Reviewer #1  
  
The paper should be considered for publication after revision, as it attempts to fill an important knowledge gap in the urban climate space. A few recent publications (e.g. Rosenzweig et al, 2018) attempt to address the question of geographic scope and thematic coverage, but this paper provides a more comprehensive and up to date review of the literature.

**General Comments**

One of the primary shortcomings of the paper, is its depth coverage and lack of nuanced analysis of cities from the global south (identified as a gap), but there has been an expansion of the literature in this space in the AR5 and post-AR5 period.

The choice of Web of Science and Scopus as the core source of literature may be a limiting factor as both have limited coverage of the highly interdisciplinary space of urban climate change (line 41). A test of this method may have been its expansion using Google Scholar that has a wider pool of literature in this area. It is also unclear whether the authors have examined the fairly extensive language literature on East Asian and Latin American cities.

The reviewer raises some important issues. However, in the first instance, we must emphasise the scale of the analysis being conducted here: in the initial submission we surveyed the coverage and content of 3,440 case study articles. While this survey is undoubtedly incomplete, it is far more comprehensive in scope, if not in depth, than all efforts we are currently aware of. Hence we argue that limitations in literature scope should be considered in light of this new level of ambition.

Of course, systematic omissions are possible. Indeed, a central thread in our article pertains to literature biases, therefore we must ensure a good balance of coverage. To do so we have taken three further actions: (1) we expanded our search to other literature databases; (2) we conducted our search in French and Spanish; and (3) we conducted new analysis looking at the post-AR5 literature expansion.

**1. Additional databases**

The next-best database available to us is EBSCOhost. Applying our search query here, we gain a further 1584 articles, of which 416 are identified as case studies and are added to the pool.

The reviewer specifically mentions Google Scholar (GS); here we encounter more difficulty. First of all, GS has a much poorer functionality for literature searches compared to Web of Science (WoS) and Scopus. Second, it typically returns a large volume of imprecise results (Haddaway *et al* 2015). And third, unlike other databases, GS actively prohibits users from bulk downloading document meta-data, particularly abstracts. Together this renders GS almost useless for the type of bibliometric analysis we perform in this article – which is based on the automated screening of thousands of abstracts for city names.

Nonetheless, we are interested in whether GS results offer more cases on Southern cities, as suggested. Therefore we manually screened the results from a GS search, importing 100 relevant articles (33 additional, non-relevant articles had to be discarded during this process). Many of these documents are books, book chapters and reports. In these cases we had to search for introductions or summaries that describe the case study locations, importing these to our database instead of abstracts. After this process we identify from the GS set a further 27 case studies, with the regional breakdown shown in table 1.

The main systematic difference we note in the GS sample is the higher proportion of cases on Latin American & Caribbean cities. Hence the assertion that we overlook cases on this region may be correct. However, our expectation is that the total number of additional case study documents to be obtained is low – due to the limited number of actual cases we obtain from the “most relevant” GS documents (7 Latin American cases out of 133 search results). We record these findings as limitations in the methodology section.

|  |  |  |  |
| --- | --- | --- | --- |
| **Region** | **n (GS sample)** | **% in GS sample** | **% in total sample (WoS, Scopus, EBSCO)** |
| AFRICA | 1 | 1.7 | 3.7 |
| ASIA | 21 | 36.2 | 37.9 |
| EUROPE | 16 | 27.6 | 25.5 |
| LATIN AMERICA AND THE CARIBBEAN | 7 | 12.1 | 4.7 |
| NORTH AMERICA | 12 | 20.7 | 23.8 |
| OCEANIA | 1 | 1.7 | 3.7 |

Table 1

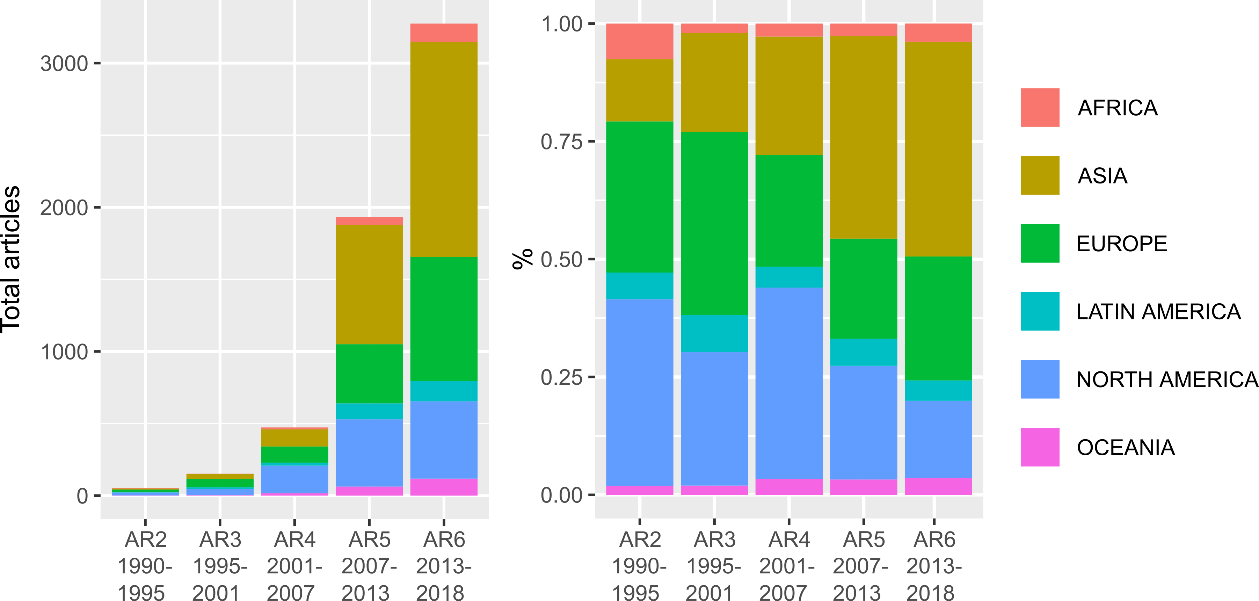
**2. Other languages**

We translated our search query into Spanish and French and applied it in WoS, Scopus, EBSCOhost and Google Scholar. This derives no extra results for WoS, Scopus and EBSCOhost – since these platforms already translate and catalogue all abstracts and titles in English, all relevant foreign language articles are captured by our original query.

In GS we find an additional 18 publications in French and 78 publications in Spanish. Considering this surprisingly small number of documents (of which we would expect approximately two dozen case studies), the aforementioned difficulties with GS, and the fact that we would have to translate the abstracts in these documents, we have decided not to import them into our database. Nonetheless, we note these figures in the methods section and refer to them in our limitations (pp x).

**3. Post-AR5 literature expansion**

Figure 1 below shows the growth and fractions of case studies by region and AR period. This demonstrates that we indeed capture a large (and increasing) share of literature on Asian cities. However, it also shows a more limited expansion of articles focusing on Africa and Latin America. In view of this and the non-English language searches we have performed, we see little evidence for an expansion of articles from the global South (excluding Asia) in the post-AR5 period. We attach this figure to the Supplementary Information (SI Fig 4) and cite it in the main text line XX.



**Figure 2: Total articles and regional proportions of case study literature by IPCC Assessment Period**

Overall, our judgement based on these extensions in search strategy, languages and analysis is that few further cases can be easily identified for cities in the global South. This raises the more practical problem of how to ensure case studies are available for secondary analysis. We now discuss this in a paragraph in the conclusion (line XX).

**4. Additional changes to analysis**

After these additions, and updating for recent publications (during 2018), we now capture 4,444 documents, covering 6,066 cases on individual cities. The relevant text on these figures has been updated. This addition of new documents and databases has two important consequences:

First, because EBSCOhost does not index citations, we can no longer conduct the citation analysis (SI Text Figure 2). Hence we now delete this and references to it in the main text.

Second, we have re-run the topic modelling on the larger set of documents. There are strong consistencies between this analysis and previous, with some differences…

The suggested quantitative typology of cities does not appear to be robust and may be influenced by the bias in the sample used. For example, heating degree days is a significant variable, but because cooling needs are not examined, cities in tropical or composite climates do not apparently appear in the clustering. Fuel price data masks a complex regime of subsidies and carbon taxation in multiple geographies. Similarly, energy poverty is a defining characteristic of Asian and African cities. Have the authors not found this in the literature or is there a methodological gap in the search terms that they are using?

TO DO: Typology… compare a few different options instead of just one?

A useful analysis of co-benefits i.e. synergies and trade-offs between mitigation and adaptation that appears embedded within the clustering of the three most important themes by city in Figure 5 is not drawn upon. The conflation of the quantitative typology in the upper part of Figure 5 with the case study literature below is potentially confusing at worst or dense at best.

Regarding adaptation, we address this specifically in point #12 below.

TO DO: Typology… Explain further in text and response? Or reconsider approach?

Similarly, economic and pricing incentives seems to be an unexplored theme in the paper. Is this because of the underlying literature or a limited search for these terms that are otherwise significant in the mitigation literature?

We can only answer this question with reference to content we find in these case studies. Our topic modelling analysis suggests economic and pricing policies are not widely discussed as headline approaches to urban mitigation. For instance, the set of keywords gathered under the main policy topic (“climate governance”) all refer to generic governance issues, mitigation action plans, as well as multi-level coordination. We invite the reviewer to look at the documents highly correlated with this topic for confirmation: <https://apsis.mcc-berlin.net/tmv_app/topic/127213/>. Instead, keywords such as “income” and “price” are found under the “households” topic, showing the importance of economic incentives for the analysis of household behaviours in the case study literature (<https://apsis.mcc-berlin.net/tmv_app/topic/127227/>).

TO DO: paragraph on this in the main text?

It may be useful to highlight in the abstract the finding that world regions and city scales with most future relevance are systematically unrepresented in the literature. The limited scope of comparative regional/urban case research is an important finding that does not find its way to the conclusions/abstract. That case study analysis is an underdeveloped field that has limited generalisability, is buried dep in the text.

TO DO: REWRITE ABSTRACT

The paper provides a useful analysis of both knowledge gaps and multiple ways forward, crisper articulation in the conclusions may enhance its utility to both scientists and policymakers.

**Detailed/Technical Comments**

1. Line 48: GDP per capita is a measure of income not wealth

We have replaced “wealth” with “income”.

2. Line 48: infrastructure development is different from access, which is identified as an important variable in AR5

We have changed the text to: Cities vary in many dimensions, including size, income and rates of access to different infrastructures (such as housing and energy services).

3. Line 53: there is still an open debate on the definition of urban agglomerations, cities and urban areas. Why confound this by adding metrpoles in the mix? There is no consistency in national definitions of urban areas. It may be useful to reference the urban frame work (UN, 2014) that is used by the paper

We have replaced “metropoles” with “cities”. Please note the suggested citation is already provided in the caption to the figure that is referred to in this paragraph.

4. Line 80: both infrastructure and building transformations are important as identified by the paper. Why privilege only one? Does the literature actually support this statement? The case study shows extensive literature on East Asian cities, which are currently significant contributors to urban and global emissions. There are large middle-income countries in this region.

We have changed the text to: “With the window on the 1.5°C and 2°C goals rapidly closing it is essential to immediately initiate urban infrastructure transformations and comprehensive low-carbon retrofitting ~~in wealthier Northern cities~~, worldwide.”

The point of this paragraph is to emphasise the trade-offs between different geographical foci in urban case studies: cases on wealthier and high-emitting cities are needed, especially considering their current and historical responsibility for carbon emissions; yet, much more work should be done on up-coming cities, where a large potential to ‘lock-in’ emissions exists. This has been clarified with the following changes:

“The current focus of case study research on wealthier and high-emitting cities is congruent with current debates in climate ethics: that responsibility for drastic mitigation action rests on the shoulders of high-emitters. Nonetheless, the majority of future urban emissions growth will originate from up-coming Asian and African cities, where ongoing processes of urbanization and infrastructure development provide a window of opportunity for establishing urban designs consistent with low-carbon mode choices and building use. Guiding these growing cities towards compact, low-carbon urban forms requires a major shift in research focus.”

5. Lines 85 and 86: Energy poverty is a defining characteristic of Asian and African cities. Have the authors not found this in the literature or is there a methodological gap in the search terms that they are using?

Actually we find only 14 documents that refer to “energy poverty” directly in the title or abstract. These are mainly situated in India, Bangladesh and South Africa. We consider this topic out of scope for our review, as it is large enough to require a dedicated study, particularly to capture non-urban (rural) case studies.

6. Line 93: a more careful use of the term agglomeration may be useful

We have changed the text to: “We can safely presume that coverage is even worse for small cities, even though data on the number of ~~small agglomerations~~ these remains incomplete.”

7. Line 98: economic and pricing incentives seems to be an unexplored theme in the paper. Is this because of the underlying literature or a limited search for these terms that are otherwise significant in the mitigation literature?

We address this issue in the general comments above.

8. Line 99: a definition of ‘well-being’ or relevant citation that helps clarify what the authors mean may be useful. This is a very ‘catholic’ term, open to a wide range of interpretation

This is an introductory sentence, with no firm definitions in mind. Our point is to frame this section by suggesting that ‘urban climate mitigation’ research encompasses a wide variety of topics – and methods are needed to uncover such content across large literatures. We have simply removed ‘well-being’ to avoid potential confusion.

9. Line 103: missing cooling needs in tropical regions is a major omission, given that a core theme of the paper is climate change

TO DO: MISSING LITERATURE

10. Line 113: the claim of ‘unsupervised learning’ being less subjective, is not substantiated in the paper. This is a complex and contested territory that may be avoided. For a sense of the complexity involved see the IPCC guidance notes on uncertainty and the considerable literature in this space (e.g. Mastrandrea et al, 2010)

TO DO: Not sure, communicate with authors? I think the reviewer missed the point here.

11. Lines 118 to 124: Many key themes identified in IPCC AR5 are missing in the analysis: energy efficiency, energy storage, smart grids. Unclear whether this is a gap is in the method used or the literature. It would have been useful if piecewise continuity from the systematic reviews in this space (many of them cited) and the thematic analysis were maintained through the paper, else key threads/mitigation options may be missed

TO DO: MISSING LITERATURE; relate topics to AR5 topics

12. Line 125: Climate adaptation is a field of knowledge in itself. A clear analysis of its relationship to mitigation may be useful. Conflation as just another term isn’t very useful.

We agree with the reviewer – this analysis could be repeated for adaptation case studies, in far more detail. We therefore decided to review our search query to understand why adaptation cases were being included and to remove these if possible. We found this was due to the keyword search for “climate policy” and have adjusted the search to remove this combination, while preserving mitigation relevant combinations (e.g. “climate mitigation policy”). As a result “adaptation” no longer appears as a topic in the analysis in the second section, and the manuscript has been adjusted throughout.

13. Lines 128 to 130: unsubstantiated statement. There are many other possible explanations of the importance given to emissions and urban form in East Asia, including government policy. Is there more substantial evidence on this from the regional literature?

TO DO: Felix, do you have any thoughts? The relevant section is:

“Scaling up the analysis from individual documents to groups of documents, we observe that emissions accounting and urban form are frequent subjects of case study research situated in Asia (Figure 3), perhaps reflecting strong investments into engineering disciplines and education in China and South Korea (44% of all students in China graduate in science & engineering, compared with 16% in the US) (Gonzalez-Brambila *et al* 2016). This contrasts with the ubiquity of urban governance research, capturing research on policies and policy-making, in all other regions.”

14. Lines 138 to 140: is this gap in the literature not an artefact of the sources used (Scopus and Web of Science) compared to the relatively rich regional literature in this space on these themes?

We are unable to answer this question, since there is no “comprehensive” body of case study literature to compare with our study. Indeed, we believe our attempt is the most comprehensive so far, particularly of the academic literature. Nevertheless, we condition these statements with the following:

“Individual cities are hotspots for particular mitigation topics. Low-carbon transportation case studies are well developed for Beijing and London, but scarcely researched in New York City, Tianjin, Los Angeles, Tokyo and Chicago (Figure 4).”..

15. Line 164: is the ‘uneven distribution’ of adaptation vs. mitigation literature in Africa and Latin America evidence of a differentiation in the key challenges faced there?

See comment #12 above.

16. Lines 169 to 170: a more nuanced articulation of this finding in relation to the available IPCC AR6 chapter outlines may be useful. Other chapters than the ‘demand chapter’ may benefit from this attention.

We have decided to remove this statement and write a further paragraph in the conclusion highlighting the importance of case study evidence.

17. Lines 188 to 194: more could be done with the chord diagram, in relation of existing analysis from other sources {IPCC AR5, Bartlett and Satterthwaite, 2016, Rosenzweig, 2018 etc.)

The first two of these references are large volumes and the last we are not familiar with (a full reference is not provided by the reviewer). We are therefore not sure what is being requested and cannot respond.

18. Line 263: any useful inferences on behaviour change as a synthetic response that could find a place here?

We do not understand this comment and haven’t taken any actions.

19. Line 271: aren’t households, businesses etc. stakeholders rather than urban systems (energy, water, mobility etc.)

We have changed “systems” to “stakeholders”.

20. Lines 275 to 277: No relevant findings on city-region and system boundary definitions e.g. urban decarbonisation is linked to the decarbonisation of national electricity grids?

In this sentence we state a series of key research questions, rather than demonstrate any findings. Still, we have added the following to the sentence:

“what impacts do urban activities such as decarbonisation generate, within and outside the city?”

21. Line 283: Impacts is a well-defined technical term in the climate literature, largely in the adaptation space. What kind of impacts are we speaking of here?

We have changed “impacts” to “emissions” for further clarity.

22. Lines 298 to 307: significant speculation in this paragraph. This is an extensive grey- and non- Web of Science/Scopus literature in this space, both from national (NDCs) and local governments and global urban networks (C-40, GCOM, ICLEI etc.). Without an analysis of this literature some of these extrapolations into the science-policy interface may be unfounded.

TO DO: think about UCCRN

23. Line 308 and 309: IPCC AR5 (Ch 8, Wk Gp II) suggests an urban typology - a critical comment on that they be useful, if the authors consider it appropriate.

TO DO: typology comparison

24. Line 332: missing cooling days is a serious gap in this paper as noted earlier. Temperature elevation on top of urban heat island effects, is the core challenge in many regions.

TO DO: missing literature section

25. Line 337: a comment on non-C02 drivers from the literature may be useful esp. SLCPs/back carbon, N20 and O3

??

26. Line 359 to 370: It may be unfashionable, but is technical, economic and financial feasibility not emerging as concerns in the literature?

??

27. Line 373: Many of the measures identified in Fig 5 are supply-sided measures. Emphasis on demand-side measures an oversight or a systematic bias in the case study literature?

TO DO: typology

28. Lines 394 to 395: Would a sub-regionalisation of Asia help unbundle this finding? Clubbing oil-producing nations in the Gulf, with South Asia and East Asia may be a simplifying assumption but may mask the generalisability of the facts.

This is a fair point, but we have no space for further elaboration at a sub-regional level. Of course, our method for scoping case study evidence would allow this, even down to the city scale (as shown in Figure 4).

TO DO: make a point about the scalability of analysis in the conclusion

29. Lines 402 to 403: Since both 1.5 to 2 C and adaptation have formed a subsidiary element in the narrative, would it be useful to comment on possible findings on these questions?

We have since removed adaptation from the analysis, please see comment #12 above. We now cite the 1.5°C special report here.

30. Figure 1: not all national and regional capital cities in the world are 1-10 million in size. Unnecessary to conflate facts

We have changed the text to:

Figure 1 shows the spread of case study research across four different city sizes, from a small number of familiar ‘mega-cities’ (over 10m inhabitants), to large cities between 1-10m, and hundreds of medium (0.3-1m) and smaller (<0.3m) cities. The majority of research so far has focused on larger cities, with specific mega-cities receiving particular attention: Beijing (284 articles), New York (146), Shanghai (140) and London (117). Other cities are mentioned in fewer than 100 articles each.

31. Figure 2: the salience of the data may be better represented if the bubble sizes were proportional to the share of global urban population (peach) and case studies (blue). Highlighting the world numbers would help with a visual benchmarking.

We experimented with proportional bubble sizes and total numbers, but felt these additions distracted from the simple statement we wish to communicate: that small cities are under-researched relative to their peers. The share cases studies versus global urban population (in 2030) is provided in the SI text, Fig 3.

32. Fig 3: would be improved if a proper city population size scale were provided or a graphical indication of the proportion of

The size of the dots on each city are not scaled by population size, but number of case studies, as indicated in the caption text. We think that including population size would unnecessarily complicate this figure.

33. Fig 3: It is unclear what regionalisation the authors have used, the typical regionalisation schemed include: UN Population Division (?), IPCC, World Bank etc. Oceania is a rather mixed up category that mixes up Australia, New Zealand a number of SIDS from the Pacific. A footnote or citation may be in order. It may be useful, if the authors consider it appropriate to run an analysis between cities of fossil energy exporting cities vs. others

We use the UN Population Division regionalisation. The caption to Figure 2 now reads: “Population data and the United Nations Population Division regionalisation are from ref (UN DESA 2018), using agglomeration data where available.” We will consider the fossil energy exporting categorisation for a further, more fine-grained publication, due to length constraints in this format.

34. Fig 4: the rationale for ordering the x and y axes is unclear. The visual patterns that emerge from a reclustering by increasing population size for example could be rather different. The mixing up of Adaptation with a range of mitigation options is confusing and possibly not appropriate, unless pulled out as a separate row?

We do not understand – the axes are categorical and no information should be inferred from their ordering. However, we clarify in the caption that the results in each column should be interpreted individually: “The colour scale is normalised by city (column), indicating the main topic focus of case study literature within each city.”

We address the adaptation point in a separate response (#12).

35. In the supplementary data section, the sequence of graphs could be improved by moving: Fig 4 (total population) up front, followed by Fig 2 (growth rates), then Fig 5 (case study coverage), Figure 2 and 3.

We are not sure why this new sequence improves understanding. We have chosen to keep them as before, following the order of referencing in the main text.

TO DO: add sections to the SI?

36. Supplementary data Figure 3: is the global distribution of case studies vs. *population size*

As suggested, this now reads “Figure 2: The global distribution of urban case studies versus population size.”

37. Supplementary data Figure 6: is an important figure in terms of case comparisons but is unclear. For example, the referencing of cities within a region in the chord diagram e.g. North American cities referencing other North American cities requires extreme visual acuity to pull out. A better graphic scheme may be useful.

TO DO: re-design figure??

38. The relationship between Supplementary data Fig 6 and 7 is intriguing but unexplored, analytically and in the text.

TO DO: check on the internationalisation of large-n case studies

39. Aren’t Supplementary data Tables 3 and 4 using fraction and proportion interchangeably. Wouldn’t a simple % calculation be easier to read?

TO DO: ask Max

Reviewer #2

This is an interesting paper and points to some clear gaps in the literature, and ways existing literature could be exploited to gain more information, and makes recommendations which are valid for furthering this field. The suggestions that case studies are done and used by researchers and urban policy makers, is a strong point, but could be made a little more compelling for both communities. Therefore, the main aspect which I would suggest strengthening is a question of how some of the recommendations which are made will contribute to or result in the stated changes at the science policy interface. I refer below to a few specific places where elaboration of this nature would strengthen the paper, as well as s few other small revisions, and would recommend publication with the suggested modifications.

**Primary comment:**   
Line 289- 297. How would review of cases which are confined to a geographically narrow scope, with the aim to initiate policy learning within that scope by drawing on a wider set of study designs and connected topics directly engage municipal stakeholders, integrate prioritizing agendas and enhance the quality of urban science policy. I think missing here is a link to the motivation and mechanisms by which the policy makers in the associated municipalities will receiving the synthesized information from the review, will it be ‘translated’ for best use for policy makers or will reviewers be done and published in ways which serve municipal authorities more directly? What will be motivations for municipalities to engage in the synthesized data. Also, a question of whether the synthesis of case studies being reviewed would be done in consultation with municipalities is important to explore and specify, as information can be lost of skewed in further processing.

TO DO: reframe section… focus more on the use of case studies, science-policy interface

**Line 404-408**  
Why does this method result in more bottom up learning between cities? How does this method (above others) facilitate this and what would direct results/benefits be to both researchers and municipalities who are looking to learn from the reviews supplemented by city topology classification?

Additionally, this question of motivation and how this will be done could be incorporated more explicitly into the opening questions to be addressed in the paper, lines 32-36.   
  
  
**Additional comments:**

Table 1 is hard to read and I would restructure it so that the spacing between the different review titles is more clear. Currently the titles blend together.

Relating to figure 4, I think a stronger introduction should be given to what the figure is trying to convey in the text. I think it is a compelling figure, but it would be useful to state what can be gleaned and the main things within the figure which you would like the readers to be draw to. I think this could easily be done by extending the sentence on line 135, and adding a few additional sentence before going into the example on low-carbon transportation. Without an explanation this figure is a lot to take in.

TO DO: check, re-write text here

Relevant to the explanation on lines 184-187, I think that it would be useful to have figure SI6 in the main text as this builds a bit of a foundation of the recommendations which are subsequently made, and the visual representation in the figure is clearer than the explanation currently given in the text.   
  
Is line 188 again referring to SI figure 6, not 5?   
  
Relating to figure 5, there should be some inherent uncertainty in the size of the circles in the first box, on quantitative topology. To clarify the ‘scope’ of each circle, I think it could be useful to provide bounds for each circle size, (i.e. what is the highest amount of energy use which classifies Ahmedabad as low energy using cities, indicate by the point like circle in the energy use row. If there is not a strict range, then this gives me the indication that these circle represent and average of energy used by cities in a given typology, and therefore uncertainty in these values should be noted or illustrated in some way. Also relating to figure 5, in the text, line 329 ‘Figure 5’ is in bold, this should be corrected to normal text.

TO DO: think about typology

Overall I think that this paper is well organised and provides an interesting perspective, which is not currently represented in the literature.  
  
  
Reviewer #3  
  
This paper opens up an important question but it does not provide a convincing answer. Some of the answers are very true (that the scholarship on cities and climate change focuses on large cities and particularly in the North) but these facts have been known for very long. It would be good to examine if measures have been proposed in, let’s say, the last decade and whether this pattern of research could be changed. Or whether we can identify empirical evidence of the reasons for these biases. Unfortunately, the authors do not seem to find this an important question, and they assume that calling for more research in smaller cities is sufficient to change current science drivers and orientations.

TO DO: discuss why particular cases are emphasised

Some of the ideas of the paper are simply not right. I cannot recognise the characterisation of the urban studies field. The only references to major urban studies journals (such as Urban Studies and the International Journal of Urban Regional Research) are to the two very similar papers by Michael Storper and colleagues which represent a polemic view of the state of the field. These are polemic papers which seek to create debate and yet they are presented here as if they represent the state of the field.

We have diversified the references and respond to the specific literature suggestions below.  
Some of the more interesting conclusions such as the typology of cities are approaches which are used in the private sector such as Atkins Future Proofing Cities which already proposed a very similar typology six years ago (see references below). The two options proposed by the authors about future research (city-based and comparative) are already widely used. Moreover, these are not the only approaches! The diversity of the field is really important to create and develop innovation and sectoral approaches and comparison of the development of specific innovations and technologies across cities are really important to understand the transfer of ideas from location to location. The fact that many comparative studies look at similar policy approaches in different locations is ignored in this paper.

**Specific comments**

The lack of originality of this paper relates with an inaccurate conceptualisation of the field of research on cities and climate change. There is a dearth of references to work in global environmental governance.   
  
For example, I do not see any representation of studies that have looked systematically at climate change action in cities. A well-cited example that comes to mind is, for example, Broto, V.C. and Bulkeley, H., 2013. A survey of urban climate change experiments in 100 cities. Global environmental change, 23(1), pp.92-102. This kind of work is only superficially referred to, but it builds on local reporting of climate action and has had a strong impact on the UNFCCC approach to sub-national climate action (as demonstrated for example in the NAZCA platform for climate action).   
  
There is also a complete oversight over big data inspired research on cities, such as that impulse by the Yale Data-driven Research Initiative (<http://datadriven.yale.edu/> )

TO DO: emphasise complementarity of approach with big data, cite Felix’s new paper

Finally, there have been regionally-specific studies that the authors overlook. Many are focused on Europe or in the US, and yet, they provide an angle which is ignored here. Another well-known example is: Reckien, D., Flacke, J., Dawson, R.J., Heidrich, O., Olazabal, M., Foley, A., Hamann, J.P., Orru, H., Salvia, M., Hurtado, S.D.G. and Geneletti, D., 2014. Climate change response in Europe: what’s the reality? Analysis of adaptation and mitigation plans from 200 urban areas in 11 countries. Climatic change, 122(1-2), pp.331-340.

TO DO: note regional studies

There are numerous sector-based studies some of which have been highlighted by the authors in their systematic review part, but they overlook those which do not do a systematic review, but rather use case studies to develop frameworks. There have been many example in the energy field in journals such as Environmental Innovation and Societal Transitions or Environmental Science and Policy. Overall, there seems to have been a problem with the use of keywords and the range of papers that the authors have unveiled, with less attention to well know studies which have had influence in policy circles. This underscores the limitations of big data analysis, in comparison with experience-based understandings of the field that reach better informed solutions with less data, as already explained by Bent Flyvberg in his analysis of the need for social research that takes seriously the principles of phronesis.

Also this paper ignores policy studies from UN-Habitat, UCCRN, OECD, the EU etc or trade reports such as ATKINS/DFID Future Cities Report, which contained one of the first attempts to compare systematically cities and develop city typologies using big data or classic work from ICLEI.

The difference is that these used qualitative coding of case studies, but the insights maybe the same or in more depth than the ones presented here.

There are inaccuracies and inadequate statements in the text. Some examples:   
“Yet in the assessment of urban climate solutions, case studies are an unexploited resource.”- this is incorrect. In fact case studies are used very often, but in isolation, around ideas of best practice. It is more precise to say that a systematic analysis of 2500+ cases has not been done.

TO DO: agree, we also state this…

“There are strong size, regional and topic biases in case study coverage, and sparse efforts to conduct comparative analysis and systematic reviews.”- this is true, and it also relates to the structures of knowledge production; similar critiques are relevant in the climate change field more broadly.

TO DO: reflect on knowledge production, addressed above in general comments (reviewer #3)

“For instance, urbanists often state that cities share common structural (political, economic, or geographic) characteristics that drive urban phenomena, leading to differing path dependencies in energy consumption.”- which urbanists? Actually most urbanists state exactly the opposite: that cities follow context-specific trajectories and that one city’s experience cannot be reduced to those of other cities. The statement on comparative urban studies made before overlooks the vibrant debate on comparison that is taking place within urban studies. Comparison in urban studies is not about finding commonalities (which is often considered an outdated approach) but about relating particularities with general insights. The classic article from Bent Flyvberg on the use of case studies is a good place to start. Unfortunately this is not appropriate, because it shows an outdated view of the urban studies field and it is this outdated view what is used to mount a criticism of it.

“These consist of a wide spread of quantitative, qualitative and mixed review approaches that are well-documented in the health sciences literature”- but there is no recognition of the multiple studies in this area in environmental governance which is a lot of more relevant. Unfortunately, the authors do not demonstrate they know these.

What are they specifically?

The typology approach is something used, particularly among consultants. See the report of Atkins on Futrue Proofing Cities for very similar typologies of cities: <https://www.atkinsglobal.com/en-gb/group/sectors-and-services/services/future-proofing-cities>

“Implicit in much sustainability research is an asymmetry of knowledge exchange: cases in the global North are relevant for the South, but not vice-versa” – this is simply incorrect. In fact, decades of sustainability research show that cases in the North are often not relevant for the South. Moreover, innovations from the South also spread to the North- and the cases are so many that I do not even need to explain them. You would need to make some sort of analysis to see what is more common but as things stands, this is not an acceptable assumption.

TO DO: check recent Harini article?

My view is that what drives research in the North are funding patterns and researchers’ interests and constraints. As I say above, there is not an analysis of that.

TO DO: again, knowledge production…

If you use UNDESA data you need to use the 2018 revision, rather than the 2014 one.

Done.