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The Cryosphere Discussions

Interactive comment on "Effects of local advection on the spatial sensible heat flux variation on a mountain glacier" *by* Tobias Sauter and Stephan P. Galos

Anonymous Referee #2

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General comments

In this paper the authors make use of high resolution Large-Eddy Simulations (LES), considered as a pseudo-reality testbed, to evaluate the spatial variability of the sensible heat flux over a glaciated area in the Italian Alps. The output of LES has been used for assessing the impact of using the pseudo-observations at few sites placed on the glaciers for extrapolating wind and temperature by means of linear extrapolations. The authors conclude that the magnitudes of the surface (sensible) heat flux errors are strong enough to significantly affect the surface energy balance and derived climate sensitivities of mountain glaciers.

Although rather well written and with clear figures/tables, in my opinion the paper has

several weak points and does not provide sufficient evidence in support of the authors' conclusions.

A still-open key scientific question is highlighted, i.e. how the assumptions generally made for extrapolating meteorological forcing field from sparse point observations impact the estimated local and glacier-wide melting rates. In particular, the focus is on calculation errors of the sensible heat flux distribution. However, the authors quantify this impact only comparing sensible heat flux calculations, whereas it should be assessed in comparison with the overall energy and mass balance (or melt rates). In addition, due to computational restrictions, they only perform calculations for one hour on a clear-sky day in summer 2013. I suggest evaluating the impact of sensible heat flux calculations vs. the surface energy balance, in different meteorological conditions. Moreover, I'm wondering if the mass balance measurements on Langenferner (http://acinn.uibk.ac.at/research/ice-and-climate/projects/langenferner) could be used for estimating the impact on local and glacier-wide melt rates.

The authors claim that 'the pseudo-reality atmosphere is not required to be an observed real world case, but needs to be plausible in the sense that relevant processes are realistically simulated'. It is unclear what is meant with relevant processes. In section 4 the authors say that sections 3.1, 3.2 and 3.3 demonstrate that LES capture these relevant processes, but in these sections there is only a description of model results (some of them are obvious) and complete absence of comparison with real-world observations. In my understanding, the plausibility and realism of LES is only assessed based on the authors' personal knowledge of the atmospheric circulation over mountainous terrain, but I'm not sure that it is sufficient. On the other hand, Figure 1 shows several weather stations in the study area. Why not using these data for checking the realism of calculations? How can it be assessed that LES is superior to the bulk approach, without any comparison with real-world observations?

There is confusion between point-site process understanding and interpolated/extrapolated input meteorological fields from sparse meteorological observations

coming from on-glacier sites. If it's true and obvious that process understanding at individual sites is not sufficient to fully characterise the micrometeorological conditions over glacier surfaces, the practical or operational need to achieve such full characterization remain questionable (and in any case is not quantified in this paper).

Interpolation/extrapolation of meteorological data from on-glacier sites has limited practical usefulness. In operational model applications, there are almost no input data coming from inside the glaciers. In particular, I refer to applications aimed at exploring the climate sensitivity of glaciers, which is mentioned by the authors. Because the climatic sensitivity can be defined as 'the ratio of changes in the 2 m temperature above a glacier to changes in the temperature outside the thermal regime of that glacier (Greuell and Böhm, 1998), there is little usefulness in testing the errors coming from interpolation/extrapolation of pseudo-observed (or better, calculated) wind and temperature coming from points located inside the glaciers. It could be more useful to test calculation schemes recently proposed in the literature (cited by the authors) starting from off-glacier weather stations.

Finally, I would recommend this work for publication in The Cryosphere only after major revision and addressing of the main points reported here and in the following specific comments.

Specific comments

Page 1 line 5 and 7: please add the percentage in under-overestimations, and also the percent error in mass balance calculations. Small-scale heat flux, glacier heat fluxes... please be consistent throughout the paper and try to use always the same wording (i.e. sensible heat flux)

Page 1 line 8 and 9: it is unclear if site selection and flow direction refer to data measurements, extrapolations, or validations

Page 1 line 9-11: this is not adequately quantified in the paper. The magnitude of

sensible heat flux calculation errors and their impact on the surface energy balance and on the derived climate sensitivities should be calculated and several numbers should be added also here in the abstract.

Page 2 line 10: consider replacing 'can make over' with 'can represent'

Page 2 line 13: consider removing 'peculiar'. In this period it is partly unclear to which mass balance studies dealing with small scale variations of melt rates the authors are referring to

Page 2 line 16: what is meant exactly with 'the deficiency of monitoring activities'?

Page 2 line 26: an open scientific question

Page 2 line 27: be fully answered

Page 2 line 28: I suggest to state more clearly the aim(s) of the study

Page 3 line 19-20: SBL, SGS, please define acronyms

Page 4 line 9-10: it has been recognized

Page 4 line 13-14: the negligibility of assumptions should be demonstrated and/or possible errors coming from assumptions should be quantified

Page 4 line 19-22 and page 6 line 14-16: please see the previous comment on assumptions

Page 6 line 23: I suggest adding some references for aerodynamic roughness heights

Page 6 line 25: ERA-Interim reanalysis data (also p5 l21)

Page 6 line 27-28: from which hour to which hour of the day?

Page 7 line 9-12: this part is somewhat unclear and it looks like the authors adjust the DEM (the only real-world component in this work) to the requirements of the numerical model. Is it correct? Please see comment to Page 4 line 13-14

Page 9 line 4: please reword ‘the intensity of the cross-valley circulation’ to improve clarity

Page 9 line 23-24: consider replacing ‘do not jointly appear with high wind velocities’ with something like ‘do not appear in the areas with high wind velocities’

Page 10 line 34: can you quantify (or estimate) the percent contribution of the sensible heat flux to the total energy balance in your case study? This would be important for understanding the impact of calculated sensible heat flux on local-scale and area-averaged energy and mass balance

Page 11 line 10-11: can you provide some numbers in support to this statement?

Section 3.4: in Figure 1 two weather stations are shown on the glaciers. Why data coming from these weather stations were not used for checking the reliability of LES experiments?

Page 11 line 19-23: with the authors, I recognize that this is a strong assumption, in particular over glaciers with such high range of elevation (2595-3750 m), quite different from the end-of-summer situation reported for Arolla by Brock et al., (2000). It should be possible to map the snow cover for the selected day, or to use another day with available snow cover data (e.g. from Landsat imagery). Alternatively, the authors should at least quantify the possible errors stemming from this assumption.

Page 12 line 2-3: on which bases the authors say that the SGS model ‘seems to work well’ in their study?

Page 12 line 4: maybe reword the title as ‘Estimation of the sensible heat using the Bulk-Approach’

Page 12 line 20: replace ‘given that’ with ‘in case’ (I guess that it is meant where there is a weather station measuring the required variables)

Page 12 line 20-27: please consider moving this part in the following section

Page 12 line 26-27: this is a strong statement, because there is complete absence of comparison between modelled and observed (relevant) processes. What are relevant processes? How can the authors assess that LES captures observations, without reporting observations or without citing literature on this topic?

Page 13 line 3: please replace ‘pseudo-observed’ with ‘calculated’. I guess these are temperature and wind speed data calculated using LES, is it right? Please specify

Page 13 line 5: surface heat flux, surface sensible flux, or surface sensible heat flux? Please be consistent

Page 13 line 7-9: please explain why there are differences at the two Za and Z0 sites, given that (in my understanding) wind speed and temperature at these sites are the same using the bulk method and the LES (i.e. they differ in the rest of the analysed area, but not at Za and Z0).

Page 13 line 20: the average sensible heat flux (please, add % error in the text). How big is the impact on glacier-wide total energy balance calculations?

Page 13 line 27: for which wind direction?

Page 13 line 28: using linear extrapolations across the glaciers?

Page 13 line 29: in my opinion there is an equivocal use of the term ‘bulk method’, which is a method for calculating turbulent exchanges, referred to the calculations using linear extrapolations across the glaciers. I would suggest clarify/avoid ambiguities

Page 13 line 33 and in the following: please check or clarify, if gradients are too large (in absolute value) underestimations of temperature and sensible heat flux should occur in the upper parts of the glaciers. Moreover, in absence of model validation, why the LES model has to be the right one and the Bulk has to be the wrong one, a priori?

Page 14 line 3: also in this case I suggest to calculate the relative importance of these errors in the overall energy balance of the glacier

Page 14 line 5-8: this part is methodological and should be moved at the beginning of Sect. 4.2. It also deserves rephrasing to improve clarity

Page 14 line 12: it is unclear why the authors selected only a clear-sky case study

Page 14 line 17-22: I have several points, which could/should be at least partly addressed or discussed in the manuscript. In particular they concern: i) the practical or operational need to fully characterise the micrometeorological conditions over glacier surfaces; ii) the linear extrapolation of forcing fields from sites placed over glaciers (again, almost never available in practical model applications); iii) related to the previous point, the climate sensitivity has to be assessed with respect to climatic conditions observed outside the microclimatic influence of the glaciers.

Page 14 line 20: here and elsewhere, I suggest speaking about differences and not errors, because the comparison is between calculations and not between calculations and observations

Page 14 line 30: percent error of what?

Page 15 line 1-2: when small-scale variations of surface energy balance are required? Please add this in the introduction and recall it here and/or in the abstract

Page 14 line 6: using off-glacier stations for what?

Comments on the figures and tables:

Figure 1: this image lacks east-north coordinates or inset displaying wider geographical setting of the study area. Four weather stations are reported, whose data are not used in this paper

Figure 2 (and following maps): I suggest adding some contour line (or hillshaded DTM, like in Fig. 1), which is needed for a better understanding of the local topography and of its effects on the calculated variables

Figure 9: in the caption just begin with 'differences in the surface....' and correct

pseudo-observations coherently with the text

Table 3: I suggest adding LES estimates and % differences (not error, please correct also in Table 2) as in Table 2.

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