Ms. No.: PM-16-587

Title: Recreational physical activity in natural environments and implications for health: A population based cross-sectional study in England Corresponding Author: Dr. Mathew P. White

Authors: Lewis R Elliott, PhD; Timothy J Taylor, PhD; Benedict W Wheeler, PhD; Anne E Spencer, PhD; Angie Bone, MD; Michael H Depledge, PhD; Lora E Fleming, PhD

Dear Dr. White,

Thank you for your submission. The initial review of your aforementioned manuscript is complete. We are unable to accept it, in its present form, for publication in Preventive Medicine. However, we are willing to consider a revised version if you can address the concerns and suggestions from the Editorial Office and those in the reviewers' critiques. These comments are appended at the end of this message.

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We look forward to receiving your revised manuscript.

With kind regards,

Eduardo L. Franco, DrPH, FRSC, FCAHS

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REVIEWERS' COMMENTS FOLLOW:

To the reviewers – please note that the original submission was 3,499 words and the editor has stated (see below) that we still only have a maximum of 3,500 words in which to include all of your requested changes. Since the manuscript was already very short most of your requests have had to be added to the Appendices in order to keep within this very tight word limit.

Reviewer #1: The authors present an interesting article on national MENE survey data on physical activity in natural environments with estimates of the associated economic benefit using a method established by Beale et al. in 2012. This provides a useful approach to help make the case for natural environments as a physical activity resource in economic terms, which is increasingly powerful given the ever-growing financial pressures on local authorities/those responsible for the provision and maintenance of natural environments.

We thank the reviewer for these comments and for highlighting the paper’s contribution to the field. We also thank the reviewer for the extensive comments below and very clear referencing to lines in the text which greatly helped us address their points.

My only substantive comment on the paper relates to classification of moderate intensity physical activity (MPA). The authors used the Compendium of physical activities, which resulted in the following being classified as MPA: walking with/without a dog; visiting an attraction; fishing; off road driving/motorcycling. All of these have the potential to be low intensity. I would argue that much of the walking undertaken in natural environment is below moderate intensity and that visiting an attraction and fishing are unlikely to be MPA. As such, the health benefits of such activities will be overestimated by this classification, leading to overestimates of economic benefit. I understand that a classification must be used, but I do not see any recognition of this issue. At the very least this should be a limitation. Ideally, sensitivity analysis could be undertaken with these classified as LPA, to provide a more conservative estimate. On a similar note, the limitation of self-reported physical activity is noted, but the likely overestimation as a result of social desirability bias could further increase the estimated benefits.

The reviewer raises a number of good points and we also understand his/her apparent discomfort with including activities such as walking the dog and fishing as reflecting potentially moderately-intense activity. We were also somewhat uncomfortable with including off road driving and discussed among ourselves not including it in the analysis as it did not seem to be an outdoor activity. However, as the reviewer also acknowledges the MET rates and cut offs we used are those set out by Ainsworth and colleagues (2011), i.e.: Sedentary = 1.0-1.5 METs; Light = 1.6-2.9 METs; Moderate = 3.0-5.9 METs; Vigorous = 6.0+ METs. As these cut offs are widely accepted and used around the world we were reluctant to suggest and test our own alternatives or ultimately make our own decisions about which activities should “count” as physical activity in nature, which is why we decided to leave ‘off-road driving’ in our analyses.

Although the reviewer does not say what cut-off they would like, s/he seems to imply a cut-off of >3.5 METS. Nevertheless the WHO’s global strategy on diet, physical activity and health also supports the definitions of moderate and vigorous physical activity used in the current article (<http://www.who.int/dietphysicalactivity/physical_activity_intensity/en/>), and it this perspective that underpins the WHO HEAT tool which we used to try and check the robustness of our approach. If we were to set a higher MET rate as counting as “moderate” we would have to abandon the HEAT tool approach, and by implication it may look as if we are suggesting that regular walking has no meaningful benefits for health, contrary to evidence (e.g. Hamer M, Chida Y. Walking and primary prevention: a meta-analysis of prospective cohort studies. *Br J Sports Med.* 2008; 42:238-43). Moreover, since the economic estimates based on the QALY approach – assuming walking counted as MPVA – were highly similar to the WHO HEAT tool estimates, we were reassured that we were not over-estimating the benefits using the QALY approach. Nevertheless we agree with the reviewer that we are assuming that the walking and fishing etc. undertaken by our sample do adhere to the MET rates proposed by Ainsworth et al. and we agree with the reviewer that this may not always have been the case. Consequently we have added additional text (see below) to Appendix A7 (due to insufficient space in the main body).

We are also less worried about social desirability effects in the current work than we might otherwise have been, because the questions pertaining to visiting nature did not mention physical activity or health at all, they merely asked for a description of the visit, its length and activities undertaken, which we only subsequently attributed METS to, and the question on physical activity frequency was embedded in a broad range of demographics rather than many questions on health. In support of this we can compare the current data with that from the Health Survey for England. For instance, the HSE found that in 2012, 43% of men and 32% of women self-reported meeting the guidelines as operationalised using the 5 x 30 minutes a week approach: <http://www.hscic.gov.uk/catalogue/PUB16988/obes-phys-acti-diet-eng-2015.pdf>. By contrast, only 17.8% of our total sample reported meeting guidelines which is much closer to the levels established using accelerometers in the 2008 HSE sub-sample. Thus although there may still be some over-estimation, we suspect it was far less in this sample than the nationally recognised HSE instrument precisely because the focus of the current survey was not on health behaviours. Again, although we think this discussion might be an interesting addition to the current paper, word restriction limit our ability to include it in the main body, so we have also added it Appendix A7.

Thus A7 now includes the following:

*“That the data were self-reported raises a number of issues because we assumed that respondents were: a) accurately reporting the duration of self-reported activities; b) engaging in the level of intensity associated with these activities, as set out by Ainsworth et al.,[21] for the entire duration; and c) accurately reporting the frequency of physical exercise over 30 minutes a week. We recognise that if any of these assumptions weren’t met the current approach may result in an over- (or under-) estimation of the benefits. In an attempt to mitigate the first two issues, all self-reported visit duration was capped at just 30 minutes, despite many visits being significantly longer (i.e. Mean visit duration = 54 minutes; Median duration = 40 minutes). We believe this reduces both the possibility of over-estimation of visit duration and intensity duration, because the average visitor (40 minutes) could be effectively stationary for 25% of the time (10 minutes) and still meet the 30 minute threshold for activity.*

*We are less worried about social desirability effects in the current work than we might otherwise have been, because the questions pertaining to visiting nature did not mention physical activity or health at all, they merely asked for a description of the visit, its length and activities undertaken, which we only subsequently attributed METS to, and the question on physical activity frequency was embedded in a broad range of demographics rather than many questions on health. In support of our suggestion, we compared the current data with that from the Health Survey for England. For instance, the HSE found that in 2012, 43% of men and 32% of women self-reported meeting the guidelines as operationalised using the 5 x 30 minutes a week approach:* [*http://www.hscic.gov.uk/catalogue/PUB16988/obes-phys-acti-diet-eng-2015.pdf*](http://www.hscic.gov.uk/catalogue/PUB16988/obes-phys-acti-diet-eng-2015.pdf)*. By contrast, only 17.8% of our total sample reported meeting guidelines which is much closer to the levels established using accelerometers in the 2008 HSE sub-sample. Thus although there will inevitably some inaccuracy in our estimates based on self-report data, we suspect it was far less in this sample than the nationally recognised HSE instrument precisely because the focus of the current survey was not on health behaviours. Clearly, however, further research using more objective measures of naturalistic physical activity in different natural environments is needed to help assess the robustness of our assumptions and to provide even more accurate assessments in future work.”*

We hope that the reviewer will be satisfied with this discussion of the issues and that we have had to place it in the appendices section given the extremely tight word restrictions under which we are operating.

Minor points:

1. The authors should recognise that there can be no assertions that the natural environment was the trigger for physical activity behaviour. They refer to limitations around substitutability, but I would recommend that this is expanded to highlight that we still do not know if the natural environments promote physical activity, or if active people chose to undertake some of their activity in natural environments. The latter is still valuable given the evidence that there are additional benefits of 'green exercise' (e.g., Thompson-Coon et al. 2011), but the physical activity promotion question remains unanswered.

The manuscript makes no contentions that the natural environment elicits physically active behaviour. The reviewer argues that active people ‘may choose to undertake some of their activity in natural environments’ and we would agree. Table 4, for instance, makes this relationship absolutely explicit; many people who meet physical activity guidelines only make 1 or 2 visits to nature per week suggesting that most of their exercise occurs elsewhere. Moreover, we also present extensive data on visits to nature that did not involve physical activity of a moderate intensity – see Table 1 and Table 2, reflecting our argument that although some nature visits involve physical activity, many do not. All we are saying is that some people may find exercising in nature less threatening and more motivating than exercising in urban or indoor settings. We also agree with the reviewer regarding the potential mental health benefits of exercise in green space over and above those from exercising in urban settings (i.e. Thompson-Conn et al. 2011) and have noted this in line 347 (ref 34) but since we were unable to assess these potential additive effects we did not comment on them further.

1. Abstract: Make it clear where you report 95% Cis

In order to adhere to the strict word limit of 250 words we have only included an explicit mention of 95% CIs in the first set of relevant brackets (i.e. following the estimate of 8.23 million visits) and hope that is sufficient to alert the reader of the meaning in the other brackets following mean estimates. To allow space for this we have trimmed the concluding sentence.

3. Abstract: add a few words to explain the use of the HEAT walking tool

Again we agree that further details would be nice but to include them would take us well over the 250 word limit and we cannot see how to subtract any further words elsewhere without obscuring meaning for some other part of the abstract.

4. Intro: Line 71 - where the authors refer to 'although for Scotland see [14]…' add a sentence to summarise the findings from this work.

In retrospect we have decided to remove this reference from this point because, although the data used was from a nationally representative sample of Scotland, it did not attempt to calculate the total amount of activity in natural environments, used a much smaller sample than the present research (n = 1,890) and was primarily focused on mental health outcomes.

5. Intro: Line 78 - add a few words to explain a little more regarding use of survey weights to allow population estimates of visit type and frequency.

We appreciate the reviewer’s request though word limits in the main body do not allow for this. Consequently we have now provided a summary of the weighting in the Appendix material (A1). We hope that the reviewer finds this helpful although we also note that the weighting is extremely complex and for a full description the reader will need to consult the two technical documents referenced.

6. Intro: Line 83 - delete 'was estimated…'

We have deleted this and changed it to ‘made’.

7. Methods: Line 93, delete 'across the country' (you have already said nationally representative).

We have deleted this and the associated sub-clause concerning reduction of geographical biases.

8. 'out of doors' is very odd wording to me. I assume this is the wording used in MENE. If not, suggest changing.

The reviewer is correct, this is the language used in the original survey so we would like to keep it in. It was for this reason we used inverted commas to denote it was taken from the survey.

9. Methods: Line 108-113 - be clear in terms of the advantages that using these weights confers

We appreciate that we did not make this clear and have now added the following sentence: “*The use of these weights was necessary to make extrapolations from the current sample of individuals and visits, to the entire adult population, per year*.” (lines 110-111).

10. Methods: Line 143 - delete 'd)' (as there is no a-c)

We thank the reviewer for noticing this typo.

11. Methods: Line 149 - how did you define / measures urbanity if residence?

As we are unable to give further details in the text, due to word limits, we give a brief summary in Appendix A6. We hope this is sufficient.

12. Methods: The authors state that ethics was not required. I find this surprising as I have always had to seek permissions for secondary data analysis, even if through proportionate ethical reviews. This needs to be well justified.

We recognise the reviewer’s point but maybe there are different requirements in different academic settings. The data is freely available and open access on the internet, and all participants are anonymised and exact home geolocations removed. In our own institution, there is no requirement to get ethical clearance for such research. Nevertheless, we have altered our sentence on this issue to highlight the lack of need at our particular institution, rather than the currently broader statement which may not apply in all contexts, including that of the reviewer: line 114-117 *“Our institutional ethics board did not require a formal ethics application for the current analysis of this secondary, anonymised data.”*

13. Methods: the authors refer to those who are active <5 days/week as inactive. I would suggest that insufficiently active or active below recommended levels would be a more accurate reflection.

We agree with the reviewer that this would be a more accurate reflection and have made the suggested changes throughout the manuscript.

14. Results: Line 242- the authors wrote 'Of those who made active visits, 96% visited 'regularly', so for simplicity, we assumed all active visitors could be classified as 'regular'. I am not sure I follow this logic. 4% of the 8.23 billion active visitors is nearly 330,000 people. Surely this is a large enough figure to take in to account? Otherwise, a stronger justification is required.

In hindsight we recognise that this sentence is neither compelling nor necessary so we have deleted it. In the section that follows we look at specific frequency of visits in relation to QALYs anyway.

15. Citations: check formatting as there is inconsistency in placement of citations before/after punctuation.

We thank the reviewer for noticing this. It seems that we had inadvertently placed the citation after full stops but before commas. This has now been standardised to following both full stops and commas.

16. Be consistent in use of " or ' when reporting survey questions/response categories.

Thanks again to the reviewer for noticing this. We have now standardised this throughout using single inverted commas.

Reviewer #2: This paper is very well written and makes a contribution to the growing evidence base in this research area.

Many thanks to the reviewer for their overall positive appraisal and as with reviewer 1 we would like to thank the reviewer for raising the points below.

This study describes the number of self-reported active visits to natural environments in England. It involves a very large sample size who completed a survey comprising details of type of activity, duration of visit and environment type. The study focuses on those meeting recommended physical activity guidelines either fully or partly in natural environments. The annual value of these active visits was calculated as £2.18 billion, implying public health benefits.

We already know that there are considerable public health gains from engaging in moderate levels of physical activity. Other databases and surveys also report frequency of visits and types of activities conducted in natural environments. However, the novelty of this article is that the authors calculate the total amount of physical activity taking place in these environments and the associated monetary social value. This is an interesting and important approach with a dataset of this size. Although the paper makes an interesting contribution I think the authors need to address the following comments:

Methods

Study design and sample subsection

I appreciate that the details of the two weighting variables are published elsewhere, but it would be useful for the reader to know what 'weekweight' and 'weekVweight' stand for.

This request echoes that of Reviewer 1 point 5. We hope that our reply above and the addition in the Appendix is sufficient to give the reader a better overview of these weights.

Data and variables

Are you not interested in the type and intensity of the main activity too?

We agree that we would be interested if we had that data. Unfortunately the survey does not identify a “main” activity so we have no way of knowing which activity was the “main” one where more than one activity was listed. We therefore thought it most prudent to simply disregard those visits where we did not know what the main activity was. We realise that this is likely to lead to an under-estimation of the overall benefits but thought this more conservative approach would be preferable.

Does the dichotomising of the visit duration also relate to the recommended 30 mins of activity bouts?

This is true, therefore we have added the following to this sentence: “To avoid suggesting over precise duration estimates, duration was dichotomised as being either <30 or ≥30 minutes, *a meaningful threshold in terms of meeting recommended physical activity guidelines*.”

Results

I suggest you improve the formatting of Table 1 so that is fits on one page.

We understand why the reviewer is saying this but the table formatting is currently in line with the guidelines for authors (e.g. double spacing etc.) and presumably will be appropriately formatted to fit the journal’s page layout in the copy editing phase by the publishing team.

The total number of active visits in Table 1 is 8,233,668, but the active minutes in bold equate to 8,234,500, representing a difference of 832? The annual visit calculations appear to add up correctly.

I would suggest you double check the total calculations too.

We very much appreciate the careful scrutiny the reviewer gave the data, and thanks to their vigilance we did find a number of small calculation errors which we have now rectified. The first occurred when pooling the data for the population estimates. This error was in the ‘>30 minutes, low intensity activities’ – which should have read 937,000, rather than 936,667. The second and third errors occurred during the addition stages as noticed by the reviewer. Firstly, as they correctly identified the Active total should have been 8,234,500, rather than 8,233,668. Second (and partly as a consequence of these earlier errors) the overall total should be 42,169,168 instead of 42,167,004. We have also altered the associated standard errors. Of note, however, the associated %s do not change to the nearest decimal point so these remain unaltered in the table.

Further as the reviewer suggested we also checked all other figures in Table 1 and found that the N for visits for multiple activities was also wrong and should have been 345,169,500, and that the final total should have been 2,829,500,000 rather than 2,829,499,834. Both of these changes have now been made and we again thank the reviewer for spotting these mistakes. Concerned about these errors we have also checked Tables 2 and 3 but the data in these all appear correct.

Of note, none of these changes affects our headline statistics presented in the abstract or discussion because the changes in our key variables (e.g. number of adults making active visits) are only presented to two-decimal points in terms of billions elsewhere in the paper anyway.

Supplementary Table A - Please define what the \* means in the final column as a footnote

Apologies that this footnote was accidentally omitted, this has now been re-added. See also comment directly below.

According to Supplementary Table A 76.8% of people not owning a dog accounted for 66.8% of the active visits, whereas 23.2% of population owning a dog accounted for only 33.3% of the active visits. Yet, in Table 2 the most popular activity was walking with a dog (67.8%, <30 mins; 44.1%, > 30 mins)?I think this needs to be addressed in your discussion.

We believe the reviewer has misunderstood what the data in Supplementary Table A shows and we acknowledge this may have been due to ambiguous column labelling on our part, and apologies for this confusion. Although the column says “active visits” what this actually means is the number of individuals who made active visits – the information the missing footnote should have contained. As well as adding the footnote we have therefore also re-titled this (and the previous) column to make this clearer, i.e. changing ‘Active visits’ to ‘Individuals who make an active visit’. We hope that clarifying this helps the reviewer better interpret the apparently conflicting findings – which are explained simply by the fact that although dog owners represent less than a quarter of the population they account for a third of all active visits, showing, on average, they make more active visits that non-dog owners. We hope that we do not now need to discuss this issue in particular in the discussion now that we have better labelled Supp. Table A.

Table 2 - Does road running really happen in a natural environment?

As a keen runner, the first author regularly runs along small country lanes, especially in winter when fields and paths are often waterlogged, and would characterise this running as taking place in a natural rather than an urban environment. Of more direct relevance, this is how the respondents to the survey answered this question and we are reluctant to challenge this. It is also worth noting that the development of this, and all other, survey items were undertaken in consultation with the public and end users of the dataset, so the reviewer can be confident that this activity is both understandable as a recreational activity conducted in a natural environment and of interest to user groups involved with outdoor recreation in nature.

The authors state they are incorporating a wider range of activities in their analysis compared to the natural England WHI scheme economic and health assessment. However, 88.9% of respondents report walking as their main activity (< 30 mins) and 70% (> 30 mins).

We recognise the validity of what the reviewer is saying but this does not undermine our contention, there are still 30% of active visit types that the WHI analysis did not consider, moreover that analysis only focused on individuals in that scheme rather than all individuals across the entire country and all 15 types of environmental setting. We make it clear throughout the paper that walking is by far the most frequent activity and focus on walking in the HEAT tool analysis. Another advantage of our analysis was that it used a much larger and more representative sample. Consequently we have altered the sentence as follows: “*Building on an estimation of the benefits to health associated with a scheme to promote walking in natural environments,[24] the current study estimated the potential value to health associated with a wider range of physical activities undertaken during recreational visits to natural environments across England, and using a much larger and more representative sample, using Quality Adjusted Life Years (QALYs)*.” We hope the reviewer feels this more accurately reflects the advantages of the current work.

Table 2 is very lengthy and seems to over complicate the key message. If the authors are primarily interested in active visits, should it not just focus on >30 mins moderate and vigorous activity? The title refers to 'active visits', but the table also includes inactive/sedentary activities.

We are very sympathetic to this point and prior to submission we had exactly the same discussion among the co-authors. Ultimately we felt that it was important to show readers the full data so they could also see which activities were undertaken for less than 30 as well as more than 30 minutes, in order to set the full context. We recognise however that the title was therefore misleading so we have changed it by simply deleting the word ‘active’ thus reflecting all visits. If the editor agrees with the reviewer that this data in not central to the main thrust of the paper we are happy to edit the table as suggested by the reviewer, or perhaps put the full table in the supplementary materials and only present the ‘active visits’ data here.

Table 4: Under the 'self-reported exercise a week' column I would define the categories as 'meeting guidelines' and 'not meeting guidelines', rather than >5 x 30 mins and <5 x 30 mins for simplicity.

Again we sympathise with the reviewer but we are reluctant to do this as the second group do appear to meet the guidelines through their reported frequency of ‘active visits’ to nature, but are not recognising this when asked to state how many days a week they undertake 30 minutes or more physical activity in general, thus it is not true that they “do not meet guidelines”. Although it may be harder to read as it is, we feel this is a more accurate portrayal of the data that is ultimately less ambiguous. Again, though, if the editor agrees with the reviewer we will of course make this change, though we note neither of the other reviewers raises this as a problem.

The total number of individuals should be 3,201,332 not 3,201,322

We thank the reviewer for spotting this typo.

Table 4 - I'm assuming some of the calculated totals are affected by rounding off, as they don't always add up as reported

Again we thank the reviewer for their careful scrutiny of the data. With respect to the number of QALYs per year they are absolutely correct and this difference of 3 QALYs was due to rounding. We have now acknowledged this below the table. With respect to the annual welfare gain we noticed a slightly larger error not merely attributable to rounding and have now corrected this error which should have read 2,183,210,983, rather than 2,183,282,201. Again the slight difference, of £1, from the simple sum in the columns is due to rounding. This correction does not alter the headline messages elsewhere in the paper. Concerned that we may have made a similar mistake we also checked the data for walking (Supplementary Table B) but found no comparable errors. Again though there were slight differences in the column totals for QALYs (3) and values (£1) reflecting rounding and this has also been acknowledged under that Table.

The article is predominantly descriptive and I wonder if there is scope for the authors to conduct some multi-level modelling to analyse what predicts individuals being active in a natural environment.

We understand the reviewer’s request, but in this instance, we feel that a descriptive approach suits the central aim of the paper which was simply to estimate how much activity is done and what this might mean for overall health budgets, rather than attempt to answer a different question relating to who is active in natural environments, all else equal. We agree that this might make an interesting paper but we feel this is beyond the scope of the current paper with its extremely tight word count. We are also far more familiar with the kind of approaches the reviewer suggests and hope to conduct this kind of work in the future.

The majority of the data relates to dog walking in an urban park, not all types of recreational physical activity in a wide range of natural environments. I think this needs to be made more explicit in the discussion.

We believe such an alteration would slightly misrepresent the findings as the data do pertain to all of the kinds of physical activity and natural environments included in the MENE survey. We agree that the majority of visits were to urban parks and dog walking constitutes a large number of visits but the aim of the paper was exactly to show this kind of thing. Of note, we can’t tell from the data as presented whether dog-walking occurred in urban parks – this would require many more tables of data to demonstrate the full relationship between where specific activities took place. Nevertheless in line with the reviewer’s request we have added more about the fact that walking was the predominant activity in lines 296 (where we discuss the HEAT results) and in the first line of the conclusion (line 367). Indeed this ties in nicely with the statement in our conclusion that “even regular walks in the park can have meaningful benefits to health”.

Reviewer #3: The aims of this study were i) to estimate annual adult levels of physical activity occurring in natural environments across England; ii) to assess the potential health benefits associated with this activity in terms of QALYs; and iii) to estimate the monetary impact of the social value of these QALYs.

The authors conclude that 19.5% of the population made at least one 'active visit' (i.e. ≥30 minutes, ≥3 METs) to natural environments in the previous week. These visits were associated with an estimated 109,164 (101,736, 116,592) QALYs annually, and an annual value of these visits of approximately £2.18 billion (£2.03, £2.33). These findings demonstrate that natural environments provide the context for a large proportion of England's recreational physical activity and highlight the need to protect and manage such environments for health purposes.

Comments:

1.-The authors have performed a pooling analysis across six waves of the MENA survey (period 2009-10 to 2014-15). Have the authors analyzed trends in the number of people who made "active visits" across these six waves or whether the estimated number of visits is consistent through the different waves?

We understand the reviewer’s interest in this issues, and we believe it relates to reviewer’s 2 enquiry with respect to multi-level modelling. However as with our response to their point (and below) although we agree that these temporal trends might be an interesting avenue for further research we feel such additional analyses are beyond the scope of this very short paper. More information on simple trends in visit frequency over time can be found in the MENE reports issued by Natural England (http://webarchive.nationalarchives.gov.uk/20140910073652/http://www.naturalengland.org.uk/ourwork/evidence/mene.aspx). Further, for information about the current work, the estimated number of ‘active’ visitors, for instance, was as follows across the 6 waves: W1 = 8,057,001; W2 =7,673,002 ; W3 = 8,036,001; W4 = 8,368,001; W5 = 8,726,001; W6 = 8,542,000. Thus, although there does seem to be a slight increase over time this is by no means linear with relative drops in W2 and W6 compared to previous years. Again, given space constraint we believe introducing this kind of dynamic to the paper is too complex at this point though we of course welcome further work along these lines in future.

2.-The authors could include a multivariate model to ascertain the sociodemographic variables associated with active visits.

As with our response to Reviewer 2 we believe this might constitute another paper entirely. The aim here was openly descriptive, as it needed to be, to establish the number of people who engaged in a sufficient amount of activity to be entered into the QALY calculations. We agree that it would be interesting to explore which sectors of the population benefited most but believe this is beyond the scope of this very short paper. We would of course welcome such analysis in future but this paper is very much a proof of concept, as we state in the discussion, rather than an attempt to definitively answer all possible interesting questions. We take it as an encouraging sign that the reviewer can already see further ways in which this work could be taken forward in future.

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