The authors would like to thank the reviewers for their thoughtful reviews, and constructive comments and suggestions. Our replies are given directly after the comments (in bold); text that has been added/revised is shown in red font.

General comments:

The authors focus on the contribution of particle-particle interaction to growth and determine a maximum error of the growth rate for the collision controlled scenario. They do not explicitly state that this error represents a maximum overestimation of the growth rate (there are several statements mentioning this "upper limit" of the GR [page 3, line 85; page 7 line 191; p 10, line 312] or "maximum possible error" [abstract]; however, it may be interpreted by the reader as the maximum value of the error). It may also be worth mentioning the possibility of GR underestimation caused by deposition losses, dilution and losses to pre-existing particles.

The effect of pre-existing particles on GR errors is discussed as a representative case for several processes (wall loss, dilution and pre-existing particles). This, according to the authors, is justified by findings on the similarity of those processes with regard to effects on the nucleation as described in a recent study (McMurry & Li, 2017). In the present manuscript it is assumed that particle sinks of any form mainly reduce the monomer concentration. Thus, the main effect is the reduction of nucleated particles and this limits coagulation which, according to the authors reduces the error in the GRs. However, loss of particles to the wall, to preexisting particles or by dilution is not considered by the analysis methods discussed and thus potentially lowers the GR obtained from the respective methods (e.g. in a case with low particle growth where uptake of vapor by the walls is limited while the walls may represent a perfect sink for particles). This results in underestimation of the GR.

In the manuscript errors of the analyzed GRs are discussed with regard to the analysis methods applied which are not suitable to produce size and time dependent GRs. The result of those methods is rather an array giving GR for various particle sizes and different measurement times. Further, the methods have inherent errors as they attribute any change of the PSD to growth. Thus, these methods in general are not suitable to produce realistic GR. However, in some specific cases they are. The present manuscript does not provide the necessary information to distinguish between situations where the methods can safely be applied or not. The reason is the fact that possible underestimation of the GR is not discussed (e.g. low GR in a chamber with considerable wall loss and/or dilution may lead to considerable underestimation of the GR by applying one of the methods used). Thus, I suggest removing statements on situations featuring safe usage of those methods and replacing them by statements indicating where the methods cannot/should not be applied. Maybe the authors should also point out once again the possible alternative methods for data analysis which do not suffer from the errors discussed in this manuscript in the conclusion section.

Reply to general comments:

We find the review very constructive and have improved our paper accordingly. Major changes include

1. We added Sect. 3.4 and Fig. 6 in the revised manuscript to qualitatively show that in the presence of strong particle sinks, true growth rate can be underestimated by measured growth rate. In such nucleation scenarios, the particle size distribution approaches steady state after a certain time with the measured growth rate approaching 0, but the true growth rate remains finite and is thus underestimated by measured growth rate.
2. Since we do not study underestimation of growth quantitatively, we changed 'maximum possible error' or similar expressions to 'maximum overestimation of GR_{true} by GR_m' or similar expressions throughout the manuscript.

3. Statements regarding safe usage of using measured growth rate as true growth rates have been removed; instead, we mainly focus on the discussing the simulation results presented in the paper and avoid making overly general statements.

Reply to specific comments:

p.2, line 40 (f): "Coagulation is accounted for with the coagulation integrals in the GDE and is a relatively well understood process that can be described with reasonable confidence in models." A reference would be helpful

We included Chan and Mozurkewich (2001) and Kürten et al. (2018). In the former reference coagulation rates were measured experimentally and Hamaker constant were otained by fitting experimental data. The result were then applied in the latter reference to analyze CLOUD data.

p.2, line 41 (f): "Growth involves processes that are not well understood for chemically complex aerosol systems, such as the atmosphere." Reference or examples plus references would be helpful.

We included Barsanti et al. (2009), Riipinen et al. (2012) and Hodshire et al. (2016) as references.

p.4, line 95 (f): "Our results help to inform estimates of uncertainty for complex aerosol systems, such as the atmosphere, where errors are difficult to quantify." How is this possible as the present manuscript deals with nucleation of a single molecule species which is formed at a constant rate?

We think our original statement is a bit overreaching. The corresponding text now reads "Our results help to inform estimates of uncertainties for systems with a single nucleating species, or systems that can be modeled in a similar way to a single species system (Kürten et al. ,2018)."

p.6, line 158: "and $\boldsymbol{E_k}$ is the particle the evaporation rate". Remove the second "the".

'The' has been removed.

p.7, line 190 (ff): "We believe collision-controlled nucleation (E=0) in the absence of other particle loss mechanisms such as wall deposition (W=0) and scavenging by preexisting particles ($\sqrt{L}$=0) provides an upper limit to errors in GRm for a constant rate system (R=constant)." The error represents a maximum overestimation of the GR. A"maximum error" would also mean that it is bigger than the maximum underestimation of the GR which may not be true. Thus this statement is too general to me.

Agreed. We reworded the sentence to be "We believe collision-controlled nucleation (E=0) in the absence of other particle loss mechanisms such as wall deposition (W=0) and scavenging by preexisting particles ($\sqrt{L}$=0) provides an upper limit for overestimation of GR_{true} for a constant rate system (R=constant)."

p.7, line 199: "Most noticeably, particles grow considerably faster at early stages of simulation" Do the particles really grow faster or do they seem to grow faster? What is the reason?

The following sentences were added to explain the faster particle growth at the early stage of simulation: "This occurs because evaporation depletes clusters and correspondingly increases monomer concentration. In the absence of pre-existing particles, monomer concentration accumulates until the supersaturation is high enough for nucleation to take place (see figure 2c). The accumulated monomers then rapidly condense on the nucleated particles, leading to the rapid particle growth shown in figure 2b."

p.9, line 275: "Note for the range of $\sqrt{L}$ values examined, the presence of preexisting particles alter GR_{true}/GR_m values by no more than 50%." The GR_{true}/GR_m ratio ranges from roughly 0.35 to about 1.1 which is more than 50% (see Fig. 4b)

The original text "Note for the range of $\sqrt{L}$ values examined, the presence of preexisting particles alter GR_{true}/GR_m values by no more than 50%" is a comment on collision-controlled nucleation ($E=0$). Fig. 4b shows the difference between each curve (corresponding to different $\sqrt{L}$ values) is indeed less than 50%. To avoid confusion, "for collision controlled nucleation" is added to the original text.

p.10, line 306 (f): "In practice, this means measured growth rate based on all the four representative sizes can be a reasonable substitute of the true growth rate in a similar nucleation scenario." As the possibility to underestimate the GR is not discussed, this statement does not hold true. Further, "similar nucleation scenario" is a vague statement. When would an experimental set of data be similar?

This sentence has been deleted and the analysis in the revised manuscript is focused only on the simulation results.

p.10. line 312: "Collision-controlled nucleation without preexisting particles results in an upper limit (up to a factor of 6) to discrepancies between true (GR_{true}) and measured (e.g., $GR_{m,mode}$) growth rates." It could be mentioned that this statement refers to simulated data (e.g.: Simulation showed that collision-controlled) otherwise it is too general.

Agreed. The sentence in question now reads "Simulated data shows that collision-controlled nucleation without pre-existing particles leads to an upper limit (up to a factor of 6) of overestimating true growth rates (GR_{true}) by modal growth rates ($GR_{m,mode}$)."

p.10, line 318 (f): "Both evaporation and preexisting particles bring GR_{true}/ GR_m closer to unity by decreasing the number of nucleated particles. In the case of evaporation, GR_{true}/ GR_m also increases as a result of elevated monomer concentration." This statement in general is not true. Evaporation and preexisting particles reduce the ratio GR_{true}/GR_m by reducing the overestimation caused by coagulation. In case the GR is underestimated (i.e. $GR_{true}/GR_m < 1$; caused by e.g. wall losses/dilution combined with weak particle growth) by the analysis methods, the combined effect of evaporation and preexisting particles would even increase the error

The sentence now reads "Both evaporation and scavenging by preexisting particles can reduce the concentration of particles formed by nucleation. Lower particle concentrations reduce the effect of coagulation on GR_m, so overestimation of GR_{true} by GR_m is lower than is found in the absence of these processes". In addition, we added section 3.4 to briefly discuss the situation where strong particle sink processes (i.e., sufficiently large values of M or $\sqrt{L}$) lead to steady state particle size distributions. In these cases, measurements would not reveal any particle growth after a certain time and GR_m would approach 0.

p.10 line 324 (f): "In this case, GR_m based on all representative sizes can be a good approximation of GR_{true} due to negligible coagulation effects." This statement, similar to the previous one, is too general as it considers only the possible overestimation of the GR (caused by coagulation). However, if the analysis method does not account for methods different from coagulation (e.g. dilution, wall loss, deposition), there may still be a significant difference between the measured and the "true" GR.

This statement has been deleted since it is too general.

Barsanti, K. C., McMurry, P. H., and Smith, J. N.: The potential contribution of organic salts to new particle growth, Atmos. Chem. Phys., 9, 2949-2957, 10.5194/acp-9-2949-2009, 2009.

Chan, T. W., and Mozurkewich, M.: Measurement of the coagulation rate constant for sulfuric acid particles as a function of particle size using tandem differential mobility analysis, Journal of Aerosol Science, 32, 321-339, https://doi.org/10.1016/S0021-8502(00)00081-1, 2001.

Hodshire, A. L., Lawler, M. J., Zhao, J., Ortega, J., Jen, C., Yli-Juuti, T., Brewer, J. F., Kodros, J. K., Barsanti, K. C., Hanson, D. R., McMurry, P. H., Smith, J. N., and Pierce, J. R.: Multiple new-particle growth pathways observed at the US DOE Southern Great Plains field site, Atmos. Chem. Phys., 16, 9321-9348, 10.5194/acp-16-9321-2016, 2016.
Kürten, A., Li, C., Bianchi, F., Curtius, J., Dias, A., Donahue, N. M., Duplissy, J., Flagan, R. C., Hakala, J., Jokinen, T., Kirkby, J., Kulmala, M., Laaksonen, A., Lehtipalo, K., Makhmutov, V., Onnela, A., Rissanen, M. P., Simon, M., Sipilä, M., Stozhkov, Y., Tröstl, J., Ye, P., and McMurry, P. H.: New particle formation in the sulfuric acid–dimethylamine–water system: reevaluation of CLOUD chamber measurements and comparison to an aerosol nucleation and growth model, Atmos. Chem. Phys., 18, 845-863, 10.5194/acp-18-845-2018, 2018.
Riipinen, I., Yli-Juuti, T., Pierce, J. R., Petäjä, T., Worsnop, D. R., Kulmala, M., and Donahue, N. M.: The contribution of organics to atmospheric nanoparticle growth, Nature Geoscience, 5, 453, 10.1038/ngeo1499, 2012.