

The Cryosphere Discuss., referee comment RC4
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Comment on tc-2022-64

Álvaro Ayala (Referee)

Referee comment on "Estimating degree-day factors of snow based on energy flux components" by Muhammad Fraz Ismail et al., The Cryosphere Discuss.,
<https://doi.org/10.5194/tc-2022-64-RC4>, 2022

PAPER SUMMARY AND RECOMMENDATION

Ismail and co-authors investigate how degree-day factors (DDFs) depend on the components of the snowpack energy balance. Assuming a snowpack close to melting conditions and a negligible cold content, the authors connect DDFs to the variations of each energy balance component by means of a set of widely used equations. In this way, DDFs are related with different characteristics and conditions, such as elevation, latitude and meteorological variables. The authors provide several summary tables and figures that can be used by other researchers to estimate DDFs in poorly monitored regions using minimum data requirements. Additionally, the authors estimate the impact of climate change on DDFs. They conclude that cloud cover and snow albedo are the main processes controlling DDFs and that DDFs cannot be treated as constant parameters.

The study is appropriate for The Cryosphere. The article is well written, but some parts describing the equations can be shortened. I think that the authors do a valuable contribution. Having tools to estimate DDFs is a good idea, and it can be useful for researchers working on the snow hydrology of poorly monitored regions. However, I think that the article needs to be improved before being suitable for publication. Please see my main comments.

MAJOR COMMENTS

1. Presentation and role of the datasets

- Field dataset: The purpose of including the datasets from Brunnenkopfhütte and Naran

stations is not clearly presented. The authors should mention in the Introduction what is the role of these datasets in their study. Are they used as validation, or test sites? Do the authors make tests at the catchment or point scales? Importantly, the use of the Naran dataset comes a surprise in the middle of the discussion section.

- Climate change dataset: Please provide more details about this dataset and add this analysis to the objectives of the study.

2. Discussion section

In this section, the authors continue their analysis and calculations, but they provide almost no comparisons with the results of other studies. The authors should discuss their results using the literature presented in the Introduction. Additionally, I recommend the inclusion of some other references regarding the spatial and temporal transferability of degree-day factors (or temperature factors) and melt parameters that, in my opinion, are missing (Ohmura, 2001; Carenzo et al., 2009; MacDougall and Flowers, 2011; MacDougall et al., 2011; Gabbi et al., 2014) . The limitations of the approach proposed by the authors and the assumptions made through the article should be more discussed. For example, the authors validate their approach using only one monitoring station, can the authors include more data? There are certainly more datasets available for which DDFs have been derived. Otherwise, this is an important limitation of the study that should be discussed.

3. Conclusions and recommendations

As the aim of the study is to “quantify the effects of spatial, temporal, and climatic conditions on the DDFs” and the conclusion is that “DDF cannot be treated as a constant parameter”, what are the recommendations of the authors to a researcher modelling the snow hydrology of poorly monitored catchments? Should that researcher use a range of parameters from your equations? How large should be the variability of DDFs in space and time? Different DDFs for each sub-catchment, slope or elevation band? How often should the DDFs change in time? Every week, month or season? I think that the article would benefit from such discussions and recommendations.

MINOR COMMENTS FOR THE AUTHORS

12-13: I would add “At mid-latitudes, seasonal snow ...” because this seasonal pattern is not necessarily found on every snow and ice dominated mountain catchment (e.g. tropical glaciers).

13: I think that the concept of snowmelt runoff is wider than what the authors are describing. The authors are describing only the process of melt whereas snowmelt runoff include other processes controlling the movement of excess meltwater through a catchment.

21: is physically based -> is based

22: I don't think that the formulas are "approximate", they just have limitations and assumptions.

23: observed -> field-derived. DDFs cannot be measured in the field because they are not a physical quantity.

30: "albedo is likely to be higher", there are also other reasons, such as lower radiation and temperatures, aren't they?

35: It would be interesting to mention somewhere in the Introduction that researchers usually select DDFs values from other studies and that the spatial transferability is not always good [e.g. Carenzo et al., 2009; Wheler, 2009].

35: The authors should briefly mention at the end of the Introduction what is the role of the Study area in the article as Section 2 "Study area" comes as a surprise. See my main comment.

79: "longer time periods" Can the authors be more precise? Weeks, months, years?

81-82: Also, the spatial variability of air temperature does not fully describe the spatial variability of the energy balance.

118: Please mention the country

123: The Kopp reference is not necessary here as the authors also have a DEM of the catchment.

171-172: "The balance of the energy fluxes over the surface of the snowpack". Please note that Q_G (ground heat) is not a surface flux. By including ΔQ and Q_G , the authors are describing the energy balance of the entire snowpack and not only the surface, which has not heat capacity [den Broeke et al., 2011]. Otherwise please clearly define what is the control volume considered by the authors.

179: The length of this section can be reduced.

182: No reference is needed for equation 5

241: I'm a bit confused, when the authors correct by elevation, what is the term that goes in eq. 6, K_z or K_T ?

277: Please clarify at what height above the surface are P_v and T_a measured.

300: What do the authors mean by "a probabilistic reasoning"?

344: I think a step or equation is missing here and it should be that relating RH and p_0 . Or how do the authors calculate p_v ? Also, are the authors assuming saturated conditions at the snow surface?

354/375: Sections 3.2.5 and 3.2.7 don't read as "Methods". They seem a review on the subject. As both terms (Q_G and ΔQ) are neglected by the authors, I suggest the shortening of these sections and to move them to the beginning of Section 3.2 where a suitable justification to neglect them can be provided.

422: Delete "approximate".

431: higher altitudes, as well as dry climates.

504: As wind speed is highly variable in space and time, I don't think that the authors can refer to "typical values". It would be better to write something such as: "... can be roughly estimated based on the topographic and climate characteristics of the study site".

551: I think that this is the first time that the authors mention the goal of these dataset. Please see my main comments.

579: I believe that this is not clearly a discussion section because there are almost no

comparisons against other studies (and almost no references). Instead, the authors present more results and analysis. Please my main comments.

592: This is the first time that the authors mention these data. Please properly introduce this site and the dataset in section 2. Also explain what is the purpose of including this dataset.

598: Please change the word "altitude" by "elevation" throughout the article. Altitude is the vertical distance between an object and the earth's surface.

606: Why does the solar angle change with altitude?

693-695: Not clear, please reword.

702-705: This belongs to methods. The climate change analysis should be introduced earlier in the manuscript. Provide more details about these data, are those values an average of different GCMs?

697: Musselman et al. [2017] is an excellent article regarding slower melt rates in climate change scenarios.

SUGGESTED TECHNICAL CORRECTIONS FOR THE AUTHORS

11: Meltwater

11: Consider: "Meltwater from mountainous catchments dominated by snow and ice is a ..."

36: Meltwater

42: Delete "for the prediction"

44: Delete "runoff". The authors discuss only the process of melt.

59: Add "using runoff" after DDF

61: Delete "runoff"

68: by the inclusion

72: the position

95: Since melt depends ...

117: system and lies

119: delete about or ~

127: a standard

128: Brunnenkopfhütte site

146: Delete "concrete", or maybe use "actual".

221: Delete ", " after disadvantage

283: the above relation

284: ... snowpack amounts to

294: Add “,” after parameterize

294-295: and their effects on radiation depend...

329: the snow and the snow surface

371: even during extreme weather conditions

588-590: Please rewrite these lines for clarity.

591: “The example”, what example?

638: see Table

650: in Table

FIGURES

Figure 1: I think that m (instead of cm) are enough for “High” and “Low” in the legend.

Figure 4: Why is the clearness index (K_T) at a given elevation larger for overcast conditions than for clear sky? Shouldn't be the opposite? Please clarify

Figure 10: Why exactly do DDFs on each panel (present and scenarios) decrease as the season progresses but in Figure 9 DDFs increase as the season progresses?

TABLES

Table 1: Explain the name of the variables.

Table 1: Please provide SRm in W/m²

REFERENCES

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