We would like to thank A. Ansmann for taking the time to comment on our paper and for drawing our attention to his own interesting publications on similar subjects. Below, we respond to the points which he raised.

"The recent paper of Seifert et al. (JGR 2011, now available at the JGR webpage, paper in press) should be mentioned. Could be done already in the introduction. This paper deals with the influence of the Eyjafjallajökull volcanic ash on heterogenous ice formation in 'real world' tropospheric clouds. The study is based on lidar observations at Leipzig and Munich in April 2010. I recommend to discuss the observations and compare the findings with your laboratory studies."

Reply: We have now cited you paper in the introduction as you recommend.

"One question: How can freezing temperatures of about -10C be explained by immersion freezing? According to the study of Fornea et al. (2009), temperatures of <-17C are required for immersion freezing to take place. Contact freezing may start at -8 to -10C. Please discuss!"

Reply: As we discuss in our paper, the IN which cause nucleation at this temperature are very rare, of the order of 1-10 per sample. Once the sample is diluted, the freezing temperature drops significantly. Fornea et. al. used about 5 single droplets, containing single aerosol particles, and would never have seen this behaviour.

"Sections 4.1.1, 4.1.4, and 5: IN concentrations (INC) and the relationship to aerosol particle concentration (APC, coarse mode) are discussed. Seifert et al. (2011) also present results in this respect (INC, APC, APC/INC), and thus give numbers for INC at different heights after the Eyjafjalla eruptions in April 2010."

Reply: We discuss the aerosol particle concentrations which were measured by instruments on-board aircraft in the ash plume and relate these to our laboratory measurements of the IN properties of ash. These aircraft based measurements are direct observations of the ash plume and are the most accurate way in which the characteristics of this plume were quantified. The modelling carried out and described by Seifert et al. are interesting, however given the enormous uncertainty already in the initialisation of such a model, we would prefer to stick with the direct observations described in our current manuscript.

"Sections 4.4, 4.5, and 5: The lidar observations in Seifert et al. (2011) indicate: there was ash everywhere in the troposphere up to the tropopause over central Europe during the first week after the strong eruptions in April 2010. Thus entrainment of dry ash particles (triggering contact freezing) into cloud layers was always possible. Thus, not only immersion and deposition freezing may have played a significant role. Cloud cover (mid and upper tropospheric clouds) was most probably increased due to the presence of ash according to the lidar observations. This is also mentioned by Seifert et al. (2011)."

Reply: We found that it is very unlikely that the ash particles were dry. Large amounts of sulphur species were released during the eruption, and observations in the ash plume show raised concentrations of sulphate aerosol. Some of this would have also partitioned to the ash. We mention this when discussing our deposition freezing results. As these coated particles approach a supersaturated (cloud) environment, they will take up water. We therefore believe that contract freezing is unlikely to have a large impact on ice cloud formation in this case. Contact freezing may be possible if many large particles are sedimenting through a cloud layer, but by the time the ash plume had reached Europe, rapidly sedimenting particles would already have been lost.

“In conclusion, our experience (based on lidar observations during SAMUM 1, pure Saharan dust impact on cloud ice formation, Ansmann et al., JGR 2008, and now after the Eyjafjalla eruption) tells us that ash is at least as good as dust (if not better) regarding the initiation of ice nucleation. This is in agreement with the impression of Bingemer et al., ACPD 2011. Could be mentioned.”

Reply: We appreciate these findings by Seifert et al, however we have very comprehensively looked at the IN properties of the ash and compared it with various mineral dusts in our laboratory, where quantities like particle concentration and freezing temperature can be measured relatively exactly. However, we find no indication that the ash is anywhere near as good an IN as the mineral dusts we have examined. These results are confirmed with a different technique in the manuscript of Steinke et al (ACPD 2011).

Once again, thank you for your comments.