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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Review and parameterisation of bi-directional ammonia exchange between vegetation and the atmosphere" *by* R.-S. Massad et al.

Anonymous Referee #3

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Summary:

This manuscript presents a general parameterization of the two-layer bi-directional NH_3 air-surface exchange model. This is a well written paper that will make a significant contribution to the literature and, I expect, will find wide use in the ecological and air quality communities. Given the subject matter, potential impact, and generally high quality of the results, ACP is an appropriate outlet for this work. The authors should be commended for taking on the task of synthesizing the available NH3 flux data for the purpose of constructing generally applicable parameterizations for soil and vegetation emission potentials, as well as the cuticular resistance to NH3 deposition. The paper

represents a major first step toward this goal. That being said, the weakness of the paper, which the authors acknowledge, is that for some elements there are not enough data yet available to develop robust parameterizations, leading to large uncertainties. In other cases multiple techniques have been used to collect data, which exhibit some systematic differences, further complicating data interpretation and parameterization. Overall the authors have done a responsible job of presenting the weaknesses and uncertainties of the data and in comparing the resulting parameterizations to field data. However, there are some areas, as described below, where more description and detail are needed. Though some additional work is required, I believe the authors can address these comments. Subject to thorough treatment of these comments, I would recommend publication.

General comments:

Construction of generally applicable relationships between Γs and system total N input is a significant advancement for NH3 modeling. The authors have done a good job assembling and interpreting data from the numerous studies represented in Figure 5. In general I think the resulting parameterizations represent a useful first step and I anticipate they will be widely used. That being said, I do have a few concerns about the methods by which the original data were adjusted for comparison and, in general, the comparability of some of the data. First, the metric on which the Γs parameterization is based is total annual N input. For field studies describing semi-natural systems this value is either given as atmospheric deposition measured at the site or can be derived from air quality models. For arable systems, however, I expect that in some cases the published results only include application rates for the specific growing season under investigation, rather than annual fertilizer input for the site. Have the authors accounted for this in summarizing the field measurements in figure 5? Also, it looks as though not all of the data from Tables 2 and 3 are included in Figure 5. I may have overlooked something in the text, but the authors should explain the criteria for including data in Figure 5 and discuss how this may affect the resulting parameterizations for Γs.

It is unclear to me whether the parameterizations resulting from Figure 5 (equations 7 and 8) exclude data from laboratory experiments. Firstly, in either case, I am not comfortable with the term Ndep being used to describe fertilizer application rates in laboratory experiments. This term should only be applied to studies carried out in the open atmosphere. Secondly, the laboratory experiments (green symbols) in Figure 5 a and c show very little or no correlation between Γs and total N input. Why is this? I agree with the authors it is unlikely that this results primarily from uncertainty in estimating N input. However, a more detailed discussion of other potential reasons is warranted.

Understandably, the text is weighted toward the development and discussion of the parameterizations for Γs. In my opinion, the treatment of Γg would benefit from more detail. A more complete description and summary of available data, similar to what is done for Γs, would add context to the description in section 10363. The authors should also include at least one graphic illustrating the agreement b/t the proposed parameterizations for Γg and field data. Furthermore, as I discuss below, the rationale for setting Rg = ∞, and therefore ignoring Γg, for unmanaged systems and managed systems with overlying canopy requires a more indepth justification.

Specific comments:

10336, line 6: remove “here”

10347, line 2: “has sometimes provided smaller values than the gas exchange measurements”. This statement stops well short of describing the seemingly systematic difference between the approaches for measuring or estimating gamma. A stronger statement or further description is needed.

10350, line 2: The authors tend to downplay the disagreement between the 3 techniques for semi-natural systems. The authors should more directly acknowledge the disagreement and provide a brief discussion of the possible reasons.

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10350, line 26: “By contrast, cutting seems to. . . .” . Only point (iii) is relevant to Γs.

10350, line 17: “Field fertilizer application results in. . . ..”. As a statement regarding NH3 emissions this sentence seems out of place. Are the authors referring to the peak in Γs?

10362, 1ine 3: Would canopy height be provided as a model calculation?

10362: The rationale for settin Rg = ∞, and therefore ignoring Γg for unmanaged systems and managed systems with overlying canopy requires a more indepth justification. In managed systems, while the overlying canopy may recapture most of the emissions, the emissions themselves may be large, and therefore significant with respect to the net-canopy scale emissions. In unmanaged systems, particularly forests, the emissions will indeed be much smaller but again may be important in terms of in-canopy NH3 cycling, and therefore play a role in the net canopy exchange. I do realize there is a lack of data from which to soundly parameterize these components of the model but my feeling is that Γg for unmanaged systems and managed systems with overlying canopy should not be ignored.

10363, line 3: the phrase “shortening of data” may be unclear to some readers.

10364, line 20: I do not agree with the statement that most of the N fertilizer is lost to leaching after the first rainfall, particularly NH4+. In fact, in some cases a second large emission pulse is observed following the first rain event after fertilization, as rainfall directly stimulates chemical transformation of the fertilizer and mobilization into the soil thereby stimulating microbial processing. The authors should acknowledge that equation 20 will not capture such dynamics. As mentioned above in my general comment, the section on the temporal dynamics of Γg after fertilization requires more detail and would benefit from the presentation of graphics demonstrating the agreement between the proposed parameterizations for Γg and field data.

Figures and Tables:

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NH3 units in Table 1 should be ug/m3

Caption for Table 2 should be consistent with the text regarding Rw and Rw(corr).

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 10335, 2010.

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