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

Anonymous Referee #3

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The paper analyses a severe episode of air pollution related to sulphur dioxide in the coastal industrial town of Rijeka, Croatia, by using air quality modelling and especially focusing in high-resolution meteorological model. Although the paper is well organised and detailed and the methods represent the state-of-the-art, there are some aspects of the paper that require a major revision. My main comments are detailed below.

The authors have focused on just one typical situation and have not analysed other episodes or situations leading to air pollution episodes; they have not established and/or revised in depth the meteorological and air quality dynamics in the area. It would be nice to find out how results of just one situation can be extrapolated to other

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episodes or annual situations. If not, the results may not be evident and permit an ambiguous interpretation of the results. I recommend that they should be more specific concerning the meteorology and pollutants dynamics and the influence of local source emissions in Croatia and specifically in the area of study. That would strengthen the results and the contribution of the manuscript.

Furthermore, the simulations carried out with the MEMO model are presented but not discussed in detail and do not contribute to the description of the meteorological situation, so I would recommend that the mention to MEMO should be skipped from the manuscript.

Other minor comments are related with the writing of the manuscript. A revision of minor aspects related to English should be addressed by a native speaker. Also, the quality of figures 6 to 14 should be improved (at least the axis should be readable).

My main concern is about the lack of high-resolution air quality modelling in this very complex zone. The 50 km resolution of the EMEP model is not fine enough to describe air pollution dynamics in the area and its results (SO₂ fields or concentrations) are not shown in this paper. In addition, in the area studied the external contribution of pollution may be important, and therefore the definition of how this contribution is taken into account becomes essential.

The authors state that SO₂ modelling will be shown in another paper, so I would transfer EMEP modelling discussions to that other paper and would focus on meteorological parameters and simulations. It would be nice to submit both papers (meteorological and air quality modelling) together for consideration. If the authors consider submitting just one paper, in this manuscript there are no sections or paragraphs describing the skills of the EMEP model for reproducing the air pollutant concentrations (biases, errors, normalized bias, root mean square errors, etc). Furthermore, there are model intercomparison exercises showing skill scores for several European models within the CityDelta intercomparison exercises (Van Loon et al., 2007; Vautard et al., 2007; Cu-

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velier et al., 2007, among others) and therefore a brief section describing analogous statistical discussion should be presented. The authors may use the US EPA (1991; 2005) indicators and/or reference values by the European Daughter Directives for air quality modelling uncertainty in order to have an idea of the ability of the model for reproducing air quality-related phenomena.

Therefore, it is difficult to judge the quality of the manuscript if the SO₂ air quality simulations do not support all the hypothesis set in this manuscript when meteorological experiments are performed.

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

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