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Biogeosciences Discussions

Interactive comment on "Implications of albedo changes following afforestation on the benefits of forests as carbon sinks" *by* M. U. F. Kirschbaum et al.

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Response to reviewer 1

We are pleased with the overall favourable assessment of the reviewer. We also appreciate that the reviewer considers the manuscript to be well-written and have made a few further changes to the text with which it would hopefully be assessed even more favourably. There were just a few specific issues noted by the reviewer, and we have responded them below. The reviewer pointed out that afforestation need not be from pasture. This is obviously correct but our specific study dealt with the case of afforestation of pasture. Hence, we believe that it is more appropriate to retain the statement as

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given as being more appropriate in the context of our specific work.

The reviewer also elaborates on the work of Juang et al. (which we cited), and we have checked that Section and the paper by Juang et al. (2007) again and made some modifications to more accurately reflect the finding of Juang et al. (2007). The reviewer points out that only it is only under steady-state conditions that net radiation is zero. We agree that our statement was not strictly correct and have omitted the reference to net radiation being zero as that is not required for our calculations, and with its omission, it should be scientifically correct as well.

The reviewer noted that we stated that evapotranspiration is similar for pastures and pines and asked for supporting evidence. In the context of our paper, the important aspect is whether total latent heat flux is different between forest and pasture, and we have added two extra citations, a general one, and one giving measurements from our experimental forest, in support of that important key difference. We have instead omitted the statement about similar evapotranspiration in forests and pastures.

The reviewer also asked about studies of the carbon balance of this stand. One of our co-authors (Peter Beets) has been intimately involved in measuring the growth of this stand since its original planting, and two papers that detail the work have been cited in our paper. The numbers are, therefore, well supported by empirical observations. The reviewer may have also slightly mis-understood that section in that the decaying thinning constitute several years' of growth so that it is not too surprising that the carbon loss from this accumulated total can match even rapidly growing new growth for a few years. We believe that our statement is correct, but have added some extra words to clarify that the 'growth' referred to referred to the growth in total carbon stocks (rather than then growth of individual trees).

The reviewer also asked about the life cycle of the wood, and what that might contribute over time to the global atmospheric CO2 increase. The life cycle of wood, and its contribution to global carbon stocks, is a very important research and policy question,

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8, C4629–C4631, 2011

but outside the scope of the current work. We therefore do not think that it would be appropriate to add a few speculative and incomplete statements in this context. The reviewer also asked about changes in carbon stocks in the soil, and states that a “full carbon balance should include the net ecosystem exchange of carbon rather than the stand biomass”.

It is generally observed that soil carbon decreases after planting pines on former pasture sites (Guo and Gifford, 2002; Paul et al., 2002; Kirschbaum et al., 2008). However, the loss is generally small (i.e. a loss of less than 10 tC ha-1 vs a gain of several hundred tC ha-1 - see our Fig. 3a). Measurements at Puruki itself suggest an even smaller soil carbon loss of only about 4 tC ha-1 by the end of the rotation (Beets et al., 2002). It is difficult to quantify such changes with great confidence for any specific stand because variability in soil carbon measurements within any stand is generally greater than the change due to reforestation. Inclusion of the 4 tC ha-1 loss observed at Puruki would have reduced our carbon sequestration numbers by about 2%. Hence, because of its small importance and uncertainty about the actual magnitude of change, we did not include soil carbon changes in the calculations for our experimental site.

References:

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Interactive comment on Biogeosciences Discuss., 8, 8563, 2011.