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Interactive comment on "Using 3D turbulence-resolving simulations to understand the impact of surface properties on the energy balance of a debris-covered glacier" *by* Pleun Bonekamp et al.

Anonymous Referee #2

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Overview:
- This is an important study that sheds light on the small scale variations in turbulent heat fluxes across the surface of a debris covered glacier using a high-resolution computational fluid dynamics model applied to the near surface atmosphere. It uses a novel approach to provide valuable insight into the relative importance of key, typically measured meteorological variables and, I believe, will be of great interest to the debris covered glacier scientific community (particularly to distributed modelers).
- The author/s clearly has/ve a solid understanding of the physical processes under-

lying the stated observations in the simulation results. I think this strength could be highlighted more by, in a renamed Results and Discussion section, explaining the physical processes first, then how the results demonstrate them.
- The authors show that variations in melt over the surface of a debris covered glacier is due not only to debris thickness but also to variations in turbulent fluxes. Perhaps this is intuitive, but this study is the first to show it by simulating spatial patterns in wind, humidity, and temperature. This paper shows that turbulent fluxes are important to understanding the development of ice cliffs, which have previously been shown to be ablation hotspots. It importantly shows that surface heterogeneities are an important driver of energy exchange.

General comments:
- The discussion of the study's focus on micro meteorological variables must be honed. Turbulent fluxes are determined by wind speed and surface roughness length, as well as by temperature (sensible) and humidity (latent). In the abstract, the manuscript reads "turbulent fluxes, wind fields, moisture and temperature. . ."; in the conclusion, the manuscript reads "turbulent fluxes, wind fields, surface specific humidity and temperature for a debris-covered glacier is investigated." I suggest reframing the language around the purpose of this study: specifically, not listing (and, thereby, implying) turbulent fluxes as separate from wind, humidity, and temperature.
- This manuscript needs English editing (grammar and punctuation) beyond what can be provided in my review; I made some suggestions, but the manuscript needs major editing for readability. The English hampered my comprehension of the scientific basis of the paper.
- I found the Introduction especially confusing to follow–partly because of wording choices (e.g. however, thus, and nonetheless in a single sentence) and partly because there is insufficient detail on key elements of an introduction but superfluous detail on non-essential inclusions (e.g. methods and wall modeling). The authors do not describe LES and DNS beyond spelling out the acronyms, and the authors do not

discuss the reasoning behind a spatial resolution of ∼1 m. The section needs clearer language to communicate a revised structure of problem/question –> hypothesis –> aims (generally exploring turbulent fluxes) –> objectives (specific, describing methods). I find that the statements of the problem (incomplete understanding of the drivers of heterogeneous melt patterns) and hypothesis are roundabout and unclear. In the last paragraph of the introduction, the aim and objectives are intermixed.
- Simulations: I think the suite of simulations provides valuable insight into the different variables in the energy balance. However, the author needs to make a distinction between humidity moisture and surface roughness topography/DEM as well as improve the explanation for the source of the distributed temperature and humidity data. A priori, it seems that it could be useful to conduct simulations with halved temperature and humidity deviations from the means. I think that the justification and explanation for choosing the 7 simulations needs to be strengthened and clarified, as I miss the reasons for performing the specific simulations.

Specific comments:
- Title: suggest simplifying to "using 3D turbulence-resolving simulations to investigate the energy balance of a debris-covered glacier"
- Abstract: needs to be original and not contain exact sentences from the body of the manuscript. It would be appropriate to mention that you designed a series of simulations that differed in input parameters in order to isolate and investigate the effects of varying those parameters.
- Line 12: remove first "total"
- Line 20: suggest replace "ascertain" with "provide insight into"
- Line 32: explain/rephrase "non-saturated surface" or provide a reference
- Line 43: add a citation for gravel. Be more specific that you are talking about surface roughness lengths. Later, you use surface roughness interchangeably with topography (and DEM). Specifying length here would eliminate subsequent confusion.
- Line 51: what does "spatial melt" mean? You don't cite any distributed energy

balance model on debris covered glaciers: e.g., Reid et al (2012), Fyffe et al (2014)
- Line 54: "we" or "they"?
- Line 55: there are many remotely-sensed observations. If excluding these, be specific. Also, missing references with data: Vincent et al (2016), Nicholson et al (2018), Nicholson Mertes (2017)
- Line 58: this is a bold claim. Be specific for what observations are over short time spans.
- Line 60: modeling would lend significant new insights, but you haven't argued it is "essential." Language implies it is the only method to shed light on the question, whereas it is only one approach.
- Line 64: "heat fluxES"
- Line 65: "gradientS"
- Line 65: Steiner et al (2018) found that bulk methods overestimate turbulent heat fluxes. . . seems relevant to mention
- Line 66: summarize the "many assumptions" since this point is central to the problem you aim to address
- Line 70: "therefore" is for results, not for clarification. Suggest id est here. "and therefore wind, humidity and temperature fields" –> "(i.e. its wind, humidity and temperature fields)"
- Line 86: "we are converging to that range in this study": meaning unclear
- Line 105: inconsistent formatting
- Lines 107 - 108: rephrase sentence
- Figure 1 and most subsequent figures: include axis units and labels!
- Line 131: suggest section title "field measurements." Section as a whole needs tightening of language to be more to-the-point. It is difficult to decipher meaning.
- Line 135: what is the purpose of this citation?
- Line 149: what is the purpose of these citations? Consider adding the words "following" or "after" if that's what you mean
- Lines 151 - 152: info in sentence "we only. . . the model" needs to be added to the

previous section to explain the extent of the microHH domain
- Line 157: if this dash is to indicate negative, make sure it is on the same line (and page!) as the following number... and that Fig2A has the stated range included in its colorbar.
- Line 159: suggest rewrite "2- Line 162: suggest replace "used... LES)" with "a computational fluid dynamics model designed to simulate turbulent flows in the atmosphere through direct numerical simulation (DNS) and large eddy simulation (LES)."
- Line 164: suggest replacing "what could be interpreted as" with "which effectively renders it"
- Line 166: refer the reader?
- Line 170: instead of what, which (error appears many times)
- Line 171: instead of therefore, thereby
- Line 173: instead of are, is
- Lines 193 - 194: these lines need review with respect to units and consistency. (Density is not kg/kg; what is "thermal diffusivity for heat" with a value of 0.1 m^2/s? If you mean thermal diffusivity of water, give a calculation with specified T and P. Should be $\sim$0.1 x 10^{-7} m^2/s)
- Line 196: "accumulated temperature" is not intuitive. Please explain.
- Line 209: here and elsewhere, meterS when more than 1
- Line 210: condition is
- Line 232: themselves
- Line 233: suggest "are periodic, such that air flowing out of one side of the domain will enter on the opposite side."
- Line 249: the table lists seven experiments, not six
- Lines 263 - 264: these sentences are superfluous
- Line 265: By this point, I am missing an in-depth description of experiment design and what question each experiment was designed to answer. A reader can possibly deduce this from the results, but the purpose should be stated more explicitly.

- Table 1 caption needs proofreading (homogenous or homogenously?; should be "spatially varying")
- Line 273: 2012 (not 2013)
- Line 300: suggest "all fluxes are defined as positive towards the surface except for the conductive heat flux"
- Line 302: suggest renaming the section "results and discussion" and including more discussion rather than assuming that the reader can deduce the significance from the figures (e.g. line 308: "the effect of the surface roughness on the SHF and LHF is evident (figure 3A - F)"). This section would benefit from an overview of the fact that the authors perform seven experiments designed to very key parameters that control turbulent heat fluxes in order to investigate the relative importance of various controls. Then, the subsections and figure captions could be strengthened by statements of which tests were designed to test which variables.
- Line 306: introduce that for temperature you contrast HET_T with HOM_{DEM} and mention how you plan to incorporate "REAL"
- Figure 4: this figure summarizes the results of the experiments very well. The caption could benefit from a reminder of the sign convention for fluxes.
- Section 3.1.4: it is not clear why only three of the experiments are discussed in this section. The last sentence of this section raises a very important point, which should be discussed further.
- Line 351: "spatial variations in"
- Figures 5, 6, 7: what kind of cross sections are these? (Reference the black lines in figure 2 in the manuscript text around figures 5 - 7.)
- Line 360: use topography rather than DEM. The two are not interchangeable.
- Line 364: "ejections." What about diffusion and advection?
- Line 377: "reduce," but can't it also increase? Suggest "alter."
- Figure 7: the last column of some figures is striking because the change with height (cold to warm versus warm to cold) differs between experiments. Must discuss this and other features of the figures.

- Line 372: point out specific features in figures
- Lines 395 and 402: LC or LT?
- Figure 8: dry debris and wet debris (change word order). It would be more intuitive to group the dried debris as A and B, the wet debris as C and D, etc.
- Lines 395 - 396: this sentence seems to negate the importance of the following figure. Needs clarification.
- Line 405: paraphrase "REAL case" for clarity
- Line 407: "spatial distribution of surface moisture. . ."
- Line 410: good insight into physical properties. Not clear how to disentangle effects of topography from effects of debris.
- Lines 411 - 413: this is an important point which is difficult to discern because of the language. Rewrite. Also, author(s) need to distinguish their contributions from physical principles. SHF more sensitive to T_{surf} for dry debris is true b/c water has a higher heat capacity than air. "10 times as high" in next line is something learned through the model.
- Lines 416 - 417: If these are averages, why is there an uncertainty? And why is it greater than the average?
- Line 421: "near-surface air is saturated" contradicted at end of paragraph
- Line 429: weighted how? Approach needs an explanation.
- Line 441: Suggest "figure 9 shows conductive flux into the debris under the seven simulations."
- Line 452: can you say anything here about the physics with respect to thermal conductivity, density, and heat capacity? Conductive heat flux is determined by the temperature gradient in the debris, so this is expected. Clarify what additional insight your simulations provide.
- Table 2 line 480: what is the significance of the non-normal distributions, for which standard deviations exceed the means? Consider using other statistical metrics instead/also.
- Table 2: refer to this table in the text

- Table 2: what is the breakdown of ice-cliff area vs. debris area in the domain? Mention in caption and discuss in text if you contrast in a table.
- Lines 467 - 472: relate to recently published findings on ice cliffs in HMA
- Line 477: observationS of what value(s)?
- Lines 504 - 505: clarify with "every 10 seconds of [time interval] in the simulation"
- Figure 10: label ice cliff face, give variables in headings for each row of figures (A - F, G - L, and M - R), and specify the exact time interval in the caption. Also, should be 660$<$x$<$500.
- Line 529: "and is" to "that was"
- Line 531: turbulent fluxes likely play
- Line 532: it is appropriate to refer to the figure, but the reader cannot "see" the windspeed derivative where the ice cliff changes slope. Labeling the ice cliff and circling the region of interest on the figure would help.
- Line 535: "wind flow does flow": rewrite
- Section 4: suggest renaming as "sensitivity to Reynolds number" and start with a short description of the Reynolds number and why you chose to perform a sensitivity test on it. The first paragraph of this section states that both DNS and LES are impossible. The meaning of this paragraph is especially difficult to discern from the English that is used.
- Lines 545 - 550: what is the effect of the different resolutions on the profiles near the surface, where the difference is most apparent? You show the resolution is not too large for achieving accuracy, but could the same patterns be captured with a resolution larger than one meter? How much larger?
- Lines 561 - 563: rephrase
- Line 556: topography, not orography
- Section 5 "Limitations": this section especially needs proofreading by the author(s). The writing makes it difficult to discern many of the concepts, which are ones important to the paper.
- Line 576: add that debris moisture is important to not only turbulent heat fluxes but

also the conductive heat flux that ultimately melts glacier ice
- In this discussion of moisture, can you add some discussion of season and any implications of your findings for the monsoon season in particular?
- Line 598: here, it sounds like the AWS station you used *happened* to be in a spatially-representative place but that you got lucky because "in reality it remains very hard to locate a station such that it is representative for the whole domain." If this is not the case, please change the language. Additionally, it would be helpful if you quantified the amount of bias that could be introduced by upscaling point measurements not representative of the domain.
- Line 615 - 618: these lines read that you investigated the impact surface temperature and specific humidity have on surface specific humidity and temperature. Please rewrite with greater clarity.
- Line 642: I would think that that the bare ice on ice cliffs has a higher albedo than debris covered surroundings; explain or cite otherwise.
- Line 648: this paragraph is important to include only if you quantify and show an example of the large biases that are possible.
- Line 661: add labels to axes in the .gif's (videos)
- References: cite publications in The Cryosphere, not The Cryosphere Discussions where possible (e.g., Rounce et al., 2015)

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