Answers to Reviewer 01 Comments

Note: Reviewers comments in **bold**, answers in *italics* type.

This article describes the implementation of an analytic steady-state water balance on 221 sub-catchments in central Asia. It is very well put together, reads easily from start to finish, has extensive useful figures showing the results, a careful splitting of the maths into "just the important results" in the main text and all the "tedious derivation" in the Appendix, and some thoughtful caveats in the Discussion.

The authors want to thank the reviewer for these kind words. We are also grateful for the thorough comments below which we individually address.

It is nit-picking to point out on line 169 that you only need dx(t)/dt=0 in Eq(2), as a value of w0=0 would indicate an impermeable land surface where P=Q, or a volume with no storage such that the evaporative process has no opportunity to occur.

This is, of course, correct and we propose changing the inline equation correspondingly to dx(t)/dt=0 in the revision.

Figure 8 does show very low values of w0, and linear fits crossing the x-axis, but there is no indication in the text that w0 ever reduced to zero (or negative); Zr as a component of w0 was positively correlated to temperature which always increased in the future periods used.

The D66 Zr root zone depth is very low in the high mountain parts of the study region, i.e., the Pamir plateau and the highest elevation parts in the Kyrgyz Naryn catchment. We propose to add this information to the main text in the paragraph starting on line 419.

The linear fit crossing to below zero values is misleading as it is non-physical. We thus also propose to edit the Figure 8 correspondingly to not extend the best fit linear model to the Zr<0 region.

Given that the solution to the water balance is a single line equation that is solved explicitly, why do the authors go down the path of using differentials to estimate changes in Q? Such an approach is usually taken when the computation is much more complex or requires some iterative solution, so for small enough perturbations the differential is a good estimate of change in output. You already need to calculate new values of E and P and all their components for the three future periods, so the new Q value could be calculated directly and compared to the baseline period. The only indication of the values (or magnitude) of the sensitivity coefficients is Figure 11 which

visually indicates that, in this environment, change in Q is a simple tug-of-war between increasing precipitation event depth (alpha) and increasing active soil depth (Zr), with both Ep and Lambda of little importance.

This is a valid point. There are different approaches to compute sensitivity of discharge to climate and different routes can be taken. This includes a) the direct computation (alluded to by the reviewer), b) the derivation of analytical climate sensitivities (approach followed in the paper), c) the intercomparison between calibrated models of many basins across a consistent geographic area, d) the empirical estimates of impacts from historic changes in a basin, and e) the establishment of empirical relationships between runoff, temperature, and precipitation for basins in comparable hydroclimatic zones[1]. Our approach was inspired by the paper by Roderick and Farquhar which analytically defined the climate sensitivity coefficients for a steady-state Budyko model[2]. We believe that this approach is best suited to identify and attribute expected changes in water availability to key hydrological model factors determining it, given the limited data availability in the region under consideration.

The reviewer describes the relevance of the individual sensitivity coefficients as tug-of-water between increasing alpha on the one hand and w0 (Zr) on the other, with the other sensitivity coefficients of little importance. This is a very nice way to put things and we believe that the discussion in our manuscript should be extended along these lines in discussion in Chapter 3.2 Quantification of Climate Impacts. With these results at hand, we should also more clearly point out that a 30-years climate time scale might be short when viewed from the perspective of typical time scales over which rooting depths and soils change over a larger domain. In other words, the relative importance of changes in w0 might be much more damped as, increasing the relative importance of the other sensitivity indices accordingly. We suggest adding this point also to our discussion.

The caption for Figure 15 is wrong. According to the map key, these are "relative changes in" precipitation frequency and depth.

This is correct and we suggest editing the caption of the Figure to reflect the fact that these are relative changes.

Figures B2-4 show the same parameter, Zr, but with three different ranges on the same continuous rainbow colour scale. They cannot be easily visually compared and require a non-linear bin-style scale that is the same for all three. Continuous colour ramps are most useful in remote sensing applications particularly, where the individual items being coloured are relatively small, the same shape and size, and there are very many of them (the false shaded DEM under the figures is a perfect example). For only 221 irregularly shaped, sized

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[1] Sankarasubramanian, Vogel, and Limbrunner, "Climate Elasticity of Streamflow in the United States."
[2] Roderick and Farquhar, "A Simple Framework for Relating Variations in Runoff to Variations in Climatic Conditions and Catchment Properties."

and arranged polygons this is wasted. I would also suggest moving Figure B5 to follow B1 as they are the saturation and wilting points of the soil, or potentially just show a single map with their difference which is the input (s1-sw) to w0.

These are two very good suggestions. We thus suggest recoloring the current Figures B2-B4 accordingly so that they become comparable and to move B5 to become the new B2.

Perhaps the most important follow-on, and unfortunately it may no longer be a simple analytic solution, is to consider inter-annual variation (see eg. Zhang et al, 2008 using a Budyko framework). It may be possible to remain analytic on an annual basis with storage change zero over a year, but storage carryover between quarterly or monthly sub-annual intervals in a matrix equation. From the human perspective, it is not only important to know how much more flow we can expect but also when. If the water is used for irrigation of crops but will arrive in a different month in the future, then which crops are most appropriate is an issue. If most of the future flow is concentrated over a short period, then this could lead to flooding or loss of opportunity for use in irrigation or domestic water consumption. This also has implications for management with regard to storage in-stream via dams, off-stream with other engineered structures, or some form of managed aquifer recharge to mitigate flow seasonality.

Zhang L, Potter N, Hickel K, Zhang Y and Shao Q (2008) Water balance modeling over variable time scales based on the Budyko framework – Model development and testing. Journal of Hydrology, 360, 117-131, doi: 10.1016/j.jhydrol.2008.07.021

Yes, the reviewer brings up a very important point, one that we also tried to allude to in the current text in the paragraph starting on line 585. We propose to extend the discussion in the main text of the article along the lines suggested here and to also cite the suggested reference to the Zhang et al. 2008 paper. The mismatch in timing of water availability versus supplies in selected regions was already brought up by Siegfried et al. (2012) and remains an important future issue in the region and elsewhere. This is even more true under the currently observed deglaciation and the associated loss of water storage in the form of ice in high mountain Central Asia (see lines 585 and following).

The reason that we had decided to put our focus on changes in the mean annual water balance in the first place was the following. Out of the total of 299 station throughout the region that we could localize, time series discharge data from only 135 gauging stations was available, i.e., from less than half of the entire set of stations. Hence, focusing only on these, we would have had to abandon the truly regional approach pursued here and work with much less data to understand climate impacts on the seasonality of rivers. We believe however that our study can serve as a first diagnostic for providing estimates of changes in the mean water balance and then guide more detailed studies on a per subcatchment basis.

We hope that the replies to the reviewer’s comments/suggestions are satisfactory to her/him.

Kind regards, Tobias Siegfried (on behalf of all the co-authors).