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General comments

This paper proposes to analyze the joint effect of glacier retreat and revegetation (due to climate warming) on the overall water balance of glacier-covered catchments for long term evolution (up to 500 years into the future). It does so with a simplified model whose possible outcomes are studied for different glacier retreat and revegetation scenarios, for two different climate types. The studied climates are continental and maritime climates, which are emulated by adjusting the glacier mass balance rate with elevation according to observed rates in these climates. No actual data is used in the presented study but the model parameters are selected in light of known /reasonable values for existing glacier catchments.

The idea of studying the possible evolution of catchment-scale water balance resulting from climate warming with a simplified model is appealing; it has the potential to explain in simple terms the possible outcomes (temporal increase of total basin runoff, overall decrease on the long run) without obscuring the involved mechanisms by a complex input-output model. In its current form, the results of the analysis are however hardly surprising and essentially say that "with more vegetation we get less runoff", which corresponds to an oversimplification of high alpine hydrology.

Thank you for your comments and careful review. Although some of the results may not be surprising, we feel that previous literature has not systematically analyzed the parameters influencing annual runoff, and thus our results provide a simple framework for understanding variations in runoff that should be relevant to a broad range of researchers and resource managers. As pointed out in this review and in the other reviews, additional complexity could be added to the model that would produce positive and/or negative feedbacks, but that would not change the general results from this study. In the introduction, we highlight that we are trying to understand (1) how a suite of fundamental parameters (glacier slope, climate regime, etc.) control the shape of the long-term annual runoff curves and (2) how sensitive the runoff curves are to changes in these parameters.

I am a hydrologist by training, with little knowledge in ice flow modelling. From my perspective, the used one-dimensional, depth- and width-integrated flow model, combined with different glacier mass balance rates seems to be a reasonable approach to generate different glacier retreat scenarios under climate warming. I find it, however, surprising that the authors choose an approach that does not allow to study the effect of the actual glacier shape (here a simple rectangle has been chosen) and that this aspect is not further discussed.

The rate of glacier volume change, which drives variations in glacier runoff over secular time-scales, is governed by two feedbacks: a negative feedback with glacier length and a positive feedback with glacier surface elevation. These feedbacks are well captured by simple flow line models, although it is correct that spatial variations in glacier width will modify the glacier evolution. We tested the model sensitivity to glacier width by using a trapezoidal basin (in map view) whose width varies from the ice divide to the terminus by ± 5 degrees. These variations have limited effect on peak runoff (~1%) and changes in time to peak and end runoff were easy to predict. Essentially, all other things equal, glaciers with large accumulation areas have higher end runoffs due to the smaller fractional area change and a slower decrease to end runoff. The large accumulation areas provide some buffer against climate warming as long as the glacier does not fully disappear. In the manuscript we now motivate our choice of using a parallel-sided valley and discuss how variable glacier width might affect the variations in runoff.

Regarding the hydrological side of the study, I have to admit that as I hydrologist I can only warn against the use of such oversimplified assumptions without sufficient discussion of the implications. To actually study the fundamental controls on the high alpine water balance, these fundamental controls and what we know thereof should be reviewed in detail before building a model.

My critic is the following: The parameterization of the effect of colonization is summarized by ***two simple assumptions****: "First, we assume that the catchment becomes increasingly vegetated following deglaciation and that the type of vegetation only depends on time since deglaciation. Second, as areas of the catchment become colonized, the rate at which water is evapotranspired increases until reaching a maximum value representative of the climax vegetation state."* ***While the first assumption seems reasonable*** *(some references would certainly be useful), ...*

The first assumption is based on the time since deglaciation being highly correlated with vegetation types, biomass, and cover (Crocker and Major, 1955; Burga et al., 2010; Chapin et al., 1994; Klaar et al., 2015; Whelan and Bach, 2017; Fickert et al., 2017; Wietrzyk et al., 2018). The assumption does not include any variations in vegetation regrowth with altitude, which have been shown to affect vegetation growth rates primarily through its influence on air temperature (Cowie et al., 2014; Whelan and Bach, 2017). Yet, succession rates have been shown to be comparable at different altitudes throughout glacier recession as changes in air temperature with altitude are offset by climate warming (Fickert et al., 2017). We have added the following citations to the manuscript, and thank you for the suggestion.

Crocker, R. L. and Major, J.: Soil Development in Relation to Vegetation and Surface Age at Glacier Bay, Alaska, J. Ecol., 43, 427–448, 1955.

Burga, C. A., Krüsi, B., Egli, M., Wernli, M., Elsener, S., Ziefle, M., Fischer, T., and Mavris, C.: Plant succession and soil development on the foreland of the Morteratsch glacier (Pontresina, Switzerland): Straight forward or chaotic?, Flora, 205, 561–576, https://doi.org/10.1016/j.flora.2009.10.001, 2010.

Chapin, F. S., Walker, L. R., Fastie, C. L., and Sharman, L. C.: Mechanisms of Primary Succession Following Deglaciation at Glacier Bay, Alaska, Ecological Monographs, 64, 149–175, https://doi.org/10.2307/2937039, https://esajournals.onlinelibrary.wiley.com/doi/abs/ 10.2307/2937039, 1994.

Klaar, M. J., Kidd, C., Malone, E., Bartlett, R., Pinay, G., Chapin, F. S., and Milner, A.: Vegetation succession in deglaciated landscapes: implications for sediment and landscape stability, Earth Surface Processes and Landforms, 40, 1088–1100, https://doi.org/10.1002/esp.3691, 2015.

Whelan, P. and Bach, A. J.: Retreating Glaciers, Incipient Soils, Emerging Forests: 100 Years of Landscape Change on Mount Baker,
Washington, USA, Annals of the American Association of Geographers, 107, 336–349, https://doi.org/10.1080/24694452.2016.1235480, 2017.

Fickert, T., Grüninger, F., and Damm, B.: Klebelsberg revisited: did primary succession of plants in glacier forelands a century ago differ from today?, Alpine Botany, 127, 17–29, https://doi.org/10.1007/s00035-016-0179-1, 2017.

Wietrzyk, P., Rola, K., Osyczka, P., Nicia, P., Szyman´ski, W., and We˛grzyn, M.: The relationships between soil chemical properties
and vegetation succession in the aspect of changes of distance from the glacier forehead and time elapsed after glacier retreat in the Irenebreen foreland (NW Svalbard), Plant and Soil, 428, 195–211, https://doi.org/10.1007/ s11104-018-3660-3, 2018.

Cowie, N. M., Moore, R. D., and Hassan, M. A.: Effects of glacial retreat on proglacial streams and riparian zones in the Coast and North Cascade Mountains, Earth Surface Processes and Landforms, 39, 351–365, https://doi.org/10.1002/esp.3453, 2014.

... the second assumption omits an important body of hydrological literature of the effect of vegetation on the water balance, and in particular the effect of forest (e.g. Andreassian, 2004). Forests show typically increased ET fluxes during younger states as compared to the climax state.

Whether the typical vegetation succession to be expected in glacier catchments leads to a continuous ET increase with vegetation cover increase, remains to be demonstrated. I am not aware of literature on this topic (but it might well exist of course). In general the evolution of hydrological / geomorphological / pedological processes in moraines (and related runoff processes) can be assumed to be still largely unknown (see an ongoing project description here: http://gepris.dfg.de/gepris/projekt/318089487?language=en).

The second assumption is that as areas of the catchment become colonized and vegetation biomass increases, the amount of precipitation that does not contribute to runoff on an annual scale, ET, increases until reaching a maximum value representative of the climax vegetation state. The assumption is based on a general understanding of the relationship between biomass, vegetation cover and decreased basin runoff. A variety of processes are expected to cause annual ET to increase including: increases in vegetation biomass, type, percentage cover, and temperature (Jaramillo et al., 2018; Andréassian, 2004; Barnett et al., 2005), yet as the reviewer rightly points out there are few studies on changes in evapotranspiration throughout vegetation succession following deglaciation. Results for non-glaciated paired watershed studies show a clear decrease in annual basin runoff moving from the time of initial reforestation to the establishment of climax forest (Andréassian, 2004; Filoso et al., 2017). Changes in evapotranspiration rates through the transition period from initial reforestation to climax state in non-glaciated basins is variable. Some studies show approximately monotonic decreases in annual basin runoff from reforestation to climax forest (Andréassian, 2004, and references within; see there Fig. 8). However, others show a non-linear decrease in basin runoff after deforestation, with younger states having higher evapotranspiration rates than climax state (Andréassian (2004), and references within; see there Fig. 9). Thus, there are two scenarios and the debate between them continues, either evapotranspiration on newly revegetated land is lowest at first and progressively increases until climax state or evapotranspiration is initially lowest and increases rapidly before decreasing and stabilizing above deforestation levels at climax state. These conflicting results have been explained as particular to different species of tree with the latter, non-linear increase in evapotranspiration, measured primarily for eucalyptus trees (Andréassian, 2004).

Our modeling is of plant growth in a previously deglaciated basin, where transitions in evapotranspiration have yet to be extensively studied. However, based on evidence for the first assumption we can assume that vegetation biomass, types, and cover are all increasing with time since deglaciation. There are multiple studies showing that increased biomass and reforestation leads to higher levels of evapotranspiration and decreased annual basin runoff (Sun et al., 2010; Klaar et al., 2015; Jaramillo et al., 2018; Bosch and Hewlett, 1982; Andréassian, 2004). In our general modeling we choose to model evapotranspiration as monotonically increasing in a stepwise manner throughout vegetation growth for the following reasons. First, we are attempting to study general basin characteristics so exceptions to general rules (e.g., eucalyptus trees) are of less importance. Second, the step wise increase in ET allows us to focus on specific stages of vegetation and not the exact transition between stages which is less well understood; most studies show an eventual increase in ET and interception after vegetation reaches a climax state (Andréassian, 2004). These two assumptions provide the basis for our landscape modeling throughout glacier recession. We have more clearly delineated the justification for these assumptions in the methods.

Jaramillo, F., Cory, N., Arheimer, B., Laudon, H., van der Velde, Y., Hasper, T. B., Teutschbein, C., and Uddling, J.: Dominant effect of increasing forest biomass on evapotranspiration: interpretations of movement in Budyko space, Hydrology and Earth System Sciences, 22, 567–580, https://doi.org/10.5194/hess-22-567-2018, 2018.

Andréassian, V.: Waters and forests: from historical controversy to scientific debate, Journal of Hydrology, 291, 1 – 27, https://doi.org/https://doi.org/10.1016/j.jhydrol.2003.12.015, 2004.

Barnett, T. P., Adam, J. C., and Lettenmaier, D. P.: Potential impacts of a warming climate on water availability in snow-dominated regions, Nature, 438, 303 EP –, https://doi.org/10.1038/nature04141, 2005.

Sun, G., Noormets, A., Gavazzi, M., McNulty, S., Chen, J., Domec, J.-C., King, J., Amatya, D., and Skaggs, R.: Energy and water balance of two contrasting loblolly pine plantations on the lower coastal plain of North Carolina, USA, Forest Ecology and Management, 259, 1299 – 1310, https://doi.org/https://doi.org/10.1016/j.foreco.2009.09.016, 2010.

Bosch, J. and Hewlett, J.: A review of catchment experiments to determine the effect of vegetation changes on water yield and evapotranspiration, Journal of Hydrology, 55, 3 – 23, https://doi.org/https://doi.org/10.1016/0022-1694(82)90117-2, 1982.

I do think that the approach is interesting. The hydrological assumptions should however be a bit more elaborate, including good references for glacier catchments and a detailed review of what we know today about the evolution of the water balance of newly vegetated areas in such catchments. If no sufficient literature can be found, possible hypotheses should be discussed in detail. This literature review should also include the important ongoing discussion what the effect of decreases in snow to rainfall ratios has on the catchment-scale water balance (Berghuijs et al., 2014). The relative decrease of snowfall might significantly contribute to the reduce of basin-scale runoff (add to the effect of vegetation).

We have found equivocal studies on the impact of changes from snow to rainfall on annual streamflow. Basins in southeast Alaska show strong seasonal changes but no discernible trend in annual streamflow from moving from snow-dominated to rain-dominated climate regimes over 20 year climate oscillations (Neal et al., 2002). A review of studies in the western United States found no clear trend in how mean annual streamflow responded to changes in precipitation phase across different basins (Tague and Dugger, 2010). Finally, a study of non-glaciated basins across North America suggests that a change in phase of precipitation from snow to rainfall results in larger interannual variability, and lower mean annual streamflow (Berghuijs et al., 2014). These differing results do not allow for the determination of a simple modeling parameter to include for changes in annual runoff associated with changing precipitation regime in a glaciated basin. We briefly justify why we choose to not include the effect of changing from snow to rain in our modelling of annual runoff for glaciated basins. We also mention the possible effect of the alternative hypothesis and how it affects our results.

Neal, E., Walter, M. T., and Coffeen, C.: Linking the pacific decadal oscillation to seasonal stream discharge patterns in Southeast Alaska, Journal of Hydrology, 263, 188 – 197, https://doi.org/https://doi.org/10.1016/S0022-1694(02)00058-6, 2002.

Tague, C. and Dugger, A. L.: Ecohydrology and Climate Change in the Mountains of the Western USA – A Review of Research and Opportunities, Geography Compass, 4, 1648–1663, https://doi.org/10.1111/j.1749-8198.2010.00400.x, 2010.

Berghuijs, W. R., Woods, R. A., and Hrachowitz, M.: A precipitation shift from snow towards rain leads to a decrease in streamflow Nature Climate Change, 4, 583–586, 2014.

Similarly, a topic that should be discussed (even if not included in the analysis) is the interaction between glacier retreat and groundwater recharge. Not much is known so far about this topic but glacier retreat might change the relative amount of water that is available to vegetation in the non-glaciated part.

Thank you for this suggestion. Reviewer 3 also pointed out a number of processes that we neglected in our model. In response to those comments, we briefly discuss how processes such as groundwater recharge might modify our model results for annual basin runoff. We note that changes in glacier mass balance have been shown to affect groundwater recharge, however the impact to basin runoff is seen more strongly at seasonal rather than the annual timescales we are modeling (e.g., Liljedahl et al., 2017).

Liljedahl, A.K., A. Gädeke, S. O'Neel, T.A. Gatesman, and T.A. Douglas (2017), Glacierized headwater streams as aquifer recharge corridors, subarctic Alaska, *Geophys. Res. Lett.*, 44, 6875-6885, doi:10.1002/2017GL07383.

To summarize, to increase the value of this study, I suggest a good literature review of the impact of glacier retreat and the associated reduction of snow- to rainfall ratio on the water balance of high alpine catchments. Based on this, key processes and their synergy and possible unknowns should be identified. Based on this, the hydrological model can either be kept as is (but with more realistic future scenarios) or be refined. At the very least, the hydrological simplifications should be more explicitly discussed.

We acknowledge that there are limitations to the assumptions that we made for both the landscape and glacier models. These assumptions were made due to either a desire to understand a few fundamental parameters that influence basin runoff or a lack of consensus on various processes. We prefer not to add additional model complexity at this point and chose to focus on the key processes/parameters. In the revised manuscript we added justification for our chosen model parameters and also discuss how some of the parameters identified by the reviewer that were not included in our model, such as the snowfall to rainfall ratio and changes in ET, may affect trends in runoff (see also response to reviewer #3).

Detail comments:

- *Regarding the future ET fluxes, the reference to a paper that studied forest versus crop / pasture across the globe in non-mountain environments (Zhang et al., 2001) is probably not adequate.*

This issue was also raised by reviewer #3, who suggested a number of additional studies that we have now included in the paper.

- *The concept of **"runoff ratio"** is an engineering concept that was developed to separate precipitation into surface runoff and infiltration at the event scale (e.g. for the application of the so-called rational formula). What is used in this model is the **"annual runoff ratio"**, which is the ratio between total basin runoff and the total incoming precipitation. The total basin runoff is the sum of direct surface runoff and fast and slow subsurface runoff processes (and not the "runoff over an area of land"; the latter are the result of soil – vegetation interactions and groundwater recharge / release processes. This should be clear to avoid confusion for non- hydrologists.*

Thanks. We have clarified this in our revision.

- *the conclusion should give clear indications about what should be explored on the hydrological side (not just the glaciological side)*

We agree, and we have addressed model limitations in more detail in the manuscript (discussed above and in response to reviewer #3).