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Interactive comment on "Spatial debris-cover effect on the maritime glaciers of Mount Gongga, south-eastern Tibetan Plateau" *by* Y. Zhang et al.

Anonymous Referee #1

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Review of Zhang et al: 'Spatial debris-cover effect on maritime glaciers on Mount Gongga, south-eastern Tibetan Plateau'

The authors present a modeling approach to quantify the impact of debris cover on glacial mass balances in the eastern Tibetan Plateau at Mount Gongga. This study relies on ASTER-derived thermal properties to identify debris coverage and thickness and other meteorological data to drive a mass-balance modeling approach. The authors argue that debris coverage accelerates glacial melting and that there exist no statistical significance in retreat rates between debris covered and debris-free glaciers in this area. This manuscript addresses the important issue of debris cover that is not well understood and requires additional research to provide more accurate glacial mass balance predictions.

The manuscript is generally well written, uses proper English language and grammar. However, the manuscript is on the lengthy side and many long sentences stretch over several lines – these should be shortened or split. For example, the Results are interesting, but too long. It appears that some Discussion has been mixed into the Results. I urge the author to carefully re-read their Results, for example the last paragraph of section 4.2.2.

The general approach of this work is scientifically sound, but it appears that there are shortcomings in the detail. For example, the calculated mass balances are not compared to field measurements (or DEM differences), the meteorological data used as boundary conditions for the glacial-mass balance model are sparse and/or have low spatial resolution. If the goal is to calibrate the impact of debris cover on glacial mass balance, a different study region with denser data may be more appropriate. There is little validation of existing meteorological or glacial data available.

I am little perplexed by the general conclusion (impact of debris coverage) and several statements in the Results and Discussion that highlight that debris-free glaciers are retreating about half as fast as debris-covered glaciers. Can these glaciers be easily compared? Do they have similar AAR, hypsometries, mass balances, etc?

How accurate are the ELA (or snowlines?) form the CGI? The glacial areas are likely to be accurate.

Why are thermal resistance field measurements unrealistic (page 2422, line 1)? They are very valuable for calibration and validation purposes, especially if the field site is chosen carefully and is representative for a satellite/DEM gridded dataset. Some data are shown in Figure 3, where the authors present a validation of ground-surveyed debris thickness with ASTER-derived thermal resistances. A more meaningful comparison would by a ground-surveyed debris thickness (X-axis) vs. ASTER-derived thermal resistances (Y-axis) plot. While the pattern is convincing, the debris thickness measurements have much less variability than the thermal resistance values.

I am uncertain if the NCAR reanalysis data provide the spatial resolution to estimate net radiation at the required scale. In other words, unless some downscaling has been performed, the entire study area is covered by one NCEP/NCAR reanalysis grid cell. Maybe NCAR/NCEP data can be used as boundary conditions for a WRF model to derive sufficiently accurate net radiation measurements – but even that will require ground validation. I emphasize that the authors mention the pixel size of the ASTER TIR data, but do not refer to the spatial resolution of the NCAR data. Generally, when referring to reanalysis data, a more precise citation or reference to the NCAR version is necessary (as well as the used grid-cell size). Along the same lines, I argue that the precipitation gradient can not be interpolated into large heights. There exist several studies analyzing the spatial and elevation-distribution of rainfall that document a rainfall peak at $\sim$4km, but less moisture above. I note that the ground-control stations to derive some of the gradients do not include stations at high altitude (i.e. > 5km). Since there are no mass-balance measurements presented and only modeling results are given, it is crucial to have reliable boundary conditions for the mass-balance model.

One additional note regarding mass balances: The authors use a stereo-airphoto DEM from 1989. This could easily be combined with the 2000 SRTM DEM to give a first-order decadal-scale mass balance. This approach could be used to roughly verify model results.

The mass balance setup (equation 2) is rather simple and the model of accumulation (e.g., snow/rain mixture at threshold temperature) is only moderately applicable, but certainly a first-order approach.

Figures: Figure 4 is interesting, but I find it extremely cumbersome to read with the colorscale. The colorscale should be simplified and should only contain 3-5 colors (max. of 5).

Wording: Conclusion: replace ‘considerably significant’ with significant

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