

Trees halve urban heat island effect globally but unequal benefits only modestly mitigate climate-change warming

Corresponding Author: Dr Robert McDonald

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

This work provides a rare, global look at the impact of trees on reducing UHI. This allows for cross comparisons like those across climate types. Some papers have looked at tree cooling efficiency across the globe, but this paper offers additional insight into economic variables and the influence of global warming on the relationship.

I think the work is interesting, but overall some of the climatic/geographic discussion and considerations are lacking. Some examples:

1. I have never heard of mezone being used to describe a climate. Instead, this is a description of an ecosystem. Why not stick with the climate zones defined in Figure 2?
2. I have also never heard of "tree cover" being a land-use type or desert as a land-cover type (line 114).
3. A simplistic view of the UHI is presented, only counting for impervious surface % and not mentioning additional variables like surface geometry that play a large role.
4. I did not see a strong scientific explanation of why results vary between climate zones.
5. In addition to water resources, are there limitations in arid regions based on type of trees that can survive there? (e.g., tree height and canopy width).
6. My largest concern is that latitude is not considered. A global study like this should account for latitude because it affects day length and sun angle, which in turn will affect UHI strength and the geometry associated with tree shading, and even the climate classification. Latitude was not discussed at all, but it is known to affect UHI.

At times when reading this work, it feels like experts applied their methods to a problem, instead of addressing the problem as content experts. This can be improved by tightening the climatological and geographical analysis and discussion, with some examples noted above.

Reviewer #2

(Remarks to the Author)

Thank you for the chance to review the manuscript "Tree canopy halves urban heat island effect globally but disproportionately benefits higher incomes and suburbs". This was a fun paper to read and had an interesting take on how to conduct global scale urban heat research in the face of limited global data. I think with some work this paper could be a solid addition to the field of global-scale urban heat research. But currently the way it is structured requires a lot of work to focus the narrative and make it more understandable to the reader. I have two major concerns that need revisions should this paper continue on to acceptance.

The First issue is one of scale and significance to the field. I understand the limitations submitting to a short-form journal in

regard to the text. However, in reading this paper there are so many methods and types of analysis, it gets hard to follow throughout the main text. The authors are conducting a full analysis of CE for hundreds of cities, a UHI analysis, a sensitivity analysis using higher res data for three cities, a sensitivity analysis of the sensitivity analysis comparing WBGT to AT in those three cities, analysis of AT cooling under climate change scenarios, an analysis of the distribution of current AT cooling and future cooling, and an analysis of tree cooling by national income inequality. The combined impact of this chain of analysis make the paper difficult to follow exactly how each of these results are connected. I suggest flow diagram of the methodological approach that could be included in the supplemental. This will greatly help the reader track how the data, analysis, and outputs are all connected.

Throughout the introduction the authors fail to introduce a convincing argument for what makes this an novel study to be included in Nature Communications. They lay out the importance of heat as an epidemiological concern, and that climate change is increasing heat exposure, and that trees are a tool to address that exposure. Then the authors jump to the fact there really hasn't been a global analysis of tree-derived air cooling yet, and that is kind of the total of the rationale for the paper. The authors need to focus this narrative to what is a major gap in our knowledge. Yes, have a primary global air cooling study is great, but aside from the need to create a global dataset, what is the novel question be tested here?

Research goals 1 and 2 are sparse in providing a rationale why this is novel, their goals as described are just the data collection. Goal 3 is getting to a really interesting question (i.e. we know trees differ in cooling benefits across climate regions, but how is that going to shift in future climates and why). And then the introduction just skips setting up other analysis the authors include (the three-city, the WBGT). I suggest a re-write of the introduction to direct the reader to what is really novel in these results, which then provides the rationale for why this study needed to take place.

Secondly, in the method that defines the urban-rural gradient causes some concern. As understand the SUE was developed using a coarser land-cover dataset. Which means it works very well when the removal of built-up area constitutes the wide majority of an urban area. However, I am concerned that with the higher resolution Wordcover data, there is more tree canopy and non-built-up pixels still residing within the urban matrix, still after the removal on non-urban, non-water, and non-snow pixels. By including those pixels into the "rural" data, there is the potential for increasing the bias. I would suggest a simple way to adjust for this would be first clip out the urban core or city polygon from the FUA, then run the SUE. This way you can be sure that potential high-temperature biased tree pixels are not included within the rural data set.

There are other issues I will list in line-comments below, some that require a reconsideration of the analysis, some minor comments on the text. But the largest one to be addressed is that of the authors are currently not convincing me in the text that this paper is at the importance of a major article. There is a solid work is in there, it just needs to be brought to the surface and cleaned up.

Line comments:

Line 53-54. "Heat waves are also projected to get more intense with climate change" – this was just stated on line 51-52

Line 66: This is vague, what constitutes a "row of street trees"? There are multiple studies that can provide a rough average of cooling as a factor of %tree canopy, which is more direct a metric than the dimensionless "row of street trees"

Line 73-74: Incorrect. Global AT meta-analyses exist. See Li et al 2024 <https://www.nature.com/articles/s43247-024-01908-4>, Su 2020 <https://www.sciencedirect.com/science/article/pii/S0168192319303818?via=ihub>

Line 75-76: Yes, while most multi-city studies use LST, the authors should not ignore the multiple multi-city studies that focus on AT (Venter et al 2021 and Du 2024, both of which includes one of the authors of this submitted paper. There is also Hu et al 2019, Naserikia et al 2024, Ibsen et al 2024, as examples of multicity AT tree canopy research)

Line 78 – 79: Again, it's not totally clear, there have been global metanalyses

Line 88: first mention of UHI, no where in the intro does the text differentiate from urban-rural cooling (the UHI effect) and within city cooling. While these follow similar land-cover driven mechanisms, the type of study is different. As mentioned, at this point the reader (me) does not know what this paper is going to be addressing or why.

Line 116-117: I am curious if this pattern is the same in each climate? Also, are higher income countries more clustered within certain climates as well?

Line 126-127: Also that arid areas have greater evapotranspiration, which can increase the transpiration of trees beyond what is ecophysiologically expected, especially those with irrigation, thus increasing localized cooling (Winbourne et al 2020).

Line 133-134 : I might of missed this in the discussion, but do you address why the arid climate time impervious surface% has a different effect on CE?

Line 236 – 238: Good point, planting configuration matters!

Line 265-267: Do you think this is because the cooling mechanisms of trees have a physiological limit. I.e. there is only so much surface temperature reduction by shade possible, and only so much transpirational cooling allowed by the physiology of a tree?

Line 281-286: This is all well and good, but these discussion points has been stated so many times before. Yes, more trees should be focused to places with current low levels of cover, which corresponds with lower income countries and areas. I think this study needs to also note the limitations that this scale of analysis does not actually provide guidance on either the finer-scale places to plant trees (where we know tree configuration has an effect on cooling benefits as mentioned in line 236), or the type and size of tree (which we also know has an effect across many cities, again see Li et al 2024 among many other single-city studies). It is my own bugaboo, but the "past is not prologue" doesn't ring true when the call-to-action is the same as I have read many times. This work gives a very important result (trees can reduce UHI but only to a certain point). The authors seem to shy away from expanding on that (what is the mechanism behind that limitation in trees?, what does that mean for within city heat and not just UHI?). So much analysis went into this study, I would like to see a little more interpretation aside from what has been said in many urban heat papers.

Reviewer #3

(Remarks to the Author)

The manuscript addresses an important topic on the global cooling benefits of urban tree canopy. However, overall, the manuscript is not very well written, without a clear focal question, and with results poorly presented. The study attempts to cover many different aspects -global estimates of air temperature (AT) reduction, distributional inequality, comparison with UHI and climate change but the narrative lacks a clear focus. Results and discussion are often mixed together, making it difficult to follow the main findings. Many conclusions are either self-evident or speculative, and the manuscript does not substantially advance the field.

Major Concerns

1. Lack of a focused research question

The manuscript sets out multiple objectives, but the key scientific gap is not clearly articulated. It remains unclear what central question the authors want to answer and how this study advances beyond existing literature.

2. Mixing results and discussion

Throughout the results section, interpretation and speculation are interwoven with data presentation. This makes it hard for readers to distinguish between empirical evidence and authors' interpretation. Results should first be clearly and objectively presented, and only then interpreted in the discussion.

3. Findings are often trivial or already well-established

Several conclusions (e.g., tree cover declines with population density, higher-income countries have more trees, suburban areas receive greater cooling than dense city centers) are already widely known. The novelty and scientific contribution of these descriptive analyses are limited.

4. Problematic inference regarding WBGT vs. AT

The claim that tree canopy reduces WBGT 3.1 times more than AT, and that health impacts may therefore be underestimated, is problematic. WBGT and AT are fundamentally different measures and not linearly related. A 1°C reduction in AT does not correspond to the same health implications as a 1°C reduction in WBGT. The conclusion is therefore overstated.

5. Inconsistency between models

The manuscript combines statistical regression models with process-based i-Tree simulations. These approaches differ in resolution, input data, and assumptions, yet their results are used interchangeably without sufficient validation or comparison. This undermines confidence in the robustness of the conclusions.

6. Unrealistic scenario design

Counterfactual scenarios such as "complete loss of tree canopy" or "uniform 10% increase in canopy" are overly simplistic and lack real-world relevance. More policy-relevant or empirically grounded scenarios should be considered.

7. Limited contribution to the field

The analyses remain largely descriptive, with limited theoretical insight into mechanisms or actionable guidance for policy. The study falls short of addressing more meaningful questions such as the marginal effectiveness of greening in vulnerable areas, scale dependencies of cooling benefits, or the alignment between cooling potential and social vulnerability.

Minor Concerns

Some conclusions are not adequately supported by data. For example, the claim that the effect of country-level income on total cooling is stronger than that of aridity lacks statistical testing.

Case-specific comparisons (e.g., Phoenix vs. Gothenburg) are presented without deriving generalizable rules.

The statement that finer-scale variation is underestimated by a 1 km model is self-evident and does not constitute a meaningful finding.

The term "population-weighted mean average density" is unclear and should be defined more precisely.

Conclusion

In its current form, the manuscript reads as a collection of loosely connected analyses rather than a coherent scientific contribution. Major restructuring and sharpening of the research question are needed before the paper can make a meaningful impact in the urban heat mitigation literature.

Reviewer #4

(Remarks to the Author)

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Thank you for your thorough revisions based on the reviews. This is a much stronger manuscript with a clear gap presented and more appropriate use of terminology. All of my concerns have been addressed.

Reviewer #2

(Remarks to the Author)

I sincerely want to thank the authors for address all my comments and the other reviewers comments so thoroughly. I can see the narrative and rationale for the study is much clearer, and the addition of the buffering analysis has further refined the conclusions. Thank you.

I have no more comments to add on aside from the authors in the response state that the title of the paper has been changed to "Trees almost halve urban heat island effect globally but disproportionately benefit higher incomes and suburbs", but the current title is changed to "Trees halve urban heat island effect globally but unequal benefits only modestly mitigate climate-change warming"

I would like a brief clarification on what the current title of the paper will be.

Other than that, I have no more issues that need resolution

Reviewer #3

(Remarks to the Author)

The authors have responded carefully and thoroughly to the reviewers' comments, and the revised manuscript is technically sound and clearly written. The analyses are comprehensive at the global scale, and the main conclusions are well supported by the data.

Reviewer #4

(Remarks to the Author)

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

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We have now thoroughly rewritten the manuscript in response to comments from 4 reviewers. This document describes changes made in the manuscript in response to reviewer comments. Original text from reviewers is in black below, while our response is in green. We begin by describing at a high level the changes made, then provide a point-by-point response to specific comments.

Major changes made:

Here, we describe at a high level the changes made. Note that some of the text describing changes made is identical to our responses to specific reviewers' comments below, but we thought it clearer to list directly our major changes made at the start of this document, even at the risk of duplication.

Clarified novelty-

We describe more clearly what is novel about our analysis in the rewritten Introduction, and list three ways our analysis is novel:

- Consideration of both UHI and tree cooling in the same analysis, using air temperature rather than the less physiologically meaningful LST
- Climate change projections compared to tree cooling patterns
- A globally comprehensive analysis of all 8,919 large urban areas, a quantity of cities that is 100x bigger than previously published papers that looked at AT cooling.

More precise framing

We frame more precisely our research goals, articulating three new research questions that have not been answered before with a comprehensive global assessment:

- a. How much bigger would the AT UHI be on average without urban tree canopy?
- b. How much is AT cooling from current or plausible future urban tree canopy able to mitigate AT warming due to climate change?
- c. How equally are AT cooling benefits distributed with respect to climate, level of economic development, and population density?

Editing to focus the manuscript

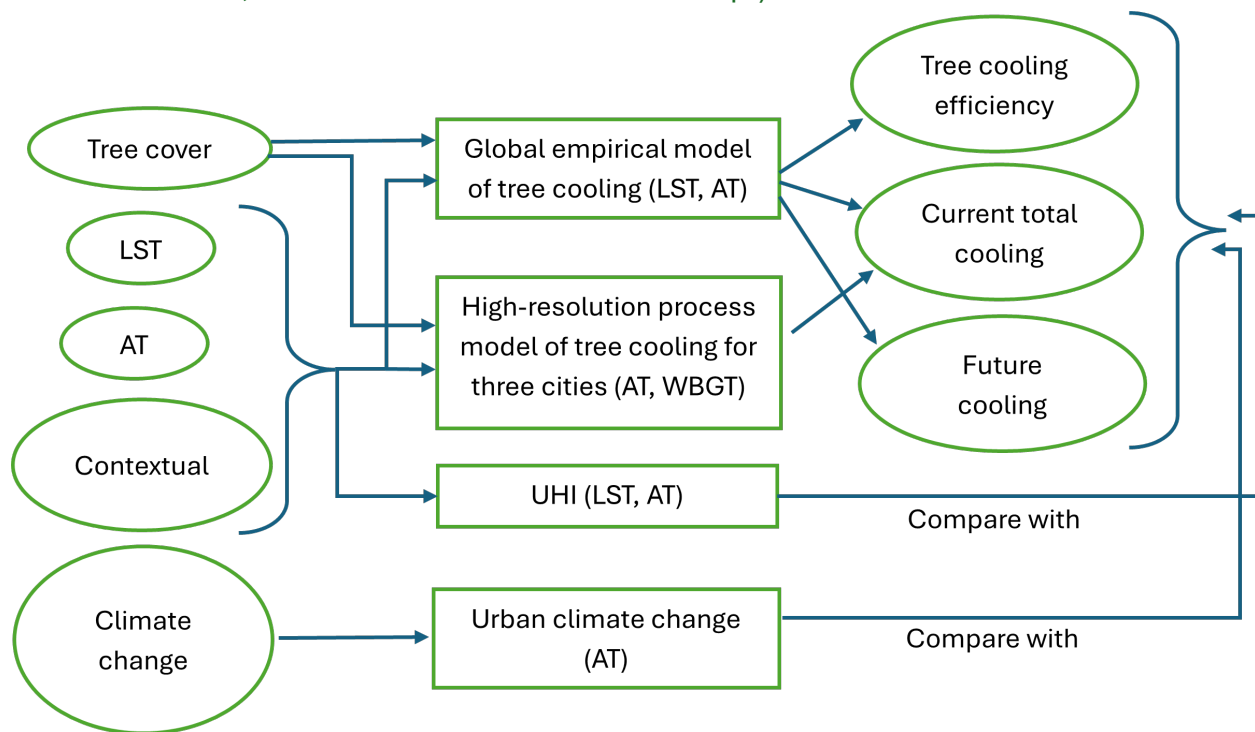
The following high-level changes have been made to the manuscript. Each is described in more detail below in our response to reviewers' comments:

Title- Changed to present our findings relative to the three research questions we are considering, within the 15-word limit of the journal.

Abstract- Revised to present our findings relative to the three research questions we are considering.

Introduction- The addition of discussion of UHI and inequality, to better align with our results; More clear discussion of novelty of the paper; Revised research questions.

Note that at the end of the Introduction we have added a flow chart of our methodology, at the request of one reviewer. We believe this should make it easier for readers to follow the logic of our analysis (particularly as readers will have not yet read the Methods section, which comes later in the manuscript).



Increased interpretation and discussion of results

We have completely rewritten the Results section, to achieve the following goals:

- 1.) Exclusion of citations and interpretation, which is moved now to the Discussion section.
- 2.) Align our results with the three new research questions around which we frame the paper.

- 3.) Address specific issues from reviewers, related to how to accurately present our findings.

Then, the discussion section has been rewritten to focus less on the policy-relevance of our findings, and more on the interpretation of our results, paying especial attention to issues flagged by reviewers, such as the need to discuss why our results vary by climate.

Clarification of the gap in knowledge this paper fills

We list three gaps in the literature in the Introduction, presenting (as much as space allows) a review of relevant past literature, and then in the last paragraph of the Introduction describe how our analysis fills these three gaps.

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

This work provides a rare, global look at the impact of trees on reducing UHI. This allows for cross comparisons like those across climate types. Some papers have looked at tree cooling efficiency across the globe, but this paper offers additional insight into economic variables and the influence of global warming on the relationship.

We thank Reviewer #1 for their helpful comments and suggestions.

I think the work is interesting, but overall some of the climatic/geographic discussion and considerations are lacking. Some examples:

1. I have never heard of mezine being used to describe a climate. Instead, this is a description of an ecosystem. Why not stick with the climate zones defined in Figure 2?

Change made. We believe the reviewer is referring here to the word “mesic”. We have reviewed the text in the manuscript so that it corresponds exactly with what we did in our analysis. The climate zones we present data for in the paper are indeed those presented in Figure 2. We have avoided use of “mesic” as a synonym for “humid”.

2. I have also never heard of “tree cover” being a land-use type or desert as a land-cover type (line 114).

Change made. We have rewritten the sentence to be clear as to its meaning: Desert or semi-arid biomes tend to have less tree cover.

3. A simplistic view of the UHI is presented, only counting for impervious surface % and not mentioning additional variables like surface geometry that play a large role.

Change made. We have added context and a citation in the Introduction noting that urban surface geometry (e.g., building height, spacing, and orientation) influences UHI intensity by affecting the energy balance. We note also to the reviewer that the empirical datasets we use of LST (measured) and AT (measured directly but interpolated using information on LST and other factors) inherently capture the integrated effects of these factors, including surface geometry. Thus, pixels representing urban structures that enhance solar absorption or limit ventilation will tend to record higher LST and AT values, meaning these geometric influences are implicitly accounted for in our analysis.

4. I did not see a strong scientific explanation of why results vary between climate zones.

As described above in major changes, we have completely rewritten and expanded our interpretation and discussion of our results, both in the Results and (especially) the Discussion section. In particular, we discuss why more arid climates may have a different functional relationship between tree cover and AT than do humid climates.

5. In addition to water resources, are there limitations in arid regions based on type of trees that can survive there? (e.g., tree height and canopy width).

In our revised Discussion section, we note that aridity affects not just tree cover (which we observe), but also likely tree height and canopy width (although not directly observed in this study but empirically observed in other studies).

6. My largest concern is that latitude is not considered. A global study like this should account for latitude because it affects day length and sun angle, which in turn will affect UHI strength and the geometry associated with tree shading, and even the climate classification. Latitude was not discussed at all, but it is known to affect UHI.

We thank the reviewer for highlighting the importance of latitude in influencing UHI magnitude through its effects on solar insolation, day length, and sun angle. We have made three changes to respond to this comment.

First, we have now fully incorporated latitude as a potential explanatory variable in our analysis. However, latitude was not a statistically significant predictor of AT anomaly for FUA (our response variable), which was our response variable. See text for details.

To understand why this may be the case, we ask the reviewer to note that our regression focused on the anomaly in AT within a FUA, not the absolute value of the AT. That is, our regression models describe within-FUA patterns in AT anomaly, how much hotter and colder particular pixels within a FUA are due to local factors like land cover. Variables that change primarily among FUAs, such as latitude and aridity, by contrast, affect the FUA-level average AT, and not its spatial variation within each FUA.

We allow the slope of regression parameters relating to AT anomaly and explanatory variables to vary by aridity zones (which were better predictors than Koppel climate zones). We directly tested the ability of Latitude categories to replace aridity zones, but this was a significantly worse model fit.

Second, we have included a supplementary figure to show that aridity zones (and climate zones) also correlate with latitude. On average FUAs close the equator (low latitude) have higher summer AT, while those at higher latitudes have lower summer AT. Therefore, the empirical UHI (as well as what it might hypothetically be without any tree cover) are correlated with latitude. Our findings are similar to previous research (Wienert et al. 2005; Tzavali 2015), showing that UHI intensity was greater at high latitudes than low latitudes.

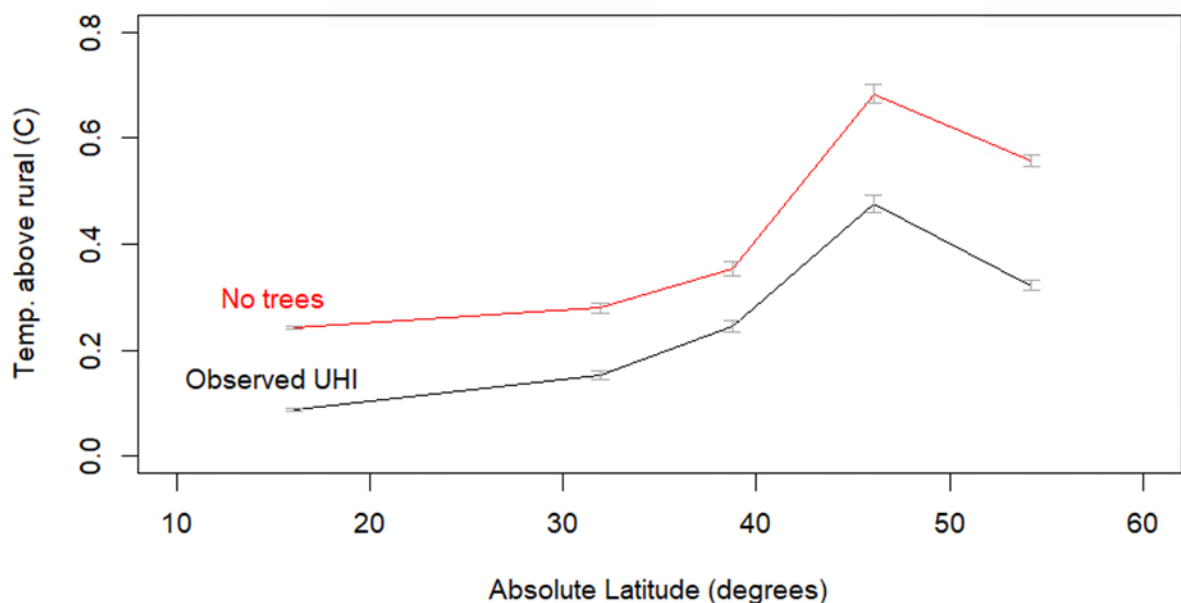


Figure S1. The relationship between latitude and the urban heat island (UHI). The world's FUAs are divided into quintiles based on absolute latitude, and the average observed UHI is calculated, based on the SUE algorithm. Also shown is the hypothetical UHI that would occur with no tree cover, calculated using our global regression model. Error bars are standard errors of the mean.

Third, we have expanded the discussion in the Introduction accordingly of this issue, situating latitude among the factors that control urban–rural temperature differences globally.

At times when reading this work, it feels like experts applied their methods to a problem, instead of addressing the problem as content experts. This can be improved by tightening the climatological and geographical analysis and discussion, with some examples noted above.

Please see the description above of how we rewrote the manuscript, especially the sections “Increased interpretation and discussion of results” and “Editing to focus the manuscript”, which we believe have tightened the manuscript and its climatological and geographical analysis.

Reviewer #2 (Remarks to the Author):

Thank you for the chance to review the manuscript “Tree canopy halves urban heat island effect globally but disproportionately benefits higher incomes and suburbs”. This was a fun paper to read and had an interesting take on how to conduct global scale urban heat research in the face of limited global data. I think with some work this paper could be a solid addition to the field of global-scale urban heat research. But currently the way it is structured requires a lot of work to focus the narrative and make it more understandable to the reader. I have two major concerns that need revisions should this paper continue on to acceptance.

The First issue is one of scale and significance to the field. I understand the limitations submitting to a short-form journal in regard to the text. However, in reading this paper there

are so many methods and types of analysis, it gets hard to follow throughout the main text. The authors are conducting a full analysis of CE for hundreds of cities, a UHI analysis, a sensitivity analysis using higher res data for three cities, a sensitivity analysis of the sensitivity analysis comparing WBGT to AT in those three cities, analysis of AT cooling under climate change scenarios, an analysis of the distribution of current AT cooling and future cooling, and an analysis of tree cooling by national income inequality. The combined impact of this chain of analysis make the paper difficult to follow exactly how each of these results are connected. I suggest flow diagram of the methodological approach that could be included in the supplemental. This will greatly help the reader track how the data, analysis, and outputs are all connected.

We thank Reviewer #2 for recognizing the value of our study and for the insightful comments that have helped us to improve our study.

Change made. As part of the broader revisions to the paper, we have added a flow diagram as requested. We have also rewritten the text to use subsection names that correspond to this flow diagram, while making as clear as possible which part of the analysis we are discussing, and how the various parts of the analysis fit together.

Throughout the introduction the authors fail to introduce a convincing argument for what makes this an novel study to be included in Nature Communications. They lay out the importance of heat as an epidemiological concern, and that climate change is increasing heat exposure, and that trees are a tool to address that exposure. Then the authors jump to the fact there really hasn't been a global analysis of tree-derived air cooling yet, and that is kind of the total of the rationale for the paper. The authors need to focus this narrative to what is a major gap in our knowledge. Yes, have a primary global air cooling study is great, but aside from the need to create a global dataset, what is the novel question be tested here? Research goals 1 and 2 are sparse in providing a rationale why this is novel, their goals as described are just the data collection. Goal 3 is getting to a really interesting question (i.e. we know trees differ in cooling benefits across climate regions, but how is that going to shift in future climates and why). And then the introduction just skips setting up other analysis the authors include (the three-city, the WBGT). I suggest a re-write of the introduction to direct the reader to what is really novel in these results, which then provides the rationale for why this study needed to take place.

Change made. We thank the reviewer for this helpful comment. We have substantially rewritten the introduction to better establish the novelty and rationale of our study and to clearly articulate the major knowledge gaps that our research addresses:

- 1.) **Clarifying the study's novelty and knowledge gaps.** We describe more clearly what is novel about our analysis, and list three gaps in the current scientific knowledge that our research helps fill:
 - a. Consideration of both UHI and tree cooling in the same analysis, using the more physiologically meaningful air temperature (AT) measurements rather than LST.
 - b. Climate change projections compared to tree cooling patterns
 - c. A globally comprehensive analysis of all 8,919 large urban areas, a quantity of cities that is 100x bigger than previously published papers that looked at Air temperature (AT) cooling.
- 2.) **Refining and reframing the research goals.** We frame more precisely our research goals, articulating three new research goals that have not been answered before with a comprehensive global assessment:
 - a. How much bigger would the AT UHI be on average without urban tree canopy?
 - b. How much is AT cooling from current or plausible future urban tree canopy able to mitigate AT warming due to climate change?
 - c. How equally are AT cooling benefits distributed with respect to climate, level of economic development, and population density?
- 3.) **Linking all analyses and situating the work in context.** We mention UHI and the WBGT analysis, placing our analyses more in the context of existing literature. We have also added a paragraph on inequality, among cities and within cities.
- 4.) **Expanding integration with existing literature.** We have also, based on comments from all the reviewers, increased the citations and text in the Introduction to other related literature, to show our paper's novel contribution relative to these earlier papers.

Secondly, in the method that defines the urban-rural gradient causes some concern. As understand the SUE was developed using a coarser land-cover dataset. Which means it works very well when the removal of built-up area constitutes the wide majority of an urban area. However, I am concerned that with the higher resolution Wordcover data, there is more tree canopy and non-built-up pixels still residing within the urban matrix, still after the removal on non-urban, non-water, and non-snow pixels. By including those pixels into the "rural" data, there is the potential for increasing the bias. I would suggest a simple way to adjust for this would be first clip out the urban core or city polygon from the FUA, then run the SUE. This way you can be sure that potential high-temperature biased tree pixels are not included within the rural data set.

Change made. We thank the reviewer for this valuable methodological suggestion. To address these concerns, we have expanded our analysis to include an additional UHI estimation using a “buffering” approach conceptually similar to what the reviewer recommends (see new text in the Methods section for details). This approach, which has been widely used in the literature, excludes the urban core when defining rural reference areas and thereby reduces the potential influence of residual tree canopy or non-built pixels within the urban matrix. We have then rewritten the manuscript to present both the results with the SUE algorithm, as well as with the buffering algorithm.

As the reviewer anticipated in their comments, the SUE algorithm has slightly lower estimates for UHI than the buffering algorithm, in part because tree canopy and non-built-up pixels that within the urban matrix are included in the estimate of UHI. This leads to a slightly lower fraction of maximum potential UHI mitigated by trees ($41.3 \pm 1.3\%$) with the buffering algorithm than with the SUE algorithm ($48.6 \pm 1.3\%$). To accurately reflect these results, we have updated the manuscript title to: “Trees almost halve urban heat island effect globally but disproportionately benefit higher incomes and suburbs”.

There are other issues I will list in line-comments below, some that require a reconsideration of the analysis, some minor comments on the text. But the largest one to be addressed is that of the authors are currently not convincing me in the text that this paper is at the importance of a major article. There is a solid work is in there, it just needs to be brought to the surface and cleaned up.

Change made. We have thoroughly rewritten the article to highlight its novelty and importance and improve the presentation of our results.

We have greatly expanded the consideration of past studies in our Introduction, and list three gaps in current scientific knowledge. We then briefly state how our research helps fill these three gaps (details of the implementation are provided in the Methods section) while answering three specific, novel research questions that have not been answered before in a comprehensive global assessment:

1. How much bigger would the AT UHI be on average without urban tree canopy?
2. How much is AT cooling from current or plausible future urban tree canopy able to mitigate AT warming due to climate change?
3. How equally are AT cooling benefits distributed with respect to climate, level of economic development, and population density?

Line comments:

Line 53-54. “Heat waves are also projected to get more intense with climate change” – this was just stated on line 51-52

Change made to avoid repetition.

Line 66: This is vague, what constitutes a "row of street trees"? There are multiple studies that can provide a rough average of cooling as a factor of %tree canopy, which is more direct a metric than the dimensionless “row of street trees”

Change made. We have rewritten the sentence to clarify that the review article in question looked at empirical (field) measurements of AT at different distances from rows of street trees, not remotely sensed estimates of tree canopy and LST. We note that we discuss average LST TCE found in other papers elsewhere in the manuscript.

Line 73-74: Incorrect. Global AT meta-analyses exist. See Li et al 2024

<https://www.nature.com/articles/s43247-024-01908-4> , Su 2020

<https://www.sciencedirect.com/science/article/pii/S0168192319303818?via=ihub>

Change made. We thank reviewer for providing these references. We have updated our text to more precisely state what is novel in our analysis, as well as cite these studies where appropriate.

While meta-analyses of tree cooling efficiency that examine results from multiple studies across regions and samples using various approaches and data sources are available (including the works from Li and Su), global analysis using a consistent methodology are missing.

Note that one thing that is novel about our study is that it is a complete, global survey of AT tree cooling for all large urban areas, rather than a study or metanalysis of a sample of cities. Li et al. looked at 110 cities/regions and Su et al. looked at 35 cities, compared to our analysis of all 8,919 large urban areas. As we now discuss in the Introduction, this allows a more complete analysis of the distribution of AT tree cooling benefits across climate, level of economic development, and urban form.

Line 75-76: Yes, while most multi-city studies use LST, the authors should not ignore the multiple multi-city studies that focus on AT (Venter et al 2021 and Du 2024, both of which includes one of the authors of this submitted paper. There is also Hu et al 2019, Naserikia et al 2024, Ibsen et al 2024, as examples of multicity AT tree canopy research)

Change made. We have included some of these citations, where appropriate, and we have been clear about what is novel about our analysis. We now list three research gaps in scientific knowledge and describe how our paper helps fill them.

We thank the reviewer for the specific paper suggestions, and we reviewed each of them carefully to determine how to cite them in our manuscript. We have also conducted a literature review to add other relevant papers to the Introduction, around the UHI, climate change, and tree cooling. Some specific comments on the reviewer's suggested citations:

We note that Venter et al. 2021 and Hu et al. 2019 focus on the UHI, not tree cooling per se (the term "canopy urban heat island" in these papers is essentially the UHI in AT), but we have now cited these papers in the new portion of the Introduction discussing UHI science.

Naserikia et al. 2024 compared AT with LST for 55 cities, but their study is not about tree cooling or the UHI per se. For that reason, we did not cite it in our rewritten Introduction.

We agree that Du et al. 2024 looked at AT cooling for a large sample of European cities, and this paper is now cited at an appropriate place in the rewritten Introduction. Similarly, Ibsen et al. looked at AT cooling for 8 US cities and is now cited in the Introduction.

We have also highlighted in the introduction that a point of novelty of our paper is our globally complete analysis of all 8,919 large urban areas.

Line 78 – 79: Again, it's not totally clear, there have been global metanalyses

Change made. We have rewritten this sentence to be clearer. We now acknowledge and reference the previous literature on this topic, including metanalyses of sets of cities, while highlighting our paper is (to our knowledge) the first globally complete analysis of AT cooling of all 8,919 large urban areas.

Line 88: first mention of UHI, no where in the intro does the text differentiate from urban-rural cooling (the UHI effect) and within city cooling. While these follow similar land-cover driven mechanisms, the type of study is different. As mentioned, at this point the reader (me) does not know what this paper is going to be addressing or why.

Change made. The Introduction has been rewritten to present our research goals and novelty more clearly, which also has changed the presentation of the UHI effect. There is now a paragraph describing UHI literature, which aims to address the reviewer's point about the UHI effect versus within city cooling. Note that one of the goals of this paper is comparing the magnitude of the UHI with the magnitude of the AT tree cooling effect.

Line 116-117: I am curious if this pattern is the same in each climate? Also, are higher income countries more clustered within certain climates as well?

Change made. We have added a supplementary figure showing the relationship between country-income and the aridity categories used in this study. The reviewer is correct that, on average, higher-income countries are in more humid regions and lower-income countries are in more arid climates, although there are significant exceptions to this trend (e.g., some of the world's richest countries are states in the Middle East with arid climates but high incomes due to oil and natural gas production).

The new supplementary figure added is shown below:

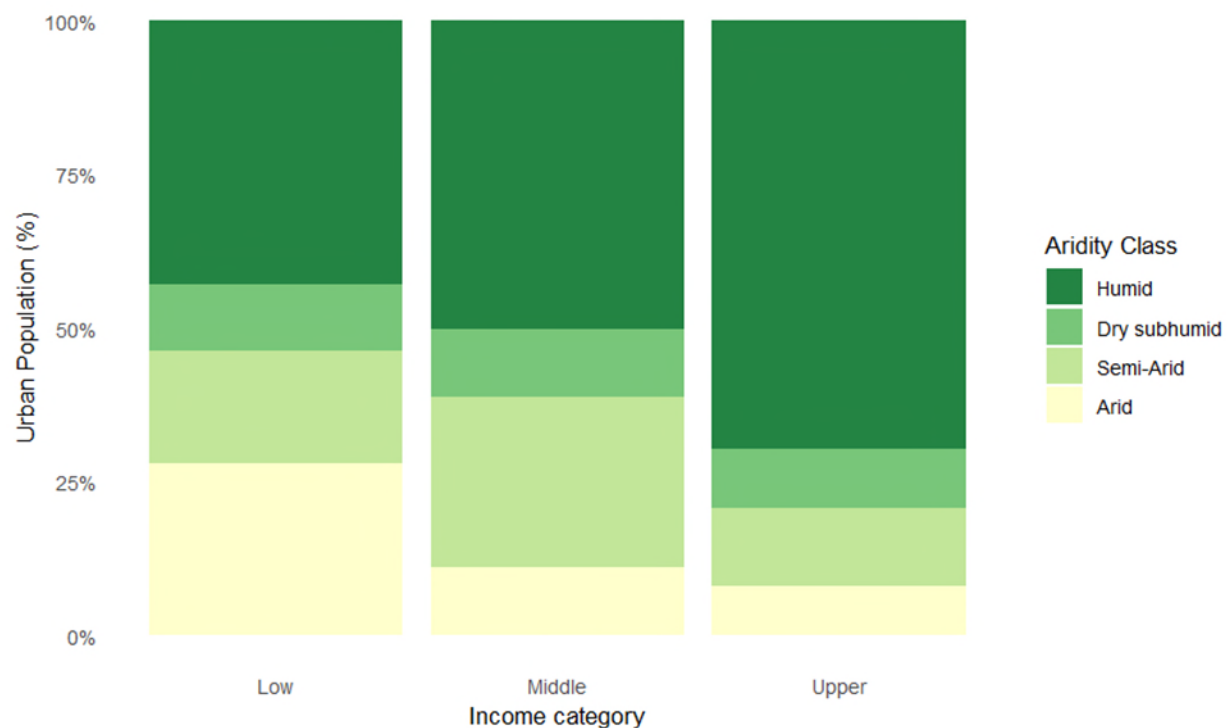


Figure S2. The percentage of urban dwellers in different aridity categories in the world's large functional urban areas (FUAs), as a function of country-level income category. The Y-axis shows the cumulative percentage, see text for details. Note the different total urban populations in large FUAs in each income category: Low (138 million), Middle (2,491 million), and Upper (918 million)

Line 126-127: Also that arid areas have greater evapotranspiration, which can increase the transpiration of trees beyond what is ecophysiologically expected, especially those with irrigation, thus increasing localized cooling (Winbourne et al 2020).

Change made. We have updated the sentence to reference this briefly.

Line 133-134 : I might of missed this in the discussion, but do you address why the arid climate time impervious surface% has a different effect on CE?

We have rewritten the Discussion section to better discuss the interpretations of our results, including this question (also raised by Reviewer #1, see the response there).

Line 236 – 238: Good point, planting configuration matters!

We have retained this sentence during our rewrite, as we agree with the reviewer is an important point.

Line 265-267: Do you think this is because the cooling mechanisms of trees have a physiological limit. I.e. there is only so much surface temperature reduction by shade possible, and only so much transpirational cooling allowed by the physiology of a tree?

Change made. We thank the reviewer for this insightful observation. We agree with the reviewer's interpretation that the magnitude of tree-mediated cooling is constrained by both physiological and practical limits. We have revised the Discussion section to make this more explicit, noting that (i) biophysical limits to shading and evapotranspiration impose an upper bound on how much trees can reduce air and surface temperatures, and (ii) in densely built environments, available space and conflict with infrastructure further restrict potential canopy expansion. We believe this clarification helps frame the observed ceiling in cooling effects within both ecological and urban design contexts.

Line 281-286: This is all well and good, but these discussion points has been stated so many times before. Yes, more trees should be focused to places with current low levels of cover, which corresponds with lower income countries and areas. I think this study needs to also note the limitations that this scale of analysis does not actually provide guidance on either the finer-scale places to plant trees (where we know tree configuration has an effect on cooling benefits as mentioned in line 236), or the type and size of tree (which we also know has an effect across many cities, again see Li et al 2024 among many other single-city studies). It is my own bugaboo, but the "past is not prologue" doesn't ring true when the call-to-action is the same as I have read many times. This work gives a very important result (trees can reduce UHI but only to a certain point). The authors seem to shy away from expanding on that (what is the mechanism behind that limitation in trees?, what does that mean for within city heat and not just UHI?). So much analysis went into this study, I would

like to see a little more interpretation aside from what has been said in many urban heat papers.

Change made. In our revised Discussion, we have given less focus to the policy implications of our result, and more to the “limits to cooling” points that reviewer emphasizes.

Reviewer #3 (Remarks to the Author):

The manuscript addresses an important topic on the global cooling benefits of urban tree canopy. However, overall, the manuscript is not very well written, without a clear focal question, and with results poorly presented. The study attempts to cover many different aspects -global estimates of air temperature (AT) reduction, distributional inequality, comparison with UHI and climate change but the narrative lacks a clear focus. Results and discussion are often mixed together, making it difficult to follow the main findings. Many conclusions are either self-evident or speculative, and the manuscript does not substantially advance the field.

We thank Reviewer #3 for their comments. We have substantially rewritten the manuscript to highlight its novelty and to clearly present our advancement to the field by presenting three research gaps in the scientific knowledge that our study helps to fill.

Note that there are three new research questions listed that better align with the analysis results presented. We have also greatly expanded the consideration of past literature in the Introduction.

We have addressed each of the reviewer’s points below through substantive revisions, with each change representing a clear advance on existing literature. We respectfully invite the reviewer to read the rewritten *Introduction* and revised framing considering these updates, and we hope the revised version now fully addresses their concerns.

Major Concerns

1. Lack of a focused research question

The manuscript sets out multiple objectives, but the key scientific gap is not clearly articulated. It remains unclear what central question the authors want to answer and how this study advances beyond existing literature.

Change made. We have rewritten the Introduction section to more clearly present existing literature, state clearly what is novel in this study, and then focus on three research questions:

- How much bigger would the AT UHI be on average without urban tree canopy?
- How much is AT cooling from current or plausible future urban tree canopy able to mitigate AT warming due to climate change?
- How equally are AT cooling benefits distributed with respect to climate, level of economic development, and population density?

2. Mixing results and discussion

Throughout the results section, interpretation and speculation are interwoven with data presentation. This makes it hard for readers to distinguish between empirical evidence and authors' interpretation. Results should first be clearly and objectively presented, and only then interpreted in the discussion.

Change made. We have separated results and discussion in the revised manuscript. Note that this makes the text slightly less compact, since we must reintroduce to some extent some of our findings in the discussion section, but we agree with the reviewer that this change improves clarity and overall readability.

3. Findings are often trivial or already well-established

Several conclusions (e.g., tree cover declines with population density, higher-income countries have more trees, suburban areas receive greater cooling than dense city centers) are already widely known. The novelty and scientific contribution of these descriptive analyses are limited.

Change made. We appreciate the reviewer's comment and have adjusted the emphasis in the revised manuscript, to focus more strongly on the novel aspects of our findings. Topics that describe well-established patterns (e.g., the relationships between tree cover, population density, and income) now receive more concise treatment. We have, however, retained some mention of these topics, because they are of clear policy relevance and practical importance, and because, importantly, this study provides the first global comprehensive survey of all large FUAs that has been able to assess the magnitude of their impact on AT tree cooling. This empirical confirmation at global scale helps benchmark local studies and highlights the significance of these relationships in shaping urban heat exposure worldwide.

4. Problematic inference regarding WBGT vs. AT

The claim that tree canopy reduces WBGT 3.1 times more than AT, and that health impacts may therefore be underestimated, is problematic. WBGT and AT are fundamentally different measures and not linearly related. A 1°C reduction in AT does not correspond to the same health implications as a 1°C reduction in WBGT. The conclusion is therefore overstated.

Change made. We have revised the manuscript to clarify our meaning. Our intent was not to imply that they are linearly related and agree with the reviewer that AT and WBGT have different health implications. We have rewritten the Results section to highlight why it is helpful to examine sites of different levels of aridity (Phoenix, Lisbon, Gothenburg), as the relationship between AT and WBGT depends in part on aridity (or other meteorological parameters related to aridity).

5. Inconsistency between models

The manuscript combines statistical regression models with process-based i-Tree simulations. These approaches differ in resolution, input data, and assumptions, yet their results are used interchangeably without sufficient validation or comparison. This undermines confidence in the robustness of the conclusions.

Change made. We thank the reviewer for this valuable comment. We have rewritten our Methods section to clarify the relationship between the statistical regression model and the process-based i-Tree simulations. The comparison of two different kinds of models, operating at different spatial scales, is meant to give the reader a sense of similarities and differences between the two models.

By highlighting both convergence and divergence between these models, we aim to reinforce rather than weaken confidence in our conclusions. Agreement between two conceptually distinct approaches increases robustness of inference at the global scale, while any differences provide useful context for interpreting uncertainty.

6. Unrealistic scenario design

Counterfactual scenarios such as "complete loss of tree canopy" or "uniform 10% increase in canopy" are overly simplistic and lack real-world relevance. More policy-relevant or empirically grounded scenarios should be considered.

Change made. We have addressed this important point in two ways.

First, we have clarified in the manuscript that the two original scenarios (complete loss of tree canopy and uniform 10% increase in tree canopy) are not intended as short-term or policy-relevant projections of changes. Rather, they serve specific analytical purposes. The

“complete loss of tree canopy” scenario allows the estimation of the cumulative impact of current tree canopy on AT and how much greater UHI would be without currently extant tree cover. The “10% increase in tree canopy” scenario allows estimation of TCE, a widely used metric of how a 10% change in tree canopy cover would change temperature (i.e., a sensitivity analysis). An estimation of AT TCE for all large cities globally was one of the goals of this research.

Second, we agree with the reviewer that consideration of a realistic, policy-relevant scenario of tree cover change would be of utility to policy makers. Such a scenario is difficult to construct in a sophisticated way for all large cities globally, as we often do not have information on what laws and policies and customs constrain tree planting and canopy enhancement. However, in consultation with policymakers involved in the UNFCCC process, we identified a policy interest in knowing how much additional tree cover could plausibly reduce AT. That is, policymakers expressed an interest in knowing how much additional tree cover could plausibly reduce AT, and whether this was a meaningful fraction of the expected increase in urban AT due to climate change.

We therefore constructed a third, more realistic, policy-relevant scenario that is integrated into the manuscript and Abstract, the “maximum plausible increase in tree canopy” scenario. This scenario constrains any increase in tree canopy coverage to keep total tree canopy cover below the 90th percentile of tree cover within a given aridity and impervious surface category. As described in the Methods, this approach provides a realistic constraint on how much tree cover can be plausibly added, given climate and urban form constraints.

7. Limited contribution to the field

The analyses remain largely descriptive, with limited theoretical insight into mechanisms or actionable guidance for policy. The study falls short of addressing more meaningful questions such as the marginal effectiveness of greening in vulnerable areas, scale dependencies of cooling benefits, or the alignment between cooling potential and social vulnerability.

Change made. We have completely rewritten the manuscript to present clearly the areas of novelty of the study and the specific research questions we are answering that have not been answered before in the literature.

Specifically, we list three ways our analysis is novel:

- Consideration of both UHI and tree cooling in the same analysis, using more physiologically meaningful measurements of air temperature (AT) as opposed to surface temperature.
- Climate change projections compared to tree cooling patterns
- A globally comprehensive analysis of all 8,919 large urban areas, a quantity of cities that is 100x bigger than previously published papers that looked at AT cooling.

We then list three specific, novel research questions that have not before been answered in a comprehensive global assessment:

- 1.) How much bigger would the AT UHI be on average without urban tree canopy?
- 2.) How much is AT cooling from current or plausible future urban tree canopy able to mitigate AT warming due to climate change?
- 3.) How equally are AT cooling benefits distributed with respect to climate, level of economic development, and population density?

We respectfully invite the reviewer to view our results from the perspective of these clarified research aims. We agree that other topics, such as the marginal effectiveness of greening in vulnerable areas or the detailed alignment of cooling with social vulnerability, are important but fall beyond the feasible scope of this global-scale analysis. Given suggestions by other reviewers to maintain focus on our core objectives, we have not expanded into these areas, but we highlight some of them in the Discussion as promising directions for future research.

Minor Concerns

Some conclusions are not adequately supported by data. For example, the claim that the effect of country-level income on total cooling is stronger than that of aridity lacks statistical testing.

Change made. We thank the reviewer for noting this point. We have strived throughout our manuscript to ensure that all statements are supported by statistical evidence and accompanied by measures of uncertainty. Note that this specific sentence comparing the relative influence of country-level income and aridity has been removed during the broader rewrite of the Results, in particular rewriting the text to accommodate the redesigned Figure 5.

Case-specific comparisons (e.g., Phoenix vs. Gothenburg) are presented without deriving generalizable rules.

Change made. We have rewritten the text in this section to be clear what readers should draw from the case-study comparisons.

The statement that finer-scale variation is underestimated by a 1 km model is self-evident and does not constitute a meaningful finding.

Change made. We have deleted the sentence we believe the reviewer was referring to, which we agree was not needed in the text. The revised text in that section now focuses on the substantive point we intended to make: “This fine-scale variation is missing from our global model, which means that while we may be capturing average patterns of cooling by trees, we may be underestimating the variation in the individual exposure to the heat mitigation benefits of trees.” The fact that fine-scale variation ($<1\text{km}$) is of the same order of magnitude as coarse-scale variation ($>1\text{km}$) has important implications for evaluating heat risk within cities and would not necessarily be the case for other weather-related risks. We believe this clarification is important, as the scale of temperature variability within cities is comparable to that observed across larger regions, unlike other climate variables such as precipitation. Precipitation varies less at small spatial scales (under 1km) than it does over large spatial scales ($> 1\text{km}$), excepting situations with strong topographic or climatic gradients.

The term “population-weighted mean average density” is unclear and should be defined more precisely.

Change made. Our use of language here was incorrect, since we used both “mean” and “average”. What we mean by “population-weighted” is defined in the Methods section of the revised manuscript. We have added a parenthetical statement here however to clarify.

Conclusion

In its current form, the manuscript reads as a collection of loosely connected analyses rather than a coherent scientific contribution. Major restructuring and sharpening of the research question are needed before the paper can make a meaningful impact in the urban heat mitigation literature.

Change made. We thank the reviewer for this important comment. We have substantially restructured the manuscript to improve narrative coherence and sharpen the alignment between research questions, methods, and results. The revised Introduction, Results, and Discussion now follow a clearer logical progression, supported by an added analysis flow diagram that visually summarizes the structure of the study. We hope these revisions make

the overall contribution and scientific narrative more cohesive and impactful within the context of urban heat mitigation research.

Reviewer #4 (Remarks to the Author):

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

We thank Reviewer #4 for their help in the review process, and hope that our responses to the other reviewers' comments above have addressed any concerns you might have had.

Comments from reviewers

Original text from reviewers is in black below, while our response is in green.

REVIEWERS' COMMENTS

Reviewer #1 (Remarks to the Author):

Thank you for your thorough revisions based on the reviews. This is a much stronger manuscript with a clear gap presented and more appropriate use of terminology. All of my concerns have been addressed.

We thank the reviewer again for their comments.

Reviewer #2 (Remarks to the Author):

I sincerely want to thank the authors for address all my comments and the other reviewers comments so thoroughly. I can see the narrative and rationale for the study is much clearer, and the addition of the buffering analysis has further refined the conclusions. Thank you.

I have no more comments to add on aside from the authors in the response state that the title of the paper has been changed to "Trees almost halve urban heat island effect globally but disproportionately benefit higher incomes and suburbs", but the current title is changed to "Trees halve urban heat island effect globally but unequal benefits only modestly mitigate climate-change warming"

I would like a brief clarification on what the current title of the paper will be.

Other than that, I have no more issues that need resolution

We apologize for this inconsistency between the manuscript text and the document describing changes made. The final title of the paper is as listed in the manuscript text: "Trees halve urban heat island effect globally but unequal benefits only modestly mitigate climate-change warming".

We thank the reviewer again for their comments.

Reviewer #3 (Remarks to the Author):

The authors have responded carefully and thoroughly to the reviewers' comments, and the revised manuscript is technically sound and clearly written. The analyses are

comprehensive at the global scale, and the main conclusions are well supported by the data.

We thank the reviewer again for their comments.

Reviewer #4 (Remarks to the Author):

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

We thank the reviewer again for their comments.