Dear reviewer #3,

we thank you for your comments on our manuscript. Below we present our answers (black color) to your comments (grey color) accompanied by the changes we performed in the manuscript (blue color).

Major comments:

1) I have concerns about the research contribution of the presented work. Both the ART model and the ICON model are described elsewhere. The equations presented in section 3, 4 and 5 are largely standard.
   We disagree with the reviewer in that point. Moreover, we think that when describing a new model system it is absolutely necessary to give the reader access to the underlying equations. From our own experience we know how boring it is to search this basic information in consecutive, often even grey literature.
   Furthermore, the following is stated on GMD website in the description of Model Description papers (http://www.geoscientific-model-development.net/submission/manuscript_types.html):
   The main paper should describe both the underlying scientific basis and purpose of the model, and overview the numerical solutions employed.
2) The volcanic ash source function is new, but its presentation is not well-motivated. What was wrong with the old source function? This could be strengthened by comparing the new treatment to the old treatment and by quantifying the improvements (if any).
   As there is no old version of the source function in ICON-ART there is nothing to compare with. The parameterization of the emission of volcanic ash is a crucial step in volcanic ash forecast. In case of ICON-ART we developed a parameterization based on recent findings that followed the Eyjafjallayökull eruption. The description of the vertical profile of the emissions is to our knowledge new.
3) I assume that this paper falls in the category of "Model Description paper" as described on the GMD website http://www.geoscientific-model-development.net/submission/manuscript_types.html. The paper falls short on several criteria listed here: Code availability and licensing: The text mentions that ART can be obtained from the Institute for Meteorology and Climate Research at KIT. What is the license (e.g. GPL, BSD, CC-NC)? What about the ICON part?
   As this point was also raised by the executive editor, we refer to the answer we give there.
4) Verification and validation: The three application examples are examples at best. I'm sure that large amounts of work went into this part, but for a manuscript in the category "model description paper", I would expect more comprehensive and rigorous V&V, using appropriate quantitative metrics. For example for the comparison with the ERA-Interim analysis, more than one particular day should be used, and the agreement/disagreement should be quantified with the usual error metrics. The authors may want to think about if they rather focus on one case study rather than doing three.
   As the reviewer mentions our paper is a model description paper. In the description of this category at the GMD homepage it is stated that at this stage no validation of the results is required. Moreover it is stressed that examples should be given showing the capabilities and

the functionality of the model. For that reason we will stay with the three examples as presented because they document the capabilities of ICON-ART 1.0.

Minor comments:

5) It would be helpful to know the computational resources that are required to perform the simulation, especially if this could be compared to existing models. How does coupling of ART to ICON impact the scalability of the ICON model?

   It is always difficult to compare computational resources of different models as this depends heavily on the machine where the computation was performed, the compiler setup (e.g. version or optimization level) and the chosen model setup. In general, ICON scales nearly perfect with an increasing number of cores and ART does not affect this behavior. E.g., using 12 ART tracers for volcanic ash and sea salt aerosol lead to roughly a factor of 3 compared to an ICON simulation without ART. This test was performed with MPI parallelization only on different numbers of cores between 64 and 1024. We added a small passage concerning the scalability:

   We performed tests with different numbers of cores (powers of two between 64 and 1024) and found roughly a factor of 3 for an ICON-ART simulation compared to an ICON simulation without ART. The ART simulation for this purpose was performed with volcanic ash and sea salt aerosol switched on. This shows that the scalability of ICON applies also to ICON-ART.

6) Equation 10: There should be a factor of pi/6 relating the third moment to the volume (and mass).

   The formulation in the brackets (page 575, line 8-9) was misleading. The $\Psi_{3,l}$ in equation 10 is a mass mixing ratio as stated in the text above. The index 3 was chosen as mass mixing ratio is assumed to be proportional to third moment. We adapted the text in the brackets:

   (the indices 0 and 3 are chosen due to the proportionality to these moments of the distribution)

7) Table 3: Are these bin centers or bin edges?

   These are bin centers. We modified the table heading to make this clear:

   The diameters of the size bin centers are denoted by d_l.

Typos:

8) I suggest avoiding the use of "get", and use for example "obtain" instead. This occurs several times in the paper.

   We identified the 'get' the reviewer is referring to and changed them to 'obtained':

   - By this procedure we obtain a dimensionless vertical emission profile that is shown in Fig 1b.
   - Based on these measurements we obtain distribution factors f_l given in Table 3.
   - (…) where N_r is the total number concentration of rain droplets and \rho_w the density of water, we obtain (…)
   - Assuming large particles D_p,l > 1 µm and a constant representative rain drop size of D_r= 5*10^-4 m we obtain (…)
   - For particles with a diameter below 15 µm we obtain (…).

9) p. 581, line 15: should be "a one-dimensional"

   We corrected this:

The turbulent fluxes within ICON are treated by a one-dimensional prognostic TKE turbulence scheme (Raschendorfer, 2001).

10) p. 582, line 3: should be "In the cases of …"

We corrected this:

In the cases of gases and particles this surface flux is determined by the dry deposition process.

11) p. 583, line 2: should be "size and terminal fall velocity are small"

We corrected the 'is' to 'are':

With the assumptions, that size and terminal fall velocity of aerosol particles are small compared to rain drops, (…)

12) p. 586: "Realisation": do you mean numerical implementation?

We changed the section heading to 'Numerical implementation'.