Response to comments from reviewer RC1

Note that reviewer's comments are in black font, and responses in blue font.

The overall focus of this paper is interesting; nocturnal evapotranspiration is an under-appreciated part of the hydrologic cycle that represents water loss without accompanying carbon gain (something that many resource managers might like to avoid). Thus, the result showing that nocturnal water loss (or NWL) represents a significant fraction of total ET across a wide range of biomes is likely of interest to a wide audience. The comparison of observed and modeled NWL rates is interesting in that, while the total magnitude of NWL is relatively similar between data and models (6.3 versus 7.9%), the relationship between modeled and observed NWL rates is virtually non-existing across sites (e.g. Fig 8a). This suggests some process-level room for improvement in the models.

We appreciate the positive opinion about the relevance of our manuscript.

Overall, I found that the study was largely exploratory; the mechanistic explanations were limited to a simple spearman correlation analysis (e.g. Fig 4) of observations, and little discussion of how mechanistic representation of key processes in the models might affect the inter-model variability. While purely objective-oriented exploration of network level data can be useful, at the same time, better closing the gap between observations and models requires that underlying mechanisms be understood and carefully linked.

Towards that end, I have a few suggestions below for enhancing the mechanistic perspective of the paper that could ultimately leave the reader with a better understanding of not only how much water is lost at night, but also why this happens at different rates across ecosystems and models.

1. Much of the introduction reads like a list of previously published papers on the topic. While it is important to acknowledge this prior work, it would also be quite helpful to review for the reader the various mechanisms that could contribute to high NWL (e.g. not only incomplete stomatal closure, but also non-negligible cuticular conductance, and nocturnal evaporation from soils and canopies, snow sublimation). From there, it may even be possible to craft some expectations about in which ecosystems, and when, NWL should be especially prominent in the observations.

The introduction is modified and extended according to the suggestion.

2. Likewise, some discussion about how the different models treat relevant processes and parameters could allow for a more informed understanding of why they differ so widely in their estimation of NWL. The authors suggest that most of the models employ the Ball-Berry stomatal conductance model (e.g. Page 2 Line 23). . . Is this true for the models studied here, and do they adopt similar formulations for the intercept of this model? Knowing precisely how these models treat nocturnal conductance would go a long way towards understanding if the cross-model differences are linked to model ecophysiological representation.

We completely agree. We expanded the discussion on factors affecting inter-model variability and introduced a new figure (previously Fig. S2 in the Supporting Information). Yes, we note that most of the analyzed climate models' stomatal conductance formulations are based on the Ball-Berry model. Note that the complexity of CMIP5 models, and the fact that not all *models are equally well documented, hinders a simple explanation of inter-model variability. In addition to how individual models represent nocturnal conductance, other factors such as planetary boundary evolution and soil parameterizations might also influence the inter-model variability. Thus, we consider this more detailed analysis to be outside the scope of our study, but nonetheless an interesting topic for a follow-up article.*

3. Related to (2), I found it quite interesting that model differences were related to near surface temperature (page 12, line 6); unfortunately, this result is buried in the SI. I would urge the authors to bring this result into the main text, and also expand the discussion about why this correlation exists.

We appreciate the suggestion. We now include this as Figure 7 in the revised manuscript and expanded the discussion.

4. The mechanistic analysis of the data is limited to correlations between NWL rates and VPD, wind speed, and soil moisture. I agree that these are important drivers of ET. However, even though incident solar radiation is zero at night, energy is still required to drive ET at night. The paper would strongly benefit from a discussion of where this energy comes from, which would require consideration of sensible and ground heat fluxes. . . and thus provide additional mechanistic insight.

We appreciate the suggestion. We now include in Fig. 4 also the relation of NWL with net radiation (i.e. longwave radiation during the night). We additionally expanded the discussion accordingly.

I also had a few concerns about the treatment of the flux data.

1. The analysis relies on datasets that are largely gapfilled. While gapfilled data are necessary for estimating annual sums, they are not required for exploring relationships between ET observations and meteorological drivers. Can the authors repeat the analysis for Figure 4, but using only data that pass the quality control test?

This was already the case for Figure 4. We now clarify this in the text and figure caption.

2. The flux observations have been corrected so that the energy budget is fully closed. This correction is quite controversial in the flux community, especially since the mechanisms causing the lack of energy balance closure are still not fully known (and at least one school of thought suggests that much of the problem could be linked to sensible heat flux). Thus, I urge the authors to repeat the analysis without the energy balance correction, and include a summary of those results (at least in the SI).

We appreciate the insights. We now include this also in the manuscript and provide more information on the uncertainty of the EC fluxes. Figure 2 now includes four different NWL estimates from FLUXNET sites: without energy balance correction, and with the 25, 50 and 75th percentile of the distribution of energy balance corrected fluxes.

A few other comments:

Page 1, Lines 15-20. Much of the first paragraph is not well written. It states that ET is an important process but does not tell us specifically why we should be concerned about NWL *specifically. Moreover, the logic is not clear: the authors tell us that VPD, temperature and wind speed affect ET, and that half of the diurnal cycle is night, therefore NWL can be important. This conclusion does not follow from the premise (missing is a discussion about the prevalence of VPD, temperature and wind speed conditions that could generate substantial nocturnal ET).*

We reformulated the paragraph.

Page 3, Lines 1-5: This paragraph, which discusses the overall objective of the study, is quite short and lacks detail. Here would be an excellent place to discuss some expectations as to how NWL relates to "different meteorological and land cover conditions." The model-data comparison should also be mentioned here, and perhaps expectations offered as to which models are best equipped to accurately describe NWL patterns.

We reformulated and expanded the paragraph. In addition, note that our study follows an exploratory approach rather than specific hypothesis testing, which is why we do not provide any assumptions besides the known influence of abiotic factors like temperature, VPD and wind speed on evaporation/sublimation from the soil or canopy.

Section 2.1.2: Are the Fluxnet2015 data corrected for LE storage terms at night? Is this important?

The relevant data processing is described in the text and the referenced FLUXNET website. To our knowledge the FLUXNET2015 data does not account for LE storage in the air between the ground and measurement level.

Page 7, Line 4: The relationship between VPD and NWL may not be linear if stomatal conductance decreases when VPD is high, even at night.

We now also explicitly mention this in the text.

Page 11, lines 20: The discussion of nocturnal stomatal conductance here is interesting; it strikes me as a bit of a missed opportunity not to explore patterns of nocturnal surface conductance from the data (it is relatively straightforward to invert flux tower ET measurements with the Penman-Monteith equation to obtain half-hourly surface conductance, e.g. see Wever et al. 2002 https://doi.org/10.1016/S0168-1923(02)00041- 2). Doing so would illuminate whether cross-site differences in NWL are driven largely by biotic versus abiotic factors.

We find this suggestion very interesting and an excellent idea for a more specific study on surface conductance. Our main goal here is to provide a first more general overview of NWL across the globe from observations and climate models.

Figure 7: Considering that the models and data don't agree at all on the site level, can we really have much confidence in these future projections?

The inter-model variability of future NWL_f projections is indeed large as shown in Fig. 7d and acknowledged on page 9 lines 10–12. Future studies could aim at reducing inter-model spread and constraining future projections.