I. Anonymous Referee #1

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General comments

This short ms describes a simulation experiment using U.S. Forest Inventory and Analysis data, to examine how residence time of downed woody debris (DWD) might change in the future under various climate and forest type scenarios. This is a very limited analysis, but potentially useful, given the importance of DWD for wildlife, management, carbon cycling, etc. The ms is well written and topic appropriate for Biogeosciences.

- *We thank the reviewer for their comments and insight. We agree that this work has broad applicability in wildlife habitat management, carbon cycling, and fire ecology.*

I have three general concerns. First, the authors' methods and conclusions seem to be seriously called into question by the just-published Bradford et al., "Climate fails to predict wood decomposition at regional scales" in Nature Climate Change (see DOI below). I'm sure the authors will want to cite/discuss/compare with this publication, which finds that only with aggregated data (as here) does climate control DWD decomposition. This seems to have obvious implications for Russell et al.'s assumption that temperature is the primary mechanism controlling future DWD decomposition.

- *We cite and include discussion of the Bradford et al. study in our subsequent revision (Introduction, Results, and Discussion sections). Local-scale factors (e.g., termite biomass and fungal colonization) no doubt play an extremely important role in determining wood decomposition rates. However, in the absence of such information collected at broad regional scales (such as the US Forest Service's Forest Inventory and Analysis database), the use of climate information to inform decomposition patterns is the focus of this technical note. As our emphasis is related to climate, we are unaware of any approaches that consider future global change scenarios and their impacts on the distribution and abundance of such local scale factors. Incorporating coarse estimates of local scale factors and their associated dynamics would no doubt increase the uncertainty of our estimates in changes in residence time compared to changes based solely on climate regime.*

Second, a number of the methodological details need to be clarified. See below.

- *We clarify these details raised by the reviewer, as noted below.*

Third, I'm not sure this is novel, significant, or sophisticated enough for publication in Biogeosciences. To say that residence time of DWD will probably decrease in a warmer climate: : :probably true, but I don't know if this fairly simplistic analysis really makes that case.

- *We agree that the residence time of DWD will decrease in a warming climate is a scientifically plausible statement. The value in this analysis is quantifying the rates of decrease in the residence time of DWD and the implications for ecosystem dynamics, which to our knowledge has not been reported in the literature in this region or in others across the world. For example, Table 3 and Figure 2 quantify projected decreases in DWD longevity in terms of years, which can be included in various ecosystem simulation models. Figure 3 assesses the impact of these decreases in dead wood longevity in* *terms of C flux, allowing a broader assessment of the influence of global changes scenarios on the temporal dynamics of the deadwood C pool.*

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Specific comments

1. Page 9014, lines 1-4: poor grammar, reword

- *We reworded the sentence in the abstract: "Forest carbon (C) is stored through photosynthesis and released via decomposition and combustion."*

2. P. 9015, l. 19-20: by definition, no?

- *We reworded the sentence in the Introduction: "Increased rates of decomposition will likely reduce the duration that woody debris is available for dead wood-dependent organisms."*

3. P. 9015, l. 29: why is transient responses quoted?

- *We removed the quotations.*

4. Equation 1: Vol is initial volume?

- *Correct. We confirm this is initial volume in the fourth paragraph of the 'Analyses' section.*

5. P. 9018, l. 17: what is a cumulative link mixed model?

- *We provide a brief description of the model with a citation: "Cumulative link models (CLMs) are a type of ordinal regression model in which response variables are considered categorical or ordered (Agresti 2007)."*

6. P. 9020, l. 19-21: what about Bradford et al. just published, 10.1038/CLIMATE2251?

- *We reworded this statement: "Hence, in the absence of local-scale factors to use as a surrogate for decomposition (e.g., Bradford et al., 2014), employing temperature differences under future climate scenarios may be used to at least in part to explain DWD flux across the eastern US."*

7. Figure 2: maybe not the best way to display these data; most of the plots are empty space

- *We have instead presented this figure in a table to show the values of the changes in residence times of these data. This now appears as Table 3, and reference to subsequent tables and figures have been adjusted accordingly.*

II. S. Burrascano (Referee)

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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.

- *We thank the reviewer for their comments and insight. There is no doubt tremendous uncertainty derived from two aspects of our analyses (1) statistical uncertainty resulting from the decay class transition model predictions and (2) climate uncertainty from the chosen RCP scenarios. We do feel that data from the national forest inventory are the best to address this question in the US because of (1) the numerous species occurring in the region across a range of forest types, (2) the range in DWD piece sizes measured, and (3) the variability in climate as one transitions from southern to northern latitudes, and (4) line intercept sampling and decay class designation for DWD in the US inventory is similar to other countries, particularly in North America.*

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.

- *We agree that modeling decomposition rates (in addition to projecting changes to climate) is challenging given the spatial and temporal variability in the dead wood decomposition process. Despite the statistical variability, the modeling analyses conducted in the manuscript may lend to assessments of general differences in downed woody debris decomposition, e.g., the decrease in DWD residence time measured in number of years for a general species group (i.e., hardwoods vs. conifers) in a broad geographic region (i.e., northern vs southern eastern US).*

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.

- *We realized that the citation was unclear for the climate data access page we used. Indeed, the climate data portal also includes the ability to access climate information from the eastern US. We have updated the citation to a more appropriate webpage where we obtained the climate data for these sites. (US Forest Service 2014 citation, “Custom climate data requests” page). Although the Rehfeldt 2006 citation refers to the western US, we wish to retain this citation as it reflects the methodology and techniques used for obtaining climate data for the eastern US states (i.e., the thin-plate splining techniques).*
- *Data from the eastern US are employed because remeasurement data are available in the region which have informed previous DWD modeling efforts. Remeasurement data are not yet available for all western US states. We clarify this is the second sentence in the ‘Study area’ section.*
- *As Figure 1 indicates, the greatest percent difference in climate parameters is associated with temperature (using the number of degree days as a surrogate) as opposed to moisture (as reflected in mean annual precipitation). To enable the use of*

the developed DC transition models and the finding that moisture-related variables increase only modestly when compared the temperature increases (~7% as indicated in the last paragraph of the "Future climate" section), the use of DD5 as an explanatory variable is implemented throughout this analysis.

- *We support the implementation of DWD length as opposed to log diameter in the development of the decay class transition models. We expand on this in the 'Analyses' section following the presentation of equation 2: "The finding that LEN was a more effective predictor of decomposition than log diameter in these DC transition models is consistent with other studies that suggested a lack of a consistent relationship between log diameter and woody debris decomposition (e.g., Harmon et al., 1987; Radtke et al., 2009)." The reviewer points out the results of some of our statistical tests from our previous published paper, which states that using length alone was appropriate for the development of the model. Hence, we continue to rely on a parsimonious model that seeks to depict DWD decomposition patterns that uses the published models.*
- *We agree that considering future dead wood inputs (i.e., Figure 3) would provide a more realistic assessment of carbon fluxes associated with downed woody debris. However, we hesitate to forecast dead wood inputs as this approach would add to the uncertainty already inherent to the decay class transition model and climate change scenarios. Not including future inputs does not limit the performance of our developed decay class transition model, despite the fact that Figure 3 analyzes a static downed dead wood C pool. We favor presenting the general relationships between DWD decomposition and climate and allow readers to infer how the parameters might be used in specific ecosystem simulation models to project future downed dead wood C stocks.*

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.

- *Although we consider woody debris separate from other ecosystem simulation model components, we disagree that the woody debris pool is not linked to tree growth and mortality components within a modeling framework. For example, low-growth trees are more likely to suffer mortality compared to trees growing in more favorable conditions, and these mortality trees will ultimately be direct inputs into the standing and downed woody debris pools.*

- *We agree that more local examinations are needed to validate results from the developed models, particularly as local-scale factors contribute to determining wood decomposition rates (as brought up by Reviewer 1). Until such long-term experiments emerge across the region, insights from the US' national forest inventory may be the most appropriate data for developing and testing dead wood models that seek to forecast forest C dynamics under future global change scenarios.*

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.

- *We include the Janisch et al. and Mackensen et al. references in the third sentence of the 'Analyses' section.*

III. Other additions by the authors

- *We include an Acknowledgements section.*