

## ***Interactive comment on “Transport and dispersion of atmospheric sulphur dioxide from an industrial coastal area during a sea-breeze event” by C. Talbot et al.***

### **Anonymous Referee #2**

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Talbot et al. present a combination of SO<sub>2</sub> measurements and mesoscale numerical modelling to study the dispersion of SO<sub>2</sub> from an industrial source in a coastal area that is dominated by a sea-breeze event. They only present data and model results for a single day which is a pity as I'm sure under different synoptic conditions other dispersion effects might be important so that one would gain a better impression of the relevance of this land-sea breeze circulation. This paper is a follow-on to a paper that has been published in Boundary Layer Meteorology (Talbot et al, 2007, TBL07 in the remainder of this review); in the current manuscript they expand their previous discussion by using SO<sub>2</sub> as a tracer. As I will explain below, the treatment of SO<sub>2</sub> in the current study is effectively that of a passive tracer and here they are presenting only

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data from one of the 4 days that were discussed in the BLM manuscript where they also discussed the transport of a passive tracer. I don't feel that the current manuscript advances significantly beyond TBL07 and furthermore it has quite a few severe weaknesses (see below). Therefore I recommend to reject this paper in its current form.

### Major comments

I will not comment on the discussion of the general effects of sea-breeze as described in this manuscript as they seem to basically be a repetition from TBL07.

Choice of study date: From the information given in sections 2 and 3 of the paper I understand that the SO<sub>2</sub> data is from an air quality monitoring network so data should have been available for the whole study period of TBL07 so why was only one day chosen for the current study?

Data available for comparison: In addition to - from my understanding - unnecessarily restricting the number of study days the available data for a model - field comparison for SO<sub>2</sub> is very sparse. No vertical information is available which would be crucial to test the effect of the sea breeze on the SO<sub>2</sub> distribution as it is predicted by the model. How do we really know if the model performs well, especially when the distribution of SO<sub>2</sub> is dominated by vertical transport? The only direct comparison of field SO<sub>2</sub> data with modelled SO<sub>2</sub> is given in figure 5 where the numbers have been scaled because the emission inventory for SO<sub>2</sub> is so poor that the absolute numbers apparently differ dramatically. Also the timing of the occurrence of the SO<sub>2</sub> plumes at the stations is in my opinion not as well reproduced as claimed in the paper.

Chemical reactions included in the model: Even though the heterogeneous chemistry of SO<sub>2</sub> is mentioned in section 5.3 (with some errors though as detailed below), it was apparently not included in the model (p. 16005, l. 10-11), so why talk about it? As the gas-phase oxidation of SO<sub>2</sub> is very slow on these timescales, SO<sub>2</sub> effectively is a passive tracer (including dry deposition) in this model setup. This is confirmed when comparing figure 7 of this manuscript with figure 16c in TBL07 which look very similar

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to me like: basically like a 2D and 3D depiction of the same basic process.

This brings me to a fairly irritating point: In addition to these two very similar plots, figures 1, 2, 3, 8c of this manuscript look exactly like figures in TBL07. In addition to bad scientific style this - if they really are the same figures as in TBL07 - is a violation of Springer's copyright on TBL07, as no reproduction permissions are listed in the current manuscript.

Chemistry part: It is not at all clear what the chemical mechanism is used for and what it comprises (details see below). On p. 15999, l. 18-20 you mention the "aqueous-phase chemistry scheme" but according to p. 16005, l. 10-11 no aqueous phase chemistry is included - please clarify. What gas phase reactions that are of relevance for this study are included? You don't show any results from the chemistry part of the model other than SO<sub>2</sub> concentrations which have severe weaknesses as mentioned above and below. If you don't include heterogeneous chemistry, then the only relevant reaction is SO<sub>2</sub> + OH which immediately leads to the question: what are the OH fields in the model based on?

### Minor comments

p. 15992, bottom - 15993, 1st para: The derivation of the source strength is very weak. This by itself would be a reason for me to only study an idealised passive tracer.

p. 15993, l. 3. (also p. 16006, l. 13): The pH of a solution is NOT abbreviated as "PH".

p. 15999, l. 10: It is unclear to me what type of initial and boundary conditions the ARPEGE simulations were used for - meteorology or chemistry? If for meteorology, why didn't you use ECMWF data which might be more suited as this is based on operational data.

p. 15999, l. 22.: What do you mean by "low concentrations", what are they based on, and what are they? Please list.

p. 16003, l. 14 and reaction (1.1): HSO<sub>3</sub> should NOT be an ion as you are talking

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about the gas phase.

p. 16003, l. 21 - 23: The number of aerosol particles can be increased only by nucleation of new particles which does occur but which you don't mention at all. These lines are completely unclear.

p. 16004, top:  $\text{H}_2\text{O}_2$  might well be the dominant aqueous oxidant for  $\text{SO}_2$  under these conditions so I don't understand why you don't discuss it. Ozone would be relevant if sea salt aerosol were present which would be likely in a sea-breeze event but which you don't even mention.

p. 16004, reaction 4.3: Technically this should be an equilibrium reaction but under atmospheric conditions it only goes in the opposite direction of what is indicated by the arrow.

p. 16005, l. 3 - 10: In general this is a very well-known point. It is not clear to me how you can use model results to show its importance for the  $\text{SO}_2$  field esp. when you don't include aqueous phase chemistry.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15989, 2007.

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