Dear Dr Smith,

Thank you for submitting your manuscript to Health and Place.

Referees have now examined it, and their clear opinion is that in its present form, it would not be suitable for publication. The comments below contain suggestions as to how it might be rewritten to render it acceptable. Please resubmit your revised manuscript by Feb 16, 2021.

When revising your manuscript, please consider all issues mentioned in the reviewers' comments carefully: please outline every change made in response to their comments and provide suitable rebuttals for any comments not addressed. Please note that your revised submission may need to be re-reviewed.

To submit your revised manuscript, please log in as an author at <https://www.editorialmanager.com/jhap/>, and navigate to the "Submissions Needing Revision" folder.

Health and Place values your contribution and I look forward to receiving your revised manuscript.

Kind regards,

Jamie Pearce

Editor-in-Chief

Health and Place

## Editor and Reviewer comments:

Editor(s): We apologise for the late decision on this manuscript which was due to a large number of potential reviewers declining our invitation to review the manuscript (and taking some time to communicate their decision to us). We have now received two thorough reviews from experts in the field. The reviewers maintain that the study provides interesting information relevant to the readership of H&P. However, they also have concerns regarding the structure and focus of the manuscript, and the methodology and interpretation of the findings. For example, Reviewer 1 suggests that the Introduction should focus more on health inequities and socioeconomic differences in physical activity participation. They also question the terminology used (access rather than distance). Both reviewers seek a more detailed explanation of the methodology (e.g., unit of measurement of distance; the reason for including non-working age as a covariate in the models). Reviewer 1 is proposing additional analyses that would be useful for the purpose of the study (examination of increase in participation as a function of increases I number of parkruns and decrease in distance to parkruns).

Both reviewers ask for a more detailed discussion of the external validity of the study and study limitations (e.g., selection bias due to study design and including only those who finished a race in the participation rates, inability to examine participation by gender, multiple deprivation measured only at one timepoint). They also suggest that the authors discuss parkrun as only one of the many forms of physical activity that may attract a specific segment of the population rather than considering it as a type of activity with wide impact and appeal. Reviewer 2 suggested that the authors expand their explanation about why participation rates in deprived area were lower despite better access.

We hope that the authors will consider addressing the reviewers’ comments and are looking forward to receiving their revised manuscript in due course.

### Response to Editor

We thank you and the reviewers for taking the time to provide such substantive feedback, and are pleased that the reviewers think that the study is interesting for the readership of H&P. We have substantially revised the manuscript to address all the issues raised by the reviewers and we are confident that this has greatly improved the quality of our study. A detailed response to each of the reviewers’ comments is provided further below.

The key changes we have made are as follows:

1. We comprehensively rewrote the introduction with a clear focus on health inequalities and socioeconomic differences in participation in physical activities, drawing from the wider literature.
2. Adjusted the focus of the paper to be on ‘distance’, in km, rather than ‘access’ and discussed why these two things are not equivalent in the discussion section. We have added the unit of measurement of distance (km) throughout the paper.
3. Rewritten the discussion to include an extended commentary on why more socioeconomically deprived communities have lower participation rates, referencing recent papers in the literature.
4. Added a brief explanation on why we used IMD data from only one year (2015) in the method section and provided further clarification in the response to Reviewer 2: IMD data cannot not be compared across years (as explicitly stated by the Office for National Statistics) and should not be interpolated. It is also unlikely that the results would be any different, because the correlation coefficients between IMD scores across years are, rather depressingly, very high (r >0.96), suggesting that the ranking of areas by deprivation has changed little in the last 10 years.
5. We removed non-working age as a covariate in the models, since, as pointed out by Reviewer #1, it is unintuitive and the results are difficult to interpret.
6. Another paragraph was added to the discussion section, in which we consider the external validity of the study, including the limitation of missing data.
7. Unfortunately, we were not able to provide all additional analyses suggested by Reviewer #1. In one of their comments they state “*The results could be more substantial by including how much the participation increased in proportion to the increases in number of parkruns, as well as the decreased distances*”. This analysis does not seem feasible: simultaneously estimating the effect of the distance to the nearest event and the effect of the number of events in the proximity (potentially using a gravity model?) for all ~33,000 LSOAs and over time would require a completely different, sophisticated, and computationally challenging piece of analysis. Such complex analysis is not within the scope of this paper. However, we may have misinterpreted the suggestion and so further clarification may be helpful.
8. We did, however, conduct the other additional analysis requested by Reviewer #1, stratifying the regression model by urban/rural status. The results were very interesting and showed a lack of variation for both IMD and ethnic density in rural areas, which explained the significantly different effect of distance on parkrun participation between rural and urban areas. The results are provided in full in appendix and referred to in the methods and results section.

We would like to thank you and the reviewers again for your thoughtful comments and we are looking forward to your decision.

Many thanks and best wishes

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## Response to Reviewer 1

Thank you for taking the time to review this study, and for providing constructive feedback. We are pleased that you found the study relevant and important. We hope that our changes to the manuscript have addressed your concerns.

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| Reviewer comment | Author response |
| MAJOR |  |
| I find your use of the concept access problematic, since what you are using is distance and not actual access (that would need to take into account such as road network and topography as well as travel time (and perhaps costs), measured by modules such as e.g. Network Analyst (ArcGIS), QGIS or iGraph). Therefore, as long as you are using geodesic distance I would suggest to refer to it as distance. It is still relevant to include some discussion about what role distance play in access, similar to what you have done. | We used geodesic distance as a proxy for access, but we recognise that it may differ from the actual travel time needed for people to reach the nearest parkrun event. We have changed the text to refer to ‘distance’ rather than ‘access’ throughout the manuscript. We also revised the relevant part in the method section to clarify this issue:  “*For each of the 32,844 LSOAs, we computed the geodesic (i.e. direct linear) distances between its population-weighted centroid and all parkrun events that were in operation on the 15th of the respective month, and then selected the shortest distance. We took this as a proxy for access to parkrun events, whereby a shorter distance to the nearest event was assumed to reflect 'better access'. The limitations of this are outlined in the discussion.”*  Furthermore, we added a paragraph in the discussion section, reflecting on the relevance of ‘distance’ in relation to the perceived accessibility of parkrun events.  *“Overall, our findings also suggest that the location of events may be less important than their social meaning, cultural relevance, and local perceptions of their accessibility– which has been discussed by Macintyre (2007) and supported in single UK localities (Jones et al. 2009). We speculate that the perceived accessibility of parkrun events contributes to the lower participation rates by people living in more deprived areas regardless of geographical distance. parkrun is just one form of outdoor, community-based physical activity that may have different appeal and accessibility to some groups more than others. Further research into parkrun, and with comparison to other outdoor community-based physical activity events is needed to understand the perceptions of accessibility and appeal held by local community members. This will help to understand whether accessibility issues relate to characteristics of the activity itself (e.g., type, time, location, duration) or wider social determinants of health and in turn, how these can be addressed to tackle inequalities and develop inclusive strategies to participation.”*  We revised the paragraph in which we consider the limitations of using geographic distance as a proxy for access.  *“In this study, we used distance to the nearest parkrun event as a proxy for access. However, geographic distance may be a sub-optimal measure of the ability of different groups to attend events. A 5 km distance may be more difficult to transverse in a city than for those with a car in rural areas. A model which uses estimates of travel time using travel distance and predicted transport mode may yield a better proxy for travel access, and adding a consideration of other forms of perceived access (e.g. travel expense, safety) may improve our understanding of the determinants of participation.”*  Finally, we have also changed the title to: *Socioeconomic inequalities in* ***distance*** *to and participation in a community-based running and walking activity: a longitudinal ecological study of parkrun 2010 to 2019* |
| To make the study relevant in a broader perspective, in line with your objective, the text could be altered a bit – as it is now the interest behind the study appears rather to be parkrun as a phenomenon than to understand the underlying mechanisms behind the socioeconomic gradient in participation in different physical activities – for example is the introduction focused on parkrun. I suggest that you in the introduction focuses on previous research of health inequity, socioeconomic difference in participation in physical activity in general as well as effect of similar interventions. To strengthen the relevance of the study further, see the following two comments. | We thank you for the useful suggestion. We have revised the introduction. The first three paragraphs now provide the broader context of health inequity and socioeconomic differences in participation in sporting events in general.  *Please see the first three paragraphs in the introduction sections.* |
| What would further strengthen your credibility, I believe, is to extend a discussion of the external validity, and be more transparent of the limitations of the study design and adjust the claims there after - the most apparent is that it is not the individual but the compositions of the LSOAs that are analyzed, which takes the results further from understanding what mechanisms drives the individual to participate or not, as well as how the background variables among the participants are distributed. | We have expanded the paragraph on this limitation in the limitation sections:  *“It is also important to note that this study is an ecological study at the level of the LSOA, and all findings have been discussed at the level of the community rather than the individual, so as to avoid an ecological inference fallacy. Future research, with access to more detailed data on the characteristics of individual parkrun participants, may be able to better understand individual-level drivers on participation.”* |
| Another aspect is to what extent parkrun to be representable for “free weekly outdoor physical activity events”, and I suggest to include this in the Discussion as well as to be careful not to claim this in the text at large, but rather be transparent that parkrun is a form of outdoor physical activity that might attract some groups more than other. For example, other free weekly outdoor physical activity events such as yoga, dance, skateboard events or other activities might attract other groups, meaning that there are a selection bias in participation in parkrun, if what you are aiming at studying participation in outdoor physical activities at large. | We have added an additional paragraph in the limitations section:  “.*..as a walking and running event, parkrun is not representative of all types of physical activity. It may be the case that different communities, or even sub-groups within communities, would engage differently to other types of events.*” Also included in the discussion is “... “*parkrun is just one form of outdoor, community-based physical activity that may have different appeal and accessibility to some groups more than others [...] This will help to understand whether accessibility issues relate to characteristics of the activity itself (e.g., type, time, location, duration).*”  We have also changed the title, to reflect that parkrun is not necessarily reflective of all community based running and walking activities (others exist):  “*Socioeconomic inequalities in distance to and participation in a community-based running and walking activity: a longitudinal ecological study of* ***parkrun*** *2010 to 2019”* |
| In the introduction, you write that ”As a grass-root, citizen led community organization, parkruns are established by enthusiastic volunteers in their local community” – you could reconnect and develop this in the Discussion and discuss what effect that has on who/what subculture is participating and how that has an impact on who feels comfortable to join. The specific nature of parkrun might, probably, partly explain the socioeconomic gradient in participation. For the results to be more generalizable and, as you write, to fill “an important gap in the literature as an exemplar of how community events to increase physical activity can grow, and adds to our understanding of how that growth occurs in different communities”, parkrun could be characterized further to make it comparable to other free outdoor physical activity events in future studies that can continue the mapping of what activities attract different groups/subcultures and why. | Thanks for this suggestion – this is indeed an important consideration. To address this, we have added a new section in the discussion section:    “*Our findings also suggest that the location of events may be less important than their social meaning, cultural relevance, and local perceptions of their accessibility– which has been discussed by Sally Macintyre (2007) and supported in single UK localities (Jones et al. 2009). We speculate that the perceived accessibility of parkrun events contributes to the lower participation rates by people living in more deprived areas regardless of geographical distance. parkrun is just one form of outdoor, community-based physical activity that may have different appeal and accessibility to some groups more than others. Further research into parkrun, and with comparison to other outdoor community-based physical activity events is needed to understand the perceptions of accessibility and appeal held by local community members. This will help to understand whether accessibility issues relate to characteristics of the activity itself (e.g., type, time, location, duration) or wider social determinants of health and in turn, how these can be addressed to tackle inequalities and develop inclusive strategies to participation.*” |
| There is also the risk of selection bias from each LSOA that is not reflected in the results due to the design of your study (that you don’t study who is participating but rather how the area from which they derive is composed) – and even though you do not claim the results to reflect the SEP etc of each participant – it could be discussed in the paper, i.e. to what extent these results could be reflected in the individual, and the risk of the selection from each LSOA having a socioeconomic gradient as well, meaning that the socioeconomic gradient in participation is even larger than reflected by the results of this study (for example if parkrun also attracts those with highest SEP in the more deprived areas). | It is correct that this paper is an ecological study at the level of the LSOA. We have explicitly stated that we can only make inferences at the level of the LSOA. We added a paragraph in the discussion/limitations section to make this unambiguously clear and point out the risk of the ecological inference fallacy:  *“It is also important to note that this study is an ecological study at the level of the LSOA, and all findings have been discussed at the level of the community rather than the individual, so as to avoid an ecological inference fallacy. Future research, with access to more detailed data on the characteristics of individual parkrun participants, may be able to better understand individual-level drivers on participation.”* |
| Similarly, the design of the study does not enable to see the gender of the participants, and you might lose some explanatory potential - for example the gender roles might vary between both socioeconomic and ethnic groups, and if, say, within certain cultures, women are not expected to run but do other activities, the participation rate might fall almost 50% compared to another group (but this pattern might not, then, be found in other outdoor physical activities, such as yoga etc). This is not reflected in this study, and not even mentioned, even though you write that one of the core aim of Sport England’s funding was to improve participation among women and girls. I therefore suggest to include some reflection about this in the Difscussion and let future studies complement your results in that aspect. | We thank you for your suggestion. We have added a short section on this in the ‘limitations’ section to cover this:  *“[...]. Since we do not have data on parkrun participation by gender it is not possible to know if this is an important factor. Future studies should attempt to obtain data on participation split by gender to determine whether the socioeconomic factors influence male and female participation differently.”* |
| I also find it problematic that you compare rural areas to urban ones, and even though you mention this, I would suggest that you stratified the analyses to come closer to understanding what results reflect differences due to a socioeconomic gradient and the different infrastructures of rural vs urban life. | We have conducted the main analysis again, stratifying by urban/rural classification. The results are summarised in the results section and provided in full in the appendix.  *“When urban and rural areas were assessed separately, we found that for urban areas, parkrun participation is also highest in areas with low deprivation and low ethnic density (bottom left), and lowest in areas with high deprivation and high ethnic density (top right). In rural areas, however, the relationship is less clear and could not be thoroughly investigated, because there are no rural areas with high deprivation and high ethnic density. A similar trend can still be observed (see tables 7 and 8 in the appendix).”* |
| Minor Comments |  |
| No research question? | We have revised the last paragraph in the introduction section, now clearly stating the aims of this paper:  “*This paper aims to investigate the trends in access to, and participation in, parkrun events between 2010 and 2019 in relation to socioeconomic deprivation and ethnic density. We utilise rich datasets from parkrunUK and the Office for National Statistics, including the weekly number of parkrun finishers from each of the 32,844 Lower layer Super Output Areas (LSOAs) in England over the ten year period from 2010 to 2019*” |
| The aim is mention first in the Discussion – better to mention it in the Introduction and Abstract. | The aim of the paper is now clearly stated in the last paragraph of the introduction section – please also see our response to the previous comment. We have also comprehensively revised the Discussion section. |
| For the reader to better understand the demography and how large the differences and variations are between LSOAs, I suggest to include another graph showing ex ethnic density, population density and one or two of the variables included in the IMD (ex employment and education). | We already provide comparably extensive descriptive statistics for the non-time varying variables. Even though we agree that illustrations of distributions, e.g. using histograms, are more informative, it seems unusual to include those in the manuscript. We could create additional plots and place them in the appendix. However, all variables and data used in the paper come from publicly available data sets. For most of them, extensive documentation exists and so we would refer any readers who are interested in, for example, the distribution of IMD in England, to the respective ONS publications (the data sources are references in the manuscript). |
| I suggest to remove Table 6 and just mention, similar to what you do, that due to the high number of LSOAs which had no finishers in the earlier years (2010 to 2013), the standard errors are very large when using a quasi-poisson GLM model, and it is therefore not included. | We ensured that Table 6 is only included in the appendix. We would think that there is no harm in keeping it in, but we leave this to the Editor’s discretion. |
| The three first paragraphs in the Discussion describes the results, I suggest you consider to shorten this, or integrate them into the Result. | We have comprehensively revised the Discussion section, including the first three paragraphs – please the respective part in the Discussion section. |
| In Table 3 the unit (%) is missing. | We have inserted the symbol in Table 3. Thank you for identifying this. |
| The results could be more substantial by including how much the participation increased in proportion to the increases in number of parkruns, as well as the decreased distances. This is ex relevant information for policy makers – ex when decreasing the distance with 50%, there was a 25% increase of participants etc. | It is not entirely clear what kind of additional analysis is being suggested by the reviewer. From our understanding, however, the proposed analysis does not seem feasible: simultaneously estimating the effect of the distance to the nearest event and the effect of the number of events in the proximity (potentially using a gravity model?) for all ~33,000 LSOAs and over time would require a completely different, sophisticated, and computationally challenging analysis method. Such complex analysis is not within the scope of this paper. However, since this was classified as a minor comment, we think that we may well have misinterpreted the reviewer’s suggestion. Further clarification may be helpful. |
| A possible section bias in only including those who finished (if finishing required better physical shape). If possible, I suggest to use number of participants instead or at least do an estimate how many that do participate but not finish. I also suggest to include some information of the distance of the parkruns– for example, are they always a set distance, or what is the average distance? If, say it is 3 km one can assume that the selection bias is low, while if it is 8 or above, only those in good physical shape are able to finish. And if so, connect this to previous studies on a possible socioeconomic gradient in physical shape. | The parkrun distance is always 5km (this is stated in the introduction):  *“parkrun is a charity that organises free, weekly, timed* ***5 kilometre outdoor events*** *in the community for people aged 4 and above to participate as runners, walkers or volunteers.”*  It is common for participants to just walk the 5k distance, and since parkrun events are usually round tracks, it is unlikely that physical fitness is a relevant bias in our study. Nevertheless, we have added text in the limitations section to describe the fact that we include only those who finish the parkrun and scan their barcode as participants:  “... We also include only those who finish the parkrun and scan their barcode as participants. Feedback from parkrunUK suggests that the vast majority (>90\%) of participants do finish the 5k walk/run and scan their barcode. However small biases may exist if those from areas with higher levels of socioeconomic deprivation are less likely to finish or scan their code.” |
| What is the value of including the variable non-working age? If you are to use it I suggest you develop its relevance (ex: since it does not say anything about the age of who is participating – what does it really say and why is it relevant?) as well as clarify if it includes both children (up to 18?) and older people (after 65?). And how are the results of such variable to be understood? For example, if areas with lower SEP have comparably more children, and areas with higher SEP have comparably more pensioners they might appear to have similar age structure when in fact it is very different. If possible to stratify the variable into 1-18 and 65+, respectively, that would probably be better (if you do not have reasons why this way is better?). | This is a very good point and we thank the reviewer for raising this problem. We assumed that people who are over the age of 65 and those who are under 18 participate less in parkrun events. It is true, however, that the association between those subgroups and IMD are inconsistent: areas with a higher % of elderly people tend to be less deprived, while areas with a higher proportion of children tend to be more deprived. This makes it difficult, if not impossible, to interpret the coefficient of the % non-working age variable. We have thus decided to remove the variable from the analysis. Note that this does not affect the results of any other coefficient beyond the third decimal. We take this as evidence that non-working age was no relevant confounder in any of the analyses. |

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## Response to Reviewer 2

Thank you for the review of the paper. We apologise for the ragged organisation. We hope that we have addressed your concerns & comments and that the manuscript has improved as a result of these changes. Our point-by-point responses are in the table below:

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| Reviewer comments | Authors’ response |
| The formatting and organization of the article was a little ragged. For example, why was the first-cited reference in the manuscript #7, and why were the reference numbers out of sequence (e.g. reference 10 appears before reference 5). Table 1 (variables and data sources) appears in the Introduction, and first mention of this table is in the Methods section (2.2). | The bibliography style used is that of the recommended Elsevier latex template in Overleaf. This is why the first cited reference in the manuscript is #7 and why other references are also out of sequence (they have chosen to put the bibliography in alphabetical order). I suspect that the typesetters will be able to correct this prior to publication.  Table 1 will be in the method section, again, this has been moved automatically by the latex template which locates the tables based upon where they are most aesthetically pleasing. This will be changed prior to the publication. |
| The methods text does not tell the reader that geodesic distance was measured in kilometres; and no reference to the distance metric is provided for Table 4 in either title, the table body, or a footnote. | We have added a reference to (geodesic) distance being in km in the methods section, within table 4, as well as table 1, and in the results section. In response to a comment by Reviewer 1, we also refer to ‘distance’ instead of ‘access’ throughout the manuscript. |
| Several words at the beginning of sentences had lower case first-letters which need to be capitalized. | The parkrun brand is always lower case, even at the start of sentences. We find this very frustrating too. We believe that ‘parkrun’ is the only word not capitalised at the beginning of sentences. |
| The first sentence of the results section in the abstract reports' trends to 2018: was that supposed to be 2019? | The use of 2018 in the abstract was a typo, we have changed this.Thank you for pointing this out to us. |
| Multiple deprivation was only measured at one timepoint (2015). What implications - substantive or methodological - might flow from this for examining socioeconomic trends between 2010 and 2019? Is it problematic having the main exposure variable measured 5 years after some of the outcome data were collected: this would seem to contravene any causal inference logic. | It is correct that we only used IMD data from 2015. This is because the ONS states that IMD scores from different years cannot be compared (because of changes to the basket of indicators and weights) and IMD scores should not be interpolated.  (see e.g.: <https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/833947/IoD2019_Research_Report.pdf>)  Even if comparable IMD data were available for each year, it is unlikely to change the results because IMD scores are so highly correlated between years. The plot below shows the Pearson correlation coefficients for IMD scores from 2010, 2015, and 2019. They are, rather depressingly, very highly correlated (>=.96 Pearson correlation coefficients), suggesting that the ranking of areas by deprivation has changed little in the last 10 years. It is unclear how much of these differences are due to changes in ONS methodology.  To ensure maximum transparency the methods section for IMD now reads:  *“The socioeconomic deprivation of LSOAs was measured using the 2015 Index of Multiple Deprivation (IMD), a measure of relative deprivation. The IMD combines 37 indicators from seven domains (income, employment, education and skills, health and disability, crime, housing and services, and living environment) into a single score. The score ranges from 0 (least deprived) to 100 (most deprived). We only used IMD data from 2015, as IMD data is not comparable across different years and the ONS does not recommend constructing time-series by interpolating IMD estimates.* ” |
| Why was RII for distance estimated as less deprived areas being further from an event? It seems intuitive (and in keeping with the health inequalities literature) to have expected more deprived areas to be further from an event: and to have examined it this way would have been consistent with how the authors estimated the RII for participation (i.e. less deprived areas having higher participation, which is what we would expect). | The calculation of the RIIs is fully consistent in the sense that in both cases, the predicted outcome for the least deprived is divided by the predicted outcome for the most deprived IMD group. We state this in the method section and provide a guide for interpreting the results (see below). We think that adopting the suggested change in the RII for just one figure would be inconsistent – we would thus worry that it might cause more confusion than clarity.  *“[...] The RII for geodesic distance was computed as the ratio of the predicted distance to the nearest parkrun event from the least compared to the most deprived LSOA, using a linear regression model with IMD as the only predictor. The latter was calculated as the ratio of the predicted number of finishers from the least compared to the most deprived LSOA [...]. For geodesic distance, a RII > 1 indicates that less deprived areas are further from their nearest parkrun, while for participation an RII > 1 indicates that less deprived areas have higher parkrun participation rates.”* |
| I agree with the authors that there might not have been a clearly discernible socioeconomic gradient in distance between 2010 and 2013, but throughout most of this period (and between 2010 and 2019) LSOA in the most deprived quintile had the 'better' access (i.e. shortest distance to the event). Absolute differences between deprivation quintiles are as important as relative differences and focusing on narrowing gradients (inequality) can sometimes distract from the equally important task of improving the objective situation of the most disadvantaged (poverty). | We agree – thank you for this comment. We have adjusted the text to read:  “*Figure 1 shows the mean geodesic distance to the nearest parkrun event (access) for each of the IMD quintiles (and overall in black) over time. A table containing the numeric values can be found in Table 4 in the appendix. The mean distance decreased super-linearly in the first four years (from 34 km in 2010 to 10 km in 2013), and took another six years to reduce to less than 5 km. This effect is notable in all IMD quintiles.*” |
| I acknowledge that the authors touch on the subject, but can they provide some further cautious speculation about why participation rates in deprived areas were lower despite better access? Maybe insights can be cleaned from the broader health inequalities literature: there is now a well-established evidence base about why people from disadvantaged areas or backgrounds are less likely to participate in a wide range of readily available activities, services, or facilities (e.g. transport, costs of equipment, stigma, cultural barriers, awareness of and responsiveness to promotion messages). "Build it and they will come" is only true if contexts and circumstances are conducive to engagement! | We have added an additional section in the discussion section to provide greater context and discuss the relevance of distance to the nearest parkrun event in relation to the perceived accessibility:  “*Overall, our findings also suggest that the location of events may be less important than their social meaning, cultural relevance, and local perceptions of their accessibility– which has been discussed by Sally Macintyre (2007) and supported in single UK localities (Jones et al. 2009). We speculate that the perceived accessibility of parkrun events contributes to the lower participation rates by people living in more deprived areas regardless of geographical distance. parkrun is just one form of outdoor, community-based physical activity that may have different appeal and accessibility to some groups more than others. Further research into parkrun, and with comparison to other outdoor community-based physical activity events is needed to understand the perceptions of accessibility and appeal held by local community members. This will help to understand whether accessibility issues relate to characteristics of the activity itself (e.g., type, time, location, duration) or wider social determinants of health and in turn, how these can be addressed to tackle inequalities and develop inclusive strategies to participation*”. |