

Interactive comment on “Effects of land management on large trees and carbon stocks” by P. E. Kauppi et al.

P. E. Kauppi et al.

pekka.kauppi@helsinki.fi

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Response by authors to Referee 1

Referee #1 presented valuable points on the manuscript. This helped us further to improve the study. We show that that the relative contribution of large versus small trees varies spatially and temporally. We disagree strongly with Referee #1 that “Finnish and US regions would not be sufficiently large and diverse”. Even the smallest region covers 11.3 million hectares and is diverse. Each study regions consists of hundreds of thousands of individual forest stands, with large diversity in terms of climate, soil conditions, tree species, tree density and age. We show distinct differences in the pop-

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ulations of large trees between the five regions. There are undisputable differences between the regions in their forest history. The change of the stock large trees is not correlated with that of woody biomass as the whole. We argue that the trends differ between regions, because the tree population have responded to forest management. We make the novel point that the presence of large trees is responsive to management decisions. This is an important conclusion with policy implications. We agree with the comment that Discussion and Conclusions must be separated. We removed the reference to Nabuurs et al. (2013), which discussed the limited persistence of forest carbon sinks. Our focus is on the relative role and drivers of large trees as the stock evolves over time. Our conclusions imply that sequestration has persisted and can persist for a long time, but this is not our main point. Therefore, we now concentrate on the main results of ours, which are important and novel. We show that big trees have made a large contribution to biomass expansion in four regions. The evolution and presence of large trees is determined by the management history as opposed to other conceivable drivers such as climate change, nitrogen deposition or CO2 fertilization. Our results indicate regional differences in the evolution of large trees over a long period of time. We agree that “trend detection in a smaller less diverse region is more likely than trend detection in a large diverse region”. However, we find this comment irrelevant in this context, where all regions are large and diverse. It is key message that the regions differ in terms of their forest trends, because the management history is not unanimous. “The reasoning behind the comparison of Finland and the US should be justified and discussed in the manuscript”. Thank you for this comment, we have added text. We agree with the comment that the threshold values which define “large” trees are arbitrary. We used size cohort thresholds as they have been reported in the original statistics, changing only the American inch metrics to SI units. This is transparent and is valuable for future studies, as the trends can be easily followed when new data becomes published. We added text on this. Comment regarding p. 2743 line 11, we disagree (see above). Comment regarding p. 2743 lines 13-24 is relevant as Reviewer 1 admits. The section was misplaced in Discussion. We have moved the section into

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Introduction. Thank you for this comment. This prompted further changes in the organization of text between sections. Comment regarding p. 2743 lines 27-28. We added a reference. Our formulation “. . . may be offset. . .” (p. 2743 line 26) shows that it is not our purpose in this research to assess, whether the model results of Betts (2000) and Bonan (2008) and Swann et al. (2010) can survive an empirical test. Comment regarding Page 2744 lines 19-28 is inappropriate. Management impacts are clearly given in this section, and there is a large literature on this including a paper published in Science and received very well in the community. The literature supports the view that management affects forests. It is inconceivable that it would not do so. Using empirical material we show in this paper that management affects specifically the presence of large trees. This is logical, of course, but has not been quantified in earlier research. This is a critical and novel section of our research. Comment regarding Page 2745 lines 1-14. We accept the criticism and remove this section as it merely gives the specific background of land management history for this area in northern Europe and is not crucial for understanding the results. Comment regarding Page 2745 lines 19. We replace word “significant” by word “large”. Comment regarding Page 2745 lines 25-27. Unfortunately, harvest statistics are not available broken down by stem size cohorts. Moreover, harvest statistics are far less reliable than those on the growing stock. This is no major drawback, because stock statistics, which we show contain the sufficient information for drawing the conclusions of our paper. Comments regarding Tables and Figures are valuable and we have taken them into account along with similar comments by the other Reviewer and have improved the tables and figures.

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