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Interactive comment on "Technical Note: Linking climate change and downed woody debris decomposition across forests of the eastern United States" *by* M. B. Russell et al.

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The paper uses a combination of models, i.e. modelling of downed woody debris decomposition rates and modelling of future climate scenarios, to forecast future changes in deadwood residence time with insights on its repercussions on forests carbon balance.

In general, with an increase in temperature and precipitation a more rapid decomposition of deadwood is expected in the study area, as in most temperate forests, with cascading effects on deadwood dynamics.

Modelling decomposition rates in forest sites is per se extremely challenging. The first

author already faced this topic in a previous paper (Russell et al., 2013 – Ecological Modelling), and I guess he knows that it is difficult to monitor the in situ decay process for whose modelling a high degree of uncertainty remains. Indeed, even adopting broad decay classes, a model specifically developed by the author for decay class transitions predicted the correct decay class observed after five years in approximately 50-70% of the observation. We know that a high degree of uncertainty affects also climate change scenarios.

Based on these premises, it is clear that the degree of approximation that may affect the combination of two models with such a high degree of complexity can only be used to draw very general conclusions, rather than a quantification of a specific process.

On top of these general observations on the unfeasibility of the paper aims, especially of the second one (forecast ecosystem-level C-flux for DWD using the static and dynamic climate scenarios), several of the components of the models used in the paper have intrinsic approximation or are coarsely described leaving room for doubts. For instance the climate data and the climatic scenarios are based on two references of the western U.S. (Rehfeldt, 2006; USDA, 2014) that are used to model climate and climate change for the Eastern United States. This incongruence is never even mentioned in the paper, nor the use of such data for the eastern U.S. is justified anyway. I have further doubts on the synthesis of climate and climate change based on a single variable (i.e. the number of degree days greater than 5 C°), moreover the selection of this single variable for the purposes of the paper is never motivated if not by the fact that "projected changes in DD5 were more apparent compared to precipitation variables" (page 9020, line 12), therefore, based on my understanding, the authors deliberately chose the variable that would have resulted in the higher variation in their future predictions. Also the use of the length of woody pieces rather than their diameter is somehow puzzling. Indeed the paper on the effect of plant traits that is cited by the authors (Cornwell et al., 2009) states that "Log size is known to have a negative effect on decomposition rates (Mackensen et al., 2003; Janisch et al., 2005)". I suggest that the authors consider this references and that accurately explain their choice of neglecting deadwood piece diameter in favor of their length. I see this may derive from the work carried out in Russell et al., 2013 but also in that paper the choice of not using diameter variables is not fully explained. I report here the sentence that should motivate the variables selection: “As a measure of decomposition potential across the study plots, the number of degree days greater than 5 C° (DD5), coupled with the length of the DWD piece (LEN; m) and DC as measured at T1, were used to estimate the DWD DC transitions for the M data. Incorporating additional climate variables into the modeling framework (e.g., growing season precipitation, length of frost-free period, mean annual temperature/precipitation) and various measures of DWD piece size (e.g., large-end diameter, combined variable of large-end diameter squared multiplied by length) did not reduce Akaike’s information criteria and log-likelihood values.” I am not familiar with the type of model that was used but the text suggests that DD5 and deadwood piece length were used in the initial model, whereas the other variables were only used to check if their contribute would have substantially modified the previous model. I do not understand from this methodological description if using diameter variables from the beginning would have resulted in a different model. Finally the second aim is pursued not taking into account deadwood inputs and this strongly limits the ability to model deadwood dynamics ad related carbon fluxes.

In general I think that combining two models with a high degree of uncertainty and based on partial and approximate data does not allow for an actual quantification of ecological processes. Coming to the conclusions drawn in the paper, personally I do not agree with the authors on the need for a model that combines the two models used in this paper with further models of tree growth and mortality (pag. 9023, lines 19-22). Doing so further approximation would be added, unless models are used which are derived from accurate, even if more local, datasets that may give insights on the actual ecosystem processes rather than on broad scale approximations.

Please consider the following references: Janisch JE, Harmon ME, Chen H, Fasth

B, Sexton J (2005) Decomposition of coarse woody debris originating by clearcutting of an old-growth conifer forest. Ecoscience, 12, 151–160. Mackensen J, Bauhus J, Webber E (2003) Decomposition rates of coarse woody debris – a review with particular emphasis on Australian tree species. Australian Journal of Botany, 51, 27–37.

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