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# *Interactive comment on* "Simulating shrubs and their energy and carbon dioxide fluxes in Canada's Low Arctic with the Canadian Land Surface Scheme Including biogeochemical Cycles (CLASSIC)" *by* Gesa Meyer et al.

**Anonymous Referee #1**

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General comments:

The manuscript describes modifications to the Canadian Land Surface Scheme Including biogeochemical Cycles (CLASSIC) model, and it evaluates simulations of surface-atmosphere carbon dioxide (CO2), water, and energy exchange against observations from eddy covariance and carbon flux chambers for a dwarf-shrub tundra site in Canada's Southern Arctic ecozone. The study added new shrub plant functional types (PFTs) to the physical and biogeochemical sub-modules of the CLASSIC model. Additionally, the CLASSIC model was modified to correct for a high ground evaporation bias which affects simulated energy fluxes and water-stress impacts on plant productivity. Simulations of the CLASSIC model were run to estimate annual and seasonal CO2 fluxes, and model estimates were compared for three parameterizations (shrub, tree, and grass PFTs) and evaluated against observations. The ground evaporation bias correction, implemented as a new formulation of the coefficient B (used in the calculation of specific humidity at the surface), improved agreement between simulated and observed values for several variables (e.g., soil moisture in the upper soil layers, soil temperature in surface and deeper soil layers, spring and summer latent heat flux (LE), and CO2 fluxes). When simulations with the new shrub PFTs were compared against simulations with grass or tree PFTs, the shrub simulations had the best agreement with observations for variables such as leaf area index (LAI), soil and litter carbon pools, vegetation height, mean rooting depth, summer soil temperature, and late growing season soil moisture. Notably, the shrub PFT improved simulation of vegetation burial by snow, which impacts net energy fluxes, phenology, and the timing and magnitude of net CO2 fluxes. Together, the evaporation bias correction and new shrub PFTs led to improved agreement with observations considering the magnitudes of both net and component CO2 fluxes. Results demonstrated the importance of incorporating appropriate PFTs in process-based models, particularly in terms of capturing the physical and biogeochemical impacts of shrubs in tundra ecosystems.

The manuscript is well written and well organized, and the lack of tundra-specific PFTs in the CLASSIC model is clearly framed as a problem to be addressed. The presented figures and tables provide clear evidence that the modifications to the CLASSIC model (both the addition of shrub PFTs and the correction of the ground evaporation bias) led to improved estimates of carbon and energy fluxes at the dwarf-shrub tundra site. The manuscript also acknowledged continuing modeling challenges, including overestimated net carbon uptake in the early growing season, overestimated winter CO2 loss, and overestimated evapotranspiration during snowmelt (even with the new B formulation), and it provides suggestions for future model modifications that can address these challenges. The authors might want to consider elaborating further on any significant trends present in the observed or modeled data over the 2004-2017 study period. For example, it would be helpful to report whether there were significant positive trends in growing season vegetation productivity, potentially indicating increases in shrub growth over the simulation period. Additionally, were temporal trends present in annual respiration, soil temperature, active layer depth, growing season length, or other variables? If significant trends are present, do they provide further information about physical or biogeochemical processes occurring at the site over the study period? Overall, the manuscript presents important modifications to the CLASSIC model that help to improve understanding of the drivers of $CO_2$ flux dynamics in shrub-dominated tundra ecosystems.

Specific comments:

Line 42: It might be worthwhile to mention that some emissions observed during the winter could be driven by the diffusion of stored $CO_2$ that was produced during the non-winter period (although the magnitude of the contribution is not known, e.g., Natali et al. 2019).

Lines 101-103: It sounds as if respiration processes Ra and Rh are determined as a function of C pools within the biogeochemical sub-module (i.e., daily timestep). Are daily respiration rates also determined as functions of soil temperature or other inputs? (E.g., Lines 514-515). What determines the base soil respiration rate (e.g., Line 517)? Is Ra estimated as a function of daily carbon assimilation?

Do the model parameters involved with estimating respiration vary by PFT and/or soil layer? It would also be helpful to briefly describe how changes to soil carbon pools are calculated in the different soil layers.

Line 111: Perhaps mention why these particular PFTs were selected to be added to the biogeochemistry sub-module? I.e., cold broadleaf deciduous shrubs, broadleaf evergreen shrubs, and sedges.

Line 117-122: Perhaps move this information to the Introduction section, to better frame the high ground evaporation bias as a problem that is addressed in this paper.

Section 2.1.2: The process for estimating parameter values for the new shrub PFTs seems a little unclear. It sounds as if parameter values developed by Wu et al. (2016) were used as a starting point for parameters in the biogeochemical sub-module. Were parameters further updated based on values published in the literature and ensuring that Equations (7) and (8) are satisfied?

Table 2: Can the parameters be categorized as physics sub-module vs biogeochemical sub-module?

Line 173: What is the area of the study site?

Line 191: Are the eddy covariance measurements of turbulent CO2 flux and energy fluxes collected year-round? E.g., in Figures 4, 5, and 6, the eddy covariance observations are not shown for the winter period. The Table 5 caption states that observations were only available during the growing season. Perhaps this can be clarified in the main text.

Additionally, what is the approximate area of the eddy covariance tower footprint? Is the tower footprint area heterogeneous in terms of vegetation communities and soil wetness/surface water features, and would footprint area dynamics potentially impact measured fluxes?

Line 241: Over what time period are the meteorological observations available? (E.g., 2004-2017?)

Table 5: Do the observations reported here refer to only the eddy covariance tower measurements? When it is stated that observations were only available during the growing season, does this refer to uncertainties with the forced diffusion chambers being measured during only one winter and having limited spatial coverage?

Lines 412-417: Are simulations of GPP, Rh, and Ra all sensitive to the interannual variability in meteorological forcing? Which component fluxes drive the switch to annual net CO2 sinks during certain years?

Line 442: Even with the new B formulation, ground E at the time of snowmelt remained overestimated. Is this based on comparison with field measurements? Is it inferred based on the simulated ponding during and after the snowmelt period?

Line 464: CLASSIC simulated ET to be 50 +/- 22% T for the last week of July 2004-2017 using the shrub PFT simulation with the new B formulation. Does this represent an improvement relative to the original B formulation?

Line 498: Here, do the three methods refer to eddy covariance, chamber, and model simulations?

Line 551 (and Line 410): Is the annual CO2 loss of 17 g C m-2 yr-1 estimated from the combined EC and chamber estimate the NEP value that is not shown in Table 5? Is this value not reported in Table 5 due to the uncertainties related to combining the two observation datasets?

Lines 565-572: Regarding the finding that growing season net CO2 uptake is more sensitive to environmental conditions in the grass PFT, are there differences in the amplitude of the CO2 flux seasonal cycle among the shrub, grass, and tree PFTs? E.g., among the different PFT simulations, does the timing of peaks in Reco and GPP – and any potential mismatch in timing of the Reco and GPP peaks – potentially shed light on changes in amplification of the net CO2 seasonal cycle?

Technical corrections:

Line 30: Need opening single quote at beginning of 'greening' Line 66: 'kind' needs to be pluralized. Line 108: Unclear how 'habits' is being used in this context. Figure 4. Caption: extra parentheses after d Line 19 in Supplement: It's stated that the PFT-dependent parameter Äś ÌĚ is given in Table 4, but is this meant to be Table 2? Line 413: t-test p-value one-sided or two-sided Line 463: t-test p-values one-sided or two-sided

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Discussion paper