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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lidar observation and model simulation of a volcanic-ash-induced cirrus cloud during the Eyjafjallajökull eruption" *by* C. Rolf et al.

Anonymous Referee #2

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Review of

Lidar observation and model simulation of a volcanic-ash-induced cirrus cloud during the Eyjafjallajökull eruption

by C. Rolf et al.

General comment:

In this study the formation of an ice cloud from volcanic ash particles as seen from LIDAR measurements over Juelich is investigated; in addition, detailed model calculations are carried out in order to interpret the measurements. In general, this is an appropriate contribution to ACP. I recommend minor revisions, i.e. some issues should be clarified before the manuscript can be accepted for publication.

Major points:

1. Humidity as derived from ECMWF and radiosondes:
   How reliable are the humidity values as obtained from ECMWF operational analyses? Although the data assimilation scheme of IFS does now allow for ice supersaturation (to be checked if the Tompkins et al., 2007, scheme is already implemented), the humidity values in the upper troposphere/lowermost stratosphere stem from standard operational radiosondes (usually of type Vaisala RS80/90/92). As we know from many former studies (Miloshevich et al., 2000; 2009; Wang et al., 2002; Leiterer et al., 2005), standard humidity sensors in radiosondes underestimate real values of humidity tremendously in the cold temperature regime ($T < 233K$), thus we cannot expect realistic ice supersaturation values in ECMWF humidity fields (i.e. up to homogeneous nucleation thresholds in the range $RHi_{hom} \sim 140-160\%$). The same is true for the standard radiosonde data, i.e. the Essen radiosonde will not produce high humidity values. Thus, the use of ECMWF and standard radiosonde data in order to rule out homogeneous nucleation just from humidity measurements is very risky and maybe without solid justification - at least without any further argument. One additional way in order to corroborate the results from modelling indicating heterogeneous nucleation and (almost) no contribution from homogeneous nucleation would be to correct manually the Essen radiosonde data, using the standard procedures as given in the literature (depending on the type of radiosonde, i.e. RS80/RS90/RS92, see e.g. references given above). If there is also no high humidity signal in the corrected values, then the hypothesis of low humidity values in ECMWF might be more appropriate or plausible. For the manual correction I would suggest to use high-resolution raw data (e.g. 10 sec mean values), if possible.

ACPD

12, C5663–C5667, 2012

Interactive Comment

2. Sedimentation of ice crystals:
   There are two issues regarding the 2D structure of the ice cloud as simulated with the model:

   (a) From the box model simulations it seems that the simulated ice cloud does not sediment in the same way as the observed cirrus cloud. A reason for this could be that the simulated cloud has too small ice crystals. This brings me to the question, which shape of ice crystals is used in the model MAID? There is nothing stated in the manuscript, however there are some statements about size distributions in terms of mean mass/effective radius. Which shape of ice crystals do you assume in the model (influencing also the diffusional growth) and what is the terminal velocity relation, which is applied? It might be that assuming a spherical shape of ice crystals could lead to wrong falling behaviour. Although it is not clear, which shape of ice crystals typically occur in the UT/LS region, for sure they are not spherical.

   (b) It was also not clear to me, how the 2D structure was build up. Do you really use a kind of column model (i.e. some boxes on top of each other, coupled by sedimentation fluxes) or do you just use the model along many different trajectories, investigating only thin boxes (this is how I understood the description). If the latter is true, this could also be a reason for the strong difference: Since you have not really a column, once the ice crystals are falling out of one single box, they are lost, i.e. they never ever appear in another box in the lower part of the profile. Thus, they cannot quench ice nucleation in lower and warmer boxes; therefore, in the lower boxes, ice nucleation will take place, leading to many small ice crystals in the lower part of the cloud, which will stay there longer. This could also be a reason for the differences between measurements and modelled cloud. So to say, you miss the fallstreaks, i.e. the large ice crystals falling from the top of the cloud far below into subsaturated air. To overcome this problem, a real column model would be necessary (if not already done). However, to check the impact of pre-existing ice, fallen down from below, it might be reasonable to include a sedimentation flux through the top of the box in order to allow 'falling' ice crystals to quench ice nucleation in lower parts of the vertical profile.

Minor points:

1. Adiabatic change of temperature:
it was indicated that the box model is driven by temperature and pressure values, derived from trajectory calculations. Are these values directly used or are they recalculated? Generally, there is a problem by using these values directly, because they are not consistent with the assumption of adiabatic changes along the trajectory, as needed for box model calculations. Actually, the MODEL field temperature is used as trajectory output, thus diabatic source terms are included, which might influence the model calculations. Please clarify this issue.

2. Resolution of ECMWF vs. resolution of trajectory calculations:
In section 2.3 a resolution of $1^\circ \times 1^\circ$ of ECMWF data was indicated, whereas the trajectory output shown in figure 2 is given at resolution $0.2^\circ \times 0.2^\circ$. How does this fit together?

3. Setup of small-scale variations:
In section 2.3 it is stated that the peak-to-peak variation of the temperature noise has a typical length of 10 min. What is the physical basis for this setting?

4. Statistical error from different realisations:
It would be nice to indicate the statistical spread of the different realisations of trajectories in terms of different temperature fluctuation sets. Could you give some information about that? And from this point of view a setting of just 2 realisations is maybe too small, also for a case study.

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12, C5663–C5667, 2012

5. Mixing timescale:
In section 2.3 the mixing timescale of air parcels was mentioned without any further specification. Could you indicate the physical background in a few sentences?

6. Missing idealized runs with pure homogeneous nucleation:
In section 4.1.2 idealized simulations are shown. However, idealized runs prescribing just pure homogenous nucleation are missing and might be useful as comparison in figure 7. Additionally, I miss a qualitative (or even quantitative) comparison of the heterogeneous/homogeneous nucleation competition with former studies on this field (e.g. Kärcher et al., 2006; Gierens, 2003; Spichtinger & Cziczo, 2010). It should at least be mentioned that the results are quite similar to former studies

7. Vertical velocity estimations:
It is not clear to me if the existing trajectory calculations were used for deriving vertical velocities (at least on the large-scale). Since the trajectories are available, this might be a good corroboration of the idealized study using a prescribed vertical velocity in a certain range.

Technical comments:

- Page 15686, line 15: 'few' instead of 'view'
- Figure 6 is very hard to read, please change colours and use much larger dots.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15675, 2012.