

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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Received and published: 2 June 2009

We would like to thank to Reviewer 3 for the constructive comments and suggestions.

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 highresolution meteorological model. Although the paper is well organized 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.

Remark (3.1) The authors have focused on just one typical situation and have not analyzed other episodes or situations leading to air pollution episodes; they have not

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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 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 strength the results and the contribution of the manuscript.

Reply (3.1) According to our knowledge, annually, a few episodes with the high levels of SO₂ (daily mean between 130 - 143 $\mu\text{g m}^{-3}$) occur, and they always occur during the wintertime (e.g. 15 December 2006, 6 January 2007, 20 January 2008; <http://zrak.mzopu.hr/default.aspx?id=22>). Nevertheless, the investigated episode is characterized by unusually high SO₂ concentrations (daily concentration over 350 $\mu\text{g m}^{-3}$; Figure 3), and thus, it could not be considered as a typical (please, also see Reply 1.2). Additionally, in the revised version, results of air quality models at resolutions of 10 and 1 km (for more details please see second paragraph of the Reply 1.1) will further support our conclusions regarding the major role of the local sources and small scale meteorological conditions in the episode occurrence .

Remark (3.2) 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.

Reply (3.2) Accepted.

Remark (3.3) 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).

Reply (3.3) Accepted. The quality of the figures will be improved.

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Remark (3.4) 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.

Reply (3.4) In the revised paper the high-resolution air quality model is employed (please see Reply (3.1) and Reply (1.1) and Appendixes 1 and 2).

Remark (3.5) 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; Cuvelier 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.

Reply (3.5) The development and verification of the smaller scale chemistry model EMEP4HR is beyond the scope of this paper (please also see first paragraph of the Reply 1.1). Nevertheless, for the information, in the Appendix 4 are some statistics for all three models. It is obvious that the agreement between the modeled and measured

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concentrations for the severe episode increase with the model resolution. This also implies that the episode was mainly caused by nearby, local sources.

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

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Appendix 4: Statistic indices for the SO₂ concentrations.

	Mean values ($\mu\text{g m}^{-3}$)	Maximum values ($\mu\text{g m}^{-3}$)	Correlation coefficient	RMSE	d-index of agreement
EMEP 50km	2.24	6.14	0.18	292.3	0.54
EMEP4HR 10km	18.72	45.35	0.51	276.5	0.56
CAMx	241.01	780.62	0.53	171.4	0.72
Measurements	242.42	597.50	1.00	0.00	1.00

Fig. 1.

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