

Interactive comment on “The potential of observed soil moisture dynamics for predicting summer evapotranspiration in a successional chronosequence” by J. A. Breña Naranjo et al.

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We appreciate the reviewer’s comments, particularly the insight into the question of robustness of the approach to different stages of forest succession, which adds to the other two reviewers comments. As an editor decision has already been made, we keep this online reply fairly short and general, replying mainly to your points no. 1 and 5 and will address all detailed comments about data and model clarifications in the revision.

The reviewer finds the question of sensitivity to different successional stages somewhat contrived. We may not have made this clear enough, however, it is more the other way

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around: known AET-succession relations created the incentive for this study and hence the approach should be tested in a forest succession. The three Campbell River sites to the best of our knowledge are the only ones providing the data for that.

We think that in the many de- and reforested regions around the world where no flux tower data is available (and hence no such accurate measurements for AET), but where AET nevertheless is an important water cycle component that is changing with succession, an easier-to-measure soil moisture data driven model might be most useful, in particular if the watershed is comprised of many differently aged forest stands and installing several eddy-flux towers is impossible.

As for the model itself: somewhat similar model applications as cited in the manuscript are already documented in the literature, and while this study adds to those by using longer series, the main new aspect was to see how it works along a succession. However, we would like to emphasize here that the aim was not to detect or explain the physiological reasons for AET changes during succession, which undoubtedly would require additional data about the sites and a different approach. Our phrasing may have been misleading. We will improve the introduction and aims to clarify this.

We agree that we should also reconsider the results in this respect (as criticized in comments 3 and 4) and will do that based on the updated results on a more substantive sensitivity analysis (see comments to other reviewers).

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