The Cryosphere

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Discussions

# Interactive comment on "On the interest of positive degree day models for mass balance modeling in the inner tropics" *by* L. Maisincho et al.

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Received and published: 16 July 2014

Please find enclosed in the supplement section the pdf version of this letter in sake of clarity.

We sincerely thank Reviewer 2 for his/her interesting and constructive review. The main remark concerns the surface energy balance computation due to our assumption made on the sub-surface processes, and on the resulting uncertainties in the melting box measurements. Here we show that our assumption is physically based and has no impact on the computed melting amounts. We also show that melting of the ice located at depths below 20 cm under the surface is totally negligible suggesting that melting

boxes allow getting the total surface and sub-surface melting amount. However, we agree that the uncertainty related to the melting boxes measurements should be more accurately presented. The Reviewer presents several constructive comments on the paper structure, and we agree in changing our paper organization accordingly.

In this letter, we address a response to each of his (her) comments (between quotation marks) hereafter.

"This paper presents an interesting reworking of data collected on the Antizana 15 Glacier in Ecuador. Data originally used for surface energy balance (EB) modeling has been combined with additional meteorological data to assess the use of PDD modeling on a tropical glacier. The authors argue for the necessity of simplified models to calculate melt over longer timescales than afforded by EB models due to differences in meteorological data requirements. Whilst the subject deserves discussion, the paper needs serious reworking to become a key resource in conversations concerning model choice, and future approaches. I would recommend restructuring of certain sections of the paper, and strengthening of rationale and methods before the paper is accepted for publication."

As proposed to Reviewer 1, we will give significance of every statistics to demonstrate that the method is statistically robust and support our results. We will include a more meaningful description and interpretation of the model uncertainty to warn model users on the potential uncertainty of the model. Finally, we will describe more accurately the SEB modeling as requested, which is the main method that the reviewer is commenting. However, in this response, we will demonstrate that our SEB is fully robust and that there is no need to assess sub-surface conduction (and solar radiation transmission). (See hereafter).

"General comments: - I would recommend reworking of the energy balance data. On page 2645 (model description), it states that 'conduction into the ice/snow ... was ignored', which I found surprising given that several recent papers from the tropics have

argued for the inclu-sion of subsurface processes, and additionally the incorporation of penetrating short-wave radiation (for example Mölg et al. (2008, 2009)). Additionally, the second author of the present paper led a publication using a more advanced model on blue ice in Antarctica (Favier et al., 2011), and so it should not be too onerous to remodel the data using an updated approach. Whilst I appreciate that the present paper is comparing with already published results, it is difficult to take the results as correct when there are clear issues with the approach."

This comment is interesting but does not stand in our case because surface and sub-surface melt are observed almost every day at 4900 m asl (the glacier is temperate and there is no seasonality in ablation). As a consequence, surface and subsurface temperatures are reaching 0°C at least once a day. This means that over one day, the heat storage below the surface is zero. Thus, at a daily time scale, the positive energy amount available at the surface (i.e. R + LE + H, where R is net radiation, and LE and H is turbulent latent and sensible heat fluxes) must be used to melt the ice at the surface or below surface, otherwise this would mean that an energy source (or sink) exists below the surface, which is not possible. Whenever heat conduction (G) and solar radiation penetrates into the ice (hereafter referred to as QPS), the above observation means that subsurface melt plus surface melt must be exactly R + LE + H. In other words, G + QPS induce a reduction of the surface melt (because less energy is available at the surface) but this amount is exactly compensated by a subsurface melt which is in turn due to G + QPS. This is a key point to get energy conservation.

In our approach, melting results from R+LE+H. Melting begins when the negative energy amounts accumulated during the night are exactly compensated by the positive amounts accumulated during the day. This does not occur exactly when surface temperature reaches 0°C, because melt may occur at the surface while subsurface is still frozen. As a consequence, our approach induces a small shift in melting initiation, but our total daily melting amount is correct. Since we are using daily melting amounts, Favier et al. (2004) assumptions have no impact here.

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Assessing the correct amount of sub-surface conduction and solar energy has thus 2 main interests: 1. To estimate the proportion between surface and sub-surface melting, in order to assess where melting occurs; 2. To correctly compute the surface temperature when this variable is not directly measured in the field. Indeed, the surface temperature is a key variable of the energy balance since it is necessary to assess the turbulent heat fluxes and the longwave radiation emission by the surface.

In our study, point 2 is not a crucial point for us because the surface temperature was measured with a CG3 pyrgeometer. One may say that CG3 presents an important uncertainty and that the surface temperature, which is computed with Stefan Boltzmann equation and longwave radiation emission from the surface are quite inaccurate, present a large uncertainty leading to differences with the actual observed melting. This method was validated with ablation stakes in Favier et al. (2004). Nevertheless, to demonstrate that CG3 uncertainty does not yields erroneous results, as suggested by reviewer 2, we compared results Favier et al. (2004) with the heat budget obtained with Favier et al (2011) model (Supplementary Figure 1), assuming that surface temperature is modeled. This allows accounting for the error induced by the measurement uncertainty caused by the CG3 sensor. In this model, the subsurface conduction and solar radiation transmission are considered. This second modeling is expected to be less accurate, because surface temperature has very large impact on the final surface heat budget and a high quality modeling of surface temperature is required. Nevertheless, Supplementary Figure 2 shows that the differences between modeled and measured surface temperature are low. Moreover, there are low differences between the modeled surface elevation changes by the two approaches (i.e. from Favier et al. (2004) and from Favier et al., 2011). This means that computation from Favier et al. (2004), which assumes that G is zero, has no impact on the final computed melting amounts.

The remark from reviewer 2 is however interesting due to point 1 listed before. Indeed, the melting boxes are filled with 20 cm of ice or snow and then represent a limited

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ice or snow layer in terms of depth (20 cm). As a consequence, the melting boxes measurements may underestimate the total melting if the melting at depth below 20 cm under the surface was very significant. To verify this point we applied Favier et al. (2011) model, and analyzed the amount of melting occurring at depth below 20 cm under the surface. On the 2002-2003 studied period, the total melting amount was 5,975 mm w.e, while melting occurring under 20 cm depth in the ice/snow was 116 mm w.e., that is 1.6 %. This term is thus totally negligible here.

Finally we agree that Mölg's model (Mölg et al., 2008, 2009) is really interesting and we would be keen to apply this model on Antizana Glacier to compare whether it gives better results than Favier et al. (2011) approach. In Mölg's model, the inclusion of subsurface radiation and conduction gives information to interpret processes when re-freezing processes may occur over several days as observed in Peru. Application of this model by Gurgiser et al. (2013) on Shallap glacier shows interesting results and demonstrates that this approach is relevant. However, a thorough analysis of the surface energy balance presented in this paper (see Gurgiser et al. (2013, Table 2)) show that this model still presents several problems to compute the sub-surface heat fluxes. Indeed, Andrew MacDougall already partly pointed out this problem in his review of Gurgiser et al. (2013) in The Cryosphere Discussion. Indeed, he asked: "Page 4031 line 24: How can the ground heat flux always be negative? Unless the glacier is warming up (non-temperate), however, it is stated that the glacier is assumed to be at freezing point below your 14-layer ice heat flow model component?". We agree with this observation. Here, we reproduced Table 2 of Gurgiser et al (2013) (see Supplementary Table 1 of this response). In this table SWnet is the net shortwave radiation budget, LWnet is the net longwave radiation budget, QS is the turbulent sensible heat flux, QL is the turbulent latent heat flux, QG is the heat flux into the glacier body, and QPS is the fraction of net shortwave radiation that penetrates into ice or snow, and QC (or G) the conductive heat flux at the glacier surface. Andrew MacDougall's remark suggests that SWnet + LWnet +QS + QL should be equal to the total melting –(QM + QMS) (where QM is surface melting and QMS is subsurface melting). However, we

note that there is a 10% difference between the surface heat budget, and the total melting (see supplementary Table 1, please compare 57 W m-2 and -52.W m-2). As a consequence, this may suggest that the subsurface modeling is not yet totally validated in Mölg's model and that interpretations on the potential impact of subsurface melting in our results based only on this approach would be rather speculative.

"- I would recommend inclusion of a short section detailing the justification of the use of the PDD method, including demonstrating the potential limitations of ignoring potentially important processes."

This remark makes sense and we propose to include a longer discussion of the relationships that exists between the energy fluxes (actually, the incoming shortwave radiation SWi and albedo) and Temperature to as a justification of the potential use of the PDD. Moreover, we will include a paragraph on the potential limitation of using a model that ignores sublimation.

"For example, in Section 4.1. the authors describe a series of results that indicate that melting is not dependent on air temperature alone, but also on heating of subsurface layers, potentially shortwave radiation penetration among oth-ers."

This comment is not totally clear to us. Does the reviewer mean that the negative mean daily temperature threshold suggests that subsurface melting has to be accounted for in our computations? If this is the case, our answer is no because refreezing at night is not sufficient to lead to conditions where surface melting occurs while the subsurface is still frozen during the entire day. Indeed, what we suggested with this threshold is that the surface and the subsurface temperature may be 0°C at least during several hours during the day (and that melting occurs) even if a negative mean daily temperature at 2m is measured. Indeed, the contribution of SWi is so high during the day that it largely compensates for negative H caused by a mean negative temperature gradient. Moreover, a negative daily mean temperature does not mean that air temperature is not positive during the day, around noon for instance. This second point explains

why cumulative half-hourly positive temperatures are a better variable to characterize diurnal melting than daily mean temperature.

"It would be worthwhile to identify the potential contributions from such fluxes, and to indicate to what extent preexisting conditions have an impact on melt rates, some-thing that was not addressed in the original Favier et al. (2004) paper."

The contribution of each flux to melting is addressed in Table 3 of Favier et al. (2004a) and in Table 2 of Favier et al. (2004b). These two papers are largely presenting this point (see for instance section 5 in Favier et al., 2004, with 4 full journal pages on this point only)

"Additionally, in the introduction section the authors make contradictory statements about what require-ments must be satisfied in order to use a PDD model, and so I would recommend that the authors make a clearer statement about necessary physical conditions (especially page 2640)."

Actually the only requirement is that heat fluxes should be related to temperature. Other remarks on the annual cycle mainly justify why the model correctly reproduces the seasonal cycle of melting in the mid-latitude and high-latitude. Our study suggests that this second part is effectively not a sine qua non requirement. We propose to clarify this part of the introduction.

"- The method section should be restructured. Basic data should be presented before model description."

We propose to change the organization of the paper according to this remark.

"Additionally, it would be helpful to include comments about any post-processing of meteorological data (for example, how was vapour pressure calculated from relative humidity, did the sensors suffer from riming? etc)." When the reviewer writes about vapour, we understand he/she refers to the surface heat budget computation. These data are fully presented in Favier et al. (2004). However, this remark makes sense

concerning the temperature and precipitation data, and we propose to include several sentences on the data quality and on the post-processing.

"Whilst I appreciate that the data has been presented before, and is about to be pre-sented in another paper, the underlying methods and basic data descriptions should be available to readers without having to read a whole body of work to understand (and verify) the datasets used."

Except the data published by Favier et al. (2004, 2008) all the data presented in this paper are unpublished. Nevertheless, a full section is already presenting the data. We will clarify this section and write exactly where each data is used in the study.

" Also, the following points should be noted: i) Stations should be renamed when moved as their data is not continuous or compa-rable (3.3.1-3)."

This only concerns AWSG2 which was installed on Glacier 15 in 2002-2008 but on glacier 12 in 2006. This station was however always located at 4900 m asl except during 9 month in 2003. During the latter period the station was installed at 5000 m asl on Glacier 15. As a consequence, there are 3 situations for this same station. We will thus separate the 3 cases as suggested and write AWSG2 (on Glacier 15 at 4900 m asl), AWSG3 (on Glacier 15 at 5000 m asl) and AWSG4 (on Glacier 12 at 4900 m asl) in the new Table. Elevation and location will be clearly specified in the table.

"ii) Were all data collected at 5000 m asl elevation corrected? (3.3.1-3)"

You mean corrected from elevation? Yes, this concerns AWSG3 in 2003, this was cor-rected using comparisons with AWSG1. Moreover, corrections were necessary when the station AWSG4 was on glacier 12 in 2006: the data where corrected according to comparisons made with AWSM and to the relationship between AWSM and AWSG2.

"iii) Were other time steps considered to be able to use AWS datasets (eg monthly) instead of reanalysis data? (3.3.1-7)"

Francou et al., (2004) already presented the good relationship that exists between

surface ablation in the ablation zone and monthly reanalyzed temperature at 500 hPa on the grid cell that includes Antizana volcano. We did not directly apply our approach with daily means obtained in the field, but we believe such an application is beyond the scope of our analysis. Indeed, in this paper, we wanted to assess whether a model at daily time might be applied or not. As described in our response to reviewer 1, we also applied Blard et al. (2007) model which is based on a an approach at a monthly time step and it allowed getting good correspondence with the observed glacier retreat chronology in Ecuador, meaning that a monthly time step is adapted.

"iv) Did NCEP1 or AWS data have a better fit?"

AWS data have a better fit than NCEP1 (see Figure 9).

"(3.3.1-8) v) Most glaciological data depends on an unsubmitted paper which makes it difficult to evaluate the validity of the results (largely because this paper has not been through a review process). This is significant as the current paper depends heavily on the unsubmitted paper as a reference. Additionally, it would be instructive to include comments about validation or error checking of data, as well as explaining what is meant by 'significant' differences between measurement approaches. (3.3.2-all) - Whilst most of the results and discussion section outlines most of the important outcomes of this paper, I would recommend adding a subsection at the start of Section 4."

The paper by Basantes Serrano et al. is currently under review in Journal of Glaciology. As described in our letter to reviewer 1, this paper presents two main points: an update of the mass balance time series of Antizana Glacier 15 (one of the longest of the entire tropical region), and an adjustment of the annual mass balance time series computed using the glaciological method with the use the geodetic method. The point mass balance data measured on Antizana Glacier 15, even on shorter periods, have been presented in former studies that allow any reader of the paper to know all the necessary information about the measurements and their quality. You can refer to Francou et al.

(AMBIO, 2000), Francou et al. (JGR, 2004), Favier et al. (JGR, 2004), Rabatel et al. (TC, 2013). In addition, the Antizana Glacier 15 belongs to the GLACIOCLIM network, all the data are freely available on the GLACIOCLIM website (http://www-lgge.ujf-grenoble.fr/ServiceObs/), allowing any reader to get a full knowledge of the used method. This glacier also belongs to the WGMS, and mass balance data are also available through the WGMS website. As a consequence, if we can present a new version of the manuscript, and if the paper by Basantes Serrano et al. is still under review, we will change this reference by former published studies to allow the reader to access available literature.

"The added section should outline meteorological conditions and data used, so that the reader can better comprehend what is meant by 'Period 1' and 'Period 2', in order to better understand what 'windy' means etc. Whilst this data has been previously published, it would aid understanding of the sections that follow."

This has already been described in Favier et al. (2004a&b) and Francou et al. (2004). However, we will present mean wind speed values during the 2 periods.

"This section should lead into a concrete statement of why the PDD method is suitable. As it stands, the justification of model choice is weak and hard to follow."

We agree with this remark and we propose to include several sentences on this point.

"This would lead into the current section 4.1 well, so the reader never thinks to question the approach. In section 4.1 I would recommend the following, so as to clarify sources of confusion: i) What measurements are the albedo thresholds based on? At the station, or over the lysimeter boxes? What is the error? (2651-22)"

This threshold was not obtained from albedo measurements. In the field we distinguished dirty and clean ice according to the presence or not of dust at the surface of the ice filling the melting boxes. This is quite subjective, but we observed afterward in Figure 1 that this separation was not stupid since this classification was leading

to separate values according to a threshold of 0.35 approximately. However, several points present albedo values that are very close to this threshold (see Figure 1 of the submitted paper). As a consequence, this threshold is quite subjective, and this may explain why we obtained a different threshold when we tried to calibrate the "best albedo threshold" in section 4.2.

"ii) How was sublimation from the boxes considered? (2651-22)"

There is no need to consider sublimation because we do not measure the mass of the snow/ice collected in the box at two different time steps. Melting boxes are constituted by two recipients. One recipient with a mesh at its bottom is included in the second recipient (Supplementary Figure 3). Thus as we directly measure the water volume of the melted snow/ice that flew down below in the second box, we only consider melt and not ablation. Nevertheless, please note that sublimation was measured during the same field campaign with "poor man's lysimeters" that consist of translucent plastic boxes filled with snow or ice and inserted so as to reproduce the surface condition of the glacier as faithfully as possible. The weight of these boxes was measured at regular intervals (see Favier et al. (2004a) for details).

"iii) The poor correlation of mean daily temperature and measured melting rates suggest there are other important processes acting on the boxes. How were other external factors taken into account (i.e. what is the error?). Did you think to investigate radiative penetration or subsurface heating issues? (2652-5/6)"

We already discussed on this point previously in our response. Radiative penetration has negligible impact on melting amounts errors.

However, as discussed in our response to Mauri Pelto, the way the melting boxes were filled may have an impact on the final measurements. Indeed, for ice, we tried to insert the big ice block and next to fill the holes with smaller ones until getting a similar surface than in the surroundings. We kept attention in keeping dust on the ice. Of course it has an impact on results, but please note that the surrounding surfaces are

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almost always impacted by cryoconite holes, and roughness is though quite important. Nevertheless, comparison between melting amounts from melt boxes and from surface energy balance data (see Supplementary Figure 4, which is an update of Figure 3c, in which the 1/1 line was included) suggest that this effect is reduced, since measured melting is generally lower than the modeled one, likely because initial liquid water is retained by/between the small ice blocks due to capillarity. As a consequence, several processes induce uncertainties in melting boxes measurements. However, getting a strict uncertainty range due to these effects is quite difficult to give. Difference with the surface heat budget are around 5 to 10 mm w.e. d-1, but these differences are not due to the melting boxes uncertainty only, since the albedo is not measured directly above them. A 10% uncertainty is however possible, and may explain part of the poor correlation between daily melt and daily mean temperature, as suggested by the reviewer.

"iv) What are the error ranges on the DDFs? (2652-29)"

DDFs are deduced from the slope of regression line of Figure 3b. Statistics performed on these slopes suggest that DDFs uncertainties are : ±2.1 mm w.e. d-1 °C-1 for snow, ±1.6 mm w.e. d-1 °C-1 for clean ice ±4 mm w.e. d-1 °C-1 for dirty ice

These amounts justify the minimum and maximum ranges that we used on DDFs for snow and ice. Concerning the sensitivity test for ice, we also considered that DDF may change according to the presence or not of dust at the glacier surface, inducing that we tested a largely more important range of values (see Table 5 in the submitted paper).

"v) Fig 4: No relationship appears to be significant. What are the p-values associated with the regression values?"

We already gave a response on this point to reviewer 1, and relationships are significant. The DDFs were computed with coefficient from Figure 3b, which are all systematically significant at least at p<0.05 even for snow, which is the less significant correlation coefficient of Figure 3b. Moreover, except for the mentioned coefficients of

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Figure 3 and those of Figure 4, all our R are systematically given with the number of points of the studied sample allowing any author to compute p-values. In the paper, except Figure 3, when we say that correlations are significant it is at $p=0.001$, but for a sake of clarity we will replace the number of point by the degree of significance. However, we propose to clearly write p values in the new version if the Editor allows us to submit a new version of this paper.

"Comments related to Section 4.2 (which follow on from comments above): i) Please include a figure or a statement related to melt amounts, also sublimation values obtained from lysimeters and energy balance modeling (2654-2/6)"

We will include a sentence on melting boxes uncertainty. Concerning sublimation, as stated before it does not impact melting boxes measurements. However, sublimation is neglected in the model and this assumption has an impact on the final modeled mass balance. As requested by Mauri Pelto, we will include one sentence on the resulting uncertainty.

"ii) A clean to dirty ice threshold of 0.45 seems very high. How much sediment or water etc is necessary to define a 'dirty' ice surface? What measurements/observations are these values based on? (2655-1)"

We already answered this point: Clean or dirty ice clearly depend on the amount and on the origin of dusts accumulated on the ice, but without getting any composition of the mineral and concentration of dusts, any comparison with other sites would be purely speculative. This explains why we did not perform any description of this parameter in its physical context. However, albedo never goes below 0.25 on Antizana glacier and is generally around 0.35 when the ice is covered by cryoconite holes. The lowest values where only reached in 2002-2003, mainly after the eruption of Reventador volcano. We believe that the frequent snow precipitation may induce a quite clean ice leading to a quite high threshold. However, we propose adding a discussion of this parameter with other values available in the literature.

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"iii) Should emphasize that the paper is only interested in calculating melt and not total ablation. (2655-17)"

We propose to add this sentence. However, we also propose to include an estimate of the uncertainty resulting from the assumption made on sublimation.

"Comments on Section 4.3 - 5: i) Sublimation percentage on page 2656 line 18 is useful, but would be helpful earlier in the paper."

We propose to include it earlier in the text.

"ii) The PDD sensitivity section should be expanded, and appear at the start of the results section. This analysis can be used to help justify the approach and choice of parameters."

As written in our response to Reviewer 1, we propose to present an error estimate based on the sensitivity test and on comparison with field data. Here, we propose to include the resulting value at the beginning of the results section.

"- Finally, I would like to commend the authors on a well written discussion section. The only downfall is that by the time the reader reaches the discussion, they have serious doubts about the model, and some measurements (many of which are clarified within the discussion sections, but should have been described sooner). I would encourage the authors to rewrite the method and results sections to the same level of clarity as the discussion section."

We will follow this suggestion and organize the method, results and discussion accordingly.

"Other comments: As I envisage that the text will require serious reworking in order to be accepted, I have focused on specific comments that should be implemented in the revised version of this paper."

Please see above.

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"- Title: I would suggest changing 'interest' to 'use' - Standardize the glacier name. It changes from Antizana Glacier to Antizana 15 glacier to Antizana 15 Glacier, etc, which is difficult for the reader to follow"

We agree and we will include it.

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Gurgiser, W., B. Marzeion, L. Nicholson, G. Kaser, and M. Ortner: Modeling en-

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âĂČ Supplementary Table and Figure captions

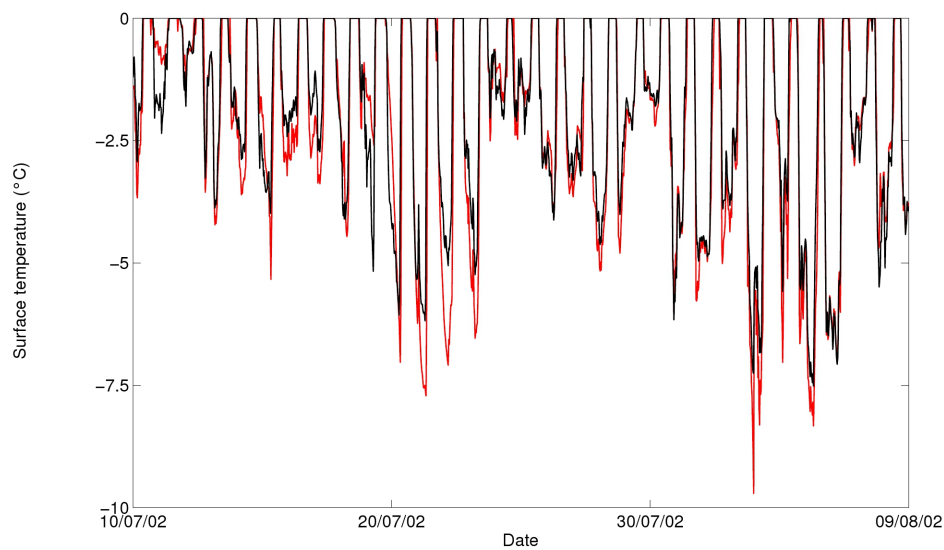Table 1: Comparison between surface heat variables and melting given in Table 2 from Gurgiser et al., (2013).

Supplementary Figure 1: Modeled surface level according to Favier et al. (2004) approach (thick red line), according to Favier et al. (2011) approach (thick grey line) and PDD approach (thin grey). Thin black line represents the ice level modeled by with the PDD approach. Grey circles are the measured surface elevation from stakes, and error bars are ±150mm.

Supplementary Figure 2: Modeled surface temperature with Favier et al. (2011) approach (red) compared with measured surface temperature (black). Mean and standard deviation of differences between modeled and measure surface temperatures over the studied 2002-2003 period are 0.11°C and 1.06°C respectively.

Supplementary Figure 3: melting box scheme

Supplementary Figure 4: Comparison between measured melting and modeled melting with the surface energy balance

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/8/C1310/2014/tcd-8-C1310-2014-supplement.pdf

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|                        | Data from Gurgiser et al (2013), YEAR 1 |
| ---------------------- | :-------: |
| SWnet (or S)           | 76        |
| LWnet (or L)           | -25       |
| QS (or H)              | 10        |
| QH (or LE)             | -4        |
| SWnet+LWnet+QS+QL      | 57        |
| QG (i.e. QC + QSP)     | -10       |
| SWnet+LWnet+QS+QL+QG   | 47        |
| QM                     | -45       |
| QMS                    | -7        |
| QMS+QM                 | -52       |

**Fig. 1.** Table 1: Comparison between surface heat variables and melting given in Table 2 from Gurgiser et al., (2013).

**Fig. 2.** Supplementary Figure 1: Modeled surface level according to Favier et al. (2004) approach (thick red line), according to Favier et al. (2011) approach (thick grey line) and PDD approach (thin grey). Th
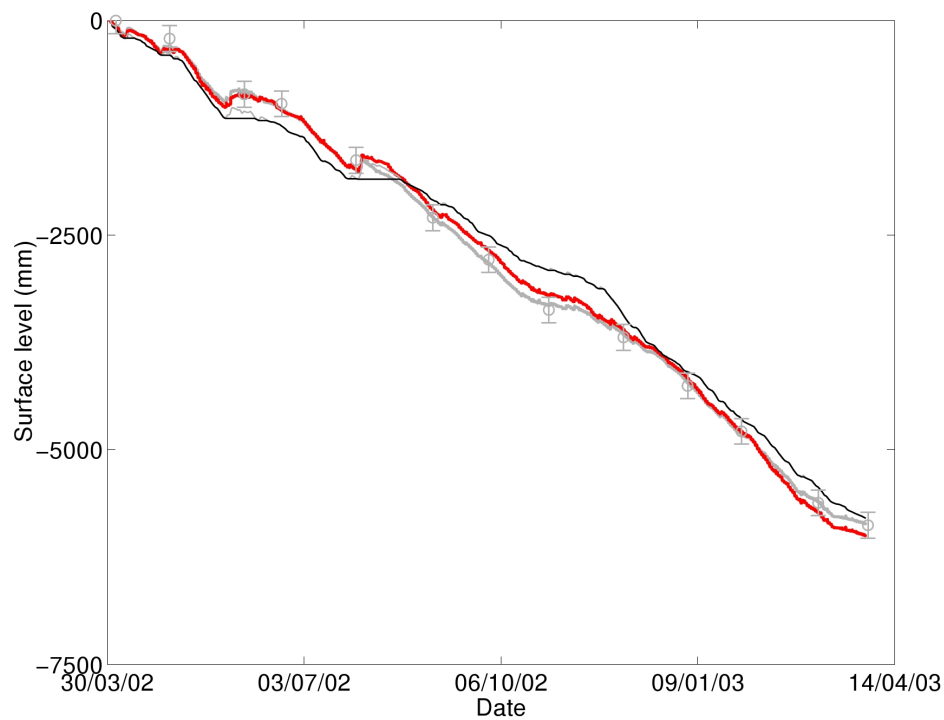
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**Fig. 3.** Supplementary Figure 2: Modeled surface temperature with Favier et al. (2011) approach (red) compared with measured surface temperature (black). Mean and standard deviation of differences between mode

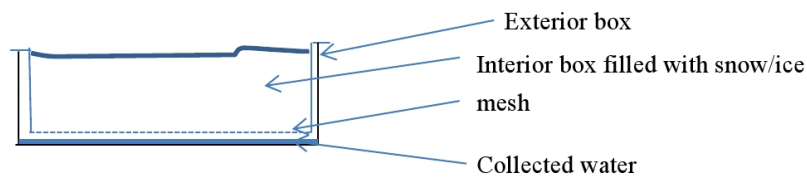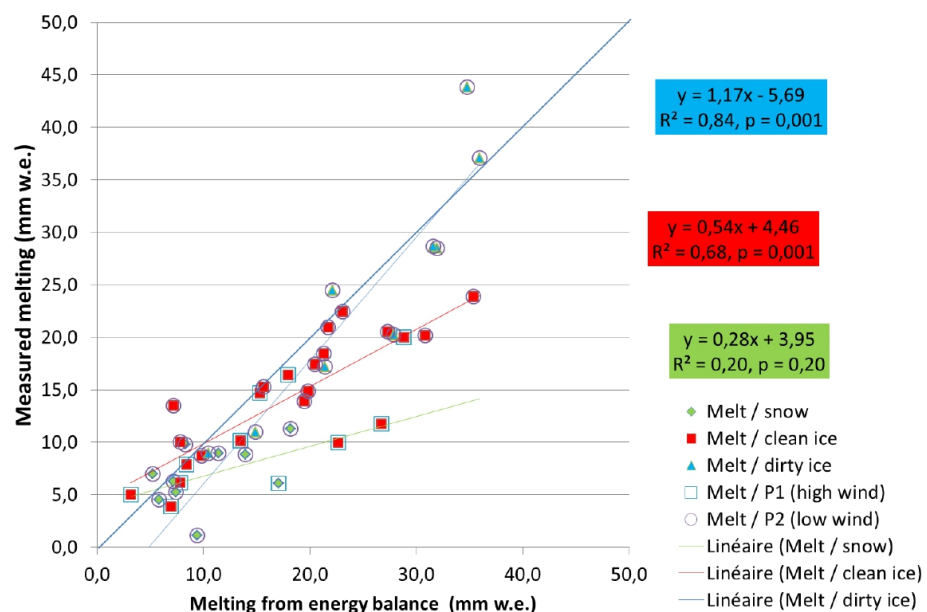**Fig. 4.** Supplementary Figure 3: melting box scheme

**Fig. 5.** Supplementary Figure 4: Comparison between measured melting and modeled melting with the surface energy balance