

Comments to Moring et al. (bgd-12-10059-2015)

The present manuscript describes a model for the (bi-directional) ammonia exchange of a pasture urine patch with the air. The model is process-based but relatively simple with respect to vertical resolution in the soil (one empirically adjusted soil layer). The model has been tested/adjusted using an experimental dataset by Laubach et al. (2012). While the development of a urine patch emission model is very timely and scientifically useful, the present manuscript suffers from a number of shortcomings that have to be eliminated, improved or adequately addressed before the paper is acceptable for publication. The major and minor issues are listed in the following (with truncated page numbers).

MAJOR COMMENTS

1) a) The authors state (P64,L20f) that the present model "can be applied on both field and regional scales. However, it is not clear, how the urine patch model is supposed to be applied on larger scales (e.g. with full temporal and spatial resolution or in a statistically integrated/parameterized form). Thus a (short) outline should be added on how the application to larger scales and/or the incorporation into larger scale models can or should be done.

b) The name "Ammonia generation from grazing (GAG)" is a bit misleading for the present specific model describing ammonia emission (or exchange) of urine patches only. It needs to be considered whether fresh urine patches (less than 10 days old) are really the only ammonia source on grazed fields. What about the dung patches and the grassland areas which are not very recently affected by excreta?

2) A sort of big-leaf (multi-resistance) model is used to describe the bi-directional exchange of the urine patch including the grass vegetation. I seriously doubt the usefulness of including the vegetation exchange within the limited urine patch area due to the following reasons:

- In my opinion, no representative stomatal or cuticular compensation point can be derived for an area recently covered by urine.

- For the (short range) re-deposition of urine derived ammonia, the area directly downwind of the urine patch is more important than the patch area itself due to very effective horizontal displacement of the air column by the mean wind (even for heights of a few centimeters above ground) and the much lower compensation point.

- The way in which the aerodynamic resistances (R_a and also R_{ac}) are used here appears erroneous. These resistances are defined for a vertical turbulent transport over a spatially homogeneous (and virtually infinite) surface. This is not appropriate for isolated small patches. There the exchange is clearly dominated by horizontal advection (from/to the surrounding areas not affected by urine) and not by turbulent transport.

Due to these reasons I suggest to either omit the ammonia exchange with vegetation on the patch area or to improve it by considering the short-range deposition downwind of the patch.

3) In my view the present comparison with the measurements of Laubach et al. (2012) represents rather a model calibration and not a "model validation"! Important model parameters like e.g. the thickness of the source layer Δz or the soil buffering capacity β seem to be adjusted to the measured 6-day dataset. This is problematic because it remains unclear how these parameters behave for different soil types and management conditions.

4) A weak point of the model seems to be the lacking vertical resolution within the soil. The infiltration and mixing of liquids (urine and rain water) actually depends on the profile of soil water content (not just in the uppermost layer). I am somewhat confused about the use of different soil layers: the "urine affected soil layer" (P74 L6), the "source layer" (P74 L19) and the "soil evaporation layer" (P77 L4). It is not clear to me, how these layers of different depth

are actually related in terms of soil moisture in the model, because the soil moisture quantities used in the equations are not clearly related to a specific layer. Thus the entire water (and TAN) budget should be presented in a more consistent way with specific depth layer attribution.

MINOR COMMENTS

P61 L17-18: This is a strange formulation. In my understanding it should read "showed that the measured parameters are well captured by the model"

P64 L2 (and entire text): The term 'inverse volatilisation model' is not really appropriate here in my view. A multi-resistance model is an equilibrium model that does not have a clear physical calculation direction (and thus can be equally used in various ways). Not the model itself is inverse but only the specific use of it. Thus one could say instead that "the model was used in an inverse mode to calculate the soil resistance".

P64 L7: Isn't that rather the forward than the reversed mode? (see previous comment).

P66-70: Many parts of this section are just (very detailed) descriptions of previously existing model parts adopted from the literature. If this part is not fully omitted (cf. comment 2) it should be shortened considerably.

P80: It would be much more informative for the reader to move Table S2 to the main text, and instead move the Eqs. 39-53 to the suppl. material. The latter equations are not understandable without the corresponding reactions.

P80 L11: Does "no CO₂ emission in the basic GHG model" mean that you did not consider the CO₂ and carbonate related equations in the pH modelling? As far as I understand, the normal soil respiration (without effect from urine) generally produces a high CO₂ concentration in the soil pores that affects the carbonate concentration and pH of the soil water. Did the authors you consider this? And, if yes, how was the soil CO₂ concentration determined?

P80 L21: what does "lg" mean here? Is it the natural logarithm (often denoted by "ln") or the decadal logarithm?

P81 L2ff. Why did the authors decide to choose only data from such experiments ("where the NH₃ emission flux was measured from several urine patches deposited relatively close in time")? Why did they not consider e.g. enclosure measurements of individual/simulated urine patches or field measurements of real grazing fields?

P82 L12: It would be better to present this relationship as a labelled equation that can be referred to (instead of repeating it on P86).

P83 L6: In which depth was the water content measured in the experiment? Can it be assumed representative for the Δz layer?

P90 L9: Correct to "has a considerable effect"

P90 L19f: I do not understand this sentence. Needs rephrasing.

P107 Fig.4d: Why is there no increase in soil water content at the very beginning of the experiment due to the urine addition?