

## ***Interactive comment on “Quantifying present and future glacier melt-water contribution to runoff in a Central Himalayan river basin” by M. Prasch et al.***

**Anonymous Referee #1**

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General comments:

This is an impressive study from an equally impressive larger project (BRAH-MATWINN), with meritorious goals of modeling changing hydrology through an entire catchment and distinguishing contributions from various components, including explicitly changing glacier storage. The Lhasa River Basin (LRB) is an important study site, too. This region has drawn attention as a site of climate change impact, since it features large populations downstream and large concentrations of ice (glaciers, permafrost) upstream. Observations are sparse, glacier climate science is controversial, and the future implications have been poorly cited (i.e. IPCC FAR). The authors state (multiple times) that this motivates them to not only understand future climate change impacts close to glaciers, but also where people “live and use” the water further downstream.

C2613

Notably their PROMET model is a spatially distributed, process-based (using physics to solve energy/mass balance) integration that does not rely on close calibration and parameterization. This is excellent work, and the authors should be congratulated.

However, the merits of this paper should be reviewed not on the overall project or modeling framework, because such has already been established and published elsewhere. Here, the authors claim to present a coupled glacier (SURGES)-climate downscaling (SCALMET)-hydrology (PROMET) model-based “analysis of the temporal dynamics and spatial pattern of the rainfall, snow- and ice-melt contribution to river runoff under past and future climatic conditions for the Lhasa River basin LRB. . .to account for the specific and unique role of the glaciers for the downstream regions.”

But in fact, how much of the results presented here are novel?

This paper actually reports on a previously published hydrologic modeling experiment; the model results (both “past” 1970-2000 and “future” scenarios A1B1, A2, B1 through 2080) for the LRB have already been published for the Upper Brahmaputra River Basin (UBRB); many of the figures in this paper are re-printed (or slightly altered) from Prasch et al., 2011a, Adv. Sci. Tech (<http://www.adv-sci-res.net/7/61/2011/asr-7-61-2011.pdf>).

Presumably the authors justify this additional publication as a report on details of the model verification in order to make further comments upon the implications for the glacier melt contributions (or actually lack thereof) to surface runoff in the LRB. But the authors should explicitly mention this overlap, and clarify exactly how this paper is distinct up front in the Introduction; as written, the information on the scientific context is not given until the opening paragraph of Part 4. This should be moved forward and amplified to clarify the contributions of this work, and how it is actually different.

Results: Authors do not see a major change in % ice melt given future scenarios of altered (warmer) climate, except in most glacierized sub-catchment. They imply this is “astounding” which may be overstating the significance. Glaciers at this scale are a very minor component. It is known that they have a temporary capacity to increase

C2614

flow, and decrease variability, but that this changes as the ice reservoir is depleted. Here, the rate of reservoir reduction is offset by the increase in ice melt area. This underscores the importance of being able to dynamically model ice flux, and also account for the historical progression of the hydrograph rather than 30 yr averages to account for interannual variability, and perhaps groundwater residence time. Nevertheless, it is a compelling conclusion and the authors make a strong case for more careful empirical observations of hydrological fluxes in this (and other) glacierized headwater region.

Also, it is critical to establish parameters of “successful” model application given lack of data. Perhaps because the model is only physics-based (and not parameterized), the model performance metrics are a bit low (especially for Table 2, where Nash-Sutcliffe efficiencies, the cited quality criteria, are <0.50 for full monthly values).

Presuming sufficient justification for novel material, this is a nicely written paper. What follows are some more specific comments on some aspects that were not clear and a list of specific technical edits to assist minor revisions.

#### Specific comments

In the Intro, the authors also elaborate the utility of their approach to the problem of making meaningful predictions of future hydrology in the face of changing climate and a dearth of monitoring. Other theoretical work that has considered the nature of glacier storage release and transient impact (buffering) to streamflow seems important to mention (e.g. Collins and Taylor, 1990; Jansson et al., 2003; Huss et al., 2008; Moore et al., 2009). Also, the timing and magnitude of the expected glacier melt ‘peak’ in discharge is important to diagnose for water resources in changing mountain environments. Other research should be noted, particularly the integration of modeling and observations by Baraer et al., 2012 in the Andes, a region that is predicted to be highly vulnerable to changes in ice mass.

The glacier model: The glacier model is presented as explicitly solving subgrid scale processes, but the paper is not very forthcoming on details, and tends to cite work

C2615

from obscure publications for details. The glacier model seems able to extrapolate temp, wet-bulb temp, pressure and wind at sub-grid scale resolution but what scale is this? How is it determined? Is it SRTM? Yet, it uses the 1x1km grid for precipitation and radiation. This seems to overgeneralize the most important potentially heterogeneous variables. There is no glacier flow component, and the authors do acknowledge and discuss these limitations well. They describe a snow metamorphism component vis-à-vis changes in albedo. But how is albedo represented, and how sensitive is this parameter?

“Since snow that accumulates at the higher elevation levels is transformed to ice as explained above, it does not accumulate endlessly” This needs more explanation. So mass is not transferred down slope; rather, melt is instantly redirected to the stream flow from all cells (?)

Likewise, in lieu of a dynamic ice flow model, “glacier geometry is adjusted both in the case of melt out or growth of the ice reservoir on different elevation levels in reducing or respectively increasing glacier area.” This is not clear how all this works; what does Fig. 4B and C actually show? There is no demonstration of the glacier geometry “adjustment”. . . is this a forced re-shaping to fit valley morphology?

It would be important to show how this glacier model compares to actual glacier area evolution over the historical time span.

Since the future conditions are thus mostly a temperature signal, then this model will reach a sudden threshold and instantly release melt. This appears to be what happens in the scenarios.

Detailed edits:

Fig. 1: mention that the sub-basins are labeled (presuming they are, and not the met stations?).

Interestingly, the pour point to Brahmaputra River is ungagged, so there is no ground

C2616

truth (?). There is a gage at Lhasa, but the upper glacierized catchment of Yang-baijing is hydrologically disconnected to Lhasa.

P4560, L25: “until 2000” is not as clear; does this mean from 1970-2000?

P4561 (last paragraph of section): re-write. “While the basin is only 2% glacierized as of 1970, it was selected for study because. . .”

It would add to continuity of the paper to describe what data are available at this point, since readers have already been directed to location Fig. 1 that includes symbols for met and discharge (what sensors, length of record?).

P4562 L3-8: too long; break into 2 sentences.

P4562, L27: this paragraph is unclear, and too wordy.

P4565, L18: change to “vanishes”

P4570, L24: should be evaporates

Fig. 4: this is a replicate from Prasch et al., 2011a:

Figs. 6 and 7: the Lhasa data are identical to Prasch et al., 2011a.

Fig. 10 is just the fraction of snow/precip that is already published in Fig. 12 of Prasch et al., 2011a.

Table 1: some redundancy in observed variables reported; can this be consolidated?

Table 5: clarify this is modeled output

Table 7: Lhasa in 2011-2040: is 242 correct or typo? This table is not clear from caption; use “/” in explanation of values.

Table 8: this is complex, and re-iterates the values seen in previous tables. Are all necessary? Either way, the caption that explains that the diameter of the red/orange circles should be clarified; it accounts for “precip plus ice storage changes” but this is

C2617

awkward; is it not actually “precip + storage change + ET” to be inclusive of GW? Isn't the while/“bluish” circle recording the ice storage change?

References cited:

Baraer et al. (2012) *J of Glac* 58 (207).

Huss M et al. (2008) *Hydrol. Process.*, 22(19), 3888–3902.

Jansson et al. (2003) *Journal of Hydrology* 282, 116-129.

Moore RD et al. (2009) *Hydrol. Process.* 23(1), 42–61.

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Interactive comment on *The Cryosphere Discuss.*, 6, 4557, 2012.

C2618