Open comments

In the caption for Fig. 9: "where the snow density is higher then 900 kgm−3)" should read "higher than"

R: This has been corrected.

4941-4: The following description is both tentative and generic. ". . ..which can trans- late into runoff to the surrounding ocean, depending on the evolution of the ice sheet hydrological system"

R:The sentence has been revised to read "…which ultimately translates into runoff to the surrounding ocean."

4941-10: reword, .."compared to bare ice surface ablation rates."

R:The sentence has been revised to read: "… increases the ice ablation rate relative to that of bare ice at the surface (e.g., Tedesco et al., 2012a)."

4942-12 and 14: Provide reference

R:We added a reference

4947-21: The mean anomaly should be referred to here instead of on the next page at 4948-6.

R:The reference on the next page is to the MAR albedo change over the 2000-2012 period and not the mean anomaly for 2012. We feel that the following page is the best place to refer to this change, as we discuss it in relation to the calculated MODIS trend.

4947-26: The fact that MAR temperature anomalies are limited close to coast because near surface temperatures are near the melting point for most of the season, does not appear to be a reliable statement. 4948-8 notes the coastal temperature records set. If this was MODIS surface temperature reference the statement would be true, but this was made based on MAR it seems, please clarify.

R:MAR temperature anomalies are limited at the edges of the ice sheet. The language used was a bit ambiguous and has been corrected to reflect that we are referring to the edges of the ice sheet and not the ice-free coast of Greenland. It can be seen in Fig 1a that MAR anomalies along the coast (i.e. defined as tundra) are quite large relative to those over the ice sheet near the coast, consistent with the coastal temperature records. This supports the statement that the lower anomalies along the edge of the ice sheet result from ice surface temperatures that cannot rise above 0°C. MAR is certainly capable of simulating the influence of ice sheet surface processes on air temperature, as it models fully the exchange of energy between the surface and the atmosphere, and there is no indication here and in previous studies that it is inaccurate in this respect.

4948- 14: Add GrIS before melting.

R:The change has been made.

4948-16: Quantify how many days during the 2012 melt season experienced above normal melt.

R:We added the following section 'with 79 of 92 days in JJA with melt extent greater than average'

4948-23: It is worth elaborating on not just the 97% but the number of days with melt area above 70 %, which is level that has only been broken once, in 2002. This is also I believe the only year with more than a single event breaking 60%, and had four according to Figure 2a.

R:We have elaborated as requested. The following text was added: "…the individual product following Mote and Anderson (1995) produced a maximum single-day melt extent of 90% on 11 July, compared to 23% on average for 11 July, and > 97% over the period of 11-13 July. The previous maximum melt extent was 77% on 28 Jun 2002, but three days in summer 2012 exceeded the 2002 maximum. The melt extent exceeded 60% of the ice sheet a total of 10 days in 2012, compared to three days in 2002; only one other year (2005) had even a single day of melt extent exceeding 60% of the ice sheet.."

4948-27: Figure 4b of Mernild et al (2011) does a particularly good job of indicating the temporal variation of maximum melt extent through the season for each year. Would this approach along with Figure 2a, provide a useful visual quantification.

R:We added Fig. 2c, following reviewer's suggestion.

4949-7: A reference in this paragraph to the values of Mernild et al (2011) would be appropriate.

R:We added a reference to Mernild et al. (2011) in the introduction.

4949-11: A key aspect of the earlier initiation of melt beyond the immediate margin is the potential for an earlier evolution of the hydrologic system. One measure of this is supraglacial lake filling and emptying dates. It would be nice to include the timing of lake filling and emptying in 2012 for comparison if the data is in hand. I would suggest just focus on one area such as that of Box and Ski (2007). If the data is not in hand then ignore.

R:There are several studies ongoing on this aspect but the current literature is still lacking and we would prefer not to get into this detail for now.

4949-17: It would be useful maybe in a table to provide the 2012 melt season length at various elevations even if it is only in one region, Northwest Greenland for example.

R:A new Table 2 includes melt duration for a transect in west Greenland at approximately 72°N was included.

4953-7: A brief reference to the K-Transect data would be useful which shows that both S5 at 500 m and S9 at 1500 m initial significant surface ablation noted by the sonic rangers occurred simultaneously around Julian day 144-148.

R:We are not sure what the reviewer is referring to. In our version of the published manuscript, the line 7 at page 4953 does not refer to the K-transect.

4953-15: Possibly the wrong location in the paper but would it be useful to briefly reference with respect to the two main July melt events the Summit Station temperature records?

R:We have added a reference to Nghiem et al. (2012) showing the two main melt events in July 2012 in the near-surface temperatures recorded at the Summit Station.

4954-17: What is the implication for 2013 already in the region just below the ELA?

R:Once the 2012-2013 winter snowfall accumulation melts in the summer 2013, zones with lower than normal albedo (inherited from the summer 2012) could appear. This could induce abnormal melt amount in the summer 2013 even if the near-surface temperatures are not exceptional in JJA 2013. This effect is shown in a sensitivity MAR experiment by using the snowpack state from May 1997. This confirms that the previous summers conditions the melt of the next summers.

4954-29: What are the mean values of the various types since the NAO changed in 2006?

R:Over 2007-2011, 19 % of the JJA days are classified as low pressure-like days, 34 % as anticyclonic days and 39 % as day with a general circulation over Greenland similar to the JJA climatological mean. In 2012, it is 16, 55 and 28% respectively. We refer to Fettweis et al. (2013) (who include now the summer 2012 in their analysis) for more details about this classification.

4956-7: For example what was melt season length in 2012 at the ELA, how many days above the previous normal was this?

R:The number of melt days anomalies are shown Fig.4 b It depends on where the ELA is considered (Fig. 6a) but the melt season was about 1 month longer than normal at the North-West of the ice sheet for example.

Anonymous Referee #2

General comments

This is a good paper about an important event (record surface melting of the Greenland Ice Sheet in July 2012), based on a wide variety of complementary climate model and satellite datasets. In general the analysis is well done, although the quality of written English can be improved in places. I recommend publication once the following points have been addressed.

Specific/minor comments

page 4940, line 25 "During the past decade, surface melting over the Greenland Ice Sheet has been increasing" (and in reference list): please add the following reference:

Hanna, E., Huybrechts, P., Steffen, K., Cappelen, J., Huff, R., Shuman, C., Irvine-Fynn, T., Wise, S. and Griffiths, M. (2008). Increased runoff from melt from the Green- land Ice Sheet: a response to global warming. Journal of Climate, 21(2), 331-341, doi:10.1175/2007JCLI1964.1.

R:The reference was cited and added to the reference list.

p.4941, lines 18-20: reword to "drivers, IN ORDER to overcome the limitations of a single method, WHICH THUS PROVIDES a more more comprehensive understanding of the phenomenon under observation."

R:The sentence was actually rather long, and therefore was split into two: "…drivers. This enables the limitations of any single method to be overcome, providing a more comprehensive understanding of the phenomenon under observation."

p.4941, l.26: add that "satellite-derived snow/ice surface temperature" data have been available since 1979, to make consistent with the other time series datasets listed in the same sentence.

R:Since we do not analyze "satellite-derived snow/ice surface temperature" data from before 2000, we cannot make a claim as to records relative to this period. We only use MODIS for this study.

p.4943, l.13 "Microwave sensors can also detect sub-surface liquid water" - please add up to how far beneath the surface (I think just a thin surface skin)?

R:We added a sentence in the phrase to explain this.

p.4943, l.15: reword to "the difference between the two datasets is generally small (ABOUT 1-2K at MOST but below that on average..."

R:The change has been made as suggested.

p.4945, l.9 "data within 0.04 of the median are not rejected" - how was this threshold determined?

We refer to Box et al. (2012) for details about the approach used for MODIS data.

p.4946, l.26: change "physically based" to "physically-based" .

R:The suggested change has been made.

p.4947, l.3 "A et al. (2012)"??

R:The author's last name is indeed simply "A".

p.4948, l.10: change "Hanna et al. (2012a)" to "Hanna et al. (2012b)".

R:The change has been made.

p.4948, l.24: change "more than tripled the 1981-2010 average" to "more than four times the 1981-2010 average"?

R:The comparison is complicated by the use of multiple satellite products (Nghiem et al.) versus the average for an individual product on a single day. The text has been modified to clarify: "The Nghiem et al. (2012) work was based on multiple satellite products; the individual product following Mote and Anderson (1995) produced a maximum single-day melt extent of 90% on 11 July, compared to 23% on average for 11 July, and > 97% over the period of 11-13 July. The previous maximum melt extent was 77% on 28 Jun 2002, but three days in summer 2012 exceeded the 2002 maximum. The melt extent exceeded 60% of the ice sheet a total of 10 days in 2012, compared to three days in 2002; only one other year (2005) had even a single day of melt extent exceeding 60% of the ice sheet."

p.4948, l.25: change "The 2012-updated trend..." to "The updated 1979-2012 trend...".

R:The sentence has been changed as suggested.

p.4948, ll.26/27: Give percentages for the melt trends discussed on these two lines.

R: We added the trends as percentage.

p.4948, l.10: Regarding the correlation coefficient r=0.23 given, is this statistically significant?

R: We added the p-value for the two trends.

p.4949, l.22: change "standardized melting index" to "standardized melt index".

R:In previously published literature, the index is referred to as the "standardized melting index", so we retain this nomenclature here.

p4950, l.5: change "To this aim" to "Following this aim".

R:The sentence has been changed as suggested.

p.4951, l.6: change "This quasi-absence" to "The relative lack". Change "more sunny" to "sunnier".

R:The suggested changes have been made.

p.4951, l.10: change "rainfall" to "rain" and "snowfall" to "snow".

R:The changes have been made as suggested.

p.4951, l.18: change "Box et al. (2012) ruled out" to "Box et al. (2012) discounted".

R:The suggested change has been made.

p.4951, l.24 "Differences between the two maps..." - which two maps?

R: "Differences between the two maps" has been rephrased to "differences between Figures 7a-c and Figure 7d".

p.4953, l.7: change "modelled bare ice exposed area remained relatively low" to "mod- elled bare ice exposed area remaining relatively low".

R:The suggested change has been made.

p.4954, l.23: change "associated to the NAO" to "associated with the NAO".

R:The suggested change has been made.

p.4955, l.1: change "The identified frequency of the JJA days classified as anticyclonic during summer is the highest in 50 yr (e.g. 2007..." to "The identified frequency of the JJA days classified as anticyclonic during summer 2012 is the highest in 50 years (compared with 2007...".

R:The suggested change has been made.

p.4955, l.5: change "correlated to" to "correlated with".

R:The suggested change has been made.

p.4955, l.26: change "melting in Greenland" to "melt in Greenland" or "Greenland melt". Similarly, change "melting" to "melt" on following line.

R: "melting in Greenland" was changed to "melt in Greenland" and "melting" was changed to "melt" on the following line.

p.4956, l.9: change "2012 hydrological year" to "2011-12 hydrological year".

R:The suggested change has been made.

p.4956, l.22: not sure what you mean exactly by "local conditions" - please explain.

R:We mean the snowpack heritage from the previous summers at the beginning of the 2012 melt season.

p.4960 The reference Hanna et al. (2012b) was submitted to International Journal of

Climatology (NOT Journal of Climate) and has now been provisionally accepted for publication.

R:The reference has been changed to:

Hanna, E., Fettweis, X., Mernild, S.H., Cappelen, J., Ribergaard, M., Shuman, C., Steffen, K., Wood, L., and Mote, T.: Atmospheric and oceanic climate forcing of the exceptional Greenland Ice Sheet surface melt in summer, Int. J. Climatol., accepted.

Figure 5 caption, 3rd line: change "anomalies are given in respect" to "anomalies are given with respect" .

R:The suggested change has been made.

Figure 6 caption, 3rd line: change "anomalies are two times above" to "anomalies are at least twice".

R:The suggested correction has been made.

Figure 6 caption, 6th line: change "run-off" to "runoff".

R:The suggested change has been made.

Fig. 7 caption, 3rd line: change "data is" to "data are".

R:The suggested correction has been made.

Anonymous Referee #1

Received and published: 7 January 2013

This paper uses various methods to identify and explain aspects of the anomalous year 2012 for the Greenland ice sheet. The text is well written en the paper is therefore pleasant to read. The results are very interesting and novel. I can identify a few areas in which the current manuscript should be clarified or improved, which are listed below. I suspect these won't be major hurdles on route to publication, although some comments I consider quite serious.

The second part of the abstract seems out of proportion to the amount of attention spent on atmospheric patterns in the paper itself. This may be because this part of the story was added at a late stage. The information given itself is quite interesting, but please make sure that the abstract and conclusions section give a good representa- tion of the entire paper either by adding detail to the results / discussions sections, or shortening elsewhere.

R:At the reviewer's suggestion, the abstract was shortened so that one sentence refers to identified driving mechanisms of melt (surface characteristics and atmospheric circulation), and the final sentence places the results in a larger context. Considerable attention is already given to atmospheric circulation in the discussion section, but we agree that the conclusions may have overemphasized atmospheric circulation changes. The conclusions have been revised so that the 1st paragraph summarizes the results section, the 2nd summarizes the discussion section (including our identification of both surface and atmospheric contributions to recent records), and the third places our findings in the context of future projections. We feel that the conclusion section is the best place to introduce discussion of future projections as it places this work in a larger context and introduces potential future research topics.

p 4943 l 29-30: Why 2 separate methods (Mote vs Tedesco)? The latter is not used in some parts of the manuscript. It would be better to stick to one algorithm and make sure that the given uncertainty covers all plausible values.

R:We feel that providing results from multiple algorithms applied to the same type of measurements (e.g., microwave) would provide more confidence in our results and we would like to keep this in our paper.

MAR is a fine model, but is still only a model as opposed to the other observational methods you apply. The other tools mentioned in the methods section come with an uncertainty, as they should, but not MAR, on which your results rely heavily. The con- fidence in the results in this paper will be increased if the uncertainties of MAR are discussed as well, for those parameters shown in the results section (albedo, firn prop- erties, accumulation/ablation). In section 3.4 a good opportunity to properly compare MODIS and MAR albedo and to explain the differences is missed (see below).

R:We discuss studies that have validated MAR results in section 2.3. It is difficult to provide quantitative estimates of MAR uncertainty for SMB in particular, given sparse measurements of SMB and its components. The MAR outputs (albedo, SMB) has been mainly validated along the K-transect and at Swiss Camp but this does not mean that the MAR performs well in the other areas of the ice sheet. With respect to the spatial variability of the SMB, uncertainties along the K-transect can not be generalized to the whole ice sheet. In addition, there is no time series of the firn properties measurements.

As for the re-analyses, uncertainties can be computed only by carrying out ensemble simulations by varying some parameters in the models. But, as for ERA-INTERIM or MAR, performing ensemble simulations is computationally expensive (requiring of the order of at least 6 months). A parallelized version of MAR is being developed and this will provide a mean to perform sensitivity analysis.

However, we have added a discussion of the results of Vernon et al. *(2013) and* Rae et al. (2012)*, who each compared simulations of SMB from four methods (including MAR)*

applied over the Greenland Ice Sheet with available observations, to section 2.3. Alexander et al. *(2012; reference below) conducted a comparison between MAR and MODIS albedo for which a manuscript is in preparation. Preliminary results are referred to in section 2.3 and section 3.4.*

Alexander, P., M. Tedesco, X. Fettweis, S.A. Margulis, M. Navari, J. Box, and C. Chen (2012) Assessment of modeled albedo and bare ice extent (2001-present) in the regional climate model MAR using satellite data, *Fall 2012 AGU Meeting,* C13F-0687.

Please add detail on the subsurface scheme of MAR in 2.3. How is refreezing calculated? How many layers are there? How is the firn layer initialized at the start of the run, using which densities? These are important questions given the results and 'synthetic sensitivity experiment' presented later on.

R:Details on the number of snow and ice layers, model initialization, and refreezing have been added to section 2.3.

Last paragraph of 2.4: Not completely clear to me. Please clarify (simplify) or shorten. I don't understand what signal is lost by using this 'averaging kernel' nor what the scaling factor does.

R:We replaced the existing section with the following:
"We compute the temporal mean of the monthly fields and subtract that mean from each field, so that the residuals represent the monthly departures from the mean. We convolve each monthly residual field with a Greenland averaging kernel chosen to minimize the combined measurement error and signal leakage, as described by Velicogna and Wahr (2006), to obtain an estimate of Greenland mass-per-area in units of cm of water averaged over the ice sheet. Like any filtering process, this convolution has the potential of causing a loss of signal. To correct for this, we follow Velicogna and Wahr (2006) and determine a scaling factor by applying this analysis procedure to several simulated, but plausible, ice loss patterns. We multiply each monthly mass-per-area estimate by this scaling factor to obtain variations in the total mass of the ice sheet (in Gt) about its temporal average."

All methods get plenty of attention in the methods section (which I like), except for the reanalysis data; please add a few lines.

R:We think that the reanalysis data sets do not need to be described again. We have only added references to each of them.

Caption Fig 1b. Typo: 'for the'. Also, the figure gives JJA temperature, not annual mean.

R:The suggested corrections have been made.

p 4948 l 4-5: Why are the trends in MODIS LST and MAR air temperature so

different? They are not the same parameter, but you'd expect the latter to increase as least as much as the prior because of increasing near surface inversion strength (since LST will be limited to 0 C at all times). I was hoping for an explanation of this here or later on in the paper, but couldn't find it. It is important to explain though as you'd want to have both data sets appear as reliable as possible. Also, please add the +- value for the MAR trend of 1.4 C.

R:The MAR (resp. ERA-INTERIM) JJA near-surface GrIS temperature trend over 2000-2012 is 1.3°C (resp. 1.1°C). As explained before, it is difficult to evaluate the uncertainty in the model results without carrying out ensemble simulations. Knowing that the MAR inter-annual variability is driven by the forcing ECMWF reanalysis, the trends simulated by MAR or ERA-INTERIM are similar.

However, observations were assimilated in ERA-INTERIM and indeed, the limitation to 0°C of the surface temperature should induce lower trend in LST than in the near-surface temperatures. This suggests that MODIS could overestimate the trend as a consequence of sensors changes in the MODIS based time series (Box et al., 2012). This interpretation has been added to the discussion of the trends.

Please explain why maximum melt extents for 2012 don't match in figure 2 A and B.

R: Thanks for pointing out this discrepancy. We found an issue with the melt extent data in plot 2b ad fixed this.

p 4948: I do not find it particularly interesting that the melt extent has tripled, especially since it deals with just one event that did happen in 2012, but not in other years. These statistics would be completely different for two other days in the year. Moreover, if one warm event were to occur every year from now on, causing the entire ice sheet to melt during a short period, you would see no increase in the melt extent any more (beyond 100%), which would not indicate climatic stabilization. The changes in melt extent are amplified by the hypsometric effect and therefore a deceitful climate indicator. One page later, you mention the record in the melting index, which also suffers from this amplification and therefore also not a very useful parameter.

R:The comment about the tripling of the melt extent has been removed. We are puzzled about the suggestion that an indicator is "deceitful," a term which suggests a deliberate attempt to mislead. The indicator represents a physical change in the surface of the ice sheet, which has implications on surface albedo, etc. While it is true that it has shortcoming as a climate indicator (i.e., the hypsometric effect mentioned by the reviewer), it is nonetheless a useful indicator of the effect of climate change on the surface of the ice sheet. While the maximum melt extent is limited to 100% of the ice sheet on any given day, other metrics given, such as the duration of melt, are available.

p 4949 l 15: Give Tedesco values as well or use one algorithm as suggested earlier.

R: Done.

Fig 4: Why limit this figure to JJA? There are many days of melt missed out on. For instance 2010 had a very warm month of May on the west coast, and even melt late in the year. These melt events count as well, and will cause 2012 and 2010 to give more similar values in Fig 4c. You do not take into account the lengthening of the melt season as data are presented currently.

R:The JJA period was chosen to be consistent with previous maps published in the literature. Graphs showing the entire year are available in Figure 2. A map showing the entire year does not look substantially different than the JJA map.

Which brings me to the other important comment on this section, which I find important. How do you define the onset of melt? In many years the ice sheet experiences one or more melt periods before the actual summer melt season starts in June. You need to be very clear in how the onset is determined, because one can find arguments for and against including these sometimes substantial melt periods. Not including this information leaves the reader suspicious.

R:The melt onset is defined as the first day of melt during the year. The definition has been added at first reference to melt onset.

One of my main concerns is also related to this. In section 3.2, and later on in the manuscript, you make short but powerful statements concerning the entire ice sheet. 'Melt at this elevation started then'. This is over-generalizing matters. The atmospheric conditions can be highly variable over relatively short distances, so at best you can discuss regions of the ice sheet in this fashion. Especially snowfall, which gets attention later on, can't be discussed for Greenland as a whole. Please discuss in terms of regions (ablation vs accumulation zone, north vs south, etc).

R:We agree with the reviewer that anomalies are spatially dependent. But, we feel that discussing these anomalies at the scale of the whole ice sheet is enough here for the scope of this study. When it is needed, as for the precipitation (North vs South), we have regionally discussed the anomalies.

p 4950 l 16-23: You need a better spatial view discussing accumulation with Fig. 5. Where was it that the precipitation fell? If it falls in the ablation area, it will matter to melt/runoff. If it's at high elevation/latitude, it won't as much. Besides, 5b shows more accumulation in 2012 than in 2010, but 5a doesn't (really); please explain.

R:We added the snowfall map anomaly in Fig. 6

Figure 7 is a nice figure but can be improved. I don't see the point in having 3 MODIS albedo snapshots (a-c) while you present JJA MAR albedo. Please show JJA for both MODIS and MAR. By doing so you can better identify the differences between the two methods. Now the differences between a-c and d are explained by 'intrinsic differences between the approaches' (which is not an explanation), 'the difference baseline peri- ods'

(which would be solved by including a JJA MODIS map), and 'the spatial resolution of the two data sets' (which doesn't cause different albedo patterns and can be solved easily). Since albedo is a prime driver over the SMB this deserves attention, also to build confidence in MAR performance which this paper relies on.

R: We switched the MODIS figures with one JJA figure, as requested.

Table 2: Why is the error only listed for summer values and not for annual mass change? And how do you justify calculating the error as the 2 sigma of the smoothed versus unsmoothed time series? This gives a value of temporal variability, not of (measurement?) error. Please explain what you mean by this 'error'.

*R:*We added the following text to better explain the procedure and account for reviewer's request:

End of section 2 : The scaling factor is computed as described by Velicogna and Wahr (2006), and is determined by applying this analysis procedure to several simulated, but plausible, ice loss patterns. We use the mean of the scaling factors from these different patterns, as our preferred scaling factor. And we use twice the range of these scaling factors as the scaling factor uncertainty when computing the mass loss uncertainties described below
End of section 3.5: The uncertainties of the summer and yearly CMA results are computed by smoothing the monthly CMA values, subtracting that difference from the unsmoothed values, and computing the 2-sigma scatter of the residuals. For the summer 2003 and 2011 results, we also add the uncertainty caused by using May values instead of June values. We add (in quadrature) this uncertainty determined by the scatter in the time series, to the uncertainty caused by errors in the scaling factor (see above), to obtain the total uncertainty.

We also added uncertainty in the table on the annual values.

Section 3.5: Please discuss the downside to using a fixed period for the hydrological year (accumulation / ablation in one year being attributed to another, if e.g. melt were to occur in fall) and mention the impact on your results.

R:This has been acknowledged in section 3.5. The MAR model shows that the melt amount occurring in fall is more than 100 times lower than the one occurring in summer and therefore, the hydrological year can be considered as constant trough the investigated period.

Section 3.5: Please compare GRACE mass loss to MAR SMB to see if the 2012 mass balance anomaly can be fully explained by the SMB or whether there is an ice dynamic component to the story as well.

R:For this, we refer to Sasgen et al. (2013) who compare in details MAR outputs and

GRACE measurements over 2012. They show a very good agreement (biases of ~10 GT/yr) between MAR and GRACE as well as that the 2012 mass balance anomaly can be fully explained by the SMB.

Fig. 9: Include in the caption that this figures shows MAR data.

R:Done

p 4954 l 3-17: This is an interesting experiment, to re-run the model for 2012 with the 1997 snowpack. But how do we know that the 1997 or 2012 firn conditions in MAR were anywhere near reality? If they weren't, then this experiment is misleading.

R:Spring 1997 was chosen because it comes notably after the year 1996 when the SMB was one of the highest of the last two decades due to heavy snowfall and a cold summer. Therefore, there are few low albedo zones inherited from previous summers in the spring 1997. In addition, most of them are covered by about 2 years of snowfall accumulation. Using the spring 1993 should give similar results. The aim of this sensitivity experiment is only to show that the previous summers conditions the melt in the next summers. This explanation has been added in the main text.

I understand that there aren't many firn measurements to use to validate MAR, and that this is not something you'd want to spend too much time on since this paper is not meant as a MAR validation effort, but the reader still needs to know the difference between the 1997 and 2012 firn to interpret this paragraph. How about including a plot showing firn temperature and density for a cross section of the ice sheet, for 1997 and 2012? This will allow the reader to interpret the green line in Fig 6,7,9 much better and is interesting in itself.

R:We added a figure showing the different firn and temperature conditions in 1997 and 2012.

Conclusions section: Please mention the uncertainties as well, not just the SMB anomaly value etc.

R:Uncertainties have been added for GRACE.