General comment

We would like to thank the anonymous referees very much for the thorough review of our manuscript. We appreciate the efforts and very detailed review of our work, which have provided constructive comments that significantly improved the manuscript. As a result of the extensive revision, several aspects of the methodology have been improved with implications for both the results and conclusions, and we summarize them below. We reply to the comments of the reviewer separately on the next pages.

The changes in methodology were done as a result of the valuable suggestions for improvement of the methodology we received from both referees:

- We redid calculations using the Randolph Glacier Inventory version 2.0.
- We now take into account projected changes in temperature and precipitation at a monthly scale, using monthly delta change values, instead of annual average changes in temperature and precipitation.
- We now use a thirty-year time span for the climatic reference period, instead of ten years.
- We now derive updated glacier areas by applying volume-area scaling at an annual time step, instead of a monthly time step.
- We improved the parameter uncertainty analysis

These major changes in the methodology lead to differences in results and adjustment of the conclusions in the revised manuscript. The main conclusions from the revised manuscript are:

- The range in projections for the CMIP5 ensemble is larger than for the CMIP3 ensemble.
- The CMIP5 ensemble shows higher projections for winter temperatures compared to CMIP3 while summer temperature projections are similar for both ensembles.
- The CMIP5 ensemble shows higher precipitation projections for the summer months compared to CMIP3 ensemble, while precipitation projections for the winter months are similar for both ensembles.
- The CMIP5 ensemble leads to a slightly wider range in projected glacier extent compared to the CMIP3 ensemble.
- It is imperative to use a representative selection of climate models and emission scenarios that span the entire range of possible future climates in climate change impact studies.
- Climate change signals should be analysed at a seasonal scale, when used to assess the response of glaciers to the changes in climate.

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Anonymous Referee #1

SUMMARY

The paper presents a new methodology for computing glacier evolution at basin scale by using a sub-grid parameterization and volume-area scaling. Future scenarios for glacier evolution in the Amu Darya and Syr Darya basins (Central Asia) and the period 2010-2050 are carried out by forcing the model with both, CMIP4 and CMIP5 scenarios. In the current form, the paper is well organized and well written, but major problems subsist in the presentation of both the methodology and the results. The non-introduction of some variables which are used all of a sudden and the inconsistency in some part of the description makes the reading somewhat frustrating at times. Moreover, the concept of "uncertainty" needs apparently to be re-thought – the statement that assuming a given climatic scenario, the "estimated error in glacier extent" by 2050 is estimated as low as 4.1% appears a bit too optimistic (an euphemism for saying ridiculous, since that is even better as what the total glacierized area is known today…). I firmly believe that the ideas presented in this publication are both valid and of real interest, but at the current stage the description of them is certainly not ready for publication. I would encourage the authors to resubmit the paper after major revisions.

We appreciate the reviewer's major effort and the time spent for the thorough review of our manuscript. Furthermore, we appreciate the reviewer's constructive suggestions for improvement of the methodology and the manuscript and we have taken his suggestions very seriously and we believe the manuscript has improved significantly as a result. We address the reviewer's comments point wise below.

GENERAL COMMENTS
1) Simple things first: (a) If a variable, an abbreviation or an acronym is used, it has to be defined! And this has to happen in the text when it first appears, and not somewhere in the caption of a figure. (b) Before a paper is submitted, it is well worth to verify that all figures are referenced in the text at least once, and that the references are correct. (c) If the text points at a table for some particular value, this table should of course contain it. (d) If a value given in the text is shown in a figure as well, the two should be consistent.

1)
a) This has been corrected in the revised manuscript: all variables, abbreviations and acronyms are now defined at their first mentioning, leading to more clarity and easier reading.

b) The references to Figure 8 (Figure 7 in revised manuscript) were mistakenly substituted by references to Table 2. We corrected this in the revised manuscript.

c) This confusion is due to the same error as described for (b), and this has been corrected in the revised manuscript.

d) We have rechecked all numbers in the text and made sure they are consistent with the figures and tables.

2)
The methodology is insufficiently described, and at stages, the description appears contradictory. The most difficult thing to understand is at which scale the methodology finally operates: At a 1km grid-cell scale, or at the scale of basin average? Moreover, some criticism can be made from the conceptual point of view: Updating glacier area at monthly scale based on volume-area scaling and mass balance seems not very suitable, especially for small glaciers, since seasonal signal of the mass balance are then directly transferred to the glacier area.

This is a valuable comment, and we have made a consistent effort to address it. In the revised manuscript the 'Methods'-section has been rewritten. We provide a more extensive description of the steps taken and mention for each step explicitly at which scale it operates. Besides, we added examples of each step taken and improved the figures.

We acknowledge the reviewer's criticism regarding the volume-area scaling at a monthly scale. We have therefore modified our method and now only apply the volume-area scaling at an annual scale, as suggested by the reviewer. While we calculate the mass balance each month, we apply the volume-area scaling to derive a new glacier area each year at the beginning of a new glaciological year (in this study on October 1st, the approximate end of the melting season), to avoid the protrusion of the mass balance's seasonal signal to the glacier area.

3)
The understanding of "uncertainty" needs to be revisited. Currently it is mostly used for indicating the spread between results when the model is forced with different inputs. This assumes that (a) the model structure is perfectly capable of mimicking reality (which is certainly not the case), (b) the estimated parameter set is absolutely correct and constant in time (which is probably not the case either), and (c) the simplified climate scenarios reflect the future evolution of climate… The effort for assessing parameter uncertainty presented in Section 4.3. is intriguing, but, apparently, doesn't plays any role when the results are presented.

Uncertainty in climate change impact studies has different sources, including measurements errors, natural variability and model structure (Katz, 2002). Model predictions are affected by many uncertainties from various sources, among them the errors in model input (forcing) data, parameter uncertainties, the description of boundary and initial conditions, and the model structural deficiencies (Ajami et al., 2007). In the climate modelling community there has been an effort to identify the main sources of uncertainty in future climate, and in the revised manuscript we refer to some key studies in this field. Since the spread between GCMs has been identified as a major source of uncertainty, we consider this extensively.
In our study we focus on two sources of uncertainty:

1) Uncertainty stemming from the range in climate change projections
2) Uncertainty stemming from parameter uncertainty

There are other sources of uncertainty (examples of which are also mentioned by the reviewer in later comments). There is uncertainty in present glacier extent and volume, uncertainty in volume-area scaling, uncertainty in climate evolution, uncertainty in climatic forcing for the reference period, uncertainty in mass-balance time series, problems of extrapolating calibrated parameters over the whole region, capability of the models to actually mimic reality in light of the simplifications, assumptions and hypotheses made therein. We are aware of these uncertainties. However, in this study we are mostly focusing on the uncertainty in the projections for temperature and precipitation, which is referred to by the reviewer as "spread" in climate change scenarios. We do this because of the growing consensus in the climate modelling community that the spread in GCMs outputs can be the main source of uncertainty in the future climate (e.g. Hawkins and Sutton (2009, 2010)). Additionally we estimate uncertainty stemming from the model empirical parameter uncertainty, given that the glacier model we use relies on an empirical approach to calculate melt rates. We are aware that the method used to assess the evolution of the glacier cover is simple and as such affected by errors/simplifications, but we use it only for a comparison of the effects of the two ensembles on glacier responses. We included a paragraph discussing the other uncertainties, which are not assessed in this study in the 'Parametric uncertainty' section:

"Besides uncertainty in glacier extent as a result of the uncertainty in the climate change projections, the projected glacier changes are subject to other uncertainties. These include parametric uncertainty, uncertainty in present glacier extent and volume, uncertainty in the volume-area scaling, uncertainty in climate evolution, uncertainty in climatic forcing for the reference period, uncertainty in mass-balance time series and uncertainties stemming from simplifications and assumptions applied to the model."

Furthermore we added a paragraph to the 'Results & Discussion' section explaining the limitations of our methodology in light of the scale it is applied to:

“Given the limitations discussed above, we are aware that the glacier model used in this study is too coarse to reproduce the response of single glaciers and the complexity of processes involved. The model choice is imposed by the limited amount of data available and the large scale of our application. However, the model is suitable for our aim, i.e. to translate downscaled future climate scenarios into glacier response at the basin scale, and to assess how the spread and differences in the future climate scenarios transform into differences in glacier response.”

The reviewer mentions that our assessment of parameter uncertainty doesn’t play a role in the presentation of our results. Here, we disagree, as the uncertainty in the glacier extent projection stemming from parameter uncertainty is visualized in Figure 12 and also mentioned in the text on page 12705 and 12706, lines 28-29 and 1-3 respectively. For the revised manuscript we improved the parameter uncertainty analysis as we doubled the number of sampled parameters sets (from 25 to 50 sets), and increased the error range for the observed mass balance from one standard deviation to two standard deviations. We changed figure 12 and we now show for each of the GCM ensembles what the uncertainty in climate change projections means for the range of projections of decrease in glacier extent. We also show for the most extreme projections what the additional uncertainty is stemming from parameter uncertainty. In this way the total uncertainty both from the variability in climate change projections and from parameter uncertainty is visualized. This is also described in the revised manuscript and we added an explanation on which sources of uncertainty are taken into account and which are not. In fact, parametric uncertainty is important (8.6%) and we state this in the revised manuscript.

4)
The current “discussion” section is very “prosaic” and of rather low information density. It can definitively be condensed in the current content and maybe expanded with some topics which are currently missing – as the meaning of “uncertainty” for example…

We condensed the ‘Discussion’-section to model improvement issues, and merged it with the ‘Results’-section. We moved parts of the text to either the ‘Introduction’ or to the ‘Conclusions’, and we removed redundant parts of the text.

5)
Some sentences in the “conclusions” need to be removed as they either belong to the introduction or make claims about topics never analyzed in the manuscript. The conclusions should recapitulate the presented work. It is not the right place for speculations.

This has been revised following the reviewer’s suggestion.

6)
A general stylistic suggestion is to make the formulations of some sentences more “timeless”. As an example, the formulation “The latest climate change projections […] generated for the upcoming fifth assessment report […]” in the abstract, will be outdated at latest in one years’ time...

This has been revised as much as possible.

SPECIFIC COMMENTS
P 12692, L 13-15. Please revisit the sentence: (a) “estimate changes in glacier extent as a function of glacier size” is a pleonasm and (b) a “glacier mass balance model” per se is not sufficient for estimate[ing] changes in glacier extent” – for doing this some”sort of glacier dynamic model (or a parametrization thereof) is necessary.

We changed the sentence to “…we force a regionalized glacier mass balance model to estimate changes in the basin’s glacier extent as a function of the glacier size distribution in the basins and projected temperature and precipitation.”

P 12693, L 21. Maybe only a detail but I would avoid the wording "melt-water dominated rivers" when referring to Amu- and Syr Dary. Whilst it is well known that this rivers are (strongly) influenced by snow- and icemelt, no study exists so far to my knowledge which actually shows that the meltwater share is larger than 50%. In that sense I would prefer the wording "melt-water influenced rivers", that seems more "cautious" to me.

This has been revised as suggested.

P 12694, L 10. State a year to which the "present total glacierized area" refers to. It is not 2012, is it?

The data used is based on GLIMS data from 2003-2007 and Digital Chart of the World data which are older (from 1960 onwards). For the revised manuscript we based these numbers on the Randolph Glacier Inventory version 2.0 (RGI 2.0) (Arendt et al., 2012) dataset which for Central Asia is compiled of data from different sources gathered at different moments in time. We calculated the glacier covered areas using RGI 2.0 and provided the proper reference in the text. We rephrased the sentence into: "The total glacierized area is 10,289 km^2 (1.3% of total 799,261 km^2 basin area) in the Amu Darya basin and 1596 km^2 (0.14% of total 1,117,625 km^2 basin area) in the Syr Darya basin, as calculated from the Randolph Glacier Inventory version 2.0 (Arendt et al., 2012), which for Central Asia is a compilation of data acquired between 1960 and 2010."

P 12695, L 4-5. Calling a 10-year period a "climatic reference" is not admissible. "Climate"is defined over a period of at least 30 years. Changing the wording would be sufficient in principle, but the statement suggests that the interpretation of the results will be done against this reference! Although there is no argument for not doing so in principle,it is very misleading (and not correct) to "sell" this comparison as an assessment of "climate change"!! Please re-think on the interpretation of the results!

The 10-year climatic reference was indeed a shortcoming of our study. To address this we redid the analysis and generated a climatic reference dataset spanning 30 years (1978-2007). We used temperature data from Princeton's Global Meteorological Forcing Dataset (Sheffield et al., 2006) and precipitation from the APHRODITE dataset (Yatagai et al., 2012). 2007 is the last year of the reference period because the APHRODITE dataset ends after 2007 at the moment. We rewrote section 3.2 as follows:

"A dataset of precipitation and temperature of high spatial and temporal resolution spanning thirty years (1978-2007) is used as reference for the climate change assessment. For this period, we use the Asian Precipitation Highly-Resolved Observational Data Integration Towards Evaluation of Water Resources (APHRODITE, (Yatagai et al., 2012)) dataset for precipitation and Princeton's Global Meteorological Forcing Dataset (PGMFD, (Sheffield et al., 2006)) for temperature. APHRODITE is a long-term continental-scale high-resolution daily precipitation product based on a dense network of rain gauges, with spatial resolution of 0.25˚. The PGMFD was constructed by combining a suite of global observation-based datasets with the National Centers for Environmental Prediction–National Center for Atmospheric Research (NCEP–NCAR) reanalysis and it has a daily resolution and a spatial scale of 0.5˚. Daily precipitation data are bilinearly interpolated to 1 km resolution from the APHRODITE 0.25˚ gridded precipitation dataset. Gridded daily average near-surface air temperature data at 1 km resolution are obtained by bilinear interpolation of the PGMFD 0.5˚ gridded temperature dataset, which is subsequently corrected for elevation using the 1 km DEM and a vertical temperature lapse rate (Table 1)."

P 12695, L6. Please give more information on the "PERSIANN dataset" (since I've never come across that): Who collects this data? Who provides them? What do they contain? Where are they retrievable? Are they of public access?
P 12695, L 6-8. This is confusing: So "PERSIANN" is not a dataset (as claimed in the previous sentence) but a "neural network"? Please be consistent in the naming, and give the information requested above.

As we do not use the PERSIANN dataset in our new climatic reference dataset, we do not go into detail about this dataset. Yet, further information can be found in Hsu and Sorooshian (2009).

P 12696, L 6-9. These sentences are unclear, and further explanations are urgently required. What is the aim of the "sampling"? And what is "sampled" at all? What is happening after the "weighted percentiles according to the inverse number of simulations per scenario" have been computed? And why is the weighting important at all?

As stated in the text, all scenario's have an equal weight in our analysis. As the different scenarios contain different numbers of GCM runs, weighted percentiles are calculated.

P 12696, L 16. Please change the wording: The range stated in the sentence (i.e. 1.3 to 2.4 C) is not an "uncertainty" in the temperature projections, it is only the spread of individual model runs!

Please see our previous comment considering uncertainty. We moved this paragraph describing the differences between the CMIP3 and CMIP5 ensembles to the 'Results & Discussion' section, to focus more on the assessment of the climate change ensembles. However, we would like to note that this is the terminology used in several papers from the climate communities (see e.g. Hawkins and Sutton (2009), specifically for air temperature).

P 12696, L 13 - P 12697, L 2. Somewhere you need to state that all the figures presented here refer to annual values! It may be worth giving a short overview on how the changes spread across the year, since in the literature it has been pointed out several times that changes in both temperature and precipitation may differ throughout the individual seasons. In particular it would be interesting in this respect to have a comparison between CMIP3 and CMIP5 projections.

It is mentioned that these are annual values in the manuscript and that the numbers refer to the change over 60 years at P12696, L10. This is a very useful comment. Following the suggestion of the reviewer, in the revised manuscript we included a comparison of the distribution of delta change values for temperature and precipitation per month, for both the CMIP3 and the CMIP5 ensemble. This shows indeed interesting seasonal differences regarding the change in precipitation and temperature. An additional figure with box-whisker plots is added to illustrate this additional analysis (Figure 9). The seasonal variation is discussed in the updated 'Results & Discussion' section of the revised manuscript. We also use these monthly changes in the further analysis.

P 12697, L 5-10. I'm not completely sure but hasn't this updated version of the GLIMS data been included in the Randolph Glacier Inventory RGI (Arendt et al., 2012),? And hasn't the RGI improved upon the Digital Chart of the World (DCW)for the region of interest? I thought so… And by the way: If the DCW was used also by Raup et al. (2007), the figures of glacier area given at P 12694, L 10-12 definitively do not refer to "present"…

In the analysis we used the GLIMS dataset, but with updates provided by Tobias Bolch (as mentioned in the acknowledgements). We now redid the analysis with the RGI 2.0 updated with the glacier outlines provided by Tobias Bolch, as these updates are not incorporated in the original RGI 2.0 inventory. The updates include outlines for the large glacier systems in the Fedchenko glacier region, which are not available in RGI 2.0 as well as more accurate outlines for numerous other glaciers in the Pamir and Tien Shan mountain ranges. For Central Asia the RGI 2.0 is compiled of data from different sources acquired at different moments in time, including data from the DCW. We assume the RGI 2.0 dataset updated with the glacier outlines provided by Tobias Bolch to be representative for the situation at the end of our reference period (2007). This is to our knowledge the best dataset available at the moment. Section 3.4 has been rewritten:

"Glacier covered areas in the Amu and Syr Darya river basins are extracted from the Randolph Glacier Inventory version 2.0 (RGI 2.0) dataset (Arendt et al., 2012). For Central Asia this dataset is a compilation of data from different sources, acquired at different moments between 1960 and 2010. We updated the RGI 2.0 with more recently mapped glacier outlines provided by T. Bolch (see acknowledgements). The updates include outlines for the large glacier systems in the Fedchenko glacier region, which are not available in RGI 2.0 as well as more accurate outlines for numerous other glaciers in the Pamir and Tien Shan mountain ranges. We assume this compiled dataset of glacier extent to represent the glacier extent at the end of the reference period, and to form the starting point for the future simulations of glacier extent.

From this dataset with glacier extents, the size distribution of glaciers is extracted. Figure 2 shows the distribution of glacier sizes in the two basins. In the Amu Darya and Syr Darya river basins combined, 50% of the total glacier area consists of glaciers with a surface area smaller than 25 km^2 and 11% of the glacier area consists of glaciers smaller than 1 km^2. The median glacier size in the basin is 0.21 km^2. From this distribution 26 different glacier size classes are defined and used for further analysis (Figure 2).

The initial fractional glacier cover per 1 km grid cell is also extracted from the dataset with glacier extents, to be used as starting point for the glacier model simulations. Each 1 km grid cell of the 1 km DEM is assigned a fractional glacier cover varying from 0 (no glacier cover) to 1 (entirely covered with glaciers) (Figure 1).

The observed average annual mass balance in the region's mountains is approximately -0.47 m water equivalent (w.e.) between 1978 and 2007, based on five glaciers with mass balance records in the region (WGMS, 2011) (Table 2)."

P 12697, L 21-22. Well, I wouldn't call 5 glaciers "several" (and also "bench mark" should be removed: that's just what's available; it hasn't been chosen by any particular criteria - beside accessibility ;-)). Please state "five". Moreover, in the text, the mass balance is stated to refer to the period 2001-2010 whereas in Table 2 it refers to 1991-2010. Be consistent (and if you correct the Table to be consistent with the chosen "reference period", recompute the values from the original data!).

We have revised this (see rewritten paragraph above). Since we now use a longer climatic reference period for the revised manuscript, we also use mass balance observations covering this longer period, which are complete for the five glaciers initially used. These values are updated in the text and in the tables.

P 12698, L 12. I'm not sure if the statement "We repeat the reference period four times" (remove "(Sec. 3.2)", it is distracting at this point) is comprehensible for someone not dealing with "delta-change" methods. Maybe you can think of another formulation?

In the revised manuscript, we now select a random year from the 30-year reference period for each year in our projection and add the projected changes in temperature and precipitation to that particular year. We don't repeat the climatic reference period anymore as we did prior to changing the climatic reference dataset. This is also explained in the text of the revised manuscript.

P12698, L 18-20. This statement is very important, and should be highlighted previously. Better, however, would be to change the approach: Reducing the information of the CMIP3 and CMIP5 model runs to linear, annual delta change seems very "rude" since it neither exploits the available information adequately nor it is necessary. Unless you have a very strong argument for not doing it (and if so, you should state it) I urge you to follow an approach along the lines of what proposed by Bosshard et al., HESS, 2011, i.e. computing delta-changes on a daily scale. Moreover, it would be similarly easy to avoid the need of linearly interpolating the changes over a given period, since in principle, "deltas" can be computed for any 30-year period. The idea of computing the "deltas" in a running window of 30-years seems then almost obvious, and should therefore be done.

There is a tradeoff between the complexity of the downscaling method and the number of models that can be included. We choose to maximize the number of models we include. We do agree that an annual delta change is rather crude and to account for seasonal changes we now use monthly delta change data. In the revised version we still linearly interpolate the changes over 60 years, because we use the quantile approach and we do not downscale specific GCMs. This improvement leads to a significant change in the results induced by intra-annual differences in climate change projections.

P 12699, L 9. The "distribution of elevation" of what? I guess the elevation distribution of the glacierized surfaces within a 1km grid cell? Please state it clearly!

With "distribution of elevation" we mean the distribution of terrain elevation as derived from the 90 m DEM within a 1x1 km grid cell. We clarified this in the revised manuscript and consistently use 'terrain elevation instead of "distribution of elevation".

P 12699, L 10. What is F_G?!? I first thought the elevation distribution of the glacier, then the area of the glacier, and eventually, I found that it is meant to be the "fractional glacier cover". Please introduce this variable adequately.

This variable is the fractional glacier cover as introduced on page 12698, line 25. In the revised manuscript we changed the abbreviation from F_G to G_F as suggested by the reviewer in a later comment, to avoid confusion with the symbology used for the standard normal distribution in equations in the manuscript. In the remainder of this response letter we use G_F to refer to the fractional glacier cover.

P 12699, L 11-12. This is very unclear to me. In particular, the statement "assuming that […] the glacier distribution is proportional to the elevation distribution and glaciers occupy the highest (coldest) end of the elevation distribution" seems contradictory to me. In the first part, it sounds like the elevation distribution of the glaciers is simply obtained by multiplying the elevation distribution of the whole grid cell with the according fraction of glacierized area assigned to the grid-cell. In the second however, it seems that the area is just "filled" from top to bottom. As an example, let say the considered 1km-grid-cell is a inclined plane (i.e. the distribution of area with elevation is uniform) and the glacierized fraction is 0.5; would every elevation have half of the area glacier covered and the other half not, or would you assign "totally glacierized area" for the elevations above the mean elevation and "glacier free area" to the elevations below that (or even something else?)? I personally prefer the first option…

The area is indeed "filled" from top to bottom. In the mentioned example the glacierized area would indeed occupy the elevations above the mean elevation and the lower part of the elevation distribution would be free of glaciers. We definitely prefer this option above assigning all elevations within the grid cell with the fractional glacier cover. Although depending on the geomorphology within the grid cell, it is more likely that ice prevails in the higher (and colder) parts of the grid cell in general.

*P 12699, L 13-20. Ok, I probably got the rough idea, but the way it is formulated is not sufficiently clear, and I even believe the stated Eq.1 being not correct. Beside the fact that the chosen symbols in Eq. (1) are prone to create confusion (once you use "F" as cumulative pdf, as often in statistics, and once for defining a fraction…), what happens when H_GLAC is outside the range of the considered grid cell? This is going to happen very often (in particular if one considers that F_nˆ-1(etc.) becomes –inf and +inf for F_G equal 0 and 1, respectively), isn't it? And what is for F_G=0.5? Isn't what you want in this case H_GLAC=H_AVG? But in the way Eq.1 is formulated, F_G=0.5 would give H_GLAC=H_AVG+0.67*H_SD (since [in R notation] qnorm(1-0.5/2)=qnorm(0.75)=0.67), and that's probably not what you want…*

In the revised manuscript we changed the abbreviation of the fractional glacier cover from F_G to G_F to avoid confusion with the symbology used for the standard normal distribution in equations. It is true that H_{GLAC} cannot exceed the range of the elevation distribution within a grid cell. We now limit the function by the maximum elevation within a 1 km grid cell as derived from the 90m DEM. The problem mentioned by the reviewer of $F_N^{-1}\left(1-\frac{G_F}{2}\right)$ becoming –Inf for $G_F = 0$ does not occur as this calculation is only done for those grid cells with $G_F > 0$. These constraints have been added to the equations in the manuscript.

For $G_F = 0.5$ H_{GLAC} is not equal to H_{AVG}, but $H_{GLAC} = H_{AVG}$ when the fractional glacier cover equals 1. In that case the average terrain altitude in the grid cell is representative for the average elevation of the glaciers in the grid cell. This also implies that $F_N^{-1}\left(1-\frac{G_F}{2}\right)$ cannot become +Inf since G_F cannot be larger than 1. G_F = 0.5 would indeed yield that H_{GLAC} equals the 0.75 quantile of the elevation distribution within a cell, which is exactly what we want, since we fill the cell from the top.

P 12699, L 18-20: I'm not sure to understand this statement either: Does it means, that you assume one and the same "hypsometric curve" for all basins? I.e. do every 1 km grid-cell with elevation 4500m have a glacierized area of 45% (that's what I would say according to Fig. 7)? Probably not, in the sense that you certainly first discern between cells containing glaciers and not according to the glacier outlines, don't you? If you do so: State it. If you don't: you should!

We indeed assume one and the same hypsometric curve for the two basins. We stated this more clearly and repeatedly in the revised manuscript. It is also correct that we first discern between cells containing glaciers and not according to the glacier outlines. We stated this in the revised manuscript.

P 12699, L 22. Better refer to Fig.4 than to Sect.3.4.

This has been adjusted.

P 12699, L 23 - P 12700, L 2. This is inconsistent with previous definitions: at P12699 L6 you stated that H_GLAC is the "median glacier elevation in a 1 km grid cell", now you say that T_GLAC is "the representative air temperature for the mean elevation of the glacierized fraction of a 1 km grid cell", implying that H_GLAC is the mean elevation of the glacierized fraction… Choose one definition and stick to it. Moreover, state why Eq.(2) is required! Why is using T_AVG not sufficient? And perhaps more importantly: Why do you not compute T_GLAC directly from the station data, as you say you do with T_AVG (P12695 L11ff)?

We revised this consistently as follows: H_{GLAC} is the mean elevation of the fractional glacier cover in a 1 km grid cell. As we "fill" the grid cell's fractional glacier cover from the top, the representative temperature for the glacierized fraction (T_{GLAC}) is lower than T_{AVG} for $G_F < 1$. As we calculate the melt using T_{GLAC}, using T_{AVG} would result in an overestimation of melt rates, thus we calculate T_{GLAC}. We added the explanation for this in the revised manuscript. Computing T_{GLAC} from the temperature dataset or lapsing T_{AVG} from H_{AVG} to H_{GLAC} does not make a difference, since the same temperature lapse rate is used for both.

P 12700, L 3-11. Sorry, what? What is the averaging over the two basins good for? At this stage, it is just not comprehensible. Please give a hint of what you aim to do before you actually do it – it's a lot easier to understand then!

In the revised manuscript we wrote an extra paragraph at the beginning of section 4.2 to introduce the methodology used for the glacier model. In this paragraph we explicitly describe all steps taken and at which scale (e.g. grid cell, basin) the steps are done. In the revised manuscript we consistently provide the aim of a calculation step before describing the calculation itself.

P 12700, L 12-14. Please state that, if everything you do is correct, using" overline{T}_GLAC and overline{H}_GLAC" or "overline{T}_AVG and overline{H}_AVG " makes no difference (actually you could even use the average of the station measurements and altitudes…).

This does make a difference for grid cells with $G_F > 0$, as explained above.

P 12700, L 15-20. Honestly I don't see the need of introducing the concept of AAR: Why you don't simply state that accumulation and ablation area are discerned through H0 (accumulation area = glacierized areas abowe H0, and viceversa)? That's all you do, isn't it?

Although introducing the concept of *AAR* is not strictly necessary we believe it does add to the clarity of the text. We refer to the *AAR* more consistently throughout the manuscript.

P 12701, L 5-7. What do you mean by “composite DDF that includes the relative proportions of debris […] covered glaciers”?? Does it mean that you have two distinct DDF for debris-covered and debris-free glaciers, and that the DDF you are actually using is a (weighted?) mean of the two? More information is required! And remove either “relative” or “proportion”, these are synonyms.

The composite *DDF* is indeed a weighted mean of the *DDF* for debris-covered glaciers and debris-free glaciers. We added this explanation in the text.

P 12701, L 8. No! Positive degree days (which I firmly believe you mean with d_m, and if not, do so!!) are not the “NUMBER of days [..] with T>0C”!! It is the SUM of degrees for T>0, which is very different!

This has been revised and there was an error in Eq. 9. We adopted the formulation which is consistent with Radić and Hock (2011).

P 12701, L 15. Does this imply that the precipitation is uniform over the whole area? If not, what do you mean by “monthly precipitation”? The sum over the whole region? Why do you not use the sum of precipitation for altitudes above H_0, since at P12695 L7 you claim that you have a spatially distributed precipitation field?

The monthly precipitation is the average of the monthly precipitation sums for the grid cells with $G_F > 0$.

P 12701, L 18-19. Where are the coefficients in Eq. (8) coming from? In the publication you cite (Bahr et al., 1997) only an exponent is stated (and it is 1.36, not 1.375), but no factor (that what in Eq. (8) reads 0.12) . However, in Bahr et al. (1997), a reference to Macheret et al. (1988), reporting an exponent of 1.379 for some 103 glaciers in the Altai and Tien Shan mountain ranges is given. Maybe you are referring to that value? Since the publication is in Russian and not retrievable to me, still the question remains where “0.12” is coming from? Moreover, according to your coefficients, the units of V and A should be kmˆ3 and kmˆ2 respectively, and not mˆ3 and mˆ2…

The coefficients originate from Van de Wal and Wild (2001). We changed and improved the description of volume-area scaling in the revised manuscript. We use the formulation where the area is scaled to a mean glacier thickness, similar as in Huss and Farinotti (2012). In this formulation $h = c \cdot A^{\gamma}$, with *c* and *γ* being scaling parameters. We now use the same scaling parameters as Radić and Hock (2010) used for mountain glaciers.

P 12701, L 19-22. No! Please do not use volume-area scaling at monthly scale! In this way you end up by having shrinking glaciers during summer, and growing glaciers during winter, and that considering area! That is not what you want, do you? Especially for small glaciers this is absolute nonsense! If you stick to VA-scaling, please do it on a year by year basis (more fancy ideas, like removing seasonality from the monthly time series, seem not worth of pursuit to me…).

This is a valuable comment. We changed the model in that we now do volume-area-scaling on a year by year basis. The scaling is done in October for each year, at the approximate start of the glaciological year in the region. We have changed this in the revised manuscript.

P 12701, L 23. This sentence should be placed somewhere before introducing VAscaling. And state explicitly that the monthly mass balance is computed by C_m-A_m.

This has been corrected.

P 12702, L 2-4. I don’t understand this one… Why do you correct the temperature and not DDF? The whole idea behind temperature-index method is to calibrate these coefficients! My guess that your argument is, that DDF was calibrated previously for individual glaciers (that’s what you stated at P12701 L8-9), but remember that in general, factors calibrated for a given glacier and a given resolution, are not transferrable neither in space nor in scale. All kind of other uncertainties which you claim are offset *by correcting the temperature (L6-10), could be offset by DDF as well (keyword "equifinality"…). And by the way: Why do you call CorT a "parameter" at L 6? Is it a factor used for multiplying the temperature or something like that? Give an explanation how the correction is implemented.*

CorT is a factor correcting the temperature to fit the glacier model with the observed mass balance for the reference period. Our argument to correct temperature and not the *DDF* is indeed that *DDF* was calibrated previously in a related study. It was however not calibrated for individual glaciers, but for the two entire basins in a related hydrological study, covering the same area. In this sense, there is no transfer of *DDF* in space or scale. In the revised manuscript we added the statement that the related study for which *DDF* was calibrated covers the same study area. We further clarified how the correction is implemented in the manuscript.

P 12702, L 1. Later in the text it becomes clear (make it clear at this stage already!) that only the average mass balance for the available period is used for calibration. Why? Why do you "waste" the time series (by considering the average only) of records, if they are available?

We are interested in simulating the behavior of the glaciers as a result of climate perturbations at the basin scale. We do not model individual glaciers, and therefore we use an average mass balance for the five glaciers in calibration. This regionalization is justifiable over a longer period, but not at smaller time steps.

P 12702, L 10-13. Well, this figure looks not convincing to me at all: The AAR reported in Bahr et al, 1997 (actually a cross reference to Bahr, JG, 1997) refers to the 1980s-1990s, and is about 10% smaller than what your model says! Neither is a 10% difference in AAR a "good match" (actually that is more like a pretty massive difference, considering that you would never expect to find AARs above 60-70% for large regions!), nor would you expect an increase in AAR by the time you are referring to! Please reformulate the sentence!

We removed the comparison of *AAR*.

P 12702, L 19. There is no change in glacier area stated in Table 2! Did you intend to refer to any of the figures?

We intended to refer to figure 8 (Figure 7 in the revised manuscript) and this has been revised.

P 12702, L 20-21. Now I'm really confused: Was your approach not operating at 1km resolution already? That's what I understood at P12697 L17-20, P12699 L10-15, or P12699 L23-P12700-L1! What do you mean then at P12698 L5 when stating "[…] the 5 glacier model (operating on the 1 km scale)"? I urge you to revisit the whole "Methods" chapter for improving the clarity!

We rewrote the entire 'Methods' chapter and explicitly explain for each modeling step at which scale it is performed and what the aim of each step is. We improved the structure of the entire section, including the accompanying figures.

P 12702, L 23. Again, there is no data referring to 2011-2050 in Table 2 (and please check your references when submitting something the next time…).

This has been revised.

P 12702, L 26. Concerning "assuming that the glacier distribution is proportional to the distribution of elevation": Similar comment as for P 12699, L 11-12.

This has been revised.

P 12703, L 1-4. And what are c1 and c2? Moreover, give a reference for Eq (9), and give a hint for the need of a parameterization (in contrast to the exact solution).

We used an empirical approximation of the normal distribution because of software limitations in the previous version. We now use the exact solution in the methodology. Furthermore we added examples on how the equation is used.

P 12703, L 5-6. Has the ordinate in Fig. 9 been accidentally reversed? F_G is told to be the fractional glacier coverage, which for me means "F_G=1" = "total glacier cover". Why should a grid cell with "mean elevation = 4000m "have F_G=1 for H_GT=3400 and F_G=0 for H_GT=4800? I would expect exactly the opposite…

The ordinate has not been reversed. The purpose of this figure was to show how the fractional glacier cover changes more gradually for a grid cell with a small surface elevation distribution (small standard deviation) when H_{GT} shifts to higher elevations compared to a grid cell with a large surface elevation distribution (large standard deviation). In the revised version of the manuscript we decided to omit this figure.

P 12703, L 9-11. How can these values be read out of Fig. 10? According to Fig.10a and 10b, I would say that the three curves look almost identical in both panels, meaning that "P-30%, T+2" is equivalent to "P+0%, T+3", "P+0%, T+2" is equivalent to "P+0%, T+2", and "P+30%, T+2" is equivalent to "P+0%, T+1". It is unclear to me how one can deduce the equivalence between P+20% and T+1.. And by the way, place "Fig.10" before "panel A and B".

As the model sensitivity analysis is not contributing to the main message of the paper, and it mixes sensitivity to meteorological input, to glacier size and to melt parameters, we decided to remove this section and the accompanying figure in order to sharpen the paper's focus, which is also suggested by Anonymous Referee no. 2. The responses to the following four comments are thus less relevant as they refer to a section which is no longer included in the revised manuscript.

P 12703, L 19+20. Add "Fig.10" before both "panel C" and "Panel D".

The figure is not included in the revised manuscript.

P 12703, L 20-24. The aim of this observation is not very clear to me. Is it meant to provide a rationale for testing the sensitivity of DDF? If so, the albedo-argument is probably not the most important one: DDFs vary from glacier to glacier because of local meteorological and topographical conditions more than they vary because of albedo…

We agree that local and meteorological conditions are indeed important factors influencing melt rates. This section is no longer included in the revised manuscript.

P 12704, L 6. And what are DDF_CI and DDF_DC?? You never introduced them!

This section is no longer included in the revised manuscript.

P 12704, L 8.Rempve parenthesis before "." (it doesn't close anything).

This section is no longer included in the revised manuscript.

P 12704, L 12-15. I'm not convinced that I have understood the general idea: Your glacier response model (i.e. the VA-scaling kind of update) is driven by mass balance alone, isn't it? So why do you "use" a glacier mass balance (same page, L 12)? If you force the cumulative mass balance to be the same for different model runs, I very much would expect that at the end of the simulation, you would get the same variations in glacier volume. In any case you should get it if you approach is mass conserving, isn't it? Please explain what causes the model runs to have different trajectories.

As stated in the text, we do not force the cumulative mass balance to be the same for the different model runs. We make different parameter sets with different values for T_{lapse}, DDF_{CI}, DDF_{DC}, MB_{OBS} and, associated calibrated *CorT*. This explains that the model runs have different trajectories. Please note that we doubled the number of parameter sets from 25 to 50 and doubled the uncertainty range for the observed mass balance from 1 standard deviation (SD) to 2 SD for the revised manuscript to make the parameter uncertainty analysis more reliable.

P 12704, L 15-19. Please add a Figure showing the results of this experiment!
The results are shown in Figure 12. In the revised manuscript this figure has been updated, now showing the results for the 50th percentile (Q50) values of temperature and precipitation change, for the very warm (Q90) and very dry (Q10) case, for the very cold (Q10) and very wet (Q90) case, for the very warm (Q90) and very wet (Q90) case and for the very cold (Q90) and very dry (Q10) case. The uncertainty range is also shown for the two most extreme projections for both ensembles (CMIP3 and CMIP5).

P 12704, L 19-20. I don't completely agree with this sentence: The analysis carried out in this section gives a hint of the spread model parameter can have when still leading to the same result. Since the result itself is fixed a priori (again according to L12), the experiment doesn't add information on the "uncertainty" of the model simulation. In my opinion, the only way to tell about model uncertainty in this case, would be in a "classical" calibration-validation kind of scheme: Calibrate the model for a particular time period, compute the results for a second time period, and compare them to measurements you believe in. I'm aware that the available measurements are not very suitable (the only things you have are the mass balance series for some sparse glacier), but then, the only "honest" claim you can make is that you do not have the means of assessing the uncertainty…

In this study we assess the uncertainty stemming from uncertainty in climate change projections (our climate input) and parametric uncertainty. In this section we assess the parametric uncertainty. In the analysis the result is not fixed a priori as we allow the parameters (T_{lapse}, *DDF*, MB_{OBS}) to vary within prescribed uncertainty ranges. The *CorT* parameter is calibrated for each parameter set to mimic the observed mass balance over the reference period (with the observed mass balance being variable with a 2 SD uncertainty range in the uncertainty analysis). To assess the uncertainty associated with simplifications in the model indeed a calibration-validation scheme would be necessary. Unfortunately this is not possible. We do not model individual glaciers, so we cannot validate our modeling results in this way. Neither are glacier extent datasets available which cover multiple and fixed moments in time to do a validation in this way. There is a tradeoff between spatial scale and physical detail that can be included in a model. At this scale methods to assess future glacier evolution are scarce and many large scale hydrological studies deploy bold assumptions on how glaciers will develop in the future. We are therefore confident that our approach is an important step forward as glacial retreat is now a function of both precipitation and temperature projections with melt model parameters constrained by regionally averaged observed historical mass balance trends.

P 12704, L 25 - P 12705, L 4. I'm not sure what Fig. 11 is actually showing: Is each point in the figure the result of only one particular model run, in which a particular delta change in P and T has been assumed? Where does the uncertainty assessment of Sec 4.3 enters the game? Is it considered at all? And what kind of quantlies are used for the delta change? Are they derived by lumping all emission scenarios shown in Fig.2 and 3 (and discerning between CMIP3 and CMIP5)? And please state absolute numbers (and not quantiles) for the delta changes in Fig. 11: It is impossible to reconstruct them from Fig. 2 and 3!

In this figure each point is the result of a model run. So, this figure covers all combinations of ΔT (10th to 90th quantile) and ΔP (10th to 90th quantile). We included this figure as it allows to derive the relative importance of uncertainty in ΔT and ΔP for the projection of decrease in glacier extent. In this figure the parametric uncertainty is not included. However it is included in Figure 12. As we force the model with monthly delta change values and use quantiles consistently in the entire manuscript we don't use absolute numbers in Figure 11, but also quantiles. The absolute values can be derived on an annual basis from the box-whisker plots in Figure 3 (Figure 8 in the revised manuscript) and on a monthly basis from the new Figure 9 included in the revised manuscript.

P 12704, L 5-6. Please check the numbers (and do this for the whole manuscript!): According to Fig.11, I would say that for delta_T=q50 and delta_P=q10 the color states the range 46-48%, whereas in the text you say42%), and for delta_T=q50 and delta_P=q90 the result must be around 40% (and not 37% as in the text)…

The numbers quoted here are indeed incorrect. As the results have changed in the revised manuscript, the numbers and figure have changed also. We double-checked all numbers in the revised manuscript.

P 12705, L 14-16. Same comment as for P 12696, L 16: This is not an uncertainty range!

Please see our response to the reviewer's comment regarding uncertainty.

P 12705, L 20. What's "AR5"? Of course the "upcoming IPCC report", but you never defined it!

For consistency with the entire manuscript we changed the name to CMIP5, which we define in the Introduction.

P 12705, L 20-21. Ok, let's try this one: WHEN YOU TALK ABOUT SOMETHING YOU SHOULD DEFINE IT FIRST! Probably the "median case" is delta_T=q50 and delta_P=q50, right? But what is the "dry and warm case"?? Delta_T=75 and delta_P=q25? Delta_T=90 and delta_P=10?Or delta_T=97.5 and delta_p=X? It could just be anything!

This has been revised.

P 12705, L 8 - P 12706, L 3. I don't' believe that you believe the stated numbers being realistic: Event glacier area mapped from satellite imagery are commonly assumed to be precise at some 5% only. No-one will ever believe that with a very simplified approach as proposed the glacier extent in 40years time can be predicted with a "estimated error in glacier extent" of 4.1%. Please re-think what "error in glacier extent" should include (uncertainty in present glacier extent, including glacier volume, uncertainty in climate evolution, uncertainty in the data used for model calibration, including meteorological and mass-balance time series , problems of extrapolating calibrated parameters over the whole region, capability of the models of actually mimic reality, in light of the simplifications, assumptions, hypotheses made therein, etc…), and discern it very clearly from the concept of "spread in model results".

As explained in the response to the reviewer's comment regarding uncertainty, we estimate the uncertainty resulting from uncertainty in the climate change input and parametric uncertainty. The numbers stated here solely refer to the uncertainty in total glacier extent from parametric uncertainty. As mentioned in our earlier response we clarify in the revised manuscript which sources of uncertainty we include in our analysis and which sources of uncertainty we don't take into account. Regarding the parametric uncertainty, this uncertainty range is wider in the revised manuscript as we increased the uncertainty range for the observed mass balance.

P 12706, L 14. No. Ice flow models do not necessarily require "detailed knowledge of glacier velocities" – that's what they compute. Such datasets are rather very useful for validation…

To calculate the glacier velocities, detailed information of the glacier bed geometry and ice thickness distribution are required. These are not available for the study area. Even if so, it is not possible to model ice flow dynamics at the 1 km^2 spatial scale. We changed this sentence in the revised manuscript: "…require detailed knowledge of glacier bed geometry and ice thickness distribution."

P 12706, L 25. Well, in the publication you name, an analytic parameterization is proposed, which could theoretically be used without any "time series of high resolution DEMs"…

In the cited work, the Δh-parameterization is described as "a function relating the elevation of the glacier surface to the surface elevation change (equivalent to ice thickness change) occurring over a given time interval." For the areas studied the authors used DEMs derived from topographic maps and aerial photography to calibrate the parameterization. For the large study area in Central Asia we rely on the

SRTM DEM, which is not that accurate, and does not provide time series of DEMs. The authors also propose a Δh-parameterization for glaciers without multiple extent measurements in time. But at the same time the authors state: "the applicability of the Δh-parameterization outside of the European Alps is given, however, requires a recalibration based on repeated DEMs for very different glacier types."

P 12707, L 25. Consider citing the recent published work by Huss and Farinotti, JGR, 2012 as well.

We included citations to this work.

P 12706, L5 - P 12708, L13. In light of the rather minor density of information in the whole section, I would suggest to condense it. The text is well written at this stage, and almost reads like a "story" but it could be shortened significantly.

We condensed this section. We removed parts, and also moved parts to the 'Introduction'.

P 12708, L 15-18. Remove the first two statements: They belong to the introduction, and certainly not to the conclusions of the study, which did not covered this topics at all.

We moved these statements to the 'Introduction'.

P 12708, L 23-27. Somewhere you need to state that these figures refer to the change 2010 to 2050!

We added this statement.

P 12709, L 2-4. Why? Give an explanation.

We added explanation in the revised manuscript.

P 12709, L 6. Remove "as well as in terms of downstream water availability" – the study did not address the topic.

Although this is not addressed in the study, we believe it is important to make a link to the hydrological consequences, as this is what in the end will have implications for society, rather than the changing glacier extent itself.

STYLISTIC COMMENTS
P 12692, L 15-16. Try to avoid "model [-s / -ing]" 4 times in a single statement.

We changed the sentence to "This glacier mass balance model is specifically developed for implementation in (large scale) hydrological models, where the spatial resolution does not allow for simulating individual glaciers and data scarcity is an issue."

P 12692, L 19. Remove"the" before and "projections" after "CIMIP3"

We rephrased this.

P 12692, L 25. Consider "glacier evolution" instead of "glacier extent" (since the wording was already used in the same sentence)

We did this.

P 12693, L 3-4. Sort the references by year of publication.

We did this.

P 12693, L 4-5. Consider ", the reason being the lack of…“ instead of “. The underlying reason of this ongoing debate is the lack of…“

We reformulated this.

P 12694, L 2. Although the wording is “impressive”, I would remove the expression “geopolitically complex region”: It does not contain any information, as a definition of what “geopolitically complex” means is missing Merge the sentence with the following one, i.e. “The sources of the Amu Darya and Syr Darya rivers are located in the Pamir and Tien Shan mountains respectively…”.

We did this.

P 12694, L 17-19. This statement can be removed.

We did this.

P 12695, L 24: This sentence confirms that the wording “latest set of simulations” should be avoided: The data you used are one year old. I believe a couple of GCMruns have been done in the meanwhile ;-)

We changed P 12695, L 20 to: “We use the set of global climate change simulations which is used as basis for the upcoming fifth assessment report of the Intergovernmental Panel on Climate Change (IPCC), the CMIP5 multi-model ensemble.”

P 12696, L 2. Insert “,” between “report” and “is also”.

Done.

P 12696, L 19. What is “it”? Certainly the 90% and 10% quantiles, as in the previous sentence, but that is plural…

We reformulated the sentence.

P 12703, L 15-16. Consider "contribute substantially to the total ice volume in the basin“ instead of "contain substantial parts of the ice volume of the basin”

This section is not included in the revised manuscript.

P 12704, L 7. “Gaussian deviates”? You probably mean “normally distributed (random) variables”…

This has been corrected.

P 12705, L 5. Remove either “extent” or “retreat” in “glacier extent retreat”.

We removed extent.

P 12704, L 11-13. Please revisit you wording and be consistent in the use of adjectives: At P12702 L12 you claimed that a deviation in AAR by 10% is “a well match”, whereas now, a difference in total area by 5% is claimed to be “striking”…

We removed the sentence.

P 12708, L 23. This statement (i.e. higher warming = larger range of projections) is not correct as such (e.g. a (hypothetical and absurd) projection of a+20C warming would certainly make all glaciers melt completely. The range of the projection would then be simply 0). Thus remove “thus” ;-)

OK.

COMMENTS TO FIGURES

Fig. 2 (Fig. 3 in revised manuscript)*. Not sure if it is stated somewhere (but even if so, it may be worth repeating it in the caption): How are this values computed? Are they means of the GCM grid cells covering your region of interest? If so, how many grid-cells does it includes?*

In the text it is stated that these values are computed as means of GCM grid cells covering the study area. We added this also to the caption of the figure.

Fig. 3 (Fig. 8 in revised manuscript)*. Consider adding a vertical line for better dividing the CMIP4 and 5 scenarios.*

We added a vertical line to both plots.

Fig.4 (Fig. 2 in revised manuscript)*. It may be worth to state the actual number of glaciers in each bin on top of the individual bars.*

Ok, we added that information.

Fig. 6 (Fig. 5 in revised manuscript)*. H_SD should be H_AVG+H_SD (or an arrow as for F_G is required between H_AVG and the current position of H_SD).*

We have corrected that.

Fig. 8 (Fig. 7 in revised manuscript)*. This figure is never called in the text! Moreover: Label of left ordinate: "% of 2010", not "to". Label of right ordinate: remove "." after "m".*

The references to this figure were mistakenly replaced by references to Table 2. We have corrected this and changed the figure's labels.

Fig. 9 (not in revised manuscript)*. See comment for P 12703, L 5-6.*

Please see the reply to the earlier comment.

Fig. 10 (not in revised manuscript)*. What do you mean with "change in glacier extent in 2050"? Why 2050? The panel show a time series... Moreover, what do you mean by "baseline" properties? And "ceteris paribus" is probably not very common to most of the readers- me included…*

This figure will not be included in the revised manuscript. If it had been included, we would have removed "glacier extent in 2050" since the figure indeed shows the evolution of glacier extent from 2011 to 2050 and not just 2050. Ceteris paribus is used to state that the other parameters are unchanged when the effect of changing a parameter is investigated.

Fig. 11 (Fig. 11 in revised manuscript)*. Please state absolute numbers (and not quantiles) for the delta changes – it is impossible to reconstruct the values from Fig. 3!*

As we use the quantiles throughout the entire manuscript, we also state quantiles in this figure. Absolute numbers corresponding to the quantiles can be derived from Figure 3 on an annual scale and from a new figure (Figure 9) included in the revised manuscript monthly values can be derived.

Fig. 12. Please add labels at least to the ordinates of the bottom plots. For the upper plots, consider showing only an enlargement of one specific region. At the current scale of the plot, it is very hard to see something.

We made three separate figures for this. The first figure (Figure 12) is an extended version of the bottom panels and shows the decrease in total glacier area in the Amu Darya and Syr Darya basins for 2008-2050 based on the CMIP3 (left panel) and CMIP5 (right panel) model runs for the 50th percentile (Q50) values of temperature and precipitation change, for the very warm (Q90) and very dry (Q10) case, for the very cold (Q10) and very wet (Q90) case, for the very warm (Q90) and very wet (Q90) case , and for the very cold (Q90) and very dry (Q10) case. An error range is added to the two most extreme cases which is derived from the uncertainty analysis on critical model parameters and observed glacier mass balance. The second and third figure (Figure 13 and Figure 14) are similar to the upper panels and shows the projected fractional glacier cover in 2050 for selected areas in Central Asia, as suggested by the reviewer. We selected the Central Pamir and an area in the Tien Shan. We show the initial fractional glacier cover as derived from the Randolph Glacier Inventory 2.0 including updates. The figure has panels showing the simulated fractional glacier cover in 2050 for the CMIP5 model runs (left panel) and changes in fractional glacier cover with respect to the initial situation (right panel). One set of panels shows the fractional glacier cover for the 50th percentile (Q50) values of temperature and. Another set of panels shows the fractional glacier cover for the very warm (Q90) and very dry (Q10) case. A last set of panels shows the fractional glacier cover for the very cold (Q10) and very wet (Q90).

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