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We would like to thank the authors of the short comment for taking the time to provide this feedback to us. We respectfully disagree to most arguments, but evidently we are pleased to take this opportunity to rebut your points and reply to your feedback.

General

The discussion paper presents an analysis of precipitation estimation by inverse precipitation-stream flow modeling, aimed at proving that a) precipitation gauged by valley stations and b) TRMM remote sensing estimates of precipitation for the Upper Indus Basin (UIB) grossly underestimate actual precipitation.

What we do in the paper is to inversely model high altitude precipitation using the glacier mass balance and we validate our findings by making a first order estimate of streamflow, which we compare to observed records on an annual basis. This is a novel approach and the concept has been successfully tested (and published) at a smaller scale for the Hunza basin [*Immerzeel et al.*, 2012]**. So, we do not perform inverse precipitation-streamflow modelling as suggested.**

As an alternative to TRMM and gauged data, the authors use ERA-interim and MERRA reanalysis products to derive basin-wide mean annual precipitation. The products are artificially corrected, whereby closure of the basin-scale mean annual mass balance equation Q=P-ET+MB serves as a constraint.

This is not correct. In our approach we use the APHRODITES precipitation dataset as a basis and we correct this dataset using precipitation gradients and a presumed elevation of peak precipitation based on published relationships between precipitation and elevation (see caption Table 1). We constrain these precipitation gradients based on the glacier mass balance trends [*Kääb et al.*, 2012a].We validate our finding using streamflow observations after correction with ET. To account for the large uncertainty in ET we use four different ET products (all published) and the MERRA product is one of those.

Losses to groundwater and buffer effects due to longer residence times of water in alluvial deposits (generally composed of silt, sand and gravel) are neither addressed nor mentioned.

We correct precipitation in areas above 2000 m asl, where the terrain is general steep, soils are shallow and the abundance of extended areas with alluvial deposits is limited. We have assumed that over the observed period from 2003 until 2007 there is no net loss or gain of groundwater in the upper Indus basin. We do acknowledge that groundwater may play an important role in the hydrology. A study in the Himalaya in Nepal shows that fractured basement aquifers play an important role. They fill during the monsoon and they purge in the post-monsoon thus causing a natural delay in runoff of a few months [Andermann et al., 2012]. However this does not imply significant net gains or losses over multiple year periods, which is what we consider. Interesting to note here is that the authors themselves also assume a negligible net groundwater flux on an annual timescale ([*Reggiani and Rientjes*, 2014]. We agree it is an important topic and in the revised manuscript we will add a discussion related to role of groundwater and the potential additional uncertainty it may cause.

The discharge Q is the observed long-term mean annual stream flows for various sub-catchments, ET is estimated from reanalysis data or an energy balance model, while glacier mass balance accounting (MB) is based on ICESAT satellite altimetry (25 sqkm resolution).

ICESAT is a space-borne laser altimeter and it provides point measurements of surface height in tracks. These data were processed into regional trends in glacier mass balance and the approach is published in Nature [*Kääb et al.*, 2012a]. So it is by no means a gridded dataset at 25 km^2 resolution.

In the inverse model, precipitation P is considered as the dependent variable. The analysis window is 2003-2007. The verification of the mass balance closure is achieved by means of a grid-based distributed hydrological model (PCGLOB) (1 sqkm grid resolution, daily time step), which estimates net precipitation (P-ET) and contains glacier mass balance accounting (MB) with the aim to reproduce observed flows (Q) at a series of observation points.

We do not use the hydrological model PCR-GLOBWB, which we think the authors are referring to, but we use the corrected precipitation, the ensemble average of four evapotranspiration products and the glacier mass balance to make a first order estimate of average annual runoff. We compare this estimate with observation as an independent validation.

From modeling and an uncertainty analysis in which several precipitation correction model parameters are drawn by Monte Carlo analysis, it is concluded, that the mean annual precipitation over the basin must equate 913±323mm/year. This value is approximately a factor three higher than the estimates stated in several earlier publications (Immerzeel et al. 2009, 2010; Bookhagen and Burbank 2010).

That is correct. Understanding the water balance at large of a complex basin such as the upper Indus which lacks direct observations has been a quest of many scientists and slowly but steadily progress has been made including our present study and previous work [*Immerzeel et al.*, 2009, 2010, 2012, 2013, 2014; *Pellicciotti et al.*, 2012; *Ragettli et al.*, 2013; *Lutz et al.*, 2014] and the work of authors [*Mukhopadhyay and Khan*, 2014a, 2014b; *Reggiani and Rientjes*, 2014]. To our opinion new insights should be allowed in science and that is in fact how progress is made. The comments should be directed to this particular paper being under review and not at papers that have already passed a rigorous process of peer-review by themselves.

Actual evaporation is estimated as an average of four widely disparate products, including ERA Interim evaporation (i), MERRA reanalysis evaporation (ii), an estimate using an energy balance model (iii) and an estimate computed by PCGLOB via soil moisture accounting (iv). The average value and spread between the four products is 359±107mm/yr. In the works by Immerzeel et al. (2009, 2010) and Bookhagen and Burbank (2010) evaporation is neglected.

Yes, one of the things we have learned in previous years is that actual ET (and possibly sublimation at high altitude even more) may play a significant role in the water balance. However, actual ET is notoriously difficult to measure and even if there are point measurements available they are by no means representative to the entire upper Indus. Therefore we have decided to use the ensemble average of four different actual ET products which are all published in peer reviewed journals. We acknowledge these products are subject to uncertainty and the ensemble average ET for the upper Indus is 359 mm/yr and the spread is 107 mm/yr. We take this spread into account in our estimate of annual runoff used to validate our approach.

In their own water balance study in the upper Indus the authors used an average actual ET of 200 ± 100 mm/yr. They base this estimate on a paper by Bhutyani from 1999 [*Bhutiyani*, 1999] who estimates evapotranspiration over the Siachen glacier using an empirical formula which is only a function of air temperature and which is developed by the United Stated Geological Survey to estimate lake (!) evapotranspiration in the US (!). We therefore deem our approach much more suitable to estimate upper Indus actual ET.

The paper seems to be another attempt (e.g., Immerzeel et al. 2012a, 2013) to come up with more realistic results than those first published in Immerzeel et al. (2009), where a mass balance analysis of the UIB was performed using basin-average TRMM precipitation estimates of 300 mm/year for the 2001-2005 period to drive the SRM hydrological model (Martinec, 1975). From the modeling results at that time, the authors reached the conclusion that to close the mass balance at Besham Quila gauging station (upstream of the basin outlet at Tarbela Reservoir), where 460 mm/year is the observed long-term mean annual flow, the supplementary discharge required to close the water balance must come from non-renewable glacier wastage at a rate of 1% per year. The authors cited these results in another sequel article (Immerzeel et al., 2010). In Immerzeel et al. (2012b), the Indus basin was labelled as “hot spot” based on the 2010 findings, including the water supply perspective. In Immerzeel et al. (2009) actual evaporation as a forcing term is set to zero. If included, it would lead to a higher (and even more unrealistic) glacier melting rate to close the water balance.

The authors seem to provide comments here on a paper published in remote sensing of environment of 2009, so there is no immediate need for us to respond to this here, however we are happy to provide some context.

This 2009 paper was the first in a sequence to unravel the Indus water balance and it has been cited 127 times (Scopus) by numerous scientists working in this field. It was also the time when the Karakoram anomaly was still a revolutionary idea postulated by Ken Hewitt [*Hewitt*, 2005], but the idea made it only to the mainstream as late as 2012. It was also the period before the IPCC discussion on the Himalayan glaciers. At that time, we were one of the first to attempt to model the entire upper Indus using a simple hydrological model forced with TRMM precipitation and MODIS snow cover and validated by runoff. We noted indeed a mismatch between the total runoff and the total TRMM precipitation and in the paper we discuss two options in careful terms based on the knowledge available at that point in time: (i) the mismatch is caused by an underestimation of precipitation and (ii) it is caused by a negative glacier mass balance. Now, in 2015, we believe the first reason is the most plausible.

In our view, the discussion paper suffers from a series of conceptual shortcomings:

Firstly, the authors continue to look at a very short time window (2002-2007), ignoring longer, climatic, time scales. For instance, when the 50-year trend of the observed Indus flows at the inlet of Tarbela Reservoir, downstream of Besham Quila, is considered, it should have become outright apparent that flow data exhibit an essentially stable trend from 1961 to date, as indicated by Reggiani and Rientjes (2015) and Mukhopadhyay and Khan (2015a). Moreover, the cumulative reservoir inflow volumes at Tarbela for the 1999-2009 decade were actually 4% below the 1961-2009, 50-year average (see Table 2 in Reggiani and Rientjes, 2015), the same time window for which Kääb et al. (2012) estimated a non-renewable ice mass loss from ICESAT altimetry data equivalent to 231± 46 m3/s of mean annul discharge at Tarbela. This equivalent discharge is 10% higher than the observed long-term mean annual flow and casts doubts on the reliability of the satellite-based ice mass estimates for the UIB. As a result, one should question if the satellite-derived mass balance estimates can be considered and used as an estimator variable for glacier mass balance accounting, and as in this case, to derive inferences about precipitation.

The ICESAT altimetry data are an established means to assess trends in glacier mass balance ([*Kääb et al.*, 2012b, 2015]) and this is not a topic to be debated here. This specific observation seems to re-open a previous discussion. We are well aware of your discussion with Andreas Kääb about the brief communication in the Cryosphere regarding this topic (http://www.the-cryosphere-discuss.net/8/5857/2014/tcd-8-5857-2014-discussion.html) and we strongly support arguments provided by Andreas Kääb in his reply.

We constrain our precipitation correction by glacier mass balance observations from ICESAT which were only available for the 2002-2007 period. Andreas Kääb has computed the mass balance trends for the three zones we have considered (Himalaya, Hindu-Kush and Karakoram) with a similar to his Nature paper [*Kääb et al.*, 2012a]. Overall the mass balance trends over this period are slightly negative (see Table 1). The reason is that the Karakoram anomaly does not overlap significantly with the Indus basin boundary [*Kääb et al.*, 2015]. Moreover, we take into account a (considerable) uncertainty in the mass balances to estimate the uncertainty in our precipitation estimates (paragraph 2.1 and Table 1).

The authors base their argument on Tarbela flow which drains only about half of the upper Indus basin we consider in our study. The fact that observed flows (also subject to errors by the way) are stable does not contradict our findings. There are many factors influencing streamflow and a potential change in snow melt regime is the large unknown here.

Finally, even if the glacier mass balances were positive then still precipitation would be significantly underestimated in particular in the north-western part of the basin. The precipitation in the APHRODITES dataset (and other data sets as well) is simply too small to account for the large glacier systems found in the upper parts of the basin. That is our point.

Secondly, different studies have addressed the issue of estimating realistic precipitation and actual evaporation rates. For the Upper Indus Basin (UIB), a large number of gridded rainfall products have been examined. For instance Palazzi et al. (2013) and Reggiani Rientjes (2015) studied several precipitation reanalysis products showing that the basin-average precipitation in the UIB is indeed at least double the rates indicated by the TRMM 3B43 product in Immerzeel et al. (2009; 2010) and in the order of 675± 100 mm/yr, thus significantly higher than those recorded at valley stations (Archer and Fowler, 2004). Several studies with weather stations placed over limited periods at high altitudes have indicated that actual precipitation in the high altitude mountainous areas is significantly higher, reaching up to 2000 -3500 mm and higher of w.e. per year (e.g. Wake 1989, Cramer 2000, Kuhle 2005, Winiger 2005), to then decrease higher up, an already well-known phenomenon (see Fig. 8 in Mukhopadhyay Khan, 2014a).

This is exactly the point of our paper and based on our approach in this paper we estimate the basin precipitation to be 913 ± 323 mm/yr, which is indeed higher than the TRMM 3B43, APHRODITES and most other commonly used gridded products (see our introduction). The authors themselves estimate the basin precipitation to be 675 ± 100 mm/yr, but this is only upstream of Tarbela (about half of the area we consider). Considering the uncertainty margins our estimates do not differ significantly. We have used the work of Matthias Winiger and Ken Hewitt [*Winiger et al.*, 2005; *Hewitt*, 2007, 2011] to estimate values for the elevation of maximum precipitation. Both have decades of field experience in the region. Our final results match well with field observation of high altitude accumulation.

Also, estimates of actual evaporation are provided, which have been presented in literature based on few field experiments at highly glaciated mountain ranges including the Himalayas at large (Buthyani, 1999, Khattak et al., 2011) and valley-based stations (see Fig. 7 in Mukhopadhyay and Khan, 2014a). In particular, Buthyani (1999) indicated a mean annual total evaporation rate in the order of 200 mm/yr for Siachen glacier based on glacier mass balance. In the discussion paper the authors rely on i) gridded estimated actual evaporation with mean values which are at least a factor two higher than observed in glaciated areas in the Himalayas, ii) possibly inconsistent satellite-based glacier mass estimates, iii) and short-term flow records as independent variables to draw inferences about precipitation. The more robust approach would be to rely on evaporation and precipitation estimates and trends to infer on glacier mass balance. In this case, it would become apparent that satellite-derived mass balances are not sufficiently reliable to serve as support in inverse modeling of precipitation.

Most points have been covered already earlier in this reply.

The Khattak paper (2011) does not discuss ET, but only temperature, precipitation and stream flow. Buthyani (1999) provides an ET estimate for a single glacier based on an empirical formula developed to estimate lake evaporation in the US based on air temperature only and in Fig 7. of Mukhopadhyay and Khan (2014a) the Penman-Monteith reference ET is given for selected station. Reference ET is very different from actual ET and average values plotted here are about 2.5 mm/day, which is about 900 mm / year. This considered we believe that our approach to estimate basin wide ET is much preferred. The

points regrading satellite-based glacier mass estimates and stream flow records have been covered above.

Thirdly, the authors chose to ignore long-term observed flow time series. An inverse modeling attempt like the one proposed here, with multiple uncertain independent variables (i.e. ET, MB), cannot replace or serve as a substitute to any sound analysis of observed stream flow data.

We think it does, in particular when uncertainty are considered as rigorous as we do. Relying entirely on streamflow analysis will not solve this puzzle as ET, snow melt, glacier melt, sublimation, rain and groundwater dynamics all have their role in streamflow and isolating specific components from streamflow only is an ill-posed inverse problem that is impossible to solve.

Neither does an inverse steam flow modeling on a time window of half a decade convey a sense of confidence when conclusions need to be drawn on long-term, climate-controlled glacier mass storage.

We do not draw any conclusion on glacier storage, but only on glacier storage changes, which respond directly to the climate. Moreover, we do not apply inverse stream flow modeling as Reggiani et al. suggest. Instead, we use average stream flow estimates as a means to validate the inverse modeling based on glacier mass balance (with good results).

An analysis of longer flow records in space and time would provide considerably more insights into the mass balance of the basin than numerical modeling alone (in this context we recall that satellite-altimetry derived mass balance in glaciers in extreme topography (Kääb et al., 2012) is essentially an application of reflected electromagnetic wave signal interpretation, which has not undergone any thorough validation for the particular region yet). Rising trends of August flows in the central and eastern Karakoram imply decreasing glacial storage at rates of 0.553 – 0.645 mm/day/year and 0.186 – 0.217 mm/day/year in the Shigar and Shyok watersheds respectively, whereas in the western Karakoram (Hunza watershed) falling trend of August flows implies increasing glacial storage at a rate of 0.552 – 0.644 mm/day/year (Mukhopadhyay Khan, 2014b; Mukhopadhyay et al., 2014; Mukhopadhyay Khan, 2015b). Such rates should be reconciled with the precipitation trends to infer changes in the regionally- averaged glacier mass balance.

This has been covered several times before. In the revised version we will include a water balance estimate for the three regions we consider.

Fourth, the distribution of the various parameters in the uncertainty analysis of precipitation are assumed with a (log-)Gaussian distribution, which the authors have not demonstrated to relate to actual empirical distributions in the region that could in principle be quite different (e.g. bimodal, skewed, non-Gaussian etc.). The analysis only yields the uncertainty of their precipitation correction model which they have assumed and inserted into the model “a priori” based on values taken from the literature and not necessarily the actual uncertainty of precipitation, which is yet unknown. The precipitation uncertainty analysis pursued in this way is thus akin to a prediction that directly or indirectly causes itself to become true, by the very terms of the prophecy itself (Merton, 1948).

We respectfully disagree. There are 6 parameters which play a key role in our approach (HREF, HMAX, DDFd, DDFdf, TS, MB). The uncertainty in these parameters jointly determine the uncertainty in the precipitation gradient and the resulting precipitation field. We use log-Gaussian distributions for positively-values parameters and Gaussian if parameter values can also be negative. These are maximum entropy priors preferable if no additional information about the actual distributions is available. We base the parameter range on literature values (some collected during our ownfield campaigns) and we run a rigorous Monte Carlo simulation of 10,000 runs. This forward stochastic approach to uncertainty assessment is a well-accepted approach in science, if no direct information on output uncertainty is available.

The research and results presented in the paper do not provide relevant benefit towards understanding the hydrological balance in UIB. Findings on gridded precipitation and actual evaporation products are significantly higher than those shown in recent publications, whereas long-term streamflow analysis and aspects of glacier mass storage are not analyzed. The underlying assumption that the water balance can be closed by inversely estimating precipitation results in basin-average precipitation estimates that are likely overrated. Given the essentially stable (or statistically insignificant falling) long-term trend in observed stream flows at the basin outlet, the truly important scientific issue is not an estimation of the absolute value of the basin wide mean annual precipitation, which can hardly be achieved in this terrain, but validation of glacier mass loss estimates against the background of a hydrological balance of the basin and spatial patterns and trends in precipitation, as a function of summer and winter seasons. Such an analysis is needed to validate the mass balance of the glaciers and melting rates variously given in Immerzeel et al. (2009), Kääb et al. (2012) and Gardelle et al. (2012, 2013). Consequently, the discussion paper opens more questions than it provides answers, while the methodological approach does not contribute much of value in this respect.

We believe our work is an important step forward in understanding upper Indus hydrology and it provides a new independent estimate of high altitude precipitation using changes in glacier mass balances derived from ICESAT laser altimetry, which is a proven technique published in high quality scientific journals.

Many uncertainties remain until the upper Indus hydrology is understood entirely and this will be the challenge for the years ahead; snow melt dynamics, evapotranspiration, sublimation, high altitude atmospheric dynamics, monsoon vs. westerlies, groundwater. Once we have a better understanding of these processes we may be able to unravel trends in observed river flow.

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