We have appreciated all the referees constructive comments on our manuscript. In the following we reply within the open discussion to some of the main points that all the referees have raised. Then, a point-to-point response of the major problems highlighted by each reviewer is given. On the other hand, a full response, with the address of the minor problems (such as editing tips) will come with the edited version of the manuscript.

Best regards,

Massimo Frezzotti on behalf of co-authors

1. **The poor quality of the English is one of the most noticed flaw highlighted by the referees.**

   Those comments are unexpected because the text was edited by a native English speaker of the American Journal Expert (www.journalexperts.com) before submission. Apparently the editorial handling done was not enough, we apologize for this lack of accuracy and, if we will be able to submit a new version of the manuscript, we will enhance the readability of the paper, paying more attention on the English formulation.

2. **As for English readability we will improve the logical structure and the coherence within each section applying more synthesis of the presented materials.**

   In particular we will rewrite the "Result" section where our novel results will be highlighted whereas information related to previously published paper will be displaced in the introduction/background sections or deleted if not needed.

3. **All referees pointed out the general lack of discussion about differences between accumulation and TSI.**

   An extended analysis between those variables was out of the scope of this paper, which is focused on understanding how SMB of the Antarctic ice sheet has changed over time. We simply try to highlight the similarity between TSI and AIS accumulation on longer time scale periods. In order to satisfy the referee's request and obtain more quantitative information about these relationship we have generated two comparable time series of TSI and accumulation (Figure R1), both smoothing the continental accumulation time series with a 40 year running average (the same smoothing applied to TSI data, Steinhilber et al. 2009, data available ftp://ftp.ncdc.noaa.gov/pub/data/paleo/climate_forcing/) and sampling this smoothed time series every 5 points (the same sampling rate of TSI data).

   Not surprisingly, a raw correlation (R) between TSI and accumulation over the whole 1200-2005 period shows a not high value (+0.5, significant at 95% level), enhancing if a general indirect effect of TSI over accumulation exists (Figure R2). The accumulation seems to slightly leads the TSI (~+5 year), opposite to model predictions (Schwartz et al. 2007) which stated a lag of 0/-20 years due to the thermal inertia of the climate system. However we believed that our small lag in R is not significant due to the raw sampling rate and the various smoothing used.

   Most probably, if a relationship between accumulation and TSI occurs, it is indirect and almost certainly linked to a teleconnection in atmospheric circulation forcing through *complex feedback. Recent papers point out a shift in regional atmospheric circulation induced by grand solar minimum (Martin-Puertas et al., 2012, Steinhilber et al., 2012). Other forcing factors, such as volcanic aerosols and greenhouse gases concentrations, have changed in time, overlapping and obscuring the solar fingerprint. Steinhilber et al. (2012), in a very recent paper on the TSI reconstruction from ice core and tree rings, put in evidence a generally good agreement between solar forcing and Asian climate. They also noted periods without any coherence where other forcings, like volcanoes and greenhouse gases concentrations,(and their corresponding feedbacks) deeply influenced the climate. Eichler at al. (2009) analysed the importance of solar forcing on an isotopic record from continental Siberian Altai showing the importance of the indirect sun-climate mechanisms involving ocean-induced changes in atmospheric circulation. Those findings are in agreement with the running correlation over different time windows (100, 200, 300 year) between TSI and our accumulation series in Figure R3. The plot shows a very high significant correlation for the central period (1400-1650 AD), especially for the 200 yr running window, but general decrease in correlation values at the tails of the series.*

4. In the presented form the hypothesis of connection between blocking high and accumulation seems fairly speculative.

When the anticyclone high forms, it blocks the zonal flow splitting it upstream in two branches and creating anomalous conditions downstream. Most of the blocking high phenomena in the Southern Hemisphere occur generally in the latitude band 35-55° (Tibaldi et al., 1994) or even at higher latitudes than normal (60-70°, Scarchilli et al., 2011) over the oceans. Various studies (Marques and Rao, 2000; Renwick, 1998; Gibson et al., 1995) show that the Eastern Pacific Basin and, with a lower frequency, the Tasman Sea, in different season, are the most important areas where blocking anticyclones last.

Previous papers have already pointed out the correlation between blocking-anticyclone and snow accumulation in most part of Antarctica (e.g. Masson et al. 2004; Goodwin et al., 2003, Schlosser et al., 2010; Hirasawa et al., 2000). The precipitation is found to be highly episodic, the majority of the events occurring in connection with (blocking) anticyclones and, correspondingly, amplified Rossby waves, which lead to advection of warm and moist air from relatively low latitudes. When a blocking event occurs, the air masses full of moisture, in the Southern branch of the splitted flow, are forced to be adiabatically uplifted in a colder environment producing more precipitation than normal over Antarctic coast, upstream the block. On the other hand, downstream the block, the anomalous dry effect is seen for the most part over the sea in the reduced zonal flow with small and negligible consequence over the Antarctic continent.

In order to highlight the connection between atmospheric features we calculate the number of blocked days for each longitudinal sectors 5° wide (Figure R4) and we correlate it with snowfall (Figure R5, lower panel). The calculation is made with the procedure described in Scarchilli et al. (2011), in order to consider also those formed at higher latitude than normal, applied to the ECMWF ERA INTERIM reanalysis 500 hPa Geopotential Height field on a regular grid of 1°x1°. The snowfall time series are created from the +24h forecast ERA INTERIM snowfall field on a regular grid of 1°x1°. Both blocking index and snowfall fields represent annual cumulative values from daily values over the period 1980-2011.

Generally speaking, the strong statistically significant correlations are apparent over the Antarctica upstream the blocked longitudinal sector, whereas the strong statistically *significant anti-correlated areas, representing the dry effect of the blocking high, appear for the largest part over the sea in the downstream zonal flow.*

Focusing the attention on the continent, it can be noted that, positive correlation values are apparent and statistically significant over East Antarctica, and they are followed downstream by anti-correlated areas with much lower values in modulus which are not statistical significant. Over West Antarctica the presence of blocking high caused a larger and more apparent dipole effect in the correlation field. In particular the positive area values are spread over the Eastern Ross Ice shelf and part of Marie Byrd land, whereas the anti-correlated area enclose only the Antarctic Peninsula with anti-correlation maximum at the base of it. Verified the positive correlation between blocking high and precipitation in most part of Antarctica, it is possible to note the positive trend in number of blocked day (however not significant) for longitudes between 300°-60° (Figure R5, upper panel) which are consistent with the accumulation increase along the IDEA traverse during the last decades.

RESPONSE TO MAJOR COMMENTS OF THE REFEREE#1 (C78–C81, 2012)

My first major comment is of technical nature. Although most of the time it is clear what the authors intend to convey, the English must be checked and formulations streamlined by a native English speaker to enhance the readability of the paper.

We will enhance the readability of the paper in the final version as explained in answer 1 in the section above.

My second major concern is the discussion section 4, which remains rather speculative and qualitative. The first major point of discussion should be the fact that ice core-derived temperatures from H and D isotopes do not correlate with accumulation rates, as stated in the second paragraph in section 4. Why is this so? This result is significant as it challenges the often-made assumption that high-accumulation episodes are associated with above-normal temperatures in Antarctica, linked to well-developed meridional air mass transport. Moreover, it challenges the hypothesis that accumulation Antarctica will increase when atmospheric temperatures increase in a future warmer climate. If for instance the authors have the impression that isotope-derived temperatures from ice cores do not robustly represent atmospheric temperatures, this is also a significant finding, as it would challenge the way temperature records from ice cores are currently used. Anyway, this topic deserves more discussion than it gets now.

Correlation between the ice core-derived temperature and snow accumulation is not the target of the present paper. Snow accumulation rates can be determined at high resolution sites by counting annual layers. In low-accumulation areas, as largely encountered in the interior of East Antarctica, individual annual layers cannot be resolved. In general, accumulation rates in Antarctica are reduced during glacial climate conditions. At low accumulation sites, the past accumulation rates are supposed to be proportional to temperature reconstructions derived from the stable water isotope composition. The accumulation is assumed to be thermodynamically controlled by the change in saturation water vapor pressure at the inversion layer, which is a function of temperature (see supplemental material of EPICA Community Members, 2004; Schwander et al., 2001). The error of such accumulation rates is estimated to be up to 30% or more.

The often-made assumption that high-accumulation episodes are associated with above-normal temperatures in Antarctica, linked to well-developed meridional air mass transport, is not corroborated from the present snow accumulation/water stable isotope studies. On the other hand, the ratio of heavy to light water *molecules in precipitation is influenced both by the source temperature and the isotopic composition where evaporation occurs, and by the moisture pathway toward the site and temperature at the site when the precipitation occurs (Masson-Delmotte et al., 2008).*

For paleoclimate interpretations it is often assumed that changes in source temperature/ location are negligible and that changes in the stable water isotope composition of ice primarily reflect changes in temperature at the ice core site. (see Masson-Delmotte et al., 2008). Conversely, most of the papers that have analysed isotope and snow accumulation from seasonal to decadal scale in Antarctica do not show a clear correlation between "warm/cold" isotope and "high/low" snow accumulation (see e.g. Stenni et al., 2002; Oerter et al., 1999; Abram et al.; 2011; Fernandoy et al., 2010; Graf et al., 2002; Mulvaney et al., 2002; Divine at al., 2009). The snow precipitation process in Antarctica is driven by several factors, which present differences between the coastal and the inland plateau and from area to area. Stable isotope of water is used as proxy of accumulation in the reconstruction of long ice core records, because no other snow accumulation proxy are available.

On page 832, line 14, it is stated that cyclic variations in accumulation in the full stacked record are mirrored by total solar irradiance from the GRIP core. Judging from Fig. 4, there is temporal correlation for some parts of the record, while it is absent or even changes into anti-correlation in other parts (e.g. 1700-1900). My question is: what would explain these periods of high and low correlation? Is the correlation for the full record significant?

The reviewer is right in identifying that the stacked record mimics TSI in some part and not others. We tried to articulate our hypothesis in the answer 3.

Another aspect of the discussion is the role of blocking anticyclones. The authors assert that these systems lead to higher coastal precipitation; in my opinion they would at the upstream side, but a dry anomaly would be expected at the downstream side. Are these dry anomalies somehow underrepresented in the data set?

The reviewer overall view about blocking anticyclone is correct. However a more complex effect of blocking high over precipitation is better highlighted in the answer 4

A third aspect of the discussion is the role of drifting snow sublimation. In my view, the potential impact of temporal variations in drifting snow sublimation on ice sheet integrated SMB is overstated in the discussion. In a recently published paper in GRL, Lenaerts and others show that, integrated over the ice sheet, the interannual variability of drifting snow sublimation is small, 12 Gt yr^{-1}, being only approximately 0.5% of the total accumulation over the ice sheet. Comments?

The recently published Lenaerts et al. (2012) paper is based on data obtained from a regional atmospheric climate model RAMCO 2.1/ANT with a spatial resolution of ~27 km. Field and satellite observations show that wind-driven sublimation rates are lower in plateau areas but very large in slope areas, and they account for 20–75% of the precipitation (e.g. Frezzotti et al, 2004; 2007; Eisen et al., 2008). A paper in publication on J. of Glaciology (Scambos et al., in press) suggests that all current surface mass balance models for the East Antarctic Ice Sheet overestimate mass input to the ice sheet by 46 to 82 Gt yr^{-1} of the total inferred accumulation for the regions above 1500 m elevation. This is mainly due to the low ability of the atmospheric model to reproduce the high speed of katabatic wind and correlated wind-driven sublimation. Conversely, Lenaerts et al. (in press) show that the increase of the RAMCO2.1/ANT model resolution from 27 km to 5.5 km can improve the ability of the model to forecast the wind speed maxima in Adelie Land. As a result the surface mass balance shows much more local spatial variability at 5.5 km resolution that is controlled by drifting snow erosion.

RESPONSE TO MAJOR COMMENTS OF THE REFEREE#2 (C463- C463, 2012)

The first and most obvious flaw of the paper is the poor quality of the English which makes it often difficult to read (in addition to problems #2 and #3).

We will enhance the readability of the paper in the final version as explained in answer 1 in the section above

The manuscript displays relatively little logical structure or coherence within each section or sometimes even within the paragraphs themselves. The most obvious example is the Discussion section. Along the same lines, the Introduction does not provide any clear motivation for the study (how do lines 17-28, p. 825 relate to the rest of the Intro?). Some paragraphs are excessively short (one sentence) and appear disconnected from the rest of the text (p. 826, l. 9-11; p. 828, l. 15-18; p. 832, l. 8-10).

We accept the comment and we will improve the paper readability in the revisited paper.

Some methodological aspects are contradictory: the authors argue that "unassessed records" should be discarded and yet they include them in "most of the analyses" (p. 828 l. 11). The discussion of Fig. 2a (p. 831 l. 1-17) is based upon the results from full ice core array. However, Fig. 2a is actually based only upon the assessed array.

In Figure 2a we presented the results based on assessed array (Ω) because they are the most reliable for the reasons expressed in p. 828 l. 11 and highlighted by the reviewer. On the other hand in the discussion at p. 831 l. 1-17 we present the results obtained with both assessed (Ω) and unassessed records in order to give to the reader the instruments for recognizing the possible misleading issue (see figure R6 A and B for all the results and relationship). However we did not underlined that the most important dataset is the Ω dataset. We will clarify the discussion in the revised manuscript.

In the Results section, the authors seem to primarily discuss the results from other studies and only incidentally their own results. One has to wait the end of the "Results" section to see Fig. 2 actually described.

We accept the suggestion of reviewer. The revised manuscript will be arranged as explained in answer 2 in the first section.

The reference to the figures sometimes seems to be out of place (p. 829 l. 18) or introduced in a clumsy fashion (p. 829 l. 18). Rather than just "Fig. 2", the authors should rather refer to Fig. 2a or Fig. 2b. A lot of the Results section deal with the temporal variability of the SMB during specific periods. Yet, no reference is made to Fig. 3, which is only discussed at the very end of the Results section. Much more synthesis/integration of the presented material is needed.

We accept the suggestion of the reviewer. The discussion of the results presented in Figures 2 and 3 will be re-arranged.

The authors derive SMB values for the entire continent by simply taking the arithmetic average of all available records. However, as they point out, the spatial distribution of the cores is heterogeneous. Therefore, the averaging method should take into account the spatially-varying density of the records. For example, one could estimate the spatial footprint of each ice core record using output data from

global reanalyses as in Monaghan et al. (2006). At the very least, if the authors decide not to use spatially-weighted averages, they should include some discussion about this issue.

We agree with the reviewer about the general necessity to use an averaging method which take into account the spatially varying density of the records. However we believe that, considering the dataset available and the temporal scale analysed in this paper, it is not possible to use something more complex than a regional average, instead of averaging the record all together. We tested the reconstruction of the continental stacked record obtained from the average of the 3 regional records, in order to minimize possible biases due to a too high record density in particular areas. Figure R1 shows that differences between the new continental stacked record and other obtained from a simple average of all the 21 ice cores are very small (< 1%) and are not statistically significant.
On the other hand Monaghan et al. (2006) developed an innovative and more complex technique for the study of snowfall spatial variation in the last 5 decades (1955-2005). The novel methodology is based on the "Kriging" spatial interpolation method which allows to calculate unknown values of a certain variable over a grid, starting from few known points of the same grid. The interpolation is ruled by a predictor function which explains the variation in space of the variable. Monaghan et al. reconstructed snowfall variation over Antarctica for the past 5-decades, by blending some instrumental measurements (ice core and snow pits data, daily observations, etc.) with simulated precipitation from the ECMWF ERA 40 snowfall field and computing the predictor function by exploiting the information about spatial variability provided by the 1980-2005 period of gridded model precipitation data. The 1985-1994 was chosen by the author as reference decade: this will allow the ERA-40 precipitation to be calibrated against instrumental record and adjusted for biases. We decided to not use this technique because Reanalysis ERA40 dataset stops at 1958. In this way is not possible to calculate the predictor function prior to that data. Moreover it is not possible to interpolate our data using the model predictor based on one of the last 5 decades because this implicitly means that the atmospheric dynamics is not changed in the last 800 yr and this is certainly a very questionable assumption.
Finally, Monaghan et al. (2006) assume that each stacked ice core record is a proxy of the regional basin accumulation. On the other hand following our SMB reconstruction, variation in SMB over centennial scale is better correlated to altimetry than to regional basin accumulation. This is in agreement with Genthon et al. (2009) which stated that approximately 75% of the predicted precipitation increase will occur in the periferical area at surface elevation below 2250 m.

The conclusions based upon the relationship between Antarctic SMB and Total Solar Irradiance (TSI) are certainly the weakest scientific aspect of the paper. Sachs et al. (2009) only show that the southernmost position of the Pacific ITCZ coincided with a minimum in TSI, *not* that the southernmost position of the Pacific ITCZ is correlated with the TSI. And yet, the authors seem to use the TSI as a proxy for the position of the Pacific ITCZ, for the tropical tele-connections to high southern latitudes, for the PDO, etc., which is obviously highly questionable.

We stated in the manuscript that "Sachs et al., (2009) provide strong evidence that during the past millennium, the southernmost position of ITCZ ***occurred at Sporer minimum****". Sachs et al., 2009 wrote " The southern-most position of the ITCZ during the past millenium probably* ***occurred about AD 1420****,....One possible scenario is the lower-than-modern* ***solar irradiance*** *during the LIA may have provided the forcing to cool the Northern Hemisphere, which in* ***turn drove the ITCZ*** *close to Equator. AD1420 in particular, ..., corresponds to the minimum solar irradiance (Fig. 4g; the so-called Sporer Minimum) and, except for a brief temperature minimum at about AD 1700, the coldest Northern Hemisphere temperatures of the past 1,200 years". We did not* ***correlate the TSI with the Pacific ITCZ.*** *Delaygue and Bard (2010) pointed out that the lowest solar activity is found during the Sporer Minimum (around AD 1450). Several authors report a link between solar minimum and change in atmospheric circulation with a shift of ITCZ and Southern Westerly Wind positions during the past millennia (e.g. Swingedouw et al., 2011;Verschuren et al., 2000; Thresher, 2002; Varma et al., 2011; Ineson et al., 2011; Steinhilber et al., 2012; Martin-Puertas et al., 2012). It is not one of our goal to use the TSI as a proxy for the position of Pacific ITCZ. We only proposed a similarity between our snow accumulation records and TSI record. Our intention was to point out the lack of correlation between global or Southern hemisphere temperature and snow accumulation, whereas to* *highlight a correlation between snow accumulation and atmospheric circulation driven by the Pacific Intertropical area, as already pointed out from others Authors (see manuscript).*

Figure 4 shows periods where the SMB anomalies and the TSI anomalies are either in phase or out of phase. In particular, the authors seem to minimize the periods of out-of-phase relationship by stating that "the correlation... nevertheless 'are' well inside the one-sigma uncertainty", an argument which is by the way very unclear. Why don't you calculate a correlation coefficient to test the association?

The reviewer is right in identifying that the stacked record mimics TSI in some part and not others. We tried to discuss in more detail our hypothesis in the answer 3.

RESPONSE TO MAJOR COMMENTS OF THE REFEREE#3 (C474–C474, 2012)

While the paper contains a lot of interesting information, the organization and writing of the paper could be significantly improved. In the results section, I sometimes found it difficult to tell what the authors themselves did, as nearly every sentence references prior papers, except for in the 2 last paragraphs of the section. Do these references simply refer to who published the original core data, or are these all previously published assertions? If the latter, then much of this information goes in the intro/background section.

We accept the suggestion of reviewer. The manuscript will be arranged as explained in answer 2.

There minor grammar, punctuation and word usage errors throughout the paper. More importantly, there are several instances of minor inconsistencies (e.g., is the assessed dataset from 21 or 51 records?), or, instances of basic information being treated as assumed knowledge, despite the fact that it may not be for all readers (e.g., casual references to proxy record sites; isotope record-derived parameters are referred to almost off-handily in the discussion, but never mentioned or defined/explained in the data description section, etc.). Not all readers will have worked with proxy records. These types of issues should really be worked out before a paper is submitted the first time.

The entire available dataset over the whole Antarctica is composed of 66 records. Only 21 records show accumulations larger than 70 kg m^{-2} yr^{-1} which is approximately the minimum accumulation annual value allowing the construction of a time series at annual sampling interval. Only those 21 records could be used to create a staked records at the continental and regional scale with decadal temporal resolution. The remaining 45 record can be divided in two sub-sample: the first 30 for which is it possible to define a value of SMB, and the other 15 for which it is not possible to derive a SMB value, due to the unknown conditions upstream at the site core or the high SMB spatial variability at local scale. The 21+30 records, where the SMB values could be considered reliable, form the assessed record (Ω). All information about the proxy record site used in the analysis are reported in detail (Lat, Long, elevation, SMB, reference) in table 1 (supplementary material) and in Figure 1.Howeverclearly the text must be improved.

The reason why we did not define/explain the isotope proxy is due the fact that the manuscript was submitted to a specialised Journal such as "The Cryosphere", and the basic knowledge of ice proxy was assumed.

The blocking-anticyclone hypothesis sounds potentially plausible, but I have some notable concerns regarding their argument, and without further work it is fairly speculative. More detail and some schematic diagrams might be needed, especially if they intend this to be a main point of the paper, as implied from the abstract. For example, it is not clear to me that there is any evidence for increased

blocking cyclone frequency, since “intensity” is not the same thing, and as they state themselves, frequency and intensity tend to be anti-correlated on decadal timescales.

We try to improve the analysis of the complex effect of blocking high over precipitation in a in the answer 4

Additionally, since the authors are basing their argument largely on the apparent correlation between AIS SMB and proxy-derived irradiance, they should try to explain (a) why the variability of the irradiance record does not correlate with the AIS accumulation between 1700 and the early 1900’s (something they gloss over in the paper), and (b) why, when it does correlate, the irradiance sometimes appears to lag slightly behind the accumulation rate; based on my understanding of their argument, it should be the other way around. I’m wondering if the correlation, when it exists, is not causal, but more indirect.

The reviewer is right in identifying that the stacked record mimics TSI in some part and not others. We tried to discuss in more detail our hypothesis in the answer 3.

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FIGURE CAPTIONS:

Figure R1. Continental standardized accumulation time series (black line) calculated as explained in the paper and its 40 years running average (light blue line) sampled every 5 year (blue point). Red line represents TSI standardized anomaly calculated as Steinhilber et al. (2009).

Figure R2. Correlation at different time lags between TSI standardized anomaly and accumulation record smoothed as explained in the text. Red line represents the threshold value corresponding to the 95% statistically significant level under a two tailed t-student test.

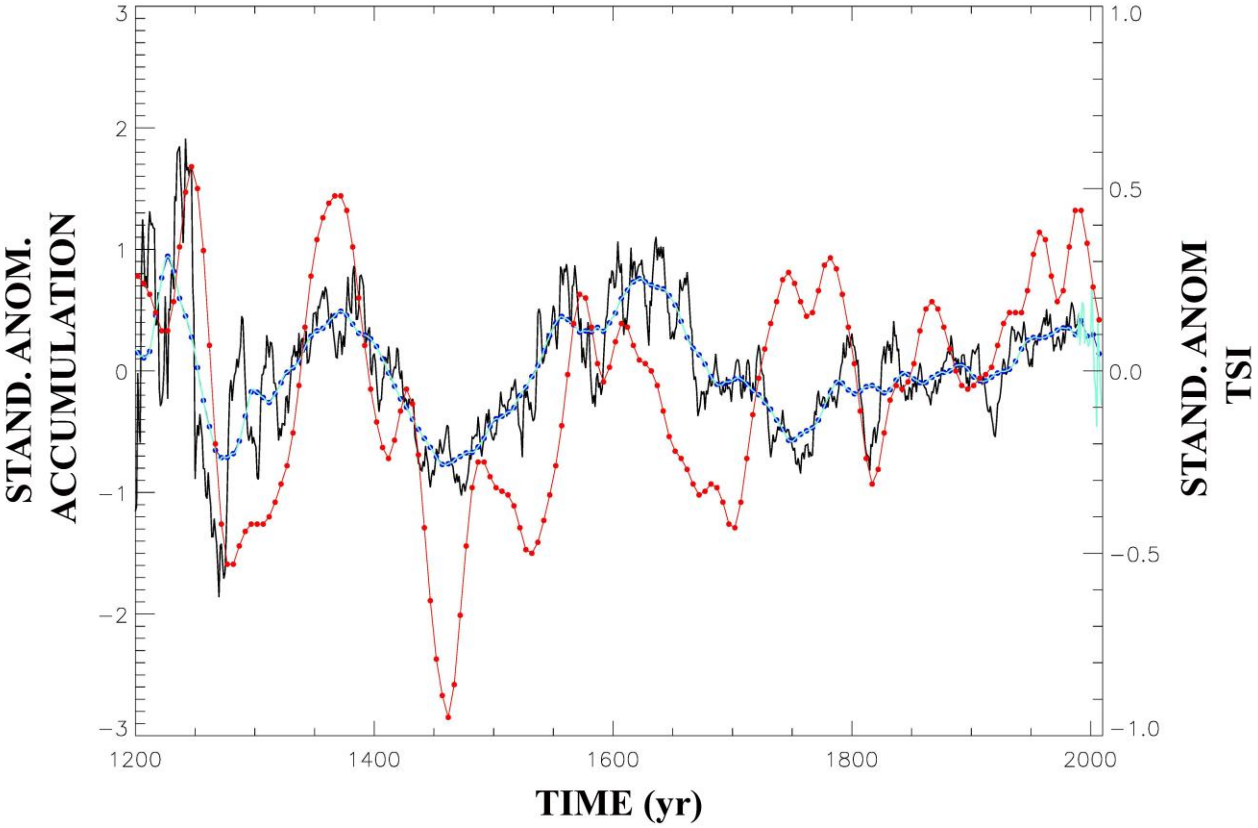
Figure R3. Running Correlation in 100, 200, 300 year time windows (red, green, and blue lines, respectively) between TSI standardized anomaly and accumulation record smoothed as explained in the text. Red, green, and blue line (100, 200, 300 year windows, respectively) represent the threshold value corresponding to the 95% statistically significant level under a two tailed t-student test.

Figure R4. Lower Panel) contour of the annual number of blocked day for each longitude sector (5° wide) from 1980 to 2011. Upper Panel) Trend of the annual number of blocked day time series for each longitude sector (5° wide), expressed as the ratio between the trend per decade and the time series average over the whole period (1980-2011).

Figure R5. Spatial correlation between cumulative annual ERA INTERIM snowfall and cumulative annual blocked days calculated as Scarchilli et al. (2011) for different longitudinal sectors (blue vertical lines and numbers represent the sector involved in the correlation). Black lines represent correlation values significant at 95%.

Figure R6. Geographical distributions of the β40/β150 and β40/βtot ratios (upper panels), and linear relationship between β40 - β150 and β40 – βtot (lower panels) for the assessed (A) and the unassessed (B) datasets. Triangles and circle represent sites with SMB larger and lower than 300 kg m^{-2} yr^{-1}, respectively.

FIGURE R1



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STAND. ANOM
TSI
TIME (yr)
3
2
1
0
−1
−2
−3
1.0
0.5
0.0
−0.5
−1.0
1200
1400
1600
1800
2000

FIGURE R2

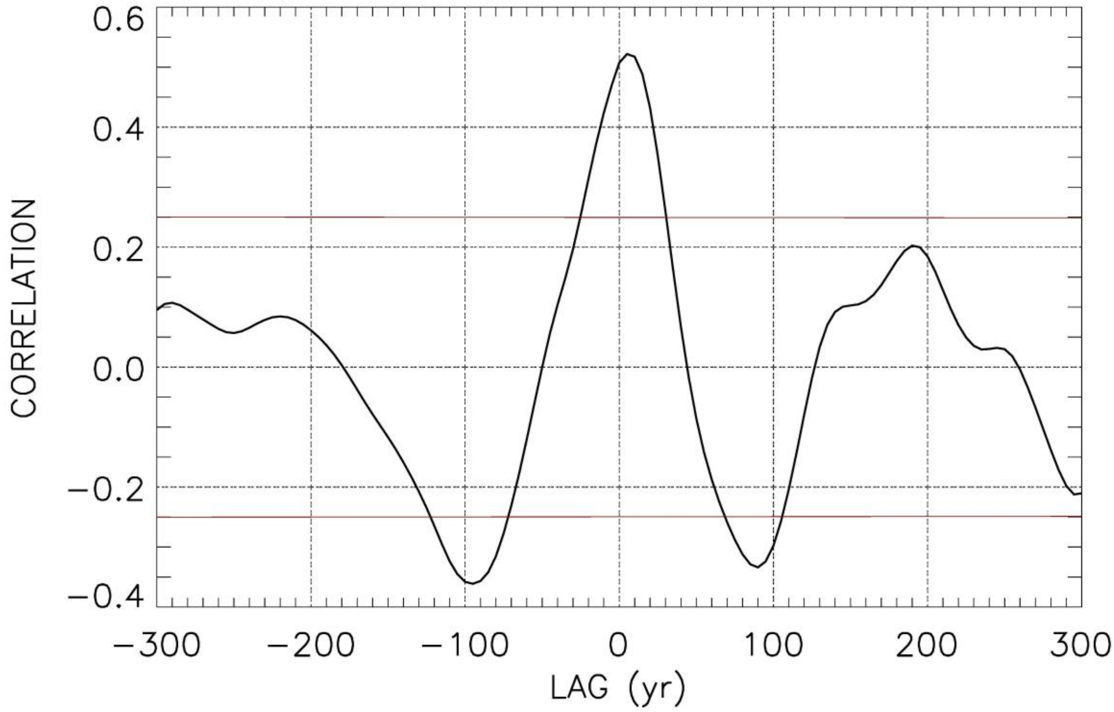


Figure R3

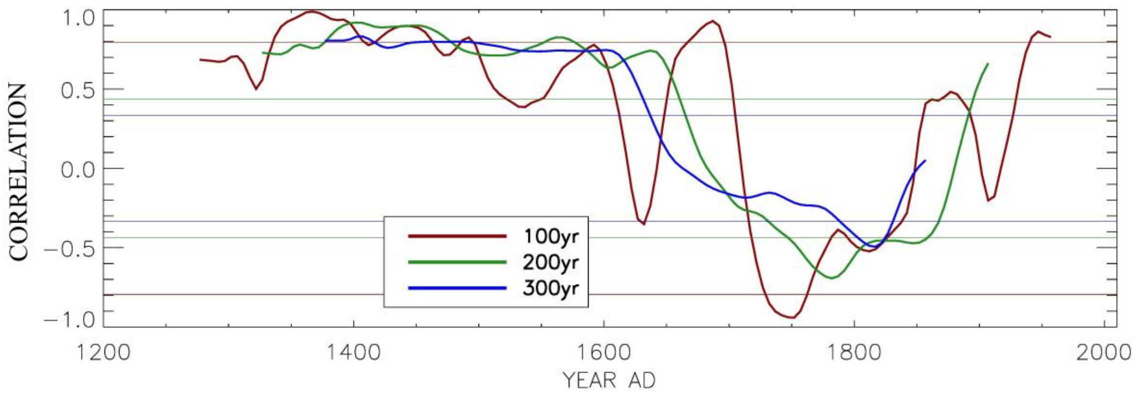


Figure R4

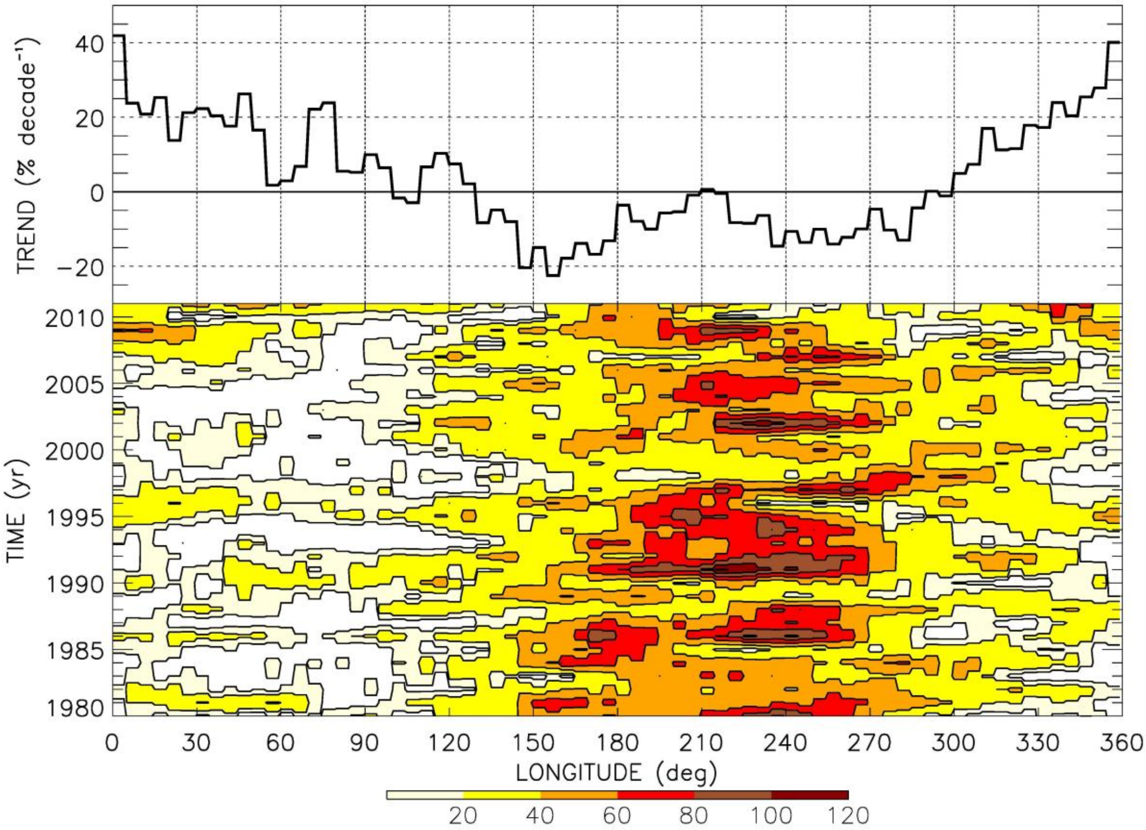


Figure R5

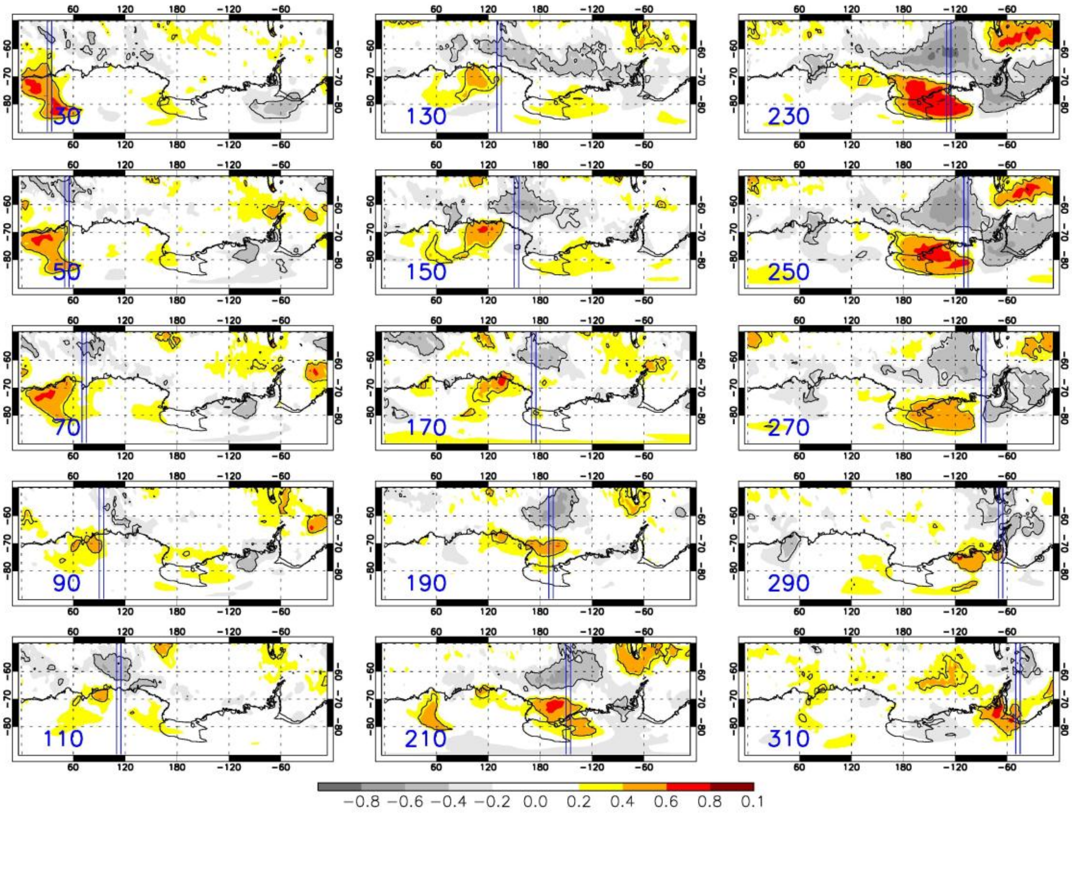
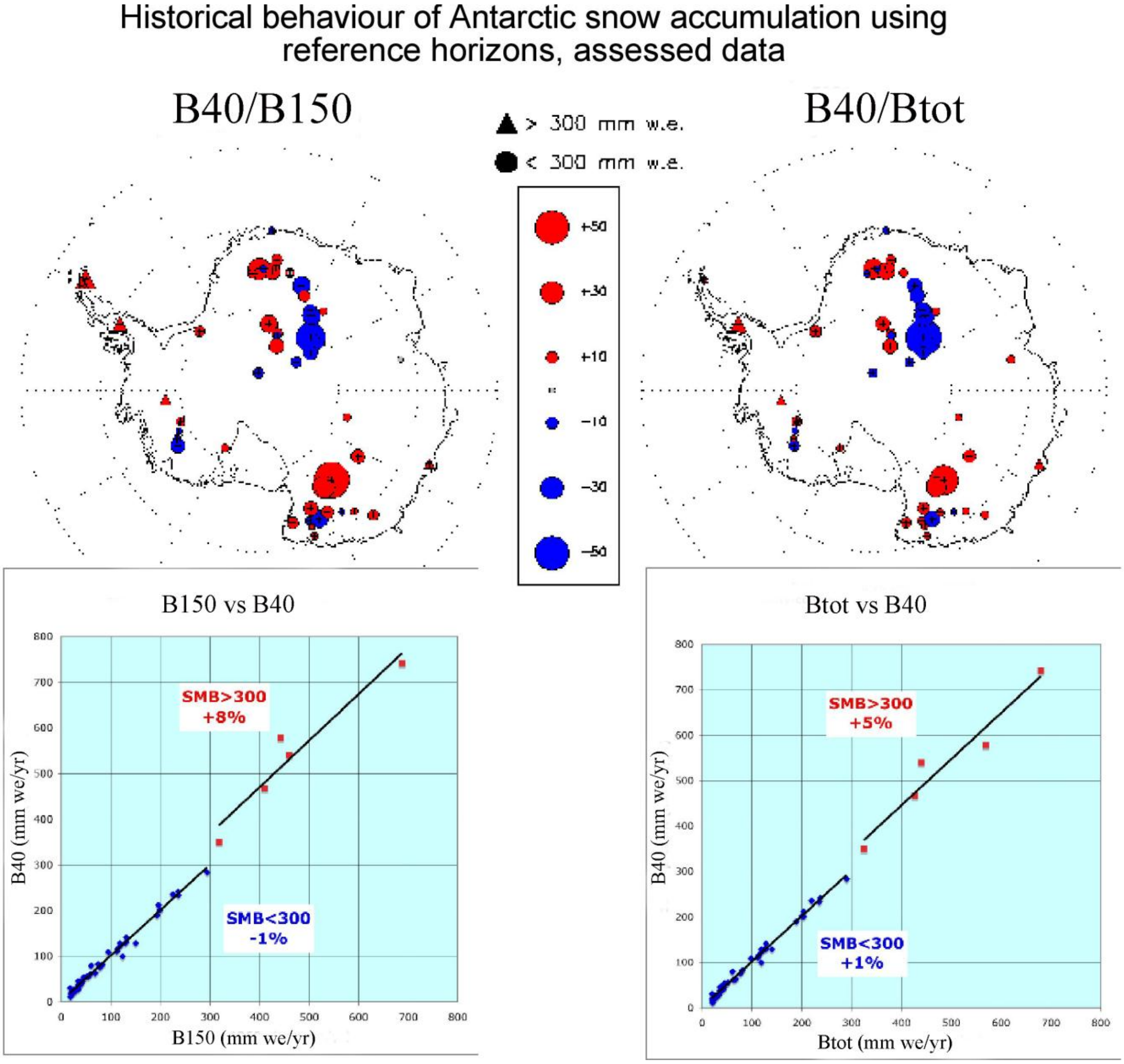


Figure R6

A



Historical behaviour of Antarctic snow accumulation using reference horizons, assessed data
B40/B150
B40/Btot
> 300 mm w.e.
< 300 mm w.e.
+50
+30
+10
−10
−30
−50
B150 vs B40
SMB>300
+8%
SMB<300
-1%
B40 (mm we/yr)
B150 (mm we/yr)
Btot vs B40
SMB>300
+5%
SMB<300
+1%
B40 (mm we/yr)
Btot (mm we/yr)

B

Historical behaviour of Antarctic snow accumulation using reference horizons, unassessed data