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Interactive comment on "Separating precipitation and evapotranspiration from noise – a new filter routine for high resolution lysimeter data" *by* A. Peters et al.

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General comments
Like every environmental time series, lysimeter weight measurements typically exhibit stochastic fluctuations ("noise") around the true signal. In the past these were often treated as if they were negligible when evaluating the water balance of a lysimeter at daily or hourly intervals. However, recently researchers began to analyse high-frequency measurements down to 1 measurement per minute, in order to ensure that short-term phenomena, such as evaporation from plant leaves after rainfall, are not ignored (e.g. von Unold and Fank, 2008). At this sampling frequency, every

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positive fluctuation would be counted as precipitation, and every negative fluctuation would be counted as evapotranspiration. Traditional denoising methods fail, because they cannot fully eliminate these errors and often leave some residual noise, which consequently leads to an overestimation of both precipitation and evapotranspiration fluxes. This problem can be encountered by setting a threshold value for significant changes between two individual measurements; however, it appears wise to choose a lower threshold for calm and a higher threshold for capricious conditions. The authors of this study present a sophisticated, data-driven approach to set both this threshold and a moving window for initial denoising dependent on the signal strength and noise intensity of a lysimeter's weight signal. They describe their proposed procedure in an understandable manner and show exemplary applications to data measured during different meteorological conditions, and they arrive at conclusions that both contribute to scientific understanding and stimulate future research. I strongly encourage both the editor and the authors to publish this well-written and valuable article in a timely manner, but I do have some questions, comments and minor corrections which I will discuss in detail below.

Specific comments

- p. 14647 l. 6–8: *"the flux leaving the soil-plant system towards the atmosphere within a certain time interval is given by evaporation (E [mm]), interception (I [mm]) and transpiration (T [mm]), often summed up to evapotranspiration (ET [mm])."*
  Please make it clearer that you mean evaporation of intercepted water and not the process of interception itself.

- p. 14647 l. 11–12: *"The reference evapotranspiration (ET_0 [mm]) is often determined with a class-A pan."*
  Class A pan evaporation is not the same as (grass) reference evapotranspiration

as described by Allen et al. (1998).

- p. 14647 l. 18–19: *"In order to determine P and ET, the masses of lysimeter and seepage water have to be measured in high temporal resolution."*
  Please elaborate on why this is necessary, since traditionally, lysimeter water balances were often evaluated much less frequently than 1 min^{-1}. I suppose it would be helpful for some readers to explain why this is (or is not) a bad idea and why calculating the mass balance, for example, between two measurements 30 min apart can be significantly different from calculating a 30 min mass balance using 1 min measurements. A brief discussion on definitions would be a valuable addition to this. For example, should we count small precipitation events that evaporate almost immediately and are not recorded by low-frequency sampling systems in long-term water balances? Where do we draw the line between negligible and significant events? Which time scales are of practical interest and which are of interest for researchers? All of these are fundamental questions that have to be discussed with the increasing availability of high-frequency, high-precision measurement devices. I do, however, understand if the authors deem this discussion beyond the scope of their rather technical paper.

- p. 14648 l. 4–5: *"small mechanical disturbances (e.g. caused by wind)"*
  and p. 14649 l. 11–12: *"in periods with low wind speed the data are more accurate than in periods with high wind speed"*
  Just out of curiosity: Have you seen this in your data (should be relatively easy to test), or is this an assumption? Do you believe that this noise is always symmetric around the mean signal? Using local regression or moving average filters makes it appear so during residual analysis, but this does not necessarily need to reflect the true nature of the stochastic errors.

- p. 14648 l. 9: *"the measurement noise is not a constant value"*
  Please rephrase. If noise was a constant value, it would not be noise but a

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systematic error.

- p. 14652 l. 1: *"The typical way to filter noisy lysimeter data is (i) to introduce a smoothing routine, like a moving average with a certain averaging window w, and then (ii) to apply a certain threshold value δ, accounting for measurement"*
  While this is a reasonable approach, I don't think I would call it "the typical way". In fact, I would argue that, as of writing this review, there is no such thing as a "standard method" for the estimation of P and ET from weighing lysimeter data. Often the mass balance is evaluated at intervals greater than 1 min, where noise is certainly present, but not as apparent as in high-frequency measurements. In a few cases, this is preceded by slight smoothing using a simple moving average, a Savitzky-Golay filter, or, less common, Wavelet denoising. However, I am not aware of cases where a smoothing-and-thresholding procedure was applied that were not published relatively recently.

- p. 14652 l. 20: *"Data with small noise (smooth evap in Fig. 2) need a small value for δ"*
  I don't think so. The choice of a suitable threshold parameter is a tradeoff between noise-canceling and signal-preserving properties. Since, in this case, less noise has to be canceled, the threshold parameter **can** be smaller, but it certainly does not need to be. In fact, if the threshold is too low, one may count, for instance, autocorrelated errors that are "traced" by the smoothing algorithm, as significant. The thresholding procedure is, from my experience, very robust in terms of sensitivity to high values of δ (but it does have a cut-off point when δ is too low, i.e. when it falls below the noise).

- p. 14653 l. 5–6: *"Second, a moving average with adaptive window width is applied"*
  Why did you decide to use a moving average, despite its poor spike-preserving properties? A Savitzky-Golay filter would not be much more computationally ex-

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pensive, since it is only numerically equivalent to the center point of a local polynomial regression, but mathematically equivalent to a weighted moving average (Press, 1992).

- p. 14653 l. 8: *"the software is available from the authors"*
  Why not directly release it as supplementary material then?

- p. 14653 l. 16: *"The order of the polynomial must be high enough to guarantee that it can describe the data in the time window reasonably well"*
  What would be the practical implications of just using a straight line fit? From what I understand, a lower-order polynomial would lead to only slightly lower moving windows and slightly higher threshold values, both of which I see nothing wrong with. Fitting k_{max} polynomials and calculating a model selection criterion for each point seems unnecessarily impractical and computationally expensive to me.

- p. 14655 l. 8–11: *"This is especially important, when the amount of data to be filtered is large. In this study we used approximately 2×10^5 data points, meaning that 2×10^5 polynomial fits had to be conducted."*
  Actually it would be $k_{\mathsf{max}} \times n_{\mathsf{data}}$ fits, i.e. 1.2×10^6 in this particular case, not to mention the additional calculation of Akaike's information criterion (although that part may be negligible). This is another reason why I would recommend investigating the consequences of simply using a straight line fit.

- p. 14655 l. 21–22: *"w_{max} for events with no evaporation or precipitation"*
  It is in the nature of your approach that only the "intensity" and not the duration of a signal is considered. For example, an absolutely straight line with a nonzero slope (e.g. a constant evaporation rate over a long time) does not get smoothed with $w = w_{\mathsf{max}}$, although this would be preferable. Do you have an idea how to solve this problem?

- p. 14657 l. 8: *"For all three filters, the threshold value was 0.081 mm"*

As you mention in p. 14659 l. 4–6, this value is probably a bit too small. I would assume that choosing a threshold value slightly too high would have less negative implications than choosing one slightly too low. Therefore, I believe that adding the case of $\delta_{\text{min}} = \delta_{\text{max}} = 0.24\,mm$, i.e. an AWCT (adaptive window, constant threshold), to this comparison would be very interesting.

- p. 14657 l. 27–p. 14658 l. 1: *"The strong wind event is better described by the AWAT filter as by the SG filter and equally well as by the MA filter"*
  How do you know?

- p. 14658 l. 18–19: *"not perfectly filtered anymore"*
  Unfortunate choice of words, in my opinion. Implies that you have a measure of perfectness and that your filter performs perfectly during certain situations.

- p. 14659 l. 1: *"residuals are more or less normally distributed"*
  Have you tested this? If not, "symmetrically" would probably be a more suitable wording.

- p. 14659 l. 12–13: *"as long as a change from t_{i-1} to t_i is regarded to be insignificant, the value for t_{i-1} is kept"*
  This should be in the methods section, not in the results. The explanation in p. 14652 l. 16–19 is somewhat short anyway. It should be described in more detail how this is not simple thresholding, but what I would call "thresholding with memory" (which is necessary to preserve the shape of the cumulative signal).

- p. 14659 l. 14–15: *"This leads to an underestimation, and thus to negative residuals for evaporation events and to an overestimation and thus positive residuals for precipitation events. "*
  I would humbly suggest performing the residual analysis only for those t_i at which a change in mass was counted as significant, or to find a suitable interpolation

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procedure between two such points, in order to eliminate the step-like shape of the reconstructed signal.

- p. 14660 l. 7–9: *"The fluxes estimated with the SG filter can even increase as w increases. This might be due to the fact that the SG filter tends to oscillate"*
Very true and important statement, and even more obvious when using non-adaptive procedures. From my experience, a rather narrow w and a sufficiently large δ generally yield satisfying and robust results, though. I found δ to be relatively robust if larger than a certain cut-off point (which is lower than the noise-intensity itself). That was, however, only tested using fully synthetic data and measurements from a modern lysimeter with three precise load cells, not a lever-arm counterbalance system that is known to exhibit stronger and oscillating noise (e.g. Nolz, 2013).

- p. 14661 l. 3–4: *"it is not recommended to use the Savitzky–Golay filter for evaluating lysimeter data"*
I found this statement a bit too generalized to be derived from a single study on very specific events, measured with a lysimeter featuring a somewhat dated weighing technology.

- p. 14661 l. 8–10: *"Figures 6 and 7 show that $\delta_{max} = 0.24$ mm was a much better choice than $\delta_{max} = 0.081$ mm. Thus, it is concluded that δ_{max} can be set to any reasonably high value."*
Alternatively the variable threshold procedure could simply be omitted. I found it difficult to compare those two cases, since one is AWAT and one is AWCT. Again, these would be easier to compare if you examined a case where $\delta_{\text{min}} = \delta_{\text{max}} = 0.24\,mm$.

- p. 14661 l. 13–14: *"Choosing w_{max} carefully with expert knowledge"*
Or trying to derive it from the data, leading to a fully adaptive filtering method.

But such methods have to be tested thoroughly using both synthetic and real data that feature a broad range of meteorological conditions.

- p. 14661 l. 15: *"For our benchmark events, including very different atmospheric conditions,* $w_{max} = 31$ *min led to the best results."*
  This is in agreement with my own experiences.

Technical and stylistic corrections

- Whole document: ET should be typeset like all other water balance variables. Either make the other variables upright as well, or use the `mathit` command to typeset slanted variables with multiple capital letters without too much space between them.
- p. 14648 l. 11: *"up to 5 times"*
  Small numbers should be spelled out, i.e. "up to five times", as you did in the preceeding line.

References

Allen, R. G., et al. (1998). "Crop evapotranspiration-Guidelines for computing crop water requirements-FAO Irrigation and drainage paper 56." FAO, Rome 300: 6541.

Nolz, R., et al. (2013). "Interpretation of lysimeter weighing data affected by wind." Journal of Plant Nutrition and Soil Science 176(2): 200-208.

Press, W. H. (1992). Numerical recipes in Fortran 77: the art of scientific computing, Cambridge university press.

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