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Interactive Comment

Interactive comment on "Validating a 1-D SVAT model in a range of USA and Australian ecosystems: evidence towards its use as a tool to study Earth's system interactions" *by* G. P. Petropoulos et al.

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R1C1: The introduction could be greatly shortened. The authors give a motivation for the study of land-atmosphere interactions, as well as a history of land surface modeling, neither of which are necessary. Basically the last paragraph of the introduction (p2444) would suffice. It describes the paper's objectives and the methodology used to meet those objectives. The previous few pages are mainly superfluous. ANS: We shortened the introduction as requested by the reviewer above, although we have kept some reference to the motivation for the study of land-atmosphere interactions and previous

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works on the model to provide some background to the work undertaken.

R1C2:The authors clearly describe the metrics they are using in section 4.3, but none of the metrics are relative, which I found an impediment to judging the quality of simulations. Specifically, what does a mean bias error of say 50 W/m2 imply? If the mean energy flux is 500 W/m2, then perhaps that is a 'good' simulation; if the mean is 50 W/m2, it is likely not. Without a reference to the actual observed value, the reader cannot determine this. For example, in section 5.3, the authors state that because the RMSD was the lowest of the examined fluxes, and the average mean bias error is 2.84 W/m2, latent heat (LE) was well reproduced. However, an examination of figure 6 shows LE to exhibit quite a lot of scatter compared to some of the other variables. In addition, most of the values are 150 W/m2 or less, indicating that the RMSD of _40 W/m2 is quite significant. This undermines statements such as "The model showed excellent precision in reproducing daily trends of LE fluxes in most sites evaluated" ANS: We appreciate the reviewers point, and to address it we have estimated a few additional statistics which we have added to the updated tables of our revised manuscript. We have also added comments in the main text of our manuscript in different sections (results, discussion, conclusions) related to the new information. By including information on the daily average observed and modelled mean energy fluxes for use as reference, we have tried to remove subjective assessments of accuracy and based our interpretation of the results on more objective statements. We believe that it is now much easier for someone to appreciate the discrepancies between the model predictions and the corresponding in-situ for the different days on which the model was evaluated.

R1C3: Some statements were confusing: p 2454 "A systematic underestimations of Rnet was evident, leading to an overall satisfactory agreement between the model predictions and in situ observations"; why is a systematic underestimation considered satisfactory? ANS: We have amended the sentence to reflect essentially that a constant or systematic and more pronounced underestimation by the model leads to poorer agreement with the in-situ data over this site. We hope is clearly explained now.

R1C4: End of section 5.1, the authors note that larger errors for Oz sites occur feb-june, while the converse is true in the Ameriflux sites; the authors may wish to add that these time periods correspond to summer for each region, and are therefore consistent. ANS: We have added the required information to this section as per reviewer request.

R1C5: The paper contains no figures showing actual simulated fluxes. This would helpful in understanding the characteristics of the errors, as well as the observations. ANS: We thank the reviewer for their comment; we have now added a figure showing an example of two days of simulated and observed fluxes for as well as information on the RMSD in relation to the percentage of observed fluxes (e.g. RMSD for LE is within 10% of the observed fluxes) in each table. We have also made reference to the trends seen in the discussion section.

Interactive comment on Geosci. Model Dev. Discuss., 8, 2437, 2015.

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