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Interactive comment on "Measurement and modelling of the dynamics of NH_3 surface-atmosphere exchange over the Amazonian rainforest" *by* Robbie Ramsay et al.

Anonymous Referee #2

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Ramsay et al. analyze a month of dry-season observed NH3 fluxes in the Amazon with a popular NH3 surface exchange model. The observational dataset is very unique. They infer the ratio of apoplastic NH4+ to H+ concentrations from their observations, and examine how different parameterizations of leaf wetness influence the overall agreement between measured and modeled NH3 fluxes. The authors have a novel set of leaf wetness measurements that shows the importance of getting this variable correct in modeling NH3 surface exchange. Overall, the paper is very well written, and I think it will be an important contribution to the peer-reviewed literature provided the authors make some adjustments.

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Discussion paper

Major concerns Do the authors have an indication of how well the Wesely stomatal resistance model simulates stomatal resistance at ATTO? Is it consistent with an inversion of water vapor fluxes? I doubt Wesely captures much variability at all, which raises the question of how much can be inferred about cuticular resistance when the authors are likely not capturing much variability if at all with Wesely stomatal resistance? In general, I'd like to see more discussion of uncertainties in the flux, as well as in Ra, Rb, & Rs might affect the authors' results. I also didn't find the discussion of stomatal conductance increasing with leaf temperature to be convincing.

I would also like to see a more in-depth analysis than Figure 6 in arguing that there really is one 'best' model (i.e., I would like to see a figure that is more convincing that variability is better captured when leaf wetness observations are used).

Line comments Line 32 – how can bidirectional exchange have been considered a perfect sink? the subject of the modifier is incorrect Line 46 – is Ramsay the companion paper? please specify Line 67 – I'm not sure what 'this model' refers to – the authors have discussed several different types of model. Line 69-70 – please rephrase this sentence – the authors only present models that accurately simulate the observed fluxes? Line 79 – what is an aerodynamic canopy height? Line 85 – rephrase to something like 'NH_3 fluxes can be considered representative of a homogenous rainforest' Line 86 – so the footprint of 5.2 km is for high wind? Line 119 – wind speed and direction were measured by eddy covariance? Line 130 – I think the authors are using two different terms to describe vertical concentration differences Line 247 – why don't the authors use the same form of Ra as used for the AGM? Line 150 – 'this canopy resistance approach'? what canopy resistance approach? do the authors mean their way of inferring Rc through residual of 1/vd and Ra + Rb? Line 163 – notional mean? can the authors provide a clearer definition of χ_c Line 182 – conceptual mean? Line 190-3 – this is a very long sentence – will the authors split it up? Figure 2 – add shading for different periods Figure 3 – can this plot be four panels? it's hard to read as is. this plot is also not colorblind friendly Line 242 – I think the statistical summary is on Figure

4 Line 274 – ‘furthermore’ to what? can the authors spell out what they’re implying here? Line 295 – is the parameterization novel? or is it novel that the authors actually have observations of leaf wetness? Line 335 – aren’t the authors deriving fluxes from concentration measurements? perhaps clarify how the technique here is different from Trebs and Adon Line 344-7 – I guess I would have liked to see this information upfront (I was wondering throughout) Line 364-5 – Is this really true? Leaf temperatures usually accompany increases in vapor pressure deficit – but in Urban et al. 2017 VPD is controlled for. Line 363-370 – generally it was hard for me to follow what this paragraph is referring to – can the authors remind the reader what aspect of their results they are discussing? Line 373 – I think this statement is too strong Line 372 -9 – there is a lot of information here (from the sentence starting with ‘Nevertheless’ to the end of the paragraph), and I’m not sure much of it is needed except the last sentence. Line 388 – please remind the reader that emission potential = apoplastic ratio (or is it an inverse relation?) Line 392 – reference for values of ratio being as low as 5-10? Line 392-5 – this sentence is really long - please make the point clearer Line 395 – are the authors suggesting the soil is nitrogen-poor at ATTO? please clarify Line 395 – “impact” → “decrease” Line 397-9 – suggest removing explicit value judgement here Line 404 – suggest using the model simulation names here so the reader doesn’t have to dig in the text for what a,b,c,d, etc are Line 405 – doesn’t this make sense, given that the ratio is constant whereas Rw gives the estimate more variability (Ok, I see this is discussed later on, but the authors should consider including this info here, as well as reframing a little) Figure 6 – I don’t really find this analysis all that convincing. can the authors really say one model is better than another? maybe if would be helpful if the authors highlighted better agreement when the leaves are wet. Line 415 – to some degree, doesn’t the modeled value also have measurement uncertainty? because the authors are driving it w/ observations Line 423-4 – I’m confused by this sentence – aren’t leaf wetness and RH model inputs? Line 432 – what about just a better RH-> leaf wetness parameterization? I think this sentence needs to be adjusted Line 440 – would be helpful to remind the reader here that the 38.5 is the inferred value, and the

50 is the upper bound of the inferred value Line 448-9 – I think this sentence makes the value sound more uncertain than it is Line 461-2 – it seems like the authors are saying the same thing twice in one sentence Line 467-9 – I'm not sure the authors can comment on whether these conditions diverged from mean climate – maybe just say campaign average – the observations are from 1 month of 1 year Line 471-2 – I would suggest cutting this sentence here – it makes your results seem questionable without more discussion of why the difference is ok (which the authors do well in the discussion) Line 476 – The phrase 'somewhat larger' is ambiguous – I would urge the authors to be more concrete with their wording here Line 479-1 – yes in that the authors could see how their best fit model from the dry season works for the wet season, but Wesely stomatal conductance is not going to capture differences between wet and dry Line 494-5 – can the authors be more concrete here? do they really not think

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