The paper has been improved and indeed more information on the experiment and the analysis can now be discerned. There are some crucial issues in the main result that appear to be open to alternate analyses. The change in the approach now taken by the authors is good but the use of dry GR introduces digressions and considerable confusion to this reviewer. Below I calculate and report a 'wet' GR that the particles actually undergo in the experiment. This alternate analysis suggests the enhancement in collisional rates due to van der Waals forces is less than their analysis suggests. The authors' discussion of the issues raised in (1) and (2) will illuminate the best way forward.

(1) The explicit inclusion of the particle's water content in the analysis is quite important for growth rates and the derived collision rate coefficients. Yet there is only a modest change in the results! Inspection of the previous eqn (S1) revealed that a factor of two was used 'to include collisions in both ways'. This factor was not justified in the first version (thanks to Chris Hogan's close look) and now the authors have dropped it, claiming that another factor of two canceled it. Can the authors explain the evolution of their thought in developing eqn (S1)? Where did the 'collisions in both ways' idea come from? It seems that previously they used the k_{coll} from Niemenen et al. It is not clear how the factor of two was canceled out in the last version and what is going on with the revised calculations.

(1b) The assumption that the dry volume of the gaseous H_2SO_4 molecules can be assigned to the change in the dry volume of the particles has not been shown to be true. Furthermore, H_2SO_4 hydration is a matter of wide variability according to the quantum chemistry studies; why choose Henschel over Temelso and how much of an effect does an alternate choice make?

(2) The growth factor expression seems a bit odd and needs a reference. It seems a more standard expression would have in the numerator the density ρ time weight fraction *w* of the appropriate RH in the instrument. Looking at the data for 5 nm dry mass diameter, the numerator has a value of 1.68 to get the *gf* in the figure. This is the density of 76 wt % SA. Applying this *w* to the numerator, *gf* is 1.24 for a dry mass diameter of 5 nm.

(3) Using the data in Figures S1(b,c, [SA]=2.1e7 cm^{-3}) and applying *gf* of 1.2 and 1.28 for the 2.9 and 7.7 mass diameter, one gets an experimental GR of 3.2 nm/hr over the 3.2 to 8 nm dry mob. diameter range. Compare this value to the GR calculated using the 'wet' GR from eqn. (7) of Verheggen and Mozurkewich (JGR, 2002), one gets a GR of 2.17 nm/hr for 48 wt. % SA (the SA-content for the midrange dry mass diameter of 5 nm): 1.95 nm/hr from the first term on the RHS, involving growth by SA uptake, and 0.22 nm/hr for the 2nd term, particle swelling due to the change in composition with size. Incorporating the size of the condensing molecule into eqn (7) of Verh. and Moz. (2002), by applying a factor of $(1+d_v/d_p)^2$ to the first term and the total GR is 2.6 nm/hr, about 20 % less than the experimental GR. This analysis has a hard-sphere GR that departs from experimental somewhat less than what appears to be in the paper.

(4) Composition data is incredibly important here and Fig. S4a is a welcome figure. Yet the composition etc. inside the instrument is also needed (the numerator in the *gf* eqn). Having said that, the Fig. S4a SAWNUC composition data has not really been put to any stringent tests: it was the nucleation rates - for cold conditions - that were verified in Ehrhart et al. 2016 (or have I missed something in that work?) Variations in ρ and *w* should be considered in the uncertainty analysis. Also important in this is that ammonia was present which is not considered in SAWNUC, thus more uncertainty.

(5) As to the data for growth between 1.8 and 3.2 nm mobility diameter. The 1.8 nm data does not show fidelity to the assumed time dependence as there is a slow climb in the count rate over the hours (Fig. S1). What is the physical reason for the assumed shape of the appearance curve (a systematic concern)? Also, the composition of the 'dried' 1.8 nm particles would be most affected by charge, even the SAWNUC calculations allude to that (Ehrhart (2016) plot). Ammonia might affect the smallest channels differently than it would the larger channels. Aside from systematic biases, there is a significant random uncertainty in the appearance time for the small diameter channels, many (5 or 10?) minutes. To better serve the reader, there needs to be two paragraphs added to the main text. (i) An experimental paragraph on how the DMA's were deployed and the experimental conditions in them (e.g., 5 % RH is due to dry sheath gas diluting the incoming moist sample aerosol? Change in temperature upon sampling? Sampling arrangement?) (ii) A paragraph describing the main assumptions in the appearance time method, e.g. answering the questions raised above.

(6) A paragraph in the Supplement describing the Inside method is needed. For example, how do values appear continuously and even at lower diameter then the measurements it is derived from? Why was the TREND method not selected? Pichelstorfer et al. notes that this method is good for capturing the leading edge of growing aerosol. There is also a log normal method. It is notable that there seems to be only partial agreement between these methods when comparing to experimental data.

(7) It is not clear how S9 (previously S6) was obtained nor what the authors mean in the different nomenclature between the terms on the LHS of eqns. S1 and S9. Is one of them an average GR?