Author's response

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Contents

Dear UAR,

Thank you for the opportunity to submit a revised draft. We list below in inline format my brief responses to reviewers' comments and attach at the end tracked changes that show precisely the additions and deletions.

Best,
Authors

1 Response to Editors

As you will see, the reviewers were somewhat divided on the relevance of your manuscript to a journal such as Urban Affairs Review. We believe it has relevance, but perhaps you could emphasize the policy and political implications of your finding to a greater degree. More generally, as Reviewer 3 notes, you need to emphasize the relevance and importance of your findings. Finally, Reviewers 2 and 3, and especially Reviewer 2, brought up measurement issues and ecological inference issues in your methodology that need to be addressed. Overall, we think that the reviews are very good and you should follow virtually all of their points.

2 Response to Reviewer #2

This paper establishes a positive correlation between county-level population growth and reported happiness on the Subjective WellBeing (SWB) survey administered by the CDC. It builds nicely off of Delken (2008) and Glaeser et al (2016) and works as a short research note, but it doesn't belong in UAR – there is nothing related to politics, governance, or public policy in the note

Yes, the note remains mostly technical/empirical. But we have added a bit on policy.

and it's difficult to know how to interpret the results at the county-level due to ecological inference issues.

Using brfss at county level is not new–see eg Glaeser et al. (2016), Okulicz-Kozaryn and Mazelis (2016). We do agree that lower level of aggregation such as census tract would be better, but such data covering and representative of multiple cities does not exist for the US.

An urban econ, geography, or regional science journal would be a much better fit.

Editors disagree.
Here are a few suggestions for the authors as they revise this piece to find a good home for it:

1. I found it very difficult to understand how many of the variables were measured. SWB is an ordinal variable but the authors are treating it as continuous, and I'm not sure how they are achieving the mapping.

we elaborated now; and added logit robustness check in supplementary online material–see subsec 'logit'

I also couldn't figure out how the crime index was measured.

explained now calculations in appendix

2. It was very odd that the crime index had a positive correlation with SBW in all Table 3 regression results. Could the authors comment on this?

crime has expected correlations in correlations table (table 2); it table 3 it serves as a mere control variable, not main variable of interest–we do not focus on its interpretation here; and as shown in correlations table (table 2), crime correlates with other variables–these correlations impact effect of crime on swb
future research could focus more on crime, perhaps differentiate by type, and look at interactions; but this is not the focus in this research note

3. I am concerned that even with sample weights the data are not designed to be representative at the county level. My understanding is that the BRFSS weights are designed to create representative samples at the state-level, not the county-level. Something like MRP seems like a better strategy, or at least a more fleshed out discussion of the sampling and weighting methodology.

yes, regular brfss is representative of states, but there's also smart version; as we say in the papaer: "We use the SMART (Selected Metropolitan/Micropolitan Area Risk Trends) version of BRFSS that is representative of counties."

4. Finally, it's difficult to know how to interpret the results at the county level given that we don't know where the growth is concentrated in the county. E.g. the residents who are reporting higher SWB don't necessarily live in the part of the county which is actually growing. Perhaps the authors could restrict the analysis to counties that only encompass one major city and see if the results are the same.

Again, we do agree that lower level of aggregation such as census tract would be better, but such data covering and representative of multiple cities does not exist for the US.

We dont want to cherry pick resticrtct sample unless absolutely necessary; but as a robssutness check appendix has results in 2 versions, for counties bigger than 100k only, and for counties bigger than 200k only; and results are similar to those in the body of the paper

3 Response to Reviewer #3

The research note explores the relationship between individual feelings of well-being and urban population growth and decline. While this is potentially interesting, the paper needs to explain more clearly why this is an important research question.

we added more motivation and theory at the top of 'theory' section

The author emphasizes that there are not many other studies of the relationship between SWB and population growth and says this gap is "remarkable." However, the article does not really explain the significance of studying the relationship, other than saying that it's been understudied. Why is it important to understand the link between SWB and population growth in urban areas? Why is the strong statistical correlation that the author finds important (especially given that the substantive relationship is small).

we added a paragraph in introduction and in discussion on the significance of studying the relationship

and we elaborated on interpretation in discussion section–why strong correlation albeit small effect size is indeed important

The literature review is appropriately brief for a research note but needs some additional clarification. For example, the author(s) reference research on countries (e.g. Germany, countries in Africa) as well as research on US urban areas. They should more clearly identify the focus of the research that they are summarizing and explain how the cross-country versus within-country focus might matter for findings related to happiness and urban growth.

yes, we added a bit more: calrify we focus on localities/counties within the US–and indeed this is one of the contributions as most SWB research is at person and or country level; and we highlight that within the US population changes are dynamic

In footnote 4, the author(s) reference two articles that "argue that unhappiness predicts population decline or happiness predicts population growth," rather than the other way around. This seems like an important question of causality that the author(s) needs to grapple with in the body of the paper. Rather than stating in a footnote that they "believe" that the causality goes in the other direction, the author(s) should explain why they believe this. Can their analysis address this important question of causality?

done, we took these studies out of the footnote to the body, and we added a brief discussion/our engagement

At times, the paper seems to be starting with the finding of a strong correlation between SWB and then looking to existing literature to see if there's some way to explain the relationship. This feels particularly the case for the theory section of the paper. How does this discussion of theory inform/guide the analysis? At the moment, it feels disconnected from the discussion of methodology and from the interpretation of results.

we added more motivation and theory at the top of 'theory' section

In the analysis, the dependent variable is the subjective wellbeing measure (SWB). The author(s) note that "over 90 percent of respondents were either satisfied or very satisfied with their lives." The author(s) should discuss how/if this skewed distribution matters for their analysis and their findings. If most people are satisfied or very satisfied with their lives in US metropolitan areas, then why would we expect this to be an important cause of population growth or decline?

Right, yes, again we argue that population change affects SWB, rather than the ther way round.
We further have a deeper look in supplementary online material by showing a listing of all counties and proportions of people in each satisfaction category: `https://colab.research.google.com/drive/1fFzDc73LbGAC-G6_I58FV1fH691NAs7_#scrollTo=yVA_81vS9DXC`
Not unusual in swb research that in wealthy societies people either satisfied or very satisfied, still very small ¡=.02 proportion on very dissatisfied is at least somewhat surprising

Hence now added robuistbness check where we combine 2 top and 2 bottom categories into 0/1 variable in 'logit' section in appendix

In the models, the author(s) include a measure of 'population loss' as well as alternative measures of population change. The author(s) explain the rationale for different ways of measuring population change, but say nothing about the measure of population loss. How does this measure compare to the ones for population change? Is it a dichotomous variable? Why does the population loss measure need to be in all the models, in addition to a population change measure? How does its inclusion impact how we should interpret the results?

this is just for robustness, to be conservative, an additional control; now in appendix we also include a model without population loss–results are similar
now we also clarified in the variable description table that this is a 0/1 variable

References

Glaeser, E. L., J. D. Gottlieb, and O. Ziv (2016): "Unhappy Cities," *Journal of Labor Economics*, 34, S129–S182.

Okulicz-Kozaryn, A. and J. M. Mazelis (2016): "Urbanism and Happiness: A Test of Wirth's Theory on Urban Life," *Urban Studies*.

4 Tracked Text Changes

(see next page)