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The Cryosphere Discussions

Interactive comment on "Diagnosing the extreme surface melt event over southwestern Greenland in 2007" *by* M. Tedesco et al.

M. Tedesco et al.

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We very much welcome the approach by Tedesco et al. for putting a complex and process orientated view on the recent mass balance developments on the surface of the Greenland ice sheet. The authors clearly go beyond the simple approach of correlating the mass balance with air temperature and precipitation only and look into the driving energy and mass fluxes instead.

The paper analyses different drivers for the seasonal melting on the Greenland Ice Sheet surface. Both surface and atmospheric information stem from remote sensing and modelling approaches and no link to measured data at and near the ground is provided. We identify this as a crucial gap in the presented study.

The exceptional surface melt in 2007 has been confirmed with in situ observations

made along the K-Transect by the team of M. van den Broeke.

Van den Broeke, M., Smeets, P., Ettema, J., Van der Veen, C., Van de Wal, R., and Oerlemans, J.: Partitioning of melt energy and meltwater fluxes in the ablation zone of the west Greenland ice sheet, The Cryosphere Discuss., 2, 711-736, 2008.

Reply: We cite this new paper in the first paragraph of the revised manuscript

Although their approach is slightly different and the target is the calculation of mass balance, e.g. Box et al. (2004) also provide information on fluxes contributing to the mass balance, based on measurements and regional scale climate modelling (MM5).

It is necessary to refer to these findings and to respectively discuss the new results. Also energy and mass balance studies carried out by several authors at individual points or over limited sections of the Greenland Ice Sheet need to be implemented or discussed.

Reply: MM5 does not have as complex a snow/energy balance model as the MAR. In particular, in MM5 (Box et al., 2004), the Greenland ice-sheet surface is represented by a diffusive multi-layer surface model with ﬁxed surface properties and the use of a ﬁxed surface albedo (0.8). This can lead to large errors in the simulated net radiation budget over melting surfaces during the summer. It is of course important to give credit to past studies. At the end of Section 1, we now mention how our MAR applications complement a series of recent studies of the mass balance and climate of Greenland using regional climate models, citing Box et al. (2004) and Lefebvre et al. (2003) as examples.

Added References:

Box, J., Bromwich, D.H. and Bai, L.-S: Greenland ice sheet surface mass balance 1991–2000: Application of Polar MM5 mesoscale model and in situ data. Journal of Geophysical Research, 109(D16105), 2004.

Lefebre, F., Fettweis, X.,Gallée, H., van Ypersele, J., Marbaix,P., Greuell,W. and.

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Calanca, P.: Evaluation of a high-resolution regional climate simulation over Greenland, Climate Dynamics, 25, 1, 07/2005, Pages 99-116, DOI 10.1007/s00382-005-0005-8, 2003.

Introduction: p. 385, l. 4-5: ’Variations in the extent and duration of summer melt over Greenland reflect a suite of processes intimately tied to air temperature and the surface energy balance.’ The sentence is not appropriate. The only process of concern is melting and this is exclusively the result of the respective energy balance. To allow for a better evaluation of the rather interpretative findings in this paper, a quantitative discussion about the uncertainties of the calculated results is needed. Moreover, the melt indices should be quantitatively assigned to different energy fluxes by statistical measures, e.g. in terms of the coefficient of determination.

Reply: We rephrase the sentence to remove reference to temperature. We can not add uncertainties in the MAR results because they are the outputs of a surface energy balance model. However, in response to reviewer 1, we have altered Figure 2 to show regions where flux anomalies are statistically significant and also discuss significance in the text.

An outline of the main features of the regional climate model MAR as well as the mass balance model used should be given. How is the mass balance modelled from MAR output data?

Reply: MAR is discussed at length in the paper by Fettweis (2007).

In contrast to the definition of MAR on p 385, stating that MAR is a regional climate model, on Page 391, line 19, MAR is used as a synonym for a mass balance model.

Reply: The MAR is a regional climate model coupled with a complex snow energy balance surface model, allowing for simulation of the surface mass balance. There is no need for a separate acronym..

Additionally, it would be helpful to explain the parameterization of e.g. LW_IN, surface

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albedo and other parameters (p. 388).

Reply The surface albedo and other parameterizations used in MAR are described in Lefebvre et al. (2003) which is now cited in the revised text.

Lefebre, F., H. Gallée, J. van Ypersele, and W. Greuell (2003), Modeling of snow and ice melt at ETH Camp (West Greenland): A study of surface albedo, J. Geophys. Res., 108(D8), 4231, doi:10.1029/2001JD001160. .

The authors clearly go beyond the simple approach of correlating the mass balance with air temperature and precipitation only and look into the driving energy and mass fluxes instead.

P. 390, l. 2: The 1000-500 hPa thickness provides information only about the lower troposphere while ’tropospheric warmth’ refers to the entire troposphere.

Reply: We changed the text to "lower tropospheric warmth.

P. 391, l. 17-18: ’While melting at high elevations is not contributing to this process, it leads to a reduction in ice sheet albedo.’ Which processes are meant?

Reply: The sentence has been re-written as follows: ‘While melting at high elevations is not directly contributing to runoff and sea level rise, it leads to a reduction in ice sheet albedo.

’Southerly airflow’ is mentioned in the abstract and the conclusion, but it is not addressed in the main body of the paper. Consequently, the statement remains unclear and its meaning is not shown to the reader.

Reply: We added the reference of Hanna et al. (2009) and a new figure (Figure 5, see below) showing 500 hPa wind and temperature anomalies for the summer of 2007.

E. Hanna, J. Cappelen, X. Fettweis, P. Huybrechts, A. Luckman and M. H. Ribergaard: Hydrologic response of the Greenland ice sheet: the role of oceanographic warming, Hydrol. Process., in press, 2009.

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Surface albedo: Generally, ’surface albedo’ determines the albedo at the earth’s surface. Two statements in the article are irritating if we interpret the term this way, namely p. 385, l. 10-12:’Surface albedo is key, through both influencing solar radiation absorbed by the surface and by modulating cloud radiative forcing.’ and p. 389, l. 2-3: ’...dominant effects of reductions in surface albedo over cloud cover...’ We are questioning how the surface albedo can ’modulate the cloud radiative forcing’, and what is meant by ’surface albedo over cloud cover". The two statements would, however, make sense if we consider the albedo at the top of the atmosphere (TOA) instead.

Reply: We honestly do not understand what the problem is. Variations in surface albedo are widely known to play a role in cloud radiation forcing at the surface, such as through the difference between surface and cloud top albedo and through promoting multiple scattering between the surface and cloud base. Regarding "dominant effects of surface albedo over cloud cover", perhaps our wording was confusing. We should have said "compared to cloud cover". The text has been altered accordingly.

Constructive metamorphism: P. 385, l. 10-12: ’Albedo can be highly variable, and tends to decrease through the melt season as snow grain sizes increase as a consequence of constructive metamorphism.’ Maybe ’melt metamorphism’ is meant here. In case both are meant, we propose to use ’metamorphism’ only.

Reply The term ‘constructive’ metamorphism generally refers to the situation where grain size increases. This separates it from ‘destructive’ metamorphism,which refers to the case where grain size decreases. No changes in the text are necessary. .

P. 390, l. 2: Instead of the term ’pressure layer’ either ’pressure level’ or ’air layer’, depending on what is meant, should be used instead.

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Reply: The more accurate term is actually pressure interval. The text has been altered accordingly

Fig. 2: Labels in the second row: we assume ’anomaly’ is missing.

Reply: This has been corrected.

Fig. 3 and 4:

Datum strings are not uniform. Displaying both the absolute values and anomalies within the same graph does not introduce any extra information and is therefore unnecessary here. Fig. 3b: Right axis: ’surface air temperature’ instead of ’surface temperature’. Text font is to small

Reply: we changed he font and datum string

Interactive comment on The Cryosphere Discuss., 2, 383, 2008.