Zhao et al. present a revised manuscript describing the VOC measurements and ozone formation observed in Nanjing, China. I appreciate the authors response to my suggestions of how to improve the PMF solution. The additional details describing how the authors validated the PMF solution and the expanded discussion of PMF factor assignments are much improved.

However, I do not believe the authors have demonstrated that the OBM is reasonably capturing local ozone formation; therefore, I don't believe it is justifiable to use the model for ozone isopleth calculations. The authors have presented two new figures to compare the OBM with ozone observations (Figs. S3-S4) and text discussing these results (Lines 220-266). The comparison between the OBM and observed ozone concentrations (Fig. S3) is difficult to see, but in general, it seems that the model over or under predicts ozone by a factor of 2 on any given day. The authors acknowledge a number of shortcomings of the model (not capturing meteorological conditions, not capturing transported ozone, missing precursors, etc. line 227), which may explain many of the disagreements. I do not expect the authors to capture all of the ozone features over the entire sampling period (e.g. at night or during weather events); however, the model should capture daytime ozone production, especially during the ozone episodes defined in Fig S7. The model disagreements average out to a diurnal pattern that appears to be successful (Fig. S4), and the authors use this diurnal pattern to argue that the model is successful at recreating ozone production rates. This discussion is misleading given the results from Fig. S3.

As written, I don't believe the OBM should be included in this paper. Significant work would be needed to improve the OBM (see suggestions below). The measurements and PMF results are useful, and I encourage the authors to focus on these. Furthermore, I believe the authors could still address the importance of VOC precursors in ozone formation by evaluating proxies such as OH reactivity or maximum incremental reactivity (MIR).

Suggestions for OBM:

(1) The authors define an episode based on periods when ozone exceeds 80 ppb (I assume this is hourly averaged?). Based on Fig S7, this would suggest that the authors are comparing the OBM to data collected between April and October. I would be quite surprised if the boundary layer dynamics used by the authors apply equally to ozone episodes observed in April with those observed in October. Furthermore, it is not clear if adjustments were made to the TUV model to account for photon attenuation (e.g. clouds). Why not focus on a shorter period where the OBM can be tailored to the meteorological conditions measured over a week, as opposed to 5 months?

    I would assume that the best period to choose would be (a) when winds are stable, slow, and originating from a single location (b) when skies are clear, and (c) when the boundary layer height can be modeled, measured, or well-represented by the approximation described by the authors.

(2) The authors initialize each episode event with a spin-up period of two days that uses the campaign-averaged diurnal profile of each measurement. There seems to be a lot of variability in the monthly concentrations of VOCs, ozone, and NOx (Fig S5-S7). Why not use the hourly data and constrain each event to the measurements conducted each day? Since this analysis is focused on local ozone production, it seems that this would also help to account for ozone transported from upwind sources.

(3) While the focus is on ozone formation, it's also important that the model should reasonably represent NOx and VOC profiles during ozone episodes. This not only affects radical budgets, but is also important in order to differentiate between ozone formed via reactions of NOx alongside biogenic and anthropogenic VOCs. It would be convincing to see how the model performs in reproducing VOC and NOx concentrations.