Dear Dr. Etienne Berthier,

Thank you for the opportunity to respond to the reviewers and tidy up the manuscript. We have attended to the comments point-by-point and we added the correct DOIs for the videos. We believe these changes will make the manuscript much stronger. We look forward to your response.

Thank you, Naomi Ochwat, on behalf of the coauthors.

Below you will find the description of the edits the reviewers requested and how we responded to them.

Referee # 1

General Remarks

The study by Ochwat et al. has again been modified substantially. Once again, I would like to thank the authors for making a thorough effort in revising their study. I think my comments were addressed. The study has gained a much stronger focus on the modelling and the description of the modelling approach as well as the results has further matured.

I have few comments on details and one suggestion which is more general and concerns surface lowering. Assuming that thickening from accumulation and thinning due to ice flow are in balance, the surface at the plateau would remain at stable elevations. However, melt has increased over the recent years likely leading to an imbalance and surface lowering. The terms of the imbalance seem to have three components that all cause surface lowering but their impact on mass balance differs: (i) melt which runs off (actual mass loss; amount poorly known because runoff is difficult to observe), (ii) melt that refreezes (apparent mass loss; subject to uncertainties but better known than the other terms) and (iii) melt that makes it into the PFA (likely contributing to mass loss but this is not known with certainty; this term is not well constrained and overlaps with (i)). Finally, there is a potential fourth component, namely accelerated compaction of warming firn (apparent mass loss, difficult to quantify as there is substantial uncertainty to what degree the firn warmed). While I think the authors have now outlined these different components well, I still suggest distinguishing their influence on geodetic mass balance more clearly in the discussion, in particular in Section 5.3.

Thank you for this good summary. This is a nice way to summarize the mass balance influences – we agree, (ii) is best-known here, from the firn core, while (i), (iii), and (iv) are all potentially significant but poorly understood. Indeed, one of the contributions of this study is having identified that (i), (iii), and potentially (iv) are active processes at this site, and need further work to constrain. We have rewritten the first paragraph of section 5.3 to introduce these different components, the factors that influence them, and indicate how they are typically interrelated. We then outline their likely importance in the following paragraphs, using much of the text as previously written. We appreciate the suggestion and additional clarity here.

Detailed Remarks

Line 420: I suggest removing “perpetual”.

We removed this, thank you.

Line 558: “... has played ...” Why past tense? It still plays an important role.

Yes, apologies. We fixed the tense, line 582.

Lines 586 – 587: This sentence is unclear. At the drill site, summer melting is the precondition to both refreezing and mass loss (through runoff). I think what is meant it that both refreezing and runoff will result in surface lowering (but only one of them is associated with mass loss).

Yes, that is what we mean. We reworded this and believe this is clear now, lines 625-627. Thank you.

Line 588: I do not understand “ ... remaining 27%”. Both 100 - 96 and 100 - 86 (percentages from previous sentence) do not yield 27 %.

Apologies, this was our confusion around the percentage of refreezing and runoff for total melt vs. the net surface melt. For clarity, 96% and 86% of total melt refreezes. But expressed as a percentage of the net surface melt (i.e., not including recycled meltwater), this is 91% and 73% - hence the 27% runoff, for the period 2005-2017 (see Table 2 for the numbers behind these values). As a percentage of the total melt, the runoff from 2005-2017 is 14%, as the reviewer notes. However, we decided to revise this to discuss everything in terms of the net melt, which represents the effective meltwater that is available at the surface and infiltrates the snow and firn (where it can refreeze or drain – that which drains can then either be stored as liquid water in the pore space of the deep, temperate firn, or it can drain from the system – runoff and mass loss). Thank you for catching this. We have corrected this and now more clearly discuss it in lines 627-632.

Lines 612 – 619: In its current form, the study thoroughly discusses uncertainties in the measurements and in modelling. The statements here in the conclusion, however, do not reflect the considerable uncertainties anymore. No need for another detailed discussion, somewhat more cautious wording or a brief remark on uncertainties might suffice.

This is a fair comment; we don't wish to lose the uncertainties. We added a sentence to the conclusion to help address this: "Though not observationally constrained and therefore uncertain, the modelling results suggest the likelihood of significant increases in melting and refreezing since the 1960s at this site, driving decadal-scale increases in firn temperature, ice content, and density" on lines 642-644.

Referee # 3

Comments exported from the annotated .pdf file.

Page 1

What about rainfall?

We neglect rainfall in this study, as we don't know the amount of summer rainfall at the site and are not sure how reliable ERA5 estimates of this may be. Summer temperatures are cold (mean JJA temperature of –2.4°C; Figure 5a and Table 2) and over the course of about 10 field seasons working near the site in July (LC, at the Copland Divide camp), we have witnesses numerous summer snowfall events but never a rain event. We therefore think that summer rainfall is rare, but it is likely to occur on occasion, which would be in addition to the melt totals and would also add heat to the snow/firn. This would be a good future consideration to follow up on this work, as summer rain events may become more frequent in future years and decades. We now briefly discuss this in the main text (lines 272-276).

These numbers do not add up to 0.38 m w.e.

Apologies, we mixed the observed vs. modelled ice content, whereas the other numbers quoted are from the model. We revised to report just the model numbers here, for internal consistency. Now revised in the abstract, line 18. This is also refreshed in the main text, lines 390-396, with a more explicit discussion of the amount of liquid meltwater retained in the firn (in the model).

Page 2

Please add some references for these statements.

References added (Sommerfeld and LaChapelle, 1970; Cuffey & Paterson), lines 41-42.

It would be relevant to mention the recent melt water retention intercomparison study in Vandecrux et al. (2020; https://doi.org/10.5194/tc-14-3785-2020).

Reference added as suggested, line 51.

It could be good to mention that this requires cold (deep) firn.

Revised as suggested, line 52.

Page 3

The modelling part of this study could be introduced here too.

Added as suggested, lines 62-63.

Page 5

and

Revised as suggested, line 122.

and

Revised as suggested, line 124.

I am a bit confused here. Figure 2B does present the density profile and I do not see a gap between 13 and 15 m or elsewhere in the record.

Apologies, we reworded this, lines 124-126. Enough samples were intact to estimate the bulk density and plot the density stratigraphy, but because of the greater uncertainty with Core 2, we focus most of the analysis on Core 1 (now stated).

Page 9

Note: How is this done? It is not described in the Supplementary information. Are elevation gradients used?

Sorry to be unclear here, this was done using the elevation difference of the ERA5 grid cell (2522 m) vs that at our site (2640 m), Δz = 118 m, and (i) assuming a temperature lapse rate of $-5°C\ km^{-1}$, as is typical of a glacier surface, (ii) a constant relative humidity, which then informs calculation of specific humidity for the lapsed temperature of the study site, and (iii) air pressure adjustment following $dP = -\rho g \Delta z$, with air density ρ based on the ERA5 temperature. We have added this discussion to the supplement.

There is no mentioning in this section of precipitation in the form of snow accumulation and rainfall, which both are relevant parameters for modelling snow conditions. How are they accounted for?

Sorry to omit this as well – we did not give snow accumulation much attention, but we use the ERA5 total precipitation, updated monthly in the model and assuming that accumulation = precipitation at this site. As noted above, we believe that rainfall is rare at this site, and can be neglected, although future work should test this assumption as occasional summer rainfalls may occur. ERA precipitation totals are low compared to snowpack and firn-core observations of accumulation at the site, so we scale ERA precipitation inputs by a factor of 1.6, giving an average (± 1σ) of 1.83 ± 0.32 m w.e. yr^{-1}. We added a short discussion in the main text, lines 271-276.

Doesn't ERA5 come with hourly resolution?

Yes, true, though we bridged with ERA20th century from 1965-1978 (available every 3 hours) and we also prefer not to work with the large dataset that attends the hourly data, so we chose to work with daily meteorological forcing. Our simple model of diurnal cycles (based on daily Tmax and Tmin, along with daily mean and maximum shortwave radiation) is expedient and computationally efficient in terms of the memory demands of the numerical experiments. While not discussed in the manuscript, we are working towards distributed modelling at a large scale (e.g., St. Elias range; Greenland Ice Sheet), so we are developing melt and firn models that are pragmatic and feasible at that scale. We also have an eye to future projections, where climate model outputs are seldom available with hourly precision, so we are developing and calibrating our methods with daily meteorological forcing.

Page 10

Is heat advection due to accumulation also considered?

No, we neglect this and note that in line 283.

It would have been 'safer' to repeat multiple years rather than one year for intialization.

This is true, although we cover it by including a number of sensitivity experiments around this assumption of a perpetual 1965 climatology. The year 1965 had representative mean annual and mean summer temperatures compared to the long-term means, within 1-σ, and our results are not strongly sensitive to using this year for the model spin-up, but we agree that it would be preferable to use historical forcing for the model spin-up, e.g. the period 1935 to 1965. Now acknowledged, lines 293-295. We could add this experiment if the Editor thinks this to be important, but the range of sensitivity experiments around the initial conditions covers a much larger span of conditions than the interannual variability at the site, so we believe that we have adequately addressed this sensitivity and source of uncertainty.

In fact we considered going back to ERA20c to do exactly this, a spin-up based on the historical climatology, but in looking into this we see that ERA5 has now been extended back to 1950, with a preliminary release of reanalyses for the period 1950 to 1978 in ~February 2021, since the time of our last revision. This offers an opportunity to avoid the splice between ERA20c and ERA5 in our study, and to extend the simulation back to 1950. We have done this, but we suggest not to introduce this to the revisions at this stage, as it would constitute more of a major revision (changes to methods, figures, tables, discussion) and I suspect the appetite for another round of major review is limited after such extensive and helpful work by the reviewers and Editor to date. The back-extension to 1950 using purely ERA5 forcing, with a different spin-up strategy, slightly changes values for things like the decadal trends, etc., but does not change the essential results or conclusions.

For the interest of the Editor and the reviewers, here are the results for the extension back to 1950 with ERA5 forcing now and a spin-up strategy that repeats 1950s climatology for 30 years (3*10 years), before launching into the simulation from 1950 to 2019. Figure R3-1 plots contours of daily firn temperature evolution through this 70-year period and Figure R3-2 plots time series of the main variables of interest in our analysis: summer temperatures, modelled melt, firn temperatures, firn density, and firn ice content. Results are qualitatively unchanged from those shown in Figures 5 and 6 of the manuscript. These new results are arguably cleaner, given that the meteorological forcing is all from ERA5. The results also indicate an interesting warming of the firn to ~7 m depth in 1959, which could be tempting to relate to the early 1960s inference of temperate conditions at this depth. This would be over-interpreting the model though, given the uncertainties within it. Overall, the modelling remains consistent to the results in the manuscript, with reconstructions of decadal-scale firn warming and the likelihood that deep, temperate firn and the PFA are recent features, having developed since 2013. The modelled decadal-scale increases in firn density and ice content are also robust.

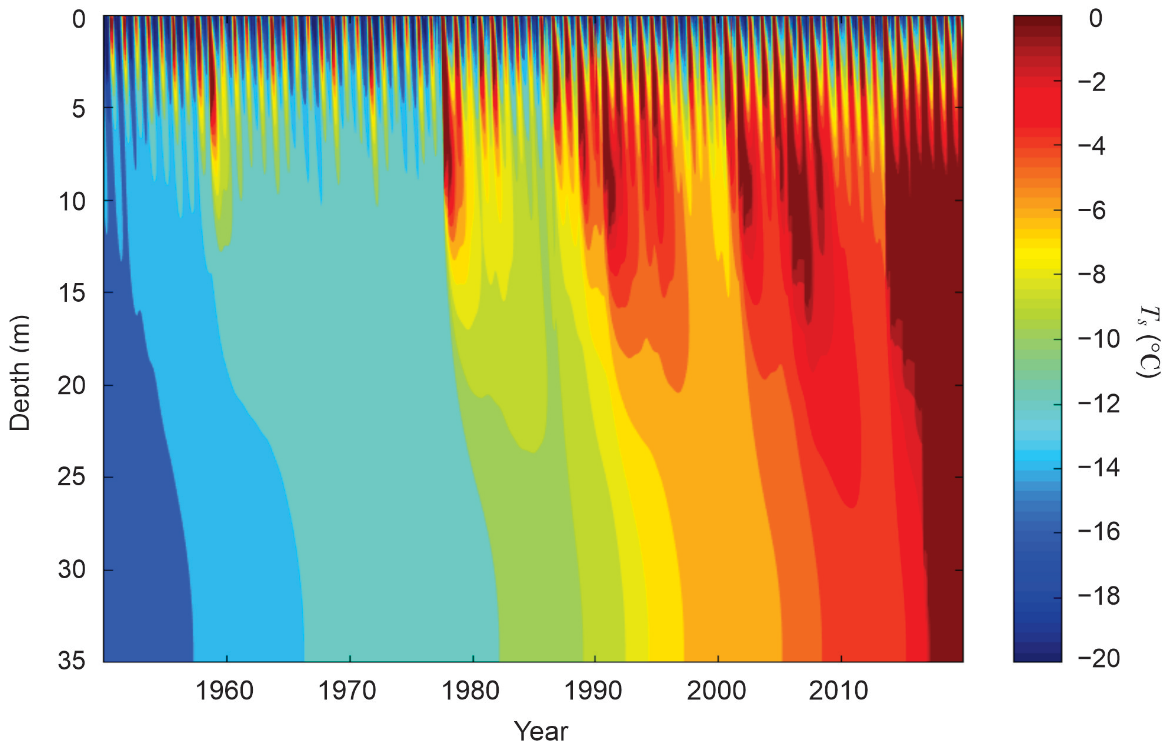

Figure R3-1. Modelled firn temperature to 35 m depth for the years 1950 to 2019, using ERA5 forcing and the reference model parameters.

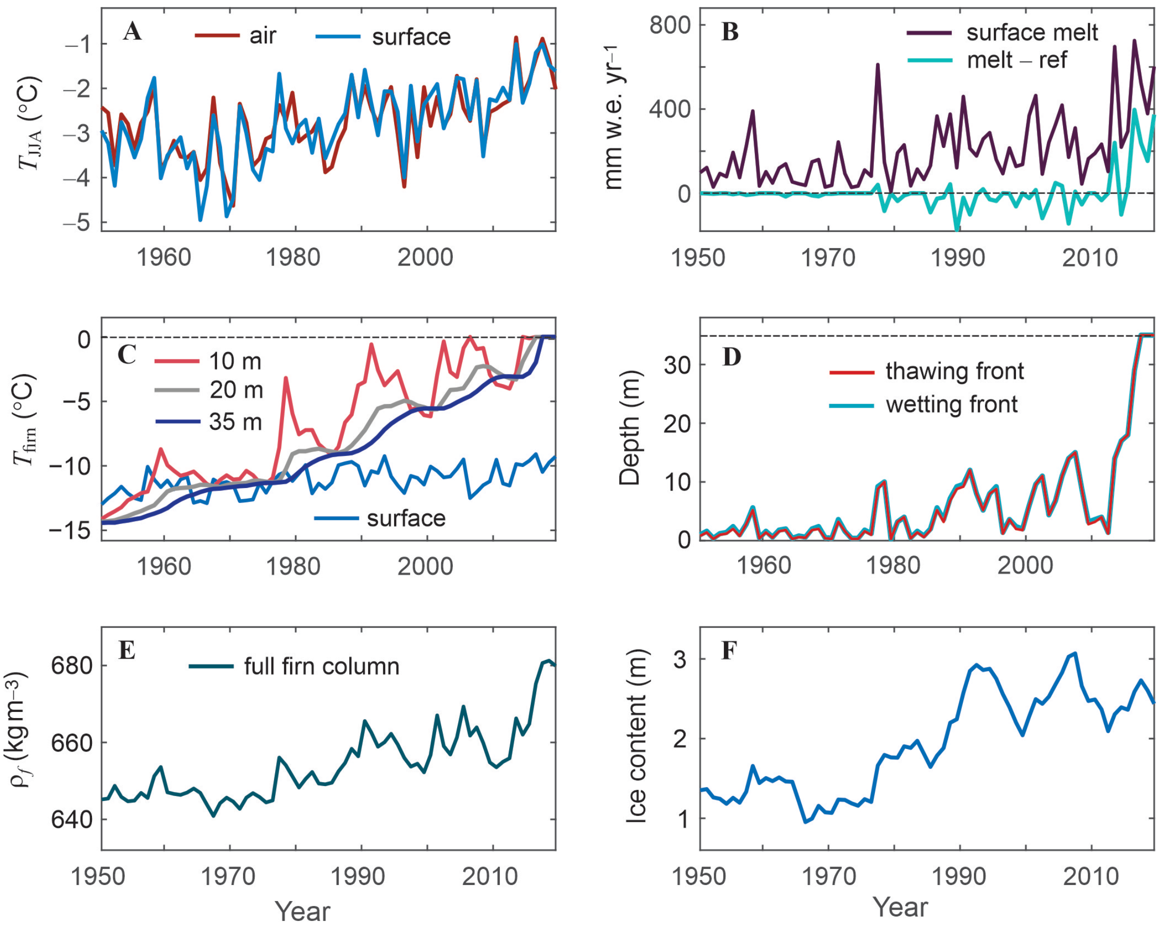

Figure R3-2. Modelled evolution of meteorological and firn conditions from 1950 to 2019 under extended ERA5 forcing. (a) summer (JJA) air and snow surface temperature. (b) Net annual surface melt and the annual melting minus refreezing, which represents drainage to the deep firn. Where negative, this is deeper meltwater that refreezes in the following calendar year. (c) Mean annual snow surface and firn temperatures at 10, 20, and 35 m depth. (d) Maximum depth of the annual thawing and wetting fronts. (e) Average firn density, and (f) Firn ice content.

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The lack of consistency between the density profiles of the two cores is quite striking (Figure 2AB). It could be worth highlighting this more, or could this be mainly the effect of problems with Core 2? Similar strong variability in firn stratigraphy over very short distances has been found in Marchenko et al. (2016, http://dx.doi.org/10.1017/jog.2016.118).

This is also true, and while bulk densities and ice contents are similar (Table 1), the stratigraphies are truly not correlated. We added a sentence on lines 347-350 to discuss this in a bit more detail. It is not likely related to the compromised samples, as the firn ice-layer stratigraphy was conducted in the field, on the full core samples and prior to sawing them into 10-cm sections.

Page 13

This is quite significant given the relatively modest trend in temperature. Is this maybe related to e.g. an increased frequency of warm spells or less summer snowfall?

It is indeed a large trend in surface melting, partly driven by the strong (~3σ) melt years in 2013 and 2017. Overall, the last decade really pulled up the linear trend, and a linear fit might be misleading, though it is statistically significant. These two summers were also anomalously warm (the warmest summers in the 55-year records, at –0.7°C and –0.9°C, or ~ +2σ above normal). While not as exceptional as the melt totals these summers, this relates to the nonlinearity of temperature and net energy effects on melting, particularly at temperatures close to 0°C. We don't examine the effects of summer snowfall within the model – it is treated as a random variable in the albedo model (as described in detail in Marshall and Miller, 2020), but its impact is modest as most summer precipitation events are snow events throughout the period, and the site has remained within the accumulation zone of the glacier, with relatively high values of albedo.

It could be added that the trend roughly matches the observed density trend.

Now noted, line 419-420.

Page 14

Temperature anomalies may also reflect differences in height above the surface the data represent (what atmospheric level do the ERA data represent?). This matters since temperature gradients near the surface are usually strong.

It is true. Within ERA5 and ERA20c, we took the surface-level data, which represents 2-m temperature and dew-point temperature. Surface-level wind speeds in ERA are at 10 m. The AWS temperature data which we use for the bias-adjustment (0.6°C) is also at ~2 m. We added a note the ERA data heights above the surface in the methods section, line 261.

Selected
Revised as suggested, line 432.

obtained
Revised as suggested, line 437. Thanks for catching this.

Was 1965 a relatively cold year compared to the surrounding years?

We discuss this in more detail in the Supplementary Information, but no, the mean summer (JJA) and annual air temperatures in 1965 were almost identical to the 55-year average: –2.5°C and –10.8°C, compared with the 1965-2019 averages of –2.4 ± 0.8°C and –10.7 ± 0.9°C. Incoming solar radiation in JJA 1965 averaged 304 W m^{-2}, compared with a normal of 298 ± 9 W m^{-2}. Net energy and melt were a bit lower than the long-term average, due to lower incoming longwave radiation, 240 vs. 255 W m^{-2} (it seems to have been a clear-sky summer), but overall, 1965 was very representative of the longer-term climatology, particularly for the 1960s through 1990s. We add a short note to this effect here, lines 452-459.

Page 15

I think it is good that this is statement is now included.

Page 16

Also this part is a valuable addition.

Thanks for these comments. We are happier now as well, with the clear and direct discussion of uncertainties and limitations. There is a strong case for further study of this interesting site, given how much we don't know about it.

Page 17

Arguably the 'reference model' could have been set to the one that matches the observations best, but the current approach works well for me too.

We did consider this too, but it is truly hard to force the model to satisfy this observation of intermediate-depth temperate firn in the 1960s – we have to push the climate forcing pretty strongly away from the ERA5 reanalyses, and no combination of model parameters on their own (admitting to some structural uncertainties in our model) were able to produce this. We find the modelling 'best guess' to be a more helpful reference model, from which we can assess how the model forcing, parameters, or physics need to be pushed to match the available observations. This informs flaws in the model, or alternatively raises questions about the validity of the borehole air temperature observation – we feel that both are in question, and hope that this is reflected in the discussion.

This is an important notion, indeed very slight changes in climate have a major impact on the state of firn!

Agreed, we were (are) surprised at how delicately balanced this site, essentially at the transition point from polythermal to temperate conditions.

It could be noted that with densities between 800-900 between 30-40 m there is not much room for a PFA to extend much deeper, since the pore-close of density is in the same range.

Added as suggested, lines 548-550.

Page 18

This may be good to rephrase, the main problem for geodetic mass balance observations is the unknown density (change), which makes it hard to interpret mass change associated with surface lowering.

Thanks for this suggestion, we rewrote this, lines 596-597.

Page 19

Again, what about rainfall? In case it is not accounted for, it should be acknowledged as a source of uncertainty (assuming there are occassional rainfall events at the site).

Agreed – we don't account for rainfall but now acknowledge this in the manuscript (lines 272-276) and also discuss that this could be an important source of uncertainty in a warming climate, which should be considered in follow-up studies at this site. Discussion added on lines 615-620.

Page 20

See my earlier comment, refreezing does not lower the surface, but rather increases the density (which is the troublesome part for geodetic mass balance estimation, which assumes constant density in time). I understand what is meant here, but the way it is formulated is not correct.

Sorry that we continue to struggle with wording this clearly. Revised, lines 656-659.

Page 27

Does this include rainfall or not?

No, rainfall is neglected and is assumed to be negligible – now directly discussed in the text, per the comment above. We also add a note to this effect in the Table, line 874.

The term 'net melt' term is slightly confusing. How to distinguish between freeze-thaw cycles and refreezing at greater depths? With 'melt', 'refreeze' and 'drainage' there is a nice set of variables that is mass-conserving, so there is no need to have another term.

Yes and no. We are also struggling to define this. We agree on melt, refreeze, and drainage – these are all well-defined and refer to the full snow/firn column. And yes, they conserve the overall system mass and energy. Net melt is something important though, which we evidently need a different name for. We thought about 'surface melt', but all of the melting is surface melt. Net melt is the actual surface ablation: the mass loss and drawdown of the surface due to melting (though not ablation in the proper sense, which also includes sublimation, wind scour, etc). Surface ablation or mass loss of the surface layer due to melting is less than the total melting, due to meltwater that is retained in the near-surface (irreducible water content) and undergoes successive freeze-thaw cycles. The same water molecules are melted many, many times by this process, as there are diurnal freeze-thaw cycles throughout the summer at this site. This consumes a significant fraction of the net energy that is available for melting – a portion of this net energy is directed to 'recycled' water/ice, essentially. As a result, the 'net melt' is less than the total melt: about 44% of total melt at this site. (It is closer to 90% at a temperate glacier site where we have studied this process, Samimi and Marshall, 2017). This is important because this is the actual amount of mass that is transferred to deeper snow and firn, as meltwater infiltration. We are sticking with the term 'net melt', for lack of a better idea here, and by analogy with e.g., total vs. net income. But we appreciate that it is not very clear, so we have attempted to better explain this, lines 869-874.

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We removed this, thank you.