The authors thank the reviewer for the effort to review the manuscript and to provide constructive comments. Our replies to the comments and our actions taken to revise the paper (in blue) are given below (the original comments are copied here in *Italic*).

Anonymous Referee #2

The authors implement a temperature-dependent organic nucleation parameterization in a global model. Simulations are compared with in situ observations. A climate forcing is calculated. The modification of the parameterisation and the in-situ observation comparison are interesting and useful, but the climate forcing calculation is questionable.

The main problem is that (looking at Yu et al. 2015), there seems to ONLY be organics mediated nucleation in this model set-up. This does not invalidate all of the findings, but one of the main outcomes of the CERN CLOUD experiment is that we know there are many different types of nucleation going on at the same time, and so models which only include one type of nucleation will seem more sensitive to changes than the real atmosphere. Figure 5 shows that the temperature dependent parameterisation does give very good agreement with observations, and I believe the temperature dependence is useful and valid in this sense. But the idea that 40% of boundary layer CCN would be lost to a temperature dependence in the organic nucleation rate only makes sense if no other nucleation is happening in the model, which is very unrealistic for the atmosphere. This also calls into question the quoted radiative forcings (radiative effects, since they do not relate to the pre-industrial?) shown in Figure 6.

Simulations which include other nucleation types should be performed to give a more accurate estimate of the climate estimates, which I am sure will be smaller than those quoted in the current version of the manuscript.

The main focus of this study is to develop a modification of organic nucleation parameterization and investigate the impact of temperature on H2SO4-Organics nucleation rates. We agree with the reviewer about the uncertainties in the radiative forcing (RF) estimates. To address the referee's concern and to follow the referee's suggestion (see below) to place the emphasis on "the realistic improvements in nucleation representation shown in Figure 5, rather than on unrealistically high model sensitivity to an overly simplistic nucleation scheme", we have deleted Figure 6 and associated text in the revised manuscript.

Following the suggestion of the other reviewer, we have added two new figures and associated discussions to explore the sensitivity of results to ΔH values and to expand comparisons with observations.

Was there any physical basis for setting the maximum value of fT to 10? If the method is valid for higher temperatures, why is it not valid for lower temperatures? If the method gives unrealistic values at low temperatures, why do we trust it at high ones?

The physical basis for setting the maximum value of f_T is that the organics mediated nucleation rates should not exceed the 3-body kinetic collision rate for forming a cluster containing two H_2SO_4 molecules and one BioOxOrg molecule (k_{max}). k_{max} depends on T as well as the mass and

sizes of colliding molecules. At T=270 K, k_{max} is about a factor of 38 higher than k_m. We have revised the manuscript to set the maximum value of f_T to k_{max}/k_m instead of 10. The modification has negligible effect on the results and conclusions of this study which focuses on the summer month and the boundary layer. The method is valid for both high and low temperature.

It says in Section 2.2 that this is the "first global modeling attempt in studying the effect of temperature on organics-mediated nucleation in the atmosphere", and this assertion is repeated in the summary and discussion section. Section 20 of supplementary materials and figure S9 in Dunne et al. (2016)'s Nature paper on nucleation based on CERN CLOUD experiment contains a temperature dependence of organic nucleation. It was not the main focus of the paper but it should be mentioned.

The work of Dunne et al. (2016) is now mentioned in the discussion.

With revisions, the paper will make a good contribution to the field and should be published. But the emphasis should be placed on the realistic improvements in nucleation representation shown in Figure 5, rather than on unrealistically high model sensitivity to an overly simplistic nucleation scheme.

Please see our reply above.