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Interactive comment on "Stand age and species composition effects on surface albedo in a mixedwood boreal forest" *by* Mohammad Abdul Halim et al.

Anonymous Referee #2

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By combining literature data with novel in-situ measurements and via chronosequencing, the study by Halim et al. analyzes temporal trajectories in surface albedo following harvest and fire disturbances in southern boreal mixedwood forests. The main findings are that i): winter and spring surface albedos following harvest disturbances are higher than those following fire disturbance; that ii) both winter and summer surface albedos "saturate" at around 50 years, and that iii) successional changes in species composition are a key driver of post-disturbance albedo dynamics.

I have several major concerns about the study methods that call to question these findings. My first and largest concern surrounds the extensive use of albedo data sourced

from the literature (referred to as "secondary data") which are connected to sites located hundreds to thousands of kilometers away. Although the dominant species compositions across sites may be similar, stand structure and other important site-specific attributes affecting the surface albedo may differ greatly across sites. These include differences in geology and soils (affecting albedo via their controls over understory vegetation compositions, soil moisture retention, growth rates), differences in latitude (affecting the direct albedo component via differences in solar geometry), and – most importantly – differences in local climate (affecting albedo via controls over soil moisture, vegetation growth and phenology, length of snow season, and important snow physical attributes such as snow depths, snow age, snow water contents). Without controlling for differences in these important site-specific factors it is difficult to arrive at robust conclusions regarding albedo-age dynamics, albedo-species composition dynamics, albedo-canopy height dynamics, and albedo-ground cover dynamics. Regarding the albedo-age dynamic, for instance, asymptotes of the presented exponential models in Figure 4 seem to be heavily influenced by the "secondary" data comprising all data points beyond 19 years. Regarding the albedo-species composition dynamic, the "secondary" data points in Figure 6 for "Summer" and "Winter" seem to be heavily influencing the y-intercepts and thus affecting the model functional form and shape parameters. Secondary data points in Figure 7 also appear to heavily influence the model fits (or lack thereof) for the "Summer" albedo-canopy height dynamics.

A second methodological concern which is also related to the augmentation of the in-situ sample with literature ("secondary") data is the difference in the definition of albedo. Much of the secondary albedo data are for a broader spectral range (e.g., 295-2800 nm) than what is measured in-situÂň at the authors' own study sites (i.e., 300-1100 nm). This is important given the high albedo of vegetation in spectra above 1000 nm and given the sensitivity of the shortwave near-infrared broad band (1300-2500 nm) to differences in boreal tree species (see Hovi et al. 2017 → https://doi.org/10.14214/sf.7753).

I also have some concern about the study's scientific value, irrespective of my concerns about the methods. None of the three major findings listed above are novel and can be distilled from a diligent review of the boreal forest albedo literature (e.g., post-fire: Lyons et al. 2008; Randerson et al. 2006; Amiro et al. 2006b; Liu et al. 2005; Wang et al. 2016 → https://www.sciencedirect.com/science/article/pii/S0034425716300888 post-harvest: Kuusinen et al. 2016; Kuusinen et al. 2014; Bright et al. 2013; Hu et al. 2018 → https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018MS001403).

Further, the study is motivated by the need to "improve climate model parameterizations" but the authors have made no attempt to explain how their results can/will achieve this. How will the presented statistical functions or empirical insights be applied in a climate modeling context, either for improving existing parameterizations in a climate model directly or for use as a climate model benchmarking/evaluation tool? Albedo parameterizations in most climate models are process-oriented and intimately tied to important forest structural attributes like leaf area index which the authors have not included. Model parameterizations are also largely oriented around important local meteorological state variables (i.e., near surface air temperatures, wind speeds, precipitation type and frequency, snow depth, etc.) which are absent in the paper. This makes it difficult to discern the conditions under which the reported findings may be applied to evaluate climate model predictions. Further, since the reported albedo dynamics for the post-harvest case are intimately connected to the specific management practices of the study region, without providing any detail about the prevailing management regime(s) of the study region it will be difficult for modelers to assess accuracy of simulated post-harvest albedo dynamics. As for the post-fire case, the finding that the near-term (< 25 yr) increases in summertime albedo are connected to pioneer birch succession (a finding reported in several of the references listed above) implies that any "improvement" to the albedo prediction capability of a climate model would need to target the vegetation dynamics routines of the model and not necessarily the "albedo parameterization" itself.

Given my concerns about the study's methods and low scientific significance, I find it difficult to recommend publication in BG. I also find it difficult to encourage a major revision involving a new analysis that excludes the use of "secondary" data given the limited number of field plots and given the narrow spectral band of albedo data that has been measured at those plots.

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