Authors response to editors review of manuscript cpd-2014-134 "A two thousand year annual record of snow accumulation rates for Law Dome, East Antarctica" by Roberts et al. Items for each review have been listed sequentially with text from the reviews shown in *italics*. Locations in the text are referenced by page and line, e.g. page 4472 lines 22-25 is given as P4472 L22-25 in the original submission.

1 Review cpd-10-C2153-2014 (Anonymous Referee #1)

My main concerns are the following issues: the paper presents data of Law Dome ice core already partially published in previous papers (e.g. Plummer et al., 2012, van Ommen and Morgan 2010, for snow accumulation; Vance et al., 2013 and in press for correlation with IPO and ENSO) with some difference, but without real discussion between the record and elaboration (e.g. spectral analysis of LDsss and LD snow accumulation). The main focus of this paper is to present the accumulation record, and to make this record publicly available to enable detailed comparison between this record and other accumulation records, as well as other climate records and indicies. Such comparisons are beyond the scope of this manuscript, but publication of the accumulation record allows other interested research groups to use this record in their own comparisons.

Addressing the particular comment about comparison with the summer sea salt record, we would not expect that these two independently derived records (summer period sea salts and annual snow accumulation) would show exactly the same spectral properties. We have rewritten this paragraph (see below, specific comment P4479–4480 L23–12) to make it clear that while ENSO band frequencies between the two records are similar, the 29.7 y period is unique to the accumulation record.

The paper discusses mainly the use of two thinning function, which provide difference up to ±3% of annual snow accumulation, this difference could be negligible at annual layer but not at decadal scale. the paper does not discuss the possibility that variation of snow accumulation could be due to change in spatial variability of snow accumulation and not on amount of precipitation variability. Law Dome site presents the highest dome gradient in Antarctica with a *very strong gradient in spatial variability in snow accumulation (around 25 kg m3/yr per km). A change in the direction of precipitation track or wind scouring could be a strong impact of snow accumulation pattern.* The following text has been added to the end of the Discussion (P4481 L25) “It should be noted that the snow preserved accumulation record is influenced by local factors such as wind removal and potential regional (Law Dome) variations driven by interactions between weather systems and local orography. Winds at Law Dome Summit are generally low (Morgan et al., 1997) and net wind removal is not believed to be a major influence at this site. However, the strong orographically driven accumulation gradient across Law Dome could conceivably lead to a local signal in accumulation variability if the climatology of cyclonic systems and wind tracks changes. Such spatial distribution changes still represent a climate signal, rather than an amplitude modulation of a relatively stable spatial distribution. Therefore, these influences might reduce coherence between the Law Dome accumulation series and the broader Wilkes Land region. The observed coherence with the precipitation minus evaporation fields in the reanalyses discussed above suggests that the local influences are in fact, not significant.”

The paper pointed out that the DSS ice core record capture broad scale variability across a large region of East Antarctica, well beyond the immediate vicinity of the Law Dome Summit, on the base of atmospheric model, but without any comparison with the analogous snow accumulation records, or comparison with previous snow accumulation reconstruction in east Antarctica (e.g. Somme at al., 2000; Stenni et al., 2001; Ruth et al., 2004) Such a broad scale comparison between different ice core records is beyond the scope of this manuscript. It would more naturally be conducted under the auspices of the Pages Antarctica 2k collaboration (http://www.pages-igbp.org/workinggroups/antarctica2k/scientific-goals), in particular the Phase 2 goal of “a reconstruction of past surface mass balance (snow accumulation rate) at decadal scale”, for which this present study will be one of a number of key data-sets. It should also be noted that the RACMO2.1/ANT surface mass balance data set has been compared to an extensive set of observations in Lenaerts et al. (2012) and therefore this does not require repetition in this manuscript. We have added the following text at P4480 27 “The correlation with the RACMO2.1/ANT data-set in this region is more likely representative, as this data-set is strongly correlated to extensive observational data (Lenaerts et al., 2012) and snow accumulation records from shallow cores in the region are positively correlated with the Law Dome snow accumulation record (see Table 2).” In addition, we added in Table 2 showing the correlation between our record and shallow ice core records in the region where ERA-Interim and RACMO2.1/ANT disagree.

The annual layer has been calculated used seasonally-varying water stable isotope verified by seasonally varying trace ions, but no data are shown or discussed, in particular about difference between signal and uncertainty also respect to nssSO4 record of Plummer et al., 2012 the comparison between snow accumulation record and isotope and chemical species are desirable. There is only one dating scale, produced by looking at all the data in an integrated manner, but with primary consideration given to the water isotope record. This is the same as Plummer et al. (2012) and only differs in the upper portion of the record where we have used a more recent and continuous firn core. As such a comparison with Plummer et al. (2012) is unnecessary. We have changed the text at P4473 L3 from “verified” to “augmented” to make this clearer. Additionally, have added the following text at P4472 L25 “Specifically, the root mean squared difference between the layer thicknesses is 3%, consistent with previous findings of a strong correlation ($r^2 \sim 0.95$) between annual ice thickness at the DSS site obtained from shallow firn cores (van Ommen and Morgan, 2010).”

Specific comments:

- P4470 L7 and elsewhere *AD -22 = BCE 22* AD -22 was intentionally used throughout the manuscript to avoid possible ambiguity over the inclusion of a year 0 in the counting scale. To make this clearer, the following text was changed on P4471 L8 from “to AD -22.” to “to CE -22 (including a year 0 in the calendar).” Additionally all “AD” where changed to “CE” including in labelling of figures 4 and 10.

- P4472 L22-25 *the record of the different snow accumulation record and relatively isotope and chemical species signal must be shown and discussed, to provide information about the significance of signal and its spatial variability* The comparison on the accumulation record with isotope and chemical species records is beyond the scope of this manuscript. Such comparisons are worthwhile, are underway in our research group and will be the subject of future publications. In addition, we would like to encourage other research groups to conduct such comparisons, and enabling these is one factor motivating our publication of this accumulation time series.

- P4478 L15-20 *what occurs if a vertical strain rate from GPS measurements is applied at the two models?* As expected, using a different constant vertical strain rate introduces a significant trend in the snow accumulation time series, for the piece-wise linear model a trend of 0.143 $\mathrm{m\,y^{-1}}$ IE per millennium. More importantly, using a vertical strain rate of $7.72\times10^{-4}\,\mathrm{y^{-1}}$ (see response document Fig 1 below) results in an average layer thickness in the upper section of the core (where thinning is negligible) in excess of 0.9 m IE, which is inconsistent with the observations. Changed the text at P4474 L12–14 from "This method assumes that any change in layer thickness is due only to the vertical strain, and that there is no long-term trend in snow accumulation rate." to "This method is unable to distinguish between a constant vertical strain rate or a linear trend in snow accumulation. Therefore, it will be assumed that any change in layer thickness is due only to the vertical strain, and that there is no long-term trend in snow accumulation rate."

- P4479 L9-10 *the strongest anomaly of 1970-2009 on the base of fig. 4 is not so clear, the phrase should be supported by more analysis, also in the general contest of Antarctica (e.g. Monaghan et al., 2006; Frezzotti et al., 2013)* The text has been changed from "The recent above-average snow accumulation rate period from 1970–2009 CE is the third largest in integrated snow accumulation excess throughout the record, after 380–442 CE and 727–783 CE." to "Considering the 10 y low-pass filtered snow accumulation time series, the integrated snow accumulation excess (I) can be defined as

$$I = \int_{t_0}^{t_1} (\breve{a}(t) - \bar{a})\, dt \tag{1}$$

where $\breve{a}(t)$ is the low-pass filtered snow accumulation time series, and the epoch t_0–t_1 defines a contiguous period when $\breve{a}(t)$ is always above or below the long term average snow accumulation ($\bar{a}$). The recent above-average snow accumulation rate from 1970–2009 CE is the third largest period of integrated snow accumulation excess throughout the record, after 380–442 CE and 727–783 CE (see Table 1).” Table 1 has been added listing all epochs where $|I| \geq 2\mathrm{m}$ IE. Also added the following text comparing the DSS record to a continental scale reconstruction “The continental-scale low snow-accumulation periods of 1250–1300 CE and 1420–1550 CE (Frezzotti et al., 2013) are reflected in the DSS record with strong negative I (but are interspersed with short periods of above average snow fall) for the epochs 1239–1302 CE and 1415–1522 CE. The continental-scale low snow-accumulation period 1660–1790 CE is also recorded at the DSS site, although with a later commencement (1691 CE) and one short, but large (I=0.623 m IE), positive anomaly between 1763–1772 CE.”

– P4479-4480 L23-12 *the spectral analysis presents different frequencies with Vance et al., 2013, the text must be improved (eg. the 29.7 yr does not appear in Vance et al., 2013).* The wording of this section lacked clarity. Wwhile some similar frequencies were observed with the summer-period sea salt record of Vance et al. 2013, it was not made clear that the two records are quite different, and show different spectral properties. We would not expect that these two independently derived records (summer period sea salts and annual snow accumulation) would show exactly the same spectral properties. We have rewritten this paragraph to make it clear that while ENSO band frequencies between the two records are similar, the 29.7 y period is unique to the accumulation record.

  Previously, we had “In contrast, spectral analysis of the 2 ky annual snow accumulation rate record (Fig. 9) shows a number of significant periodicities in the sub-decadal band of 2.4–8.5 y, and one 29.7 y period is also evident, which may be related to climate variability. The sub-decadal power at 2.4–8.5 y is in the broad band of ENSO-type frequencies. An analysis of sea salts at Law Dome has previously shown an ENSO signal in the summer-period sea salts (Vance et al., 2013), so it is interesting to note that ENSO-type frequencies are also evident in the snow accumulation rate record. Despite this, there is no significant correlation between the snow accumulation rate record presented here and the Southern Oscillation Index over the epoch 1870–2012 CE. The 29.7 y period may be related to the IPO (Power et al., 1999), for which a 1000 y reconstruction has been produced recently using multiple Law Dome records (including snow accumulation rate)

(Vance et al., 2015). The higher frequencies in the sub-decadal band (2.4 and 8.5 y) are generally more intermittent throughout the 2 ky period (Fig. 10), probably due to noise associated with surface processes and the passage of sastrugi over the site. The 29.7 y period, in contrast, is more persistent throughout the record, and there are multi-centennial epochs where this frequency is quite strong (e.g. 100–550 CE, 750–1000 CE and 1500–2012 CE). This suggests (if this period is associated with the IPO) that the IPO signal has remained relatively steady at Law Dome for the past 2 ky."

We have revised this to read "In contrast, spectral analysis of the 2 ky annual snow accumulation rate record (Fig. 9) shows a number of significant periodicities in the sub-decadal band of 2.4–8.5 y, while one 29.7 y period is also evident, which may be related to climate variability. The sub-decadal power at 2.4–8.5 y is in the broad band of ENSO-type frequencies. An analysis of sea salts at Law Dome has previously shown an ENSO signal in the summer-period sea salts, with associated ENSO-band significant frequencies of 2.8, 4.4, 6 and 7.5 y (Vance et al., 2013). It is interesting to note that ENSO-type frequencies are also evident in the snow accumulation rate record despite there being no significant correlation between the snow accumulation rate record presented here and the Southern Oscillation Index over the epoch 1870–2012 CE. The 29.7 y period is not seen in the summer sea salt record (Vance et al., 2013) but may be related to the IPO (Power et al., 1999), as a 1000 y reconstruction of the IPO has been produced recently using multiple Law Dome records. Snow accumulation rate was a necessary input parameter to this IPO reconstruction to produce a high skill reconstruction (Vance et al., 2015). The higher frequencies in the sub-decadal band (2.4 and 2.7 y) are generally more intermittent throughout the 2 ky period (Fig. 10). The damping of these higher frequency signals may be a real climate signal, but may also result from noise associated with surface processes, such as the wind-blown redistribution of snowfall and the passage of sastrugi over the site. In contrast, the 29.7 y period is more persistent throughout the record, and there are multi-centennial epochs where this frequency is quite strong (e.g. 100–550 CE, 750–1000 CE and 1500–2012 CE). Therefore, if the 29.7 y period is associated with the IPO, this suggests that the IPO signal has remained relatively steady at Law Dome for the past 2 ky. This is further reinforced by Vance et al. (2015), who showed that both positive and negative phases of the IPO could be reconstructed with high skill over both the instrumental calibration period (1870–2009 CE) and the full millennial period spanning 1000–2009 CE. ”

- P4480 L6-8 *sastrugi noise is at annual scale, and could not influence sub-decadal signal, moreover change in spatial snow accumulation pattern draw by change in precipitation or redistribution process* Again, our wording in this section was imprecise, and this has caused confusion for reviewer 2 as well. Our intention was not to suggest that sastrugi cause or destroy the high frequency signals at Law Dome. We intended to demonstrate that high frequency signals can be damped by depositional noise and redistribution of snow, and we have rewritten this section (see above comment P4479–4480 L23–12). In addition, there is far more detail regarding this section in the comments for reviewer 2.

- P4480 L10-12 *the phrase is not coherent with the reconstruction of Vance et al., 2014.* We are unsure why this phrase is not coherent with the reconstruction of Vance et al., 2015? In contrast, we think it reinforces the findings of Vance et al. 2015. Vance et al. 2015 describes an IPO reconstruction that utilises multiple Law Dome records (summer sea salts, winter sea salts, and annual snow accumulation) and clearly demonstrates that using these three inputs from Law Dome, we are able to reconstruct both negative and positive phases of the IPO with very high skill over the instrumental era (1870–2009 CE). We would suggest that the persistence of the 29.7 y signal, if it is associated with the IPO, is borne out by the fact that over the 1000 y period of the IPO reconstruction we were able to reconstruct the IPO with high skill using two independent reconstruction methods. We have reworded this section (see above comment P4479–4480 L23–12) adding further detail as to why the 29.7 y finding reinforces, rather than disagrees with, the findings of Vance et al. 2015.

- P4481 L19-20 *How much is the % of PC3? and could be significant to show a strong correlation?* Changed the text from “In contrast, PC3 shows a strong correlation with Law Dome accumulation in the Law Dome region (Fig. 12) inset).” to “In contrast, PC3 (11%)

(r=0.5, p<0.01 for the Law Dome region) shows a strong correlation with Law Dome accumulation (Fig. 12 inset)." Also changed the text in the proceeding line from "little correlation between these first two components and accumulation" to "little correlation between these first two principle components and accumulation" and the text on P4481 L23-25 from "This is a further line of evidence that Law Dome ice cores preserve tropical signals, and are not only sensitive to the dominant annular signal, which is centered over West Antarctica (Vance et al., 2013, 2015)." to "This is a further line of evidence that Law Dome ice cores are not only sensitive to the dominant annular signal centred over West Antarctica, but also preserve tropical and mid-latitude Pacific and Indian Ocean signals as shown by Vance et al. (2013, 2015)."

- Fig 4c *should show the difference between the two records* Updated this figure to show the percentage difference between the two smoothed accumulation histories. Updated the figure caption from "smoothed piece-wise linear (red) and power law (blue) snow accumulation histories. Note the changed vertical scale to highlight the similarities of the two smoothed records." to "difference in the smoothed accumulation histories (piece-wise linear-power law vertical strain) norm aliased by the power law vertical strain."

2 Review cpd-10-C2177-2015 (Anonymous Referee #2)

The uncertainty estimation could be improved. I offer two suggestions regarding the uncertainty in the flow correction, and the quantification of the spatial variability of the layer thickness data. The uncertainty estimation has been improved, see discussion below.

The discussion could also be clarified, and it would be interesting to add a discussion of the covariance of accumulation and d18O of water, which is not present in the paper. The discussion has subsections added and a paragraph has been added to discuss the relationship between the accumulation record and water isotopes. See below for details.

The Authors present two strategies for correcting the annual layer thicknesses for flow thinning, but both of these approaches have strong assumptions (for instance of a steady state accu-

mulation rate), and it is difficult to understand exactly what the limitation of these approaches are, and what the impact on the accumulation trend is. To refine this problem, I suggest that the authors do a Monte-Carlo type calculation of the thinning function, changing the driving parameters (mean accumulation rate, C, p, z0) for each of the model by one or two standard deviation, and produce a suite of accumulation time series, which they can use to 1) calculate the uncertainty in the accumulation history 2) calculate the uncertainty on the Holocene trend, and estimate pre quantitatively whether they can actually resolve the existence of the trend. In particular, I am curious on the impact of assuming a constant accumulation rate. What if the accumulation rate was 10% larger, 10% smaller? How would it change the strain rate and inferred accumulation history? We have followed the standard approach in ice core science in correcting for the convolved influences of flow thinning and accumulation changes, we have just been more explicit about the underlying assumptions. Additionally, we have gone beyond the standard approach by considering a second (and for this study site) more realistic correction to the flow thinning. The choice of the probability density functions for parameter selection in any Monte-Carlo study would either need to be based on the mean and standard error of the vertical strain rate models used in the current study (and subject to the same assumption of zero mean long term trend) or require extra constraints to be more meaningful than what we have already done. We have plans to investigate ways of obtaining such additional constraints, but that future work would require an entire peer reviewed paper on its own to do rigorously. However, we have incorporated as many of the referees suggestion as is feasible. In particular, the following text was added to the end of P4478 "Additionally, the uncertainty in the estimated vertical strain rate (and associated long-term snow accumulation rate) on the accumulation time history was assessed using a Monte-Carlo simulation. For the piece-wise linear model uncertainty in the accumulation record increases approximately linearly with depth, with an average value of 0.70% and a maximum of 1.77%. Therefore, the assumption of zero long term snow accumulation trend does not rule out a trend of 0.88% per millennium."

I understand that the timescale has been determined in other publications, but I think that it would be useful to test the accuracy of the timescale by giving the age of a few prominent volcanoes, and compare with their published date. This would be an independent constraint on *the accuracy of the layer counting.* This data is already included in the Plummer et al. (2012) paper. Have changed the text at P4473 L3-4 from "The isotope layer counting is verified by seasonally-varying trace ions to a depth of 796.138 m, which corresponds to -22 CE." to "The isotope layer counting is augmented by seasonally-varying trace ions to a depth of 796.138 m, which corresponds to -22 CE, and shows excellent agreement for the dating of major volcanic events (Plummer et al., 2012)."

Spatial variability of the layer thickness You have several cores. In the periods of overlap, how well do the layer thicknesses match between cores? I think it would be useful to add a paragraph on this in Section 2, and a plot showing the layer thickness for several cores. You mention the good overlap in d18O, but it would be interesting to comment on the overlap on layer thicknesses. Have added the following text at P4472 L25 "Specifically, the root mean squared difference between the layer thicknesses is 3%, consistent with previous findings of strong correlation ($r^2 \sim 0.95$) between the annual ice thickness at the DSS site obtained from shallow firn cores (van Ommen and Morgan, 2010).". See also response to specific comment P4480 L7 below.

correlation between accumulation and temperature In Antarctica, both accumulation and temperature are often inferred from d end18O, with the underlying assumption that temperature and accumulation are correlated by the Clausius-Clapeyron equation. You have independent information about both, and it would be interesting to comment on the correlation between them, and study the time variations of this correlation (correlation on year to year timescale, versus decadal timescales, versus centennial: : :). Furthermore, a correlation between end18O and accumulation might lead you to say a few words about the dominant circulation patterns that bring moist air to the site. Added the following paragraph to the Discussion at P4479 L17 "The accumulation series was compared with the annual δ^{18}O isotope ratio at the site over the period 174–2012 CE. Correlation analysis reveals that the two series are weakly correlated, with $r = 0.227$. While only representing a common variance of 5%, the result is highly statistically significant; the 95% confidence interval is [0.191–0.262], as computed using a method which accounts for autocorelations (Ólafsdóttir and Mudelsee, 2014). The weak relationship between isotope ratio and precipitation is consistent with earlier findings (van Ommen et al, 2004) which demonstrated a strong coupling of isotope ratio and accumulation in the glacial period but not in the Holocene. Bromwich (1988) notes the importance of circulation intensity relative to thermodynamic control of moisture content in determining precipitation, and this is particularly important at Law Dome where cyclonic influence is large. The weak control of temperature over recent centuries also reflects other findings at other moderate to high accumulation sites over recent centuries (Frezzotti et al., 2013)."

spatial coherence Page 4481, the authors suggest that there is a large spatial coherence of accumulation. Could they validate this model result by comparing their record with other records of snow accumulation? for instance Dome C, Vostok, D47, D67, and the ITASE sections. There is also a major difference between figure 11 a and b in Droning Maud land, and perhaps the authors could test using ice core data whether they see significant correlation between Law Dome and DML accumulation. Many of the DML cores are archived on Pangaea.de Such a broad scale comparison between different ice core records is beyond the scope of this manuscript. It would more naturally be conducted under the auspices of the Pages Antarctica 2k collaboration (http://www.pages-igbp.org/workinggroups/antarctica2k/scientific-goals), in particular the Phase 2 goal of "a reconstruction of past surface mass balance (snow accumulation rate) at decadal scale", for which this present study will be one of a number of key data-sets. It should also be noted that the RACMO2.1/ANT surface mass balance data-set has been compared to an extensive set of observations in Lenaerts et al. (2012) and therefore this does not require repetition in this manuscript. Added the following text at P4480 L27 "The correlation with the RACMO2.1/ANT data-set in this region is more likely representative, as this data-set is strongly correlated to extensive observational data (Lenaerts et al., 2012) and snow accumulation records from shallow cores in the region are positively correlated with the Law Dome snow accumulation record (see Table 2)." In addition, we added in Table 2 showing the correlation between our record and shallow ice core records in the region where ERA-Interim and RACMO2.1/ANT disagree.

Specific comments:

- P4470 L22-23 *used "suitable" twice. Perhaps remove one of them* Changed the first "suitable" to "appropriate"

- P4470 L25-26 *very long sentence, and...and... difficult to read, consider rephrasing* Changed sentence from “However, in order to derive accurate snow accumulation rates, snowfall must be high enough to resolve annual layering in the presence of deposition noise due to surface processes, and layer thinning from ice flow and snow densification must be suitably constrained. Furthermore, the annual“ to “However, in order to derive accurate snow accumulation rates, snowfall must be high enough to resolve annual layering where deposition noise due to surface processes exists. Additionally, layer thinning from ice flow and snow densification must be suitably constrained. The annual”

- P4471 L2 *reference weather station data* This sentence is generic in nature and not aimed at this particular site. Have changed the sentence from “The annual layering records the net input of snow at the site and includes the annual snow accumulation rate, the loss through ablation and the transport of wind blown surface snow.” to “Annual layering provides a record of the net snow input at the site which is the sum of annual snow accumulation rate, the transport of wind blown surface snow and losses through ablation.”

- P4471 L7 *add (0.68m/y) after high snow accumulation* This information is provided in the next paragraph which we feel is a more appropriate place, among other meteorological information.

- P4472 L18 *is it 1841-1887 or 1841-1987? Did you make a stack of cores, or just splice them?* 1841-1887 is correct and the cores are spliced. To make this clear the text has been changed from “The main DSS core (DSS-main) is augmented in the upper portion by three other ice cores: DSS1213, DSS97 and DSS99. DSS1213 covers the recent epoch 1989–2012 CE, while DSS97 and DSS99 are used for intermediate data in the epochs 1888–1988 CE and 1841–1887 CE respectively.” to “The main DSS core (DSS-main) is augmented in the upper portion by splicing three other ice cores: DSS99, DSS97 and DSS1213 which cover the epochs 1841–1887 CE, 1888–1988 CE and 1989–2012 CE respectively.”

- P4472 L24 *"shows good replication" : show us, be quantitative about this statement. Perhaps plot it. (even if the plot ends up in the supplement)* Have added the following text at P4472 L25 "Specifically, the root mean squared difference between the layer thicknesses is 3 %, consistent with previous findings of strong correlation ($r^2 \sim 0.95$) between annual ice thickness at the DSS site obtained from shallow firn cores (van Ommen and Morgan, 2010)."

- P4473 L5 *dating error : consider giving age of large volcanic events to back up your layer count (see general comment)* This data is already included in the Plummer et al. (2012) paper. Have changed the text from "The isotope layer counting is verified by seasonally-varying trace ions to a depth of 796.138 m, which corresponds to -22 CE." to "The isotope layer counting is verified by seasonally-varying trace ions to a depth of 796.138 m, which corresponds to -22 CE, and shows excellent agreement for the dating of major volcanic events (Plummer et al., 2012)."

- P4474 Equation 2*consider using the notations of Cuffey and Patterson : lambda for layer thickness instead of z, which people like to use for depth, like you did for equation 1* changed "z" to "λ" and "Z" to "Λ".

- P4474 L14 *"this method assumes that there is no long term trend in accumulation rate" then at the end of page 4478, you seem to deduct that you have no trend. It's not clear to me whether the statement of line 14 applies to subsequent section on vertical strain rate calculation. Consider adding a sentence here to remove the ambiguity.* Changed the text at P4474 L12–14 from "This method assumes that any change in layer thickness is due only to the vertical strain, and that there is no long-term trend in snow accumulation rate." to "This method is unable to distinguish between a constant vertical strain rate or a linear trend in snow accumulation. Therefore, it is assumed that any change in layer thickness is due only to the vertical strain and that there is no long-term trend in snow accumulation rate." In addition, we changed the text at P4478 L25–29 from "The similarity of these estimates, each based on different, yet overlapping, epochs strongly suggests that either the assumption of no long-term trend in snow accumulation rate (see Section 3) is valid or that any trend in snow accumulation rate has been linear and constant over the last 2 ky." to "The assumption of no long-term trend in snow accumulation rate can be checked using the above long-term accumulation rate estimates. Specifically, each estimate is based on data fitting over different epochs, therefore the similarity of these estimates suggests that either the assumption of no long-term trend in snow accumulation rate (see Section 3) is valid or that any trend in snow accumulation rate has been linear and constant over the last 2 ky."

- P4475 L6 *I'm not convinced that is it appropriate to use the standard error rather than the std deviation : the std error is the error on the mean, and this is probably why you used it, but the std deviation informs you on the spread of the values around this mean, and perhaps it is more informative, when you want later to assume that the accumulation is near constant.* The long term average snow accumulation rate is the intercept of the ice thickness as a function of depth data fit, i.e. it is the vertically strained ice thickness at zero depth. Therefore the standard error is appropriate and it should decrease (in general) if we added more data. When we discuss the statistical properties of the snow accumulation time series, eg P4478 L22–P4479 L6, we then discuss the standard deviation, which in general should not change much with the addition of more data (for a stationary process). To make this more explicit we changed the text on P4478 L22 from "The calculated long-term snow accumulation rate of $0.688 \pm 0.130\,\mathrm{m\,y^{-1}}$ IE" to "The mean snow accumulation rate of 0.688 ± 0.130 (1 standard deviation) $\mathrm{m\,y^{-1}}$ IE"

- P4475 L7 *how did you infer the uncertainty in the vertical strain rate?* Both the vertical strain rate and long-term snow accumulation rate are calculated as the slope and intercept of least squares fitting of the piece-wise linear vertical strain rate model to the ice equivalent layer thickness as a function of ice equivalent depth data. This has been made more explicit by changing the text at P4475 L3–4 from "Consequently, the annual layer thickness model requires just two free parameters: the long-term annual snow accumulation rate and the constant vertical strain rate." to "Consequently, the annual layer thickness model requires just two free parameters: the long-term annual snow accumulation rate and the constant vertical strain rate. These are estimated from the intercept and slope, respectively, of a least squares fit to the ice equivalent layer thickness as a function of ice equivalent depth."

- P4475 L20 *"temperature at the base below the freezing point", please include a citation to justify your statement.* Added a citation to Morgan et al 1997 to this statement.

- P4476 Equation 3 *introduce a citation to justify the equation* changed the text from "The borehole horizontal displacement (D) is approximated by a power law profile with parameters determined from the model fit:" to "The borehole horizontal displacement (D) is approximated using the shape function approach of Lliboutry (1979), namely a power law profile with parameters determined from the model fit:"

- P4477 *Consider adding subsection titles to the discussion. I suggest: 1) strain rate model 2) Accumulation history, before the paragraph starting at line 22, p4478 3) Spatial variability, before the paragraph starting at line 18 p 4480* The subheadings "Strain rate model", "Snow accumulation history" and "Spatial variability" have been added at the suggested locations.

- P4478 L21 *use mice yr-1 or mie yr-1 rather than m yr-1 ie* We have chosen to follow the existing journal style for this, and in particular follow the usage of Van Liefferinge and Pattyn, Climate of the Past, vol 9, 2013, namely $\mathrm{m\,y^{-1}}$ IE. Have changed the text at P4471 L18-19 from "0.68 $\mathrm{m\,y^{-1}}$ $\mathrm{ice\,equivalent}$ ($\mathrm{m\,y^{-1}}$ ie)" to "0.68 $\mathrm{m\,y^{-1}}$ IE (where IE stands for ice equivalent)". Changed all instances of "$\mathrm{m\,y^{-1}}$ ie" to "$\mathrm{m\,y^{-1}}$ IE" including Figure 4 labels.

- P4479 L5-6 *vocabulary overly technical. It would be much easier just to show the distribution in a plot, and eventually, overlay a classical distribution fitting your data-set, or a Gaussian distribution to highlight the skewdness/long tail* Have added the suggested figure and revised the text from " positively skewed (0.47), i.e. it has a long tail at higher snow accumulation rates. Additionally, the distribution has more mass in the tails than a normal distribution, with a non-mesokurtic (p<0.001, D'Agostino et al., 1990) probability density function, which specifically is leptokurtic, with slightly raised excess kurtosis (0.58)." to " positively skewed (0.47), i.e. it has a long tail at higher snow accumulation rates (see Fig. 8). Additionally, the distribution has more mass in the tails than a normal distribution, with a non-mesokurtic (p<0.001, D'Agostino at al., 1990) probability density function with slightly raised excess kurtosis (0.58)."

– P4480 L7 *do you expect the noise of a sastrugi passing over the site to be periodic? I am surprised by your statement, because with 0.68m/year correspond to more than 2m of snow per year, and sastrugi are not necessarily that large, so I am not sure whether you can justify a 2.5 year period as being due to dune migration, unless you can quote a paper giving a quantitative assessment of speed and amplitude of dune migration in a high accumulation site like Law Dome. However, perhaps that the presence of sastrugi adds significant noise to your accumulation record at annual resolution, which is something that you should be able to document with repeat cores, as I mentioned in the general comment section. When we have a noisy record, we usually resort to averaging to increase the signal to noise ratio. In ice cores, we can average spatially, by making a composite of many cores, or temporally; by taking the temporal mean over a number of years, so that in the mean, the signal emerges. You have a lot of cores, and perhaps a number of snow pits also, and it would be useful for you to quantify the depositional noise (spatial standard deviation of the number of meters of snowfall per year over a certain area). This way, once you know the depositional noise, you can derive how much temporal averaging you would need to do to have a decent signal to noise ratio, and not try to interpret wiggles inside of the 1 or 2 sigma envelope of the depositional noise.* This comment has been partially dealt with in the comments and modifications made according to reviewer 1 (specific comment P4479–4480 L23–12). Essentially, the wording for this section was imprecise, as we never meant to convey the idea that the sastrugi was in some way causing or responsible for the 2.5 year frequency. What was intended was that the higher frequency signals can be damped by surface depositional processes. Please see the comments for reviewer 1 to see how this section has been reworded to clarify this. The second part of this comment suggests averaging multiple Law Dome records to produce a more robust record, less prone to surface depositional processes; however, we do not have a lot of overlapping records. The original Law Dome record was drilled in 1987-93, and the top section was discarded as it was thermally drilled and not suitable for chemical analysis. Two mid-length cores were drilled in 1997 and 1999 to bridge the gap to the topmost usable section of the main DSS core, but these two sections are not overlapping. We have since collected short ($\sim$10 metre) cores in the intervening years to bring the record from 1997 to 2012. While these sections frequently do overlap, calculating average depositional noise from these cores that only span the last $\sim$12 years would greatly overestimate the noise as it would be convolved with the density profile, which otherwise does not affect the vast majority of the DSS main core. The deeper overlaps with the mid-length cores drilled in 1997-99 are very small, of the order of 1-2 metres, so no meaningful statistics could be calculated from these. We would need extra records of significant depth that overlap with DSS to calculate these statistics, and these are not available.

– PP4480 L11 *Correlation with the IPO. It would be useful to make a plot of your accumulation record with other climate variables of interest, that you discuss in the current paragraph: the IPO, ERA interim and RACMO-ANT reanalyses for the site, zonal wave 3, and perhaps the accumulation rate history at other sites that you expect to find significant correlation or anti-correlation (see my comment about spatial coherence in the major comment section).* Exploring links to other climate variables and high resolution sites is of great interest to our research group. We are currently in the process of further developing the high resolution Law Dome records, including this new accumulation record, to enhance understanding of climate modes and variables from the seasonal to annual scale in the SW Pacific/Indian Ocean sector of the Southern Hemisphere over the last 2000 years. As such, we would prefer not to add work that we are currently planning or undertaking to this manuscript, as this current and future work will be the basis of at least one standalone manuscript on its own. Additionally, by making available this dataset now, we do not limit other researchers who may be interested in utilising the Law Dome accumulation record for their work.

– P4480 L12-17 *This point about the low frequency power is hard to address because it is linked with the assumptions in your methods. I think that it belongs more to the beginning of the discussion, when you discuss the two methods.* Deleted this paragraph and added the following text at P4477 L20, "Furthermore, there is more low frequency power in the spectrum of the piece-wise linear snow accumulation rate time-series compared to the equivalent spectrum for the power law model, which is consistent with removal of the concave bias in vertical strain rates from the the piece-wise linear model."

– P4480 L25 *discussion of the reanalyses. I am a bit skeptical of reanalyses products in Antarctica, when the constraints are so weak. For instance, Nicolas and Bromwich, JClim 2014 showed that for temperature, the different reanalyses products were vastly different, and the spatial patterns described in the reanalyses were conflicting, and not supported by data (see their Figure 7). When you find a good correlation between reanalyses and snow accumulation, is it because Law Dome data are incorporated in the reanalyses?* We agree that the quality of Antarctica reanalysis produces needs further investigation, but that is beyond the scope of the current manuscript. Law Dome data is not included in the ERA-Interim reanalysis, and we speculate that the high and statistically significant correlation ($r=0.7604$, $p<0.001$) between our observed accumulation time series and ERA-Interim precipitation may be partly due to the dominance of orographic influence in the region.

– Figure 1*make the figure a little more zoomed out, so that we can clearly see the independence with the main Antarctic Plateau.* This figure had been zoomed out to include an additional 100 km of easting and northing.

– Figure 2 *could go in the online supplement if you have too many figures* Left this figure in main text, no indication from editor that this paper was over-length and keeping figures in main text improves readability.

– Figure 3 *merge it with Fig 6* Merged these figures and made dot size the same.

– Figure 7-8 *could go into the online supplement if you have too many figures* Left this figure in main text, no indication from editor that this paper was over-length and keeping figures in main text improves readability.

– Figure 10 *why did you choose to do this rather than use wavelets?* An evolutive power spectrum was used rather than a wavelet analysis to simplify comparison with Vance et al. (2013). A wavelet analysis would have shown similar features but complicated the direct comparison with Vance et al. (2013).

I suggest you add 2 figures 1) showing overlapping records to assess the significance of all these wiggles 2) Showing your reconstruction with other variables of interest (SOI, IPO, other records.) The comparison on the accumulation record with other records is beyond the scope of this manuscript. Such work is ongoing in our research group (see for example Vance et al., 2015), and will be the subject of future publications. In addition, we would like to encourage other research groups to conduct comparison, and enabling such comparisons is one factor motivating our publication of this accumulation time series.

3 Additional changes

The following additional changes were made to the manuscript.

– P4469 Added an additional affiliation to Steve George

– P4471 L14 Changed “drilled approximately 4.6 km” to “drilled approximately 4.7 km”

– P4473 L5 Changed “after” to “prior to” for clarity

– P4477 L7 Changed “~ 4 ice thicknesses (4.6 km)” to “~ 4 ice thicknesses (4.7 km)”

– P4485 L11 Updated Vance et al reference from “submitted” to “42, 127-137, doi:10.1002/2014GL062447, 2015”

– Figure 9 Changed caption from "MultiTaper Method power spectrum (Ghil et al., 2002) of snow accumulation rate time series using a resolution of 2 and 3 tapers of power law based construction. Period of spectral components above 99% significant shown." to "MultiTaper Method power spectrum (Ghil et al., 2002) of power law based snow accumulation rate time series using a resolution of 2 and 3 tapers. Period of spectral components above 99% significant shown."

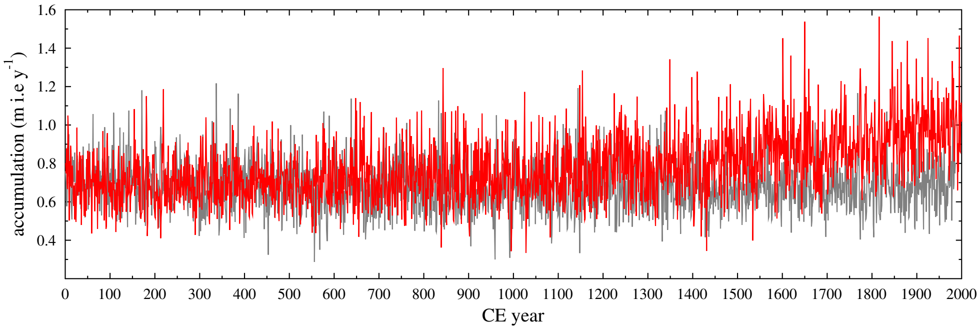

Figure 1. Annual snow accumulation rate history for -22–2012 CE using a piece-wise linear vertical strain rate model based on a fitted vertical strain rate (grey) of 6.32×10^{-4} y^{-1} and a derived vertical strain rate, calculated from surface GPS horizontal strain rate components, (red) of 7.72×10^{-4} y^{-1}.