

Potential of carbon uptake and local aerosol production in boreal and hemi-boreal ecosystems across Finland and in Estonia

Piaopiao Ke et al., 2025

General comments

This manuscript was well revised according to reviewer's comment. Authors tried to answer to the question reviewers raised and refine the manuscript. It becomes readable easily compared to the previous version. Very appreciated.

Despite author's great efforts, the conclusion, readers can get through this manuscript, still seems to be very simple. Unique part in this manuscript is authors used two different data set with CO₂ flux and aerosols. However, the conclusion might not include those data enough. It doesn't mean that experiment results are not enough. I believe all data and experimental setting are quite good enough.

Authors' conclusion is *"Overall, considering the large area of forests in Finland and Estonia, the forests in total have the largest potential of climate cooling when considering the CO₂ uptake and local new particle formation."* There are three points. 1) large area 2) CO₂ uptake 3) local new particle formation. First, authors never discussed that land size is one of factors in the manuscript. It should be re-considered whether it is valuable to make a point with the land size here. Authors explained the agriculture field is comparable to forest for CO₂ uptake during summer and high N_{neg} was observed. Based on this result, readers can assume that agricultural field can be an option for climate change policy. If not, authors should answer to the question, why forest is more important than agriculture fields with a new finding based on two different data. This is well known fact that forest CO₂ sink/summertime CO₂ sink is stronger than other areas (such as urban garden, agriculture and coastal site) and other seasons based on many of previous papers.

If the conclusion ends up with that forest is the best place for climate change policy with common knowledges, this manuscript cannot be valuable to be published.

Authors should re-consider and explain major points such as how all data sets are used to make conclusion (all data can be linked together) and what the new findings are here when two data sets are used. And then it can be considered to be published.

Minor comments

1. Reference station/reference data: It is still unclear to define Hyytiälä forest (F-HYY) as a reference site. When we think of a reference site (background site) for CO₂ flux, it might be chosen by environments without any variation/fluctuations

like costal site (see Figure 4). It is hard to understand the reason to choose F-HYY as a reference site.

2. Height of each station: When seeing table S1, the instrument heights were quite different from each other. This can make a bias to analyse NEE when their values were compared to each other. Is it enough to explain that the height can be represented each site characteristic?
3. Figure 7: no explanation of a) in the caption.
4. Figure 9: If the error bars mean 10th and 25th percentile for NEE, is it necessary to + and – value? It is hard to understand of the graph. Also, there is no explanation of a) to c) in the caption and even in the manuscript.