

***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

Received and published: 7 February 2008

I want to briefly respond to a few points raised by the authors in their reply to my comment. Firstly, the tone of my review was direct, which I don't think is inappropriate, however if the authors felt insulted in any way I do apologise for that.

Talbot et al., 2008: "The main reproach is that the current ACPD article would be a duplicate of our previous paper published in Boundary-Layer Meteorology (BLM)"

Reply by reviewer 2: I did not say that it was a duplicate, I said that "I don't feel that the current manuscript advances significantly beyond TBL07". After having read the reply, I don't see a reason to change this view.

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T08: "In this study, SO₂ is only used as a tracer but the post-breeze reservoir mechanism opens new questions about the associated chemistry. We thus discuss the potential chemical mechanisms but the whole chemistry is out of the scope of this study"

Reply: This was not clear from the manuscript, which rather left, at least in most parts of the paper, the impression that a full chemical scheme was used. You state in the manuscript that you use the chemistry version of MESO-NH which to me implies that you are actually looking at the chemistry esp. when you spend so much time on actually explaining the chemical reaction mechanism. On p. 15999, l. 18 you even explicitly write that chemical reactions were being calculated. So - what is actually being done in the model?

T08: "it is clear that our goal is not to give climatology of pollutants under sea-breeze events"

Reply: I did not suggest to do a "climatology of pollutants", I rather asked for some more details and model - data comparison esp. in the vertical which is crucial to evaluate your results.

T08: "Concerning the chemistry part: The H₂O₂ reaction with gaseous HSO₃ could have been presented in the paper but wasn't since this specie isn't abounding in the atmosphere."

Reply: I was referring to your comment on p. 16004, l. 2 about AQUEOUS phase oxidation of S(IV) by H₂O₂ which is in the polluted boundary layer the key oxidation mechanism for S(IV).

T08: "It can be effectively important to mention sea salt aerosols. These aerosols contain Cl and Br- ions which can activate with contact of halogens. Halogens catalyse loss of ozone concentrations and degradation of VOC (Adams and Cox, 2002)."

Reply: The key point about sea salt aerosols and sulphur oxidation is the low acidity of

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fresh sea salt which makes the $O_3 + S(IV)$ reaction pathway very important.

These last two points should already have been clear from my "Minor comments".

In summary, after having read the reply, I am even more unsure as to what chemical processes have actually been included in the model. It seems that the whole discussion on the chemistry was only meant as motivation for the study and then should rather have been in the introduction. From the reply of Talbot et al it seems that the main difference between the current and the previous study was the duration and location of tracer release and the fact the dry deposition was included in the current study. If that is really the main difference - why did you not state this clearly in the manuscript?

Again: it is still not clear to me in what obvious sense this study advances on the BLM paper and maintain my recommendation to reject the paper in its current form.

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

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