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

**J.-B. Wu et al.**

zifawang@mail.iap.ac.cn

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We thank referee 1 for reviewing our paper and giving valuable comments. The following are our detailed responses to the each comment:

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

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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.

Reply to comment a: We agree that the sentences about the previous studies were a little bit misleading because of inappropriate expression. And there are some other previous works, which is worthy of mentioning in this paper. We decided to revise the second paragraph in Page 2475 as: "Solar eclipses also enable the evaluation of the response of the gas-phase chemistry of photo-oxidants during a drastic perturbation in solar radiation. Plausible variations in stratospheric composition caused by solar eclipses have been addressed by some studies (Mims and Mims, 1993; Zerefos et al., 2000; Gogosheva et al., 2002; Gerasopoulos et al. 2008). There are also some studies focusing on the effects on tropospheric ozone and other photo-oxidants. A decrease of surface ozone concentration was observed during some solar eclipse events (Srivastava et al., 1982; Tzani, 2005; Zani et al., 2001). In the solar eclipse of 11 August 1999, there was a lag-time between the maximum of the eclipse and the maximum of the induced ozone decrease (Zerefos et al., 2001). Abram et al. (2000) did hydroxyl radical and ozone measurement during the solar eclipse of 1999, showing that the OH concentration was well correlated ( $r=0.88$ ) to its rate of primary production from ozone photolysis. In the same event, the partitioning of NO<sub>x</sub> between NO and NO<sub>2</sub> is determined almost exclusively by variations in JNO<sub>2</sub> (Fabian et al., 2001). More recently in the total solar eclipse of 29 March 2006, observations and modeling show that there is a significant decrease in O<sub>3</sub> and NO and an increase in NO<sub>2</sub> at polluted sites, while there is no clear impact on these pollutants at the unpolluted sites (Zani

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et al., 2007). The decrease in the surface ozone concentration that observed after the beginning of the eclipse lasted two hours, probably due to the decreased efficiency of the photochemical ozone formation (Tzanis et al., 2008).”

Comment b: 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.

Reply to comment b: The Authors agree that the resolution and the emission of the simulation should be specified. We add the corresponding text in Section 2.

(1) We revised the text in Page 2477, line 14-16 (“The WRF-Chem model used ... simulate atmospheric conditions:”) as “The WRF-Chem model used in this study has a vertical structure consisting of 27  $\sigma$ -levels extending from 1000 to 50 hPa, with a resolution of 81 km in the coarse domain and 27km in two nested domains (Fig. 1b). The following model parameterizations have been chosen to simulate atmospheric conditions:” (2) The emission description information will be added after the second paragraph in Page 2478. “The Sparse Matrix Operator Kernel Emissions (SMOKE) model is applied to deal with the emissions inventory and provide grided emission data for the WRF-Chem model. Two emission inventories are considered in this study: the regional emission with 10km resolution update from TRACE *p*emission inventory according to personal communications (Streets et al., 2003); the power plant emission data from IN *B*, with a horizontal resolution of  $0.5^\circ \times 0.5^\circ$  (Zhang et al., 2009). In order to obtain better distribution of emissions, the area emissions of East Asia have been spatially allocated based on related spatial factor, such as its population data from Land Scan 2005 Global Population Database. The road dust emission is spatially distributed according to the road length density.” (3) The Referee 1 also mentioned that a more detailed description is needed for the parameterisation of the eclipse within their simulations. And we decided to revise the method of the solar eclipse experiment,

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which could be referred to the Reply to Referee 2.

Comment c: 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.

Reply to comment c:

According to Referee's comments, we revised the last paragraph in Page 2480 as following: "The performance of the Eclipse experiment for atmospheric pollutants is demonstrated for surface O<sub>3</sub> and NO<sub>2</sub> at Hefei and Tongcheng, which are located in the path of the total eclipse and are characterized by different air pollution levels. The observation showed that during the eclipse hours, the surface ozone displayed a decrease of around 20 ppbv in Hefei, while at the relatively unpolluted site of Tongcheng the decrease of the surface ozone was much smaller. The measured NO<sub>2</sub> showed an increase of at most 10 and 3 ppbv in Hefei and Tongcheng respectively. Compared to the decrease of the observed ozone, the result of Eclipse experiment at the polluted site of Hefei showed a similar decrease pattern but in lower magnitude, with the maximum decrease of approximately 10 ppbv (Fig. 4a). The maximum increase of NO<sub>2</sub> at this site of Hefei presented in Eclipse run (Fig. 4c) was about 5 ppbv, half of the change of observed NO<sub>2</sub>. This bias may be related to the coarse resolution of the model, which may underestimate emissions at polluted sites. At the relatively unpolluted site of Tongcheng, the Eclipse run matched very well with observations both in magnitude and pattern. In general, Both measurement and model simulation in the Eclipse experiment showed a very similar pattern in surface O<sub>3</sub> and NO<sub>2</sub>, while the NoEclipse run showed steady increases or decreases typical of normal conditions. The Eclipse run can mostly simulate the basic features of atmospheric pollutants during the solar eclipse."

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Comment d: d) The authors use UTC throughout the text but at the Figures 2, 3 and 4 they use LST. I think it would be better to use a uniform time frame whether this is LST or UTC.

Reply to comment d: Because the whole solar eclipse of 22 July 2009 covered the large range of longitude, which have different time zone, we thought it was not appropriate to use LST only. In addition, it is better for understanding of reader to use LST in the Figures. Therefore, we decided to use Beijing Time (BJT) both in the text and figures: 1) Page 2476, line 16: revise “00:56UTC” as “08:56 BJT (Beijing Time)” 2) Page 2476, line 17: revise “ 03:04UTC” as “11:04 BJT” 3) Page 2478, line 23: revise “(00:55–04:15 UTC)” as “08:55-12:15 BJT” 4) Page 2481, line 11: revise “01:00–02:00UTC” as “09:00-10:00 BJT” 5) Page 2483, line 13: revise “00:00–05:00 UTC” as “08:00-13:00 BJT” 6) Page 2492, line 2: revise “00:00–05:00 UTC” as “08:00-13:00 BJT” 7) Page 2492, line 1: revise “00:00UTC” as “08:00 BJT” 8) Page 2498, line 3: revise “01:00–02:00UTC” as “09:00-10:00 BJT” 9) Revise the time labels of the figures with BJT.

Comment e: 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.

Reply to comment e: The referee’s question is “if the contour lines of Figure 7a and 7b represent the vertical wind velocity in the Eclipse simulation or represent differences

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between Eclipse and NoEclipse simulations?” Yes, the contour lines represent the difference between Eclipse and NoEclipse simulations in vertical wind velocity. The Referee also mentioned that the simulated changes in CO (increase below and decrease above) can be also justified by the changes in the boundary layer height. We agree with the Referee and add the change of boundary layer height in Fig. 7 instead of vertical wind velocity. And the corresponding text (Page 2484, line 18), “. . . in both polluted and clean areas. Although the change. . .” is revised as: “. . . in both polluted and clean areas. This could be approved by the change of boundary layer height between Eclipse and NoEclipse simulations (Fig. 7). The boundary layer height was suppressed during the eclipse period. Thus the mixing and dilution of CO within a reduced boundary layer during Eclipse will result to changes like CO increase below and decrease above. Although the change. . .”

Fig. 7. The altitude-time cross sections of the differences between model simulations (Eclipse – NoEclipse) in CO, domain-averaged over the polluted (a) and clean (b) areas. Dashed lines show the period of maximum solar eclipse in Beijing.

Comment f: 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.

Reply to comment f: We changed the whole text in Page 2485 to: “eclipse hours. It is worth noting that the change of these three radicals in polluted areas is smaller than in clean areas, which is opposite of the changes seen for atmospheric pollutants. That is because the major production of OH is from ozone photolysis (O<sub>1</sub>D + H<sub>2</sub>O) in the clean area, while the HO<sub>2</sub> + NO reaction is the most important contribution in urban air (Shao et al., 2004). Thus the loss of OH in the clean area is more significant due to limited ozone photolysis (O<sub>1</sub>D + H<sub>2</sub>O) during the solar eclipse period. In addition, Kanaya et al. (2009) showed that CO+OH, RO<sub>2</sub>+NO, VOCs+OH and HCHO+hv reactions accounted for most of the HO<sub>2</sub> production. During the solar eclipse when

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nighttime chemistry dominates, these reactions become less, resulting in loss of HO<sub>2</sub> concentration especially in clean area where CO, NO concentration is lower. Although the change in radical concentration in the clean area is larger during the solar eclipse period, the difference becomes negligible soon after the eclipse. In the polluted area however, the effect of the eclipse on radical concentrations is still clear after eclipse. The impact of solar eclipse in the polluted area lasts longer than in the clean area, which is consistent with the finding discussed above. The possible reason is that some paths to HO<sub>x</sub> production are cut off in the polluted area during the solar eclipse event, which should not be healed easily even the radiation recovers”

Comment g: 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. Reply to comment g: We agree with the Referee that it's helpful to add the percentage obscuration for each site of Figure 3. The revised figures will be presented in the revised paper.

Comment h: 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>. Reply to comment h: We agree with the Referee that it's more favorable to set the same scale for different sites. The revised figures will be presented in the revised paper.

Technical coments: 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 process” should be rather “dynamical process”. 8) Page 2484, line 25: “hydrogen peroxy radicals” should be rather “hydroperoxy radicals” Reply to Technical comments: Thanks for Referee's careful review. It's more appropriate to modify the following places as the Referee mentioned: 1) Page 2474, line 17: Add a parenthesis after NO<sub>3</sub> as “HO<sub>2</sub> and NO<sub>3</sub>)” 2) Page 2476, line 18: change “And this provides” for “

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This provides” 3) Page 2377, line 9: revise “fully consistent” as “fully coupled” 4) Page 2480, add a reference to Fig. 4 in Section 2.2.3: 5) Page 2481, line 15: revise “And there an addition zone” as “In addition, there is a zone” 6) Page 2481, line 26: revise “resulted from combined effect” as “resulted from a combined effect” 7) Page 2482, line 17: revise “dynamic process” as “dynamical process” 8) Page 2484, line 25: revise “hydrogen peroxy radicals” as “hydroperoxy radicals”

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C2997/2011/acpd-11-C2997-2011-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 2473, 2011.

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