Key:

Bold: reviewer comments

Normal text: our replies

Blue: changes in the manuscript

Page and line references refer to the discussion paper

We applied minor changes to the phrasing of a few sentences from proof reading the manuscript, but these changes are not itemized here.

Response to reviewer 1, M. Rohrer:

General comments:

1) **As mentioned, I believe that this publication is very relevant for the climatology of the Mount Kenia region. All the same, I think it is important that also readers not extremely familiar with tropical glaciers and particularly Lewis Glacier and Mount Kenia should understand your results and its significance. So, I think for the interested public not specialized in tropical glaciers and this region it would be a good idea to explain a little bit more in detail, what are the peculiarities of your data and results, without compelling the reader to consult a plethora of related papers. Of course, this does not mean that all previous findings have to be incorporated in all details, but in condensed form these which are key to understand your present data and results.**

We did not apply major structural changes in the introduction, as we think we guide the reader from paragraph to paragraph deeper into the subject, the climate setting, the findings from nearby Kilimanjaro, the results from previous studies from Mt Kenya and the open questions finally yielding to the paper's objectives. However, we added some details to underline the peculiarities of the tropical climate, the findings from Kilimanjaro and our data. In addition, we structured the two possible interpretations of our results from this paper in two bullet-point sections in the conclusions to clarify the relevance and the implications of using strongly receded glaciers as climate proxies.

<u>Changes in the manuscript:</u>

P 3899, L 19ff were rephrased to: *In contrast to the thermal seasonality in the mid-latitudes, the annual cycle in the tropics is dominated by a hygric seasonality, expressed in the "long rains" (March to May) and the "short rains" (October to December), driven by the passage of the Intertropical Convergence Zone (e.g. Mutai and Ward, 2000) and modulated by sea surface temperatures in the Indian and Pacific Oceans (e.g. Yang et al., 2014a). During the recent decades precipitation variability in East Africa shows a drying trend especially in the long rains (Schmocker, 2013; Yang et al., 2014b) due to changing sea surface temperature patterns, which reasons are not completely understood yet and might be caused by both natural variability (Yang et al., 2014b) and/or anthropogenic origin (Williams and Funk, 2011). As a consequence of the tropical hygric seasonality and year-round warm air temperatures, tropical glaciers can experience ablation and accumulation at any time during the year, although accumulation tends to be concentrated in the regional wet seasons. This is in contrast to mid- and high-latitude glaciers where separate accumulation and ablation seasons are pronounced.*

P3889, L 24 was rephrased to: *While some initially speculated that glacier recession on Kilimanjaro is caused by rising local air temperature, subsequent physical, process-resolving studies – based on* *and evaluated with in-situ observations – revealed that glaciers on Kilimanjaro are most sensitive to changes in atmospheric moisture and precipitation…*

P 3890, L 28 was rephrased to: *Although pioneering for their time, 2.5 years of in-situ meteorological data is now available to allow a more rigorous assessment of the climate sensitivity of the glaciers here and…*

2) Moreover, you present some data which sound strange for readers who have been never on Lewis Glacier. Why is mean fresh water snow equivalent 350 kg/m3? Perhaps, solid precipitation on Lewis glacier is in most cases extremely wind pressed graupel or very wet snow or a similar peculiarity can be given as an explanation.

Density measurements of fresh snow on Mt Kenya are scarce. Those available are sampled graupel that ranged in density from 330-430 kg/m³. Sicart et al. (2002) report fresh snow densities around 300 kg/m³ for a tropical glacier in Bolivia. Although unusually dense, similar to old snow values at mid latitude mountain glaciers, this value is reinforced by energy balance modeling studies that established that model optimization also required high fresh snow density (Mölg et al., 2008; Nicholson et al., 2013). This anomalously high snow density could be explained by the frequency with which solid precipitation falls as graupel, the immediate snow aging due to radiative energy surplus even during snow fall events, and because snowfall can be wet at this site.

Changes in the manuscript:

We added a sentence to the caption of Table 2 on P 3916: *Fresh snow density was obtained by in-situ sampling and similar values around 300 kg m^{-3} were previously reported for topical mountains in Bolivia and Tanzania (Mölg et al., 2008; Sicart et al., 2002).*

We clarify that we just record solid precipitation by adding a paragraph on P 3892, L 16: *All-phase precipitation measurements are not available at the frequency required for the modelling, so in this study only solid precipitation determined from surface height changes recorded by a sonic ranger, and limited field measurements of snow density, are considered. Fresh snow density in the field was found to be in the range of 330-430 kg m^{-3}, presumably indicating that snowfall here is typically very wet and/or contains a high proportion of graupel. Although very high compared to higher latitude snow measurements, the fresh snow density of 315 kg m^{-3} used here was based on these measurements and model optimization, and is in line with other measurements at tropical glaciers (Sicart et al., 2002).*

3) Why is air temperature divider between snow and rain 4.5°C? Why so high?

This threshold of 4.5°C defines the temperature above no accumulation can occur because all precipitation is liquid. This value is based on measurements and is the highest air temperature when a positive surface height change was detected. (Note: We derive precipitation from quality checked (Nicholson et al., 2013) positive surface height change measurements thus, only the solid fraction is considered and we do not know if there was concurrent liquid precipitation. However, we have no evidence of rain at the altitude of the glacier, neither from the Mt Kenya National Park Rangers being up there daily, nor from personal communication with Stefan Hastenrath. Additionally, during more than ten field visits between 2009 and 2014 we never experienced liquid precipitation at the glacier altitude). We do not have concurrent all phase precipitation measurements at this time resolution to do a more rigorous assessment of the air temperature dependence of the precipitation phase change.

Changes in the manuscript:

We clarify that we just record solid precipitation by adding a paragraph on P 3892, L 16: *All-phase precipitation measurements are not available at the frequency required for the modelling, so in this study only solid precipitation determined from surface height changes recorded by a sonic ranger, and limited field measurements of snow density, are considered. Fresh snow density in the field was found to be in the range of 330-430 kg m^{-3}, presumably indicating that snowfall here is typically very wet and/or contains a high proportion of graupel. Although very high compared to higher latitude snow measurements, the fresh snow density of 315 kg m^{-3} used here was based on these measurements and model optimization, and is in line with other measurements at tropical glaciers (Sicart et al., 2002).*

We rephrased the parentheses on P 3903, L 6 to: *Although the MBM specifies that all precipitation is liquid above 4.5°C (which is the upper air temperature threshold at which solid precipitation was recorded at LG AWS), …*

4) Perhaps, naturally ventilated air temperature means sometimes almost not ventilated …and large amounts of reflected sunlight hitting the air temperature sensor… or so…

We addressed this issue in Nicholson et al. (2013, P 1207): *"Naturally ventilated air temperature measurements can suffer from radiative heating when insolation is high and wind speed is low and the sensor shielding is inadequate (e.g., Georges and Kaser, 2002), and in the case of combined temperature and relative humidity sensors, the membrane that protects the sensors from contamination further impedes natural ventilation. The Met21 radiation shield used at LG combines white outer lamina and black inner louvres that prevent direct solar radiation reaching the sensor from any direction. The manufacturers specification reports that comparative studies between this type of radiation shield and an aspirated sensor indicate that at wind speeds <1ms^{-1} with intense solar radiation, measured air temperature can be 0.5°C above that recorded by an artificially ventilated sensor. At LG, times with low wind speed coincide with overcast skies, and so in the absence of concurrent ventilated temperature and humidity measurements required to explicitly evaluate any impact of radiative heating on the sensor, we have assumed that the radiation shielding is adequate. Comparison of the 2°C-binned hourly air temperature from the Vaisala sensor with that of a Campbell Scientific 107 thermistor installed within the same radiation shield (Mölg et al., 2008) indicates that differences between the two sensors exceed the error margins of the sensors in only 0.02% cases, so measured temperatures appear unaffected by the sensor membrane."*

Changes in manuscript

We added a sentence referring to this specifically on P 3892, L 8: *Nicholson et al. (2013) demonstrated that the shielded, unventilated air temperatures and relative humidity measured at this site do not appear to suffer from distortions from solar heating of the sensors.*

5) Moreover, present, past and future Climatology of Mount Kenia is not really mentioned, except of a short indication that there is a 'long rain' and a 'short rain' season bringing accumulation and the sentence, that 'the East African glaciers capture a climate signal from atmospheric levels, between approximately 5 and 6 km a.s.l , where our knowledge of climate change is scarce and controversial. This is of course the case, on the other hand, there are long term precipitation measurements – especially on the agriculturally used slopes of Mount Kenia(!) - indicating that 'long rains' in MAM are generally diminishing since (at least) the 1950ies (e.g. Schmocker, 2013) with some acceleration since 1998 (e.g. Schmocker, 2013; Yang et al., 2014). In the Mount Kenia region 'short rains' in OND seem to be stable since the 1950ies with perhaps some slight upward trend since the 1998 climate shift (e.g. Schmocker, 2013). Additionally, it

could be a valuable information that it is not clear - for the time being - whether GCM precipitation projections are properly reflecting future precipitation development for Mount Kenia (e.g. Yang et al., 2014).

It seems to be clear that sea surface temperature patterns (SSTP) control precipitation variability over East Africa (EAPV). However, some issues are not finally solved and the complexity of the system not completely understood yet, because there are too many unknowns yielding to a large number of possible combinations of causes and effects each variable in space and time: What regional (coast vs inland, lowlands vs highlands) and/or seasonal (long rains vs short rains) impact on EAPV is governed by which regional source (Indian Ocean vs Pacific Ocean) and from which data sets (models vs models and vs observations) can we detect these impacts (if there are any) and what is the cause (natural variability vs anthropogenic forcing) of them? Elaborating this issue would become a paper on its own.

Specifically, in unpublished investigations we fail to establish correlations from the 18 years of Mt Kenya summit precipitation data (Hastenrath, 2005) to SSTP although those exist for the lower slopes of Mt Kenya, indicating different precipitation mechanisms for the lower slopes and the summit. Thus, we are careful in transferring results to other locations or regions and in favoring the causes of EAPV, neither the natural variability nor the anthropogenic forcing and we prefer to keep the changes in the text to a minimum and retain a neutral stance on this.

Changes in the manuscript:

We appreciate the suggested references and incorporate them in the introduction, P3889, L 19ff: *In contrast to the thermal seasonality in the mid-latitudes, the annual cycle in the tropics is dominated by a hygric seasonality, expressed in the "long rains" (March to May) and the "short rains" (October to December), driven by the passage of the Intertropical Convergence Zone (e.g. Mutai and Ward, 2000) and modulated by sea surface temperatures in the Indian and Pacific Oceans (e.g. Yang et al., 2014a). During the recent decades precipitation variability in East Africa shows a drying trend especially in the long rains (Schmocker, 2013; Yang et al., 2014b) due to changing sea surface temperature patterns, which reasons are not completely understood yet and might be caused by both natural variability (Yang et al., 2014b) and/or anthropogenic origin (Williams and Funk, 2011). As a consequence of the tropical hygric seasonality and year-round warm air temperatures, tropical glaciers can experience ablation and accumulation at any time during the year, although accumulation tends to be concentrated in the regional wet seasons. This is in contrast to mid- and high-latitude glaciers where separate accumulation and ablation seasons are pronounced.*

We rephrased the last sentence of the conclusions P 3907, L 7 to: *The large scale climatological cause of present day LG retreat, as well as of the shrinking ice on Kilimanjaro* (Mölg et al., 2009, 2013)*, is not completely understood yet and could be both natural variability and anthropogenic forcing. The first is identified in a recent study to cause the drying of East Africa especially in the long rains (Yang et al., 2014b), while for the latter a physical link from anthropogenic greenhouse gas and aerosol emissions to increased sea surface temperatures in the Indian Ocean and associated drying of East Africa has been established (Funk et al., 2008; Williams and Funk, 2011).*

Suggestions

Energy (EB) and Mass Balance (MB) Model: P 3892, L 26ff:

> **The authors refer to several other publications what the EB and MB modelling concerns. They mention also that measuring interval is 30 minutes. But does this mean that this is also the time step of the model for Lewis glacier is 30 minutes?**

As described in the paper, the AWS measurement interval is 1 minute and 30 minute averages are stored on the data logger. We did not mention, that the model time step is 1 hour for computational efficiency.

We added a sentence at the end of section 2.2, P 3894, L 12 to clarify this: *For this study all meteorological input data is provided from the AWS record and aggregated to the model time step of one hour for computational efficiency.*

Cloudiness: page 3894, Lines 17ff:

> **The authors mention several times that effective cloudiness (n eff) is an important input value, e. g. for longwave parametrization, but from where do they have this value? Is it measured, observed or parametrized in different way during day and night?**

We improved the phrasing of the first sentence in section 2.3, P 3894, L 14 to: *Cloudiness is a crucial factor governing the radiative fluxes but difficult to obtain. Thus, it is derived from parameterizing global radiation employing an approach from Budyko (1974)…*

Additionally we added a sentence on P 3895, L 22 to explain what happens during the night. *As* n_{eff} *is just defined during day-time, clear sky conditions are assumed during the night, which is feasible in this tropical environment where cloudiness, humidity and precipitation follow the pronounced daily cycle of convection.*

Fresh snow density: page 3900, Lines 23ff

> **Mean densities of 315 kg/m3 for fresh snow - or, as can be seen from table 2, the estimated value by Nicholson (2013) of even 370 kg/m3 - are unusually high. What do the authors understand by fresh snow? Snow which is measured immediately after falling? Even 315 kg/m3 for fresh snow seems to be very high, if comparing this value with long time average value of new snow, or even more so for 'fresh snow', e.g. in the Alps (Rohrer et al., 1994). Is this really snow? Or is perhaps more wind-packed graupel? As already mentioned, to give in this context some explanation would help the reader not having visited Mount Kenia so far to understand conditions on LG much better.**

Explained on P 2, point 2.

Snow/rain divider: page 3903, Lines 6ff

> **A value of 4.5°C as a snow-rain divider seems extraordinary high (e.g. L'Hote et al, 2005). How do you explain such a high value?**

Explained on P 2, point 3.

Sensitivity scenarios: page 3899, Lines 12ff

> **I find it is really a good idea to explore the sensitivity of a glacier mass balance with several mutually linked input variables. But for me, the information about how exactly these scenarios have been accessed is very sparse. It would be a good idea to say a little bit more about this. For**

example, if looking at table 3, it seems that there is no explicit precipitation scenario. What is exactly the reason for this and how are the other variables linked to the resulting precipitation (scenario)? In this context, the author's statement that the measurements are in-line with the recent decades TMPA and Era-I values is somewhat confusing to me. What is about the variable space in earlier decades or in a future world, how is the author's estimation about this? Do they have evidence that their 'scenario space' is valid for the whole period from 1930 to 2010 (for which exist glacier outlines), but as well for projected conditions up to 2100? I think this is an important information to the reader, as the authors state explicitly that a goal of this publication is to quantify climate change for a time period not covered by instrumental records.

We rephrased the first paragraph of section 2.6, P 3899, L 17 to better explain, how the scenarios were constructed and how their variable space is related to recent decades. However, we cannot make any estimate how the variable space of our scenarios relates to decades before 1980, as there is not enough observational or gridded data reflecting the summit conditions (also historical glacier extents are not useful parameters as we would need a mass balance). We don't want to speculate about the future, as the climate projections are much too coarse and are not intended to extract sub grid information for an isolated summit location.

As described by Nicholson et al. (2013) it is unlikely that a single climatic variable dominates the mass balance variability on LG. Thus, and because of the physical link between air temperature and vapour pressure (subsequently controlling the turbulent heat fluxes and the parametrizations of the radiative fluxes) exploring mass balance sensitivity of single climatic variable perturbation was rejected in favour of coupled perturbations reflecting the variability in the climate more comprehensively (Mölg et al., 2009a). To do this, alternative climate scenarios were constructed by resampling the AWS record on a diurnal basis in a manner similar to a simple weather generator (e.g. Hutchinson, 1987). Specifically, from the 773 days with meteorological data from LG AWS, 365 days (representing one arbitrary mass balance year from 1 March to 28 February) were repeatedly sampled at random until a synthetic annual climate was generated that met the following characteristics compared to the 2011/12 reference year (REF): +1 K air temperature (warm and wet, WW), -1 K air temperature (cold and dry, CD), +50% accumulation (WET), and -20% accumulation (DRY). As the sampling was purely random, individual days can be selected more than once in a single synthetic year (i.e. sampling with replacement). It was not possible to obtain temperature perturbations in the absence of any accompanying precipitation perturbation, which highlights the fact that the prevailing temperature influences hygric conditions on the mountain. In contrast, precipitation scenarios can be sampled with little changes in air temperature. Whether this (de)coupling is an effect of the local atmosphere-mountain interactions or of the large scale climate regime has not yet been explored. To maintain the actual hygric seasonality, the selected days were sorted according to their accumulation, with the wettest (driest) days randomly assigned to the wet (dry) seasons, minus one week to smooth the transitions between them. As there is insignificant seasonality in air temperature in this inner-tropical setting (Hastenrath, 1983), forced assignation of the annual temperature variations was neglected. Although the period of available AWS data is short, it has been shown to be representative for the recent decades in terms of monthly ERA-interim air temperature (1979–2012) and TRMM precipitation (1998–2012) time series (Figure 3 in Nicholson et al., 2013). Thus, the synthetic scenarios can be considered representative of perturbations possible over the recent past, but it is not clear if they capture the full variable space of climatic conditions prior to 1980. Nevertheless, these synthetic annual meteorological scenarios provide a useful and realistic basis to assess the sensitivity of the mass balance to a perturbation in its forcing climate within the *context of the recent past.*

Tables and figures

Table 2: Why is the snow/rain divider not mentioned?

Parameters in Table 2 are needed as MBM input but were not (or even cannot) be measured at a suitable temporal resolution. Thus, they were optimized in the Monte Carlo simulation at the point scale, where the meteorological input was recorded. Since accumulation is measured hourly, there was no need to optimize a snow/rain devider. If we would have optimized it e.g. to a lower temperature, the model would have removed accumulation that was actually measured. For the distributed model runs we checked the effect of the divider as mentioned on P 3903, L 6 and P 3904, L 15. Refer also to the explanation on P 2, point 3.

Table 4: Evidently there is no explicit precipitation scenario. As mentioned, I'm not convinced if this is adequate, but anyway, as an output, I think, there is a resultant precipitation. And this quantity would of course be interesting. So, I would suggest, that you would indicate this value in this table. Anyway, it is the question, if it is not adequate to communicate the different input scenarios additionally in a graph.

There are indeed explicit precipitation scenarios, which have minor changes in air temperature compared to REF: WET, DRY, AMPW, and ATT. These scenarios exert the highest control on the mass balance! This issue may become clearer with the excellent suggestion of inserting a spider plot (P 9 in this document).

Figure 1: It would help in interpreting the climate controls if there was an additional sketch map showing LG and Mount Kenia in a greater context and the two overpasses of the ITC (perhaps in different colours for 'long rains' and 'short rains') and the corresponding prevailing wind directions indicating the dominant transport of moist air masses.

The scale of the map is too small to depict the overpasses of the ITCZ and this is not relevant for the paper presented here. Nevertheless, we followed the advice and added an overview map and information about the wind direction in the figure capture.

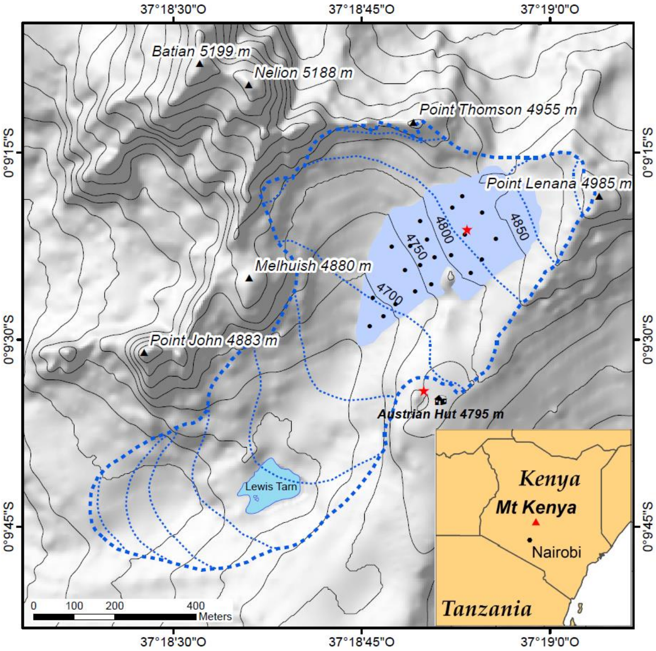

Figure 1: Overview map of LG, Mt Kenya, for 2010 (0.1 km^2) and the late 19th century (L19, 0.6 km^2, dashed). Red stars denote AWS locations, black dots the ablation stakes. L19 outline from Patzelt et al. (1984) with reconstructed contour lines. Off-glacier contours were taken from Schneider (1964) and updated for LG basin 2010 (Prinz et al., 2012). Mean wind direction for the depicted weather stations is south-east, invariant over the course of the year. Mean daily cycle of wind direction is east during the night and early morning and south from late morning to late afternoon.

Figure 3: the figure caption is somewhat confusing to me: I suppose that the indicated formula is only a part of the albedo parametrization, or not? Perhaps the authors could also enhance the figure by indicating in the figure, what surfaces are modelled in the course of time (snow, ice, …).
We agree and shifted the formula to the text (P 3896, L 14). Because of the frequent fresh snow events (almost daily) and the fast metamorphosis of snow, there is high frequent shift of the respective albedos, which is not possible color code or to visualize in the figure. Albedo thresholds are given in Table 2 but may vary depending on depth and age (for snow) and on dew point temperature (ice).

Suggestion for an additional graph:

An additional graph showing the used input scenarios in spider net plot or similar would point out in a more direct way how the different scenarios differ from one another.

We agree to this excellent suggestion and added a spider plot as Figure 7 and refer to it in the text on P 3903, L 2. *A summary of the climate variable space and resulting mass balances of the individual scenarios is given in Figure 7.*

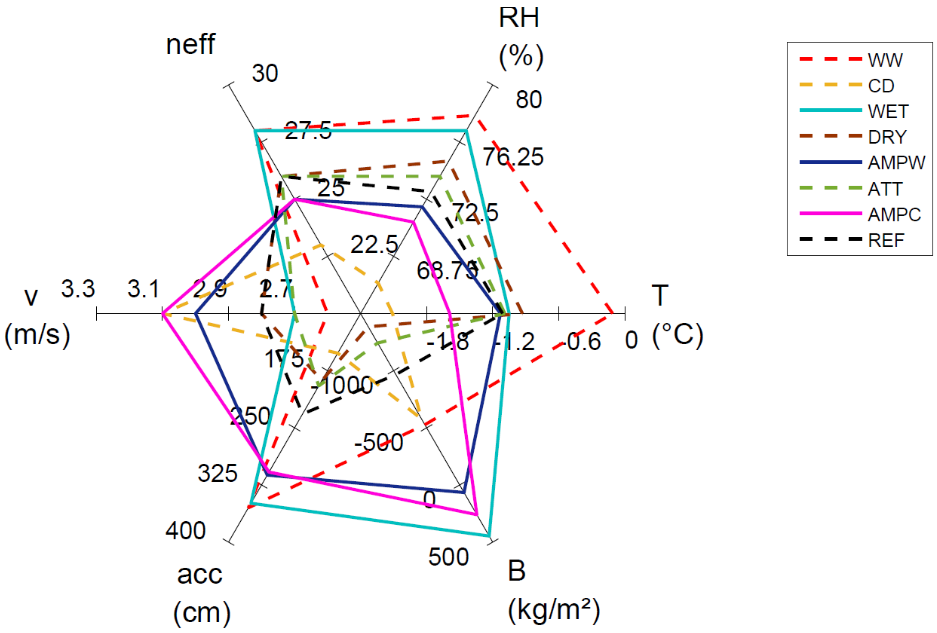

Figure 7: Spider plot showing the annual variable space of the individual climate scenarios. Solid lines depict scenarios causing positive mass balances for the current extent of LG. Variable abbreviations same as in Table 4. The REF year has similar characteristics as scenarios DRY and ATT underlining the current accumulation deficit. Positive mass balances are a result of abundant accumulation and a complex interplay between decreased (increased) humidity and cloudiness favoring long-wave surface cooling (reducing SWI).

Response to reviewer 2, D. Samyn:

General comments:

First, the authors underpin at several instances that precipitation, albedo and cloud cover are key factors controlling mass and energy balance. They propose a suite of synthetic climate scenarios to illustrate the coupling of specific climatic variables and their dominance on mass balance variability. However this coupling pattern is not straightforward, especially in the case of a glacier having so much mass/surface in a short time period. I would find interesting for the community to be able to see, in addition to (or based on) these scenarios, e.g. graphs or schematics of the direct influence of factors like temperature, albedo or cloud cover on the corresponding mass balance budgets.

There are two reasons favoring coupled parameter perturbation:

(i) Nicholson et al. (2013) cannot identify single parameters governing the mass balance on Lewis Glacier, suggesting a more complex climate-glacier interaction.

(ii) The physical link between air temperature and vapor pressure (which is derived from measured relative humidity and calculated saturation vapor pressure, a function of the air temperature): These two parameters not only control the turbulent heat fluxes, but also the parameterization of the most important energy fluxes – the radiative fluxes (scattering and absorption in the atmosphere for global radiation, ice albedo for reflected short wave radiation and atmospheric emissivity for long wave radiation). Thus, for this study using a physical and process based model, a single parameter perturbation would neglect the physics of the atmosphere-glacier interaction and would calculate results that are impossible to interpret and discuss.

We added a sentence to better explain this on P 3899, L 13: *As described by Nicholson et al. (2013) it is unlikely that a single climatic variable dominates the mass balance variability on LG. Thus, and because of the physical link between air temperature and vapour pressure (subsequently controlling the turbulent heat fluxes and the parametrizations of the radiative fluxes) exploring mass balance sensitivity of single climatic variable perturbation was rejected in favour of coupled perturbations reflecting the variability in the climate more comprehensively (Mölg et al., 2009a).*

Second, it is made clear from the authors' work, that (1) the glacier is not in equilibrium with modern-day climatic conditions, and (2) that modern-day conditions cannot be used to infer glacier retreat dynamics and climate evolution in the last centuries. However, given the apparent intercorrelation between temperature, moisture, cloud cover etc, I would find it interesting exploring which past time period covered by instrumental records or robust proxies, would provide the best constraining data set for retrieving L19 climate conditions. In other words, since, owing to glacier geometry and climate dissymmetry, the present cannot be used to reconstruct past climate at LG, is there a favourable past time window allowing so at all? Further calculations are not necessarily needed here; I think the reader might expect the authors to elaborate some more on this.

Indeed, we explain a possible climate sustaining East African glaciers already in the introduction, P 3890, L 11: *The glaciological evidence for a drier atmosphere since the late 19th century is in accordance with alternative proxy climate records that indicate that the decades immediately preceding 1880 were humid, and characterised by high lake stands and relatively abundant precipitation (e.g. Konecky et al., 2014; Nicholson and Yin, 2001; Verschuren et al., 2000).*

To remind the reader of this sentence, we repeat it in the conclusions and add it on P 3907, L 3: *The modern-day sensitivity suggests that expanding LG to the L19 extent would require a change in the mountain's moisture regime to bring substantially more moisture and, consequently, more accumulation to the mountain. The same result was found for Kilimanjaro glaciers* (Mölg et al., 2009) *and is supported by alternative climate proxies indicating abundant precipitation and high lake levels in the decades prior to 1880 (e.g. Konecky et al., 2014; Nicholson and Yin, 2001; Verschuren et al., 2000).*

Technical comments:

P. 3889, Line 7: "filtered by glacier dynamics" -> did you mean "counterbalanced by glacier dynamics" or so?

Filtered here refers to the reaction time that the change of a glacier extent needs to react on a climate signal. Glacier dynamics act as a low-pass filter and smooth high frequent mass changes resulting in extent changes.

P. 3889, Line 26 … revealed that glaciers on Kilimanjaro…

Corrected

P. 3890, Line 9: … both … are…

Corrected

P. 3890, Line 28: …additional (?) data is now available…

Changed to: *Although pioneering for their time, 2.5 years of in-situ meteorological data is now available to allow a more rigorous assessment of the climate sensitivity of the glaciers here and explore if…*

P. 3899, Line 24: …were sampled with replacement -> could you clarify this?

This is a term used in stochastic and probability theory, meaning that a single item can be sampled more than once (e.g. If it would be allowed in the lottery to draw a single number more than once, by putting it back into the pool after it has been drawn.)

We rephrased the whole paragraph and wrote explicitly: *As the sampling was purely random, individual days can be selected more than once in a single synthetic year (i.e. sampling with replacement).*

P. 3900, Line 21: … maximum… -> typo: remove the parenthesis.

Corrected

P. 3900, Line 23: "fresh snow density of 315 kg.m-3" -> can this really be called "fresh snow"? How is this value obtained?

Refer to the reply to reviewer 1, P 2, point 2.

P. 3903, Line 1-2: "For the latter it is crucial that albedo is high … to compensate for increased SWI from clear sky conditions". Could you elaborate a bit more on this?

The AMPC scenario has an amplified seasonality: Wet seasons have abundant accumulation with little temperature change to REF and very dry and cold dry seasons. The latter result in high radiative energy surplus, due to prevailing clear skies, fueling melt energy. This can be compensated if the wet seasons bring enough snow to extend the snow cover long enough into the dry season to keep the albedo high and reduce melt. Thus, AMPC result in equilibrium mass balance not because it is colder, but because the albedo is high from abundant wet season snow fall.

P. 3905, Line 18-20: "This supports the idea that a larger glacier can develop a deeper katabatic boundary layer… more difficult to entrain through advection… of warm air". Do the authors imply

here that a reduction in temperature would lead to a less negative mass balance? My question deals with my first general comment above – no temperature range is provided here that would allow appreciating to which extent 'colder' temperatures can lead to reduced precipitation on LG.
We suggest that the air temperature gradient along the glacier might have changed due to the vast changes of the glacier area and its surroundings and subsequent modifications of the glacier boundary layer. This is independent from climate scenarios.
Based on our data, we are not able to sample temperature scenarios and explain this in the rephrased section 2.6: *It was not possible to obtain temperature perturbations in the absence of an accompanying precipitation perturbation, which highlights the fact that the prevailing temperature influences hygric conditions on the mountain. In contrast, precipitation scenarios can be sampled with little changes in air temperature. Whether this (de)coupling is an effect of the local atmosphere-mountain interactions or of the large scale climate regime is unexplored yet.*

Fig. 1: The caption and the map are lacking basic features, such as country of location or a sketch map. Perhaps also include basic weather indications (e.g. wind patterns).
Refer to the reply to reviewer 1, P 8, Figure 1.

Table 4: The interpretation of the different scenarios results is not really handy from this table. An additional sketch figure with prominent scenario characteristics and results would certainly help.
Refer to the reply to reviewer 1, P 9.