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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "The pulse of a montane ecosystem: coupled daily cycles in solar flux, snowmelt, transpiration, groundwater, and streamflow at Sagehen and Independence Creeks, Sierra Nevada, USA" *by* James W. Kirchner et al.

Anonymous Referee #1

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The manuscript presents a thorough study of the daily cycles of different hydrological variables during rainless periods in different seasons, reflecting diurnal extraction of shallow groundwater by evapotranspiration and diurnal additions of meltwater during snowmelt. Basis of the study is extensive dataset of diurnal cycles of stream water level, groundwater levels, sap flow measurements, snow characteristics (snow water equivalent) and several other hydrometeorological conditions in the studied catchments (two snow-dominated headwater catchments in California's Sierra Nevada mountains).

In the introduction section authors clearly present the basis of the topic. They explain

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Discussion paper

two Âżcontrasting Âń concepts, namely, the water table fluctuations (WTF) and missing streamflow approach, which have been traditionally used for analysis of the observed daily cycles. Furthermore, the question of time lags between the daily minimum and maximum ET and snowmelt rates vs. daily stream discharge and groundwater levels cycles is thoroughly explained and gives a reader (even those without strong theoretical background) good insight into the discussed topic.

The manuscript is long; however, I see no other option to present the discussed topic in a such holistic way. Namely, the authors have successfully covered different aspects of the research: (1) extensive field measurements; (2) theoretical background upgraded by a simple, but innovative conceptual model of the riparian groundwater mass balance and (3) presentation of the remote sensing data to support the concept. All 3 parts combined give excellent and holistic picture of the discussed topic.

By using stream levels data directly, authors presented a way how the problem of rating curve uncertainty can be avoided in analysis of daily cycles of various hydrological variables which are usually obtained through different hydrological measurements. This can be problematic especially in the range of extreme values (in this case during low-flow conditions).

In my view, the paper suits well in aims and scope of the HESS journal and I strongly support the publication of the manuscript. I would like to congratulate the authors for their excellent work. Bellow I provide some specific comments which are more or less technical.

Specific comments: Line 45: Maybe the role of specific sub-catchment characteristics (the low-land part of the catchment becomes ET dominated before the headwater (high-land part) could be more directly highlighted in the abstract. In the transition period, the temporal prevalence of snowmelt over the ET and vice-versa, strongly depend on the local topography characteristics. This aspect is thoroughly discussed in the manuscript.

Line 144: I suggest changing the sentence: . . .making these basins, in terms of climate characteristics, ideal. . .

Line 235: If I understand correctly, the "absolute" sap flow values were not directly used (the sap flow instruments were not calibrated) since the temporal changes (daily cycles) of sap flow were used in the study. Could lack of the calibration (drift) also influence the timing of the sap flow daily cycle?

Line 271: The reported data gaps occurred during the observed 3 years?

Line 279: Could the elevation bands limits centered on SNOTEL stations be also shown in Fig. 1 for illustration purposes?

Figure 6: Groundwater levels shown are absolute values? I would suggest slightly changing the Fig. 6 caption sentence (line 1060): . . .averaged roughly 200, 1050, 1080, and 950 mm above the Sagehen Creek water level. . .

Line 369: Is this correct? What could be an explanation for a "static water level of 0.6m above the ground surface"?

Lines 405-408: Could authors support their statement with measurements shown in Figs. 3 and 4? This would enable readers to understand the underlying processes more easily.

Line 422: What kind of water level? Groundwater, streamflow or some kind of "general water level" as an indicator of hydrological state of the catchment?

Line 435: What is "A" in the combined forcing $P + M + G - E = A\cos(wt)$?

Line 515: Were the average lag times for the two stations assessed from low flow discharge conditions (where the diel signal is evident) or using wider range of discharge conditions?

Figure 9c (lower plot): If the cloudy days and rainy periods were excluded form the dataset, how can detrended stage fluctuations data be continuous (or are they only seemingly continuous)?

Lines 600-601: Since DEMs from Lidar data are available for studied catchments, could authors make their statement more tangible (quantifiable) in terms of the approx. % of slopes (catchment) facing north/south direction?

Lines 841-843: I would suggest mentioning also the problem related to the fact that diurnal cycles in stream water level in larger catchment (especially during the ET dominated periods) is pretty much undetectable (or unrecognizable) by the waters stage measuring equipment. Of course, this could be related to various lag times to the gauging stations as mentioned by the authors.

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