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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Understanding model biases in the diurnal cycle of evapotranspiration: a case study in Luxembourg" *by* Maik Renner et al.

Anonymous Referee #2

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General Comments

The author's objectives of the study were to use measurements of hourly incoming shortwave radiation as an independent forcing of the land-atmosphere exchange and assess the response and phase lags of surface heat fluxes. The authors argue that models of ET should be able to capture the magnitude of hysteretic loops under different conditions.

The writing is good, and the article is well structured. The major concern I have is that incoming solar radiation (Rsd) is used rather than the available energy (Rn-G). It is expected that phase lags would occur between Rsd and LE since much energy is stored in the ground surface during the day and then released at night, so it is unclear what the novel aspects of the paper really are. All the results are fairly straightforward, but again, they are to be expected based on the study design of using Rsd instead of Rn-G. Additionally, descriptions of what was assumed or used as input to the models, (specifically the PT and FAO-56 PM equations) is not adequate, only Rn is in the PT equation listed, not Rn-G as stated in the original equation, so there could be an error in the analysis. It is unclear if measured G was used in the FAO-56 PM equation, or if it was estimated, and same goes for Rn. Based on the lack of clarity as to what Rn and G model was used in the FAO-56 approach, those results cannot be assessed as is. While there is some good discussion on process, the novelty of the study is lacking. Perhaps a more useful and/or complementary analysis would be to focus on the hourly distribution of the energy balance closure ratio, and assess the controlling factors of the distribution, if any, as it relates to soil moisture and other conditions. As the manuscript is now, unfortunately, I recommend rejection with an opportunity to re-submit once the study design and novel aspects of the study are reconsidered.

Specific Comments Pg 2 Line 21-22: Would be good to give a quick summary of these metrics, and why some are more useful than others if they are to be used or referenced later. This would be good so that when the alternative metric is proposed below the reader has some context.

Pg 2 Line 29-30: LE is strongly correlated with Rsd, not the other way.

Pg 3 Line 2: I don't think that the other controls (other than Rn and Rsd) on LE remains unclear... it is pretty simple from an energy balance perspective (which is what is being discussed so far in terms of Rn and Rs) . . . LE = Rn – H – G . . . Lots to dig into with H obviously... and G, and perhaps that is where some of the controls need more study?

Page 3 Line 16: Why is Rn not used? Better yet, why isn't Rn-G (available energy) used? I don't see that why Rn (and Rn-G) is not used if the authors are indeed trying to better understand controls on LE... longwave radiation is a big component of Rn, and G lags no doubt control some of this hysteresis. I feel that the authors are missing too much energy if they just focus on Rsd.

Page 3 Line 23: The PT equation requires Rn, not Rsd, so how can you say you focus on using Rsd, but use the PT equation and not use Rn? Same with the PM equation. Please explain how and if Rs is used in these equations here and you can go into greater detail in the methods if needed.

Page 4 Line 7: but RH and VPD is coming from gridded weather data, no? So this is a forcing and outside the evaporation model, correct?

Page 6 Line 28-34: This is concerning since the heat storage in the soil slab above the G plate was estimated rather than measured. Sounds like the estimate didn't consider changes in soil moisture, which is a big factor in the potential to store heat within soils. Any errors in the estimate, or bad heat storage measurements could cause "perceived hysteresis" when comparing to other energy balance components. When was the harmonic calibrated, to dry or wet conditions, or both? Did the harmonic behave differently (have different parameters) when assessed during wet vs dry conditions as anticipated?

Page 8 Line 31: What time step was Qgap (the energy balance closure) assessed? Every 30min?

Page 9 Line 1-5: Was this done at 30min time steps?

Page 10 Line 25: The PT equations uses Rn-G, not simply Rn as written. What did you use? Rn or Rn-G (see equation 14 of the PT paper - ftp://ftp.library.noaa.gov/docs.lib/htdocs/rescue/mwr/100/mwr-100-02-0081.pdf). The phase lag results can't be interpreted until this is cleared up.

Page 11 Line 14-16: Is Rsd used and then Rn and G is estimated following the procedures of FAO-56, or is the measured Rn-G used? This needs to be spelled out to understand the results.

Figure 6. Be consistent calling incoming shortwave Rsd vs Global radiation... you say both.

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Discussion paper

Figure 7 isn't very useful since it is Rsd on the x, and not Rn-G. I guess I don't see the point since phase lag is to be expected (and greater for wet conditions as shown), and it is unclear how G was considered in the FAO approach.

Page 17 line 4-5: The authors state that "Generally, there was only a small hysteresis in the available energy (Rn – G) (Table 4)" which is exactly what one would expect if Rn-G was used. So by not including longwave and G there is phase lag, which is to be expected, so I don't see the point of the paper really… Also, there would be more phase lag in wet soil conditions, than in dry conditions since heat storage is greater when there is more water in the soil. By not considering G, you get phase lags… is there something novel to see here?

Page 20 and Figure 11: The results of the hysteresis in humidity variables are what you would expect. The VPD is lowest in the morning, and highest in the mid to late afternoon, and largely a function of es, since it is a fairly humid environment, so what is novel here?

Page 25 Line 22: Yes this was quantified, but it was expected, and it changes in time and space, based on the land surface conditions, and met. forcings.

Page 25 Line 23: Explain exactly how these results have practical application for remote sensing based models? This was never fully described, that is why this phase lag issue is so important for remote sensing studies of LE to consider or include.

Page 25 Line 30-33: There is too little information on the specifics in the paper of FAO-PM approach applied to assess if this is a correct conclusion.

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