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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Terrestrial Water Loss at Night: Global Relevance from Observations and Climate Models" *by* Ryan S. Padrón et al.

Anonymous Referee #2

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Padrón and others analyze nocturnal evapotranspiration measurements from eddy covariance and estimates from models. The analysis is interesting and certainly novel although a few methodological points need to be reconsidered in my opinion, and the text could be improved in multiple instances. Sentences like 'Lombardozzi et al. (2017) compiled evidence of this from 204 species' aren't particularly instructive. What did they find? In the paragraph at the bottom of page 1 try to make the scientific findings, not the authors, the subject of the sentences. For a discussion of this see https://schimelwritingscience.wordpress.com and the associated book. A more powerful way to synthesize the literature, which would make the present manuscript more citeable, would be to synthesize existing studies in a table to help further motivate the present analysis and be more comprehensive. The points about dew and hoar frost are

great. P 2 line 22: disentangle aerodynamic vs. surface conductances more clearly. The surface has both stomatal and boundary-layer resistances. 2.1.1: Why is the 10 W m-2 threshold used to differentiate between day and night? Sensors have uncertainty but the solar zenith angle can be calculated with extreme accuracy for environmental science applications. Are results sensitive to the 10 W m-2 threshold? I see that a zenith angle-based analysis is done in section 2.1.2 (sun up and sun down). Why are different approaches used? What are the 'cases described by Hirschi et al. (2017)'? P 3 line 30: using a static value for the latent heat of vaporization is fine, but it's easy to add its temperature sensitivity to add a bit more accuracy in the latent heat to water flux conversion. 2.1.2: The Bowen-ratio-based assumption is a bit problematic; there is extensive evidence that undermeasured sensible heat flux from large eddies plays a large role in lack of energy balance closure. That being said, these factors are less important at night where low-level jets and decoupling of the eddy covariance sensors and the canopy often dominate. 2.1.2: instead of emphasizing caution, perhaps don't use gap-filled fluxes for the analysis. This is a hard thing to do at night when eddy covariance data are often less reliable than many people believe. Thinking broadly, is 'nocturnal water flux' better than NWL given that water can be both lost and gained (but is admittedly a net loss over the time scales mostly investigated here). 3.1: why is the second threshold chosen? Is it appropriate for the site or just simply half of the previous threshold? Fig. 2 and elsewhere: what are representative uncertainties of the site-level NWL measurements/estimates? This statement should be in the Methods: "These annual mean values are computed from monthly climatologies obtained by omitting months with half or more of missing latent heat flux data." In general, the assumptions made in the flux processing for NWL for the FLUXNET2015 database needs to be explained in more detail. The statement 'Nonetheless, deciduous broadleaf forests (DBF) have an overall lower NWLf, whereas evergreen needleleaf forests (ENF) include most cases with higher NWLf' suggests to me that perhaps difficulties in measuring the surface-atmosphere flux is partly responsible here. ENF needles are more closely coupled to the atmosphere on account of their smaller dimensions and I can't think of a discernable reason why DBF would have particularly low NWL. Although perhaps relative NWL given that they are frequently found in mesic regions. Figure 4 is tricky to look at. I'm curious to know if there is a more logical way of presenting these complex data. The analysis of models is interesting and the degree of discrepancy is surprising.

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