarifies this points more clearly.

Comment

In the same section two references (Sutton et al and Milford et al.) are placed in foot notes at the bottom of the page, and there is no clear indication why these references are to be treated differently from other references in the article.

Reply:

These references have been changed.

Comment

The authors could have compared the obtained diurnal cycle in emissions from the farm house with the parameterisations in the work of (Gyldenkærne et al., 2005; Skjøth et al., 2004).

Reply:

Both Gyldenkærne et al., 2005 and Skjøth et al., 2004 refer to a model beveloped by Elzing & Monteney which Tˆ0.89 as a temperature variation effect. They obtained this value from experiments where urea was sprinkled on a fouled floor at temperatures of 5 to 20 degrees. The height of the emission peak was then fitted to temperature. In our dataset the temperature range is smaller, ranging from 14-22 degrees but shows a stronger T effect compared to the Tˆ0.89. The emission increases with a factor of about 3 over the 5-20 degree interval which is similar to what Zhang et al found.

This need not be a surprise since the T effect for this study is using a whole farm estimate with combined set of dairy and pig stables. Conclusion is that the Elzing and Monteney relationship was not enough to describe T dependency in this case. We

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added comment on this comparison in the revised paper and stated that the fit can only be considered valid for this temperature range.

Interactive comment on Biogeosciences Discuss., 6, 825, 2009.

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