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Interactive comment on "A new spatially and temporally variable sigma parameter in degree-day melt modelling of the Greenland Ice Sheet 1870–2013" *by* A. E. Jowett et al.

Anonymous Referee #2

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Dear Authors,

First of all, please accept my apologies for the time taken to deliver this commentary. I will proceed to break the review up into the following sections:

1. Reader synopsis 2. Major comments 3. Minor comments, grammar and spelling 4. Figures (comments and errors)

1. Reader Synopsis

Jowett et al. (2015) present a series of temperature variability for the Greenland Ice Sheet that are derived from two sets of model output (20CR and ECMWF) for the

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period 1870-2013. The length of the time series allows us to assess the magnitude and spatial patterns of temperature variability across Greenland during a period of climatic warming and see if any discernible trends in variability arise from the model output. Before the data can be used to assess such trends and variability, the model output from ECMWF ERA-1 reanalysis and 20CR are downscaled to 5 x 5 km cells from their coarse 80km and 2 degree (respectively) grids by (1) Performing a bilinear interpolation to a 5 x 5km grid (2) Correcting for orography differences between the DEM to which the temperatures are interpolated to and the DEM which is used in the climate reanalyses (3) Splicing the 20CR and ECMWF-ERA 1 datasets to create one long-term series by applying a DC shift to the 20CR data to bring these into line with average monthly values from the ECMWF-ERA-1 dataset (4) Using change-point analysis to detect artificial shifts in the temperature model output related to increases in constraining observations used in model synthesis and then correcting for these. Once these corrections have been applied, the variability of temperature variability (for want of a better phrase) is assessed as a function of (1) Time (1870-2013) and season (2) space (Data are time averaged in this instance) (3) How well modelled sigma fit with in-situ observations from GC-Net, DMI, PROMICE AWS networks. (4) Spatial patterns of observed-modelled sigma error (as a function of elevation and latitude) (5) As a function of climate indices (GBI and NAO).

Outcomes: This paper reinforces recent work that temperature-index models, although still valuable, need to move away from the application of a constant sigma value of 4C to model temperature variability. It also shows that that temperature variability across the GrIS has increased since 1870. This is a novel finding, underpinned by (subject to clarification of Point 1, major comment) robust data pre-processing. The rest of the manuscript shows the results of analysis that repeat and reinforce the overall spatio-temporal patterns of sigma shown in e.g. Fausto, Seguinot, Wake and Marshall papers (e.g. sigma varies in a sinusoidal fashion over a year; there is a relationship with elevation and latitude). In this respect, some areas of the paper are not novel. The fact that this analysis derives from a very long time series of model output IS novel, and the

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results are intriguing.

2. Major Comments:

Point 1: **Correction of 20CR and ERA-I reanalyses**.

As it stands, I am a little uncomfortable with the methodology (or at least the explanation of it) surrounding the correction of the temperatures, particularly the change point analysis step. Incidentally, I think that such an analysis is interesting and is possibly a paper in itself. But since it makes up a significant part of this submission, I think it needs to be explained more clearly that in its current form.

Page 5334, Line 21: *To correct for any artificial breakpoints, the mean 2m air temperature was calculated for the periods both before and after the breakpoint. The difference between the two means was then added to each year as an inhomogeneity correction in the period before the artificial breakpoint, thus bringing 25 the 2m air temperatures of the period before the break in line with those afterwards, removing the artificial step change in temperature*

Questions:

(i) If all we are looking at is standard deviation of monthly temperature (i.e. the 'spread' of 3-(ERA) and 6-Hourly temperatures over a month), then why is it necessary to go through the stages of applying what appear to be simple, temperature invariant +/- corrections to the data? From what I can tell (in the absence of equations which would probably make things clearer to the reader, and render this criticism invalid), all that is applied to the CR20 data is a series of DC temperature shifts. Surely this just shifts the 6-hourly temperatures up and down throughout the time series and doesn't shrink/expand the range of temperatures over time? I think it would be an interesting addition to see how time series of ice sheet averaged sigma varies between the two datasets for the period of overlap, instead of just focussing on time and spatially averaged means of sigma for 20CR and ERA-I.

(ii) In the statement above, what do you mean by "period"? Over what length of "period" do you calculate means that you then difference?

(iii) Criterion for detection of artificial change points is that the confidence interval (CI) of temperature change point overlaps with that of the CI of the spread of the temperature reconstruction ensemble. How do you assess the possibility that during some periods that there is no relationship and that temperature jumps are real?

(iv) You state that in the 1940s/1950s North American CR20 data that the inhomogeneities vary regionally. Does this happen in Greenland and did you correct the time series of the temperatures for the change points caused by inhomogeneities on a pixel-by-pixel basis?

(v) The text does not explain how you dealt with applying corrections in the instance where (e.g. Fig 2b, c, d, e) the temperature change point CI overlaps with CIs of several change points calculated for the spread data.

In conclusion, I think one or two equations are definitely needed in this section to clarify the points above.

Point 2. Manuscript title.

The title of the paper is: "A new spatially and temporally variable sigma parameter in degree-day melt". This leads me to believe that the paper presents a unified spatio-temporal representation of sigma that can be applied easily in ice sheet models. It doesn't. What the paper does show, using model output, is that sigma varies significantly as a function of time and space and that it would be more accurate to input a time series of spatially varying monthly sigma fields directly into a Greenland Ice Sheet SMB calculation for 1870-present. This would take a fair bit of computational grunt compared to the sigma parameterisations of Fausto (2009) and of Wake and Marshall (2015) and would likely arrive at broadly similar PDD totals maybe not, who knows?) However, I think the title needs represent the results and outomces a bit more truthfully.

Point 3. Introduction.

A suggestion. There is a lot of text that can be omitted from the introduction and be replaced by citations from the original sources. E.g. Lines 1-20 (page 5329) and Lines 1-5 (page 5330) can be shortened. Possibly use this extra space to address major comment 1?

3. Minor Comments: Page 5330: Line 15: "Not a direct function of surface temperature" -> "Not wholly a function of temperature". Sensible Heat Fluxes are partly (but directly) controlled by changes in surface temperature.

Page 5331: Sequinot -> Seguinot

Page 5332: Line 2: Agree.

Page 5333: Line 19: There are other higher resolution DEMs available for Greenland (e.g. Morlighem, 2014). What is the reasoning for correcting to Ekholm's 1996 version?

Page 5335, Line 1: Where have you fed them into the PDD model? I don't see any PDD results in the manuscript. I would also argue that this is a *less* sophisticated way of feeding in sigma to mass balance calculations due to the sheer amount of fields being read in during a 170 year SMB calculation. It is probably marginally more realistic, however that other parameterisations.

Page 5336: Line 2: omit "graphically". It is an unnecessary adverb.

Page 5336: Line 3: please explain more clearly what 'spread data' means.

Page5336: Line 4: Is shows that they clearly "coincide", not necessarily "caused by".

Page 5337: Line 14: Why no break points detected in winter? This warrants a sentence of explanation/insight.

Page 5338: Line 22: "Averaged out" -> "ice sheet averaged?"

Page5338: Line 25: You are using 'standard deviation' (Figure 7 caption) interchangeably with ‘standard error’. They aren’t the same. Standard Error of the Mean (SEM) = SD/sqrt(N), Where SD=standard deviation and N is number of observations, so you take into account the number of observations in the SEM calculation. It means that you know the mean less well with lower N, hence a large standard error. Either change the text or Figure 7 caption to reflect whether the bars on the graph display SEM or 1-SD (63

Page 5339: Line 9: I would call it statistically significant **weak** agreement (for July and Summer).

Page 5339: Line 24: You’ve sort of said the same thing twice regarding the north bias. It “shifts” and “shifting to negative”. Rephrase.

Page 5340: Line 24: Fig. 11g doesn’t exist.

Page 5340: Line 27: Fig. 11h doesn’t exist.

Page 5341: Line 6: “Blues and Purples” - non-scientific description. Delete.

Page 5341: Line 8: Where are the negative sigma trends in this plot? As far as I can see the variability is increasing in all areas as the scale bar runs from 0 to +0.05 (plus there is a labelling error - see Figure comments section below).

Page 5341: Line 8-13: Either change the scale bar in Figure 12 to degC/decade or change the text here to degC/year for parity.

Line 5342: Line 1: You haven’t performed this analysis in this paper.

Line 5342: Line 15: Are all of these references required? Checking up, the Wake reference is a mass balance time series that didn’t make use of 20CR, Hanna et al. 2011 did. Check the relevance of these to the statement that has been made.

Page 5344: Line 2: “Effects of continentality” - expand a little here.

Page 5344: Line 12: See also Box, J.E. (2006) Greenland ice sheet surface mass-balance variability: 1991-2003. Annals of Glaciology, 42(1), 90-94. (doi: DOI:10.3189/172756405781812772)

Page 5347: Line 3: Where are these negative trends?

Page 5348: Line 7: But this procedure doesn't alter variability, which is the focus of the paper.

Page 5348: Line 21: Agree.

4. Figure Comments

1. Clear figure. No comments.

2. This is a very interesting plot. What this shows is that over time (1860-present), the spread of temperature across 20CR GrIS ensemble reduces, presumably as more observations become available for later years to constrain model output. Change point analysis consistently identifies 3 periods where there is a change point in the ensemble spread series over time: (1920,1940,1965). Where these change points overlap with those detected in ice-sheet averaged monthly temperature for (MAASO months) the authors identify this as an artificial shift forced by an increase in constraining temperature observations.

Spelling error in caption: 'corretion' -> "correction"

3. The result of pre-processing the 20CR dataset. Largest corrections made to data in the summer months. I think this figure can be reformatted as follows to make the effect of the corrections clearer: i) The first line (black-dashed) for raw, uncorrected 20CR data. ii) The second line: 20CR data corrected for orography (e.g. green line) iii) The third line: 20CR data corrected for topography (ii, as above) and the DC shift to ERA-data (e.g. a blue line) iv) The fourth line: 20CR data as corrected in steps ii) and iii) but with break point corrections (inhomogeneity) applied (red line; i.e. your final corrected 20CR series) If this is what the graphs show, please make it clearer in the figure caption that this is what you have done, and add I suggest add the original 20CR

series for comparison.

4. Clear image

5. Clear image. Have you tested this resultant series for change points? It looks like 1920 shows that variability started to increase all across the ice sheet, variability stabilised around 1950 and then dropped after 1980 (although could the latter still be an artefact of the splicing?)

6. Clear image, although I am not sure what added value is achieved by having separate summer and July plots (and not June and August). They look (and I'd expect them to be) broadly the same.

7. This is a plot that is very similar to that shown in Fausto's 2009 paper and in the recent Wake and Marshall (2015) paper. I think what you are trying to show with this plot is the characteristics of the sigma distribution over 1870-2013, by month. I think it would look better (and would stand out from previous work on this) if you plotted for each month, the histogram of ice sheet sigma values. Also see Minor comment section regarding use of term standard deviation and standard error.

i) Error - you have your 'min' and 'max' the wrong way round. ii) Delete the last sentence. Observations not needed in the figure caption when they are already in the text.

8. Clear plot. Correlation coefficient of observed vs. modelled sigma. I am not convinced that this plot needs a differentiation of July from the rest of the summer. For low observed sigma of 1-2C (e.g. in the summer months), the modelled sigma doesn't show any trend (e.g. it fluctuates between 1 and 3.5C), but for observed values of sigma > 2.5C, there does seem to be a stronger positive correlation, e.g. like a 'hockey stick'. At lower observed sigma (i.e. higher temperatures), I wonder if the model is not simulating well the stagnating temperature near the ice surface well or; (and you can maybe investigate this) that the model output maybe closer to truth and that there

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is a systematic difference between AWS instrumentation used across the monitoring networks?

Also, add p-values to these correlations or in the Figure caption. I know you've mentioned them in the text but in my opinion it is also good to be able to tell the story of a manuscript via the figures alone.

9. Clear figure. Higher elevations, modelled sigma < observed sigma; Lower elevations, modelled sigma > observed sigma. Add p-values to image/caption.

10. Clear figure. I wouldn't have expected a strong latitudinal pattern Add p-values to image/caption.

Figure 9 and 10: Y label should have the same text. Modelled-observed sigma?

11. Clear figure, but the p-value plots are unnecessary. The p-values can be displayed as contours on the trend plots, I think. Then you can make these images larger. Overall, variability in temperature across the ice sheet is increasing (significantly so). I think you could do with re-labelling the figures. There are two a), two b) etc. because you refer to a Figure 11g h in the text. These don't exist.

12. Clear figure but has errors. If this shows spatial trends, the scale bar should be labelled "C/yr" for 1990-2013. Also, on page 5341 (line 8) you claim that this plot shows negative trends. I can't see any. The white area on the scale bar might denote negative trends, but how can you differentiate this colour from the land areas that you have shaded white?

Since this time period (1990-2013) covers the observational period in Greenland, it would be useful to see, time series of sigma trends from AWSs (GC-Net, PROMICE, DMI) plotted against time series of sigma from the nearest pixel to see whether the model is robustly capturing what is happening on the ground over time rather than just presenting model-observation differences of multi-annual averaged monthly sigma values (e.g. Table 2).

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13. Clear figure and exposes significant differences between sigma calculated from the two datasets in the summer months, which is crucial to overall PDD calculation. On this plot, please add a 1:1 line, and make the x and y axes equal in length, and with equal limits.

14. Clear figure. Add p-value as contours.

Interactive comment on The Cryosphere Discuss., 9, 5327, 2015.