14th May 2015

Susan J. Elliott, PhD

Senior Editor

Social Science & Medicine

Dear Dr Elliott,

Re. Manuscript SSM-D-15-00356 Energy expenditure on recreational visits to different natural environments: Implications for public health.

Thank you for the opportunity to submit a revised version of the above manuscript. We are extremely grateful to the three reviewers for their careful consideration of the original submission and their constructive and encouraging comments.

We have carefully revised the manuscript in line with the reviewers’ suggestions. The changes we have made are highlighted in the text as tracked changes. In addition to these changes, we have made some alterations to statistics quoted in the manuscript after we discovered some minor errors in reporting; these are all highlighted as tracked changes too. We also amended Figure 1 in line with these changes, but this fails to appear as a tracked change; suffice to say the narrative of the manuscript is unaffected. Furthermore, we have removed references that were mistakenly in the reference list.

Moreover, considering the comments made by reviewers 2 and 3 and our responses, we have amended the title of the paper to simply read, “Energy expenditure on recreational visits to different natural environments.” As we have explained to these reviewers, the paper is intended to be descriptive (i.e. what happens in natural environments) rather than prescriptive (i.e. to inform the design of recreational spaces), and we feel this more concise title better reflects such a narrative. We have made this change on the cover page and in the online system and hope you agree that this is a more suitable title.

We also detail below our responses to reviewers’ concern and suggestions. Line and page numbers in our responses refer to the mark-up (tracked changes document) rather than the finalised (clean) version. We hope you agree that our efforts have significantly improved the quality and clarity of the article and you feel that the revised manuscript is now acceptable for publication in *Social Science & Medicine.* We look forward to hearing from you in due course pending the reviewers’ consideration of this revised manuscript.

Yours sincerely,

The authors

Reviewer #1: I thoroughly enjoyed reading this timely, well written, well documented, and interesting paper. I have only good things to say about this paper, and recommend immediate publication subject to only two minor changes.

At the outset of this paper, the authors clearly and rightly point out that whilst diverse natural environments can promote PA, it less known what environments are associated with what activities, and that this would keenly inform public policy with respect to identifying which types of environment are most beneficial for supporting physical activity.

*Response: We thank the reviewer for their kind comments.*

They then proceed to utilize an extensive and well-suited database to explore three clearly laid out research questions/objectives. The size of the sample is particularly impressive - 71,603 respondents. It is interesting that they only randomly selected one visit to a natural environment in the last week for in-depth questioning. I understand that they don't want to devote too much space to methodology, but a bit more explanation of this approach would be most valuable to future researchers - in particular, I would assume they adopted this approach to avoid the issue of repeat observation from the same subject that plaques other activity-based data collection. It would be worthwhile pointing this out, as a minor modification. The choice of regression modeling seems very appropriate in order to control for all the various factors proposed by the authors, and to make the analysis understandable by a wide audience. I was also pleased to see that the authors (in section 4.4 Limitations) clearly address limitations of their study design, especially the issue of self-selection associated with the cross-sectional sample.

*Response: As is pointed out in section 2.1, “further details necessary for this analysis are only asked about one randomly selected visit from the last week.” Indeed it is the data collectors themselves that are aware of how accounting for more than one random visit per respondent could bias any analysis, hence why they only ask detailed questions about one visit per respondent. To make this clearer we have changed the text to read, “All responses were recorded using a Computer Assisted Personal Interviewing (CAPI) device. Once respondents had provided brief details of all visits made in the last week, the CAPI device randomly selected one visit for the interviewer to ask more detailed questions about. The aim of random selection at the point of interview was to reduce potential biases such as recency effects for recall. The data used in the current paper relate to responses to this randomly selected visit.” (Page 5, Lines 19-24).*

The inclusion of environment visited as a variable in the analysis (i.e. urban greenspace, countryside, seaside resort, other coast) is perhaps the most novel inclusion. Choosing these 4 categories is an excellent start, although I would hope future plans include a finer level of geographic analysis, if possible. That said, it appears that this variable explains only a small portion of the variance in PA, whilst other visit characteristic such as duration play a more dominant role - although after controlling for other variables, this appears to change. I would strongly recommend that the authors explore this further in the discussion - meaning, in addition to concluding that "Countryside and urban greenspace visits were associated with more intense activities than coastal visits", more clearly point out the relative extent of this versus other variables in the discussion, as a minor modification.

*Response: We agree with the reviewer that these are relatively broad categories at the moment and with more years of data we hope to conduct analysis at a finer spatial level as suggested. Nonetheless, we believe there are important insights to be gained from even this rather coarse grained level given the novelty of the approach.*

*We also agree with the reviewer that a comparison of effects aids interpretation of findings and have thus added the following:*

*“4.4 The relative importance of the environment to energy expenditure*

*An estimation of the relative importance of the visited environment in predicting energy expenditure can be derived from comparing coefficients with those for other factors such as season, SES and gender. Taking Model 1 (Supplementary Table D) we can see that, compared to urban greenspaces, the coefficients for log-transformed MET minutes of countryside (0.02), seaside resorts (0.03) and other coast (0.03) appear relatively small (though these are all based on log-transformations). Nevertheless they are comparable to the effects of season (e.g. autumn vs. winter = 0.03), larger than SES (e.g. DE vs. AB = 0.01) and only a little smaller than gender (female vs. male = -0.05). In other words, the environment seems to play just as important a role in influencing energy expenditure as socio-demographic and seasonal variables.” (Page 18, lines 13-23).*

Reviewer #2: Review Memo

Manuscript Number: SSM-D-15-00356

Title: Energy expenditure on recreational visits to different natural environments: Implications for public health

Journal: Social Science & Medicine

1) Lines 11, 12, 13: "Visits to countryside and urban greenspace environments were associated with more intense activities than visits to coastal environments. However, visits to coastal environments were associated with the most energy expenditure overall due to their relatively long duration." It is unclear what is meant by intense activities. Is this the amount of energy spent per unit of time?

*Response: As the reviewer rightly assumes, this does refer to the amount of energy spent per unit of time (minute). In lines 8-9 of the abstract we note that intensity of visits is defined by METs. Since METs have existed for over two decades and are widely known in physical activity research as a standardised unit of energy cost, we believe it is satisfactory to say “more intense activities” with the interpretation that these activities are of a higher MET rate (as the reviewer correctly assumes).*

*An alternative would be to say more “vigorous” activities, but we would not wish to mislead the reader into thinking these activities are of a vigorous intensity (as all average MET rates equate to low-moderate intensity).*

2) Line 14: "how far respondents travelled to their destination." Are there data on the type of transport used to move from home to the recreation destination? Clearly walking or biking will take more human energy that using an auto or transit.

*Response: Again, as the reviewer rightly assumes, there is data on this. As we note in the article, we control for transport mode in all analysis and also run the “walker’s only model” (page 7, lines 12-19) to account for the fact that active travel to natural environments incurs energy cost as well.*

3) Line 17: "inform tailored activity promotion efforts in the future." It can also inform park planning in regards to the creation of new parks and recreational facilities.

*Response: This is a very well made point and we have adjusted the abstract to read, “Knowledge of what types of natural environment afford the highest volumes and intensities of physical activity could inform landscape architecture and exercise prescriptions.” (page 1, lines 17-18).*

4) "People local to coasts may use them more often for physical activity." Local to coasts means living closer to coats? Use coasts more often than whom? I think you are talking about the distance decay effect in recreational geography. McKercher (2010, p. 367) states that: "The impact of distance on demand is widely accepted in tourism geography, with a number of studies demonstrating that absolute volumes of tourists decline exponentially with distance."

*Response: We chose the term “local” to denote people who started their visit near the coast. In essence it covers the range of visit start-points – one’s own home; someone else’s home; holiday accommodation; workplace or elsewhere. However, in this sample, 95.1% of all visits started from one’s own home (91.1% of coastal resort visits started from home; 91.3% of other coast visits started from home). We appreciate the ambiguity however, and therefore have rephrased the highlight to read, “People situated at coasts (vs. inland) use them more often for physical activity.” (page 2, lines 4 and 5).*

5) McKercher, B. (2010): The Implicit Effect of Distance on Tourist Behavior: a Comparison of Short and Long Haul Pleasure Tourists to Hong Kong. Journal of Travel & Tourism Marketing, 25(3-4), 367-381.

*Response: This is a compelling effect and indeed it could explain some of the associations pertaining to travel distance. In a sense, we believe this closely relates to perceived affordances (i.e. suitability of an environment for a particular visit changes with travel distance). As such, we have added a passage in the discussion to reflect this which reads, “This finding may further reflect a distance decay effect, well recognised in tourism geography (McKercher, 2008), whereby the proportion of people who perceive a particular activity affordance associated with an environment declines with increasing travel distance to that environment.” (Page 18, lines 9-12).*

6) Page 5: "considering recent research suggesting that coastal residents utilise coastlines for PA (White et al, 2014)," Finding that coasts are utilized for recreation is hardly new. This has been well-known for centuries. The literature on property price levels shows that coastal environments are very attractive for recreational use, with high demand pushing up land prices.

*Response: The reviewer is correct in saying that the finding that coasts are used for recreation is not new. However, the article we are referring to finds, for the first time, that people living nearer the coast in the UK more often meet physical activity guidelines than their inland counterparts (replicating findings from Australia – Bauman et al., 1999 Geographical influences upon physical activity participation: evidence of a 'coastal effect'). Moreover, the paper finds that the reason they more often meet physical activity guidelines may be because they directly utilise coasts for health-enhancing activity. Nevertheless, we agree that how we have worded the sentence perhaps undermines the significance of the finding so have rephrased to read, “Lastly, considering recent research suggesting that coastal residents directly use the coastline in order to achieve higher levels of PA” (page 5, lines 7-8).*

7) The MENE survey is an impressive data set.

*Response: We thank the reviewer for their comment and certainly agree.*

8) Section 2.2.1. Intensity is defined as I expected, energy used per unit or time.

*Response: Indeed, see our response to point 1.*

9) It is interesting that the Imperial system of units is used. I would expect SI to be used in an international journal.

*Response: We fully understand and appreciate this point. However, as the data is collected in England, respondents are much more familiar reporting miles as opposed to kilometres. We wanted to try and represent the raw data in the MENE dataset as closely as possible and trust that the reader will understand and accept this as well.*

10) Page 10. "Lastly, the wave of the survey was controlled for to examine yearly differences in the outcome variables." What is the wave of the survey?

*Response: The wave of the survey refers to the yearly period of data collection. To aid readers we have amended all mentions of “survey wave” in the main manuscript and supplementary materials to read “survey year.”*

11) Page 11: "For all models, activities undertaken in coastal environments are lower in METs. However, coastal visits are longer in duration and consequently incur more expended MET minutes than visits to countryside or urban greenspaces." This is interesting. Water-based recreation, such as swimming. is often of high energy use, but this is highly seasonally variable. Swimming occurs only in the summer months. We are not told when the survey data was collected. In what time of year does the survey occur?

*Response: Yes, data is collected all year round (there’s a very meticulous sampling method which ensures accurate year-round sampling). We have amended section 2.1 (page 5, line 15) to read, “Data is collected throughout the year via in-home interviews.”*

12) Page 12. The differences in METS at different locales are very similar, 2.65, 2.44, and 2.52. I wonder if there is any real difference in health benefits between these three locales.

*Response: We agree and this is a point we raise at the end of the discussion – page 20, lines 22-23 read, “Although statistically the effects reported in this study are small, at a population level the differences could be substantial.” The main concern of this paper is with ecological public health and though these differences are small, population-level differences could mean a substantial number of people doing a higher-intensity activity in one environment compared to another.*

13) There is much data on dog walking. Are there data on what percentage of people in the study owned dogs? Or in society at large? Are there differences between those with dogs and those without?

*Response: We have amended the manuscript to read, “Model 1 regressions were repeated excluding dog-walking visits (32.3% of visits; see 2.4)” (page 14, line 10) in order to clarify the large proportion of visits that were for the purpose of dog walking. As dog walking has a lower MET rate (3.0) than walking without a dog (3.5 – principally due to the stop-start nature of dog-walking), there are indeed differences between dog-walkers and non-dog-walkers – these differences can be observed in the full regression tables which are part of the supplementary material. However, we did not wish to hone the discussion in on the issue of dog-walking – as the reviewer mentions, this literature is already quite extensive and as we point out in section 3.4.5, associations do not change too much. We feel this amount of attention is sufficient for this article.*

14) Page 13: "visits to all environments were associated with longer durations than visits to urban greenspaces." This is confusing. Do not visits to urban greenspaces include visits to all environments?

*Response: We agree with the reviewer and have amended this sentence to read, “Using model 1, visits to seaside resorts, other coast, and the countryside were associated with longer durations than visits to urban greenspaces.” (page 12, lines 15-16). We have also amended a similar passage (page 13, lines 9-10).*

15) Page 13: "visiting in summer were associated with longer visit durations." So seasonality of visit is recorded.

*Response: Indeed, see our response to point 11.*

16) Page 13: "visits to the two coastal categories remained significantly longer." I envisage that visits to coasts in England means travelling from the interior of the country to the salt water coasts that surround the island. There are not many interior lakes. Therefore, I suggest that one must take longer trips in terms of distance and time, than trips to local parks and countryside. Do you think this is correct?

*Response: Indeed, as pointed out in 2.3.1, coastal refers to the seaside in both coastal categories (rather than inland waterways which are encompassed in the other environmental categories). Longer travel time is an issue, which is both why we ran different models concerning one and two-way travel, and also why we stratify by travel distance i.e. in order to elucidate the potential effects of travel time and distance.*

17) It is interesting that there are two categories of home life, urban and rural. I wonder how this is defined. I often wonder about the utility of such classifications, as access to open space is more nuanced than simple classification of urban and rural. What about semi-rural, small towns, rural second homes, etc.?

*Response: We appreciate that this definition is unclear. Therefore we have added a sentence to the end of the method section which reads, “This dichotomy is defined in line with the 2001 Office for National Statistics classification where urban areas comprise urban settlements only, and rural areas comprise villages, town and fringe settlements, and hamlets or isolated dwellings” (page 10, lines 10-13). We appreciate that there is a more recent classification (2011), but the survey began data collection before this (and so has maintained the 2001 classification for consistency) and this also mirrors the dichotomy used in Mitchell & Popham’s (2007) study (section 1.1).*

18) As I look at the travel distance findings, I try to visualize the geography. Section 3.6 eludes me. I cannot think of geographical or planning implications.

*Response: We appreciate that this part of the analysis may lack planning implications. However, the purpose of this stratification was to demonstrate how the relationships between different environments and energy expenditure differ when people are more willing (or more able) to travel shorter and farther distances. As we discuss in section 4.3 of the discussion, the uniqueness of this finding is that it provides a reason why coastal residents are more active and report better self-reported health (i.e. we suppose this is because they are directly using coastlines for physical activity, whereas those situated within a mile of urban parks or countryside display significantly less energy expenditure in these environments).*

19) Page 15: "In sum, respondents local to coasts expended more energy in these environments, and respondents travelling further afield expended more energy in the countryside." I think I can explain part of this. People living close to coasts can spend more of their leisure time outdoors, because they have to spend less time commuting to and fro. What does travelling further afield mean? Longer travel distances?

*Response: Firstly to clear up the meaning of the sentence to which the reviewer refers, we have changed the text to read, “In sum, respondents situated within a mile of the coastline expended more energy in these environments, and respondents travelling farther distances expended more energy in the countryside” (page 15, lines 16-18). Secondly, the stratification was for the purposes of seeing how people with equivalent distances to each of the environment categories interact with these environments, so we would argue that the opportunity (notwithstanding personal or physical barriers) for outdoor recreation is equal for people situated within a mile of any accessible natural environment.*

20) Page 16: (e.g. a hill affords walking). What does this mean? Does a coast or a flat field not afford walking?

*Response: The reviewer is correct is indicating that lots of diverse environments offer the same affordance. The purpose of the example was in order to present an example of an environment that offers an affordance, but which could also represent an opportunity or a barrier to any given individual (in line with Heft’s 2010 view). However, we appreciate that this is potentially confusing so have removed this example from the text (as we detail examples in later paragraphs anyway). The text now reads, “Environments offer ‘affordances’ that elicit behaviour (Gibson, 1979) and whilst these exist in reality they better reflect relationships between environments and individuals (Heft, 2010)” (lines 9-13, page 16).*

21) Page 16. It could also be that visits to coasts are associated with longer vacations than visits to other environments.

*Response: The reviewer is correct in saying that longer bouts of lower-intensity PA at coasts could also be due to holiday visitors wishing to spend lots of time at the seaside. However, as we point out in our response to point 4, over 90% of coastal visits sampled are taken from the respondent’s home (not holiday accommodation), so we feel the present interpretation that it could be due to a wider variety of recreational activities suffices.*

22) Page 17. "Running may be more suited to countryside and urban greenspaces where circular routes are more easily defined than they would be at the coast for example." This is the type of analysis that I like to see, as it leads to planning outcomes.

*Response: We thank the reviewer for their comment and are glad they appreciate this interpretation.*

23) Page 17. "they also reflect individual perceptions (more people perceive barriers towards watersports; less people perceive barriers to running in greenspaces)." I agree with these two comments, but it would be better to have references. What percentage of people in England has swimming instruction that enables water contact in open waters? You should be able to find literature that shows that runners prefer greenspaces than non-greenspaces.

*Response: Actually, these were merely meant as potential examples and not meant to reflect empirical findings. Therefore, we have prefixed these with “e.g.” (apologies for not identifying this omission before; line 3, page 17; line 4, page 17). Interestingly, although regular exercisers prefer outdoor to indoor environments for exercise (Hug et al., 2009; Restorative qualities of indoor and outdoor exercise settings as predictors of exercise frequency), to our knowledge there hasn’t been any research that distinguishes environments more than indoor/outdoor or urban/natural as of yet for this purpose.*

24) You talk about promotion of exercise, but say nothing about planning of trails, recreational sites, or parks. Why not?

*We agree that our research has potential implications for planning etc. but we feel speculation about these issues is beyond the scope of the current paper. Rather our aim at this stage was to outline in detail the current evidence of physical activity in different environments as they are currently categorised. We believe further work is needed to explore the variation within different environmental settings, e.g. which kinds of parks and park features elicit the most physical activity but this level of specificity and detail is not in the MENE dataset and thus would be conjecture at this stage. We are therefore currently reluctant to make further observations on potential implications for planning due to the already lengthy and detailed nature of the manuscript but are willing to do so if the editor thinks this is an important issue and believes we have space to do so.*

25) Page 17: "that proximity to greenspaces may be most important for urban populations in terms of self-reported health." This may because greenspace is much less common in urban areas.

*Response: Although more green space is prevalent in rural areas, this is often inaccessible (e.g. private farmland). So to clarify, we have changed the sentence to read, “proximity to accessible greenspaces” (page 17, line 18).*

26) Page 18: "health benefits of urban greenspace visits, which were visited most often by this sample" This is an important fact, but I do not remember reading in this paper the percentage of visits to each environment in this sample.

*Response: We mentioned this proportion in section 2.3.1 but regret that we did not refer back to it in this section. We have amended the text of lines 4-5 of page 19 to read, “Furthermore, the health benefits of urban greenspace visits, which were visited most often by this sample (see 2.3.1).” Additionally, we adapted the text in section 2.3.1 to read, “Respondents can select one of: In a town or city (hereafter referred to as urban greenspace; 47% of visits); in the countryside (countryside; 43% of visits); a seaside resort or town (seaside resort; 7% of visits); or other seaside coastline (other coast; 3% of visits). These were used as the environments in this study and urban greenspaces, where most visits were taken, represented the reference category” in order to clear up the relative prevalence of visits to the 4 different environment categories (lines 3-7, page 8).*

27) Page 19. "migrating to coasts." I think it is a short term visit to the coasts, not a migration.

*Response: We accept the reviewer’s point. This limitation was supposed to reflect the lack of longitudinal data on moving residence to different environments and resultant changes in health behaviours. Therefore, we have changed the text to read, “However, this research is unable to establish whether moving residence to coasts results in more active use of them, or whether more active people move there to seek PA opportunities” (line 18, page 19).*

28) Page 19: "Knowledge of these could in turn inform urban design so urban areas could better elicit longer bouts of activity." Very true. But why only urban design? Why not park design, park placement, trail design, design of urban trails going to and from the parks, etc.?

*Response: We agree with the reviewer that this would be better worded as follows: "Knowledge of these could in turn inform urban park and trail design, for instance, to encourage urban areas to be used for longer bouts of activity" (lines 14-15, page 20). However, as with our response to 24 we do not feel our current data allow us to speculate beyond this assertion at the present time.*

29) Page 20: "needs to identify salutogenic characteristics of greenspaces that can be integrated into urban design so people unable to access more expansive environments can experience comparable health benefits." True, but the park and recreation design literature is full of this material.

*Response: We agree with the reviewer that there is already an extensive literature on this topic and thus the call for more research is somewhat misleading and instead we conclude with the following statement: “Designers should consider what volumes and intensities of physical activity they wish to elicit when designing new recreational spaces or routes so the correct environmental qualities for eliciting such behaviour are selected. Knowing the behavioural affordances of a more diverse range of natural environments provides a useful starting point” (page 21, lines 2-6).*

30) This is a good paper. The large sample size provides for useful findings that could have significant implications for public health. I would like to see this paper put more emphasis on the use of these findings for better planning of urban areas, parks, and trails. This paper cries out for better trail design to facilitate movement to and from greenspaces from urban areas. The bicycle lobby groups would be very supportive of these findings. Just before I read this paper today, the local public radio had a segment on bicycle master plan implementation. The planners were in front of city council requesting more and better managed trails through the city. This plan has a design for bicycle transport throughout the local city, for both recreation and commuting. This is one example of the type of planning that could flow from this paper.

*Response: We again thank the reviewer for his/her positive overall assessment of the paper. We also agree that the results may be of interest to trail designers etc. However, as with our response to point 24, we are reluctant at this stage to comment more directly on the planning implications because we believe there is insufficient detail about the different environments in the MENE survey to allow us to do this (e.g. no explicit data on cycle trails for example).*

Reviewer #3: This is a good paper. I do not have any comments on the methodological approach, but have a few concerns regarding the policy implications of these findings for public health recommendations.

*Response: We would like to thank the reviewer for their positive assessment.*

- Introduction: in relation to the mental health benefits of physical activity, the author(s) could cite some of the evidence related to subjective well-being data ("happiness" data). There are a few recent articles related to this issue in journals such as: Journal of Economic Psychology, Social Science & Medicine, and Social Indicators Research, to name but a few.

*Response: We agree with the reviewer and have cited a recent article from Social Indicators Research that examines physical activity and pleasure (Downward & Dawson, 2015). Additionally, we have changed a citation to better exemplify the effects of physical activity on depression (Ekkekakis, 2015) (page 3, lines 3-5).*

- There is now good evidence from the behavioural sciences that individuals license themselves to do "bad" things after having done "good" things. (eg Dolan and Galizzi (2014) Because I'm worth it: A lab-field experiment on the spillover effects of incentives in health. CEP working paper). To this extent, it is plausible that a long trip to a coastline might have been associated with more physical activity, but also with more calorie intake (eg an ice-cream or two while walking on the coastline). I wonder whether the data has any information related to this, but at the very least I suggest that such a possibility should be acknowledged before policy recommendations can be advanced.

*Response: The reviewer raises the issue of moral licensing (e.g. Merritt, Effron & Monin, 2010) and this is something we thought hard about during the development of this research. Part of our exclusion criteria for this study was to exclude people who reported doing more than one recreational activity on their visit. So, for example, a respondent who reports walking a dog in this analysis would not also be eating food at the same time (if we trust the respondent’s self-reports). However, as the reviewer rightly points out, it is impossible to control for what activities are done after the end of the visit and this was not acknowledged. Thus we have added a section after our discussion on activity substitution (lines 6-9, page 19) which reads, “Additionally, our analysis is unable to account for any effects of ‘moral self-licensing’ (Merritt, Effron & Monin, 2010) where for instance, because an individual has engaged in a longer walk (something ‘good’), they feel able to ‘treat themselves’ to a bigger piece of cake (something ‘bad’) resulting in overall energy intake which may be greater than that expended (e.g. Dolan & Galizzi, 2014). Further work is thus needed to focus on more extensive observations of specific visits to see whether certain types of visit are more likely to result in such ‘moral self-licensing’ than others, with implications for public health interventions.”*

- It is also true that longer visits (eg to coastlines) might be associated with socio-economic status (eg affordability to have a car, and even if this is the case, the cost of fuel). Which is not the case with shorter distances. The role of income - for which there is a wealth of evidence - is not sufficiently discussed.

*Response: We agree with the reviewer that visit duration is affected by socio-economic status. In our analysis, we controlled both for socio-economic status and whether the respondent travelled by car. The supplementary regression tables display that lower socio-economic status is associated with less intense activities, but longer visit durations, meaning that energy expenditure between different socio-economic groups is rather equal.*

*Indeed this may explain why other studies suggest that natural environment exposure can potentially reduce socio-economic deprivation related health inequalities: Wheeler et al (2012) find that the association between good health and coastal proximity is strongest in the most deprived areas suggesting that living near the coast may mitigate some of the negative health effects of socio-economic deprivation. Mitchell and Popham (2008) display comparable findings with greenspace access. Thus, these findings may be the case because different socio-economic groups use natural environments in ways that are equivalent in terms of energy expended. This is a no doubt compelling discussion, but we feel that it is outside the scope of this paper and may form the focus of another. The paper as it stands focuses much more on health geography – i.e. the main analysis regards relationships between different environments and physical health and our stratifications are on two key geographic variables (urbanity and travel distance). We believe it is beyond the scope of the current paper (given it’s already extensive nature) to look into further issues at this time. As with the response 24 to Reviewer 2 though, we are happy to take editorial guidance on this.*

- All of the above have implications for policy recommendations. It is not enough to advise people where to be more physically active. There are a number of reasons for this; not least, the fact that information interventions work better for those who are above a level of education to start with (see for example, Meera et al. (2008) The gap gets bigger: Changes in mortality and life expectancy by education. Health Affairs, 27, 350-360.).

*Response: As noted above, and in response to reviewer 2, the current paper is predominantly a descriptive rather than a prescriptive analysis. Our aim was to identify what is currently happening in terms of energy expenditure in different natural environments with a dataset that we believe is larger and more extensive than has been used to date. Implications for policy are unclear at this stage (not least because of the potential issue of moral licensing that the reviewer highlighted). We thus avoid making general prescriptions at this stage though as above we will be willing to do so if the editor feels this appropriate.*