

Interactive comment on “The Polar Vegetation Photosynthesis and Respiration Model (PolarVPRM): a parsimonious, satellite data-driven model of high-latitude CO₂ exchange” by K. A. Luus and J. C. Lin

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GENERAL COMMENTS

"The Polar Vegetation Photosynthesis and Respiration Model (PolarVPRM): a parsimonious, satellite data-driven model of high-latitude CO₂ exchange", by Luus and Lin, presents revisions to the Vegetation Photosynthesis and Respiration Model (VPRM) of Mahadevan et al. (2008) seeking to better diagnose North American Arctic atmosphere-ecosystem carbon exchange. The authors address a timely and important topic (arctic carbon exchange) and the article is generally well-written. I have two significant first-order concerns about the study design that I feel should be addressed

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before the article is published. Those are described below, followed by more focused comments.

My first high-level concern concerns the design of the model intercomparison portion of the study. The paper presents the PolarVPRM as an improvement over the VPRM in the high latitudes: "Model intercomparisons indicated that PolarVPRM showed slightly better agreement with eddy covariance observations relative to existing models" (P 980 L 11-12); "PolarVPRM contains a number of important differences in inputs and model structure relative to VPRM, and these allow PolarVPRM to generate accurate estimates of NEE across high-latitude regions." (P 982 L 2-4). Yet the study conspicuously avoids comparing PolarVPRM diagnoses to VPRM diagnoses. I expect the PolarVPRM to provide a better fit to eddy covariance net ecosystem exchange observations than the VPRM solely because it uses more parameters (six, vs. the VPRM's four). In my opinion a quantitative comparison to the VPRM using something like AIC (Aikake, 1976) is needed to justify the additional complexity contributed by those two parameters.

My second top-level concern surrounds the parameter estimation design (section 2.1). The respiration (R) parameter optimization is performed separately from the photosynthesis (GEE) parameter optimization. It seems to me that estimating both subnivean R as well as GEE from observed NEE uses the NEE observations twice, and is therefore likely to produce overly confident parameter estimations. Because NEE is the small difference between two much larger and highly uncertain fluxes (GEE and R), using the NEE observations in this fashion discounts the possibility of equifinality ([low GEE, low R] and [high GEE, high R] could both produce the same NEE). I believe that a joint parameter optimization (see, e.g., Ricciutto et al. (2008), Beer et al. (2010), Hilton et al. (2013)) of the six parameters would be a better approach. In my opinion this experimental design needs to be explained more extensively or revised.

SPECIFIC COMMENTS

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Regarding the statement "Large uncertainties presently exist in model estimates of high-latitude NEE (Fisher et al., 2014), resulting in diverging estimates by process-based models regarding whether North America is a carbon source or sink (Huntzinger et al., 2012)." (P 980 L 26 to P 981 L 3): Huntzinger et al.(2012) place the vast majority of the uncertainty in North American NEP in the United States (see fig 2A), well south of this study's domain. Hilton et al. (2014) diagnose NEE uncertainty similarly (see fig 14) using the VPRM. If most of the uncertainty in NEE is below the USA-Canada border, then how can the net carbon balance of the continent depend on whether the high latitudes are a source or sink?

"...little inter-site variability in parameters (Lorantý et al., 2011) have indicated the tremendous potential that exists for accurate estimates of regional-scale Arctic NEE to be modeled from satellite observations." (P 981 L 6-8): Lorantý et al.(2011) consider arctic tundra, a much smaller domain than this study's domain of north of 55 degrees N latitude. Recent work (e.g. Reichstein et al. (2014)) call into question the ability of plant functional types to categorize model parameters, and Hilton et al.(2013) found little separation of VPRM parameters by PFT. Would a different partitioning for parameterization be better in the non-tundra portions of the North American high latitude (NAHL) domain?

P 982 eq (4): Tmin and Tmax are not defined. Are their definitions the same as Mahadevan (2008)? Are their values taken from literature, and if so, for the same reasons?

P 985 L 20-21: "have heterogeneous distributions in snow accumulation, and in growing season drivers of NEE" – what does "heterogeneous distributions" mean? That different vegetation classes are mutually distinguishable by snow accumulation or drivers of NEE?

"The minimum, optimum, and maximum temperatures for photosynthesis were evaluated from the meteorological and eddy covariance observations gathered at calibration sites." (P 989 L 5-7): Does this refer to Tmin, Tmax, and Topt from equation 4? Ma-

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hadevan et al.(2008) state (section 2.1 paragraph 12) that setting these parameters by optimizing to EC data will be unstable with respect to PAR_0: "Since temperature and PAR are correlated on a daily basis, inclusion of Tscale in equation (5) modifies values of PAR0 inferred from tower flux data. Moreover, were the parameters Tmin, Tmax, and Topt in equation (6) to be fit to eddy flux data along with the respiration equation (below) and PAR0, parameter values would be unstable because of correlation between the parameters; therefore Tmin, Tmax, and Topt were fixed at literature values." Please address this methodology choice.

p 988 L 25: "No gap filling was carried out for any of the EC measurements...": This is an excellent choice in this context.

P 989 L10: "observed GEE": GEE cannot be observed because it is confounded by respiration. Please explain more fully.

P 991 L 20: Does "standard calibration parameters" mean the Mahadevan et al.(2008) values?

P 992 L 19: "Visual examination of these plots": which plots? This will be easier to read if figure numbers are provided explicitly.

section 2.5: The text discusses the figures ("were first examined by plotting total CO2 exchange of high-latitude North America", "Trends over time were examined first for each year and each vegetation class, and then pixel-by-pixel across the entire model domain.") but no figure numbers are provided.

P 995 L20 and figure 2: I am having a hard time interpreting the cumulative biases in units of tons carbon per hectare. Please revise the text and figure to use a more conventional unit such as g C m⁻². This would allow readers to place the values in context with other published studies (e.g. Huntzinger et al.(2012), Beer et al.(2010)).

P 995 L 25-27: "At Atqasuk and Ivotuk, lambda was set to 0.15 and 0.04, respectively. When the optimal values for lambda were calculated for Barrow and Imnavait, values

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of 0.29 and 0.34 were identified.": In figure 1 Atqasuk and Ivotuk are calibration sites and Barrow and Imnavait are validation sites. It seems to me the lambda values should be "calculated" at the calibration sites and "set" at the validation sites. Please clarify this text.

"Furthermore, by calculating snow and growing season respiration separately according to air or soil temperature, accuracy of snow season estimates improved. PolarVPRM estimates of mean three-hourly and monthly NEE were therefore found to be in better agreement with EC NEE than mean three hourly and monthly estimates of NEE generated by CarbonTracker and FLUXNET Multi-Tree Ensemble, respectively." (P 1001 L11-15): This statement is very clear and concise. I found these ideas drowned in detail and thus difficult to glean from the results and discussion section. I think the paper would benefit a lot from a similar summary both in the abstract and early in the results and discussions.

I commend the authors for making their model results and code publicly available (P 1002 L 5-7) . This is important and still all too rare.

TECHNICAL CORRECTIONS

p 983 L 22: The abbreviation "NAHL" is used a number of times but not defined until p 988 L 9.

p 982 L 3-4: "these allow PolarVPRM to generate accurate estimates" should be changed to "these allow PolarVPRM to generate more accurate estimates"

p 991 L 11: EC "observation" should be "observations"

Table 2: Shortwave radiation is listed twice, once with units of Kelvins and once with units of W m⁻². Maybe the second entry should be soil T, not radiation?

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