We thank Jessica Lundquist for her comments on our manuscript. Below we respond (in bold type) to Prof. Lundquist's specific comments (in normal type).

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:

We thank Prof. Lundquist for these supportive comments.

1. While the authors have done a wonderful job integrating and presenting their results, most of what they show is not new.

We disagree with the claim that "most of what they show is not new".

Specifically, Figures 3, 4, and 5 illustrate the strong hour-by-hour coupling between the solar flux and the rate of rise and fall of riparian groundwater tables, during both snowmelt and ET cycles. Most of this is new. Our Figure 5 somewhat resembles Figure 6 of Loheide (2008), but that only concerns evapotranspiration cycles, and compares derived estimates of ET and potential ET, not solar flux and the rate of groundwater rise/fall (although these are obviously related).

Our conceptual model analysis presented in Section 3.3 and illustrated in Figures 7 and 8 is also new. Although the conceptual model makes broadly similar assumptions to those of Gribovzski et al. (2008), as we acknowledge on line 426, we apply our model in different ways and reach new conclusions. In particular, our analysis leads to two conclusions that are new, and in our view, significant:

> **1) The commonly observed lags between peak snowmelt or peak ET and the daily peak or trough in streamflow are largely dynamical phase lags, not travel-time lags, at least in small catchments. This result challenges the assumptions underlying decades of prior work, including Wicht (1941), Jordan (1983), Bond et al. (2002), Lundquist et al. (2005), Lundquist and Dettinger (2005), Wondzell et al. (2007), Barnard et al. (2010), Graham et al. (2013), and Fonley et al. (2016); see the manuscript for the full citations.**
>
> **2) The amplitude of the daily cycle in streamflow cannot be quantitatively linked to the daily ET or snowmelt flux, unless the time constant tau of the near-stream groundwater system is quite short. This result also calls into question over 50 years of prior studies, including Tschinkel (1963), Meyboom (1965), Reigner (1966), Bond et al. (2002), Boronina et al. (2005), Barnard et al. (2010), Cadol et al. (2012), and Mutzner et al. (2015).**

We also note that this conceptual model also explains the asymmetry in snowmelt and ET cycles in streamflow, which were pointed out by Lundquist and Cayan (2002).

The diel cycle index developed in Section 3.4 and illustrated in Figure 9 is also new. This provides a new tool for characterizing seasonal transitions between snowmelt and evapotranspiration cycles in small basins.

In Section 3.5 and Figure 10 we observe that, as snowmelt cycles give way to ET cycles, the amplitude of daily cycling in the stream nearly vanishes. We also infer that this results from destructive interference between the snowmelt and ET signals originating from different parts of the catchment. These points are also, to the best of our knowledge, new.

We also observe that the transition between snowmelt and evapotranspiration cycles occurs differently in groundwaters and streamflow, due to the fact that groundwater cycles mostly reflect local forcing and streamflow integrates that forcing over the drainage network (Section 3.5 and Figures 11 and 12). This transition occurs earlier or later at different points along the drainage network (Figures 12 and S3),

reflecting differences in snow accumulation and melt (and also in the onset of evapotranspiration) from place to place depending on altitude and aspect. This is all, to the best of our knowledge, new.

We also show that the spatial pattern in daily streamflow cycles is consistent with the spatial evolution of snow cover and vegetation activity as seen from space (Figures 13 and 14). This is also, to the best of our knowledge, new.

These new observations and inferences comprise almost the entire manuscript, whether measured by number of figures or length of text. Thus the claim that "most of what they show is not new" is factually incorrect.

Lundquist and Cayan (2002), see Figures 12-14, clearly illustrate the presence of both snowmelt and ET driven diurnal cycles in river basins.

Of course, and we say almost exactly this in the second sentence of the manuscript, "Both snowmelt and evapotranspiration cycles result from daily variations in solar flux, but are of opposite phase (Lundquist and Cayan, 2002…" We certainly do not want to give the impression that we are claiming to have discovered snowmelt and ET cycles in streamwater (indeed, these were known in the literature for decades before Lundquist and Cayan's work, as section 2 of their paper makes clear).

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.

Figure 5 of Lundquist and Dettinger (2003) is a verbatim copy of the previously mentioned Figure 14 of Lundquist and Cayan (2002). Figure 6 of Lundquist and Dettinger (2003) makes the point that snowmelt cycles switch to ET cycles earlier in drier years, based on what appears to be a preliminary analysis of daily cycle asymmetry in four years at one river. We don't think it is correct to say that our manuscript "builds nicely on this work", given that neither the methods nor the questions are similar: we use the diel cycle index rather than daily cycle asymmetry to measure the transition from snowmelt to evapotranspiration cycles, and we do not focus on year-to-year variations in the timing of these transitions, but rather the spatial evolution of those transitions in a catchment context.

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.

We certainly do not mean to be dismissive or disrespectful toward prior work. However, in some cases it is unfortunately necessary to point out where the assumptions or conclusions of these previous studies are contradicted by our data, or by accepted physical principles of water flow in hydrologic systems. For example, nearly every paper that discusses the propagation speed of snowmelt or ET cycles assumes that this propagation speed equals the flow velocity of the water itself, but that is simply not correct. It has been known for decades that changes in flow rates in hydrologic systems propagate at the kinematic wave speed, not the bulk flow rate. We have tried to point this out as gently as we can, but we cannot avoid the fact that it needs to be said.

The paper would be a much stronger contribution if the authors instead addressed why the earlier work took different approaches than here.

We do not want to speculate about why earlier studies took the approaches that they did. Obviously it is a different situation if the papers themselves reveal their motives, but that is rarely the case.

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.)

We agree that different hydrologic settings could be important, but we are not aware of evidence that supports the statement that "this fundamentally changes the role of diurnal fluctuations in groundwater on the overall stream signal." Indeed, our data suggest the opposite, because daily snowmelt and ET cycles in our granitic Independence Creek drainage are strikingly similar to those in the more groundwater-dominated Sagehen Creek basin. We believe this is because the transmission of daily cycles depends on how groundwater and soil water respond to incremental additions and subtractions of water from the surface, not on the total volume of water in storage. Note that, for example, S in Eq. (4) is transient storage (or, as the manuscript puts it, "storage above the stream"), so whether there is a large volume of water stored below the stream does not change the behavior of our conceptual model. And again, the field data support this view, because the daily cycles in Independence Creek and Sagehen Creek are similar, even though one basin is granitic and the other has an extensive groundwater system.

To make this point clearer in the manuscript, we will add the following figures and text:

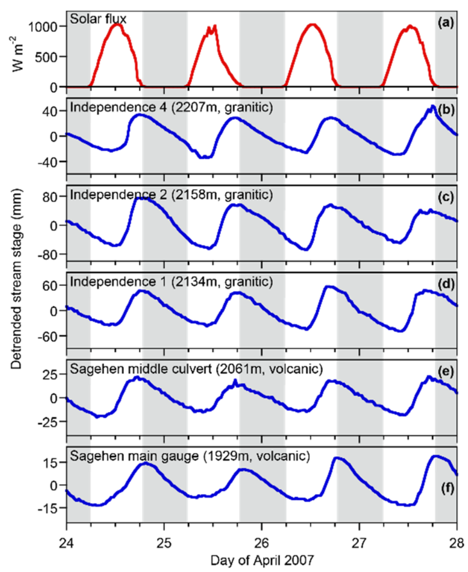

Figure A. Snowmelt-driven daily cycles in stream water levels measured in April 2007 at three locations along Upper Independence Creek, underlain by glaciated granodiorites, and two locations along Sagehen Creek, underlain by thick volcanic and volcaniclastic deposits. Stream stages were detrended using Eq. (3). The shapes and phases of the daily cycles are similar, and all exhibit similar lags relative to the solar forcing, despite the marked geological differences between the two catchments.

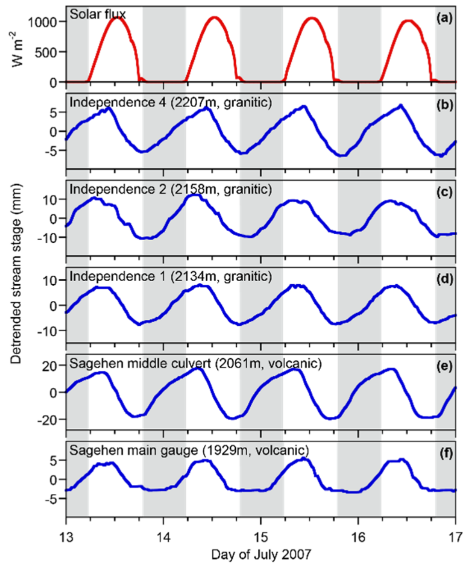

Figure B. Evapotranspiration-driven daily cycles in stream water levels measured in July 2007 at three locations along Upper Independence Creek, underlain by glaciated granodiorites, and two locations along Sagehen Creek, underlain by thick volcanic and volcaniclastic deposits. Stream stages were detrended using Eq. (3). The shapes and phases of the daily cycles are similar, and all exhibit similar lags relative to the solar forcing, despite the marked geological differences between the two catchments.

Although this conceptual model has been developed in the context of Sagehen Creek, which has an extensive groundwater aquifer, the mechanisms described here do not require substantial aquifer storage. In the model, changes in storage equal changes in discharge multiplied by the characteristic response time τ. This directly implies that the daily range of storage also equals τ times the daily range of discharge. At the Sagehen main gauge, where we can measure daily cycles in units of discharge (at the other stations we lack rating curves and thus have only stage measurements), typical daily ranges of discharge during peak snowmelt were ~2-4 mm/day in 2006 (above-average SWE), 0.2-0.6 mm/day in 2007 (below-average SWE), and 0.4-1 mm/day in 2008 (roughly average SWE). Even τ values as small as ~0.2-0.5 days are sufficient to generate significant lags between peak snowmelt and peak streamflow, implying that these lags could be associated with storage changes of only 0.4-2 mm in 2006, 0.04-0.3 mm in 2007, and 0.08-0.5 mm in 2008 (the ET cycles, and their associated ranges of storage, are about 1-2 orders of magnitude smaller). This simple calculation implies that significant dynamical phase lags can be generated from small daily variations in soil water and shallow groundwater, and that a substantial groundwater aquifer is not required.

This inference can be tested by comparing daily streamflow cycles in Sagehen Creek with those in Upper Independence Creek. The Upper Independence basin is dominated by glacially scoured granodiorites (Sylvester and Raines, 2017) and lacks the volcanic and volcaniclastic deposits that host Sagehen's extensive groundwater aquifer. Despite this sharp contrast in hydrogeology, Figs. A and B show that snowmelt and ET cycles are similar in Upper Independence Creek and Sagehen Creek. Streamflow cycles lag the solar flux curve by slightly more at the Sagehen main gauge than at the other four stations shown in Figs. A and B, reflecting the fact that the main gauge is farther downstream from its most distant headwaters (7.9 km, compared to 2.6-3.9 km for the other four stations) and integrates over a larger drainage area (27.6 km2 vs 4.7-7.7 km2 for the other stations), and thus accumulates commensurately larger kinematic wave lags. The daily cycle amplitudes also differ, due to differences in drainage areas and channel cross-sections among the different stations. Nevertheless, the clear conclusion from Figs. A and B is that the shapes of the daily cycles, and their phase lags relative to the solar flux, are strikingly similar between the granitic, glacially scoured Upper Independence basin, and the groundwater-dominated Sagehen basin. This strongly suggests that

similar mechanisms shape the streamflow cycles in both basins, despite the marked differences between their geological settings.

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.

A multi-site intercomparison study would certainly be interesting, but would be an entirely different study from the one we present here. Challenges confronting any such intercomparison would be the differences in which variables are measured at which sites, as well as the general problem of data availability (although Prof. Lundquist's Yosemite Hydroclimate Network, http://depts.washington.edu/mtnhydr/data/yosemite.shtml, is a good example of how this problem will be gradually overcome). But again, this would be an entirely different study. Beyond the comparisons between Sagehen and Independence outlined above, we do not want to make assumptions about "dominant terms in different hydrogeologic settings" for other basins without having a clear basis for those assumptions.

If the authors have questions for me regarding these comments or would like to discuss, I can be reached at Jessica Lundquist, jdlund@uw.edu.

Thanks for the very helpful discussion that we subsequently had by video conference. We particularly appreciate your suggestion that a conceptual diagram would be help readers to visualize the groundwater-stream interactions that underlie the lag in daily cycles. We will therefore add the following figure to the manuscript:

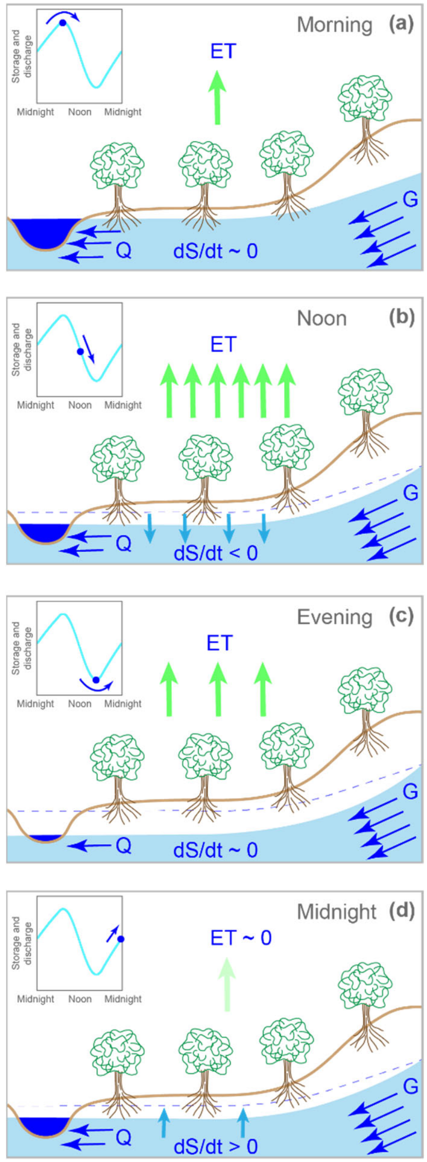

Figure X. Visualization of groundwater-stream coupling that leads to lagged evapotranspiration cycles in groundwater levels and streamflow (snowmelt cycles are similar but reversed). Streamflow is supplied by drainage from riparian groundwater, and this drainage rate is faster at higher levels of riparian groundwater storage (S). Riparian groundwater storage changes at a rate dS/dt that depends on the flux balance between streamflow (Q), evapotranspiration (ET), and groundwater recharge from surrounding uplands (G). The relative magnitudes of these fluxes in each panel are indicated by the number of arrows; upland recharge (G) is constant but the other fluxes vary from panel to panel. Inset figures show the corresponding phases of the daily cycle in streamflow and groundwater levels. In the morning (a), groundwater storage and streamflow reach their maximum and begin to decline as the evapotranspiration rate rises enough, relative to the difference between groundwater recharge and discharge, that the riparian aquifer reaches equilibrium and begins to decline. Around noon (b), high evapotranspiration fluxes lead to a strongly negative flux balance and a rapid draw-down of groundwater storage, and thus a rapid decline in streamflow (the dashed line indicates the morning high-stand of groundwater levels and stream stage, as a reference). Toward evening (c), riparian groundwater and stream stage reach their minimum and begin to rise when evapotranspiration rates and streamflows decline enough that the riparian aquifer reaches equilibrium and begins to refill. During the night (d) riparian groundwater levels (and thus stream stages) slowly rebound, because evapotranspiration is nearly zero and upland recharge exceeds stream discharge.

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.

That is certainly not our intention. In several cases, though, we need to point out limitations and inconsistencies in the literature. We have tried to do that as gently as possible, consistent with the need to also be clear.

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

Another reviewer also found this confusing, and we will change it. What we meant is a relationship that is described by a differential equation rather than an algebraic one.

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.

We will try to clarify this argument in the manuscript, because the assumptions underlying the water table fluctuation approach and the missing streamflow approach are in fact mathematically inconsistent with one another. This inconsistency has nothing to do with whether groundwater and streamwater rise and fall at essentially the same time (which they also do in our conceptual model, and also in our data).

Here is the problem. "Missing streamflow" methods must assume that daily additions and removals of water from the catchment are transmitted 1:1 to the stream; otherwise, the change in streamflow does not quantitatively reflect the snowmelt or evapotranspiration rate. But daily additions and removals of streamflow are not transmitted 1:1 to the stream if groundwater levels also vary on a daily cycle, because in this case, the daily addition of snowmelt (for example) is partitioned between <u>both</u> the daily change in streamflow and the daily change in groundwater storage.

Conversely, water table fluctuation methods assume that daily removals of groundwater by ET are reflected 1:1 in changes in groundwater storage (net of an assumed constant input from upland recharge and constant output to streamflow); otherwise, the change in groundwater level does not quantitatively reflect the evapotranspiration rate. But daily removals of groundwater by ET are not reflected 1:1 in groundwater storage if streamflow (which is generated predominantly from groundwater…) also varies on a daily cycle.

Thus the "missing streamflow" approach assumes that ET and snowmelt will only change streamflow fluxes (and thus groundwater storage will be constant), and water table fluctuation approaches assume that ET and snowmelt will only change groundwater storage (and thus that streamflow fluxes will be constant). Thus these premises really are inconsistent with one another.

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.

Our statement was that studies that attribute lags in daily cycles exclusively to travel times and flow velocities are incompatible with the assumptions that underlie water table fluctuation (WTF) methods. WTF methods assume that groundwater integrates its inputs, which will create a substantial phase lag even in the absence of any travel-time lags. If the assumptions of the WTF approach are correct, then the first several hours of the lag in groundwater or streamflow cycles cannot be attributed to travel times. Thus the two approaches do, in fact, make incompatible assumptions.

This has nothing to do with whether groundwater levels and streamflow levels are synchronized with one another (which they are in our conceptual model, and also often are in our data, as well as in your data from Tuolumne Meadow). Rather, the issue is (as stated in lines 111-121) whether the cyclic response by *both* groundwater and streamflow lags the cyclic forcing by snowmelt or ET, and whether this lag arises from a phase lag due to integration in the shallow groundwater system, or due to travel times.

The phase lag that we have identified at Sagehen is also seen at Independence Creek, and it is also present in your own Yosemite data, including at Budd Creek, Delaney Creek, and Lyell Fork below Maclure (at 2940 m, and only about 4 km below the headwaters at Lyell Glacier). Thus these different geological settings exhibit a consistent pattern of behavior. Generating this same pattern of behavior, with roughly the same lag time, by travel-time delays would require the same rather particular set of conditions (depth of snowpack, distance to channel, length of channel network, etc.) to hold across these very different settings.

Line 130: Loheide and Lundquist (2009) had observations as well.

We will change this sentence to eliminate the distinction between modeling and observational studies.

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.

We agree that there are few diurnal cycle studies in general, but even among this group, studies of coupled groundwater/surface water cycles are relatively scarce – particularly those that actually measure both groundwater and surface water cycles, along with measurements of sapflow/ET as a driver. Even scarcer are studies that have looked at both groundwater/surface water cycles in response to both snowmelt and ET, in the spatial context of elevation gradients and the temporal context of seasonal shifts from snowmelt to ET and back again.

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

As we point out above, the daily cycles in streamflow at Upper Independence, and in headwater streams in the Tuolumne basin, are similar to those at Sagehen despite the substantial difference in lithology.

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

We cannot, for the reasons described in Section 3.3. To do this by modeling requires assuming that we actually know the volume of the daily snowmelt or ET forcing, but as Section 3.3 makes clear, we cannot know this unless we also know the time constant (tau) of the riparian soil water/groundwater system. Alternatively, one could detect the spatial extent of groundwater fluctuations using direct measurements, but it would require a more extensive groundwater monitoring network than we have.

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.

Thanks, yes, this is an interesting point. The SNOTEL sites are sited along the rain-shadow gradient, so they capture the rain shadow effect rather well. In any case our results do not depend on mass balances, so they don't require that we have an ideal interpolation of the SNOTEL data.

line 335: also in Lundquist and Cayan 2002

We will add this reference.

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

We don't understand the point here. The Loheide and Lundquist study was conducted more than six weeks after snowmelt ended in Tuolumne Meadow, and the snowmelt signal in the stream was generated much higher up in the basin (where there are no groundwater measurements). Thus the Loheide/Lundquist paper does not bear directly on how snowmelt signals are transmitted to the stream, which is our focus here.

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.

We disagree with the claim that "Your Fig. 9 is in L&C 2002", which could be construed as accusing us of using prior work without attribution. We are sure that this was not your intention, but nonetheless we need to set the record straight. Below we show Figure 14 of L&C 2002 and our Figure 9, side by side.

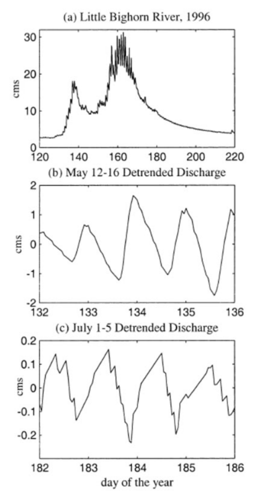

FIG. 14. (a) A 1996 hydrograph for the Little Bighorn River, illustrating how diurnal cycle changes as snowmelt forcing gives way to evapotranspiration/infiltration forcing. Periods illustrated in (b) and (c) were fit to a line, which was then subtracted out to accentuate the diurnal fluctuations.

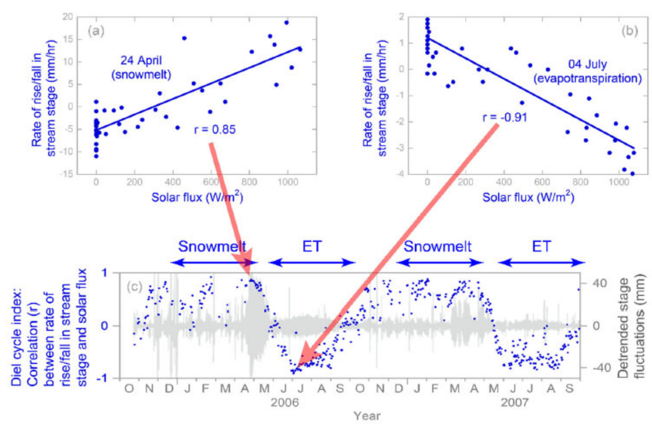

Figure 9. Correlations between solar flux and rates of rise and fall of water levels (Sagehen Creek, B transect) during two example days, one when the catchment was snow-covered and the stream exhibited a strong snowmelt cycle (24 April 2007), and another when the catchment was snow-free and the stream exhibited a strong evapotranspiration cycle (4 June 2007). In the lower plot, the correlation coefficients (blue dots) for each day indicate the relative dominance of snowmelt or evapotranspiration as generators of daily cycles in Sagehen Creek, while the gray shading shows the amplitude of the detrended daily stage fluctuations.

We are perplexed by the statement that the figure on the right is contained in the figure on the left. The figure on the left (from L&C 2002) shows only that streams can exhibit daily cycles driven by both snowmelt and evapotranspiration, and that these cycles have different shapes. Claiming that that our Figure 9 makes the same points is simply inconsistent with Sections 3.4 through 3.6 of our paper.

Below we show Figures 5 and 6 from Lundquist and Dettinger (2003). Again, we do not think that our Figure 9 is equivalent to these or contained in them. The figures below concern how the <u>asymmetry</u> of the daily cycle changes between snowmelt and ET-dominated cycles. Our Figure 9 concerns the <u>phases</u> of the cycles, and in particular, the phase relationship between time derivative of stream water levels and the solar flux (as a driver, through snowmelt and ET, of those rises and falls in stream water levels). This is quantified through the diel cycle index, which is nowhere mentioned in Lundquist and Dettinger (2003) or L&C 2002.

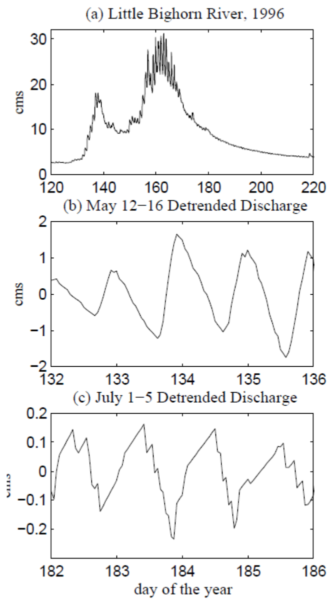

Figure 5: *(a) The 1996 hydrograph for the Little Bighorn River, illustrating how the diurnal cycle changes as snowmelt forcing gives way to evapotranspiration/ infiltration forcing. Periods illustrated in (b) and (c) were fit to a line, which was then subtracted out to accentuate the diurnal fluctuations.*

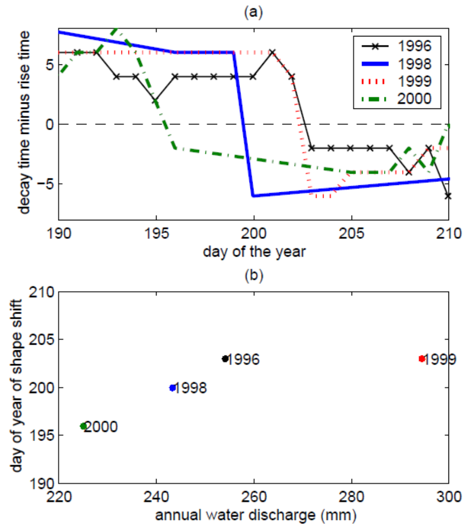

Figure 6: *(a) Shifting of the flow in the Little Bighorn River from a longer diurnal decay time (snowmelt-dominated) to a longer rise time (evapotranspiration/infiltration-dominated), as the recent snowmelt season draws to a close. (b) The date of the shift as a function of the total discharge for each year. In general, drier years shift earlier in the summer.*

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

We agree that they're asymmetric, and in fact our analysis explains *why* they're asymmetric (see our Figure 7, and Section 3.3, particularly the paragraph beginning on line 480 – where we already cite two Lundquist papers). The point of this figure was to simply show that the disappearance of the diel cycle in the stream was due to the destructive interference between the snowmelt and ET cycles. The asymmetry in those cycles is not important to this point, but we will re-draw the figure to include it.

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

You're right, sorry, that was just a typo. We'll fix it (we had it right in the reference list).

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.

Yes, but Section 2.2 of Lundquist et al. (2005) discusses these flow paths in terms of the transit time of the water, which is not what controls how rapidly changes in flow rates will be transmitted to the stream. Changes in flow rates propagate at the kinematic wave velocity, not at the mean flow velocity. This is true in snowpacks, in unconfined groundwater systems, and in open-channel flow (in confined groundwater systems, changes in flow rates propagate even faster, at the pressure wave propagation velocity).

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.

One cannot make that statement about riparian groundwater levels throughout "the Tuolumne system", but only about Tuolumne meadow, and only during the period studied by Loheide and Lundquist, more than a month after snowmelt ended there. The daily cycles studied by Loheide and Lundquist 2009 are driven by snowmelt many kilometers upstream from Tuolumne meadow (and understandably, by that late in the season, they drive the variations in the groundwater rather than the other way around, since there is no

remaining snowpack, and thus no locally-generated snowmelt, at the Tuolumne Meadow groundwater wells).

But the snowmelt cycles in the Tuolumne River entered the river somehow, and the headwater gauges (Lyell Fork below Maclure, Budd Creek, and Delaney Creek) indicate that snowmelt cycles enter the stream with several-hour lags relative to the snowmelt rate itself. We show that similar lags can arise whenever inflow rates to the stream are coupled to riparian groundwater storage, which integrates the snowmelt input itself. (In principle, this lag can also arise by kinematic wave propagation through snowpacks, hillslopes, and channels, but it would take some rather special circumstances for the resulting kinematic wave lag to be so similar in so many different settings.)

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.

We are not saying that Sagehen should be modeled differently, or saying that its geology differs in ways that are important for these purposes (although we recognize that this may be your view). We are instead saying that it is essential to recognize that in all catchments, rates of streamflow are linked to the volume of water stored in the near-stream aquifer, which in turn integrates inputs from snowmelt and removals from ET. This conceptual picture is consistent with the phase relationships observed not only at Sagehen, but also at Independence Creek and the headwater Yosemite sites.

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

We don't understand the basis for this statement. Figure 5 of Lundquist and Dettinger (2003), reproduced above, shows that snowmelt and evapotranspiration cycles have different shapes. That is different from the point made here, which is that these two cycles will destructively interfere in the stream, resulting in the daily cycle becoming weak and reversing phase as it shifts from being snowmelt-dominated to ET-dominated.

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

We don't understand the rationale behind this statement. The snowmelt-dominated signal will last as long as snow is melting in the riparian corridor, and melting in sufficient amounts that the snowmelt signal dominates over the competing evapotranspiration signal. Having more or less groundwater will not change this balance, but having more snow and melting it later certainly will. And our Figures 13 and 14 indicate that this is indeed the case here; the snow-covered fraction at Upper Independence Creek remains higher, for longer, than at Sagehen Creek.

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.

We intend to do this.