
Your Submission ASAP-D-19-00177R1

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To: "Paez, Antonio" <paezha@mcmaster.ca>

Wed, Aug 5, 2020 at 11:30 AM

Dear Prof. Páez,

We have received the reports from our advisors on your manuscript ASAP-D-19-00177R1 "Do moral communities have a spatial dimension? A spatial exploratory analysis of places of worship and violent crime in the city of Recife, Brazil."

With regret, I must inform you that, based on the advice received, the Editors have decided that your manuscript cannot be accepted for publication in Applied Spatial Analysis and Policy.

We recognise that this is a very disappointing outcome but would like to offer some more detailed commentary below that we hope explains this decision a little more. The issues raised by the reviewers [in both the original submission and in the revised piece] highlight a series of critical and arguably very tricky issues to resolve. The revised paper really does not fully attempt to redress these concerns [in part given they question various underpinning aspects of the piece]. Given this our reading is that the revised piece still falls short of adequately addressing the problems. Issues concerning the extent to which unobserved factors might be driving the results, rather than it being something to do with churches is one particular aspect that leaves us unfortunately to the decision of rejection. More specifically, we see three key concerns that the reviewers also brings to attention.

1. Background literature and motivating the research: The reviewer's comments [Reviewer 1 – first and second round of comments point to this issue] highlight the lack of theoretical underpinning.
2. Analytic approach/inference: We see this aspect as a key issue. More specifically, the lack of consideration given to confounding factors in models means that results are more speculative than empirically rigorous. We acknowledge that to redress this issue would require a completely different analytical (modelling) approach and therefore a new submission.
3. Lack of linkage to theory: Connected to point 1 [above] there is a need to introduce and weave how theory - environmental crime theories][e.g. routine activities/crime pattern theory and rational choice theory] can collectively be drawn upon to explain the results. Furthermore the lack of reference/use of theory environmental criminology, specifically as it related to a type of place/environmental setting [in your case churches and more specifically catholic churches] - Given this omission the missing theoretic grounding and lack of conceptualisation within a specified theoretic framework leaves the paper somewhat ungrounded.

Below, please find the comments for your perusal. You are kindly requested to also check the website for possible reviewer attachment(s).

The editor in charge of this decision has also offered to open a dialogue between yourselves and them if you feel that this will be helpful, who can then more fully unpack/explain the reason for rejection in more detail.

I would like to thank you very much for forwarding your manuscript to us for consideration and wish you every success in finding an alternative place of publication.

With kind regards,
Journals Editorial Office
Springer

Comments for the Author:

Reviewer #1: 1. This paper assumes that churches project a sense of 'moral community' around their locations, but this is

never operationalized or analyzed. I encourage the authors to make sure that any interpretation of the results focuses on the clustering on crime around churches - which is what is analyzed in this paper - rather than interpreting the results in terms of church types having varying levels of 'moral community'.

Example in the Abstract: 'Catholic places of worship do not project moral communities more than ice cream shops'. The more accurate interpretation would be 'Crime appears to be no more or less clustered around catholic churches than around ice cream shops',

2. In general, I am not convinced that data analysis completed in this paper is appropriate for answering the proposed research questions. In this paper, the methods can tell us whether crime is more or less concentrated around churches than ice cream parlors at different distances. But they cannot tell us if churches are positively/negatively associated with crime after adjusting for risk factors, such as income, ethnic heterogeneity, residential mobility, and other land use types. Here I am specifically concerned about the possible influence of confounding factors that have been extensively used in past research and have direct links to some of the theories outlined in the literature review, but are not considered in this study.

This leaves me feeling unconvinced regarding the results presented in this study (i.e., that crime is more concentrated around Evangelical churches). How do we know that Evangelical churches, and not other churches or traffic patterns or income or anything else, drive crime? This has obvious implications for how and where policies are designed and implemented. Moreover, how do we know that the results presented in Fig 7 and Fig 8 are due to some characteristic of Evangelical churches and not simply that Evangelical churches are more frequent than the other types of churches and the other types of land uses, and are therefore more likely to be located near to crime just because they are more located near to everything. Indeed, I think two interpretations are equally supported by the analysis in this paper: (1) Crime is more concentrated Evangelical churches because of something to do with Evangelical churches and (2) Crime is more concentrated Evangelical churches because there are more Evangelical churches.

I see two ways of addressing this issues The first - and the preferred way, in my opinion - is to construct a statistical model, i.e., a spatial regression model for point data, that would control for different risk factors and adjust for spatial autocorrelation. I think that Generalized Additive Models can be applied to point data and splines fit to the regression coefficients to allow for similar (but more robust) inferences to those presented in Fig 7 and 8. The second would be to randomly simulate the same number of control points as the number of Evangelical churches and re-run the analysis on these control points (10 times e.g.). Then, by comparing the 'real' statistics and curves to the simulated statistics and curves, it could be inferred that there is something about the churches - beyond just the total number of them - that is influencing crime patterns.

3. In the literature review section, I encourage the authors to (a) emphasize the core theories related to environmental criminology and (b) discuss how these theories specifically interact with churches. For (a), this is important for situating this research within the broader literature and for highlighting the limitations of past studies with respect to churches. For (b), this is important for motivating why the different types of churches may be hypothesized to influence crime in different ways. As the literature review currently reads, I am left wondering what the mechanisms are that link Evangelical churches, but not other types of churches, to crime.

4. What evidence is there that pharmacies, ice cream parlors, and bakeries are neutral with respect crime? How do we know that the comparison group is not influencing crime in a different (or even similar) way to churches? What does 'having a spatial distribution commensurate with places of worship' mean (i.e., is this a quantitative metric or is this based off of visual interpretation of the maps)? The authors may want to consider referencing the 'routine activity theory' as this provides some background as to how these types of land uses (as well as churches) may or may not be influencing crime. Simulated points (like I suggest above) would be another way of generating 'neutral locations' that would be, at least with respect to the data-generating process, entirely neutral.

5. It would be very useful to have the area sizes for the quadrats included in Table 3. It is challenging to make sense of the intensity numbers because they are all effectively zero, but having the areas may help readers to understand how the different intensities are calculated. Alternatively, maybe the denominator can be scaled so that the intensity measurements are interpretable (e.g., 1.03 per km² rather than 1.03e-06 per m²).

6. Related to the above comment, I am confused about the statement that 'intensity in areas with the highest levels of SED is 216.2% higher than in places with the lowest SED.' Based on Table 3, both of these numbers are effectively equal to zero (0.000004 vs. 0.000009). One number might be larger than the other and this difference may be statistically significant, but I don't understand how this is a meaningful difference in terms of making sense of how crime is spatially distributed.

7. Figures 5 and 6 present the same information as Table 3. Figure 5 is easier to interpret (see my above comment about scaling the data) and should be kept in the manuscript. Table 3 could be moved to an appendix.

8. Section 5.4 interchangeably uses rho (the greek notation) and rho (as text).

9. I would argue that the numbered list documenting the 'contributions to the literature' on p.26 is, in fact, a summary of what the paper did, rather than what the contributions of this study are. The contributions of this study would be the research results, i.e., that crime tended to be more highly clustered around Evangelical churches than other types of churches.

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