"+" = agree; "-" = disagree; "0" = neutral.

1. Does the paper address relevant scientific questions within the scope of ACP? +
2. Does the paper present novel concepts, ideas, tools, or data? -
3. Are substantial conclusions reached? 0
4. Are the scientific methods and assumptions valid and clearly outlined? +
5. Are the results sufficient to support the interpretations and conclusions? 0
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? +
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? +
8. Does the title clearly reflect the contents of the paper? +
9. Does the abstract provide a concise and complete summary? +
10. Is the overall presentation well structured and clear? +
11. Is the language fluent and precise? 0
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? +
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? +
14. Are the number and quality of references appropriate? 0
15. Is the amount and quality of supplementary material appropriate? 0

Review of:

"**On spatial scales of local aerosol production in boreal ecosystems**"

by Ezhova, Rannik, Tuovinen, Garmash, Peräkylä, Ke, Laanti, Lintunen, Kerminen, Rinne, Vesala, and Kulmala

reviewed by: David R. Fitzjarrald, Atmospheric Sciences Research Center, UAlbany, SUNY, Albany NY, US of A

General comments.

The twelve authors emphasize that reactive aerosols observed near the surface may originate and grow modestly from sites relatively close to the observation site. Line 31: "However, the question about the spatial scale of an ecosystem influencing the growth of smallest particles remains open." Line 70:"The concept of the source contribution function would allow us to estimate the size of an ecosystem needed for producing new particles at the smallest sizes, and to compare it to the typical footprints of GHG measurements." They argue that a Lagrangian model applicable to *neutral* conditions for boreal forest and adjacent agricultural fields is adequate to identify how finely one can divide a landscape for the purpose of assigning source areas for growing small aerosols. They do not make clear what they mean by considering a neutral atmospheric surface layer. I write this because they do refer to what is clearly a morning convective boundary layer—definitely not neutral in terms of surface sensible heat flux. At another place, they discuss a "shallow stable" layer but such caveats about the actual atmospheric surface layer appear only toward the end of the text.

An important issue may be discussion of the forest canopy structure. This is not a small point, since the emergence of aerosols from a forest canopy and into the lower atmosphere depends a lot on the architecture of the canopy and the mixing state of the atmospheric boundary layer. Complementing this there needs to be information about the 'typical' area of the patches that constitute the landscape mosaic of forest, wetland, and agricultural field. Do not leave this only to references. This paper is strewn with references, but the reader shouldn't be sent scurrying to other texts to find this basic information essential to this paper.

The authors need to convince this reader that that the proximity of the origin of the aerosol is essential to understang the consequent density of larger particles. Presumably, this cohort is to provide a basis for growth to larger sizes that may be important cloud condensation nuclei or act to diffuse incident solar radiation, which some believe to be the source of a 'diffuse fertilization effect' on subsequent carbon uptake (e.g., Durand et la., 2026), though this idea has a competing hypothesis related to shallow clouds and shading (e.g., Kivalov and Fitzjarrald, 2019). In either case the role of aerosols is essential to either diffuse enhancement or cloud formation.

To reiterate: distinguishing the landscape among agricultural fields, wetlands and boreal forest is important to the premise of the paper. To understand better the importance of one land use type compared to another the authors should give some examples of the relative fraction of each across the landscape. Quantify the spectrum of land cover areas that your 'mosaic' exhibits. How big are your 'patches'in the areas of Finland where they work? I've worked in the boreal forest in northern Quebec, for example, and the relatively sparse tree density might have a distinctly different 'aerosol signature' than where the current focus is. (See photo below).

Specify how to describe the mosaic of land cover types in the regions on which they focus.

a. *Why* is it important to distinguish between 'local' and 'regional' spatial scales? The authors aim to improve over crude estimates of the 'aerosol footprint'. Can the reader conclude that there might be a linear combination of the constituent landscape mosaic features to assess the overall population of reactive aerosols that emerge?

b. *Where* is their definition of these scales, 'local', 'regional'?

c. *What* are the characteristic scales in the region around where their example measurement are made. This needs to be specified in this article. Perhaps somewhere in the many references this information is given, but simply referring to '*boreal forest*' and '*agricultural use'* is insufficient.

d Line 30: "Indeed, the concentration of ions in the 2-2.3 nm range demonstrates a reasonable correlation (R = 0.61) with the concentration of 3-6 nm particles …" Perhaps explaining 36% of the variance (r^2!) is 'reasonable' to the authors, but this reader is skeptical.

3. Line 37. "The potential for 2-2.3 nm ion formation has been assessed for a few sites representing different land use, with the higher number concentrations of ions observed over agricultural ecosystems…"

Please justify why the whole effort is done for relatively light wind conditions in a *neutral stability* atmospheric boundary layer, something that rarely occurs in nature. Is this approach common in this field of has this been done because it is complicated to implement the Lagrangian model for conditions with stable layers present? *Please* include some more extensive explanation why this approach is adequate to support the arguments made. That is, how can such an assumption be defended, even as we suspect that the neutral model case may be standard starting point for Lagrangian model efforts.

Neutral observation: The plethora of abbreviations (e.g., SA, for sulphuric acid) saves space, but it sends any reader whose specialization is somewhat distant from that of the authors scrambling back and forth through the text. I see no clean alternative to the authors' approach.

In the Introduction, the authors put forward wave after wave of generalizations, supported by many references, about the time scale for aerosol new particle growth from ≈1 nm to ≈1.7 nm. This reader would welcome more information about the significance of this process. A few sentences would relieve the reader from mystery, presumably because of the importance to climate alteration by the presence of larger particles. It wouldn't be too difficult to lay this out more clearly. In the Introduction CCN are mentioned—are these particles likely to be cloud condensation nuclei?

The importance of the physical environment is shunted to the side much too quickly. This makes one wonder how the authors can be so confident in dealing with 'neutral stability', a situation that occurs quite rarely near the surface over the diurnal cycle. It would be grand if the authors would include a *modest* amount of text to explain why neutral stability was chosen. (As noted earlier, I suspect that this was done to facilitate using the Lagrangian model of the footprint function. If so, say so.) To an aging student of the turbulent boundary layer, the presence of the forest canopy brings in not only a 'displacement height' for momentum absorption, but also brings a possibility of an stable inversion at canopy top, both day and night, though these would be of quite different character. On top of this, there is the roughness sublayer above the forest.

Specific comments.

1. Line 6. "Here, we consider forest and agricultural ecosystems, and distinguish situations in which aerosol production is relatively slow and vertically distributed within the well-mixed boundary layer and when it can occur quickly close to the surface." What 'well-mixed boundary layer' is this? Is it the convective boundary layer during the day, perhaps up to 1 km in thickness? Is it the free convection surface layer, up to 50 m above the surface or a 'roughness sublayer' above canopy top?

2. Lines 20-22: Again the authors invoke for turbulent fluxes are important to describe evapotranspiration, cloud formation and the like, but these are convective boundary layer issues, with appreciable surface heat flux.

3. Line 34: Is $r^2 \approx 0.4$ a 'reasonable correlation' in this line of work? Just explain, thanks.

4. Line 70. "We limit the current study to a neutral atmosphere, representing growing season conditions with moderate winds and cloudiness." In what world does a *neutral atmosphere* represent growing season conditions? This sounds like the authors are trying to deal with a forest canopy beneath a convective boundary layer, which has a nearly adiabatic lapse rate—some might call that 'neutral', but the whole layer is motivated by mixing via buoyant eddies. This surely alters the footpring, Langrangian or otherwise.

5. Line 186. Please explain more clearly why growth from 1.7 nm to 2nm is so important.

6. Line 234. The forest height is 20 m. Do aerosol particles pass out of the canopy going against no stable layer?

7 Line 270: "Oppositely, for the measurement heights above the canopy, even at 1 m above canopy top, the peak is much less pronounce…"d due to developed turbulence above the vegetation in neutral stratification…"Why do the authors insist on neutral stratification?

8 Lines 310, 327: Please repair the text: "**Fig. ??**"!'

9. Line 362. Here is the first time the authors own up: "The effects of meteorology should be considered further in future studies. An especially striking example is the decoupling in the evenings when temperature quickly drops, …" Please include this honesty also earlier in the paper, in the Introduction.

10. Line 365: "If there are on-going emissions of VOCs from any nearby sources, dilution of these components is inefficient in a shallow stable boundary layer… " We have to wait until near the end of the paper to be reminded that the analysis is severely hampered by not taking into account the stable conditions, which can exist not only just above the canopy at night, but also within a closed canopy during the daytime. This confusion could be removed by discussing earlier in the paper what is the canopy density vertical profile (leaf and branch).

11. Line 420 equation. Please define Q. Or did I miss something?

12. Line 443. Choosing a 1km convective boundary layer over the agricultural field and a 2km one over the forest is another thing that depends on the *size* of the patches. If the forest is a small

patch, for example, the boundary layer won't show that great a change. Please comment in the text.

Fig. 2. Photograph of the Arctic Boundary Layer Expedition (ABLE3B) forest site from the air. At the center is the 30 m tower. Light-colored surface between the trees is the lichen mat.

ABLE-3B forest site (Fitzjarrald & Moore, 1994)

Image of the Hyytiälä Forestry Field Station. Image source: EU-Interact.org [image]. Retrieved March 28, 2022 from https://eu-interact.org/field-sites/hyytiala-forestry-reseatch-station-smear-ii/

13. Do you have dense, or a closed boreal forest canopy? How does the 'Lagrangian footprint' vary depending on the canopy closure?

References.

Fitzjarrald, D.R. and Moore, K.E., 1994. Growing season boundary layer climate and surface exchanges in a subarctic lichen woodland. *Journal of Geophysical Research: Atmospheres*, *99*(D1), pp.1899-1917.

Kivalov, S.N. and Fitzjarrald, D.R., 2018. Quantifying and modelling the effect of cloud shadows on the surface irradiance at tropical and midlatitude forests. *Boundary-Layer Meteorology*, *166*(2), pp.165-198.

image from Fitzjarrald & Moore, 1994: