I believe the authors still need to consider and revise the manuscript to address prior comments. The revised submission contains minimal modification to the manuscript and as it stands the authors do not fully clarify the calculation methods they apply, and they do not fully analyze the results from their experiments.

1. Equation 5 is still misleading without more explanation. The enhancement factor, if expressed in this way, would not only be a function of just the Hamaker constant and particle diameters, but also the gas pressure, gas temperature, and composition $E(A, d_1, d_2, kT, P)$. It is actually combination of the free molecular enhancement factor and continuum enhancement factor, and depends on the Knudsen number (really it is $E(A/kT, Kn)$). I recommend in describing calculations, the authors start with theoretically more appropriate expressions (those of Sceats, as shown in Stolzenburg et al, 2020, the equations in Alam 1987, or in Ouyang et al 2012) and then show where $E(A/kT, Kn)$) that they apply comes from. With regards to enhancement factor calculation, the authors only state: "The value of E(A) was obtained using a model accounting for the van der Waals attractive force and the methods to compute size-dependent E(A) can be found in previous studies (Alam, 1987; Sceats, 1989; Chan and Mozurkewich, 2001)." What was the exact equation applied? These references take slightly different approaches from one another. Assuming they are using equation (24) from Alam, 1987 for $E(A/kT, Kn)$), then the authors could clearly show their $E(A/kT, Kn)$ function, as Alam 1987 uses separate enhancement factors for the continuum and free molecular regimes. If they were instead using Chan & Mozurkewich equation (27), which does not show a transition regime enhancement factor explicitly but instead a transition regime coagulation rate, then they would still derive a $E(A/kT, Kn)$ expression with separate continuum and free molecular enhancement factors within it. The authors also need to clarify their method of calculations for the free molecular enhancement factor, was Marlow's equation used, as in Alam 1987 or Sceats's equation as in Chan and Mozurkewich?

   A final note on this topic- the authors state "We took the multiplicative factor from this model instead of the van der Waals coagulation coefficient to avoid the minimal differences among the coagulation coefficients given by different models (Lehtinen and Kulmala, 2003; Alam, 1987; Ouyang et al., 2012) at A = 0 J." This is tangential to the issue with equation (5). The issue with current presentation of equation (5) is that it hides that there are different enhancement factors for different regimes, and for van der Waals potentials, the enhancement factor in the free molecular regime is typically considerably larger than in the continuum regime. Particular care has to be taken to explain what the Knudsen numbers are for the processes examined, and how enhancement factors are calculated.

2. The authors have dismissed comments (3) and (6) from prior review without completely modifying the manuscript. As written, the manuscript (and response to reviewer comments) come off as quite hasty. I recommend that the authors include, perhaps as a supplemental table, the number concentrations of 100 nm particles the initial number concentrations of 3-10 nm particles (ideally broken up into sub ranges, perhaps 1 nm increments) for each experiment performed. The Kn range or average Kn for each bin should also be shown, as shown a best fit $E(A/kT, Kn)$ for that size range. There appear to be a number of potential valuable data in Figure 3, but they are not provided in a way that can be easily used by others.