

## ***Interactive comment on “The effects of a solar eclipse on photo-oxidants in different areas of China” by J.-B. Wu et al.***

**Anonymous Referee #1**

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Review of manuscript: “The effects of a solar eclipse on photo-oxidants in different areas of China” by Wu et al.

The manuscript investigates the effects of the total solar eclipse of 22 July 2009 on surface ozone and other photo-oxidants over central China using the WRF-Chem model. Overall, it is well structured and written. It is interesting the use of a coupled chemistry-meteorology mesoscale model for studying the effects of a disturbance like solar eclipse on ozone and photooxidants. It merits publication to ACP and I would suggest acceptance of the paper after taking into consideration the following comments.

Comments

a) In Introduction, second paragraph: The previous relevant literature is not addressed

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properly and there are mistakes. For example the authors refer to the article by Fabian et al (2001) and the reader stays with the impression that this article refers to the previous sentence which is for the solar eclipse at Thessaloniki on 11 August 1999. Furthermore they authors refer to the solar eclipse at Thessaloniki on 11 August 1999 and give reference an article for the same solar eclipse event but for Athens (Tzanis, 2005). Do they mean the Zanis et al., 2001 study for Thessaloniki at Atmospheric Environment which is refereed at the reference list but not cited within the text? In order to avoid misunderstandings for the reader, I suggest the authors to be more specific for each of one of these cases and not mixing them up. In addition there are other similar previous studies which it is worthy of mentioning them as they are among the first investigating this topic, such as by Srivastava et al., 1982, Abram et al., 2000. Abram, J. et al.: Hydroxyl radical and ozone measurements in England during the solar eclipse of 11 August 1999, *Geophys. Res. Lett.*, 27(21), 3437-3440, 2000. Srivastava, G.P. et al.: Ozone concentration measurements near the ground at Raichur during the solar eclipse of 1980. *Proceedings of Indian Natural Sciences Academy*, A48(3), 138-142, 1982.

b) In Section 2: The authors should specify what is the resolution of their model simulations. I could not find it within the text. Furthermore there is no description for the emissions used in their simulations. A description of the emissions is needed. Finally within this section a more detailed description is needed for the parameterisation of the eclipse within their simulations.

c) Page 2480, line 21: The authors state that the observed ozone at Tongcheng shows a decrease due to eclipse of 5-10 ppbv. To my eyes this is not justified from Figure 4b which indicates that the observed ozone decrease is only a few ppbv (not more than 5 ppbv). Furthermore I would suggest the authors to add a few sentences within Section 2.2.3 for the comparison of simulated versus observed ozone decrease and NO<sub>2</sub> increase seen in Figure 4.

d) The authors use UTC throughout the text but at the Figures 2, 3 and 4 they use LST.

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I think it would be better to use a uniform time frame whether this is LST or UTC.

e) The authors in Section 3.2 conclude that the downward flow may bring pollutants down to the surface, resulting in an increase in CO concentration at the surface and decrease in the layer above in both polluted and clean areas. They justify this conclusion by the negative vertical wind velocity seen in Figure 7a. My question is if the contour lines of Figure 7a and 7b represent the vertical wind velocity in the Eclipse simulation or represent differences between Eclipse and NoEclipse simulations? It is important to note that the simulated changes in CO (increase below and decrease above) can be also justified by the changes in the boundary layer height. The mixing and dilution of CO within a reduced boundary layer during Eclipse will result to changes like CO increase below and decrease above. A note on how much the boundary layer height is reduced in their simulations will be helpful and added value for the discussion of simulated CO changes.

f) Page 2485: The reader that goes through Reactions R4 to R9 stays with the impression that HO<sub>2</sub> is mainly produced through HCHO photolysis which is a small fraction to the HO<sub>2</sub> budget. It should be mentioned that HO<sub>2</sub> and RO<sub>2</sub> are mainly produced through the oxidation of CO and hydrocarbons by OH.

g) Figure 3: I think it would be helpful if the authors add for its site of Figure 3 a number with the percentage obscuration.

h) Figure 4: I would suggest the authors to use the same scale in Figs 4a and 4b for ozone. The same stands for Figures 4c and 4d for NO<sub>2</sub>.

Technical comments 1 ) Page 2474, line 16: Close the parenthesis after NO<sub>3</sub>. 2) Page 2476, line 18: "And this provides ..." should be rather "This provides ..." 3) Page 2377, line 9: I would suggest "fully coupled instead of "fully consistent". 4) Page 2480, Section 2.2.3: A reference to Figure 4 is missing. 5) Page 2481, line 15: "And there an addition zone ..." should be rather "In addition there is a zone ...". 6) Page 2481, line 26: It should be rather "from a combined effect". 7) Page 2482, line 17: "dynamic

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process" should be rather "dynamical process". 8) Page 2484, line 25: "hydrogen peroxy radicals" should be rather "hydroperoxy radicals"

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