Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1068-RC1, 2021

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics of surface energy balance and atmospheric circulation during hot-and-polluted episodes and their synergistic relationships with urban heat islands over the Pearl River Delta region" *by* Ifeanyichukwu C. Nduka et al.

Anonymous Referee #3

Received and published: 2 January 2021

General comments The study focuses on the region of Pearl River Region (China) and investigates hot and polluted episodes (HPEs) from 2009 to 2011, classified in relevance to atmospheric circulation, while it explores variability of surface energy balance parameters in an effort to detect possible synergies between HPEs and urban heat island (UHI). Numerical simulations using WRF-Chem were conducted in the domain, after evaluating model performance through observations. The subject of the study is relevant to the thematic areas of the journal. Synergistic effects between heat waves,

Printer-friendly version

Discussion paper

urban heat islands and air pollution are of particular importance in the general context of climate change and environmental threats. Although the subject of the study is interesting, the aim, specific objectives and novelty of this study should be further highlighted. Interpretation and discussion of the results remains somewhat superficial, especially regarding the role of different surface fluxes during HPE (compared to pre- and post – HPEs episodes) and how they contribute to positive feedbacks between HPEs and UHI strength in the study area. Methodological approaches need further clarification and precision in several parts of the study (in which authors seem to speculate) as for instance adopted criteria for the identification of HPEs and utilization of meteorological and air quality observations from the available stations network in the study area. Limitations of this study should be included in the discussion. Specific comments are provided below, which I hope will be helpful to the authors Specific Comments Why the period of 2009-2011 was used? Recent decade has witnessed extreme hot weather and could be more representative of recent and near future tendencies. Also, I missed some qualitative and quantitative information about the observed air quality and temperature trends in the area during the last years. For instance, are heat waves, polluted episodes, or combination (HPEs) becoming more frequent? Line 49-50 : actually the outcome of these studies is that asymmetric changes in the surface energy budget equation terms during heat waves (compared to normal summer conditions) , have been found to induce synergies between heat waves and UHI magnitude Line 70 – 75: The title of the manuscript refers to synergies between HPEs and UHIs. But here, the main aim / objectives and intentions of the study are not clearly stated. Line 86-87: what previous study? Please specify and rephrase. If you want to stress the innovation of the 'current ' study which does not duplicate a 'previous' one, just underline what has been already done and what is new in this study. There is a confusion with citations and reference list of Yim. Yim 2020, Yim 2020a and Yim 2020b, all 3 appear in the text. In the reference list they appear as 2020a and 2020b, but these two references are also identical. Please clarify and correct.

The criteria for the definition and methods for identification of HPE should be better pre-

sented and/or justified. There is a plethora of heat waves (or hot episodes) definitions in literature based either on percentiles of maximum temperature distribution (reflecting local climate conditions), fixed temperature thresholds or combination. A common duration of 3 days is usually adopted. According to the authors, the 50% percentile of T2 exceeds the critical temperature above which risks of health impact were significant (SI, line 106). Is it for Hong Kong station? Which is this threshold? This means that at a frequency of 50% HK experiences temperatures above critical values? The same for air quality thresholds. Provide some quantitative information. Are WHO thresholds exceeded at a frequency of 50% in the area? Is it for HK stations (being also cleaner than other air quality stations, according to authors?). Îd'he authors should proceed for their identification over the study period based on a more robust definition of HPEs. In the sensitivity test (Table S3 in SI) I had the impression that the percentile 50% was selected just to secure a 'good' number of HPEs (not too few, not too many?). What is the measure of the 'optimum' number of HPEs? In general the authors must be and look more confident about their choices and procedures. Lines 92-95. Here it is reported that for HPEs identification, maximum air temperature exceeded threshold at each station (I suppose the authors refer to 16 meteorological stations within PRD region, as shown in map S1b) and PM10, O3 exceeded thresholds for 50% of all air pollution stations (I suppose from the 11 air quality in total as also reported in supplement in PRD region) . But in Supplement, (section 2.2), HPEs identification seems to be based only on one station's (Hong Kong) temperature, and on 14 air quality stations within Hong Kong. Perhaps I misunderstood something, but I think it is somewhat confusing. In each case, the approach needs further clarification. Line 97-99: I do not understand the point, it is very general statement. Justify better your selection criteria. Are they more strict or less strict compared to previous studies and why? Computational cost is important, but scientific base is more important. Section 2.1 – observations. The number of stations should be mentioned here along with reference to supplement for more details on locations etc. Line 104: write 'in Supplement Information' instead of SI when first mentioned in the text. Line 106: provide links Line 115: 'trends' ? Line

Printer-friendly version

Discussion paper

118: rephrase Line 120: .. 'and synergistic relationship between HPEs and surface cover': please rephrase the part of 'surface cover' Line 199: As you refer to short wave radiation (SW), the values 1,700 and 1,500 W/m2 are unrealistic here, based also on the scale of SW. Section 3.2.3 (HY-HPE): according to criteria set for T2 , the identification of HPE requires T2 > 50% percentile, corresponding to 31.3 0C (as shown in table S3), but simulated temperatures are lower, not corresponding to heat wave criteria. In Line 218, the authors also report marginal changes in pre- mid- and post HPE, which means that no heat wave is identified in this case. Please comment on this. Provide some information/comparison between observations and simulations for the particular HPEs which you have selected to demonstrate. In each case, Figure captions should be more informative and indicate whether the results refer to WRF simulations or observations. Line 230: '. . .which have been found to be a suitable indicator for heatwave accumulation and structure because they exhibit the characteristics of an air parcel over time' ??? ' I do not understand the statement that potential temperature exhibit the characteristics of an air parcel over time . Please rephrase/correct! Line 233: OLR is mentioned here (also in line 290) but I cannot see how it is used in the analysis and results described in this section. Line 234: How were UHI intensities quantified? Do they correspond to T2 differences between 'average urban' grids and 'average rural' grids? (the same for sensible and latent heat differences) . How do they link with map of Figure S2? Please provide relevant information. What about observed UHI intensities between urban and rural stations during HPEs? Provide an example. Are there synergies? Line 238 (figure caption): replace 'Difference of T2(diff)' with ' Difference of T2 (T2(diff))' In the analysis and investigation of possible 'synergies' between UHIs and HPEs, you should better and further emphasize the 'differences' of UHI magnitude between HPE and no-HPE conditions (before and after the episodes). If and how UHI intensity is exacerbated (or possibly attenuated) during the episodes and how it links with possible corresponding differences in shortwave radiation, latent or sensible heat fluxes between HPE and no-HPE conditions. This would better illustrate and highlight possible synergies and their driving factors. Lines 259-265: the

analysis looks very tentative and authors give the impression that they speculate. The effect of aerosols on UHI depends on many factors affecting OLR and DLR and deserves some discussion (see for instance Li et al 2020, doi:10.1029/2020EA001100, or Wu et al 2017 (SCIeNTIfIC REPOrTS | 7: 11422 | DOI:10.1038/s41598-017-11705-z), also https://doi.org/10.1016/j.scitotenv.2018.04.254. What about nighttime UHI intensity, which is traditionally higher than daytime UHI but also intensified by absorbed radiation by aerosols.

Line 46 in Supplement: it seems that the cities are shown in Fig S1b, but only the locations of 16 meteorological stations stations are presented in this figure. Also, the 11 air quality stations should be indicated on the map. Section 2.2 in supplement: It is mentioned that temperature observations were derived from one single station (HK) for the identification of HPE, due to minor changes between the stations. It sounds somewhat arbitrary. What 'minor' means. If observed temperature differences are marginal, does it mean that there is no UHI phenomenon in the study area? Some of the 16 meteorological stations are rural as it comes out from Figs S1b and S2. Line 75: relative humidity and wind speed were also used for HPE identification? It is reported that only PM10 data were used due to lack of available PM2.5 data during the study period, but model performance (Table S2b) includes PM2.5 observations during HPEs. Do they refer to another period? The authors use different metrics to evaluate model performance against observations. They provide formulas for three of them, namely r, MB and RMSE. Once they decided to provide formulas for these 3 metrics, they should provide formulas for all metrics, including index of agreement. Also, please check the equations in Supplement for MB, RMSE. The formula for MB is apparently wrong (I do not understand what the symbol '+' represents here). The same is true for the formula 3 for RMSE (what . . .j=1, M. . . represents here?). Despite the wrong formulas included in the text, I want to believe that calculations and presented results based on these metrics are correct, but the authors should check it! Table S2b: it is RMSE and not RSME! Table S2b: why MB for Relative humidity is negative since Model Mean is higher than Obs Mean? Supplement, line 107 & Table S3. : 'The air quality

Printer-friendly version

Discussion paper

was considered adequate because the PM10 and O3 thresholds exceed the WHO acceptable annual and daily (Anon, 2006).'. This is confusing. First, rephrase, and then please provide more quantitative information about exceedance thresholds (annual or daily). What 'annual' means here since you used only summer-autumn data. What the 'mean' values (percentiles) in Table S3 represent? Mean daily values for the 11 regional stations? Only for HK stations? Please specify and add corresponding WHO thresholds . Line 136 (supplement' : what do you mean by 'did not vary significantly?' . Please specify/rephrase Technical corrections Figures 1, 2: change/improve color palette in line plots, it is difficult to discern variables Line 20 : replace 'was' with 'were' Line 23: please rephrase Line 34: replace 'more seriously' Line 38: replace 'heat wave' with 'heat waves' Lines 43, 46: replace 'S. Fan' with 'Fan' Line 46: 'these events'

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1068, 2020.

Printer-friendly version

Discussion paper