

Interactive comment on “Exploring atmospheric boundary layer characteristics in a severe SO₂ episode in the north-eastern Adriatic” by M. T. Prtenjak et al.

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We would like to thank to Reviewer 2 for the constructive comments and suggestions.

Remark (2.1) The authors investigate a severe pollution episode in the Rijeka area by applying the EMEP model as well as fine resolution mesoscale meteorological models. Since the results of MEMO seem not to be significant for the discussion, the mention and description of MEMO should be skipped.

Reply (2.1) Accepted.

Remark (2.2) With respect of the EMEP results it is necessary to show the contribu-

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tion of local sources to the SO₂ concentrations simulated for the Rijeka region. The analysis of the mesoscale meteorological situation within the paper shows thoroughly made simulations with WRF and very nice results. However, the conclusions are quite limited due the lack of simulated SO₂ distributions. Some assumptions (e.g. the assumed entrainment of high SO₂ from aloft on Feb. 3) could only be maintained if this assumption were supported by simulated SO₂ concentrations. The results of the WRF simulation indicate a regional meteorological situation that is indeed very favourable for the accumulation of high SO₂ concentrations. But as long as there are no chemistry transport simulations included, there is no proof that this meteorological situation will definitely result in that high SO₂ concentrations.

Reply (2.2) Accepted. The role of the small scale meteorological conditions in the establishment of air pollution episode will be further corroborated with results of the air quality models at 10 km (EMEP4HR) and 1 km (CAMx) horizontal resolutions, respectively. For more details, please see our answers to Reviewer 1 (Reply 1.1 and 1.6) and Appendixes 1 and 2.

Remark (2.3) Furthermore, there is no evidence given in the paper whether the simulated mesoscale features are unique and different from other stagnant situations which did not result in such a severe pollution situation. In the introduction the authors mention that weak wind speed and calms are frequent in the region, but are not necessarily indicators of severe pollution episodes. Therefore, the authors should discuss between this particular situation in comparison with other stagnant situations without very high SO₂ concentrations.

Reply (2.3) Please see Reply (1.2) and Reply (3.1).

Remark (2.4) The quality of figures 6 – 11 is quite poor and the figures are too small. Arrows for the observed wind could be included into the figures of the simulated wind. This would permit larger figures and also a better comparison of observations and model results.

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Reply (2.4) Accepted. The quality of the figures 6-11 will be improved in the revised paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6283, 2009.

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