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Hydrology and Earth System Sciences
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.

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Review Summary: Overall, I'm very happy to see this paper. The authors have done a nice job using an integrated and well-measured field site to present the inter-relations between multiple aspects of diurnal cycles in both streams and groundwater in a setting experiencing both snowmelt and evapotranspiration. This is a solid contribution to the field, and I recommend it be published after revisions, particularly addressing my major comments, as follows:

1. While the authors have done a wonderful job integrating and presenting their results, most of what they show is not new. Lundquist and Cayan (2002), see Figures 12-14, clearly illustrate the presence of both snowmelt and ET driven diurnal cycles in river basins. Lundquist and Dettinger (2003), which I have also attached here, with citation below*, as it's hard to find, takes this concept further (see Figures 5 and 6) by using the diurnal cycle switch to highlight inter annual variations in water supply and climate. The paper here builds nicely on this work, but it would be better to present the information as a development and illustration of already published ideas rather than a new idea.

2. At multiple points in the paper, the authors seem to dismiss earlier literature as missing key physical concepts and as being incomplete. At times the tone is dismissive and gives the impression of lacking respect for the earlier work. The paper would be a much stronger contribution if the authors instead addressed why the earlier work took different approaches than here. In many cases, this can be addressed by the different hydrogeologic settings of the basins, which fundamentally changes how the different processes interact and which matter the most. The Tuolumne studies (including many of the papers by Lundquist and by Loheide) are in a granitic basin with very shallow soils, which is quite different from the groundwater dominated Sagehen basin. This fundamentally changes the role of diurnal fluctuations in groundwater on the overall stream signal. (In the detailed comments below, I have called out places in the paper where this contrast could be addressed.)

3. With regards to 2 above, the paper lightly addresses comparisons and contrast between Sagehen and Independence Creek. These could be strengthened through better consideration of dominant terms in different hydrogeologic settings and with further discussion of how these two sites relate to the sites in the literature. Sarah Godsey, the second author, has a nice paper on how geology relates to low flow sensitivity to snow across the Sierra, and it seems like this could be a nice tie in with this study and a discussion on hydrogeologic setting.

If the authors have questions for me regarding these comments or would like the dis-

cuss, I can be reached at Jessica Lundquist, jdlund@uw.edu. I apologize for my time delay in getting this posted. *Citation: Lundquist, J. D. and M. D. Dettinger, 2003. Linking diurnal cycles in river discharge to interannual variations in climate. Proceedings, AMS 17th Conference on Hydrology. Long Beach, California. available at: https://ams.confex.com/ams/annual2003/webprogram/Paper55265.html

Specific Comments Follow:

The paper has a whole has a very nice literature review, but the intro seems to diminish, rather than highlight the work that went before.

line 91: What is an “integrodifferential relationship” ? This is confusing.

Lined 105-109: I think the Loheide and Lundquist paper is a link here. These two assumptions are compatible if the stream and the groundwater levels essentially rise and fall at the same time. Most papers state that ET flux variations are only true in this very linked riparian zone. I don’t follow the argument that they must be separate hypotheses.

Lines 115-120: Again, I must beg to differ here. The Lundquist papers focused on the early (snowmelt-dominated) season, in a granite-lined basin with a meadow/riparian system whose groundwater levels responded essentially in synch with the streamflow levels. Again, it’s not incompatible, but it’s also very nuanced. I think a better way to discuss this would be that the ideas may be system specific and not directly transferable across systems. I think most people are making simplifications that matter for their systems without explicitly discussing other possible systems. So yes, it makes sense to bring them all together, but the “incompatible” statements don’t seem right to me.

Line 130: Loheide and Lundquist (2009) had observations as well. Also, with regards to “few studies have examined things together”, it seems to me that there are few diurnal cycle studies in general, but it seems like about as many have looked at both as have looked at one.

Upper Independence Basin is more similar to the Tuolumne watershed (compare and contrast your results with the literature).

A fair bit of the literature is also concerned with how much of the riparian area actually takes part in diurnal fluctuations. âĂŤ can you address this issue?

line 240: Given the sharp rain-shadow gradient in these areas, I would recommend using the 800-m PRISM normals for distributing the Snotel rather than elevation weights (different locations at the same elevation can get quite different amounts of snow). However, I doubt that this would change any of your main results here, so this comment is mainly for future reference rather than a requirement to redo your precipitation mapping for this particular paper.

line 335: also in Lundquist and Cayan 2002

line 360: This discussion is relevant to your “incompatibility” argument, see notes above.

Your Fig. 9 is in L&C 2002, see their Fig 14. This is also in Lundquist and Dettinger 2003, a preprint from a conference (https://ams.confex.com/ams/annual2003/webprogram/Paper55265.html, also attached here). See Figures 5 and 6, which essentially show what you are getting at here.

Fig. 10: You’re using straight sinusoids. We know that they’re assymmetric. See Lundquist and Cayan 2002.

Line 429: You mean Lundquist and Dettinger (2005) here (not Lundquist and Cayan 2005).

Line 430: Again, I think it’s worth comparing and contrasting how the assumptions made in these different systems really relate to the underlying geology. In a granitic system like Tuolumne, there isn’t much of a riparian aquifer (unlike in Sagehen, with deep soils) Section 2.2 in Lundquist et al. 2005 discusses the hillslope/riparian flow

paths. Loheide and Lundquist 2009 goes on to show that for the Tuolumne system, the riparian groundwater levels are driven by the stream water levels and not vice versa. Again, you are correct that Sagehen should be modeled differently, but your paper as a whole would be a stronger contribution if you put your results in the context of the varying hydrogeology represented in the literature.

Line 584: This is illustrated in Lundquist and Dettinger 2003, see Figure 5.

Line 620: Also, Independence Greek has more granitic geology and less groundwater reserves. It makes sense in the hydrogeologic context that this would have a snowmelt-dominated signal longer.

Lines 847-850: Data do not appear to be available at this time. Please do check that everything is publicly available and clearly interpretable (with readme files, metadata, etc) before final acceptance of the publication.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2020-77/hess-2020-77-RC3-supplement.pdf

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