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Interactive comment on "Morphology, mixing state, and hygroscopicity of primary biological aerosol particles from a Chinese boreal forest" *by* Weijun Li et al.

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Reading the comments from Reviewer #2 again, s/he raises some fundamentally important concerns in the overview of "major points of criticism" starting half-way down the first page. At that point s/he listed four major bullets to concerning the experimental assumptions of the study. S/he also listed four overall comments related to the organization and writing quality in the paragraph before. Lastly, s/he listed several pages worth of detailed comments, some of which are quite significant in themselves. I think all of these comments are relevant and worth carefully considering.

I think it may indeed be possible to revise the manuscript sufficiently to raise it to a

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Discussion paper

publishable quality, but that will depend on the nature of the revisions. Most fundamentally, Reviewer #2 raised a number of concerns about the methods by which individual particles were classified. The lack of transparency on this issue indeed is one of the major areas of required improvement. After carefully adding specific details about how particles were assigned and categorized, it will be easier to evaluate the observations and conclusions. Without knowing more about the process by which particles were investigated and assigned, it is hard to know if the method itself was sufficient to support the conclusions. The question here is not just about clarifying wording, but that the clarified wording will help evaluate whether the method was sufficient or not. In particular, the question of whether the particle assignments were correct is not sufficiently addressed. Just because a systematic method is established is not a sufficient criterion to determine whether the method leads to correct assignment. For example, consider a skeptical scientist reader. Convince them that your method led to detection and consistent, correct categorization of the particle types you report.

Somewhat independent of the comments from the two reviewers, my suggestions are two-fold:

- The observations and atmospheric implications are relatively similar to works that have used similar techniques in both boreal and tropical areas, but these are not well cited in the manuscript. At a minimum I suggest you to consider additional literature, and make sure to at least briefly compare results with these in mind. I suggest doing a good literature survey of PBAP observations from forests, as well as a search related to ambient studies related to microscopy of PBAP (i.e. SEM and TEM, as you use). Then make sure that the observations you present and the conclusions you draw are put into context of these previous measurements.

- The statement in the manuscript that the work can be used as a "full database" to "be used to identify primary biological particles using single particle techniques" is overstated in my opinion. I think that that study can be revised to show an overview of observations of (bio)-particles in this region, but using the results as a database for

future reference is entirely different. This would require a substantially higher threshold of demonstrated quality, which may or may not have been achieved. To claim these data as basis for a database of atmospheric particles implies that a sufficiently systematic representation of bioparticles has been sampled and correctly analyzed. Further, the database would need to show some sort of independent verification that particles were assigned correctly, similar to above comments. Since most methods of independent identification are well beyond the scope of the work you reported, I do not expect that you would want to argue that the assignments have necessarily been verified as correct. They are merely suggestions, with uncertainties and potential assignment errors to be at least briefly discussed in the revised manuscript. So in that case I would suggest that at a minimum you scale back the conclusions to remove the concept of 'database,' and report in the context of 'observations'. This does not get around the first concerns that Reviewer #2 raised about categorization of particle type, but it will help to re-frame the conclusions a bit.

I strongly suggest keeping all these comments, including those from the two Reviewers, closely in mind as you revise and respond to all comments line-by-line. I look forward to reviewing the revised manuscript when available.

Best regards,

Alex Huffman