

Dear referee #2,

We thank you for your time on our paper. The comments and suggestions are useful to improve the quality of the manuscript. Herein we present the replies to all the comments on our manuscript named “Effects of black carbon and boundary layer interaction on surface ozone in Nanjing, China”

Referee#2

(1) Fig. 2 is too crowded. In order to better evaluate the model simulation in details, the authors should enlarge the figure. I suggest it can be separated to 2 figures. One figure only contains meteorological parameters (Fig. 2a) and another is for chemical species (Fig. 2b).

Reply: Follow the referee’s comment. The figure 2 is separated to 2 parts, Figure 2a only contains the meteorological parameters and Figure 2b only contains the chemical species. Please check the new Figure 2 in the revised manuscript.

(2) Fig. 3 has a similar problem. The Fig. 3C is impossible to read. It should be an individual panel.

Reply: Follow the referee’s comment. Figure 3 is rearranged, Fig. 3a and 3b are set as a panel and Fig. 3c is set as an individual panel. Please check the new Figure 3 in the revised manuscript.

(3) Why there is a consistent heating by BC around 1.2 km, especially at 10am. If it is due to residual layer of BC, the authors should explain it in more details.

Reply: Thank you for your comment. The consistent heating above BL is exactly due to the impacts of BC in the residual layer. We have added the explanation in the revised manuscript and please check the details in page 7 lines from 15 to 22.

(4) In the introduction, the authors should reference the work by Tie et al. (2005). Although it used a global model, it is an early work to discuss the effect of aerosols (including BC) on photochemistry and ozone. Also in Tie et al. (2017), they found that the moisture plays important roles on PBL development, especially in the aged

aerosol, including BC. The authors should state this point in the introduction.

Reply: Thanks. These references are very helpful to our study. They have been cited in the introduction. Please check the details in the revised manuscript.

(5) In previous works (Tie et al., 2009), in large cities in eastern China,  $\text{NO}_x$  concentrations are very high. As a result, increase in  $\text{NO}_x$  concentrations lead to decrease in ozone concentration of in the center of cities. However, in rural areas, the concentrations of  $\text{NO}_x$  decrease rapidly, and increase in  $\text{NO}_x$  concentrations lead to increase in ozone concentrations of ozone. In the analysis of paper, the authors should discuss this point in more details.

Reply: Thank you for your comment. The photochemical production of ozone is not only related to the photolysis rate, but also related to the concentrations of ozone precursors ( $\text{NO}_x$  and VOCs). Since the suppression of BL, the concentrations of  $\text{NO}_x$  and VOCs increased at surface (figure S2). It should be noted that, the ratios of  $\text{VOCs}/\text{NO}_x$  also increased which suggested that VOCs increased more significantly than  $\text{NO}_x$  did. In addition, the little changes of the ratio of  $\text{HCHO}/\text{NO}_y$  indicated that ozone still formed under the VOC-limited conditions. In this case, the increase of VOCs was favorable for ozone chemical formation. In our study, ozone contributions from chemistry were enhanced with the impacts of BC from 10:00 to 12:00. During this period, the photolysis rate was reduced which showed the reduce effect to the chemical production. Thus, the enhancement of ozone chemical production was more likely related to the increase of ozone precursors. In order to interpreting this problem, we have added a figure (Figure S2) in the supplement and the relevant discussion has been added into the revised manuscript. Please check the relevant information in page 7 (lines from 24 to 28) page 8 (lines from 31 to 34) and page 9 (lines from 1 to 2).