We appreciate Anonymous Referee #2's comments on our manuscript. Below we respond (in bold type) to the referee's specific comments (in normal type).

The manuscript presents a comprehensive study of the hydrological cycle in two montane catchments in the Sierra Nevada, USA. The analysis uses a large dataset to explain how groundwater and streamflow daily fluctuations are dynamically related to transpiration and snowmelt daily cycles forced by solar radiation. A simple and elegant model is used to explain these relationships.

As I understand, the main result of the study is to have identified that in small catchments the links between the daily fluctuations of streamflow (Q) and both transpiration (T) and snowmelt are mediated by the groundwater storage in the riparian zone.

Therefore, the lags appearing between the daily cycles of streamflow and their forcing variables are not due to travel times, but are associated with the dynamics of the whole system with groundwater acting as a buffer that dampens and delays the response of streamflow. This shows that methods to estimate T (or evapotranspiration, ET) using series of Q are not feasible unless characteristics of the riparian aquifer are also known.

Although the manuscript addresses a topic certainly interesting for the readers of HESS, I found it extremely difficult to read. The manuscript is very verbose and I often found myself lost in long explanations about concepts that were not really of interest or strictly relevant.

We understand that the paper may appear long by contemporary standards, in which results from a project are often salami-sliced into as many different papers as possible. Instead of splitting our results into three or four separate papers, however, we have chosen to present them together because they are interconnected. In any case, the manuscript is not that long compared to some others in HESS. It is about the same length as Kirchner and Allen (2020, https://doi.org/10.5194/hess-24-17-2020), for example, and much shorter than Kirchner (2019, https://doi.org/10.5194/hess-23-303-2019).

We disagree with the characterization of the manuscript as "very verbose". Clarity and completeness often require explaining things rather than just asserting them. We also need to allow for the fact that individual readers will have stronger background in some areas than in others. We are aiming at readers who may have some background in one or another aspect of these systems, but may not be experts in all of the topics that we are covering. Readers' points of interest may also differ. Thus each reader may find some "concepts that [are] not really of interest or strictly relevant", but different readers will have different opinions on which specific concepts those are.

Therefore, my suggestions and detailed comments listed below are mainly directed to shorten and hopefully improve the readability of the manuscript.

- Title: already the title seems long. Could it be shortened into something like "The pulse of a montane ecosystem: relating daily cycles of hydrological variables".

We will think about this. Titles always represent a trade-off between the need to be explicit and the need to be concise. "Relating daily cycles of hydrological variables", in our view, is too cryptic (which variables? relating how?). There is also the need to cover the important keywords for search purposes, which is why we have identified the fluxes and storages (snowmelt, transpiration, groundwater, and streamflow), the study sites, and the region (Sierra Nevada).

- Abstract: this is also very long. I would try to shorten it to make the key messages of the study clear to readers.

We will see what we can do. The problem here is that the paper presents many interconnected results. These go well beyond the one that the reviewer has focused on (that riparian aquifer dynamics imply that we cannot infer ET rates from streamflow cycles). If that were the only punchline of the paper, the abstract could end at line 34. But the paper also shows that groundwater cycles reflect the relative dominance of snowmelt vs. ET, and streamflow cycles integrate these signals over the contributing catchment (lines 36-

47). We also show that temporal patterns of streamflow cycles are quantitatively consistent with the spatial evolution of snowmelt and ET during the transition from winter to summer and vice versa, as viewed from LANDSAT and MODIS (lines 49-54). And for the abstract to be comprehensible to the reader, the results need to be (briefly) explained, not just asserted. We will see what can be cut or condensed, but we don't want to sacrifice comprehension for the sake of brevity.

- Line 31: "...transiently achieves mass balance." This is not clear to me. The mass balance should be always satisfied.

This depends on whether "mass balance" means "inputs balance outputs plus change in storage" (which is of course always satisfied), or "inputs balance outputs" (which is not always the case). We meant the latter. We can change "solar forcing declines enough that the riparian aquifer transiently achieves mass balance" to say instead "solar forcing declines enough that inputs transiently balance outputs in the riparian aquifer".

- L34: I would not use here "time constant", because that related to the simple model presented in Eq. 5, which assume τ to be constant to obtain an exact solution of the equation. However, as I understood reading the manuscript, the riparian aquifer might have a response time that is not constant.

That is correct. Although "time constant" is sometimes used to refer to a characteristic response time that may not be strictly constant, "response time" would be better here. We will change it.

- L90-91: is integro-differential the correct term here?

We are not referring to an integro-differential equation (that is, an equation with both integrals and derivatives). But, as stated, there is an integro-differential relationship between groundwater levels and ET: groundwater storage integrates ET, and ET is the derivative of groundwater storage, assuming other fluxes are trivial. Integrals introduce 90-degree phase lags, hence we should expect a roughly 6-hour phase lag between daily ET or snowmelt cycles and the resulting groundwater cycles (again, assuming there are no other fluxes; drainage to streamflow complicates this picture, as we describe later). Nonetheless, if the term is confusing we can remove it, and say, "…the WTF method implies that groundwater levels integrate snowmelt or evapotranspiration signals…"

- L108: I believe that the WTF method as defined by White (1932) did not account for Q because the observations were done in a desertic environment where Q was not relevant.

That is correct, but the WTF method is often applied in many situations where Q (or drainage to deeper aquifers) is not zero. In any case, we believe that the statement we made is correct: "Missing streamflow methods assume that the daily cycle in ET results only in a daily cycle in streamflow, and not a daily cycle in groundwater levels, whereas WTF approaches assume the exact opposite. One can of course question whether either set of assumptions is realistic, but they certainly cannot both be correct."

- L129: Gribovszki

Sorry! We do know how his name is spelled, and we have no idea how that typo got there. We will fix it.

- Section 2.1: I would erase the pronunciation of the catchments and historical information that is not necessary to understand the analyses presented later on in the manuscript. I don't think information about potential evapotranspiration is provided for the catchments, and rainfall and temperature are not given for the Independence basin.

Rainfall and temperature are not reported for the Independence basin because they are not measured there. (Upper Independence creek is a remote area with no roads and no trails, and we have only measured stream stage variations there. It is nonetheless interesting for our purposes because its bedrock is much less permeable than Sagehen's.) Potential evapotranspiration is not reported because it is not directly measured, and its calculation is assumption-dependent. We mention the pronunciation of "Sagehen" because colleagues have mentioned to us in the past that they don't know how to say it (and because for

many readers, hearing words in their heads is important to comprehension). In any case, it's just three added words. Is saving three words really that important?

It would be good if the description of the two catchments followed the same structure to facilitate the reading.

This is not feasible because we have a lot more that we need to say about Sagehen than about Independence.

L191-195 can be erased.

We disagree, because if this information is not provided, some readers may wonder about the possibility of human influences on the phenomena that we report.

- Section 2.2: a lot of details can be removed (e.g., precise location of gages). A lot of this information is already in Fig. 1 (latitudes and longitudes could be reported in the figure or tables instead of the text). I would move the description of the sapflow measurements (L232-239) at the end of this section. At the moment, the description starts with weirs and bores, switches to sapflow, and then goes back to bores. L241-245 can be erased.

Precise locations of gauges are not reported; the lat/long coordinates are those of the snow telemetry (SNOTEL) stations, but we can remove them. We disagree that "a lot of this information is already in Fig. 1". Although of course the *locations* of the various sensors are shown in that figure, the *relevance* of those locations will not be obvious to readers – for example, that the SNOTEL sites span the same altitude range, and the same distances from the Sierra crest, as the Sagehen catchment does – so this needs to be spelled out in the text.

The reviewer is simply incorrect in stating that "At the moment, the description starts with weirs and bores, switches to sapflow, and then goes back to bores". Lines 219-230 describe the weather stations and SNOTEL station (boreholes are not mentioned there at all, and the main gauge is mentioned only as a reference point for the locations of the SNOTEL stations). Next, lines 232-239 describe the sap flow measurements, lines 241-253 describe the water stage recorders and the stream gauging stations, and lines 255-266 describe the borehole transects.

- L256: "To account for the combined..."

Line 256 does not say anything like that. Line 276 does say, "To take account of the combined…". We don't see any difference between "to take account of" and "to account for", besides that one is four characters shorter than the other. Nonetheless, we can change it.

- L345-346: I do not think it is correct to say that solar radiation drives streamflow and groundwater fluctuations. There is an indirect relationship, as also stated at L557-558.

It is broadly acknowledged in the literature of this field that these daily cycles are ultimately derived from the daily cycle in solar radiation. It is not a 1:1 relationship, but that is not what "driver" means.

- L385-389: it is not really clear what an integro-differential system is in this context.

OK, we will change that to "Dynamical phase lags arise whenever one system component integrates another."

- L390-415: this part is rather long and it seems that is repeated more precisely after Eq. 5.

We are trying to build understanding. We want to explicitly link our analysis to the simple dynamical systems approach (lines 390-398), explain how the relationship between storage and discharge arises mechanistically (lines 398-401), give the reader an intuitive understanding of how the phase lag arises from storage integrating its input fluxes (lines 403-409), and then explain that we will be now be analyzing a simple specific example of this more general system (lines 410-415).

I would just introduce Eq. 4, say that Q is assumed to be a linear function of S (i.e., $Q = f(S) = S/\tau$) and then write Eq. 5.

There is a big difference between just assuming that Q is a linear function of S (whereupon readers will wonder, "but what if it isn't?"), and making the point that we make here, namely that even if Q is a nonlinear function of S, it will be approximately linear over small ranges of Q and S, so our analysis still works.

I would avoid mentioning that the solution is well known (erase L434) and provide the solutions in Eq. 6.

We need to mention that the solution is well known, or else readers will think that we are claiming that this is an original result, when instead it can be found (in one form or another) in almost any textbook on linear systems analysis.

I think it should be better to say that it is assumed that the period considered is without P; I do not think it is reasonable to assume P with a daily cycle as M, G, and E.

We are not assuming that P has a daily cycle, but rather that P+M+G-E has a daily cycle. Obviously this criterion is met if P is zero and M+G-E has a daily cycle.

Should there be a mention of the initial conditions for these solutions? I understand that the point is to look at cycles and the initial transient is not important; however, in Fig. 7, I found it strange that the initial values of Q were different.

There are no initial conditions, and there is no transient. That is not how Fourier methods work. Equation 6 gives *exact analytical* solutions that are valid at *all values of time*, from –infinity to +infinity. That's because Fourier methods assume that the cyclic input repeats forever, with no beginning and no end. Equation 6 is neither derived nor solved using numerical integration, so it does not require initialization and there is no transient.

What the reviewer calls "the initial values of Q" in Fig. 7 are not initial values at all; they are just the values of the solution at midnight (and midnight, every night, is exactly the same, because the cyclic input and the cyclic output go on forever in both directions). Since these are not initial values, it is not "strange" that they are different. They are different because different values of tau yield different amplitudes and phases in Eq. 8.

- L460-479: I would erase this part. In most cases the inversion of the Fourier transform will be done numerically; therefore, one can just solve Eq. 5 numerically to start with.

Yes, but if you solve Eq. 5 by numerical integration, you have to worry about initial conditions and transients (and the comments immediately above illustrate how these can become confusing), and you have to worry about numerical stability. More importantly, if you numerically integrate, you get a numerical answer but you don't get insight, whereas from Eq. 6 or Eq. 8 you can directly see how the amplitudes and phases depend on each of the parameters. (And although this is not important here, Fourier methods can be massively more efficient than numerical integration, which is why they have been extensively used in global circulation models.)

The point about the lags is clear from Eqs. 6 and their discussion.

Of course, but only for an individual cosine wave. In general, the different frequencies are damped and phase-shifted by different amounts. Thus if you have only one cosine wave (Eq. 6), your solution is another cosine wave. The input and the output have the same shape, just with a phase shift and change of amplitude. But if you have an input that is not a pure sinusoid, then *the output cycle will have a different shape* from the input cycle. Thus the apparent lag between the peaks will be different than they would be for a pure sinusoid.

- L501-504: the references to the lines in Fig. 8 do not seem correct.

Good catch! We changed the figure but forgot to update the text. We'll fix it.

- L572-573: I would erase this phrase.

We think it is important, because many colleagues have said to us, "What's diel? You mean diurnal, right?". This indicates to us that the terminology needs to be explained.

- Subsection 3.5: I am not sure this is so important to deserve a full subsection.

Regardless of the question of importance, the material does not fit with the other subsection headings. And we do think it is important. Many colleagues have interpreted the vanishing of the daily cycle as indicating that the snowmelt cycle has ceased (and then later, the evapotranspiration cycle has begun). Instead, the daily cycle vanishes because the snowmelt and evapotranspiration cycles are canceling each other out.

- Subsection 3.7: I do not think this subsection is really necessary. I found that it was not adding much to what already presented and supported by the data. I would recommend to cut this part out.

This section may not be necessary for what the reviewer considers to be the "main result" of the paper. But in our view, that is not the only important result. Another important result is that the diel cycle index reflects the spatial variation in snowmelt and ET throughout the drainage basin, and the only way to directly visualize (and quantitatively verify) this result is through remote sensing. To the best of our knowledge, an analysis like this has never been done before. Really, who knew that stage fluctuations in streamflow reflected spatial patterns of snow cover and photosynthetic activity that you can see from outer space?

- L724-725: the mismatch between the peaks in radiation and sapflow is not surprising. Vapor pressure deficit (VPD) is usually the variable that mostly drives transpiration, and I believe that VPD would likely explain the timing of the transpiration peak during the year (that's because there appears not to be water limitation).

Although we don't go into this here (because we can't do everything, and the topic is not central to the paper), the data do not support the reviewer's conjecture. VPD remains high until September or October in most years (because the late summer and early fall are hot and dry), whereas sap flow rates begin declining in July. Controls on seasonal patterns in sap flow are discussed extensively in Cooper et al. (in review), which we cite here.

- L773: I would use "changes in storage" instead of "mass balance".

If the reviewer really doesn't like "mass balance" we would suggest "flux balance" instead. "Changes in storage" isn't wrong, but it gives the wrong focus: on the trends in storage rather than the relationships between the input and output fluxes.

- L779-781: erase?

We think it is important to point this out, because otherwise readers could look at Fig. 7 and ask, "yes, but what about storage?"

- Figures: the captions of most figures are very long. Because the figures are explained in detail in the text, I would try to reduce the length of the captions, where a brief description of what the figures show should be enough.

The figure captions are written this way as part of a deliberate communication strategy. Minimalist figure captions often lead to unnecessary workload and confusion for the reader, who must jump back and forth between the figure and the text (perhaps several pages away) in order to understand what the figure says. Furthermore, many readers scan papers by looking at the figures without reading the text, meaning that the figures should be able to stand on their own.

Putting interpretations in figure captions can be a great help to readers, who can thereby get a sense of what the figures *mean* rather than just what they *are*. Experience has shown that authors often think that their figures will be self-evident (which of course they are *for the authors*, who already know what they are

trying to say), and fail to comprehend how divergent a reader's understanding may be. Thus it is a smart communication strategy to lean in the direction of over-explaining rather than under-explaining.

- Figure 2: this figure is repeated in a different format in Figs. 11, 12, and 14. I would have these data in a single figure without repetitions.

The whole point of Figs. 11, 12, and 14 is to let readers see the relationships between seasonal patterns of snow accumulation and melt, and patterns in daily cycles of in streamflow. Readers cannot see these connections if the things that they are supposed to connect are in different figures, many pages apart from one another.

- Figure 10: if Subsection 3.5 is reduced or removed, perhaps this figure can be removed as well.

For the reasons that we explained above, we think it is important to keep subsection 3.5.

-Figure 11: because sapflow and groundwater are related in this figure, I wonder whether it would be better to report the depth to the water table from the surface to show that the water table is within reach of the root system. In the caption, it is said that signals were detrended but it is not explained how.

Unfortunately we don't know the depth of the rooting zone. The detrending procedure is documented in Eq. 3 at the end of section 2.3.

- I would consider removing Figs. 13 and 14 along with Subsection 3.7.

As we explained above, section 3.7 presents a novel analysis that draws connections that have never been drawn before. We believe that this section, and the corresponding figures, should be kept.