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The Cryosphere Discussions

Interactive comment on "An updated and quality controlled surface mass balance dataset for Antarctica" *by* V. Favier et al.

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Dear reviewer,

Thank you for your thorough review and your relevant comments. Please find hereafter our response to your queries.

1. You suggested that "The compilation of SMB" and the "test of the climate models to express SMB" should be discussed in separate papers, and you write that "comparison between one of climate models to express the SMB and the observational SMB data set" is not "a very necessary discussion" and that discussions should focus more "on the geographical distribution of the data points".

This paper does not aim at testing climate models but rather at 1) presenting the current

state of available data, 2) showing that selecting the data and assuming only reliable data (according to an objective criteria) was drastically changing the final Antarctic SMB, and 3) demonstrating that data distribution is clearly too low (mainly in coastal areas and on WAI) despite more than 50 years of heavy field observations.

Regional features are evidently impacting SMB distribution but discussing on "continentality, location of sites relative to major and minor ice divides, surface slope and so on" would not give more definitive conclusions than elevation does, because these aspects are interconnected and sometimes have opposite effects on the SMB depending on the selected area. Indeed, geographical features present an interest regarding to the way the depressions intrude inland and provoke accumulation, or to the way the katabatic wind affects snow distribution. But SMB is a complex multivariate consequence of geographical features whose variations are impossible to describe with simple views of mind, based on each separate variable. Conversely, the use of an atmospheric model allows this because models are expected to physically consider SMB variations due to geographical characteristics.

Here, the reanalysis data were used because an important number of meteorological observations is assimilated (e.g. Bromwich et al., 2011), which is why reanalysis data are much more reliable than classical general circulation models. Moreover, Bromwich et al. (2011) observed that "ERA-Interim likely offers the most realistic depiction of precipitation changes in high southern latitudes during 1989–2009". Thus we believe that using ERA data was the best way to assess a correct SMB distribution in Antarctica according to geographic features, without performing a complex downscaling step.

2. Thus, when you write "By doing so, one of the two papers can provide more focus on description/construction of the database."

We believe that the paper should not be separated into 2 papers, because we do not estimate ERA-Interim quality, but rather use the model to assess the geographical impact on SMB distribution and use the data to study whether the database offers an

accurate description of the main SMB distribution features in Antarctica. This showed that the Antarctic SMB is not yet accurately constrained and that getting new data still represents a crucial issue for our community. This variable is often placed as a second order problem in scientific community due to the threat caused by acceleration of several outlet glaciers in Antarctica, and because satellite data (altimetry and gravimetry (Sherpherd et al., 2012)) were successfully compared suggesting that the SMB is now precisely estimated. However, the distribution of the SMB at a regional scale and its future variation still present very large uncertainties and we should not reduce our effort to get new in situ data. This is crucial, for instance, for a better validation of regional circulation models.

There is still a large place for other studies to test climate model quality against field data, as performed, for instance by Agosta et al. (2012) in Adelie Land. Such analysis would require comparing several different models and not only ERA-Interim to get a more general vision on model quality.

In conclusion, we suggest not to separate the paper into two publications and to keep the previous paper structure instead.

3. You write that "It seems to me that the authors' method of the rating (A∼C) has somewhat subjective aspect"

First, we did not propose the filtering process but objectively chose a rating as initially proposed by Bull (1971), and subsequently by Magand et al. (2007). The interest of this data filtering process has already been fully discussed in peer reviewed in publications (Magand et al., 2007; Krinner et al., 2008; Genthon et al., 2009).

Second, in this response letter, we propose a comparison between data presented in JARE data reports and in Fujiwara and Endo (1971) (See Figure S1). We first compared Fujiwara and Endo (1971) SMB estimates by stratigraphy with stake measurements performed in 1968-1969 (Figure S1a&b). We also compared these data to a multi-year average of stake measurements performed along the same locations

until 1975 (Figure S1b, S16 to S122, data available in JARE reports). Finally, we compared these data to long term SMB measurements on Syowa to Dome Fuji Route (Motoyama et al., personal communication). We observed that while stratigraphy data do not present any spatial variation close to the coast, the multi-year stake data clearly show an increasing trend with decreasing elevation (Figure S1a). This important increase close to the coast is also observed along the main stake-line between Syowa to Dome Fuji station (Figure S1b), which is located a few kilometers away from S16 to S122 stake-line. This comparison shows that stratigraphy data do not display the local SMB distribution, confirming that the rating proposed by Bull (1971) is relevant. Moreover, the one-year stake data from Fujiwara and Endo (1971) are displayed on the same figure, suggesting that erosion or ablation occurred between -69.6°S and -70.9°S. However, the multi-year average of stake measurements suggests that this area presents a positive mean accumulation. Thus, this comparison suggests that one-year stake data are not reliable, as suggested by Bull (1971). This comparison is only a focus on a small region, but the same conclusions would be obtained in other regions.

In present paper, we did not perform the same critical analysis of the entire rating proposed by Bull (1971) and Magand et al. (2007), because 1) this is not the focus of current paper and 2) this point has already been discussed and published several times in the past (e.g., Bull, 1971; Magand et al., 2007; Krinner et al., 2008; Genthon et al., 2009). Thus, such comparison would not offer any additional new result. Finally, this rating method has also been used in other studies, suggesting that it assumed that the data reliability and accuracy is improved when data are filtered with an objective data rating (e.g. Lenaerts et al., 2012).

4. You suggest that "even if the method is handled as B or C by the authors, some of data should be accurate and reliable depending on depositional conditions of each area." For instance, you observe that "stratigraphy provides reasonable results in low accumulation site in polar plateau if wind is weak and snow surfaces are smooth".

This would lead to a subjective rating. How can we objectively define a weak wind, a smooth surface, or a polar plateau? Moreover, you write that "missing rate of some annual layer is rather small even at Plateau Station." but this is purely subjective. Indeed, at Dome C (for instance), observation of stake networks suggests that erosion is present at 30% of the stakes, although the area presents a positive mean distributed accumulation every years (see Glacioclim stake data at Dome C, http://www-lgge.ujf-grenoble.fr/ServiceObs/SiteWebAntarc/dc.php), and the locations where erosion occurs are moving with time. Hence, missing some annual layers is rather high at plateau stations. Including such proposition is clearly subjective because depositional conditions are impossible to estimate without an accurate SMB measurement. We feel that it is preferable to apply objective reproducible criteria, even if they undoubtedly lead to the exclusion of some valid data points, instead of including potentially doubtful data in class A.

5. You write that "Agreement of the SMB values may be just accidental." You also suggest that "it is plausible that SMB data with particular local features were removed from the dataset and it can be one of major reasons why average SMB of the remaining data increased."

We added a new figure (Figure 7b) to show that non-"A" rated data do not correctly fit with the modeled SMB distribution. This is not the case in Figure 7a, where the SMB from "A" rated data exactly follows the same trend as ERA-Interim outputs. Moreover, at low elevation, the non-"A" rated data are clearly too high according to the model. This is in contradiction with your remark on the remaining SMB data increase. You write that "the authors removed a lot of data from Lambert Glacier drainage basin". This is speculative, because even if several data from Higham and Craven (1997) were removed, we largely increased the number of reliable data by including data from Ding et al. (2011). You write that the same is observed "on the Ross Ice Shelf side sites". It is worth noting that blue Ice data were excluded in our mean SMB computations, in agreement with your remark. However, we can observe that the main change on Ross

Ice shelf appears in data density, while the general pattern of data distribution over the iceshelf is conserved. Actually, the main differences between non-"A" rated data and ERA-Interim data are observed in Marie Byrd Land, along Pine Island and Thwaites Glaciers, where unreliable field data were suggesting higher SMB than the model.

6. 'In such cases, it seems that it is not always beneficial for our community to reduce number of old data.'

It is crucial observing that we did not remove any measurements in the full database. Both databases (full and rated A) will be available on the website, so that any scientists will have the opportunity to perform their own data filtering based on different criteria. Any better rating will be of great interest and the classification proposed here can certainly be improved and refined in future work. However, please note that because data density is low, inaccurate and bad data have a large impact on local to regional SMB estimates. Several unreliable data are generally associated to a same field campaign and unreliable data are distributed over large areas where measurements were never performed again (for instance, see traverses in Marie Byrd Land). Assuming that these data may be inaccurate suggests that these data strongly affect SMB interpolations. It is crucial to keep in mind that data interpolation using remote sensing data (e.g. Arthern et al., 2006) directly includes biases and propagates these biases over larger areas. A data filtering process is thus absolutely necessary to remove these large biases, and the proposed data rating is assumed to be objective.

7. Scientifically, much more important is the estimation of error size.

The error size is almost never given in any publication with SMB data, it is thus totally impossible to include this datum in the database. Moreover, this suggestion is indirectly the same as assuming that several data are unreliable, because unreliability was initially based on measurement accuracy (Bull, 1971; Magand et al., 2007). You note that the accuracy of each method may change according to the study areas. However it was important defining an objective process regardless to the study area. When data

are poorly reliable, a correct way to assess data accuracy is getting a cross validation with another method. This point justifies the inclusion of a data rating “B”, which are considered as reliable only if a second observation confirms the SMB estimation. In the present study, we did not include rated B data because it was more objective including rated “A” data only. However, this does not change the main final conclusions of the analysis.

8. You write that ‘description/discussions about the SMB data compilation seem still have some large rooms to be better’ and suggest GPR-based SMB data should be included in the database, since data are nowadays published in papers and would be rated “A” if they were considered in the present study.

The main problem of GPR is that - unlike stake measurements for example - it is an indirect measurement of SMB, thus it requires an interpretation which could lead to errors. Difficulties in signal processing or in signal interpretation and picking of the reflectors are the main possible sources of error (Verfaillie et al., 2012). Moreover, even if radargrams are available in figures, the age of reflectors is generally not identified in publications. Hence, performing a similar database with GPR data requires additional work, and requires researchers to freely provide their data. This data collection is under process by NASA (SUMup working group – PI: Lora Koenig), but this was beyond the focus of present paper. Future addition of the GPR-based SMB data will be performed once the NASA will publish their database. A short discussion is included in the text on this point.

9. When volcanic markers are used to calculate SMB, SMB data quality should be as good as using nuclear test markers. Handling of the volcanic markers (e.g., Pinatubo and Agung) is missing in the discussion and table.

This was an oversight in the Magand et al. (2007) table: volcanic eruptions are of course "A" rated data.

10. You write ’To clarify collection conditions of the datasets, I suggest the authors to

present a list (table) showing data sets included and excluded.’

Data are related to more than 200 publications, and several data must be related various additional papers. We do not see the interest of including a table with 200 full references directly in the paper, when this may be available in the database. This only allows scientists to verify that their data were correctly included in the database, but they will have this opportunity once the database will be available online.

11. “Appearance in the paper or in supplementary information is much better than appearance in the web page of the project. Web page is always under risk to be easily modified or deleted.”

We decided including the database on the Glacioclim observatory web page, because Glacioclim is a long term observatory with perennial funding from the French government. The basic role of such observatory is to insure long term observations on study sites and data distribution on the web. This observatory already accounts with the longest available mass balance time series in the Andes (Zongo and Antizana Glaciers) in the Himalayas (Chhota Shigri), and accounts with mass balance series in the Alps that exceed 60 years. Our data in Antarctica are from the 70’s (Agosta et al., 2012).

This guaranties that the webpage will not be deleted, and that data will not be modified without informing users. Moreover, delivering the database on a website will allow permanent updates which is much better than a fixed table in a published paper. Our purpose is to create a “living database”. This will also allow a continuous interaction with researchers to allow updates, but also corrections and remarks. The database will include metadata that we cannot offer in a simple paper.

12. You argue that “Reassessment and critical views for data collection of the V99 data are also necessary because there is no guarantee that V99 dataset was complete”.

We carried out a critical reassessment of the V99 database and we retrieved and consulted every publication and database cited in V99 to assess whether the information

was complete or not, and tried to perform updates before 1999 as complete as possible. We retrieved several reports sometimes prior to the 60's, which do not exist anymore in the research institutes where data were acquired. For instance, since the previous paper submission we retrieved data location from 70 ice-cores which were initially published by Clausen et al., (1979): 15 of these locations were not correct in V99. This induced an important change for "A" rated data on Ross Ice-shelf, because the lack of a reference for coordinates was leading the data to be unreliable. This demonstrates that 1) our procedure is correct because a full verification of the V99 database is necessary, and 2) that this database presents an important interest in being in permanent evolution.

However, when you write: "that large portion of the data obtained by Japanese glaciologists, published in "JARE Data Reports" (http://polaris.nipr.ac.jp/∼library/DataReports/DRglac.html) or folio series (Takahashi and Watanabe, 1997 and Takahashi et al., 1994) are not yet included in the database." This assertion is not correct. We analyzed again JARE reports and confirm that density measurements are rare, and almost only performed along Syowa to Dome Fuji route. Even though Takahashi and Watanabe (1997) presented a large amount of data for the all traverses performed in DML and Enderby land, the water equivalent were computed using a relationship between density and altitude only (see Figure 3.4 in Takahashi and Watanabe, 1997). This estimation is clearly not sufficient to get a correct estimation of the mean water equivalent. Even very simple models are based on more complex approaches, and include at least the mean annual temperature, annual accumulation rate, and the initial snow density (Herron and Langway, 1980). Nevertheless, even such a modeling is not correct to assess good estimates for density. Snow density should be measured in the field because time and spatial variations are very large. For instance, along the Fujiwara and Endo route (1971) density are available at different dates for several stake locations (see JARE reports). At each point, density measurements present differences reaching until $\pm$20% (Figure S2a). The same analysis was performed for the Glacioclim Samba stake network,

where density measurements are performed every year at every stake, until 250 cm (Figure S2b). Data showed that temporal variations at each point could reach 20% and spatial variations could exceed $\pm$25% for the same year. This directly affects accumulation values (in water equivalent) by amount of $\pm$25%. As a consequence, despite the very large amount of stake measurements in JARE report, only few data were retained in the final database because we did not keep accumulations in snow equivalent.

13. The authors should be careful to exclude Fujiwara and Endo (1971) work because age span is completely different from data of recent Syowa - Dome Fuji route since '90s.

We digitalized data from Fujiwara and Endo (1971) and included it again in the full database.

14. Besides, route trace is different between Fujiwara and Endo (1997) work and Motoyama et al. work. It seems to me that this replacing Motoyama works from the Syowa – South Pole traverse is a rough handling of earlier data. Both are valuable.

Both are valuable, but Fujiwara and Endo data are not "A" rated, and do not appear in Figure 1c. This is also justified by results presented in figure S1 (this response).

15. However, I believe that collections of the SMB data by the authors can be significantly improved surveying both published and unpublished data. Another example is more exploration of old archives. I note that large portion of the

We do not understand how we can easily retrieve unpublished data. Even though a request on cryolist may allow getting few data, it is plausible to expect that high quality data have generally been published by their author. One may ask why these data have not been published before? The data quality may be questionable, and this may include a major issue on measurement reliability and accuracy. Moreover, any laboratory disposes from many old unpublished archives. But authors generally migrated

to other topics and may not remember accurately the measurements accuracy. Sometimes scientists deceased. Getting an idea on measurement quality and accuracy in this condition is almost impossible. Finally, our approach proposes an interactive online database, which gives an opportunity to scientists to participate and submit new data or old unpublished data. Again, in our mind, it is absolutely crucial for the database to stand on firm ground in its initial version.

16. In principle, we cannot increase number/distance of ground-based data so much. Considering such a condition, I hope to find some discussions to build future SMB data points network in Antarctica.

We agree with this comment. Getting a correct estimate of the Antarctic SMB at a regional scale cannot be done with field measurements only, and cross comparison with remote sensing data is needed. However, performing new field measurements is a question of priority and funding. Unreliable measurements are mainly located on WAI where traverses are not impossible to perform because many scientific bases are located there. Previous traverses in these regions reflect this point. Several old traverses have already been revisited recently (e.g. Anschütz et al., 2009, 2011). New SMB measurements may be performed assuming current knowledge and measurement technologies, including GPR and microwave data. One should focus on areas where data number is low (Antarctic Peninsula, between Marie Byrd Land and the coast, or close to McMurdo station). A correct study of low elevation is more difficult because outlet glaciers present large crevasses that limit progression. Acquiring data in the Antarctic Peninsula is also more complex than over the plateau, but with the large range of logistical possibilities at the end of the 21st century (planes, skidoos, tractors), studying these areas is not impossible. We included a paragraph on this discussion in the text.

Minor comments:

1. The total number of SMB data was not removed because it gives information on the

difference in data amounts with V99 database.

2. The first paragraph of the introduction was improved and references were included.

3. Several papers for several IPY inland traverses were cited in the introduction. We also included the references proposed by reviewer 2.

4. You suggested including the “surface slope and wind” because they can be extracted from DEM. However, the interest of the database is not to include information which may be deduced from models or DEM, but to collect information on field data available in publications. Surface slope and wind mean speed are almost never available in publications.

5. The possessive case (our) was removed and the references to Glacioclim-Samba were removed and simplified.

6. A short paragraph on how we can efficiently increase the number of SMB data in future field campaigns as a community effort was included in the discussion.

7. Comments on the geographical significance of each data are assessed in previous paragraphs of this response letter.

8. The major cause of the stairs-like distribution of the histogram in Figure 4d is the presence of data from large stake networks (e.g. around Lambert Glacier (Higham and Craven, 1997: Ding et al., 2011), that span only a few years. This was included in the text.

9. One sentence was added to suggest that “scientific community cannot rely only on stake data to increase data density for continental scale.”

10. Instead of “reliable data”, we now write “A” rated data

11. You suggest that “data should not be rejected with a reason of inaccurate elevation data”. You are partly right, but note that 1) this concerns very few data, 2) this is mainly performed to allow comparisons between field data and ERA-Interim. Indeed, these

data are located in important slope areas where the DEM present large differences with the actual elevation observed in the field. Here we did not remove data because they were inaccurate, but because differences with the terrain used in ERA-Interim computations might be too large. Thus ERA-Interim SMB data are computed for very different elevation conditions, and cannot fit the observed SMB.

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Figure Captions:

Figure S1: a) Snow accumulation along Fujiwara and Endo (1971) route. Red squares are SMB data from stratigraphy in Fujiwara and Endo (1971), yellow dots are multi-years average of stake data along the same route, and blue squares are 1 year stake data from Fujiwara and Endo (1971). b) Red squares are SMB data from stratigraphy in Fujiwara and Endo (1971), and blue stars are the 20-stake running mean of accumulation data along Syowa to Dome Fuji traverse.

Figure S2: a) snow density along Fujiwara and Endo (1971) route. Blue squares are data in 1970 (JARE report 2), red squares are density in 1974, and green triangles are data in 1982. b) Mean snow density between the surface and 2.5 m, measured along GLACIOLCIM-SAMBA stake network between 2008 and 2012.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C2587/2012/tcd-6-C2587-2012-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 6, 3667, 2012.

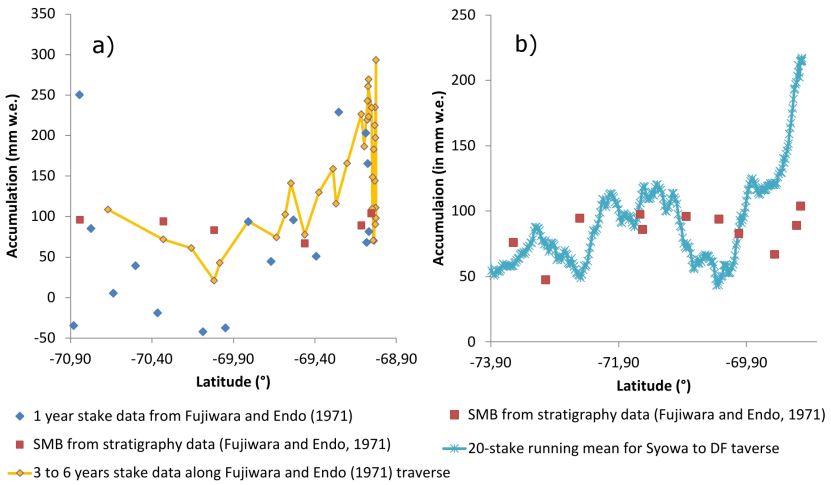

Fig. 1.

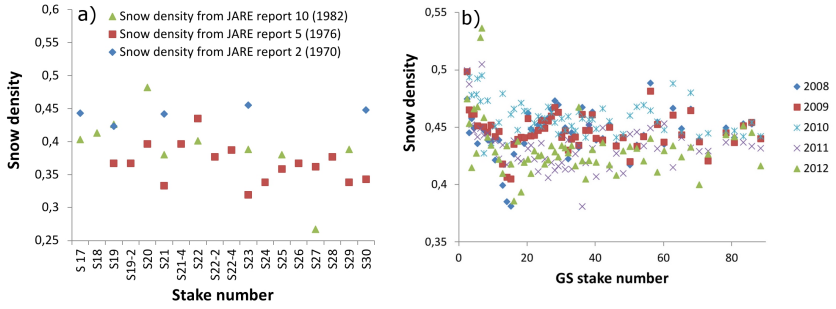
a)
Snow density from JARE report 10 (1982)
Snow density from JARE report 5 (1976)
Snow density from JARE report 2 (1970)
Snow density
0,6
0,55
0,5
0,45
0,4
0,35
0,3
0,25
S 17
S18
S19
S19-2
S20
S21
S21-4
S22
S22-2
S22-4
S23
S24
S25
S26
S27
S28
S29
S30
Stake number
b)
Snow density
0,55
0,5
0,45
0,4
0,35
0
20
40
60
80
GS stake number
2008
2009
2010
2011
2012

Fig. 2.