

Biogeosciences Discuss., referee comment RC1
<https://doi.org/10.5194/bg-2021-162-RC1>, 2021
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Comment on bg-2021-162

Branko Sikoparija (Referee)

Referee comment on "Mass concentration measurements of autumn bioaerosol using low-cost sensors in a mature temperate woodland free-air carbon dioxide enrichment (FACE) experiment: investigating the role of meteorology and carbon dioxide levels" by Aileen B. Baird et al., *Biogeosciences Discuss.*, <https://doi.org/10.5194/bg-2021-162-RC1>, 2021

General Comments

The study aimed to compare quantity of fungal bioaerosols measured under forest canopy in plot with normal and elevated atmospheric CO₂ concentrations. Also, the correlations to meteorological conditions (wind speed, relative humidity, temperature) are assessed. The study attempts to answer important research question of effects climate change could have on bioaerosol emissions in forests. The manuscript is well written, and scientific results and conclusions are presented in a clear, concise, and well-structured way. However, there are several concerns regarding methodology.

The authors should make additional effort to assure that data collected by used optical particle counters represent bioaerosols. Notable quantity of inorganic particles should be present in the size range analysed and there is no evidence that bioaerosols dominate. A notable amount of sand particles or plant debris can be suspended in the atmosphere, especially in Autumn.

The hypotheses are focused on fungal spores, and it should be better addressed in results what is efficiency of used methodology for sampling expected diversity of fungal spores in studied environments. The authors clearly indicated that the size range chosen for the analysis (1-10 µm) is just the fraction of typical size range for fungal spores (1-30 µm). But in my view this creates more serious issue than just missing the effect of eCO₂ on some specific fungi. It can result in complete miss the quantity.

Also, there is a concern that in high humidity conditions optical particle counter could detect small water droplets enhancing positive correlation between RH and particle counts.

Actually failure to validate that record from optical particle counter (PM 1-10) corresponds to total bioaerosols (if not total fungal spores) severely undermines the possibility to test research questions set by authors. Without such validation the authors have results for PM 1-10 and the relation to bioaerosols or fungal spores can only be discussed.

Specific Comments

More details about setup of instruments would be needed. For how long particle samples were taken every 60s and what is the volume of air sampled? At what height the sensors were positioned?

If high resolution wind measurements are available, I would encourage authors to check also the effect of turbulent kinetic energy on particle concentrations since the atmospheric instability could have more pronounced effect on spore dispersion than the wind speed alone.

Regarding the effects of eCO₂ the authors should indicate what direct and what indirect effects are expected to increase fungal bioaerosol concentrations. It is not clear whether the CO₂ increase is expected only in canopy layer or also at the ground layer where notable number of fungal spore sources could be growing. In my view eCO₂ is expected to increase the vegetative mass of plants but such direct effect is not so straightforward for fungal spore sources since for many the growth and sporulation might have started only after the fumigation has ended. So if the most feasible is indirect effect through increase in amount leaf litter (as discussed) would not then be meaningful to have information about the quantity of leaf litter. This way the authors speculate twice: that the leaf litter is increased and that such increase relates to airborne fungal spore emission.

Finally, since the dispersion model indicated threshold wind conditions under which mixing between plots is neglectable I suggest looking into differences in particle concentrations after wind speeds above that threshold is eliminated.