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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.

Anonymous Referee #1

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Comments on "Estimation of NH3 emissions from a naturally ventilated livestock farm using local-scale atmospheric dispersion modeling", 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.

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. 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

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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.

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 (although the stated uncertainties are probably too low).

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

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the assumption of homogeneity. I am sympathetic to this thinking. I would like to see some elaboration of the authors views on this point.

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.

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.

MINOR COMMENTS

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

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.

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