We would like to thank the referees for their effort to review our work and for their detailed and thorough comments on the manuscript that certainly helped to clarify our study. Below we respond to their comments. Referee comments are in bold font while our responses in normal font. The re-submission of the revised manuscript is then accompanied by a version where the significant changes made are highlighted to facilitate re-evaluation of the manuscript.

Referee #1

General comments

1.1. However, there is an over emphasis on the conclusion that the seasonal variation in the ice-flux component of frontal ablation is driven by surface melt and liquid precipitation penetrating to the bed and enhancing lubrication. This is a perfectly reasonable discussion, but more consideration should be paid to other processes which may contribute to the seasonality of frontal ablation.

We do not agree with this statement as we link the speed variations to surface melt and liquid precipitation penetrating to the bed and enhancing lubrication, but not frontal ablation. In the introduction of the original version of the manuscript, we shortly introduced all factors of the frontal ablation, namely iceberg calving *D*, subaerial frontal melting and sublimation $A_{f(air)}$, and subaqueous melt $A_{f(wtr)}$. In the discussion we point out now more clearly that the retreat in each year (peak in calving) does not coincide with the peak in velocity and therefore do not link enhanced basal lubrication to calving (see also following comment).

1.2. For example, Figures 8 and 12 indicate that the change in ice front position is a significant component of ablation. Why does the front advance and retreat?

In the discussion we now clarify this question by adding the following paragraph 'In general glacier advance occurs when the glacier flows at a higher rate than frontal ablation occurs and a glacier retreats when frontal ablation rates exceeds the glacier flow. Advances regularly occur in spring and summer when the velocity is especially high due to enhanced lubrication and calving rates are low (Köhler et al., 2012). The timely occurrence of the retreat in autumn is in line with observations from Köhler et al. (2012), who found that calving-related seismicity at Kronebreen predominantly occurred in autumn, after the peak in velocity. In late summer and autumn decreased velocity and warm ocean water entering the fjord, hence increased melt, undercutting and calving, lead to retreat (Luckman et al., 2015).'

Luckman et al. (2015) is a reference, which was not available at the time of submission of our original manuscript, but clarified the reason of the enhanced retreat in autumn.

1.3. What causes changes in calving rates? Is it due changes in back pressure of fjord ice, increased subaqueous melt, surface ablation? Possible warming of fjord water is invoked to account for the retreat of Kronebreen in 2011, could this not also be a regular seasonal driver?

Based on your suggestions we enhanced our discussion by accounting for the other factors of frontal ablation. Fjord ice or ice mélange does not play a major role in Kongsfjorden, as now mentioned in the discussion.

Subaqueous melt and related undercutting and calving is most likely the most important factor in changes of calving rates. Warm water was mentioned as a regular seasonal driver in Köhler et al., 2012, a hypothesis confirmed by Luckman et al. (2015) for 2013. Both references are now cited in the introduction and discussion.

1.4. A case is also made that retreat causes increases in ice flow (Abstract and Conclusions); it is not possible to verify this from any of the plots. The authors should be more specific about the timescale over which they are suggesting that this process operates, and include long term plots, 2007 to 2014, of frontal positions compared with flow speeds where available.
To provide more insides, we added more calving front outlines to the plots. For the purpose of visual clarity we did not plot all terminus positions between 2007 and 2013 but a representative time series was included for both, Kronebreen and Kongsbreen, in Fig. 7 and 11. They both show the minimum and maximum extend before, during and after the retreat in each year if available, and therefore correspond to the description in Section 5.1.2 (Kronebreen) and 5.2.2 (Kongsbreen).

Concerning the timescales we mentioned in the abstract and the conclusions, that high speed coincides with the retreat especially at Kongsbreen in 2012 and 2013. The speed started to increase in autumn 2011, and lasted for two years. We hope this becomes clear enough for the reader.

Because speed and terminus positions are not directly linked, we refrained from adding an extra plot showing these in combination. Speed profiles are already shown in fig 5., 6., 9, 10, while changes in terminus positions are shown in figures 7 and 11 and respective volume changes in figures 8 and 12.

1.5. This would also make it easier to visualize changes in 'background' speed, which should be defined at some point.
To clarify the term 'background speed' we now explain in the discussion, that the 'Background speed is the almost constant minimum speed in autumn and winter. After an effective channelized system has evolved the speed drops as there is a lack of lubricant. When there is no longer enough water supply to sustain the channelized system at the end of the melt season, an inefficient system evolves which also lacks of lubricant, unless it is not raining (see secondary speed peaks).'

__Specific comments__
Regarding the period between May and September in 2013 with poor coverage of velocity measurement a better estimate of flux across the gate for Kronebreen could be obtained by scaling the preceding or succeeding fluxes by the respective centerline velocities.
Centerline velocities do not exist at the cross section with the fluxgate or elsewhere along the fluxgate which makes an interpolation difficult. Especially because we expect higher velocities in summer than before and after that period. Therefore we refrain from scaling the preceding and succeeding velocities.

For Kongsbreen I suggest either leaving the plot blank in this gap or interpolating for the purposes of ablation calculations. For both glaciers the plotting of a flux of zero is clearly not realistic and misleading.
We fully agree and removed the flux of zero from both plots.

You report a thinning rate for Kongsbreen of 3 m/a. This is significant in terms of the error estimates for glacier thickness therefore you should include it in these estimates.

The thinning rate is on the order of 3 m/a (dynamic thinning/retreat) at a fixed position slightly upglacier of the 2013 front position, but nevertheless the height of the calving front above the sea remains more or less constant over time (but not fixed in position). Therefore the rate of thinning of 3m/a is not included in the error estimate of +-15m. We tried to clarify now in section 2.2.

In section 5.1.1 reference is made to summer speedups in 2009 but no speeds are shown for summer 2009 in Figure 6. This point arises again in the Discussions section.

We now removed the parts where we mentioned the 2009 numbers due to their too sparse temporal coverage: e.g. 'Winter background speeds and summer speedup in 2010 had a medium amplitude (Fig. 5 and Fig. 6) coinciding with the lowest melt water production during the observation period (CPDD2010 = 451 °C d).'

In the Discussions, the idea of warm water entering the fjord in 2011 is proposed to explain the retreat of Kronebreen. What is the evidence for this warming? Why would 2011 be warmer than in other years?
Please see comment above on 1.2 and 1.3.

Does the glacier retreat at the same time every year? Again, this could be elucidated using a time series plot of frontal positions.
Please see comment above on 1.4.

Do the earthquakes peak at this time every year or just in 2011?
We now mention in the discussion, that they peak in 2009 and 2010 (Köhler et al. 2012).

Section 5.1.2
A time series of frontal positions would help this description, similarly for Kongsbreen.
Please see comment above on 1.4.

Section 5.1.3
A number of different figures are given and it is necessary to refer to Table 5 try to make sense of these figures.
We now refer to Table 5 in section 5.1.3.

Please try and make it clearer in the text, for example, that q = 0.21 - 0.25 Gta-1 refers to the two different values of velocity correction factor, and the figures in brackets are the min and max bounds on each of these values, if I understand correctly.
Yes, you understood it correctly. In the method section 4.2.2 of the original manuscript we explained that **"**In this study, estimates of the frontal ablation (Eq. 1) are always given as range $A_{f\,0.8}$ - $A_{f\,1.0}$, with the lower estimate $A_{f\,0.8}$ based on a correction factor for the depth-average speed of f_{da} = 0.8 in the calculation of the ice flux" and "Additionally, we provide in brackets an upper and lower boundary of the frontal ablation, $A_{f\,min}$ and $A_{f\,max}$ based on the uncertainties (σ) of the input variables (Tab. 3)." We tried now to clarify at each occasion.

When reporting the split between terminus retreat and ice flux you then quote only a single figure? Also include a reference to Table 5.
- Done.

<u>**Technical corrections**</u>
P 6194
Line 18. 'Retreat is an …' ie delete 'The'.
- Done.

Line 26. SLE? Have used SLR earlier.
Yes, first we are talking of glaciers as contributors to sea level rise (SLR), later we use the unit 'sea level equivalent' (SLE) as we speak of loss of glacier mass.

P 6195
Line 6. 'at a rate of a few meters'
– Done

Line 7. Delete 'it'
– Done

Line 11. 'climate basal balance (B)' but SMB was used earlier?
We believe, that 'climatic basal balance' B is correct here, as climatic basal balance is the sum of surface balance B_{sfc}, internal balance B_i and basal balance B_b: $B = B_{sfc} + B_i + B_b$
see: Cogley, J.G., R. Hock, L.A. Rasmussen, A.A. Arendt, A. Bauder, R.J. Braithwaite, P. Jansson, G. Kaser, M. Möller, L. Nicholson and M. Zemp, 2011, *Glossary of Glacier Mass Balance and Related Terms*, IHP-VII Technical Documents in Hydrology No. 86, IACS Contribution No. 2, UNESCO-IHP, Paris.

P 6197
Line 10 'estimated to <u>be </u>0.25'
– Done

Line 21. 'northern branch <u>ends</u> in a deep fjord'
– Done

P 6198
Line 6. 'and there is widespread cloud cover during summer'
- Done.

P 6199
Line 16. 'We assume' do you mean this is your estimate based on the SPIRIT DEM?
Yes, you are right. We changed our sentence to: 'z_s was estimated to vary within a range of 40 ± 15 m based on a SPOT-5 SPIRIT DEM derived from data acquired in September 2007 (Korona et al., 2009).'

P 6200
Line 14. 'well established' hyphen not necessary.
– Done

Line 22. 'erroneous speed estimates' how were these identified?
We now clarified, that: 'Erroneous speed estimates (local abnormal values in magnitude) were identified by visual inspection and removed.'

P 6201
Line 15. It would be helpful to express these displacement accuracies in terms of their impact on velocities.
In this case, we think it makes more sense to look at displacements, as they are a measure of coregistration accuracy and are independent from the time interval between the two respective acquisitions. The velocities are just displacements divided by the time between the first and second acquisition. Therefore velocities are not a measure for coregistration accuracy. Moreover, these displacements accuracies do not have an influence on glacier velocities in our study, as they are within the accuracy of the offset tracking method and do not show any coregistration errors. We thus preferred to not change the text.

P 6202 Line 10. 'in <u>the</u> vicinity'
– Done

Line 15. Replace 'depth averaged speed' with 'correction factor'.
Based on your suggestion, we changed our sentence to '… based on a correction factor for the depth-average speed'

P 6203 Line 17 replace 'extend' with 'extent' (and P 6204, Line 12).
– Done

P 6205 Line 18. Observation period being 2007-2014?
We write now more specifically: 'Measured background speed during winter 2012/2013 was the highest <u>in the period 2010 to 2013.</u>'

Line 25. 'December 2007 to December 2013'?
– Done

P 6206
Line 16. Do you mean 'mean frontal ablation rates' not 'total frontal ablation'?
Yes, corrected.

Line 20. 'This period spans an entire year and is therefore unbiased…'
– Done

P 6207
Line 21. A summer peak did develop on Kronebreen at this distance from the front, are the elevations the same at this point?
We now added the information on elevation in section 5.1.1 and 5.2.1.The elevation six kilometer from the front at Kronebreen is 213 m (and at Kongsbreen it is 205 m). At P#2 at Kongsbreen it is 319 m.

P 6209
Line 5. 'but as <u>the</u> likely…'
– Done.

Line 26. Specify which glacier.
Done. The maximum speed at the calving front <u>of Kronebreen</u> stayed below the level.

P 6210
Line 5. No early velocities are shown for Kongsbreen so what does 'typical' refer to?
This behavior was modelled by Schoof (2010) and is therefore described as a typical pattern for all glaciers, not only Kongsbreen.

Line 15. '1990s'
– Done

Line 22. 'in contrast to previous years'.
– Done

P 6211
Line 8. 'depends on high SAR image resolution and persistent surface conditions.'

– Done

P 6212
Line 6. 'have been studied over multiple years'
– Done

Line 19. Use 'largest' or 'major' instead of 'vastest'.
'major'

<u>Figures</u>
The figures are generally well produced and clear. It would be useful to see a bedrock map including where measurements were obtained and therefore the extent of the interpolation.
For this study only a subset of the bedrock map across the fluxgate was available, as the whole map and the flight lines will be published separately in another paper by the Norwegian Polar Institute.

As mentioned above, time series plots of frontal positions would be helpful.
Please see comment above on 1.4.

In captions and keys be consistent using 'centreline' or 'center line' or 'centre line'.
- Done.

Figs. 3 and 4. Text on graph axes and in the key is too small. Need a) and b) labels on figure panels.
- Done.

Fig. 5. Vertical lines marking year boundaries would make seasonal effects clearer.
Done.

Fig. 6. As mentioned earlier, the text refers to both PDDs and velocities for 2009 but these are not shown on this plot.
Please see comment above on 4th specific comment.

Fig. 7. Use Dec instead of Dez in the key. Mark the location of the inferred pinning point.
- Done.

Fig. 10 As for Fig. 6.
Please see comment above on Fig 6.

Referee #2

<u>General Comments:</u>
1) The authors make an effort to provide upper and lower bounds for their ice flux and frontal ablation rates that take the uncertainty in their observations into account; however, I think that the uncertainty in their thickness estimates is underestimated for several reasons. First, the thickness estimates across the flux gates are assumed to be constant in time. Given the seasonal and inter-annual changes in velocity discussed in the manuscript, this assumption is most likely invalid.
Unfortunately, we have no seasonally resolved accurate-enough elevation measurements available, and also the GPS-units don't provide this information (code-based). Therefore we can't verify the seasonal and inter-annual changes in calving front and flux-gate height due to changes in velocity. However, we believe that a reasonable range of such height variations is included in our error budget term of thickness uncertainty. We tried to clarify in sections 3.3.1 and 3.3.2.

Second, as described above, the location of the thickness measurements for Kronebreen with respect to the flux gate are unclear. Any spatial offset between the observations and the flux gate will lead to some uncertainty since both surface and bed elevations should vary in the along and across-flow directions.
Please see comment above. We believe that such spatio-temporal elevation variations will not exceed the elevation uncertainties assumed.

Finally, the thickness estimates for Kongsbreen partially rely on water depths observed down-fjord from the 2007 terminus position. Given that the rapid retreat of numerous tidewater glaciers throughout the past several decades has been strongly controlled by the presence of bedrock depressions, it is possible that the retreat of Kongsbreen was also topographically controlled. Therefore, although the water depths varied little immediately down-fjord of the 2007 terminus, they did not necessarily maintain a constant depth up-glacier. In order to increase confidence in the thickness estimates at both glaciers, I suggest that the authors attempt to extract changes in ice surface elevation from repeat DEMs acquired during the observation period (if available). The DEMs could be used to (1) constrain temporal changes in flux gate thickness and
We hope to have addressed this comment above (1st comment) and below (2nd response below this).

(2) test the validity of the assumption that the Kongsbreen calving face height remains constant.
Please see comment above and below (1st comment by referee 2, and 3rd specific comment by referee 1), .

Assuming that the ice thickness follows the modified flotation criterion proposed by Vieli et al. (J. Glaciol., 2001) based on observations from Hansbreen (i.e., the ice thickness at the calving front is a constant fraction greater than the flotation thickness), the authors could also assess whether the it is reasonable to assume that the bed across which the terminus retreated has the same depth profile as the bed depths down-fjord from the 2007 terminus position. Therefore, the acquisition of additional DEMs would greatly increase both the accuracy of the ice thickness estimates and confidence in uncertainty estimates.
The ice thickness assumption is not valid in this case, as “with this model, the linear relation between calving rate and water depth proposed on empirical grounds is qualitatively reproduced for the situation of a slowly retreating or advancing terminus, but not for situations of rapid changes. Length changes of tidewater glaciers, i.e. especially rapid changes, are dominantly controlled by the bed topography and are to a minor degree a direct reaction to a mass-balance change” (Vieli et al. 2001). The rapid retreat rather points to an over-deepening of the fjord. That is why we believe that the minimum estimate is the best guess and is represented in the best possible way here. Therefore
we make the limitation in the original manuscript, that the reader should ‘Please note that the assumed cross-sectional fluxgate area and thus the computed ice flux represent a minimum estimate. We chose a location of the fluxgate (Fig. 1) that allows for extraction of values from all velocity maps. As Kongsbreen retreated significantly since the first acquisition in April 2012, the fluxgate was not located close to the calving front at the time of most of the acquisitions, but up to several hundred meters upglacier, where the actual ice thickness is larger and speed is lower than close to the terminus. Upper and lower boundary of q should in the case of Kongsbreen not be interpreted directly as formal accuracy but as the likely range based on the large uncertainties in the constraint of the fluxgate geometry.’
The uncertainty or, better, likely range of ±15 m calving front height used (i.e. between 25 and 55m) accounts for both variations in along and across-flow directions.

2) In the Results section, please be more specific/quantitative when describing the data (i.e., replace “most”, “lower”, etc. with percentages or values).
As we do not have a single point where we measured speed for Kronebreen (e.g. we show the velocities of three GPSs and one SAR velocity point in Figure 6 during different time periods) we

prefer to speak of speeds/amplitudes in relative terms and do not compare them directly with numbers, as they aren't directly comparable. Exact numbers can be extracted from Figure 6 if needed.

Similarly, please define your criterion for "stable" speeds and terminus positions in the methods section. As is, the reader is unsure whether "relatively stable" terminus positions are those that vary with +/- 10, 50, or 100 meters and the time scales over which you are assessing stability. The same comment in regard to speeds. If stability was assessed using the uncertainty in the datasets, please make that clear so that the reader knows stability equates to insignificant change (i.e., change not exceeding uncertainty).
In section 4.2.1 we now tried to clarify, that changes of few tens of meters are small changes and we refer to them as relatively stable front positions.

3) Finally, in the Discussion you provide an explanation for the correlation observed between speed and surface melting/rainfall. Although the effects of the seasonal evolution of the subglacial drainage system have been well-documented for a number of glaciers, please provide a more detailed explanation.
We have unfortunately no direct observation of subglacial hydrology and therefore only discuss hydrology effects on flow variation where strongly suggested by our data: Summer speedup associated with onset of surface melt (indicates basal lubrication/rising water pressure, i.e. drainage system inefficient and cannot accommodate meltwater input); slow down while melt continues (drainage system evolved and is capable to accommodate continued meltwater input); autumn events/speed-up following increase melt rates or rain events (again overcharging the drainage system and raising basal water pressure); winter events, such as following the extreme rain event. We extended on that, also in response to referee 1.

For example, is there a known lag in water transport from the surface to the bed? Does the water drain to the bed through moulins, crevasses, or both? Investigating the glacial hydrologic network is obviously outside the intended scope of this paper but it would be helpful to the reader to have a more detailed description of why the observed correlation would exist along with additional references.
See comment above. We did not study meltwater routing and hydrology processes in detail and therefore restrict our discussion to the most striking correlations between meltwater production and speed, where PDD, as a proxy of surface melt provides sufficient explanation.

<u>Detailed Comments:</u>
p. 6195, lines 14-18: It would be helpful to clarify that the cited studies all examine subaqueous/submarine melting of the terminus.
Now we make clear that 'Subaqueous melt has also been widely studied in Greenland (e.g. Holland et al., 2008) and for the Antarctic ice shelves (e.g. Pritchard et al. 2012), … '

p. 6196, line 2: Change "offset tracking on" to "offset tracking of".
– Done.

p. 6196, line 11: "deployed"? I think "employed" would be more appropriate.
– Done.

p. 6196, lines 18-21: I found that the time referencing is a bit confusing here. Was the second surge just in 1995 or from some year before that until 1995. If only in 1995, remove "until". Also, please replace "at that time" in the second half of the sentence with the year(s) for the surge you are referring to here. As is, it is unclear which surge if being referenced.

- Clarified. There is only one surge of Kronebreen mentioned in the text, which was in 1868 or 1869.

p. 6198, lines 5-6: Please reword the second half of this sentence so it is clear that polar night and cloud cover inhibit the use of optical imagery in the winter and summer, respectively.
– Done, see comment above.

p. 6198, line 23: Please be more specific than "sub-daily". The frequency of the GPS observations isn't likely to lead to any offset in your speed comparison since the SAR-derived speeds are averaged over several days, but the reader cannot assess its influence without a quantitative value.
Positions are acquired every three hours. Now clarified in section 3.2.

p. 6199, section 3.3: As described in detail in my comments above, I think some additional work can be done to estimate ice thickness and constrain uncertainty.
See major comments above.

At the very least, this section should describe where the Kronebreen thickness observations were acquired with respect to the flux gate and the fact that the glaciers may have undergone rapid retreat because they were initially grounded across basal depressions, meaning the ice thickness may not be uniform along flow.
For this study only a subset of the bedrock map across the fluxgate was available, as the whole map and the flight lines will be published separately in another paper by the Norwegian Polar Institute.

p. 6200, line 22: How do you assess whether speeds are erroneous? Do you manually remove outliers identified by visual inspection? Do you remove values that exceed the mean +/- 3 standard deviations for a small sample region?
Please see comment on the same issue by referee 1.

p. 6201, lines 23-24: I assume that the variations in the location of the flux gate with respect to the terminus are due to fluctuations in the terminus position. Please clarify in the text whether that assumption is true.
Yes, you are right. …"ice flux q_{fg} through a fixed fluxgate above the position of the calving front, which is variable in time".

p. 6202, line 15: Change "speed" to "speed correction factor".
Now: "Based on a correction factor for the depth-average speed".

p. 6205, line 3: Replace "most" with a value (percentage or "X of Y").
Done. 'In three out of four years…'

p. 6205, lines 7-8: What were the seasonal amplitudes?
It depends on where you look at. As we do not have a single point where we measured speed (e.g. we show the velocities of three GPSs and one SAR velocity point in Figure 6) we prefer to speak of speeds/amplitudes in relative terms and do not compare them directly. Exact numbers can be extracted from Figure 6. Please see also comment above.

p. 6205, line 10: Maximum, mean, or minimum summer speed?
Changed to 'Maximum summer speed'.

p. 6205, lines 10-11: Please be more specific. How much lower and higher were speeds? Does "lowest level" mean "slowest speed"? If so, also list the speed.

Please see comment #2 above. Yes you are right. “Lowest level” means “slowest speed”. We changed that in the new version of the manuscript

p. 6205, Section 5.1.2: I suggest that you define terminus stability in your methods section since you frequently describe the terminus as “relatively stable” here and in section 5.2.2.
Please see comment #2 above.

p. 6206, line 14: What was wrong with that image pair? Please state how you assess the quality of the speed maps.
The time period between the image pair of 3 May 2013 and 24 September 2013 was too large to extract speeds at the calving front. Too many changes through advance/calving occurred in that period and the offset tracking algorithm was not able to provide results at the fluxgate.
Quality was assessed by the matching signal-to-noise ratio of measured offsets. We explained now in the text (5.1.3).

p. 6206, line 16: The values listed are frontal ablation rates (Gt/a) so the beginning of the sentence should be change to “Total frontal ablation rates. . .”.
You are right to speak of rates, but ‘Mean frontal ablation rates’ is the correct term here. See response to referee 1.

p. 6206, lines 20-25: If these frontal ablation rates are calculated using a different speed dataset than was used in the previous paragraph, please make that clear.
It is a subset of the dataset used before and spans an entire year. Therefore it is unbiased towards fast flow in summer or slow speed in winter when comparing the data to other studies. We tried to clarify better.

p. 6207, line 6: Be more specific than “most”.
We linked now to Fig 10.

p. 6207, line 6: Change “This data indicates” to “These data indicate”.
- Done.

p. 6207, line 7: As with terminus positions, please define the criterion for “stable” speeds in the methods.
Please see comment #2 above.

p. 6207, lines 9-10: How far inland did the speed-up reach (in km)?
In section 5.2.1 we now clarify that ‘speeds greater than 1 m d^{-1} were measured up to 3.5 km inland from the calving front (Figs 9 and 10).’

p. 6207, lines 12-15: This sentence is a little confusing since you just said the lowest speed at P#1 occurred in autumn 2011. Were the speeds comparable before and after the event at P#1 and slightly lower at P#2? Please clarify.
No data is available at the time of the extreme rain event in January 2012, but speed was at a comparable or lower level at $P_{\#1}$ and $P_{\#2}$ before than after the event. We tried to better clarify.

p. 6208, lines 1-17: Replace “major” and “minor” in this section with values. Are “major” changes those that exceed uncertainty or some set amount?
As with the velocities of Kronebreen, we prefer to provide relative terms here as the calving front did not advance/retreat linearly across the fluxgate. Both major and minor changes are visible in figure 11.

p. 6208, line 6: Define “typical”.
Clarified.

p. 6208, line 8: I’m unsure of what you mean by “lacking behind”.
We rephrased the sentence to make it clearer: ‘Kongsbreen reached a similar position as in 2009, only the southern part did not advance as much as the rest.’

p. 6208, Section 5.2.3: As in the Kronebreen section on frontal ablation, please insert the word “rate” after “frontal ablation” since you are presenting Gt/a values.
- Done.

p. 6209, line 1: How do you define significance?
Clarified (several hundred meters).

p. 6209, Section 6: I suggest replacing “linked to” with “correlated with” since your data show a correlation between the variables. Also, I suggest ending the first sentence in paragraph 2 of the discussion after “melt water input and rainfall”. Since you are inferring that the correlation between the variables is due to meltwater effects on basal hydrology, I suggest you remove the word “influencing” and start the second sentence with something like “We attribute the observed correlation to the influence of melt water and rainfall on the water pressure at the bed and. . .”.
- Done.

p. 6210, lines 5-12: I would think that creep would close the subglacial channels if water pressures decline after the January rain event. Please provide an explanation as to why the channelized system would persist during a several month period prior to the onset of the subsequent melt season.
Yes, you are right. The observations rather point at an inefficient drainage system throughout. The water from the rain event is not evacuated and high water pressure maintained throughout the winter/spring. As winter velocities where already enhanced, the following summer speedup is less pronounced as in other years. Text extended accordingly.

p. 6210, line 15: “1990s”
- Done.

p. 6212, line 19: Change “vastest part” to “fastest period of retreat”.
Changed to 'the major part' (as suggested by Referee #1)

p. 6212, line 21: “The frontal ablation rate. . .”
- Done.

p. 6212, line 25: What is the “actual” flux? Is this the calving flux?
Actual flux is the flux ‘through the fluxgate’ as now specified in the conclusions.

Figs. 7&11: Change “Dez” in legend to “Dec”.
- Done.