Estimating degree-day factors based on energy flux components

Referee #4: Álvaro Ayala (Comments and Responses)

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.

Dear Reviewer,

Thank you very much for your comments and suggestions to improve the manuscript. As suggested, we shall restructure the results and discussion section in order to make it clearer. We shall make numerous changes in the revised version of our manuscript. Below, we repeat each of your comment and our reply to them one by one. All responses are in blue font for clarity of reading.

Muhammad Fraz Ismail

On behalf of all the authors

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.

The main purpose of using the Brunnenkopfhütte snow station data is to show how the degree-day factor can be estimated under naturally varying hydro-meteorological conditions in the field. We shall clearly mention the purpose of these datasets in the data section. The

Brunnenkopfhütte station is our test site where we have installed our snow and meteorological station. We have done a point scale analysis based on the datasets from Brunnenkopfhütte test site.

But when we are discussing the Naran station as well as Upper Indus area then our aim is to address the problem related to estimate DDFs in poorly monitored regions, where only limited data is available. We shall clarify these points in the revised manuscript.

For climate change impact assessment (i.e. temperature) the bias-corrected climate scenarios from four GCMs (GFDL-ESM2M, HadGEM2-ES, IPSL-CM5A-LR, MIROC5) driven by two representative concentration pathways (RCPs), which were provided by the ISIMIP project (Hempel et al. 2013; Frieler et al. 2017) were used (Ismail et al. 2020). We shall also clarify this point in the revised manuscript.

- Frieler, K., Lange, S., Piontek, F., Reyer, C. P. O., Schewe, J., Warszawski, L., Zhao, F., Chini, L., Denvil, S., Emanuel, K., Geiger, T., Halladay, K., Hurtt, G., Mengel, M., Murakami, D., Ostberg, S., Popp, A., Riva, R., Stevanovic, M., Suzuki, T., Volkholz, J., Burke, E., Ciais, P., Ebi, K., Eddy, T. D., Elliott, J., Galbraith, E., Gosling, S. N., Hattermann, F., Hickler, T., Hinkel, J., Hof, C., Huber, V., Jägermeyr, J., Krysanova, V., Marcé, R., Müller Schmied, H., Mouratiadou, I., Pierson, D., Tittensor, D. P., Vautard, R., van Vliet, M., Biber, M. F., Betts, R. A., Bodirsky, B. L., Deryng, D., Frolking, S., Jones, C. D., Lotze, H. K., Lotze-Campen, H., Sahajpal, R., Thonicke, K., Tian, H., and Yamagata, Y.: Assessing the impacts of 1.5 °C global warming – simulation protocol of the Inter-Sectoral Impact Model Intercomparison Project (ISIMIP2b), Geosci. Model Dev., 10, 4321–4345, https://doi.org/10.5194/gmd-10-4321-2017, 2017.
- Hempel, S., Frieler, K., Warszawski, L., Schewe, J., and Piontek, F.: A trend-preserving bias correction – the ISI-MIP approach, Earth Syst. Dynam., 4, 219–236, https://doi.org/10.5194/esd-4-219-2013, 2013.
- Ismail, M.F., Naz, B.S., Wortmann, M. et al. Comparison of two model calibration approaches and their influence on future projections under climate change in the Upper Indus Basin. Climatic Change 163, 1227–1246 (2020). https://doi.org/10.1007/s10584-020-02902-3

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.

We shall restructure the discussion section and provide more comparison insight as suggested by the reviewer. We shall also include important references specifically regarding the spatial and temporal transferability of degree-day factors.

We agree that the presented approach has its limitations, to our opinion mainly the assumption that the snowpack is isothermal at 0°C and in fully ripe state. However, the aim of the paper is not to present a new and comprehensive degree-day factor approach, which certainly would have to be validated by a number of datasets. We rather want to demonstrate how well established energy balance formulas can be applied in data scarce situations to estimate melt and translate this into degree-day factors. For this purpose, we present tools like the set of existing formulas, summary tables, and graphs and we give a number of examples in order to demonstrate the influencing factors under several spatial and meteorological conditions.

In contrast to exemplifying the individual factors, the Brunnenkopf station example shall demonstrate how these tools can be applied in a complex real-live situation and give an idea about accuracy of estimated degree-day factors. Moreover, in Figure 8 we specifically used this example to show the effect of a fully ripe snowpack vs one with a considerable cold content. Taking the limited accuracy of field derived degree-day factors, we feel it would be unsuitable to make a similar comparison with a foreign dataset ourselves without knowing all subtles of the data. However, we would be more than happy if our paper would motivate other researchers to test the presented tools with their own familiar datasets.

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.

Yes, the aim is to quantify the effects of spatial, temporal, and climatic conditions on the degree-day factors. As explained by several authors (Braithwaite 1995a, Hock, 2003, Kayastha et al. 2000), we have also recommended that the DDF cannot be considered a constant model parameter. Rather, its spatial and temporal variability must be taken into account especially when using temperature-index models for forecasting present or predicting future water availability. In section 5.6 and 5.7 of the manuscript we have tried to show that how one can estimate the degree-day factors based on only temperature data and assumed typical climatic conditions. We have presented summary tables and figures in order to get an initial idea about the range of degree-day factors based on available information.

We have showed that how the degree-day factors could change depending upon elevation. Of course if sub-catchments have different hydro-climatic conditions then it will ultimately impact the degree-day factor. There are several recommendations on changing the degree-day factors in space and time, for example on monthly as well as on seasonal basis (Kayastha et al. 2000, Braithwaite 1995a). We presented 10-daily DDF for forecasting water availability in an operational model. We shall make it clearer in the revised manuscript.

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).

Thank you very much for your comment and necessary clarification. We shall update it in the revised manuscript.

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.

We agree with the reviewer on this point. We shall clarify it in the updated manuscript. We shall also remove the word ‘runoff’ as indicated.

21: is physically based -> is based

We shall update it in the revised manuscript.

22: I don’t think that the formulas are “approximate”, they just have limitations and assumptions.

We agree with the reviewer on this point. We used formulas related to minimal data requirements. We shall update it in the revised manuscript.

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

We shall update it in the revised manuscript.

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

We are comparing period of similar degree-days so temperature will not be higher. Yes, radiation will be lower as you pointed and we shall include it in the revised manuscript.

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].

We shall update it in the revised manuscript.

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.

We agree with the reviewer on this point. We shall clarify the role of study area as well as the data sets used. We shall update it in the revised manuscript.

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

We shall update it in the revised manuscript. The longer time period (i.e. 10-daily, monthly, seasonal) as mentioned by different authors (Ismail and Bogacki, 2018; Braithwaite 1995a; Kayastha et al. 2000).

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

Thank you very much for your comment. We shall consider this in the revised manuscript.

118: Please mention the country

We shall update it in the revised manuscript.

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

We shall delete the unnecessary reference here in the revised manuscript.

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 DeltaQ 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 control volume is considered by the authors.

Thank you very much for your comment. We shall clarify by specifying the control volume in the revised manuscript.

179: The length of this section can be reduced.

Thank you for your comment. We consider short wave radiation is a very important component. We shall see where it can be shortened.

182: No reference is needed for equation 5

Ok. We shall delete it.

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?

K_T goes into eq. (6). $K_T = K_z \times K_{T0}$. We used the formulation in eq. 18 to provide the definition of clearness altitude factor K_z. Nevertheless, we shall clearly explain the clearness altitude factor in combination with Figure 4.

277: Please clarify at what height above the surface are Pv and Ta measured.

Thank you for your comment. We shall add that Brutsaert developed eq. 22 for p_v and T_a measured at screen level. In our examples, both parameters are measure at 2m above the surface.

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

We wanted to say that Badescu and Paulescu (2011) used probability distributions to develop relations between cloudiness and relative sunshine hours and showed that a linear relation is a first good estimate. We shall formulate in the revised manuscript accordingly.

344: I think a step or equation is missing here and it should be that relating RH and p0. Or how do the authors calculate pv? Also, are the authors assuming saturated conditions at the snow surface?

Thank you very much for your comment. We thought it is obvious that p_v can be calculated from RH and p_0 but we shall include this step in the revised manuscript and shall also add that we assume 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 DeltaQ) 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.

Thank you very much for your comment. We shall see where these sections can be shortened. We prefer to keep these sections here because these are in line with the equations mentioned earlier.

422: Delete “approximate”.

We shall delete it in the revised manuscript.

431: higher altitudes, as well as dry climates.

Thank you for your comment. We shall update it in the revised manuscript.

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”.

Thank you for your comment. We agree and it shall be updated it in the revised manuscript.

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

We shall update this section in the revised manuscript as suggested in 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.

We shall update this section in the revised manuscript as suggested in 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.

We agree with the reviewer. We shall update this section in the revised manuscript as suggested in main comments.

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

Sometimes elevation and altitude are used interchangeably. We shall see where this wording can be used and update this in the revised manuscript.

606: Why does the solar angle change with altitude?

Because solar angles changes from February to May. We shall add this in the revised manuscript.

693-695: Not clear, please reword.

We shall make it clear in the revised manuscript.

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?

We agree with the reviewer. We shall update this section in the revised manuscript as suggested in main comments.

The projected changes in temperature are median values as mention in Ismail et al. (2020). These are based on four models four GCMs (GFDL-ESM2M, HadGEM2-ES, IPSL-CM5A-LR, MIROC5) driven by two representative concentration pathways (RCPs), which were provided by the ISIMIP project (Hempel et al. 2013; Frieler et al. 2017).

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

Thank you very much for sharing this reference. We shall cite this article in the revised version of manuscript.

SUGGESTED TECHNICAL CORRECTIONS FOR THE AUTHORS

11: Meltwater

We shall update it in the revised manuscript.

11: Consider: “Meltwater from mountainous catchments dominated by snow and ice is a…”

We shall update it in the revised manuscript.

36: Meltwater

We shall update it in the revised manuscript.

42: Delete “for the prediction”

We shall update it in the revised manuscript.

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

We shall update it in the revised manuscript.

59: Add “using runoff” after DDF

We shall update it in the revised manuscript.

61: Delete “runoff”

We shall update it in the revised manuscript.

68: by the inclusion

We shall update it in the revised manuscript.

72: the position

We shall update it in the revised manuscript.

95: Since melt depends …

We shall update it in the revised manuscript.

117: system and lies

We shall update it in the revised manuscript.

119: delete about or ~

We shall update it in the revised manuscript.

127: a standard

We shall update it in the revised manuscript.

128: Brunnenkopfhütte site

We shall update it in the revised manuscript.

146: Delete “concrete”, or maybe use “actual”.

We shall update it in the revised manuscript.

221: Delete “,” after disadvantage

We shall update it in the revised manuscript.

283: the above relation

We shall update it in the revised manuscript.

284: … snowpack amounts to

We shall update it in the revised manuscript.

294: Add “,” after parameterize

We shall update it in the revised manuscript.

294-295: and their effects on radiation depend…

We shall update it in the revised manuscript.

329: the snow and the snow surface

We shall update it in the revised manuscript.

371: even during extreme weather conditions

We shall update it in the revised manuscript.

588-590: Please rewrite these lines for clarity.

We shall rewrite it in the revised manuscript.

591: “The example”, what example?

We shall update the sentence like, ‘In Figure 9 (a), we compare’. We shall add it in the revised manuscript.

638: see Table

We shall update it in the revised manuscript.

650: in Table

We shall update it in the revised manuscript.

FIGURES

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

We shall update it in the revised manuscript as shown below.

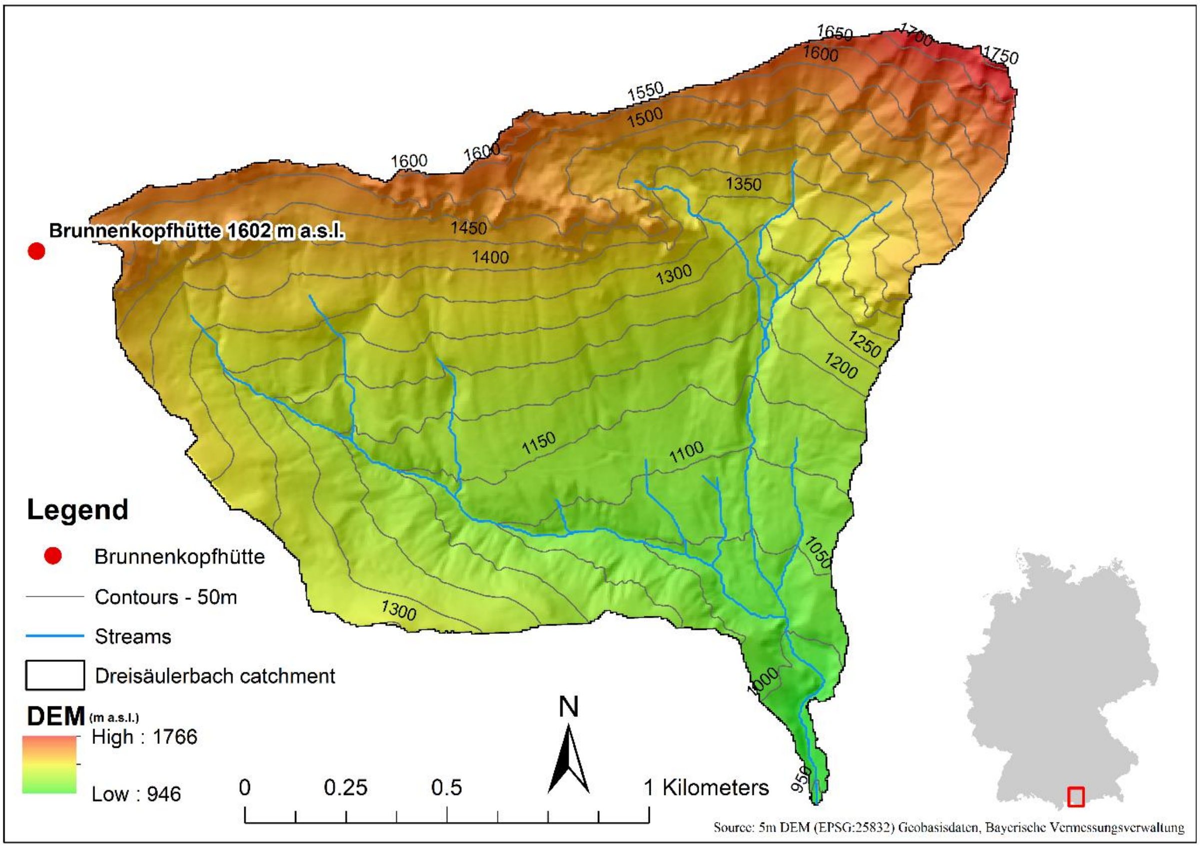

Figure 4: Why is the clearness index (K_T) at a given elevation larger for overcast conditions than for clear sky? Shouldn't be the opposite? Please clarify.

Actually, the Figure 4 is showing the relative increase in the altitude factor depending upon sky conditions (i.e. clear sky K_T = 0.75 and overcast condition K_T = 0.25). For the same elevation difference, the absolute change in clear sky is greater compared to overcast condition. We agree that we shall clarify this point in the revised manuscript.

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?

We have already mentioned in section 5.6 of the paper that in contrast to Figure (d), the *DDF* decreases continuously in all elevation zones (i.e. Figure 10) in the subsequent melting periods. It is because air temperature and thus degree-days rise faster compared to the increase in melt. We shall make it clearer in the revised manuscript.

TABLES

Table 1: Explain the name of the variables.

We shall add an explanation of each parameter in footnote in the revised manuscript. The footnote reads as follows.

T_a = Air temperature
P = Precipitation
u = Wind speed

RH = Relative Humidity
A= Albedo (*only considered when ground is snow covered*)
K_T = Clearness index
SR_{in} = Incoming shortwave radiation

Table 1: Please provide SR_{in} in W/m^2

We shall provide SR_{in} in W m^{-2} it in the revised manuscript.

References:

Ismail, M. F. and Bogacki, W.: Scenario approach for the seasonal forecast of Kharif flows from the Upper Indus Basin, Hydrol. Earth Syst. Sci., 22, 1391–1409, https://doi.org/10.5194/hess-22-1391-2018, 2018.

Kayastha, R. B., Ageta, Y. & Nakawo, M. (2000). Positive degree-day factors for ablation on glaciers in the Nepalese Himalayas: case study on Glacier AX010 in Shorong Himal, Nepal. Bulletin of Glaciological Research, 17, 1-10.

Braithwaite RJ 1995a. Positive degree-day factors for ablation on the Greenland ice sheet studied by energy-balance modelling. Journal of Glaciology 41, 137, 153-160.

Hock, R.: Temperature index melt modelling in mountain areas, Journal of Hydrology, 282, 104–115, https://doi.org/10.1016/S0022-1694 (03)00257-9, 2003.