

Response to Anonymous Referee #3

(Note: Reviewer comments are listed in grey, and responses to reviewer comments are in black. Pasted text from the new version of the paper is in italics.)

This manuscript investigates impacts of urbanization in Southern California on regional meteorology and air quality. Simulations using an innermost domain with 2 km resolution are conducted by WRF-Chem coupled with UCM. The simulations are driven by current climate and anthropogenic emissions with and without urban pixels and are applied to characterize impacts of historical urbanization on regional and temporal distributions of temperature and concentrations of NO_x, O₃, and PM_{2.5}. The authors conclude that urbanization causes daytime decreases in temperature and increases in O₃ and PM_{2.5}. In the nighttime, the simulation results present nighttime increases in temperature and O₃, while the concentrations of NO_x and PM_{2.5} show reductions. The authors attribute these changes to urban-induced modifications in various competing drivers including irrigation, thermal properties of building materials and surface roughness.

General comments:

The topic addressed is interesting and relevant to ACP readers. However, I have reservations about the robustness of the conclusions presented. In my opinion, significant revisions with new analysis and more careful model verification of the simulations are required.

We thank the review for his/her helpful comments. We believe that addressing these comments have vastly improved the quality of our paper.

The impact of urbanization is derived from the differences between temperature and concentrations of fields simulated by a WRF-Chem configuration that includes urban pixels and by a scenario where urban pixels were converted to shrub. This methodology has been presented in previous work and the nighttime impact of urbanization has been well documented in the literature. For instance, the paper by Li et al (“Achieving accurate simulations of urban impacts on ozone at high resolution”, ERL, 9, 2014) introduced similar configurations (WRF-Chem including anthropogenic emissions, with and without urbanization) and used them to derive impacts of urbanization on air quality by analyzing the differences in the simulated fields between the two scenarios. Although the region and the period of time considered in this manuscript are different, the main idea and the nighttime impact are similar. The daytime impact reported in this manuscript is questionable because its magnitude shows values smaller than the model error (see specific comment 2). Careful analysis of the robustness of the impact is needed, especially given that this impact conflicts with previous results as reported (Line 355). The authors need to emphasize what is new related to this research and how it advances the existing research on the topic.

We thank the reviewer for these comments. The first major idea presented here (robustness of the daytime impact) is also brought up in specific comments 2 and 5. Please see our responses to those comments.

The second major idea is on the novelty of this study. While this study shows some similarity in research idea with previous literature, it extend this research topic in that 1) it includes discussion

on the impact of land surface changes on total and speciated PM_{2.5} concentration, which has been seldom studied, 2) it focuses on the Southern California region where such research is limited but necessary given the high pollutant loads, and 3) it incorporates accurately resolved land surface data. We added a few sentences in the last paragraph of introduction section to clarify these points.

“... Note that this paper builds on our prior study Vahmani et al. (2016), but focuses on air quality impacts, whereas our previous research was on meteorological impacts only. While the influence of land surface changes on regional weather has been investigated in numerous past studies, its influence on regional air quality has been seldom studied in past work.”

Specific comments:

1. It is unclear why the authors chose a 10-day period of the summer of 2012? And in what basis the period chosen is “representative of typical summer days in Southern California”? Why not using more years?

We chose this 10-day period because the observed meteorology field is representative of typical summer days in Southern California, which are clear (no clouds) and without precipitation. We added a figure in the supplemental information (Figure S8) showing the diurnal cycle of averaged (observed) near surface air temperature over JJA (June, July and August) and over our simulation period. We also added a sentence in the main paper pointing to that figure.

“This simulation period is chosen as representative of typical summer days in Southern California, which are generally clear or mostly sunny without precipitation. A comparison of observed diurnal cycles for average near surface air temperatures over JJA (June, July and August) versus over our simulation period is shown in Figure S8 in the supplemental information.”

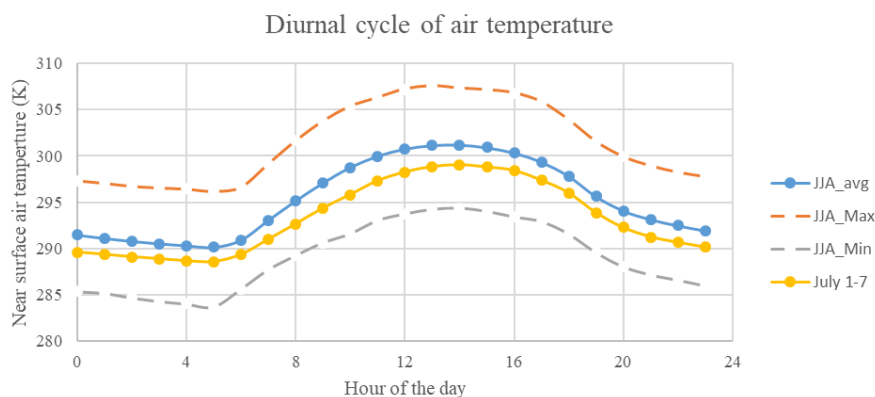


Figure S8. Diurnal cycles for observed near surface air temperature (K) over JJA (June, July and August) in blue, and over our simulation period in yellow. Observations are obtained from MesoWest (<https://mesowest.utah.edu/>), which are available at Mesonet API (<https://developers.synopticondata.com/mesonet/>). Mean values are derived by averaging over all observational sites available for the innermost domain and the aforementioned period for each hour of day. Orange and grey curves show the maximum and minimum air temperature at each hour of the day for JJA. Results show that our simulation period (July 1-7) is representative of summertime meteorology for our domain.

We do the simulations for year 2012 because it is the most recent year for which an accurate

emissions inventory is available for Southern California.

2. The statistics presented in Table 1 indicate that the impact of urbanization is smaller than the model error for all the fields analyzed. For example, the magnitude of the simulated change in O₃ is less than 5.6 ppb (Line 429). The mean and root mean square errors reported in Table 1 are 11.8 ppb and 14.6 ppb, respectively. Thus, the impact described, which is the main conclusion of the manuscript, is not robust given that it lies within the model error. Perhaps, simulations using other years could increase the statistical significance of the results presented.

We suggest that the comparison of urbanization impacts versus model error in Table 1 is not the right comparison. Instead, to assess whether urbanization impacts are statistically distinguishable from zero, we added a new statistical analysis to the paper, using the paired Student's t-test with $n = 7$ days. We did the test to check 1) whether spatially averaged changes in regional meteorology and air quality are significant within the simulation period, and 2) whether changes in spatial resolved regional meteorology and air quality are significant within the simulation period (i.e., for each urban grid cell). For 1), we edited the relevant sentences in the paper. For 2), we updated all figures with maps in the paper to mark out the insignificant grid cells with black dots, and edited the relevant description of the spatial patterns. Please see section 2.5 and section 3 for those changes. We haven't pasted the changes here because they are distributed throughout our results section, and would take up over 3 pages.

3. The authors state in the conclusion that “. . .due to historical urbanization are the main drivers of regional meteorology and air quality changes in Southern California” (Line 567). However, the simulations presented in the manuscript cannot be applied to reach such conclusion. There are several critical factors that are not accounted for. For example, the initial and boundary conditions use current atmospheric conditions and therefore do not include the effect of climate change. The amount of the background CO₂ concentration specified in WRF is fixed (assuming that both configurations use the same setup except for urbanization as stated). The anthropogenic emissions did not exist before human settlement. I suggest that the authors rephrase their motivation and conclusion, and simply focus on the impact of urbanization without attributing historical changes solely to urbanization.

We agree with the reviewer that the motivation and conclusions were not sufficiently clear in the original paper. Thus, we modified the last paragraph of the introduction section and conclusion section.

“... In this paper, we aim to quantify the importance of historical land cover change on air pollutant concentrations, and thus the “Nonurban” scenario assumes current anthropogenic pollutant emissions. This hypothetical scenario cannot exist in reality, since current anthropogenic emissions would not exist without the city, but our intent is to tease out the relative importance of land cover change through urbanization (assuming constant emissions) on air pollutant concentrations.”

“This study highlights the role that land cover properties can have on regional meteorology and air quality. We find that increases in evapotranspiration, thermal inertia, and surface roughness due to

historical urbanization are the main drivers of regional meteorology and air quality changes in Southern California. ...Our findings indicate that air pollutant concentrations have been impacted by land cover changes since pre-settlement times (i.e., urbanization), even assuming constant anthropogenic emissions. These air pollutant changes are driven by urbanization-induced changes in meteorology. This suggests that policies that impact land surface properties (e.g., urban heat mitigations strategies) can have impacts on air pollutant concentrations (in addition to meteorological impacts); to the extent possible, all environmental systems should be taken into account when studying the benefits or potential penalties of policies that impact the land surface in cities.”

4. There are some claims that need clarification. For example the authors state in line 152 “In this study, we couple WRF/Chem to the urban canopy model (UCM). . .” However, the WRF/Chem model is already coupled to UCM. I believe what the authors did is activating the option for this coupling. In line 180 “we update the default WRF/Chem to include a real-world representation of land surface physical properties and processes. . .” But again, the options for using NLCD and NUDAPT for land surface representations are available within WRF. Please clarify what is meant by “we update the default WRF/Chem”.

Thanks to the reviewer for pointing this out. We modified the sentence “In this study, we couple WRF/Chem to the urban canopy model (UCM). . .” as below.

“In this study, we activate the urban canopy model (UCM) in WRF/Chem that ...”

By “update the default WRF/Chem” we mean that we’ve used GIS-based building morphologies, satellite-retrieved land surface data, and a Southern California specific irrigation module for the simulations, which make the model simulation more representative of current day weather conditions and air quality in Southern California. We also modified gaseous dry deposition in chemistry module based on previous literature so that WRF/Chem can be compatible with 33-category land use types.

5. The ability of WRF-Chem to realistically represent urban processes requires more evaluation to better establish the credibility of the present-day scenario. The comparison between observations and simulations shown in Fig. 3 does not indicate to me a “good fit at lower values” as stated in line 294. The observed low values of temperature are around 290 K, but the simulated temperature shows low values of 287K. The difference between these values is larger than the impact reported. Therefore, better model verification should be considered. I also suggest adding to Fig. 3 panels comparing diurnal variations of observed and simulated temperature, O₃ and PM_{2.5} (similar to Fig. 4a).

For the significance of the reported urbanization impact, please refer to our response to comment 2. Figures S11 and S12 show the comparison between observed and modeled diurnal variations for near surface air temperature and O₃ concentrations. (For PM_{2.5} concentrations we use only daily values instead of hourly PM_{2.5} concentrations for reasons that are explained in the supplemental

information section S1; thus, the diurnal variation of observed and simulated PM_{2.5} is not discussed here.) In Figures S11 and S12, values for each hour are averaged over the whole simulation period for all observation sites. The results indicate that while the model underestimates both observed air temperature and O₃ concentrations, the shape of the diurnal cycle is well modeled. For air temperature, simulation results tend to capture daytime (relatively higher) values better than nighttime (relatively lower) values. For O₃ concentrations, the model predicts lower concentrations better than higher concentrations. Thus, we edited the sentence the reviewer mentioned. And we put these two figures to the supplemental information, and added a sentence in the main paper.

“Figure 3 shows the comparison between observed and modeled hourly near surface air temperature, O₃ concentrations, and daily PM_{2.5} concentrations. (Comparisons between observed and modeled diurnal cycles for near surface air temperatures and O₃ concentrations are also presented in the supplemental information, Figure S11 and S12.) As shown in Figure 3 (and Figure S11), the model simulations better capture higher air temperatures during the daytime relative to lower values during nighttime. By contrast, predictions of O₃ and PM_{2.5} concentrations show good fit with observations at low values that occur with high occurrence frequency. However, observed O₃ and PM_{2.5} concentrations are underestimated by the model at higher values that occur with lower frequency of occurrence.”

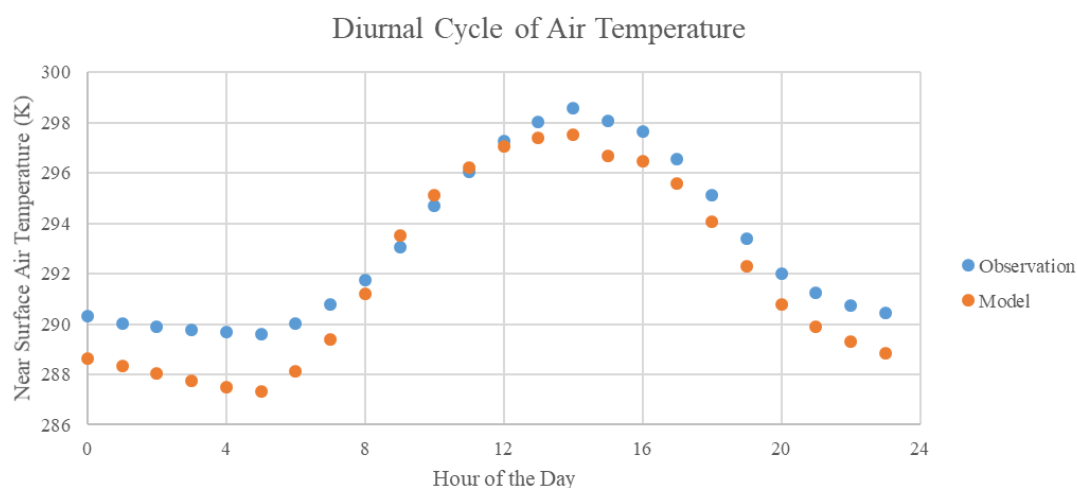


Figure S11 Diurnal cycle of observed and modeled near surface air temperature.

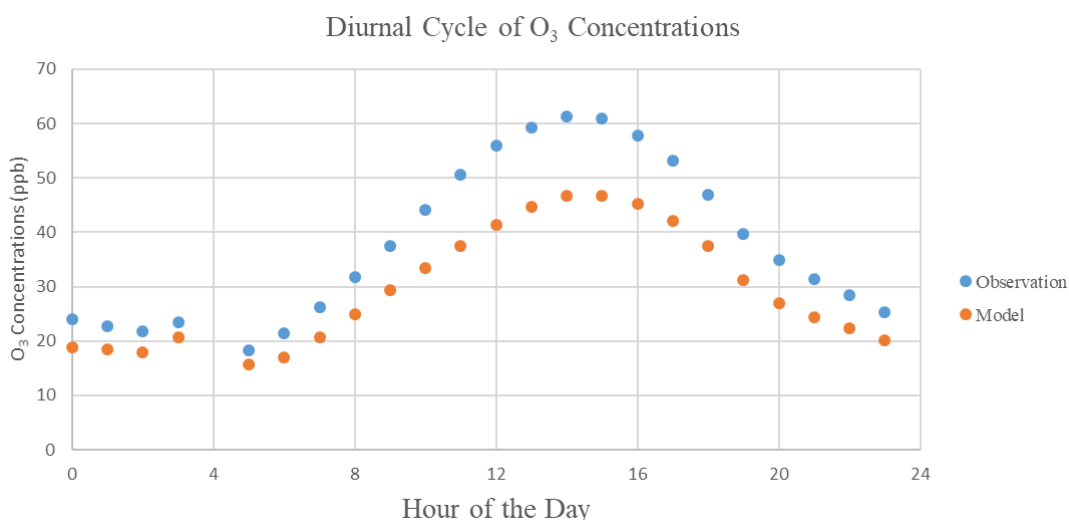


Figure S12 Diurnal cycle of observed and modeled surface O₃ concentrations (ppm).

6. Figs 5, 7 and 9 include values of simulated fields within urban grid cells only. The authors should consider superimposing in these figures values for the entire domain including nonurban grid cells. It would be very helpful to see the differences in the simulated fields within both urbanized pixels and also grid cells that remain natural in both scenarios considered.

This is a good idea. We added new versions of each figure to the supplemental information that include values for non-urban cells (Figures S13, S17 and S18). Please check the supplemental information for these three new figures. In general, the changes in non-urban grid cells are not significantly different from zero at 95% confidence interval for most places. We also added several sentences to the main paper which point to those figures.

Last sentence in section 3.2.2

“A modified version of Figure 5 that includes values for non-urban cells is in the supplemental information Figure S13.”

Last sentence in section 3.3.1

“A modified version of Figure 7 that includes values for non-urban cells is in the supplemental information Figure S17.”

Last sentence in section 3.4.2

“A modified version of Figure 9 that includes values for non-urban cells is in the supplemental information Figure S18.”

