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.

Stammerjohn, S.E., Martinson, D.G. Smith, R.C.,Yuan, X., and Rind, D.: Trends in Antarctic annual sea ice retreat and advance and their relation to El Niño–southern oscillation and southern annular mode variability, J. Geophys. Res., 113, p. C03S90 http://dx.doi.org/10.1029/2007JC004269, 2008.