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Interactive Comment

Interactive comment on "The impact of a deep convection on sulfate transport and redistribution" *by* V. Spiridonov and M. Curic

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I find this paper improved with respect to the first version entitled "The application of cloud model in the air quality assessment", but not yet in publishable form. Based on the work that the authors have done already, I believe that they can deliver to the scientific community an important contribution in a very short while. The problem addresses by this paper is of great interest, both for convective cloud and large scale simulations, but it is treated and presented ambiguously, and the results are still scarce and not convincedly. In the abstract, first phrase, it is declared that a "three dimensional compressible cloud model" is used to achieve the purposes of the paper but later, on page 395 lines 15 to 20 and on page 402 from line 25 to line 10 on page 403 , the authors claim the use of a 2D cloud model too - the reason given for its using is not clear, why they cannot use the 3D model to compute the parameters presented in Table 7? Then,

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the main objective of the paper is "to study the impact of deep convection on Ě"(page 386, line 14) is a general formulation which does not say much. Reading the paper, I identified two main objectives: 1) to test the influence of microphysical redistribution of tracer between the cloud phases when the ice phase is included and the influence of chemistry (equilibrium or kinetic approach, SO2 oxidation) which is a sensitivity study and 2) to test the model results against measurements in two meteorological conditions (3 April 2000 and 6 July 1995) which is a case study. I recommend to the authors to rewrite the paper (abstract included) presenting these very clearly and adding also results on gas phase evolution of the pollutants (integrated mass of pollutant between 4 km and the model top - that will facilitate the comparison with other works already mentioned in the paper: Crutzen and Lawrence (2000), Barth et al. (2001), Yin et al., (2001)). Also, I suggest to the authors to represent on the same graph the differences between the fields represented in the left and right panels of Fig. 8 and 13. In the present representation the differences between the left and right fields are invisible. The Fig. 11 and 12 can be eliminated and the model results can be compared with measurements only for the day of interest or with some statistics of measurements for the simulated conditions. An other aspect that the authors should clarify is the terminology used for the case study: in the abstract the two days chosen for simulations are "an intensive convective cloud activity" and "transboundary dust transport and wet deposition", and at page 404 for example the discussion is made for "continental non-polluted and polluted clouds". There are many others inconsistencies in the paper that I consider implicit that the authors will eliminate them when they will revise the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 385, 2002.