We thank both referees for their thoughtful comments and their efforts towards improving the manuscript. We have considered their comments and respond below.

Response to Referee #1 General Comments:

1) It would be more appropriate to call it weekday-weekend pattern of NO2 rather than day-of-week pattern as only the difference in weekday and weekend NO2 spatial variations is analyzed.

> We have made the suggested change

2) The total NO2 decrease in Los Angeles for all the cases considered is within 39-44% (based on Table 1). Considering that the weekend NOx emission reduction in the model is 37.5%, the reduction in NO2 due to chemical feedback seems to be in the order of only 5%. Could you comment on the extent of the contribution of chemical feedback on the observed decrease in NO2 columns in different basins? Is chemical feedback only important for getting the spatial distribution correct?

> The effects of chemical feedbacks on basin-wide NO2 are small in total. However, the basin-wide average includes regions where NOx losses increase in response to NOx emission reductions (right half of chemical regime shown in Fig. 1) and regions where NOx losses decrease in response (left half of chemical regime shown in Fig. 1). At smaller spatial scales, feedbacks are much larger, as discussed in the manuscript.

3) Could you please include how the model simulations compare with the observations on absolute scale for weekday and weekend in the three basins? Table 1 would be a good place for it. It is quite surprising that the modeled NO2 values agree with the observations within 2% of the observations as models often tend to overestimate NO2 over urban areas (e.g. Kim et al, 2009). Is the agreement biased by the low values over the background regions? Please include how the observations and model (base case) compare over the three basins.

> We prefer not to include the absolute agreement as we find that distracting from the paper's focus on the weekday-weekend pattern of NO2 decreases in the basin and its response to different parameter changes.
>
> Kim et al. (2008) did find that WRF-Chem overestimates NO2 columns relative to observations from 2005 when using the outdated emission inventory (NEI1999). We use the NEI2005 for comparison with NO2 columns measured in 2005-2007, a much better match in time. Also, we use the BEHR retrieval (Russell et al., 2011), which was shown to retrieve columns that are approximately 10 - 20% larger over urban areas than the NASA Standard Product NO2 retrieval that was used by Kim et al. Given these systematic uncertainties,

we don't think that absolute difference of order 20% or less are meaningful. However, the weekday-weekend patterns of NO2 columns that we discuss in the paper are more accurate, as systematic uncertainties affect weekday and weekend measurements equally.

Addition of a 5 x 10^{14} molecule cm^{-2} background column to the simulated values is already included in the basin-wide comparison presented in Section 2. The addition of a background to the simulations adds only 5.5% to the total mass given that the average concentration of the Los Angeles plume, defined as the region over which the observed weekday NO_2 column exceeds 2.5×10^{15} molecules cm^{-2} within the domain presented (33.3ºN – 34.8ºN, 116.25ºW – 119.25ºW), is $9x10^{15}$ molecules cm^{-2}. For the purpose of this paper, it is mostly important to recognize that the pattern of percentage weekend NO2 decreases at the far edges of the plume depend on the regional background concentration of NO2, as stated on P19179 L2-5.

4) It is quite intriguing that the authors concluded that the two weeks of model simulations is representative of the long run average considering that the weather in Los Angeles area tend to stagnate during late summer and have high ozone episode events. Please comment on how this two-week period was chosen.

The selected time period (1-14 June 2008) experienced near-average temperatures in downtown Los Angeles (within +/- 4 deg C of mean) and coincides with the lead-in to the Southern California/Los Angeles portion of the ARCTAS flight campaign (17-26 June 2008 – a period that was anomalously warm). Also, as stated in the manuscript and below, we find that the variability due to meteorology is small relative to the parameters.

> "We test whether a two week period of simulation is representative of longer time periods by simulating the base case scenario for three months and find that the magnitude and pattern of decreases simulated in the two-week scenario is comparable to that of the 3 month scenario indicating its fidelity in representing the meteorological variability of a longer-term simulation." (P19179, L5-9)

Furthermore, the focus of this study is the effect of chemical feedbacks on the spatial pattern of the weekend NO2 effect. The most important consideration, for the purpose of this study, is that the weekday and weekend columns are simulated with identical meteorology, as stated in the manuscript.

> "However, when investigating the patterns of NO2 column (e.g., weekday-weekend pattern) within self-consistent model simulations or observational datasets, many of these biases are eliminated. OMI, for instance, measures weekday and weekend NO2 columns that are subject to the same average meteorological patterns and the same average observational biases, and in our WRF-Chem model setup, the weekday

and weekend meteorology is identical. (P19180, L7-12)

5) It should be mentioned that higher temporal resolution satellite data in the future from geostationary satellite instruments like TEMPO would greatly improve and enhance the application of the framework presented here. Higher temporal resolution satellite data would likely help discern which of these parameters are most important for the spatial variation on NO2 decrease on weekends.

We add the following to the last paragraph of Section 4.

> "The launch of geosynchronous UV/VIS instruments such as TEMPO (planned for 2018 – Chance et al., 2013), will provide hourly daytime measurements and sampling every 2.5 km × 4.5 km at the surface and a significant increase of the information available to constrain the processes affecting the pattern of weekend NO_2 decreases and the NOx lifetime."

Response to Referee #1 - Specific Comments:

We have responded to the comments and have indicated where and what changes we have made to the manuscript.

19176 line 10: 0.05º
-- Corrected

19176 line 17: change to decreases in percentage.
-- Corrected

19176 line 27: Is $3.9x10^{16}$ the maximum value over the 3 years period?
--added "May-July, 2005 – 2007 average" and "regional" to clarify.

19176 line 28: Fig 1b is not present. Please update.
--Corrected to (Fig. 2, top panel)

19178 line 13: What is the basis for 37.5% emission reduction on the weekend? Is it based on the average NO2 column decrease in OMI? If so, the NO2 column decrease in OMI is due to both emission reduction as well as chemical feedback, how do you account for that? Else please include a reference or add rationale for the reduction.

-- We add references to several studies (e.g., Harley et al., 2005; Pollack et al., 2012; Oetjen et al., 2013) that infer weekend NOx emission reductions or measure changes of NO2 concentrations on order of 30-45%. Harley et al. made an estimate from fuel use data and tunnel studies. During the CalNex campaign (May – June, 2010, Daytime flight data in LA for 4 weekdays and 3 weekend days.) Pollack et al. determined weekend emission reduction using surface-based (Pasadena, 34 ± 4%) and airborne (46 ± 11%) measurements with the assumption

that NO_y, CO and CO_2 are conserved in the LA basin on a timescale of 1 day, and Oetjen et al. observed basin-wide NO_2 column reductions using AMAX-DOAS (38±24%) and satellite (33±11%) measurements.

We make reference to these studies in the second paragraph of Sect. 3 as follows:

> "We simulate the average 1 PM NO_2 column for 1-14 June 2008 with NEI2005 anthropogenic emissions (weekday) and the same model with an emission rate of $0.625 \times E_{NOx\text{-}NEI2005}$ (weekend), well within the range of values determined previously (~30 – 45% reduction, Harley et al., 2005; Pollack et al., 2012; Oetjen et al., 2013)."

19179 line 1: Could you also add the value in mixing ratio (for example assuming a typical BL height of 1 km).

The cause of the bias ($5x10^{14}$ molecules cm^{-2}) is not clear. If uniformly distributed through the troposphere (1013 hPa – 150 hPa), a column concentration of $5x10^{14}$ corresponds to a mixing ratio of ~30 ppt. If restricted to a 1 km PBL, the correction corresponds to ~200 ppt.

We add the following text: "A column concentration of 5×10^{14} molecules cm^{-2} corresponds to a concentration of 30 ppt if uniformly distributed through the troposphere (0 – 10 km) and to 200 ppt if restricted to a 1 km boundary layer."

19179 line 29: Could you please add a reference for solar glint reflectance impacts on the NO2 retrieval.

-- While it is well known that NO_2 columns are biased high where albedo is underestimated (e.g., Russell et al., 2011) and that the current operational recommendations are to remove measurements that have been flagged as influenced by sea-glint, we do not know of any study in the literature that directly explores the link between glint and trace-gas retrieval from UV/Visible measurements. We leave the manuscript as is, but welcome any knowledge of relevant studies.

19180 line 3: Please also include this information in section 2 on OMI observations. [oversampling and measurement spatial resolution]

We add a sentence to the end of the first paragraph in Section 2 (P19176 L2). "Due to the non-overlapping orbital pattern of OMI, we oversample the native footprint 13×24 km^2 at nadir to achieve a spatial resolution of approximately 10×10 km^2 for temporally averaged fields (e.g., Russell et al., 2010)."

19180 line 12: "As a result, the agreement of simulated and observed NO2 trends is more meaningful than the agreement between observations and simulations for the single period (e.g. weekday NO2 column….." How is this statement affected when the biases do

come into play via chemical feedback? How relevant are the findings, based on the percent difference on weekday and weekend, for true conditions if the model results are biased either too high or too low?

> We believe that the reader should judge their answer to this question based on the complete discussion presented in the paper. To us, the important finding is that measurements contain information about feedbacks at scales smaller than the entire basin. We are confident that the retrieval is accurate to within 25% based on in situ validation of the BEHR product over Los Angeles during June 2008 (Russell et al., 2011). The simulated columns, as discussed in Section 4, agree with the observed columns to within ~10%. We also ran simulations with 1.30x and 0.7x both weekday and weekend emissions (not shown), with the weekday weekend emission ratio held constant (0.625). We find that the simulated pattern of relative weekend differences, as expected, depends on the absolute concentration of NOx (i.e., shifting the entire domain to the right or left of chemical regime depicted in Fig. 1).

Table 1: What is the spatial distribution of Los Angeles plume? Is it within the Los Angeles basin as defined in the paper or the entire metropolitan area?

> -As stated in Footnote *a* of Table 1, the Los Angeles plume is defined as the region where "the observed weekday NO2 column exceeds 2.5×10^{15} molecules cm^{-2}" We add the following to the end of the footnote to better clarify "within the domain presented (33.3ºN – 34.8ºN, 116.25ºW – 119.25ºW)" and similarly add to P19178, L25 in the text.

Figure 2: It would be helpful to readers to have the rectangles for the three basins on Fig 2a, b and c.

> --Completed

Figure 3: What is the lowest value in the NO2 vertical column color scale? What value does color white correspond to? It would also be helpful to have the figure little larger and rectangles for the three basins

> --Completed. Will request that the typesetter reserve a full portrait page for the figure.

Response to Referee #2 Comments:

We thank the reviewer for his comments and respond below.

1) If I were writing it I would make more of a point that if the basin is NOx saturated then NOx reduction certainly is not the way to reduce basin wide ozone … The major value that I put on it is likely the man made VOC inputs are underestimated.

> Prof. Stedman is correct that both models and observations indicate that the Los Angeles basin is NOx saturated. Observations on weekends show that NO_x controls are not yet effective at reducing O_3. However, these are not, to our mind,

new ideas, and thus not publishable. The data presented here and our modeling does not address the hard question of whether NO_x controls will ultimately be the most effective control strategy for eliminating high O_3 events in the Los Angeles basin, but it does provide some key metrics for evaluating any model that would be used to make predictions in the future.

If there were anything that the authors should add it is comparison of their NEI source inventory to EMFAC modeling and the comparison of EMFAC and MOBILE to the Tunnel emissions reported by Fujita et al in JAWMA about a year ago.

We thank Prof. Stedman for pointing out this reference. As our point is about spatial variability of chemical feedbacks, the exact details of the emission inventory are not essential. We plan to look carefully at the issues raised by Fujita et al in future research.