

Interactive comment on “Accounting for multiple forcing factors and product substitution enforces the cooling effect of boreal forests” by Eero Nikinmaa et al.

Anonymous Referee #1

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In “Accounting for multiple forcing factors and product substitution enforces the cooling effect of boreal forests”, the authors aim to assess the “climate change mitigating effect of boreal forest management” (L20, Abstract) of four different stand types in Finland by comparing the radiative forcing (RF) caused by CO₂ in forests and wood products, surface albedo, secondary organic aerosols (SOA), and product substitution for two scenarios: recommended forest management practices vs. a counterfactual bare land. The authors also contrast the results obtained under the current climate and a projected 2050 climate. The subject is timely and relevant. Unfortunately, as explained below, the study design is flawed, many methodological approaches are too crude to sufficiently trust the results, and the Methods lack clarity and explanations.

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1. Study design. The main objective of the study is to estimate the RF due to boreal forest management (as noted in the first sentences of the Abstract and Discussion); consequently, the counterfactual scenario must be unmanaged forest for each of the four stand types studied, instead of a hypothetical bare land. Under the current approach, readers are misled into thinking that the results provided in Table 4 and Figures 4-6 correspond to the RF due to forest management, which is not the case. The Editor made a similar comment previously, to which the authors responded they were interested in comparing different stand types. Besides the misleading Table and Figures abovementioned, this response is inadequate because: 1) forest managers cannot realistically choose to transform one stand type into another (e.g. transform a mesic Norway spruce stand into a sub-xeric Scots pine stand); and 2) the indirect aerosol forcing is highly non-linear, which implies that the counterfactual scenario matters for the difference in SOA effect across stand types. Finally, even if the bare land counterfactual was appropriate for some reasons I am not grasping, it was not implemented properly in the study. First, the emissions of dust aerosols would be much higher for bare land than for forest and the resulting large RF could very well be larger than all the effects currently considered. Second, the GHG implications of the activities required to maintain the forest as bare land (periodic mechanical/chemical treatment, fate of the original forest carbon stocks, etc.) would also need to be considered.

2. Methodological approaches. First, the authors should have used a proper dynamic aerosol-climate model to compute the SOA RF. The approach used to compute the direct RF can only provide “[a]n order-of-magnitude estimate of the strength” (Paasonen et al., 2013) of the effect. Similarly, the approach of Kurten et al. (2003) used to compute the indirect RF is much too crude compared with state-of-the-art methods and only accounts for the ‘cloud albedo’ effect (this last shortcoming is not clearly mentioned). Moreover, the simulation setting (location(s) considered, input data, etc.) is unclear, but I doubt very much that these computations were site-specific across Finland – a major limitation given that “estimated emissions are highly dependent on meteorological factors (in particular temperature and light)” (L222-223), hence are highly site-specific.

Second, the approach for surface albedo also does not pass muster. Changes in albedo were estimated “for an area located in central Finland” (L185) instead of being estimated across the region studied; these changes were then “assumed to follow a stepwise function during the total rotation” (L192), as visible in Figure 4a,b (yellow curve), which is much too simplified compared with published results (e.g. Figures 1-2 of Amiro et al., 2006). Third, the results for the 2050 climate were computed for an unrealistic instantaneous change (“for the mean climate [...] for the year 2050”; L157-159) instead of for a realistic transient climate change.

3. Methods. The explanations provided in the Methods do not allow readers to understand how the study was performed and what the results really mean; here are only some examples. Unclear explanations: was the MOTTI stand-level forest model run across all stands (how many?) across Finland, only for the 12 combinations of stand type and site fertility, or for something else? Not enough details: how were the “0.913, 0.905, and 0.819, for Scots pine, Norway spruce, and silver birch, respectively” (L177) displacement factors obtained from the Sathre and O’Connor (2010) results exactly? Missing justifications: the 0.695 displacement factor for pulpwood based on Pingoud et al. (2010) seems to implicitly assume that 50% of the energy displaced is from coal and 50% from natural gas (their Table 2); why is such an assumption valid here? (In the first place, having displaced emissions from pulpwood should also have been explained; this rests upon a link Pingoud et al. (2010) simply assumed between pulpwood and bioenergy.) Elements that are not mentioned at all: what are the displacement factors for the other two categories appearing in Table 2 of the study, i.e. plywood and “process energy” (same as bioenergy, I assume)? Elements the authors apparently did not think about, but that should at least be acknowledged as limitations: not accounting for the methane emissions from products ending up in landfills or for the aerosols emitted from bioenergy.

References

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