Hydrol. Earth Syst. Sci. Discuss., 8, C463–C469, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C463/2011/

Hydrology and
Earth System
Sciences
Discussions

Interactive comment on "Estimation of future glaciation and runoff in the Tanimas basin, Eastern Pamirs" *by* W. Hagg et al.

W. Hagg et al.

hagg@lmu.de

Received and published: 15 March 2011

The authors thank Bettina Schaefli for her critical viewpoint, which allows us to defend our approach publicly. The referee states that climate impact studies often have "not much value" (referring to Blöschl and Montanari, 2010 in this context), which "probably" also holds for our contribution. We are convinced that this is not the case: Blöschl and Montanari (2010) indeed mention the whole set of problems which climate impact studies have to face (Hurst effect, problems with trend analyses, climate model uncertainty,. . .), but they only classify impact studies as "of little value" if the authors do not understand the reasons for the hydrological changes. Here, they are referring to complex physical models which become black box models if parameter values are not representative (Blöschl and Montanari, 2010). They suggest the use of simpler models

and this is exactly what we did. We do understand the reasons for the hydrological response. One major problem connected with climate scenarios is the high uncertainty of precipitation projections. For this reason, Blöschl and Montanari (2010) have "no confidence in rainfall-driven floods". A big advantage of our investigation area is that such events practically do not exist. Precipitation is very low in general and has its maximum in winter, when it exclusively falls as snow. Summer runoff is dominated by snow- and icemelt which are directly related to air temperature and belong to the "hard facts" on climate change according to Böhm (2008, cited in: Blöschl and Montanari, 2010). Despite their shortcomings, GCM outputs are widely used in climate change studies and have to be regarded as state-of-the-art. Anyhow, we should more clearly indicate the limitations of our approach and the uncertainty of climate models. As already mentioned in the reply to reviewer no. 1, it should be stressed that scenario-based projections are no predictions, but represent one possible future.

Bettina Schaefli mentions six drawbacks, which are addressed in the following:

1. The higher evaporation (and sublimation) in arid climates has impact on glacier melt since it represents an important energy sink. This effect has to be considered in an energy balance approach, but a degree day model can simulate melt (with differing degree day factors) in both humid and arid climates. Due to the fact that evaporation dominates throughout the year (and always reduces energy available for melt), melt rates in continental climates are even stronger correlated to air temperature than in maritime climates, where both condensation and evaporation occur. This is because air temperature is correlated with all terms of the energy balance except for latent fluxes, which can disturb this relation in both directions (Ohmura, 2001). As indicated on page 1517, ln. 14-15, potential evapotranspiration in the HBV-ETH model varies sinusoidally throughout the year with a minimum of zero on 1 February and a calibrated maximum on 1 August. This simple approach is applicable in head watersheds, where actual evapotranspiration is of minor quantitative importance for the water balance and it already proved a robust performance in Alpine and Central Asian catchments.

2. Changes in glacier surface are by far the most important land cover changes. The non-glaciated catchment area mainly consists of rocky terrain and high-mountain deserts. It is almost bare of vegetation. Few areas with mountain steppe allow livestock grazing, fodder production along rivers is negligible. In a warmer atmosphere, this pattern will not change to a degree which will influence evapotranspiration losses on a catchment scale.

3. Please see answer (1) to detailed comments.

4. The model was calibrated by runoff and proved good performance. We additionally took care that modeled glacier mass balances have reasonable values, although there exist only estimates but no measurements. We cannot see how we should check the other terms of the water balance. There are no measured values of basin precipitation or evapotranspiration. Remote sensing products are not available for the period with observed data.

5. An automatic calibration would be less time consuming, but it would neither be less subjective nor solve problems connected to equifinality and parameter uncertainty. Automatically generated parameter sets would deliver good results for differing value combinations and the subjective step would be to pick one of those. Parameter uncertainty can be reduced by multi criteria calibration. We included glacier mass balance into the calibration process, although only very rough estimates and no observational data were available. We never stated that parameter values could be different for a different period. Maybe this was misunderstood from the passage where we explain that for a shorter calibration period, the meteorological conditions of single years could influence parameter calibration.

6. The fact that daily runoff hydrographs can be reproduced by the model proves that the meteorological station is representative for the catchment to a satisfactory degree. This may be due to the low precipitation variation in the investigation area. The homogeneous weather patterns allow extrapolation of station data to large basins. In the

Alps, for instance, it would be impossible to drive a hydrological model in such a large catchment with meteorological information from one point only. It is our philosophy to use observational data if there is a meteo-station close by. In addition, there is no satellite derived precipitation data for the period with observed runoff (1980s). The cited references do not match our needs (in space and/or in time): Winsemius et al., 2009, Bookhagen and Burbank, 2009: Tropical Rainfall Measuring Mission (satellite launched in 1997) and CPC/Famine Early Warning System Daily Estimates (coverage 20°W – 55°E, 2003-present). What evapotranspiration is concerned: soil moisture in the HBV-ETH model is treated on a lumped basis and represents a mean humidity index for the whole catchment rather than a real value in nature. Evaporation losses depend on this mean humidity, which makes it impossible to relate modeled values to data derived from other sources. Furthermore, the suggested approach (Winsemius et al., 2008) is based on thermal-infrared imagery and would definitely fail on ice surfaces which cannot become warmer than 0°C. Even in tropical Africa, where the approach was developed, evaporation estimates are noisy, which restricts application to a "soft mode, for instance as a fuzzy measure of acceptability, combined with likelihood measures constructed from other information" (Winsemius et al., 2008).

Answers to detailed comments:

1. We apologize that we gave the wrong reference in the paper. Instead of Kuhn et al. (1989, 1993), we refer to Kuhn (1990), where the influence of different energy balance factors on the equilibrium line altitude is explained. Our parameterization is based on very simple steady state conditions and therefore, the approach of Kuhn (1990) is adequate for our purposes, because it considers the most important processes of the energy balance. We applied the methodology of Kuhn (1979, 1980) (see Fig. 1), which is the basis for the approach of Kuhn (1990). This formula gives an ELA increase of only 125 m, because effects of humidity and long-wave incoming radiation are not taken into account. Including these effects as proposed by Kuhn (1990) leads to the higher ELA-shift of 170 m. The most important processes in this context are the temperature lapse

rate (mainly in the ablation area) and the radiation balance. Since both the lapse rate and the radiation gradient do not differ significantly between maritime and continental settings, the most sensitive factor is the length of the ablation season (D). As proved by field data, the length of the ablation period on glaciers in the Alps is quite similar to those observed in the Pamirs (Pertzinger 1996, WGMS 2001). For these reasons, we believe that it is justified to use the value of 170 m °C-1 in this study. Deglaciation was calculated using the formula in Fig. 1 and applying mass changes of 3.37 m (+2.2°C) and 4.74 m (+3.1°C) for each glacier. The new glacier length was then automatically determined in a GIS and the new glacier area was adjusted manually for the whole glacier sample.

2. We forgot to mention the degree day factor for ice in the model description and also to provide its value in table 1 (the latter is also true for the parameter REXP). To account for the lower albedo of snow compared to ice, the degree day factor for snow is multiplied with the parameter RMULT as soon as snow disappears on glacier surfaces. The calibrated value of RMULT was 1.54, yielding a maximum degree day factor (on 1 July) for ice of 5.9 mm/(°C*d). On south facing slopes, melt is multiplied with REXP, which has a calibrated value of 1.25. This results in a maximum degree day factor for this exposition of 7.4 mm/(C°*d).

3. There is some information about precipitation seasonality on page 1513, ln. 5-6. Of course, adding a figure might help to demonstrate the aridity in summer.

4. More specific hints would be appreciated.

5. This will be corrected.

6. The model was calibrated on a daily time-step. Since seasonal water availability is easier to treat on a monthly time step it was just shown that monthly values of modeled and measured data are also in accordance. This is not necessarily the case, if the daily hydrographs have a systematic deviation in a certain period of the year, this will lead to differences in monthly values (even with relatively high Nash values). (Mean) annual

values are presented in Table 5 and prove that the water balance is closed. This is also true for the individual years, but we decided that giving the values for each year would not provide more insight.

7. We are aware that our study contains no new methodology. We believe that the strength of our contribution is that it provides results from an underreported and extremely important and scientifically, ecologically and politically interesting region.

References:

Kuhn, M.,: On the computation of heat transfer coefficients from energy-balance gradients on a glacier. J. Glaciol., 22, 263-272, 1979.

Kuhn, M.: Climate and Glaciers. IAHS Publ., 131, 3-20, 1980.

Kuhn, M.,: Energieaustausch Atmosphäre - Schnee und Eis. Schnee, Eis und Wasser der Alpen in einer wärmeren Atmosphäre. Versuchsanstalt für Wasserbau, Hydrologie, Glaziologie, ETH Zürich, Zürich, 21-32, 1990.

Ohmura, A.: Physical basis for the temperature-based melt-index method, J. Appl. Meteorol., 40, 753-761, 2001.

Pertziger, F.: Abramov Glacier Data Reference Book: Climate, Runoff, Mass Balance, SANIIGMI, Tashkent, Uzbekistan, 279 pp., 1996.

WGMS: Glacier Mass Balance Bulletin no. 5. Glacier Mass Balance Bulletin. IAHS(ICSI) / UNEP / UNESCO, World Glacier Monitoring Service, Zürich, 96 pp., 2001.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 1507, 2011.

$$\frac{\partial c}{\partial z}\Delta h + \delta c = \frac{D}{L_f}\left[\frac{\partial R}{\partial z}\Delta h + \delta R + \gamma\left(\frac{\partial T_a}{\partial z}\Delta h + \delta T_a\right)\right]$$

where $\partial c/\partial z = 1$ kg m^{-2} m^{-1}, $\delta c = 0$, $D = 100$ d, $L_f = 333.6$ KJ kg^{-1} , $\partial R/\partial z = 0$, $\delta R = 0$, $\partial T_a/\partial z = -0.006$°C m^{-1}, $\gamma =$ 1700 KJ m^{-2} d^{-1} °C^{-1}, $\delta T_a = 1$°C.

Fig. 1. Equation used to estimate the shift of the ELA, after Kuhn (1979, 1980).