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Interactive comment on “Ozone weekend effects in the Beijing–Tianjin–Hebei metropolitan area, China” by Y. H. Wang et al.

Anonymous Referee #2

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The metropolitan area of Beijing-Tianjin-Hebei (BTH) is one of the heaviest polluted areas in China. This study analyzed weekly variations of ozone using two-year in-situ surface and vertical measurements in BTH, and identified the possible causes. Therefore this paper may contribute to a better understanding of photochemical pollution and providing scientific basis for ozone control strategies. However, it has several weaknesses that need to be addressed: 1. The main goal of this study seems to assess characteristics of photochemical pollution, but the analysis presented does not quite get there. For instance, I suggest additional analysis of using daytime ozone concentration instead of daily average. 2. Table 2 is not clear, Δw and percentage should be defined. For example, in LF site, should the sign of Δw for O₃ and NO be negative or positive? 3. Fig.1 does not show consistently higher surface ozone concentrations on

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weekends than week- days, which the authors mentioned in the paper. 4. The ozone concentration at the high platform had minimal variability, but was more variable at the low platform? Please elaborate on this. 5. In Fig.4, the ozone concentration at the site BD peaks one or two hours earlier on Monday than other days, and the same thing happens for the site SQL. More discussions about the cause are desired. 6. Suggest combine Table 3-table 5 into one table. 7. Suggest reduce the number of figures (or sub figures). For instance, in figure 9, use double y-axis to facilitate the comparison of results and also to reduce number of sub-figures. 8. Is there any evidence that CO can be appropriately used as a proxy for VOCs in the BTH area? Further, is it suitable to get VOCs/NOx ratio from CO variation? Because of this uncertainty, I wonder if the results related to this approximation can be highlighted in abstract and conclusion.

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