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The Cryosphere Discussions

Interactive comment on "Evidence and analysis of 2012 Greenland records from spaceborne observations, a regional climate model and reanalysis data" *by* M. Tedesco et al.

Anonymous Referee #1

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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 representation of the entire paper either by adding detail to the results / discussions sections, or shortening elsewhere.

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.

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 confidence 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 properties, 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).

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.

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.

All methods get plenty of attention in the methods section (which I like), except for the reanalysis data; please add a few lines. Personally, I would also shorten section 2.1, e.g. by removing details such as in p 4943, l 3-4, and by referring to other papers.

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

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.

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

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.

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

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.

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.

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).

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.

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 periods' (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.

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'.

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.

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.

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

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. 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.

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

Interactive comment on The Cryosphere Discuss., 6, 4939, 2012.