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The Cryosphere Discussions

Interactive comment on "On the use of incoming longwave radiation parameterizations in a glacier environment" *by* J. Sedlar and R. Hock

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In this manuscript (MS), existing parameterizations of clear-sky and all-sky incoming longwave radiation (LWR) are applied to a data set collected during 4 melt seasons at Storglaciären, a small mountain glacier in Sweden. The first objective of this MS is to assess the performance of these different parameterizations. For the various parameterizations, screen level temperature and water vapor pressure, and observed cloud fraction are used as predictors. As data on the latter are not routinely available, an attempt is made by the authors to express the cloud fraction in terms of the atmospheric transmissivity to shortwave radiation. The second objective of the MS is to check the performance of this parameterized cloud fraction.

The MS could be an interesting contribution to TC, aiming for an audience of energy-balance modellers, but some substantial improvements to the manuscript should be made by the authors. The focus of these alterations should be on (1) an improved, and preferably extended, comparison between LWR parameterizations, (2) an improved treatment of the statistics and of the derivation of cloud fraction parameterizations, and (3) a more physically-based discussion on the correctness, applicability and limitations of the cloud fraction parameterization in terms of atmospheric transmissivity. Also, I would suggest to change the title of the MS to better fit its content. Below, I will discuss the MS in more detail.

General remarks

The first objective of the MS is to test existing parameterizations for LWR on their applicability to the Storglaciären data set. The amount of LWR arriving at the Earth's surface is dependent on the vertical composition of the atmosphere. As an approximation to the usually lacking vertical profiles of temperature, water vapor and gas concentrations, temperatures and water vapor pressure at screen level are used. To make a connection between the vertical structure of the lower atmosphere and local screen level measurements, the coefficients in the parameterizations by Konzelmann et al. (1994, hereafter K94) and Brutsaert (1975) should be derived for each specific site. The authors reach this conclusion after trying different parameters in the K94 parameterization, but this conclusion should not be surprising, as the characteristic vertical structure of the atmosphere is different for each location. The comparison between the 5 different K94 parameterizations (Table 1) is therefore not relevant. I would suggest to include only the optimal K94 fit in the comparison and leave out the other four. It would on the other hand be more useful to test other parameterizations (like the ones given in the paper by Sugita and Brutsaert, 1993) for their validity using this data set. The current suite of functional forms for clear-sky LWR is rather limited.

The authors extend the discussion to LWR parameterizations under all-sky conditions. Here, the coefficients in the cloud factor parameterization by Kimball et al. (1982) are not derived specifically for the site. This is a flaw in the setup of the comparison with the other all-sky parameterizations, and needs to be addressed. If this is done, the

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parameterization by Kimball et al. (1982) gets a fair chance in the comparison with the K94 parameterization. The reported inaccuracy of the Kimball parameterization (page 496, line 15-17) will probably be reduced significantly. Should other parameterizations be added in the revised MS, also these should be fitted to the Storglaciären data set.

The coefficient of determination r^2 (regression coefficient) is used in a confusing way throughout the MS. It tells you how much of the variance in an observed variable is explained by other measured variables. So, when trying to explain ϵ_{cs} in terms of e_a and T_a, the r^2 will be identical for every parameterization in which these two quantities are used. The r^2 will increase as more explaining variables are added. This is seen in table 1, where all but one parameterizations show the same r^2 for clear-sky data, which is to be expected. It is therefore surprising that this is not the case for the different K94 fits to the all-sky data in table 1, where r2 ranges from 0.73 to 0.77. In table 2, where cloud fraction is parameterized in terms of atmospheric transmissivity, the r^2 keeps improving as more explaining terms are added to the parameterization, but this is partly to be expected. I would suggest the authors to perform a statistical test, i.e. an F- or χ^2-test, to see whether the addition of extra terms is warranted. The RMSE is, as the authors point out, the quantity of preference when comparing different parameterizations based on the same predicting quantities. Furthermore, I would suggest to explain the term mean bias error (MBE), and what it adds to the RMSE - otherwise, it could be left out.

Some of the parameterizations proposed in 4.4 do not comply with physical considerations. For instance, the linear fit of τ to n gives a $\tau > 1$ for $n = 0$ and the quadratic fit does not give a monotonically decreasing n as τ increases. A simple linear relation between τ and n could perform much better if it is forced through $(\tau, n) = (0.8, 0)$, for example. Some scatter in figure 4(a) may come from variations in the solar zenith angle, and presumably, a simple linear relation would significantly improve when the incoming shortwave radiation is scaled to the maximally possible amount of shortwave radiation at the surface, for which empirical formulas are readily available. Adding these kind of theoretical, physically-based considerations to the functional form of $n(\tau)$ seems a more viable way to improve a parameterization than simply adding more polynomial terms.

The relation between cloud cover and atmospheric transmissivity is a tricky one, especially in glacier environments, as pointed out by several authors (e.g. Shine, 1984; Fitzpatrick et al., 2004). When surface albedos are high, multiple reflections of shortwave radiation between the surface and the cloud base add to the incoming shortwave radiation flux, so that τ becomes dependent on cloud cover, cloud optical depth and surface albedo in a non-trivial way. The relatively good performance of the simple $n(\tau)$parameterizations in this study may come from the fact that only melt season data is used, and that the multiple reflection effect is reduced since Storglaciären is only a very small glacier surrounded by low-albedo surface in summer. Does this mean that the parameterization is indeed limited to small glaciers? How well would the winter-time performance of the parameterization be for Storglaciären, or on other valley glaciers which receive more shortwave radiation in winter? And how well are parameterizations expected to perform in winter, when LWR variability is generally higher? I would suggest the authors to expand on the applicability and limitations of the parameterization of cloud cover.

Lastly, I would suggest another title which reflects better what is actually done in the MS, on the lines of *Testing longwave radiation parameterizations under clear and overcast conditions at Storglaciären, Sweden*.

Specific remarks (the numbering here refers to page.line)

488.12 The use of vice versa is not correct here.

489.2 While I agree that accurate LWR parameterizations are desirable, the way its importance is motivated deserves reconsideration. Although LWR is by far the largest source of melt energy, its sensitivity to a change in the forcing is smaller than that of other terms, like net shortwave radiation. At a temperature of 280 K, the dL↓/dT is

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only about 2.5 W m^{-2} K^{-1}, whereas a change in albedo of 0.05 in summer leads to an increase of tens of W m^{-2}, not taking into account the strong feedbacks involved. The magnitude of the downwelling LWR does not *per se* make its accuracy of paramount importance for energy-balance modeling.

489.17 Please rephrase this sentence so that it becomes clear what *amount* and *temperature* refer to.

492.2 Change Yamannouchi to Yamanouchi, wherever necessary (including the bibliography).

493.17 I found the reference to the work of Kimball et al. (1982) confusing. In their introduction, Kimball et al. do mention the functional form which is presented in the MS in formula (5), but they do not use it in their paper. Formula (5) should be attributed to others (see references in Kimball et al.). In the remainder of the MS, I would not refer to formula (5) as the cloud factor proposed by Kimball (496.14) or similar.

493.20 The typical values for c and p (0.22 and 2, respectively) are mentioned by Sugita and Brutsaert (1993) as they refer to earlier work, so also this reference should be changed. Sugita et al. do not use these values themselves, but instead derive values specifically for their data set, exactly the thing that should be done for the Storglaciären data set too.

496.19 *this parameterization* - it is unclear what parameterization this sentence refers to.

498.13 Presumably, the CM11 pyranometer is meant, and not the PIR sensor.

502.11 Alternatingly, this paper is referenced as Gabathuler and Gabuthuler. Please correct wherever necessary.

503.3 Change Koenig to König.

509 Extend the y-axis to -30 W m^{-2} so that curves do not disappear from the plot (also

figure 5b).

510 The figure legend should not be plotted over the data.

References

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