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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Estimation of surface energy fluxes in the Arctic tundra using the remote sensing thermal-based Two-Source Energy Balance model" *by* Jordi Cristóbal et al.

Anonymous Referee #2

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General:

There is no doubt that the northern high latitude regions are undergoing significant change and will bear the brunt of continued climate change. The authors attempt to evaluate a two source energy balance model using eddy covariance data over a range of vegetation types in the Arctic. They found that improvements in the net radiation, soil heat flux and canopy transpiration schemes were needed in this unique Arctic environment to improve model performance. This work is potentially significant as there are few tools available for exploring the impact of changes in surface fluxes due to vegetation or future environmental change at the site to regional scale.

The manuscript is very well written and in general the paper is robust but the authors

simply use an existing model and tweak a few parameters to get a better fit. I'm not convinced that there is much scientific value that this paper has added. In addition, there is much discussion about the error of the model and comparison to error rates from previous models. Much of the comparison with previous studies simply state that observed and estimated results are in line with previous studies in the Arctic tundra. What have we actually learned from this study? There is nothing in the discussion that highlights the significance or implications of the results. The discussion could benefit from exploring why the models did or did not do so well and what could be done to improve them. What are the processes that are important that are not being accurately simulated. How does the physical and ecological environment challenge the modelling? How does this sort of model add to our understanding? There is a good opportunity to enhance the study using model benchmarking. Also not sure how this would all be scaled up to the whole Arctic.

Although the subject is highly relevant, given the lack of insight from the discussion and other issues in the paper, I would recommend accept with major revision.

Specific: The authors articulate a good case for undertaking their research and there is adequate acknowledgement of the previous literature although a summary of previous Arctic modelling that is relevant to your choice of model would be advantageous. They then propose an aim to evaluate the performance of the model during the Arctic growing season. However, it is unclear to me as to why you are doing this and what the ultimate goal is? Could you articulate what the big picture implications are in the introduction? In addition, I think you need to add an argument as to why this particular model as there are so many potential models with different scales and different functions. Why not use a process-based land surface model where you can relate the differences in model versus obs with processes rather than in your case changing a few parameters to get a better fit?

The authors use measured shortwave radiation yet estimate long wave radiation from observed air and land surface temperatures. I would have thought that this is problematic for Arctic environments and could result in a large error in the net radiation. Given that highly accurate net radiation and soil heat flux measurements are needed for this approach, what is error associated with estimating long wave radiation in the model? In addition, the authors assume that G is a constant fraction of net radiation. This assumption is untested and there is clearly a large uncertainty in the probable fraction into G due to differences in surface properties such as soil type and moisture conditions as the authors point out, but particularly also the composition and structure of the various organic layers which are ubiquitous across the Arctic. It is well understood that the properties of moss and organic materials in particular influence the thermal and hydrological properties of the soil greatly. Therefore, I would like to see a more formalised assessment of the relative uncertainty in the calculation of G and Rn.

The authors give a mean value of 0.14 for cG and 0.92 for alphaPTC over the Arctic tundra. There is a rather a lot of handwaving here to suggest a single value for the entire Arctic tundra. What was the range of values across different vegetation types in the Arctic tundra. What was the error around the mean for this value? In addition what is the influence of changing cover over the growing season on both these values?

Table 2 shows the TCAV at 2 cm but this is usually an integrated measure with probes at two and 4 cm. Please check this.

G is hard to measure. There is a great uncertainty in measurements of G in the tundra because traditional heat flux plates are made with an assumed thermal conductivity for loamy soils but we know in the tundra that this is primarily organic heat and moss which has a significantly lower thermal conductivity. Therefore self-calibrating heat flux plates or corrections are required. Can you quantify the uncertainty in your ground heat flux measurements which is an important term because it feeds directly into the energy balance?

The use of MODIS LAI is particularly problematic in Arctic areas and it has been noted that the largest discrepancies in MODIS LAI are at Arctic tundra sites where the MODIS

product overestimates woody cover proportions. Given that you have no LAI observations you cannot make any conclusions about how they relate to fPAR for example on page 13 line 30. What specific product was used, was it the 250 m resolution? What was the spatial extent of your footprint for this dataset and how does that relates to the spatial separation of your sites? Specifically which QC flags were used? How were gaps treated in the timeseries? Perhaps use MODIS fPAR. Given you have tower measurements of this you could validate the MODIS fPAR and assess the error here.

It is not clear as to how you distinguish between canopy and soil in these Arctic systems for the TSEB model. What do you define as soil and what is canopy? You have no significant woody vegetation to form a canopy in the first place. The surface layer consists of mosses, lichen, Forbes and shrubs and forms a continuous layer that cannot be partitioned into soil and canopy. I suspect in general you don't have any bare soil at your sites. Hence I'm not sure why you are using a two layer model here in the first place? Can you justify the use of a two layer model here? Therefore the assumption that fPAR is equivalent to fG is not robust. To use this you will need to demonstrate clearly that this is the case.

The description of the eddy covariance data is minimal. What software was used to process the data and what algorithms and parameters were used? Exactly what quality flags were filtered? Due to the importance of determining the energy balance components for this study it is crucial to provide a thorough analysis of energy balance closure at the different sites across different periods (i.e. daytime and daily). What percentage of data were excluded due to different quality control previously mentioned as well as the three criteria mentioned. How were gaps in the data filled and worthy gap filled data used in the analysis? The criteria of a surface energy balance closure of greater than 70% doesn't instill a lot of confidence in the measurements. I would assume from this that the energy balance closure is quite low. This is probably due to the difficulty in measuring the soil heat flux. It is well known that the belowground environment is complex including not just soil but also layers of peat and organic material as well as living moss and lichen. Therefore your estimates of G will be highly underestimated and will result in a low energy balance closure. Discuss. How did you account for these in the correction of the soil heat flux plates? At what depth did you have the heat flux plates placed? I see they were 8 cm but is that below the surface in the moss? If so then your heat flux plates are not in soil but in organic material. You should use the appropriate bulk density not the soil bulk density. Also it appears that you only have one heat flux plate measurement per site which is insufficient given the spatial heterogeneity in the surface. As previously mentioned the thermal conductivity of the heat flux plate is manufactured to a standard soil which will not be representative of what you are measuring in. This will all result in very large errors in the observed soil heat flux. Please provide a thorough estimate of error and uncertainty for this particular important measurement.

Given the difficulty in measuring G and the errors associated with that it may be worth trying to take G as a residual of the surface energy balance.

In addition, what is the uncertainty (random and model) in the fluxes for each of the sites?

The measures of performance are relatively standard so I don't think you need to include the formulas here but just cite a previous reference.

The distribution of residual energy based on the Bowen ratio is not a common practice and the community in general prefers to see the original data being used. This is overwhelmingly important in this environment where there are very large errors in measurements of G and also Rn, both of which go into the available energy term. Errors in these will propagate into errors in the turbulent heat flux terms if you force them based on the bon ratio. Calculating LE as the residual of the surface energy balance equation is even more problematic as it is the sole term carrying all errors in the other terms. I would insist on redoing the analysis using only the original data and not presenting the other methods because they are so error prone.

As mentioned in the summary there is a lot of focus on model error and performance.

However, these comparisons are with often in different types of models in different ecosystems which is like comparing apples and oranges. Most published models will have some reasonable performance but we should move away from a simple reporting of the error to include better and more robust benchmarking of models. For example, this model could be compared against a simple empirical model to assess quantitatively whether the model performs any better than a simple model with local meteorological drivers. Recent papers have started to do and I suggest this is something that you could do to strengthen your paper. For example see:

Whitley, R., Beringer, J., Hutley, L., Abramowitz, G., De Kauwe, M. G., Duursma, R., Evans, B., Haverd, V., Li, L., Ryu, Y., Smith, B., Wang, Y.-P., Williams, M. and Yu, Q.: A model inter-comparison study to examine limiting factors in modelling Australian tropical savannas, Biogeosciences Discuss., 12(23), 18999–19041, doi:10.5194/bgd-12-18999-2015, 2015.

Luo, Y. Q., Randerson, J. T., Abramowitz, G., Bacour, C., Blyth, E., Carvalhais, N., Ciais, P., Dalmonech, D., Fisher, J. B., Fisher, R., Friedlingstein, P., Hibbard, K., Hoffman, F., Huntzinger, D., Jones, C. D., Koven, C., Lawrence, D., Li, D. J., Mahecha, M., Niu, S. L., Norby, R., Piao, S. L., Qi, X., Peylin, P., Prentice, I. C., Riley, W., Reichstein, M., Schwalm, C., Wang, Y. P., Xia, J. Y., Zaehle, S. and Zhou, X. H.: A framework for benchmarking land models, Biogeosciences, 9(10), 3857–3874, doi:10.5194/bg-9-3857-2012, 2012.

Page 14, line 3, the effect of what over the model? Mosses? In addition in this paragraph although you should not use the modus LA it is still consistent with seasonal growth of deciduous shrubs in particular. It is not inconsistent to have a constant fPAR where almost all incoming PAR is absorbed. The Arctic environment is highly adapted to absorbing as much energy as it can. As the leaf area of the shrubs increases during the summer the absorbed PAR is spread out amongst a greater leaf area but the fraction of fPAR remains the same.

Given this is a two layer model where are the results from the canopy and soil components. Do you even need a two layer model? Perhaps evaluate the usefulness of this type of model in this type of environment.

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