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# *Interactive comment on* "Summertime canopy albedo is sensitive to forest thinning" *by* J. Otto et al.

**Anonymous Referee #1**

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General comments:

The authors combined field data and simulations to quantify the effects of forest thinning on forest canopy albedo in summer, clear sky conditions. The topic falls in the scope of Biogeosciences. The motivation of this manuscript is sound and interesting.

I have several major concerns as follows:

1. This study was tested only under clear sky day in summer. This is a great limitation when applying this study over longer period. In forest regions in Europe, owing to transpiration, there should be clouds during summer. Clouds could be thin or thick, sparse or frequent. Whatever, entirely clear sky days are unlikely to represent summer in European forests. Albedo could substantially vary with cloudiness. For example,



see Fig 7 (g) and (h) in below paper. In mid-day, NIR reflectance differs by 0.1 between clear and cloudy days (note difference in albedo between thinning and non-thinning in this study was only 0.02). I believe this manuscript will be substantially improved once including simulations on cloudy days. I feel very simple condition would be enough such as thin, homogenous clouds which lead isotrophic incoming radiation from sky to the forest (i.e. white-sky albedo). As actual albedo is between white-sky and black-sky albedos, it will be valuable if providing impacts of thinning on these two extreme cases (black and white sky albedos).

Ryu et al., (2010). Testing the performance of a novel spectral reflectance sensor, built with light emitting diodes (LEDs), to monitor ecosystem metabolism, structure and function. Agricultural and Forest Meteorology, 150, 1597-1606

2. Forest floor albedo is very important for forest albedo in thinned condition. However, I think this manuscript used constant forest background albedo with management practices. I feel this assumption is too aggressive. Forest thinning involves destruction in understory species, reduces litterfall at stand level, increase development in seedlings, all which modulates floor albedo.

3. The difference of albedo between thinning and non-thinning was only 0.02. I felt this difference is not that significant. The albedo between pine and oak differ up to 0.15. The title in this manuscript says "canopy albedo is sensitive to forest thinning", which will be more realistic if saying "canopy albedo is sensitive to forest species". I know the authors' intention, but I do feel only difference in 0.02 does not support "canopy albedo is sensitive to forest thinning."

4. The way to compute radiative forcing includes unrealistic assumption. While this study tested model under summer, clear sky day, atmospheric transmittance factor, T, was derived from global annual average which includes four seasons and cloudy days.

5. Finally, Results include many discussions, which should move to Discussion session.

Specific comments: L89: How and why was understanding fragmented? One sentence should be enough

L95-100: Is the forest floor albedo same between different thinning experiments?

L103: The difference of albedo in 0.02 looks fairly small. What's the uncertainty in this value?

L106: The impact of SZA on albedo is important in clear sky day. For cloudy sky days, that effect is not that strong.

L108: The difference in canopy albedo between managed and unmanaged looks at most 0.02 (Line 16). That being said, decrease in the difference is unlikely substantial.

L114: albedo is also influenced by cloudiness and solar position.

L190: "the distance" is unclear. Is it the distance from the first contact of ray with tree to the forest floor?

L194: "direct light" -> does it mean beam radiation?

L228: The authors said "slight", but actually the impact of albedo definition on albedo estimates could be substantial. Abstract showed the albedo difference between thinning and unmanaged forest was only 0.02. The difference between white- and black-sky albedos could be easily higher than 0.02.

L229: In L207, parameters were estimated using MODIS "white-sky" albedo. But here the authors use "black-sky" albedo. Why did this discrepancy emerge?

L258: remove ','

L271: needle-to-shoot area ratio varies 1.1 to 1.8 in Chen 1996. Explain why 1.55 was used. Also, I hope to see how much JRC-TIP is sensitive to the uncertainty in needle-to-shoot area ration.

L309: The link is not accessible. I think K is Rsurf/Rtoa (where Rsurf is solar radiation



at the land surface which is measured in flux tower or weather stations, Rtoa can be computed as Bright et al 2012), thus the NASA weblink is not necessary.

L310: T is assumed 0.854. I think this value might be substantially varied with solar zenith angle (i.e. land surface is not lambertian) and aerosols. Also, 0.854 was derived from "global annual average", which includes clouds. However, this study was confined to clear sky day. Thus I feel the value of 0.854 is unlikely relevant in this study. Please provide more evidence why this value is used in this study.

L321: Again, the authors used white-sky albedo in MODIS whereas they used black-sky albedo for their simulation. Explain why.

L324: "shortwave scattering" is unclear. Does scattering include both reflectance (albedo) and transmittance? Also, as the authors compared black-sky and white-sky albedos, there should be more discrepancy in NIR which produces larger scattering effects in canopy.

L331: The lines in Fig 3 should be discernible. Use two of them as dashed lines or so. Unclear which lines represent simulation or observation.

L331: The observed gap fraction is never perfect. Check below paper which reported gap fraction data measured by optical sensors include scattered radiation by leaves, which enhance gap fraction. As leaves are not entirely black, observed gap fraction is always greater than "truth" gap fraction. Another point to support your simulation result.

Kobayashi et al., (2013). On the correct estimation of gap fraction: How to remove scattered radiation in gap fraction measurements? Agricultural and Forest Meteorology, 174–175, 170-183

L353: "0.54 (before 0.67)" is unclear. Where 0.54 and 0.67 came from? Is this value applicable to all study sites? Or specific site?

L354: "within the range of LIKELY values" is unclear. What do you mean "likely" values? Do Kuusk et al., 2010 reported some "likely" values? I guess Kuusk et al reported some

range and the authors' estimate is within the range. Then report the range.

L352-359: I see this paragraph includes a strange logic. The authors tuned NIR effective scattering albedo using NL-LOO data, then applied that tuned parameter to EE-Jar. There are two issues: 1) is the 0.54 correct? Is there any possibility to get the right number with wrong reasons? 2) how much are the two sites similar? Although both sites include same species, the stem density, LAI, canopy height, crown shape, background color all might differ.

L383: Why the "saw-toothed" types appear in Fig 4?

L408: I think "no thinning" is better than "unmanged" as management includes other activities.

L468-469: It is unclear which situation could increase crown volume while keeping LAI. To answer this question, crown volume should be carefully defined. Is crown volume same between full-leaf (summer) and no-leaf (winter)? Does crown volume include gaps within crown? Does crown volume only consider the outer lines in the crown?

L472-473: There are a lot of papers on what controls canopy albedo. For example, rougher surface (like pine trees) have lower albedo.

L493: Through the manuscript, I felt the authors should stress this study only deals with "clear" sky day. "Summertime albedo" could lead misinterpretation as there are frequent clouds during summer.

L570: It is unclear what is "this effect".

L588-589: Also should include cloudy days.

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Interactive comment on Biogeosciences Discuss., 10, 15373, 2013.