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Summer average urban-rural surface temperature differences do not indicate the need for urban heat reduction

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Introduction

Manoli et al. 2019 (*Nature* 573 p. 55-60; hereafter M19[1]) propose a simplified surface energy budget model, parameterized as a function of precipitation, urban population and background surface temperature (Ts) only, to model the summer averaged Ts difference between a city and its surrounding rural area (ΔTs), and from this model derive recommendations for regionally-optimized urban heat reduction strategies. Here we show that the methodology used by M19 to derive these recommendations is inappropriate, even as a first-order guidance approach, and it possesses several critical flaws. M19 rely on summer-averaged coarse-grained ΔTs only, while neglecting "canopy layer air temperature" (Ta), the temperature of the atmospheric layer below roof height where people live, and they frequently conflate the two temperatures. Ta, however, exhibits strong intra-urban variability as a function of differences in urban form between neighbourhoods[2,3] and is essential for any assessment of urban heat reduction efficiency. The coarse grain approach suggested by M19 represents urban structure (building height, building density, etc.) using population as surrogate. This approach is clearly insufficient to account for the large intraurban variability (see Fig. 1, and Fig 2b and c in Schlapfer et al.[4]), or the variability between cities (see point 2c in the Supplemental Material of this contribution), that exist worldwide. While M19 discuss and partly acknowledge many limitations of their method in their section on "Climate-sensitive urban planning," they nevertheless pursue recommendations related to heat mitigation that require explicit consideration of fine-scale Ta; meanwhile, their focus on coarse-grained ΔTs is irrelevant for urban heat mitigation across different climates.

Below we clarify two of the main drawbacks of their methodology.

1. ***There is no clear and established relationship between Ta and the Ts used in M19's method.***

Even if M19 clearly states that their model is for ΔTs, throughout the text they do not unambiguously distinguish between, and often conflate, ΔTs and ΔTa (or surface and canopy layer air UHI). For example, they use references pertaining to canopy layer UHI to support statements about surface UHI (SUHI) or vice versa (see Supplemental Material, section 1). This is important, because urban heat and its impacts can in no way be assessed solely based on Ts, and Ta cannot be simply derived from Ts. By itself, seasonally-averaged coarse grained Ts is insufficient to provide any recommendation related to heat mitigation efficiency to policymakers. Only a few studies[5,6] relate satellite derived Ts with a negative urban heat related effect (heat stress mortality) without considering Ta. However, they only use nighttime satellite derived Ts averaged over a few days (the Heat Wave period), and not the entire summer. Note that the methodology proposed by M19 does not apply to nighttime Ts.

The complexity of the urban Ts - Ta relationship is well-known and has been discussed in many previous studies[7,8,9,10]. Ta arises from multiple, complex atmospheric feedbacks, and not only from Ts. As a result, the canopy layer UHI is not a function of SUHI only but also depends on atmospheric boundary layer growth, horizontal heat transport, hot air entrainment, and atmospheric radiative processes[11]. In fact, even Fig. 4f of Zhou et al.[12] (Supplementary Material of M19), shows no correlation between canopy layer UHI and SUHI.

Further, the satellite-derived Ts used in M19 is the surface radiant temperature that represents only a subset of urban surfaces seen by the radiometer, biasing the radiometric Ts toward horizontal and away from vertical surfaces [13,14] (see Chapter 3, Fig. 3.5). Therefore, satellite derived urban Ts does not represent the complete surface participating in the energy exchange with the atmosphere. It does not fully capture temperatures at pedestrian levels and includes roof-level temperatures that are of questionable relevance to outdoor heat stress and the associated need for mitigation[15].

These points thus question the fundamental assumption by M19 that satellite thermal data alone can provide a first-order estimate of the need for, or efficiency of, urban heat reduction strategies. Moreover, it is unclear from M19 if the Ts ("*lumped skin temperature")* used in their model is consistent with the satellite derived Ts used to calibrate and validate it, therefore raising doubts about the model validation performed by the authors.

***2. The urban heat island intensity is of little relevance for urban heat mitigation*.**

Even if the relationships between "seasonal average" ΔTs and ΔTa were clearer, neither is an appropriate indicator of urban overheating or the corresponding need for heat mitigation. These differences are inadequate and often misleading metrics, as the example of Matera chosen by M19 illustrates. Many other cities have similar temperature dynamics, e.g. the arid cities of Phoenix and Madrid rarely exhibit a daytime canopy layer air UHI, but they still have significant problems with urban heat. Inhabitants of cities with low seasonally averaged ΔTs or ΔTa may still experience strong discomfort and other heat-related impacts during summer days, and hence benefit from mitigation, while cities with high ΔTs or ΔTa may not require any mitigation if thermal conditions are within a desirable range[16]. Unwanted effects due to high temperatures in cities, including human heat discomfort, building energy consumption, heat-related mortality and morbidity, etc., are a function of the thermal, radiative, moisture, and hydrodynamic conditions of the urban atmosphere and their daily evolution (i.e., including the background climate) – they are not due to differences between summer averaged urban and rural Ts as M19 strongly imply. The aim must, therefore, be to reduce the negative effects of urban heat, and not urban-rural temperature differences[16,17].

The whole paper of M19 seems to be built on the assumption that UHI (in this case SUHI) serves as one measure of what is feasible given the "hand a city is dealt" climatically. However, this is not correct. What is feasible is given by the *maximum impact of Heat Mitigation Strategies (HMS)*, which can be more, equal or less than the UHI. This is certainly influenced by the background synoptic climate, and the climate of rural areas surrounding the city, but it is not a function of SUHI. In fact, results presented in M19 indicate that the maximum impacts of vegetation and albedo as HMS occur when the SUHI is minimum. Indeed, if the rural (surface) temperature is kept constant a reduction of the urban (surface) temperature is equal to the variation of (S)UHI. But this observation simply indicates that rural temperatures are irrelevant when assessing the impact of an HMS.

This is important because M19 arrive at conclusions about HMS efficiency by comparing summer average ΔTs across cities with different amounts of vegetation located in different climates, i.e., cities with different rural temperatures. In fact, these results reveal little about the effectiveness of urban greening as a heat mitigation strategy, contrary to what is suggested by M19 in the following statement "*Despite large differences in green cover between EU (gc,u = 0.07 ± 0.05) and SEA (gc,u = 0.48 ± 0.12), observed ΔTs values are comparable in the two regions (1.1 ± 0.6 and 0.8 ± 0.9 °C in EU and SEA, respectively). This evidence suggests that efforts to reduce warming by greening cities might be ineffective under some climatic conditions.*" This statement does not indicate how Ts (and even more importantly Ta) would change from altering the greening fraction, which is the information required to compare heat reduction strategies between EU (Europe) and SEA (South East Asian) cities. M19 further note: "*That is, rural areas in SEA are more efficiently cooled by evapotranspiration due to higher water availability than their EU counterparts, making the goal of minimizing urban–rural temperature differences harder in SEA*". However, the goal of a heat mitigation strategy should not be minimizing ΔTs, but rather reducing urban canopy layer air temperature to a desirable range that, in many cases, differs substantially from the corresponding rural temperature. This seems a subtle point, but we believe **HMS studies can greatly benefit by more clearly and rigorously defining their objectives and research questions.**

Furthermore, the overall methodological approach of M19 is likely to mislead policy makers. The appropriateness of a HMS for a specific city should not be based on a comparison between groups of cities in completely different climates (for example Europe and Southeast Asia). Rather, the choice of the best HMS for cities of the same climate group should be based on a comparison between different strategies for cities within that climate group. As an example of this logic, implementation of high albedo surfaces may be less efficient for cities in humid climates as compared to arid climates, but it may still be the most efficient strategy for humid climate cities.

Finally, the statement that evapotranspirative cooling from well irrigated vegetation is less efficient in a humid climate compared to a dry climate is well known in the field as a result of basic physical mechanisms (i.e., the reduction in vapor pressure deficit). In this context, it is unclear how the findings of M19 add to our understanding of urban thermal climate and its modulation by land cover change. What would be relevant to assess vegetation as HMS is to differentiate between species and amount of irrigation, something that cannot be done with M19's model.

Conclusion

We reiterate that coarse-grained approaches based on seasonally-averaged, satellite derived urban-rural surface temperature differences only, such as proposed by M19, are not useful to policymakers interested in urban heat reduction. The variability induced by finer scale features is too large, and the importance of urban air temperature, its daily variability, and background climate too relevant, to be neglected. Therefore, recommendations derived from such crude approaches have very limited applicability. Additional material and discussion supporting our conclusion are provided in the Supplemental Material.

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Supplemental Material

1. *Examples of inaccurate use of the literature*.

Below are some examples of incorrect use of the literature in M19 in terms of conflating air canopy UHI with SUHI.

a. "*In addition, the aerodynamic explanation of UHIs is inconsistent with the observed power law scaling of urban warming with population…"* (page 55). The second part of this statement refers to results obtained for canopy layer UHIs but follows a discussion of the SUHI.

b. "*This approach constitutes a major departure from empirical analyses that lump different mechanisms into statistical correlations, for example, between ΔTs and population or urban texture* [1,12]" (page 56). The first paper cited (Oke 1973) deals with canopy layer UHI, not SUHI as the text implies.

c. "*However, an opposite correlation was observed during nighttime*[10] *and during the day in 54 US cities*[27]" (page 56). Whilst the first reference (Zhou et al. 2016) refers to surface temperature, the second one (Scott et al. 2018) studies air temperature.

d. "*In dry climates, when the water budget of urban vegetation is supplemented by irrigation, ΔTs becomes negative, creating an 'oasis' effect* [30–32]" (page 56). The first two references cited (Oke 1982, Shashua-Bar et al 2009) address air temperature, not surface temperature as one would conclude from the text.

e. Fig. S4: The use of the Oke (1973) in this context is inappropriate. First, this particular wind speed dependency has been derived for the canopy layer nocturnal UHI and second, it is valid for clear sky conditions only; both of these requirements are not met by the present work.

2. *Other inaccurate assumptions*

Besides the two major issues noted in the main text, there are several other inaccurate assumptions in the datasets, modelling and analysis:

a) The key dataset in M19 is CIESIN's (2016) "Global Urban Heat Island Dataset", which (as far as we are aware) has not undergone a formal peer review process. This dataset consists of 75000+ "urban" extent polygons, each one characterized by metadata that includes population count, urban area, and summer daytime "maximum" and nighttime "minimum" land surface temperatures (LST) for 2013 from MODIS' MYD11A2.005 8-day 1 km global product.

   The polygons are from SEDAC's Global Rural-Urban Mapping Project, Version 1 (GRUMPv1), a database known for its strong overestimation of urban areas (e.g. Schneider et al., 2009, their Table 1 and Figures 2 and 3). An example for the city of Matera is shown by our Figure S1. To showcase this at a global scale, we have compared the surface area of CIESIN's (GRUMPv1) urban extent polygons with the European Space Agency Climate Change Initiative (ESA CCI) land cover map (ESA, 2018) that has an urban class derived from two state-of-the-art global urban footprint databases: the Global Human Settlement Layer (Pesaresi et al., 2016) and the Global Urban Footprint (Esch et al., 2017).

   Our results (Figure S2) indicate that only ~12% of the urban extent used in M19 is categorized as urban by ESA CCI's product, independent of whether all CIESIN's urban polygons are used (75000+) or only the 30000 largest ones (values in brackets in Figure S2). This significant contamination through non-urban areas within "urban" extents severely undermines the basis of the magnitude of SUHI, the urban/natural albedo, and all urban characteristics (building height, urban roughness, aerodynamic resistance, anthropogenic heat).

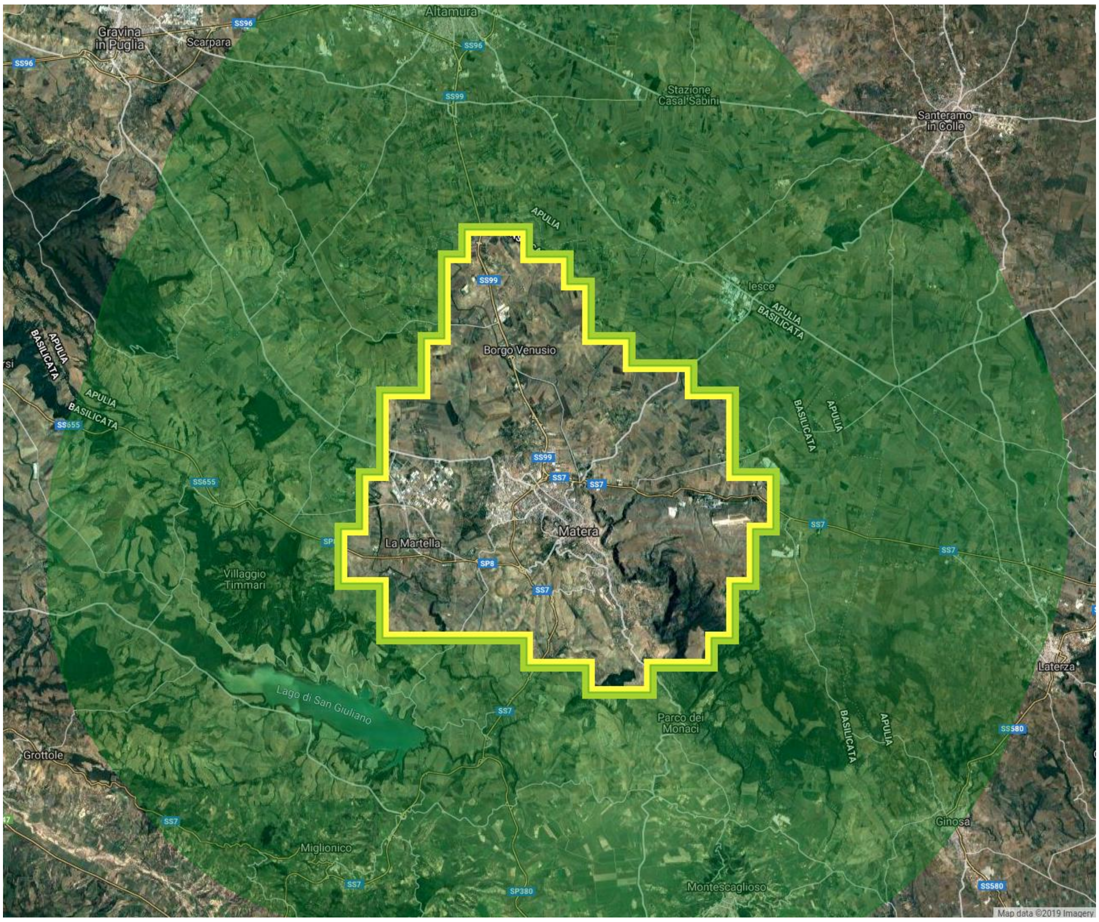

Figure S1: GRUMPv1 overestimates urban extents for the Italian city of Matera. The yellow line represents the outer boundary of the urban polygon. Green shaded areas present a 10 km natural buffer. Matera is chosen here as M19's Introduction explicitly mentions this city as an example: "*A case in point is the Italian city of Matera which, despite its dense urban fabric and the lowest green cover in Europe (less than 1% of the total area), exhibits a negative UHI*". This UHI (in reality SUHI – surface urban heat island), is thus based on the difference of the average LST within the "urban" area (yellow boundary) and its surrounding "natural" area (green shaded areas).

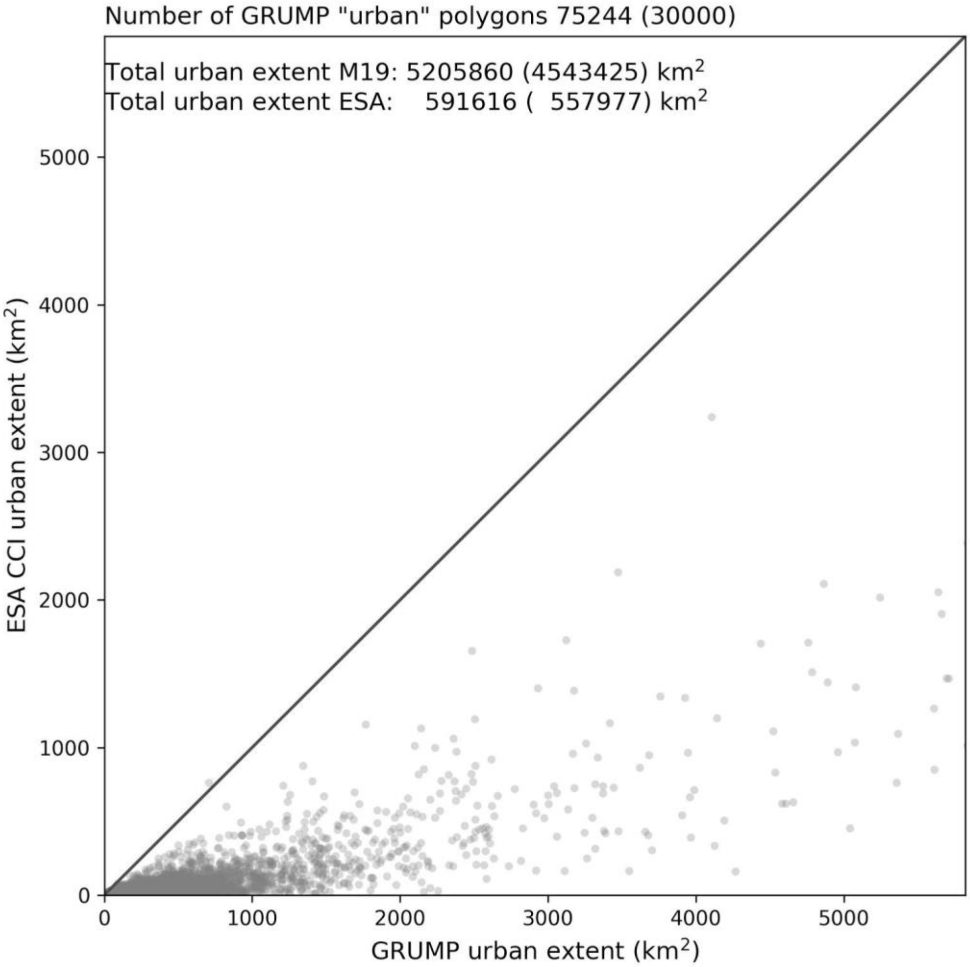

Figure S2: CIESIN (2016) strongly overestimates state-of-the-art urban areas. Comparison of urban areas (km^2) from the CIESIN (2016) dataset (derived from GRUMPv1 urban extent polygons and used in M19) versus ESA CCI urban areas (km^2) within these same polygons. GRUMPv1 urban extents larger than 10.000 km2 (10 polygons) are excluded from this figure for clarity.

b) The data used to validate the model are not consistent with the model description. In the Supplementary Information, ΔTs is defined as the season-average surface temperature difference, while, in the "Methods" section, ΔTs=(ΔTs,d + ΔTs,n)/2, where ΔTs,d and ΔTs,n are daytime and nighttime (surface) UHI, derived from the maximum and minimum surface temperatures obtained from satellites. However, the nighttime and daytime temperatures are those recorded by the satellite at 1:30 am LST, and 1:30 pm LST (see dataset web page https://sedac.ciesin.columbia.edu/data/set/sdei-global-uhi-2013/), which are not the times of minimum and maximum surface temperatures. A priori, it cannot be assumed that the two definitions for ΔTs (the one used in the model and the one for the satellite data) are equivalent. This is because the time evolution of the surface temperature, and hence the timing of maximum and minimum values, is mainly controlled by the thermal inertia of the surfaces (which is different between urban and rural areas, and a function of soil type, moisture and vegetation), and sunset and sunrise times (which are functions of latitude).

c) "Coarse-grained" in M19 refers to the city scale, over which all spatial details and rapid temporal fluctuations are averaged in order to arrive at a link between the SUHI and its drivers. The diversity and complexity of global urban systems are entirely represented by population *N*, and universal scaling laws to describe cities' growth, structures and functions (e.g. urban area, albedo, population density, building density, building heights, anthropogenic heat flux, and sky view factor (M19, Table S5)). M19 justify this approach by citing Bettencourt et al. (2007), even though more recent work by Depersin and Barthelemy (2018) refutes these simple scaling forms valid for all cities, pointing to the omission of a strong path dependency (e.g. while Barcelona in Spain and San Antonio,Texas in the USA have similar populations and background climates, the urban area, structure and cover are fundamentally different based on how these cities urbanized over time). An example of this in M19 are cities' mean building heights defined as $hc,u(N) = 1.15 \times N^{0.12}$ (Schläpfer et al., 2015). Benchmarking this relation against the mean building height for 30 European cities (EEA, 2018; Demuzere et al., 2019) (Figure S3) clearly shows that these cities do not follow this scaling, and by extension not even the USA cities that are mentioned in Schläpfer et al. (2015). This mismatch critically affects dynamic properties such as the zero-displacement height *d* and roughness length *z0* (as these are normalised by *hc,u(N))*, which in turn affects convection efficiency, shown by M19's coarse-grained approach to be one of the two "main determinants of warming".

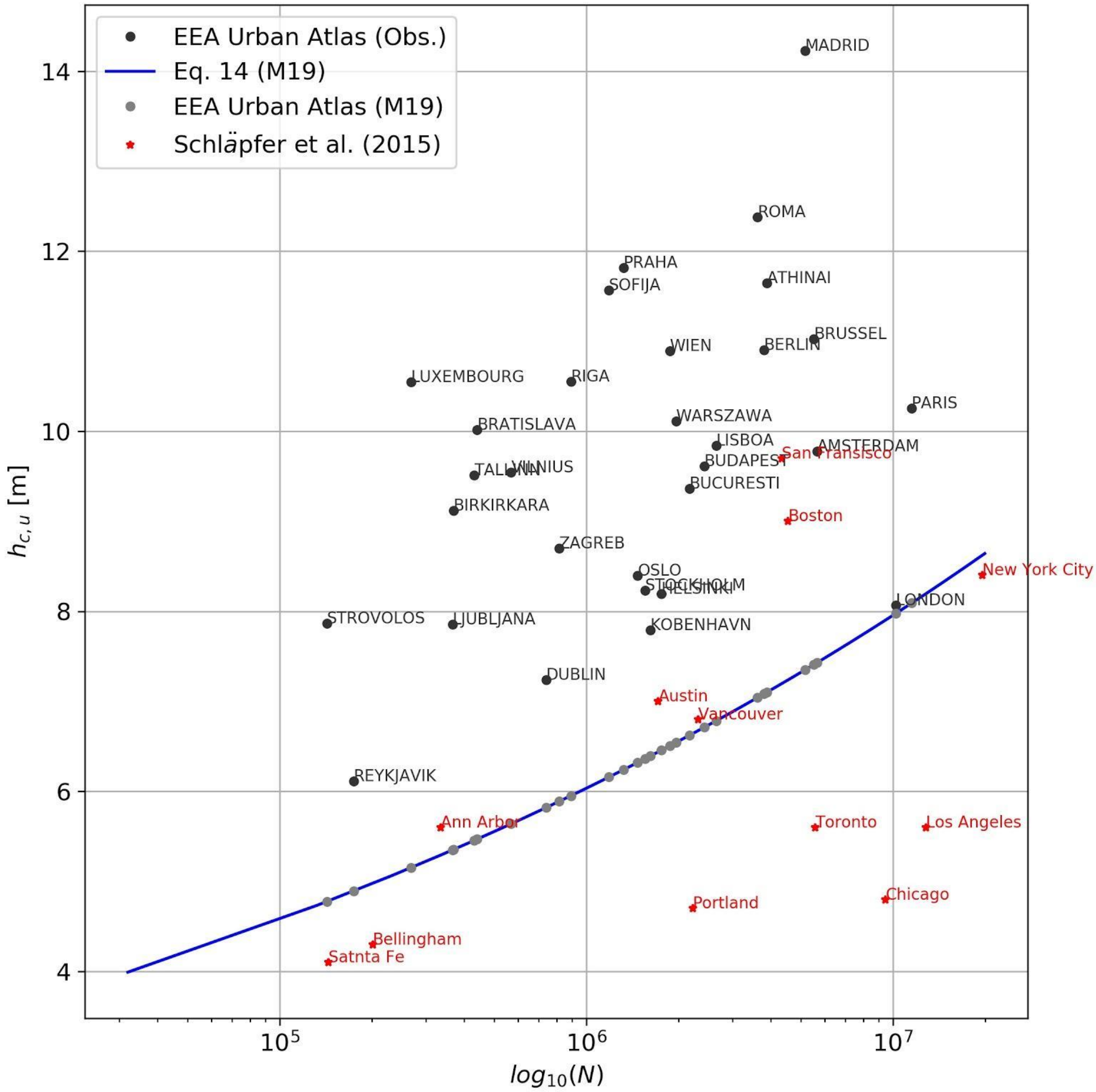

Figure S3: Universal scaling for mean building height *hc,u* not supported by observations. Black dots are observed mean building heights from the 30 EEA Urban Atlas cities. The blue line is eq. 14 from M19. Grey dots are M19's mean building heights derived for the EEA cities using the CIESIN population *N*. Red dots are USA cities from Table 1 in Schläpfer et al. (2015).

d) It is not clear the meaning of Eq. 1 of Supplemental Material. If it is for *"a control volume (of air) over a rural surface"*, the left-hand side should have the air temperature time derivative, and not the surface temperature time derivative, and the right hand side should have only the divergence of the sensible heat fluxes and the anthropogenic flux. If the equation is for the surface temperature, then the control volume should be a thin layer of soil (or the urban surfaces like building, roads, etc.), and *C* the volumetric heat capacity of the soil or urban materials, not of the air. Moreover, in this last case, most of the anthropogenic heat (for example from traffic) is injected directly into the air, and therefore should not enter via the surface energy budget (it does not directly heat the surface). Even if the time derivative is then dropped from the resolution, this is another indication of the lack of rigor in the definition of the physical meaning of key variables.

e) In the Methods section it is written, "*Hence, the applicability of the model is limited to specific locations, especially when site characteristics play a dominant role in regulating local microclimate (for example, topography, ventilation, water bodies)."* We certainly agree with this statement but believe M19 should have limited their analysis to cities that fulfill these conditions (e.g. small influence from water bodies, topography, etc.). In fact, based on our analysis, approximately one third of the 35000 largest CIESIN (2016) cities (representing 50% of CIESIN's "urban" population) do not fulfill this requirement. That is, 6876 urban areas are less than 50 km away from an ocean coastline (inland water bodies are not included), while 4075 urban areas have an altitude above 1000 m (based on USGS Global Multi-resolution Terrain Elevation Data 2010). However, all cities are kept for analysis and even model calibration. For example, Singapore (an island city) or London (not far from the sea) clearly do not fulfill the stated criteria, but they are often used as examples in the text.

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