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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.

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AC: We would like to thank the reviewer for their insightful comments and suggestions, which we believe have significantly improved the quality of this manuscript as well as our research. We would also like to note to the reviewer that we have accepted the suggestion of the other reviewer to use the Guzinsky et al. (2013) method based on the EVI and NDVI to estimate the fG. According to Fisher et al. 2008, fG is defined by FAPAR / FIPAR where FAPAR is the fraction of PAR absorbed by green vegetation cover and FIPAR the fraction of PAR intercepted by total vegetation cover. Due to a lack of FAPAR observations, we estimated fG using only FIPAR as suggested by Anser (1998) and as the results show this might have caused an overestimation of fG at the

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beginning and at the end of the growing season contributing to model-measurement disagreement. Although this was not a major point according to the reviewer, we have re-run the model using Guzinsky et al. (2013) approach to estimate fG. The new results yielded better model agreement, although it does not provide reliable fG values at the end of the season (mainly in September) and further research needs to address this issue.

RC_1: 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?

AC_1: We agree with the reviewer that the big picture motivation for evaluating TSEB performance over the Arctic tundra was not well described in the original submission, nor was our vision for upscaling to regional coverage. Our motivation is now better described in the final paragraphs of the introduction. In short, the TSEB forms the land surface model in a regional remote sensing energy balance system (ALEXI), used to model energy fluxes and ET from continental to global scales. ALEXI is currently used in NOAA OSPO's GET-D modeling system for North America (http://www.ospo.noaa.gov/Products/land/getd/index.html), and a prototype global modeling system is under development. ALEXI output has been evaluated over CONUS and lower latitude sites in Europe, but has not to date been tested over tundra ecosystems – constituting a significant fraction of the global land cover. Our pri-

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mary goal in this paper is to evaluate TSEB performance over tundra, and to identify refinements that could be incorporated into the regional/global ALEXI system. This motivation is now more clearly outlined in the introduction. We also provide a rationale for investigating a diagnostic flux system, which can be compared in future studies to process-based prognostic model output. Hain et al. (2015) performed a comparison of ALEXI and Noah latent heat flux estimates over CONUS and found the TSEB was able to diagnose missing moisture source/sink processes in the prognostic model (e.g., due to irrigation, shallow groundwater, etc). This motivation for focus on a diagnostic approach is also now provided within the introduction.

AC_1: TSEB has been already compared with other methodologies showing superior performance (e.g., Timmermans et al., 2007; Choi et al., 2009; Tang et al., 2011). This has also been included in the text.

RC_2: 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?

AC_2: Upwelling longwave radiation was computed using TRAD from the four component net radiation sensor and the Apogee IR sensors in each tower. Downwelling longwave radiation computed through Eq. 13 and estimation errors were reported in section 6.2, and showed a RMSE of 26 W.m-2.

RC_3: 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 un-

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derstood 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.

AC_3: In the original TSEB formulation, a simple approach based on the relationship between G and RNS was used (Eq. 13). For continental-to-global applications of the TSEB, we are indeed finding that variations in the main parameters of the G formulation are required – for example over rock or desert sands. However, as is explained in section 3.2 "Refinements in soil heat flux parameterization", here we developed a new simple approach to estimate G based on a phase shift between LST and G to avoid errors using a constant fraction of net radiation over the diurnal cycle. The modifications derived here help to better capture thermal characteristics of the tundra substrate. Moreover, this method also investigates use of new scaling parameters that better reflect the thermal properties of the tundra soils, as noted by the reviewer.

RC_4: 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?

AC_4: A standard deviation has been included in the text for alpha and G values. AC_4: The initial values of the PTC use in this paper were the original value of 1.26 used in other TSEB applications and a value of 0.92 averaged from the main references found in the literature focused on Arctic tundra. As a starting point for the model we consider them applicable for areas of the Arctic with similar vegetation conditions. To clarify this within the text, the following paragraph has been added in section 2: "Under stress conditions, TSEB iteratively reduces ïĄąPTC from its initial value. The TSEB model requires both a solution to the radiative temperature partitioning (Eq. 2) and the energy balance (Eqs. 6 and 7), with physically plausible model solutions for soil and vegetation temperatures and fluxes. Non-physical solutions, such as daytime condensation at the

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soil surface (i.e., LES < 0), can be obtained under conditions of moisture deficiency. This happens because LEC is overestimated in these cases by the Priestley–Taylor parameterization, which describes potential transpiration. The higher LEC leads to a cooler TC and TS must be accordingly larger to satisfy Eq. (7). This drives HS high, and the residual LES from Eq. (11) goes negative. If this condition is encountered by the TSEB scheme, ïĄąPTC is iteratively reduced until LES $\sim$ 0 (expected for a dry soil surface). However there are instances where the vegetation is not transpiring at the potential rate but is not stressed due to its adaption to water and climate conditions (Agam et al., 2010) or the fact that not all the vegetation is green or actively transpiring (Guzinski et al., 2013)."

RC_5: 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.

AC_5: The specific MODIS products used, and treatment of gap-filling and QC flags, are now more completely described in section 4.2.2.

RC_6: 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

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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. Do you even need a two layer model? Perhaps evaluate the usefulness of this type of model in this type of environment.

AC_6: The tundra canopy in the region where we have the tower measurements is dominated by a shrub canopy having an average height of 0.4 m. This overstory is likely to strongly affect the energy exchange and divergence of radiation and wind reaching the moss/lichen surface while the moss/lichen understory will act similar to a "bare soil" surface being aerodynamically smooth. The energy balance of the moss/lichen surface is computed using a 'bare soil" aerodynamic resistance for the sensible heat flux based on the "moss/lichen" temperature derived from Eq. (2), and with net radiation reaching this surface along with the estimated G term, the residual LE would then represent the mosses/lichen water use instead of bare soil. An assumption is that the soil resistance formulation is applicable to the moss/lichen understory. Given that the temperature partitioning derived from Eq. (2) which will yield a moss/lichen substrate temperature, significantly impacts the flux partitioning, using TSEB is assumed to be a reasonable approach for this ecosystem. AC_6: As it was explained at the beginning of the reviews, fG has been estimated using Guzinsky et al. (2013) methodology. This has been clarified in section 2.

RC_7: 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?

AC_7: The treatment of the EC data has now been expanded on in Section 4.2.3.

RC_8: What percentage of data were excluded due to different quality control previously mentioned as well as the three criteria mentioned.

AC_8: The first quality control excluded 20% of the data, accounting for inaccuracies in both meteorological and eddy covariance data. The second filter excluded 52% of the

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Discussion paper

data due to summer rainy conditions in the Arctic. After the precipitation filter, 10% of data was excluded because of a balance closure for 30 min timesteps less than 70%. Finally, to account for daily conditions (Rn > 100 WÂům-2 filter), around 50% of the remaining data was excluded.

RC_9: How were gaps in the data filled and worthy gap filled data used in the analysis?

AC_9: No gap filled data was used in this study; this was clarified in the text. Although gap filled data would have increase the final amount of data to evaluate the model, we preferred to have less data that are more reliable since they were derived from the measurements.

RC_10: 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.

AC_10: As mentioned in section 6.3, "the average energy balance closure using half-hour periods for the evaluation dataset was 88% which is in agreement with the average closure of 90% for these flux stations, (Euskirchen et al., 2012)".

RC_11: 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.

AC_11: We prefer to keep the formulas; we found that sometimes it is useful for the reader to have them in the text to better interpret the results.

RC_12: 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

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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.

AC_12: As explained in the text (section 6.3), lack of closure may be explained by instrument and methodological uncertainties, insufficient estimation of storage terms, unmeasured advective fluxes, landscape scale heterogeneity or instrument spatial representativeness, among others (Lund et al., 2014;Stoy et al., 2013;Foken et al., 2011;Foken, 2008;Wilson et al., 2002). Currently, there is no uniform answer on how to deal with non-closure of the energy balance in eddy covariance datasets, and methods for analyzing the reasons for the lack of closure are still under discussion (Foken et al., 2011). More recently there is evidence that non-orthogonal sonics underestimate vertical velocity causing under-measurement of H and LE on the order of 10% (Kochendorfer et al., 2012; Frank et al., 2013), although this is still being debated (Kochendorfer et al., 2013). This is the reason why in the current study a distribution of residual according to the Bowen ratio (BR) method was applied as suggested by Twine et al. (2000) and Foken (2008). In addition, LE was recalculated as the residual (RES) of the surface energy budget used in previous TSEB evaluations (Li et al., 2008). Foken et al. (2011) concluded that the different footprints of radiation, soil heat flux, and turbulent flux measurements, including the storage terms, which were postulated earlier to be a reason, have no significant influence on the energy balance closure results. In addition, the sonic anemometer and gas analyzer used in this study are Type A instrument have a typical accuracy between 5% and 10% for sensible and latent heat flux estimation, respectively while shortwave radiation and longwave radiation measure with the four components net radiometer have a 1% and 20 WÂům-2 accuracy (Foken, 2008). Additionally, the ground heat flux, including the storage term in the upper soil layer, can be determined with acceptable accuracy under most conditions (Foken, 2008). In our case we have a complete set of instrumentation to estimate G including soil bulk density data at each flux tower site.

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RC_13: 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.

AC_13: This has been clarified in the text. TCAV were placed in the soil at 2 and 4 cm depths.

RC_14: 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?

AC_14: We have used self-calibrating soil heat flux plates. This has been clarified in Table 2. In addition, we have used the calorimetric method using soil bulk density data for each site to account for soil heat storage as it was explained in section 4.2 "Model inputs and evaluation datasets". This method has been also applied for Lund et al. (2014) for tundra conditions.

RC_15: 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.

AC_15: As explained above, we have used self-calibrating soil heat flux plates, TCAV water reflectometers to estimate G. All instruments are placed in the soil and not in

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the moss layer. We have used the calorimetric method using soil bulk density data for each site to account for soil heat storage as it was explained in section 4.2 “Model inputs and evaluation datasets”. This method has been also applied for Lund et al. (2014) for tundra conditions. The soil bulk density was already mentioned in the paper and it is 758 kgÂům-3, 989 kgÂům-3 and 1038 kgÂům-3 for Fen, Tussock and Heath flux stations, respectively.

AC_15: We agree with the reviewer that having more soil heat flux plates, TCAV and water reflectometers will improve the soil heat flux calculation. In table 2 we only listed the instruments but not the number of instruments per site. We have four self-calibrating soil heat flux plates, two water reflectometers and two thermocouple averaging soil temperature probes per flux station. This has been clarified in table 2. Similar instrumentation (same amount of instrumentation) is also used in many FLUXNET sites to address the spatial heterogeneity in the surface the soil.

RC_16: Please provide a thorough estimate of error and uncertainty for this particular important measurement. In addition, what is the uncertainty (random and model) in the fluxes for each of the sites?

AC_16: Soil heat flux model error is reported in detail under Section 6.1

RC_17: 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.

AC_17: In our case, G is a relatively small term compared with other fluxes, and as we explained before, lack of closure is likely to occur due to methodological uncertainties, insufficient estimation of storage terms, etc. when processing eddy covariance data (sensible and latent heat fluxes).

RC_18: 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

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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.

AC_18: Ultimately, this would be a goal for a follow-on paper. This paper focused on the utility of adapting/refining the TSEB land surface scheme for the Arctic tundra region represented by the flux tower sites used in this study. This is the reason we used Kalma et al. (2008) study as a robust benchmark for evaluating the performance of the TSEB relative to a large number of surface energy balance models using land surface temperature. In this paper, methods for estimating evaporation from landscapes, regions and larger geographic extents, with remotely sensed surface temperatures were reviewed, and uncertainties and limitations associated with those estimation methods were highlighted. In addition, particular attention was given to the validation of such approaches against ground based flux measurements. An assessment of some 30

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published validations summarized in Kalma et al (2008) ranging from complex physical and analytical methods to empirical and statistical approaches) indicates a robust model should yield an average root mean square error (RMSE) value of around 50 W m-2 or less in estimated hourly turbulent fluxes H and LE during daytime conditions. The results from the current study yield RMSE values that fall generally below 50 W m-2 and hence considered a robust thermal-based energy balance model for the Arctic tundra.

RC_19: 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.

AC_19: The lack of FAPAR consistency has been addressed in previous comments by using Guzinski et al. (2013) approach.

RC_20: Given this is a two layer model where are the results from the canopy and soil components.

AC_20: Although the TSEB model components the overstory and understory component fluxes, there are no measurements available to evaluate the reliability of the partitioning. This is a project planned for a future study when measurements of the component fluxes are available.

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