

Hydrol. Earth Syst. Sci. Discuss., referee comment RC3
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Interesting and relevant manuscript, but requires some moderate revisions before acceptance

Werner Eugster (Referee)

Referee comment on "Resolving seasonal and diel dynamics of non-rainfall water inputs in a Mediterranean ecosystem using lysimeters" by Sinikka Jasmin Paulus et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-519-RC3>, 2021

Non-rainfall water inputs (NRWI) can be an important hydrological water source to plants in arid and semi-arid ecosystems, but also elsewhere during dry spells and drought periods. The authors argue that so far most measurements were done with microlysimeters that may overestimate NRWI if their construction is of a simple type that does not attempt to bring microlysimeter soil temperatures in good agreement with the surrounding soil. Contrastingly, a normal size lysimeter---of which the authors have 6 on site, where one was excluded from the analysis---have the advantage that a temperature control of the soil slab is possible and hence less of the problems reported for microlysimeters should result from using standard lysimeters with highly resolving weight measurement.

The authors present a full year of data from a Mediterranean site in Spain, but the focus of the manuscript is more on the method, the real measurements are more used in a proof-of-concept mode without independent and reliable (and established) validation data as obtained from blotting paper water collection and analysis. Thus, the manuscript could actually be classified as a "technical note". My suggestion is to suggest moderate revisions before accepting the paper. There are a few scientific errors that can be easily rectified in a thorough revision round (wrong physical units, mostly) and with some information the context and wording can be quite misleading and should be corrected. Moreover, the main shortcoming of the manuscript is the (1) the absence of a (at least simple) visibility sensor to be able to scientifically correctly separate fog from dew conditions, and (2) lack of robust and independent validation data for supporting the claim that the presented method---which seems to be a further development of Zhang et al.'s (2019) method---is more accurate than other approaches. Nevertheless I recommend proceeding with the manuscript, there is indeed a need for better and more accurate quantification of NRWI in all ecosystems where rainfall can be absent for prolonged periods.

MAJOR POINTS

Title: the paper is clearly focused on NRWI, and evaporation in my view is rather treated a side aspect (and not very clearly associated with NRWI except for the short discussion about downward latent heat flux being in good agreement with water vapor absorption estimates from the lysimeter). In my view a title of the kind of "Lysimeter based quantification of non-rainfall water inputs to a Mediterranean ecosystem" (maybe clearly classified as a technical note) would represent the contents much better.

62: "Recently Kidron and Kronenfeld (2020b) found that temperature inside the micro-lysimeters deviated from that in the surrounding soil, \dots"---here some rewriting is required. Firstly, the original paper makes unacceptable generalisations that require some caution when using that reference; secondly, the statement as presented here does not correctly reflect the contents. Kidron and Kronenfeld (2020b) used microlysimeters (ML) with a huge gap between ML and original soil, a cold-air trap that leads to excessive cooling of the pit at night and consequently to lower than normal soil temperatures. And that's the key effect, \textbf{not} that the soil temperature is different from the surrounding soil: if the soil inside the ML is colder than it should be, then you expect additional condensation to occur in the ML, thereby artificially suggesting and NRWI that is in fact an artifact. As you can see in Riedl et al. in Fig. 5 the soil inside our MLs is actually somewhat warmer or equal in temperature compared to the control. In this way the artifact of the ML criticised by Kidron and Kronenfeld (2020b) is avoided. Thus, the problem with Kidron and Kronenfeld (2020b) is that they generalize from their overly simplistic ML to all ML (which is not correct, a simple lid actually solves the problem, or at least reduces this artifact). A normal-size lysimeter with such a large gap around the lysimeter would also act as a cold air pit, the only advantage you have with a large lysimeter is that you could more easily add heating wires to heat the soil to more closely match the control temperature. Thus, I agree with your argumentation in lines 64--66.

169: The $10\text{ cm } T_a$ is not a real reference, see Monteith (1957). Why not extrapolate to the 1 cm height? At least you must reference and consider Monteith (1957), this is an omission which is not understandable. In my view all papers with Monteith as author or co-author are of a quality that makes them relevant even after decades, and omitting Monteith knowledge normally goes in the wrong direction (scientifically), away from what we call "progress".

313--315: see my remarks further up (line 62). This statement needs a rewording to take care of the flaw of Kidron & Kronfeld's questionable generalisation to all ML, and the fact that this overestimation could be overcome easily by using a smarter ML design with a lid to avoid the nocturnal cold-air pit.

Appendix A1: this is never referenced in the text and is a mess---either remove or rectify all the errors. If the latter is desired: C_p has wrong units; s see my comments; LW and SW have wrong units; ΔW has wrong units; use Δq instead of δq ; ε has wrong units (should be dimensionless or simply (---)); e_a has wrong units; u^* should have the asterisk in subscript

Appendix A2: Eq. (A1) would be simpler to read as
$$T_s =$$

$$\sqrt[4]{\frac{1}{\sigma \cdot \varepsilon}} \cdot \left[LW_{\uparrow} - (1 - \varepsilon) LW_{\downarrow} \right] \quad \text{end{displaymath}}$$

Appendix A2: the mathematical convention is to use either \cdot (`\cdot`) or space for multiplications of scalars, and only use \times for vector products; please update equations accordingly. Use `upwelling` and `downwelling` for radiation fluxes; there is always the potential confusion that a down-looking sensor actually measures upwelling radiation, etc. Replace the erroneous NA with (---) or (dimensionless)

Figure 6 and the text is actually a result of the study, not a discussion point. Maybe this is the reason why I think the title and text do not agree---if evaporation as mentioned in the title were the focus of the paper this aspect would have come first in the Results section, not last as an add-on in the Discussion section.

OTHER IMPORTANT POINTS

12: `eddy covariance-derived latent heat flux estimates`: since the paper focuses on NRWI I found this statement somewhat misleading because it only addresses the ET losses but not the gains that would be associated with NRWI. Moreover, the authors do not even define ET, which indicates that this was not the real focus of the manuscript.

23: horizontal precipitation does not belong to NRWI. It is rainfall (precipitation) which is not measured by standard rain gauges (but e.g. by special rain gauges on vessels).

26: it is not Feigenwinter et al. who defined the classification of fog. Rather use a primary source here, AMS, WMO, or (what I use) the AMS Glossary of Meteorology by Glickman (2000):

```
@Book{Glickman2000,
  editor = {Todd S. Glickman},
  publisher = {American Meteorological Society},
  title = {Glossary of Meteorology},
  year = {2000},
  address = {Boston, MA},
  edition = {2},
  comment = {formerly: http://amsglossary.allenpress.com/glossary/},
  url = {https://glossary.ametsoc.org/wiki/Welcome},
}
```

30: `However, differentiating between these two origins is commonly not possible (Li et al., 2021b)---`this is wrong, please read the text: Li just shows the opposite that because the sources of the vapor used in dew formation and the vapor from soil water are of such different origins, using stable isotopes allows to differentiate. You may argue about the

word "common", but stable isotopes are common by now (at least the simple-to-measure ones such as ^{18}O and ^2H in water and water vapor). MPI Jena is doing this since its establishment. Please reword to convey the correct content and context with this statement.

88: `` However, the structure of the cage allowed for grazing to maintain the lysimeters comparable with the rest of the plot."---this is a challenge and it would be interesting to read some more details how this is successfully done. As is, it is not possible to reproduce this as a reader.

128--129: Note to Editor: I cannot check the data and code which will be made available once the manuscript is the same. I normally also only publish code and data once a manuscript is accepted, but here the authors do not provide the details in the paper and expect readers to go into the code.

134: question: an animal stepping onto the column, is this not bringing ΔW outside of the accepted range and is thus treated there?

148: `` water input"---reword, NRWI is also a water input and this is not to be included here!

166: That's why Monteith (1957) uses T_a would be good if you could relate your text more to Monteith's outstanding work which is still our reference.

251: you show ET with negative sign convention, although you use a positive sign convention for λE in Eq. (2.1). Moreover, you never defined ET nor its sign convention. My recommendation is to use positive values as ET losses from the soil to the atmosphere. I have not seen papers using the reverse sign convention, yours is the first, and this confuses me and maybe also the reader. Recall that the standard hydrological budget equation is still: precipitation = runoff + ET +/- change in soil storage. Moreover, you use a positive ET in Fig. 3b. At least you need to be consistent and declare your symbols and sign conventions (if they should deviate from commonsense notation)

280: `` evaluation statistics improve by one hour"---I am unable to see this one-hour improvement in Table A2. Is there an error here, either in Table A2 or in the wording?

306: the absolute value of adsorption may be low (as total NRWI may be low), but the relative share in NRWI is quite high in my view. Maybe rather compare the relative numbers to focus on the processes leading to the different components of NRWI

377: ``At our site, this pattern is obvious and indicates that night-time EC measurements could serve to detect adsorption (Fig. 6).''---I partially disagree, but you may convince me with your arguments. In my view adsorption is not directional as dew formation (from vapor above the canopy) or distillation (from vapor below the canopy), and hence should in my view not leave the best trace in EC-based flux measurements. I though (so far) that dew formation should lead to negative λE , whereas distillation is not seen by an EC system; and adsorption should only be seen in EC fluxes if its vapor source is above the canopy, but not below. Your Figure 6 of course empirically shows better performance (qualitatively) for vapor adsorption than dew formation, but for me the explanation is not that obvious as your text implies.

397: ``LE fluxes at dry conditions''---you never defined or used LE, but this appears to be an important statement that should have appeared in Discussion already. Conclusions should not bring up new aspects that were neither addressed in Results nor in Discussion.

Figure 1: in the text it sounds as if you want to use this as a general workflow also for other sites. But then my recommendation is to avoid site-specific magic numbers in the scheme and provide the site-specific values in the caption to be clear. E.g.: $T_s < (T_{\text{dew}} - T_{\text{dew}, t})$ with the information that $T_{\text{dew}, t} = 1.4 \lambda E$ for your lysimeters and site.

Figure 1: just of curiosity because we were challenged on this aspect with our ML: why do you not use the high-quality measurements of the drainage outflow of your lysimeter to make sure that drainage loss at the bottom of the soil slab is not erroneously treated as ET? You classify $\Delta W \leq 0$ as ET, although it could be drainage loss after intensive rain. This may be an important aspect if you think the method should also be applicable elsewhere.

Figure 1: the percentile approach to separate fog from dew is in my view weak and questionable. Namely dew can only form under conditions that you classify as ``fog'' but fog droplets---if advected---can be present at relative humidities that are not showing saturated air (high rH).

Figure 4: move the text in the dial at upper right so that there are no overlaps

Figure 4: reduce the size of the end marks of the whiskers

Figure A1: there is an error on the x-axis, this is rH as a fraction, not in $\%$. Moreover Oswin (1946) and Lewicki (200?) are missing in references. And please don't chomp off the right part of the display. I also cannot see the 95th percentile in my printout, and the 85th would profit from a thicker line width. Why do you use x and y in the equation when your variables are actually rH and

SWC ? You never defined that $x = \mathrm{rH}$ and $y = \mathrm{SWC}$.

Table A2: The caption claims that the units of all values are hours day^{-1} , but the table heading claims that this is only hours; and then there might be an error: if cor means correlation, a unitless and dimensionless information, both are not correct. To be standalone you must define cor , mae and rmse (you don't use mse defined on the previous page). Moreover I cannot see what you want me to see according to the text (see comment elsewhere)

DETAILS

32: Meissner et al. is not a complete reference, year etc. are missing

42: what do you mean with "modeling frequency"---not intelligible to me

51: please add Riedl et al., accepted on 16 November 2021 at HESS. Last manuscript version available here (note that one more co-author appears in the finally accepted version): <http://homepage.usys.ethz.ch/eugsterw/publications/pdf/Riedl.2021.FINAL.pdf>, discussion paper version available via <https://doi.org/10.5194/hess-2021-317>

77 and elsewhere: data are plural in formal English, please correct

81: \textit{i} lex should be lower case; and remove the excessive white space before 20 trees

95--96: "They rest \dots ": this sentence is not intelligible to me, please rephrase in an understandable way

112: add "and" (Pt-100 and capacitive \dots)

115: add "The" in The Netherlands

120 and elsewhere: u_* always has the asterisk in subscript, never in superscript

120: add reduced space between m and ss

121: add comma in R3-50, Gill \dots

127 and elsewhere: note that Figure should be upper case if a specific figure of your manuscript is referenced; it is however lower case if you use figure for "Zahl" or "Wert" in German

131: delete "together"

139: not a number is NaN , whereas NA means not available (it is the code for missing values). That's wrong here. I assume you mean not available NA .

184: add s to describe s

190: remove s from model precitions (not model s \dots)

207: δ is conventionally only used for isotopic ratios, ∂ is used for partial differentials, and Δ is used for finite differences. Here I think using Δq instead of δq would reduce confusion if readers are familiar with isotopes, and personally I even think that using capital delta is the correct notation anyway.

209: the Clausius-Clapeyron relationship is not a straight line and thus "slope" is the wrong word here. ss is actually de/dT ; thus rewording is required

209: there is an error here, λE is not the latent heat (of whatever), but the latent heat flux . Please correct.

221: you defined γ to be in units of Pa K^{-1} , but here the implicit assumption is that it is in kPa K^{-1} . Stick to your definitions; if readers use the equation as is with saturation pressures in kPa, then the first term is 1000 times too large.

223: C_p is not specific heat of air. It is the specific heat capacity of the air at constant pressure. Please correct.

227: replace moments with periods

229: in the text you correctly use "diel", probably being aware that diurnal can also express the opposite of nocturnal. My suggestion is to modify the subsection title to match the text (diel)

247: should be "its" without apostrophe

254: a sum has no \pm unless you specify in M&M how you obtained the uncertainty (the reason: random errors of the mean have an average of zero, and thus for a sum there are no degrees of freedom to specify a random uncertainty). Please correct.

258: you arbitrarily change from ET to ET -- please homogenise (and define the version you keep)

263: your total of the components is 41.9 mm, but you specify 42.0 mm. The convention is to either specify the component that contains the missing 0.1 mm or to round accordingly if no additional component is part of the game. Note that modern round rules round 0.05 to 0.00 but 0.15 to 0.20 (this is essential to avoid drift). Another conventional rule is to round up the component that was closest to rounding down, to correctly represent the reported total.

264: I am surprised to see 50.6% vapor adsorption. This seems to be quite a large value, but maybe is correct in this ecosystem. Here some independent (e.g. blotting paper) validation of the components would really have strengthened the paper.

294: "Our observation that especially nights 295 are prone to the formation of NRWI is also documented in the literature." This is an utterly trivial statement, do you really want to keep this in a scientific manuscript? For me it is on the same level as "the grass was green and photosynthesis was important during daylight hours, as reported in the literature" .

336: should be "its" without apostrophe

379: use ```scale up"` instead of ```up scaling"`

Figure 3: suggestion to move the legend to the panels and only show the curves that relate to the respective panel. In the legend some colors are hard to distinguish as is. Moreover, having a legend outside the plot area is Excel standard, not with scientific presentations.

Figure 3: use `\verb+par(lend=1)+` to avoid the rounded (and thus unclear) endings of the bars

Figure 5: panel (e) is not described. I assume that the second mentioning of (d) should actually be (e)

General: `\LaTeX` typesets equation by assuming that characters are variables (if they are known), hence `$$\mathrm{rH}$$` and `$$\mathrm{SWC}$$` look odd in your text. Consider using `\verb+\mathrm{rH}+` and `\verb+\mathrm{SWC}+` instead

References: add doi or URL to Thom et al. (with scanned papers the doi is normally only shown on the publisher's website)

References: add doi to Sonntag et al.

References: add space before parenthesis in Zhang et al. (2019a)

References: check Dirks et al.

References: check Kosmas et al., seems to be an incomplete / corrupted entry

References: check Nair et al.

References: check Peters et al. (2014)

References: check Rodrigues-Iturbe et al.

References: generally only the doi is necessary, not doi resolved by the standard doi resolver plus the doi with the publisher's doi resolver and/or an alternative URL. See <https://www.hydrology-and-earth-system-sciences.net/submission.html#references>

References: generally rectify the entries according to the guidelines. Paper titles are normally in sentence case whereas journal names and book titles are using capitalised words

References: is there no doi/URL for IUSS \ldots

References: Meissner et al. is incomplete

References: Monteith is incomplete

References: Orchiston is incomplete

The editor is informed about my (friendly) long-term relationship with some of the co-authors.

PS: sorry for the LaTeX markups, I was not aware that HESS removed that option this year ...