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Discussions

Interactive comment on "Evaluation of dynamically downscaled near-surface mass and energy fluxes for three mountain glaciers, British Columbia, Canada" *by* Mekdes Ayalew Tessema et al.

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The paper uses the Weather and Research Forecasting Model (WRF) to dynamically downscale weather information from ERA-Interim global reanalysis to investigate potential improvement of the surface mass- and energy balances of three mountain glaciers in BC, Canada.

The set-up of WRF for such a downscaling analysis, the collection of related field data and the analysis of model in comparison to the measurements is a lot of work and a

great effort. This is fully acknowledged but at the same time, the set-up of the study is such that the negative results (no improvement through WRF) are no surprise. The study is heavily outdated in a time when weather models run at 2 km resolution operationally (e.g. Cosmo in Switzerland) and WRF downscaling work is done at the resolution of tens of meters (Gerber et al., 2018) and not kilometers as in the submitted paper. A study that has just been published and uses a 30 m resolution to generate meteorological input for glacier energy and mass balance in the Himalayas is Stigter et al. (2018) and is showing the state of the art in the field and in fact documents that WRF is able to successfully generate useful local weather input.

From our own early work (Lehning et al. 2008; Raderschall et al.,), we know that it needs a very high resolution of below 50 m to approximate wind fields in complex terrain. And if you have correct high-resolution wind fields, you can describe boundary layer processes from snow deposition (Mott and Lehning, 2010) to snow ablation (Mott et al., 2011). I would go as far as to say that the dynamical downscaling of first flow and then full weather has been initiated with these ARPS based studies and the methodology has immediately also been applied to glaciers (Mott et al., 2008; Dadic et al., 2010) including the Mölg and Kaser study (Mölg and Kaser, 2011) mentioned in the paper. Therefore, if you do a downscaling study that will not reproduce the local wind fields correctly (in this case katabatic flows), you cannot expect to see much improvement over a re-analysis on local snow mass balance estimates. Now, if this was the first attempt and all that is currently possible, then this could still be interesting. But if much older and current studies (such as the ones mentioned above) already have pushed the limits way beyond the setup of this study and state of the art downscaling in much higher resolution shows that you can even simulate individual eddies (Gerber et al., 2017) then the results as presented in the paper do not add to our scientific body of knowledge.

Let me please emphasize that I am not trying to reject the paper because our own work and other work as discussed above did not get cited. This may be seen as an omission but is not even necessary or could be fixed easily. The main point is really that the results do not allow to gain new insight and give a wrong impression on the usefulness of dynamical downscaling because the study had the wrong/outdated design despite all the good work that has been done in the execution. I would also fully be in favor of publishing negative results but not if the negative results are the consequence of an inadequate set-up such as in this study. This is unfortunate, as the paper is really well written and nicely illustrated.

In addition to the general comments , I have also one additional major set-up problem, which is the arbitrary switch-off of the cumulus convection scheme, while it is quite clear that convection will be insufficiently resolved at a 2.5 km grid resolution. Again, it then no surprise that precipitation simulations have a large error.

One final major point, which can either be a typo or a serious misconception is the statement on p. 27 l. 10 when the authors talk about adiabatic cooling in the katabatic wind on the glacier. Of course, descending air masses warm by an adiabatic process.

Some detailed comments:

p. 5 l. 6: Roughness lengths should be consistent with model resolution as sub-grid topography needs to be represented by the roughness

p. 5 l.8: Why did you not use a precipitation lapse rate?

p. 9 Eq. 2: I don't know why there is p/p_0 in this equation. This does not come from the original derivation of the bulk formula (see e.g. textbooks by Brutsaert or Stull) and the influence of air pressure is already there via the air density.

p. 19 l.11: There is some physical argument why roughness for momentum could be different from roughness of scalars (but empirical evidence is missing, see e.g. Schlögl et al., 2017). However, assuming two different roughness lengths for moisture and temperature has no theoretical justification

p. 13 l.3: You could diagnose local stability (at least over a melting surface – not

sure you had a surface temperature measurement) and then use an adequate stability correction.

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