Referee 1 general comment: 1- Timescale

The development of the timescale is the crux of this study and it is clear that considerable effort has gone into the timescale. The paper makes it clear, without explicitly stating so, that the timescales was quite difficult to develop. I have great sympathy for anyone who develops ice-core timescales. However, the timescale as presented is not convincing for two reasons. First, the isotope sampling is too low to resolve annual layers for much of the core. At a sampling interval of 10cm (above 80m), this yields only 5 or 6 samples per year for much of the timescale given the accumulation rates. You need about twice that to resolve clear annual layers, especially on a proxy such as oxygen isotopes that have relatively noisy seasonal cycles. Statements like “no ambiguity in layer counting is detectable above 62.38 m (i.e. 1933 AD)” are in direct contradiction with the need to perform major ion analysis “for sections of unclear isotopic seasonality” and I can see ambiguity in Figure 2 (near 20 and 29 m depths).

It seems odd to me that for a relatively short core, the whole thing wasn’t sampled at much finer resolution (water isotope analyses are cheap and don’t need much ice) and that aerosol analysis wasn’t performed on the full core.

Second, the volcanic matches are not convincing. In Figure 4 it appears that any small peak that rises past the 2sigma level is considered a volcano if it happens to be of the correct age. This may be because the data are normalized before identifying volcanic peaks. Regardless of the normalization issue, using ECM (or sulfur) at coastal sites to identify volcanic events is very difficult because the volcanic signal gets overwhelmed by marine inputs.

The timescale is the crux of this paper. This means considerably more effort needs to be made to describe it convincingly, for both the annual layers and volcanic matches.

Items that this paper needs:

1) A clear description of what measurements were made at what depths (i.e. where were aerosols measured and show the ambiguities and how they were interpreted)

2) An analysis of the impact of the low sampling resolution on the ability to resolve annual layers (often, low resolution leads to picking false peaks)

3) A realistic assessment of annual-layer interpretation uncertainty

4) A critical assessment of volcanic matches. I.e. why is Cerro Azul 1932 not one of the bigger, yet unmatched, peaks about a meter above or below. (this same question applies to pretty much every match, except for possibly Tambora).

5) A description of why ECM loses the annual signal yet preserves the volcanic signal

6) Why Krakatau isn’t observable in the ECM record and what the distinctive characteristics in the aerosol record are that allow it to be identified. Also a description of why the technique to identify Kratatau wasn’t applied for the full core.

It sounds like the only truly identifiable volcanic event was Tambora. The authors need to make use of Tambora, and pattern with the unknown 1809 eruption, to make a strong case that this is indeed properly matched (Figure 4 does not do this). Plot it against high resolution ECM/Sulfur/Sulfate records of this event. If the authors can demonstrate that this is a clearly identifiable match, then it would strongly support their annual layer interpretation.

Author’s response:

We respond to this comment in three steps. First, we assess the referee’s comments concerning annual layer counting. Second, we discuss the volcanic matches. Third, we address each specific item that Referee 1 suggested we consider or implement.

First, we point out that the isotope sampling resolution reported in the original manuscript was not 10 cm everywhere above 80 m. To explore this, we have nowe calculated the number of samples per year and report these in Figure R1. Other studies have worked within this range (e.g., Schlosser and Oerter, 2002). We agree that it should be stated more clearly which resolution was used at which depth, and have now added the full isotope profile with a visual indicator *of the resolution as two supplementary figures: Fig. S1 and Fig. S2. Ambiguities are now highlighted and discussed. At some depths, (e.g., between ~74 and 77 m) we increased the resolution to 5 cm, but – with an annual layer thickness of several tens of cm, in no case did the higher resolution data actually improve in the identification of annual layers. Therefore, we have not made more isotopic measurements. However, we did measure ECM at high resolution all along the core and this can be used to identify annual layers as well. It is a combination of both methods, supplemented by ionic measurements where available, that gives us confidence in our annual layer counting. For example, the ambiguities observed by Referee 1 "near 20 and 29 m depth" in $\delta^{18}O$ are resolved at 20 m by looking at the synchronous peaks in MSA, nssSO_4 and NO_3^-, and at 29 m by looking at the synchronous peaks NO_3^-, ECM and the trough in the $Na^+/SO_4^=$ ratio. However, we do agree with the reviewer that ambiguities remain elsewhere, and this is precisely why we adopted (and have retained) the approach of two age estimates: youngest and oldest. The sentence "no ambiguity in layer counting is detectable above 62.38 m (i.e. 1933 AD)" has been removed. It is now stated that this method has a ±16 year uncertainty at the base of the ice core. This new 'uncertainty' is the result of us considering the potential issues raised by Reviewer 1 and working through the entire record again in a more "conservative" way (described in Methods). Therefore, and despite the fact that the Tambora eruption still confirms the oldest estimate (see below), we added an analysis of the impact of this 16 years dating error on the trends reported in the paper in all figures and tables.*

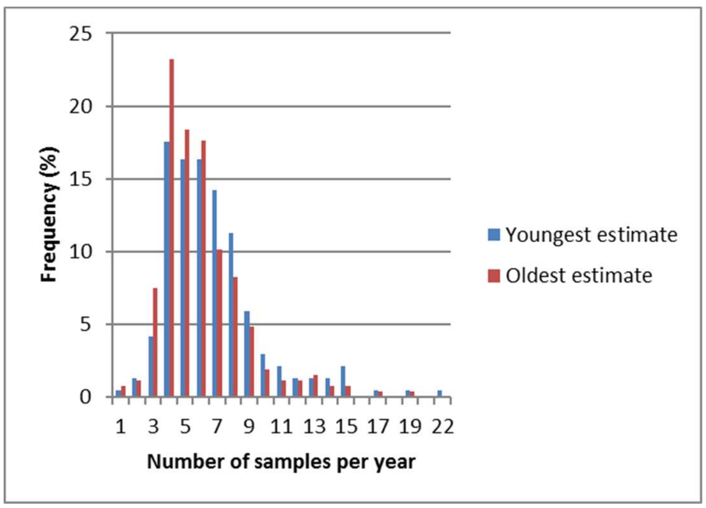

Figure R1. Distribution of the number of samples per year for the youngest and oldest estimates

Second, we agree with the referee that volcanic matching in a coastal ice core is, although not impossible (e.g. Kaczmarska et al., 2004), very difficult, even though we used ECM in combination with the annual layer counting. Therefore, we have chosen a simpler "conservative approach" along the lines suggested by the referee, i.e. only focusing on the Tambora eruption signature. In the revised manuscript we moved the high resolution ECM profile to a supplementary figure and added into the manuscript proper a figure centered on the depth range corresponding to where Tambora eruption should be visible, according to our two estimates (Fig.5). We also went back to the laboratory and made additional major ion measurements to document the Tambora eruption and show these on the same graph. We were very pleased to discover and report that there is only one peak that crosses the ECM 4 sigma threshold in the expected depth range and that it occurs at a depth corresponding precisely to our oldest age-depth estimate.

While these changes do not influence our conclusions, they do improve confidence in them and we thank Reviewer 1 for pointing us in this direction.

We now discuss each specific item suggested or requested by Referee 1:

1) A clear description of what measurements were made at which depths (i.e. where were aerosols measured and show the ambiguities and how they were interpreted)

This is achieved by the full isotope profile as Fig. S1 and Fig. S2, including a visual indicator of the resolution and an explicit indication of the annual layer boundaries identified according to the two estimates.

2) An analysis of the impact of the low sampling resolution on the ability to resolve annual layers (often, low resolution leads to picking false peaks)

This is now done with the youngest estimate, which only interprets the minimum number of annual layers.

3) A realistic assessment of annual-layer interpretation uncertainty

This is also addressed by the youngest and oldest estimates.

4) A critical assessment of volcanic matches. I.e. why is Cerro Azul 1932 not one of the bigger, yet unmatched, peaks about a meter above or below. (this same question applies to pretty much every match, except for possibly Tambora).

As explained above, in the revised manuscript we have focused solely on identifying the most distinctive peak, that of Tambora. However, for information, the oldest estimate in our revised manuscript would now be 3 years older at the same depth and Cerro Azul does indeed correspond to the peak at 61 m. We do not discuss this in the revised manuscript since it occurs in a section of the core where the mismatch between our older and younger estimates is still reasonably low (±2 years).

5) A description of why ECM loses the annual signal yet preserves the volcanic signal

ECM loses the annual signal and the volcanic signal only in some sections of the record, e.g. between 83 and 85 m. This could result from a variety of factors that we do not discuss because ECM seasonality is only used as a back-up where needed and not as a primary source of information.

6) Why Krakatau isn't observable in the ECM record and what the distinctive characteristics in the aerosol record are that allow it to be identified. Also a description of why the technique to identify Krakatau wasn't applied for the full core.

It sounds like the only truly identifiable volcanic event was Tambora. The authors need to make use of Tambora, and pattern with the unknown 1809 eruption, to make a strong case that this is indeed properly matched (Figure 4 does not do this). Plot it against high resolution ECM/Sulfur/Sulfate records of this event. If the authors can demonstrate that this is a clearly identifiable match, then it would strongly support their annual layer interpretation.

The characteristics of a volcanic peak are now shown only for Tambora, with $nssSO_4$ *and* $SO_4^{=}/Na^{+}$*, that also show a peak, outside the seasonal variations and synchronous with the ECM record. We agree with the referee in believing that the ECM signal is potentially subject to too much influence by marine inputs to act as an unambiguous indicator for many of the other peaks. We thank the reviewer for these observations and the Methods and Discussion sections of the revised manuscript have been changed accordingly.*

The Tambora peak and the associated ion record can now be seen in Fig. 5. No other peak above or below could be associated with this eruption. We associate it to a clear peak in $SO_4^{=}/Na^{+}$ *which occurs between two seasonal peaks and corresponds to high* $nssSO_4$ *value (3.3 times higher than the mean). We believe this new conservative approach is scientifically robust and lends strength to our oldest estimate of the time scale involved.*

Referee 1 general comment: 2- Description of the layer thinning correction

The corrections for flow-induced layer thinning reveal a lack of understanding of how ice flow and are clearly underestimated. In particular, it is disappointing that the authors don't make use of the detailed ice-flow modeling that's been done on Derwael Ice Rise (Drews et al., 2015) to develop the vertical thinning function. It is clear from phase sensitive radar measurements (Kingslake et al., 2014) that the simple approximations for vertical thinning have trouble replicating the vertical strain pattern under ice divides.

The Nye assumption is so obviously not applicable to Derwael Ice Rise, which has a distinctive Raymond Bump, that it should not even be considered. The authors don't supply the kink height value of the D-J model. Using a kink height of nearly 1, Kingslake et al. could still not match the pattern under Roosevelt Island. Since the authors say the kink height is below the zone of interest, I can infer that they didn't use anything greater than ~0.7. This will lead to an underestimation of the amount of strain experienced by the ice in the core. Thus the older accumulation rates will be underestimated, and it will appear that there has been an increase in modern accumulation rates. The underestimation is likely exacerbated by the preferred thinning rate of 3cm per year for the ice rise (Drews et al., 2015). Getting the thinning right is critical to primary conclusion of this paper.

<u>Author's response</u>

We thank the referee for this remark, which is certainly relevant and important. However, as we will show below, the effect of taking the Raymond effect into account does not alter the main conclusions of the manuscript.

First, we removed the Nye time scale approach from the revised manuscript, which is – as rightly pointed out by the referee – much too simplistic to be valid at the ice divide of an ice rise (we actually initially chose to show it to demonstrate the importance of using a more refined and adequate approach). Also as suggested by the reviewer, we took the vertical velocity profile from Drews et al. (JGR, 2015), which takes into account the Raymond effect on the Derwael Ice Rise through a full Stokes approach, as well as a slight amount of thinning (although the thinning is not the main factor to obtain the best fit) and ice anisotropy. This Drews profile indeed yields the best match with radar layers at depth. However, the Drews et al. (2015) strain rate profile used a mean accumulation rate that is somewhat lower that the long-term accumulation rate we obtain from the ice core. In order to determine the long-term accumulation rate we relied on an independent measure of horizontal surface strain measured on the Derwael Ice Rise. From a hexagonal strain network, we calculated horizontal strain rates ($\varepsilon_{xx} + \varepsilon_{yy}$) to be equal to 0.002 a^{-1}. Mass conservation then gives a vertical strain rate at the surface of -0.002 a^{-1}. The vertical velocity profile was then scaled to match the measured vertical strain rate at the surface. A best fit to the measured radar layers was obtained with a value of a mean accumulation rate of 0.55 m a^{-1} ice equivalent (see Fig. R2 below and Fig. 2 in the revised manuscript).

As an alternative approach, we used the Dansgaard-Johnsen model to fit the characteristics at the ice divide, as exhibited by the Raymond effect. Assuming that the horizontal velocity is zero, the vertical velocity is maximum at the surface, where it equals the accumulation rate (with negative sign), and is zero at the bed. Assuming a vertical surface strain rate of -0.002 a^{-1}, we can determine the location of the kink point (between constant strain rate above and a strain rate linearly decreasing with depth below) that obeys these conditions (Cuffey and Paterson, 2010). This approach indicates that the kink point lies at 0.9H, where H is the ice thickness. As seen in Fig. 2b, this method yields a vertical strain pattern that is consistent with that of Drews et al. (2015), especially in the first 120 m corresponding to the length of the ice core.

Both strain rates (Drews/D-J) were then used to correct the ice equivalent layer thickness for strain thinning. Layer thicknesses were then converted in from ice equivalent to w.e. for easier comparison with other studies.

While these results still conform to the previous conclusions of the paper, they are more robust and we thank the reviewer again for raising this issue. Figure 6 of the revised manuscript has been adapted to include this new, more physically sound, approach. We would like to point out that this paper is one of the few that actually investigates the impact of deformation on annual layer thicknesses in such details. We also now include Reinhard Drews as one of the co-authors.

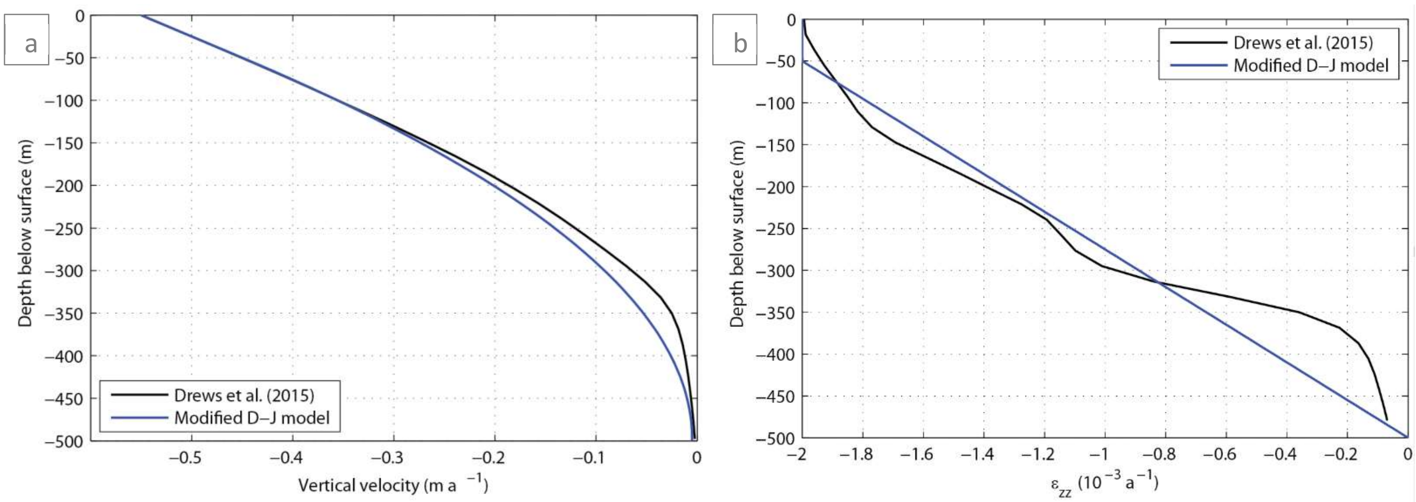

Figure R2. Vertical velocity (a) and vertical strain rate (b) profiles, according to the modified Dansgaard-Johnsen model (blue) and the full stokes model (black, Drews et al., 2015).

<u>Refereee 1 general comment: 3- Climate implications</u>

The discussion of atmospheric and sea-ice patterns seems like an after thought. I'm not sure why the authors choose to analyze only anomalously high and low years. I also wonder why the authors don't compare the inferred accumulation rate history to the climate reanalyses. Other cores (e.g. Medley et al., 2013, GRL; Morris et al., 2015, Nature Geosciences) find good correlation of annual accumulation.

<u>Author's response:</u>

In the revised manuscript, we have framed the discussion of the relation between core-derived SMB and climate parameters better. We now compare the ice-core-derived SMB with P-E estimates from ERA-Interim and RACMO2. The correlation is moderate for both (R^2=0.36 and 0.5 for ERA-Interim and RACMO2, respectively), compared to other ice cores in West Antarctica, which indicates that local wind-induced snow redistribution and sublimation are significant contributors to local SMB at the ice core site (Lenaerts et al., 2014). Subtle variations in wind speed and direction could lead to strong perturbations of the snow accumulation, especially at a wind-exposed site such as Derwael Ice Rise. However, it is unlikely that the wind has an impact on the temporal trend observed in IC12. Unfortunately, our methods do not allow explicit partitioning of the SMB explained by precipitation vs. wind processes. Therefore, we compared with CESM rather than with the reanalyses data because (1) it yields an SMB and climate time series that overlaps substantially with the ice core record (1850-2012), unlike the reanalyses that only covers ~35 years, and (2) the present-day climate and SMB are realistic (Lenaerts et al., 2016). This is now clearly indicated in the text of the revised manuscript.

We now address the specific comments made by Referee 1.

Referee 1 specific comments	**Author's response**
P1, L27-29 – your data do not actually support this because your thinning correction is much too small.	*We revised the complete strain correction by using the Drews et al. (2015) strain rates and a modified D-J model (discussed in detail above). Both are further constrained by measured surface horizontal strain rates.* *This amendment has not altered our conclusions.*
P2, L10 – you should mention timescales. Frieler et al only address glacial-interglacial changes. The most directly comparable ice-core record to yours is from Law Dome, which does not show a consistent	*We added the precision "during glacial-interglacial changes" and took more care at mentioning timescales in the revised manuscript.*

relationship between accumulation and temperature in the Holocene.	
P3L1 – in the ice-core community, continuous is generally used to mean a melting system where discrete samples are not cut. Using continuous to mean that all of the core has been sampled discreetly is confusing.	*Amended*
P3,L13 – Be specific about what you mean by local ice flow. You should really mention that it's an ice rise with a well developed Raymond Bump that has likely been stable for thousands of years.	*This is now described in Paragraph 2.1*
P4,L25 – DC-ECM does not depend on the impurity content at the crystal boundaries. It depends on the acidity.	*Rectified*
P4,L28 – a ~30cm smoothing window seems really large to me.	*We also tested with a smaller smoothing window (101 and 201) and we chose 301 points in an attempt to reduce the noise from the marine input. This does not have an impact on the Tambora volcanic horizon we discuss.*
P5,L1-3 – Did you normalize the data before identifying the volcanic peaks? If so, you can no longer reliably identify volcanic events with a threshold because the increased conductance of volcanic events would impact the normalization. Is the 2sigma threshold then for the entire data set. I'm confused, but I think this is a major problem.	*We applied the method described in Karlof et al. (2000) and Kaczmarska et al. (2004): "The Savitsky-Golay filter eliminates peaks created due to random noise or short-term chemistry events but preserves peaks expected from volcanic events."* *We now use a 4σ threshold instead of the 2σ.*
P5,L5-22 – This section should be entirely redone. Get a thinning function from the ice-flow modelers working on Derwael Ice Rise.	*Amended (discussed in detail above)*
P6,L17 – explain what changes in the ECM and why	*We changed the sentence by: "For ECM, there is also a regular seasonal signal, but it becomes very noisy below 80 m, although some seasonal cycles can still be seen for example between 115 and 118 m (Suppl. Fig. 1)"*
P6,L27 – explain how Krakatau was identified	*This sentence was removed. See response to general comment nr 1, specific items nr 6.*
P10,L11 – why are the uncertainties being presented after the results	*We modified the structure of the discussion and moved the paragraph about uncertainties to the end of the Results section.*
P10,L30 – there is a lot complexity in the position of the divide, the Raymond Bump, and the minimum accumulation (which is offset from the divide). There needs to be a much more detailed discussion of whether small (i.e. one ice thickness)	*This comment has been clipped (the last sentence is not finished) but we understand that the reviewer suggests we explain the small-scale variability of the SMB near ice divides in more detail. We have amended sections 2.1 and 3.3 accordingly.*

Referee 2 general comments:

I have only basic knowledge in dating ice cores using flow models, so I cannot assess the critics of referee #1 considering this point. The authors do show both the uncorrected data and the correction with the different models, so the reader can assess what they have done. Also, their main conclusion (positive SMB trend in the last 100 years) would still be valid for any calculation of layer thinning that lies between the two methods they use.

However, I share Referee #1's doubts about the details of the dating, particularly the use of volcanic horizons, since the attribution of the ECM peaks in Figure 4 to the different eruptions is not convincing, except for Tambora. Also, the authors do not give details about the layer counting using stable isotopes, to which depth this was possible etc. Nobody expects a perfect dating of an ice core because this hardly ever exists.

However, I think the authors should discuss the error possibilities of the dating a bit more and give a more realistic quantitative estimate of the error. Probably, within the error bounds, their main result would hold. But, see above, I cannot assess the details of the used models. The authors state that their findings (increase in SMB in a coastal East Antarctic core) are the first ones that support model predictions. This does not make them discuss how representative their results are. They compare their results with other firn/ice cores, but do not compare the temporal variations of the SMB derived from the core with temporal variations of measured and/or modelled air temperature, sea ice, or surface pressure data). Instead they look at composites for very positive and very negative years, which is, in principal, not a bad thing to do, but I would expect stronger signals here in order to be convincing. The arguments using the output from the Community Earth System Model are a bit weak. The discussion of the atmospheric dynamics involved is not clear and mixes up conditions at the coast and in the interior of Antarctica. Also, different time scales are mixed together and often it is not clear, which time period is meant when certain trends are reported.

Author's response to referee 2's general comments:

We decided to follow the advice of the referees and removed the detailed volcanic matching, except for Tambora (described in detail above). We also include an assessment of the impact of the 16 years dating uncertainty in all graphs and tables and in the main text to show that it does not change our conclusions.

As outlined in our response to Referee 1, there is a moderate temporal correlation between the SMB from the ice core and the SMB from climate reanalyses, which suggests that wind processes influence local SMB at Derwael Ice Rise. The relationships between precipitation and sea ice, SST and large-scale circulation are analyzed using output from the Community Earth System Model (CESM). CESM was selected for two reasons: (1) it yields an SMB and climate time series that overlaps to a great extent with the ice core record (1850-2012), unlike the reanalyses that only cover ~35 years, and (2) the present-day climate and SMB are realistic (Lenaerts et al., 2016). This is now more clearly indicated in the text.

We thank the reviewer for the suggestion on the significance of the signals that are found in low and high accumulation years. We have now compared the anomalies in those years with the temporal standard deviation, and adapted ex-Figure 7 (now Fig. 8) such that signals are only shown where they are larger than one standard deviation. Clearly, the signals exceed the standard deviation for the high anomaly years, but are not significant for the low accumulation years. Therefore, we decided to omit the bottom panel and only show the situation in the high accumulation years.

Referee 2 Specific comments	**Author's response**
Title: what does "recent" mean? and, to be correct, "snow accumulation" should be "surface mass balance".	*The title has been changed to: "Ice core evidence for a 20th century increase in surface mass balance in coastal Dronning Maud Land, East Antarctica."*
Abstract: It would be good to re-write the abstract after the main text has been revised.	*Agreed and done.*

P2:	
l5: increasing ice discharge	*Amended*
l8: What does the Polvani paper have to do with warming- related increase in precip? There are other papers that involve data and modelling and do not find either warming or increase in precipitation in the considered period. Please, make sure that it is clear about which time period you are talking.	*We deleted the Polvani reference and added a sentence acknowledging papers that do not find warming, except in West Antarctica. Papers that do not find an increase in SMB were already mentioned.* *We added precisions of the periods considered.*
l23: "both authors concluded that the trends were insignificant". This is not correct and not exact. Which trends? Altnau et al. found a statistically significant positive trend in SMB for the interior DML.	*We apologise for the confusion. The sentence has been changed to "Frezzotti et al. (2013) showed no significant SMB changes over most of Antarctica since the 1960s, except for an increase in coastal regions with high SMB and the highest part of the East Antarctic ice divide, and Altnau et al. (2015) found a statistically significant positive trend in SMB for the interior DML."*
P3:	
L10ff: grammar: in your sentence, "which" refers to the project.	*The sentence has been changed accordingly.*
L12: a local flow regime	*Amended*
How high is the accumulation rate? It would be good to give this information already here.	*We added this information and chose to use the previously published accumulation rate of 0.50 m w.e. (0.55 m i.e., Drews et al., 2015).*
P4:	
L3: do you mean 30mm x 30mm?	*Yes, amended.*
L13: the boundary between annual layers	*Amended*
L21: better: were carried out	*Amended*
P5:	
L5: snow burial: better: the compression of the snow under its own weight	*Amended*
It would be interesting to see the density profile here, maybe you could add this in a figure. I also miss some information about the depth until which seasonal variations in the isotope ratios can be resolved.	*We think that adding the density profile in a figure is not necessary, since it is published in Hubbard et al., 2013. However, if the referee or Editor believes this would improve the quality of the paper, we are ready to do it.*
P6:	
L3: how reliable are the CESM data for the 19th century, especially sea ice?	*That is a very good question. In fact, we have little to no observational estimates of 19th century sea-ice extent. The CESM simulated sea-ice extent in the observational period is very realistic compared to observations (Lenaerts et al., 2016) and does not show any trend in the Atlantic sector, which gives us confidence that the sea ice is treated realistically.*
L24: better: mainly derived from. . .	*Amended*
P7:	
1ff: see above. The volcanic peaks in Figure 4 seem to be pretty ambiguous in most cases.	*The correspondence with volcanic peaks has been completely revised (addressed in detail above)*
P8:	
L15ff: This is a very short and simplified view. The sea ice argument is not convincing, especially the hatched area of anomalies is fairly small and should not have a large impact on precipitation amounts. A decrease in surface pressure of not much more than 1hPa is not very much, even in a composite, and in that case, lower surface pressure does not necessarily mean higher precipitation. I'll get back to that in the discussion part.	*We do not agree entirely with the statement that the anomalies are fairly small. We find a maximum anomaly of sea ice extent of more than 30 days, which is much larger than the inter-annual variability. We agree that the surface pressure anomaly is fairly small; we have revised the text according the reviewers' comments (see below).*

L26: define "current", please.	*"current" was replaced by "recent".*
P9:	
L2: How do you define "climate-related"? What else could it be on this time scale? Could it be that the first in-situ validation of increased precipitation in coastal Antarctica is due to the fact that the drilling location is influenced rather locally? Did you compare it with temperature proxies? I am not saying it is wrong or right what you state, but you should discuss this.	*We removed the term "climate-related".* *We now discuss the spatial significance of our results at greater length.*
L8: strange usage of "refer to". Maybe better "represents" or similar.	*Amended*
L13ff. Decreasing trend: I assume you mean "negative trend". Decreasing would mean getting stronger negative with time.	*Amended*
Please, make sure that it is clear, which time period is considered in your respective comparisons.	*We agree that it was not clear and replaced all references to "the recent period" by "the last 50 years" and the "most recent period" by "the last ~20 years".*
L10: Stenni et al: 1992-1996: too short a period to consider any trend calculation	*Reference to this has been deleted*
P10:	
L5. What is the reason for the choice of the threshold? Many coastal stations have SMBs around 0.3. This seems a bit arbitrary.	*This threshold was chosen in order to be consistent with Frezzotti et al. (2013) (no threshold allows isolation of only coastal stations)..*
L9: this is covered by only two high accumulation sites..	*Amended*
L14: dating accuracy	*Amended*
P11:	
L4ff: the positive trend in SMB. . . the result of various forcings	*Amended*
L7: the air does not "hold vapor", a higher temperature means a higher saturation vapor pressure.	*Amended*
L7ff: Paragraph 4.3 is very important, but, unfortunately, it contains quite a few misconceptions (in spite of the fact that one of the co-authors is a meteorologist and expert for polar/Antarctic meteorology) and thus should be re-written: First of all, there is quite a bit of confusion of coastal and continental conditions. Several papers are quoted, of which some deal with the interior and others with the coastal areas of Antarctica, which, however, have very different precipitation regimes. Amplified Rossby waves are particularly important for precipitation in the interior of the continent, NOT for the coast. The coastal areas are always under the influence of synoptic activity in the circumpolar trough. The individual events quoted in line 18 can bring up to 50% of the total accumulation in the interior, not at the coast. And also this means the sum of all events, not one single event. 2009 and 2011 were years with such events in the interior, which of course, also bring high precipitation to some coastal areas, but are not necessarily associated with lower surface pressure, on the contrary, the pressure in the coastal areas of Antarctica is usually lower in years like 2010, where a zonal flow was predominant and the interior of the continent got less precipitation than on average.	*We agree with the reviewer that this part should be more concisely written, and that we should discriminate better between coastal and interior regions. We have revised the text accordingly.*

L25ff: SAM: what was the temporal resolution of your comparison of SAM, SOI and your data? Annual means, monthly values? You should not expect any signal in the annual mean since the SAM index has high intra-annual variations.	*This was indeed a comparison of annual mean, but we decided to delete this sentence, since it is not relevant.*
P12:	
L 4ff: you discuss topographic influences here, but never question that the result for the ice rise might be more locally influenced than climate-related (whatever that means). The topography of an ice rise influences the synoptically caused winds much more than the surrounding ice shelf or the plateau since the ice rise represents a disturbance in the main flow. This is especially surprising since the authors include the Lenaerts et al. J. Glac.2014 paper, which investigates the climate and mass balance on ice rises, in the reference list, but never discuss it in the text.	*We appreciate the reviewers comment, and we agree with it. In the revised manuscript we now include discussion of the local wind effects on the SMB.*
L19: what do you mean by "these two highly variable accumulation events"?	*Sentence amended*
L20: what is the physical explanation for DML being most susceptible to an increase in snowfall in a warmer climate? So far, a positive trend in Antarctic sea ice has been observed, which according to your findings, should decrease precipitation. (not sure about the regional trends, though, I am no sea ice expert.)	*Lenaerts et al. (2016) attributed future increase in DML snowfall partly to increasing temperature and partly to a simulated future decrease in sea ice extent. The observational record does not show any significant changes in sea-ice in the Southern Ocean region around 30-70 °E (e.g. Bintanja et al., 2013).* *However, although global sea ice area does appear to be increasing slightly in the Southern Ocean, several studies show that it this general expansion hides strong regional differences. Indeed, Stammerjohn et al. (2009) showed that the Princess Ragnhild coast area and, more generally, the Southern Ocean to the East of it, show a recent slight reduction of the sea ice season duration. This is part of a circum-antarctic bipolar pattern similar to the SAM spatial distribution.*
L24ff: see general comment. What is the temporal resolution of the investigation of the relationship between SAM, SOI and SMB?	*This comment is not linked to P.12, L24.* *Anyway, we removed the investigation of the correlation between SAM, SOI and our observed SMB data from the revised manuscript..*
L26ff: Low pressure: see above. Usually the pressure in the circumpolar trough is lower (on average) in years with more zonal flow and less meridional heat and moisture exchange (positive SAM index) than in years with amplified Rossby waves.	*That is correct, and we apologize for the misinterpretation. Since the anomalies in surface pressure are smaller than the standard deviation, we decided to omit these from the Figure and revised text.*
P13:	
L4: positive trend	*Amended*
L12ff: I do agree that the ice rise is a suitable potential drilling site for a longer core. However, you should investigate the representativeness of your results a bit closer and keep this in mind when interpreting a deeper core	*The discussion has been amended accordingly.*
References: The reference list contains quite a few publications that are not quoted in the text. Please, check.	*Thank you, we checked the reference list and removed the errors. There are still a few references that are not quoted in the text. This is because they are referred to in Table A1, and therefore, used in Figure 1.*

	These are: Anschutz et al., 2009; Ekaykin et al., 2004; Frezzotti et al., 2007; Igarashi et al., 2011 ; Jiang et al., 2012; Morgan et al., 1991 ; Mulvaney et al., 2002 ; Roberts et al., 2015; Ruth et al., 2004 ; Schlosser et al., 2014; Sommer et al., 2000; Stenni et al., 1999; Takahashi et al., 2009; van Ommen and Morgan, 2010; Xiao et al., 2004; Zhang et al., 2006.
P16: L15: new paragraph: Hofstede. . .	*Amended*
P20: l25; new paragraph: Schlosser. . .	*Amended*
P26: the caption of Figure 26 should be rephrased: "Diff. in mean annual SMB between ~1960-present and ~1816 –present (a,b)" (c,d accordingly)	*Amended*
P31: Figure 6: a) b) labels missing	*Amended*
The legend is a bit confusing, since the dotted lines claim to be a mean SMB, only the caption explains that it is mean plus/minus STD. Maybe a single line with some shading for the range of the STD would be show this more clearly. For 1992 to 2012, one would expect that the averages are not very different, given the closeness of the green and the black line?	*The Figure has now changed completely (discussed above). Since most volcanic horizons are not used as reference markers anymore, Figure 7 now illustrates the rate of change between fixed periods of 20 and 50 years.*

References in response

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Lenaerts, J. T. M., Brown, J., Van Den Broeke, M. R., Matsuoka, K., Drews, R., Callens, D., … and Van Lipzig, N. P. M.: High variability of climate and surface mass balance induced by Antarctic ice rises. J. Glaciol., 60(224), 1101–1110. doi:10.3189/2014JoG14J040, 2014.

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