**Does ethnic density influence community participation in mass participation physical activity events? The case of parkrun in England**

Response to reviewers. Note that this document is publicly available - so keep responses clean 😊

# Reviewer 1

Comments  
This paper describes an ecological study of the reach of parkrun in terms of ethnic diversity in parkrun. The paper addresses one of the outstanding issues in the evidence base on this physical activity phenomenon and uses a novel (to parkrun research) method to investigate it. The paper is essentially well-written, has interesting and useful findings and has a sound methodological approach. The question of the effect of clustering within the definition of the participation measure is my only substantive concern, and just requires explanation (see below). Otherwise I have a number of minor comments which should be considered before accepting this article for indexing.

*Introduction*  
  
Par 1, line 1: I don’t think “collection” is the right word – it implies that they are in one place when parkrun’s main asset is that it is disseminated. A small wording revision should address this.

**RS: Agreed - use Network!**

Par 4, last sentence: word missing: density would have…

**RS: happy to change this.**

 It might be helpful to have a line about physical activity rates among ethnically diverse populations here – sport is not the only form of physical activity and therefore you need to demonstrate that total activity, which is what matters for health, is also lower than for other groups/communities.

**RS: we state that “non-White-British individuals in England are less likely to be physically active”.**

*Methods*  
   
Under data sources, could the authors please indicate whether “finishers” were unique or just a total count ignoring repeat participation (this is mentioned only in the limitations but should be earlier).

**RS: agreed, this is important for clarity and we have updated this.**

The authors should describe what the potential implications are of this for the analysis and interpretation – they do mention that they do not expect it change the results in the limitations but do not provide the rationale for such a conclusion. It also begs the question why they did not use unique persons because it would be possible to do this with parkrun data. In other words they should explain why they chose to operationalise participation this way.

**RS: agreed, we have updated the publication to state why it is not possible to do this with the data we have, and also that this limitation may causes biases in the results.**  
   
Percentage working age – is there any reason why the authors chose this particular variable for age and how does it relate to the objectives of the analysis?

**RS: This variable is available in the ONS dataset and was used to control for the effect of having care homes in one area. We wanted to try and control for the effect of having a large community of particularly old people (e.g. a care home or nursing facility) in one LSOA.**  
   
LSOAs - Could the authors describe why this particular level of spatial classification was used – parkruns draw on varying areas depending on population density but also the proximity to other parkruns. Could they also state whether there were any cases where two or more SOAs were equidistant and if so how were they allocated.

**RS: The LSOA is commonly used as it is the most detailed level of statistic available from the ONS. There were no cases where a LSOA centroid was equidistant.**

Data analysis – the authors should describe what assumptions for poisson regression were tested (over-dispersion for example). They should also describe how age was operationalised.

**RS: See reviewer 3 which covers this and many other points about the Poisson regression.**

*Results*  
   
The authors talk about ethnic diversity but do not give the reader much idea about what ethnicities this covers in these areas. The authors should describe this somewhere (intro, methods, results) to give the non-UK reader some further context.

**RS: This is clearly defined as the percentage of the population who are non-white-British.**   
   
Par 1: third last line: Remove “the parkrun distance” as it is not relevant as such to the point being made.

**RS: Agreed, made change.**  
   
Par 2: The result for age has been reversed in the results from how it was described in the methods which actually makes it more difficult to understand. Is there any reason why you talk about % non-working age rather than % working age? At the very least it should be consistent between methods and results.

**RS: This is actually a typo in data sources (we have now changed this to match).**  
  
Par 3: you make reference to major areas and urban minor areas but have not defined this anywhere. Either here or in the methods would be suitable.

**RS: We have updated the data sources to include a description of rural-urban classification.**

Par on Model 3: As I read the table, despite the attenuation of the effects for IMD and Ethnic density they remained significant in the model – should be explicitly stated in the text.

**RS: Agreed, updated to read: “**shows that when both independent variables (IMD and ethnic density) are included their coefficients decrease, but remain statistically significant”.  
  
*Limitations*  
   
The authors should also note that this research was conducted in one country and the associations may be different in other countries with different geo-demographic patterns and parkrun density.

**RS: Agreed, this point have now been added, along with a call to replicate in other countries.**

# Reviewer 2

This is a much-needed analysis of potential differences in participation in parkrun. parkrun's popularity raises questions about health inequalities and these sophisticated analyses help us examine the different role that ethnic density and deprivation may play.  
  
I can't comment on the statistical analyses as they are beyond my expertise. The focus of the analyses is the total count of finishers. However, it's not clear to me why a more 'nuanced' approach has not been possible - for instance is it possible to analyse the data in a way that shows the impact of 'parkrun tourism'. Is it possible that this inflates the numbers in some way? By the way, I don't think this will change the conclusions but just pointing it out to encourage authors to give more rationale.

**RS: Thank-you for the nice comments on the paper we appreciate you taking the time to review. To answer your question: The number of finishers is aggregated by geographic area in which the finisher lives, therefore parkrun tourism should not influence the results. For example: if I live in an affluent area and travel to a deprived area to do a parkrun this counts towards runs done in affluent areas, not in deprived areas. So, I think you are correct in saying that it would not change the conclusions. However, what would be interesting in this case is to understand why relatively local parkrun tourism occurs (do people not go to their nearest parkrun because another is more pleasant). We can see an example of this in the attached.**

# Reviewer 3

The results of this interesting study are nicely presented and generally well discussed. There are three aspects of the statistical analysis that may be criticised.  
  
First, the authors have used Poisson regression, pointing out that this is commonly used for count data. However, the validity of a Poisson model relies on the assumption that bedrock variability has been reached and this in turn requires that a complete and correct model incorporating all relevant factors has been employed. Furthermore, the Poisson model is a single parameter model with variance equal to expectation. This means that, unlike the Normal model, there is no further play in the model to allow for hidden covariates. This is usually dealt with in one of two ways by modellers. The first is to incorporate a hidden 'frailty' or 'proneness' parameter. If this is assumed to follow a gamma distribution, then, integrating this out leads to a negative binomial model. This is a two-parameter model that can thus allow variances to be greater than predicted by expectation. The second is to check the residual deviance and compare this to the degrees of freedom. The ratio of one to the other then gives a factor by which variances of estimates should be inflated to allow for lack of fit due to hidden random factors. I found no discussion of this point in the paper so can only assume that simple Poisson regression was used, in which case it is likely that the quoted standard errors are too small (See Senn[1](https://wellcomeopenresearch.org/articles/5-9#rep-ref-38442-1) p13 for a discussion).

**PS: We used a simple Poisson regression model and acknowledge that the standard errors around coefficient point estimates might be underestimated. However, with a sample size of more than 32,000 LSOAs, this problem seems rather theoretical, as the standard errors are very small any way. In model 3, for example, the point estimate for ‘ethnic density’ is –0.052, and the respective standard error is 0.00004. It seems unlikely that using a negative binomial model would have any relevant effect on the parameter uncertainty and/or the interpretation of the results.**

The second point is that population should perhaps have been used as an offset in the model (see McCullagh and Nelder[2](https://wellcomeopenresearch.org/articles/5-9#rep-ref-38442-2) p206). Opinions might differ as to how appropriate this is but I would have expected to see it discussed.

**PS: Thank you for pointing this out – we did indeed use population as an offset variable in the Poisson model, but failed to report this properly in the method section. The respective paragraph has been revised in the following way:**

***We then used Poisson regression models, commonly used when working with count data, to estimate the relationship between ethnic density, deprivation and parkrun participation, controlling for potential confounding variables including: population density, ~~population,~~ age and distance to nearest parkrun event. The LSOA’s total population was used as an offset variable.***

The third point is that in controlling for measures of deprivation the authors are asking the question 'given equal deprivation is ethnicity predictive of participation?'. This, is a partial "effect". It may underestimate the role of ethnicity since part of this may be via a tendency to suffer greater deprivation. I am not suggesting that the authors' chosen analysis is inappropriate in controlling for these factors; I am just suggesting that it merits discussion.

**PS: The reviewer raises an important point: deprivation might be endogenous, i.e. on the LSOA level, there might be a ‘flow of causality’ from ethnic density to the level of deprivation. In this case, the effect of ethnic density on participation would be underestimated, as the effect would be partly (and falsely) attributed to deprivation. While controlling for this effect in the statistical model would be a challenge, we fully agree with the reviewer that this point deserve mentioning, and thank him for his thoughtful suggestion. The following sentence was added to the discussion:**

***Furthermore, we controlled for several variables that we thought would influence participation but it is possible that there are other confounding factors that have not been included. Finally, it can be assumed that there are some causal relationships between the predictors in our model (e.g. between percentage working age or ethnic density and Deprivation). Future studies should consider conducting mediation analysis, to further disentangle their direct and indirect effects.***