Review of Seasonal variation in size-resolved particle deposition and the effect of environmental conditions on dry deposition in a pine forest

General comments:

This research examines the seasonality of aerosol dry deposition velocities to an evergreen needleleaf forest in Colorado and attempts to explain seasonal differences through model sensitivity studies where various process representations are tried. The field experiment is well described, and the model sensitives are methodically presented. The main premise is that the measurements show significantly greater deposition velocities in the winter than the other seasons. This conclusion relies on considering only negative (downward) fluxes in the dry deposition velocities. This practice leads to greater Vd for winter than summer even though the net exchange velocity (V_{ex}) is much greater negative (downward) in the summer and the V_{ex} in winter is mostly positive. I'm not convinced that only counting the negative fluxes is reasonable. It seems to me that both upward (emission) and downward (deposition) fluxes could be happening simultaneously resulting in a small net V_{ex} that could be either positive or negative. Negative net flux only means that the average deposition flux is greater than the emission flux for that 30 min period, not that only deposition is occurring. This needs to be further explained and justified.

The modeling experiments designed to explore possible mechanisms for the greater Vd in winter are methodical and well described. It is concluded that the only phoretic effect that may be significant in this case is turbophoresis. These results are interesting and suggest that more models should include this effect. However, it is also concluded that the effects of turbophoresis are not sufficient to account for the higher winter Vd. To fully account for the higher winter Vd it was found that increasing the interception scaling coefficient for winter did the trick. The explanation is that microroughness on the needle surfaces is greater in the winter. This conclusion takes it for granted that interception is the key process. This view seems to be based on the apparent success of the Emerson et al (2020) model in better matching observation in forests. But it should be acknowledged that this model was developed through iterative tuning of empirical constants to observations. Also, interception is generally considered to be the least physically based process of the collection efficiencies. For example, in the supplement to the Emerson et al (2020) paper it is said: "There is no underlying physical basis for this term". After presenting the detailed theory behind the thermophoretic and turbophoretic effects, the tweaking of the interception term does not seem to have the same level of rigor. There may be other ways to modify the other collection terms to get the desired result.

Specific comments

Lns 157-159: As mentioned above, I think the assumption of separating positive and negative fluxes should be better explained and justified.

Line 238: Can you provide more explanation of the uncertainty from counting and implications for the overall uncertainty of the flux measurements?

Line 269: What are the units for the stability values?

Line 290, eqn 13: Aerosol dry deposition is better represented by $V_d = \frac{V_g}{1-exp\left(-V_g(R_a+R_s)\right)}$ from Venkatram and Pleim (1999). Eqn 13 overestimates Vd for dp > ~ 8 μm.

Line 299: b is a nondimensional parameter. There seems to be some confusion about this. See more in my comment below about line 469

Line 303: This expression makes no sense. This says that σ_w = -0.2u_*

Line 372: What is meant by this statement about bi-modal distributions?

Line 425: Why were the needles consistently colder that the air during the day? This seems counter intuitive especially if some needles were sunlit.

Line 451: The role of interception should not be stated as fact.

Line 469: There is a misunderstanding of the variable *b* from the Katul et al (2010) paper. This is a non-dimensional parameter. The viscous sublayer thickness is represented by δ in that paper and has values on the order of 0.1 – 0.5 mm. Please correct this.

Line 472: "in" is repeated

Lins 474-478: The plateau for dp > 10 μm is not the same as the plateau noted by Saylor et al (2019) in 1-10 μm range.

Figure 7: plots are hard to read.

Line 511: the macroscale roughness length used in these models only affects Ra.

Lines 554-558: Figs S14 and S15 seem to be swapped.

Line 578-579: The statement that impaction only depends on energy of the particle is not true. It also depends on the obstacle length scale used in the Stoke number.

Line 659: Expression for St should be given.

Lines 681-682: Here b is given units of m! See comment above

Eqn B1: This expression implies that SWR is smaller than PAR! They must have different units.

Line 695: stomatal conductance should have units of s/m

Fig B1: How can the leaves be so much colder than the air during the day?

Fig B2: Units of ΔT are given at K/m. Isn't ΔT a difference between leaf and air, not a gradient? Isn't even 10 K/m a ridiculously large gradient?