

Referee #3

1. Is the paper scientifically sound? If "no", please give reason. Yes. This manuscript reported a numerical study of the impact of anthropogenic heat on urban meteorology and air quality as well. This study has high impact as Yangtze River Delta is one of most densely populated city in the world.

There are a lot of previous studies which study urban heat flux. However, there are few publications that carried out systematic analyses that study the change of urban circulation caused by anthropogenic heat emissions. The WRF/CHEM methodology further allows the authors to study the change in PM and surface ozone concentrations as well. There is an important question asked many times by scientists about whether anthropogenic heat emissions contribute to global warming. Although the answers are negative, the analysis of AH in this manuscript can enhance the understanding of the magnitude of AH emission from megacities and its impact on meteorology and atmospheric chemistry as well.

Overall, the paper is well written and the references are quite up to date.

Finally, the authors are not native English writers, there are improvements to be made in the choice of words.

[Thanks for the constructive and the affirmative comments. The English is improved according to the suggestions.](#)

2. Is it of sufficient originality and interest to merit publication after attention to matters raised under 3-9? If "no", please give reason. Yes Very few papers studied anthropogenic heat emissions and its impact on urban meteorology and regional air quality at the same time.

3. Are there any errors? No.

[Thanks for the affirmative comments.](#)

4. Are there any omissions? Yes (1) Section 3.3, The term "monthly averaged differences" should be defined. Is it the difference of two means or is it the mean of two difference ? (2) The sentence "Differences that are non-significant under the 95% confidence level (student t test) are masked out." Should be clarified.

[Thanks for the constructive comments.](#)

For (1), the term "monthly averaged differences" means the average value of differences between two simulations. To avoid the misunderstanding, we define it in section 2.2 by providing the calculation expression " $(\sum_{t=1}^{744} (V_{ADDAH,t} - V_{NONAH,t}) / 744)$ ". Please see lines 208-213 in the revised manuscript.

For (2), we follow the method used in the work of Zhuang et al. (2013a, 2013b) and Liao et al. (2015). The monthly averaged differences are calculated grid by grid. So, for one grid, there are one data set from NONAH case and one data set from ADDAH case. To guarantee the differences of a variable are statistically significant, student t test is carried out based on the data set from NONAH and ADDAH for each grid. At one grid, if the difference is non-significant under the 95% confidence level, we can assert that the AH flux cannot significantly change the meteorology or air quality at this grid. The above words and following references are added in the revised manuscript. Please see lines 213-217 and the reference list.

Reference:

Zhuang, B. L., Li, S., Wang, T. J., Deng, J. J., Xie, M., Yin, C. Q., and Zhu, J. L.: Direct radiative forcing and climate effects of anthropogenic aerosols with different mixing states over China, *Atmos Environ*, 79, 349-361, 10.1016/j.atmosenv.2013.07.004, 2013a.

Zhuang, B. L., Liu, Q., Wang, T. J., Yin, C. Q., Li, S., Xie, M., Jiang, F., and Mao, H. T.: Investigation on semi-direct and indirect climate effects of fossil fuel black carbon aerosol over China, *Theor Appl Climatol*, 114, 651-672, 10.1007/s00704-013-0862-8, 2013b.

Liao, J. B., Wang, T. J., Jiang, Z. Q., Zhuang, B. L., Xie, M., Yin, C. Q., Wang, X. M., Zhu, J. L., Fu, Y., and Zhang, Y.: WRF/Chem modeling of the impacts of urban expansion on regional climate and air pollutants in Yangtze River Delta, China, *Atmos Environ*, 106, 204-214, 10.1016/j.atmosenv.2015.01.059, 2015.

5. Are any sections obscure and what additions or alternations would remove the obscurity? No

Thanks for the affirmative comment.

6. Could any sections be omitted or shortened? please be specific. Yes The conclusion looks too long. It could be shorten a bit, especially the first paragraph.

Thanks for the constructive comments. The conclusion is shorten, especially the first paragraph.

7. Are all the illustrations/tables necessary, clear and suitably captioned? Yes

8. Is the abstract adequate? Yes. The abstract is good.

Thanks for the affirmative comments.

9. Are the title and key words appropriate? If not, please suggest alternatives. Yes but I suggest to modify it to "Modeling of the anthropogenic heat flux and its effect on regional meteorology and air quality over the Yangtze River Delta region, China."

Thanks for the constructive comments. We agree with the reviewer and modify the title. Please see lines 1-3 in the revised manuscript.

Detail comments:

Abstract: It is well written.

Thanks for the affirmative comment.

Introduction:

Line 28 of page 32370, And is a preposition. It is not appropriate to put "and" at the beginning of a sentence. There are quite a few sentences in the manuscript with the same problem.

All "and" at the beginning of sentences are deleted. In the revised manuscript, please see the sentences on line 24, line 88, line 188, line 226, line 245, line 268, line 311, line 337, line 387, line 424, line 429, line 458, line 471, line 554, line 580, line 593, line 612, and line 667.

Line 10 of page 32371, delete the word "unfortunately".

The word "unfortunately" before "these studies only" is deleted. Please see line 99 in the revised manuscript.

Line 15 of page 32371, delete the word "Consequently".

The word "Consequently" before "in this paper" is deleted. Please see line 104 in the revised manuscript.

Line 10 of page 32371, delete the word "of" after the word implementing.

The word "of" after "(2) implementing" is deleted. Please see line 105 in the revised manuscript.

Section 2.

Methodology Section 2.1, page 32372 Line 7, the resolution of AH fluxes is 4km, but the domain 3 of WRF/CHEM is 9 km. How do the authors resolve this problem?

The resolution of AH fluxes (AHF) used in this study is 2.5 arcmin (about 4km), because the resolution of gridded population data is 2.5 arcmin. The AH fluxes during the period from 1990 to 2010 are all estimated to figure out the changing trend, while only those in 2010 are used in our WRF/Chem simulations of this paper. We estimate them not just for this study. We hope that these AHF data sets can be used in other similar investigations. To some extent, our AHF outputs are something like emission inventories. To fit the resolution of the domain 3 of WRF/Chem in this study (9km), we re-project the 4km AHF data to domain 3 by the latitude and longitude of each grid. Some explanative words are added on lines 261-263 in the revised manuscript to briefly clarify our method.

Section 3.2 Line 7 of page 32380, the word "more than 0.7" could be replaced by "higher than 0.7".

The words "more than 0.7" are replaced by the words "higher than 0.7". Please see line 354 in the revised manuscript.

Line 5 of page 32381, the word "more solar radiation reaches to urban" could be replaced by "stronger solar radiation reaches urban".

The word "more" before "solar radiation reaches to urban" is replaced by the word "stronger". Please see line 388 in the revised manuscript.

Section 3.3.1 Line 7 of page 32382, "Differences that are non-significant under the 95%

confidence level (student t test) are masked out." Student t test requires the data set to be normally distributed. Are the data normally distributed? Further more, it is very confusing here. In figure 6a, the monthly averaged differences are calculated grid by grid. So for one grid there is only one set of data of NONAH (T2) and one set of data ADDAH (T2). How to remove data that have insignificant differences?

We follow the method used in the work of Zhuang et al. (2013a, 2013b) and Liao et al. (2015). The monthly averaged differences are calculated grid by grid. So, for one grid, there are one data set from NONAH case and one data set from ADDAH case. To guarantee the differences of a variable are statistically significant, student t test is carried out based on the data set from NONAH and ADDAH for each grid. At one grid, if the difference is non-significant under the 95% confidence level, we can assert that the AH flux cannot significantly change the meteorology or air quality at this grid. These words are added in the revised manuscript on lines 212-217.

Line 17 of page 32382, "the adding AH fluxes" could be changed to "the addition of AH fluxes".

The word "adding" in "the adding AH fluxes" is replaced by the word "addition". Please see line 428 in the revised manuscript.

The comment "the addition of AH fluxes lead to an increase of SHF in both daytime and nighttime." is not exactly correct. Figure 7 shows that the SHF is almost the same from midnight 00:00 to 05:00am.

We agree with the reviewer. The description "the addition of AH fluxes lead to an increase of SHF in both daytime and nighttime." is not proper. So, we rewrite this part as "As illustrated in Fig. 7, the addition AH fluxes lead to an obvious increase of sensible heat flux (SHF) from 07:00 to 21:00, with the daily mean increase of 22 W/m² for January and 20.5 W/m² for July. The increases at night can be ignored because the AH fluxes are small during these time". Please see lines 427-430 in the revised manuscript.

Line 11 of page 32383, "adding AH fluxes make the PBLH rise up to over 50m" could be changed to "enhanced AH fluxes increase the PBLH by more than 50m".

As suggested above, "the adding AH fluxes make the PBLH rise up to over 50m" is replaced by the

words "the enhanced AH fluxes increase the PBLH by more than 50m". Please see lines 467-468 in the revised manuscript.

Line 25 of page 32383, "adding AH fluxes" could be changed to "enhanced AH fluxes".

The words "adding AH fluxes" are rewritten as "enhanced AH fluxes". Please see lines 481-482 in the revised manuscript.

Line 6 of page 32384, the word "re-established" is not a good choice of word. May be "modified"?

The word "re-established" is changed to "modified" as suggested. Please see line 491 and line 680 in the revised manuscript.

Line 15 of page 32384, the word "ignorable" should be "ignored"?

The words "is ignorable" are replaced by "can be ignored" as suggested above. Please see line 499 in the revised manuscript.

Section 3.3.2 Line 20 of page 32384, "an significant" should be "a significant".

The indefinite article "an" is changed to "a". Please see line 513 in the revised manuscript.

Section 3.4.1 Line 24 of Page 32385, replace "venting" by "dispersion"?

The words "the venting of air pollution" in the sentence "Adding AH changes spatial and vertical meteorology conditions, and thereby undoubtedly affects the venting of air pollution" are rewritten as "the transportation and dispersion of air pollutants". Please see line 548 in the revised manuscript.

Line 27 of Page 32385, is the PM₁₀ the "surface PM₁₀"? Is it only include the PM₁₀ of the lowest bottom layer or the integrated PBL PM₁₀?

All changes of PM₁₀ and O₃ caused by the adding AH discussed in section 3.4.1 only include the modeling results from the lowest bottom layer. The changes of other layers in PBL are discussed in section "3.4.2 Vertical changes of PM₁₀ and O₃". To avoid misunderstanding, we revise the title

of section 3.4.1 to "Changes of surface PM₁₀ and O₃", and add some brief explanation in the following paragraph. Please see line 546 and line 553 in the revised manuscript.

Line 4 of Page 32386, (just a comment) a decrease of 29.3 µg/m³ of PM₁₀ is phenomenal.

Yes, it is remarkable. So, anthropogenic heat fluxes should be considered to better understand the urban atmospheric environment issues.

Line 26 of Page 32386, "increase of O₃ causing by AH" should be replaced by "increase of O₃ associated with the introduction of AH".

The words "increase of O₃ causing by AH" are replaced by "increase of O₃ associated with the introduction of AH" as suggested above. Please see lines 580-581 in the revised manuscript.

Furthermore, the article only shows the surface ozone. As the convection is enhanced, it is interesting to show ozone at higher levels such as at 1km altitude.

Indeed, it is interesting to discuss O₃ at higher levels. In this paper, we not only show the changes of PM₁₀ and O₃ at the lowest modeling layer in section 3.4.1, but also discuss their vertical changes in section 3.4.2. As shown in Fig. 12e, 12f, and Fig. 13, the decrease of ozone occurs at 1km altitude, which can be attributed to the increase of NO (associated with the introduction of AH) at this layer. Please see lines 627-643 in the revised manuscript.

Reference section

There are a few references quoted but not found in the reference list. They are Allen et. Al, 2010, Quah & Roth 2012, Ryu et. Al, 2013, Yu et. Al, 2014, Grimmer 1992.

Sorry for these mistakes. The detail information of quoted references is added into the reference list. In the revised manuscript, please see lines 708-709 for Allen et al. (2011), lines 799-800 for Quah and Roth (2012), lines 801-802 for Ryu et al. (2013), and lines 826-827 for Yu et al. (2014). However, we do not cite the reference from Grimmer (1992) in our paper.

Referee #4

In this manuscript, the spatial distribution of anthropogenic heat (AH) emission with seasonal

and the diurnal variations is constructed over Yangtze River Delta. AH impact on meteorology and air quality were studied, especially focusing on horizontal and vertical circulations.

The effect of Urban Heat Island (UHI) by urban land-use on ozone and air quality have been reported in the world. However, limited data and simulations of UHI by AH on ozone and air quality are currently conducted. Such kinds of study, especially the seasonal differences of AH effects as presented in this manuscript, should be valuable and encouraged. The manuscript is well written and organized. The scientific issue raised in the manuscript that AH effects on meteo and O₃, NO_x and PM are interesting to the ACP audiences. I suggest publication the manuscript after addressing the comments and suggestions below:

[Thanks for the affirmative comments.](#)

1) Since the AH effects (dynamic and chemistry) are generally quite weaker than that of UHI by land-use, and the mechanisms of AH effect should be different from land-use effect especially under distinct meteorological and diffusion conditions. I suggest the authors give a quick review of the UHI effect by land-use on meteorology and air chemistry, which have recently published on JGR-atmos and Boundary Layer Meteorology. I find some of the conclusions are different from that in this manuscript, for example the impact on PBLH and ozone vertical variations. I suggest the author compare the AH and UHI by land-use effects and give a more general conclusion.

[The AH effects are generally weaker than those of UHI induced by urban land-use. So, more researchers paid attention to the latter issue in previous studies. As suggested above, we briefly review the investigations about the effects of urban land-use on meteorology and air quality in the YRD region. Please see lines 86-92 in the revised manuscript. We also quote two more references that recently published on JGR and BLM, that is "\(1\) Kang, H. Q., Zhu, B., Zhu, T., Sun, J. L., and Ou, J. J.: Impact of Megacity Shanghai on the Urban Heat-Island Effects over the Downstream City Kunshan, Bound-Lay Meteorol, 152, 411-426, 10.1007/s10546-014-9927-1, 2014." and "\(2\) Zhu, B., Kang, H. Q., Zhu, T., Su, J. F., Hou, X. W., and Gao, J. H.: Impact of Shanghai urban land surface forcing on downstream city ozone chemistry, J Geophys Res-Atmos, 120, 4340-4351, 10.1002/2014JD022859, 2015.". We find that their conclusions are little different from ours in this paper, because the mechanisms of AH impacts are different from the effects of land-use. We compare the different results, and give some briefly discussion. Please](#)

see lines 636-643 in the revised manuscript.

2) Interpret the $Q_{F,B}$, $Q_{F,T}$, $Q_{F,M}$ in equation 1.

$Q_{F,B}$, $Q_{F,T}$ and $Q_{F,M}$ represent the heat fluxes deriving from energy consumption in buildings, transportation sector and human metabolism, respectively. Please see lines 127-131 in the revised manuscript.

3) I did not find the references "Ryu et al., 2013; Yu et al., 2014" the manuscript raised on page 8 in line 18.

The detail information of these quoted references is added. In the reference list of revised manuscript, please see lines 801-802 for Ryu et al. (2013) and lines 826-827 for Yu et al. (2014).

4) on page 16, "On account that AH and its diurnal variation are only added to the sensible heat item, there are no significant differences between the simulation for latent heat flux (LH)". I suggest the authors should indicate latent heat release could be a non-neglectable parts of AH due to many AH processes related to water vapor releasing.

Thanks for this constructive suggestion. Water vapor could be released when excess anthropogenic heat is emitted from human activities. Thus, AH releasing processes might have some effects on the latent heat fluxes at surface in the cities. But in our simulation and many other similar researches, the water vapor releasing was usually not taken into account. It is an interesting issue but a little different from the main purpose of this paper. We point it out in lines 433-435 in the revised manuscript as suggested above, and also want to do further study in the future.

5) On page 17, 2m-temperature changes typical value of 1.6 °C in January and 1.4 °C in July in Shanghai, and AH fluxes make the PBLH rise up to over 140m in January and more than 160m in July in Shanghai. Please explain the reasons why surface temperature and PBLH changes are opposite in the two seasons.

The AH fluxes increase the 2-m air temperature (T_2) in both months by adding more surface sensible heat into the atmosphere. Because the AH emissions are higher in winter (Sailor and Lu,

2004; Flanner 2009), the T2 increment of 1.6 °C in January is higher than that in July (1.4°C). But in summer, the weather is more unstable and the vertical convection is easy to form. So the adding AH might induce more increase of PBLH in July. So the change of PBLH is only 140m in January, while it is 160m in July. To put it another way, if we see the relative increases of PBLH caused by adding AH, 37.5% in January is still higher than 30.9% in July. On lines 470-471 of the revised manuscript, we add some brief explanation.

6) On page 22 line 22-25, I suggest you compare ozone variations in vertical with that on the references which ozone variations induce by urban land-use.

We compare vertical ozone variations caused by AH with those induced by urban land-use from previous investigations (Liao et al., 2015; Zhu et al., 2015) as suggested above. Please see lines 636-643 in the revised manuscript.