Author’s response to reviews

Title: Who stays, who drops out? Biosocial predictors of adherence in participants attending a longer term exercise referral scheme in the UK

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Author’s response to reviews: see over
The Editor

MS: 3057835125667371: Who stays, who drops out? Biosocial predictors of adherence in participants attending a longer term exercise referral scheme in the UK

On behalf of the co-authors I thank you for the interest you have shown in this paper and the comments from the reviewers. Please find below a point-by-point response to the concerns raised. The revised manuscript is attached with amended areas highlighted as advised.

Yours sincerely

Patrick Tobi

Editor

Experimental research that is reported in the manuscript must have been performed with the approval of an appropriate ethics committee. Research carried out on humans must be in compliance with the Helsinki Declaration (http://www.wma.net/e/policy/b3.htm), and any experimental research on animals must follow internationally recognized guidelines. A statement to this effect must appear in the Methods section of the manuscript, including the name of the body which gave approval, with a reference number where appropriate.

The following statement has been added in the Methods/Participants section: ‘The study was approved by the University of East London Research Ethics Committee (ETH/09/01) and access to the data granted by NHS Greenwich and Greenwich Leisure Limited (GLL).’

Reviewer: Anne Vuillemin

Major compulsory revisions

The objective of the paper is not enough clear for the reader, neither in the text nor in the abstract.

The background section of the abstract and introduction section of the main text have been revised to clarify the objective of the paper i.e. to investigate the factors acting at the later (maintenance) stage of exercise behaviour change.

p4. It would be useful to provide the reader a reference for the definition of ERS.

A reference has been added (ref no 1).

p5. The reviewer is not certain that “mixed methods evaluation” is understandable? Please clarify “mixed”.

This has been clarified with the following insertion in the text: ‘…..which used a combination of qualitative and quantitative approaches to analyse participants exercise behaviour, experiences and barriers to participation in the scheme. …’. The authors’ evaluation report to which the statement refers has also been formally referenced (ref no 12):

p5. Where do 58% and 45% come from?

The data is taken from the authors’ evaluation report which is now included as an additional reference (ref no. 12):

p6. How the 1089 participants were registered? How were they recruited?

Participants were referred by their general practitioners to the exercise scheme. This is mentioned in the ‘Setting’ section (p5) and the text has been amended to clarify this. Text in the ‘Participants’ section has also been revised to make clear that participants’ details were obtained from the scheme’s database.
Adherers and non-adherers were defined on the basis of attendance <80% of scheduled sessions. How this choice of 80% was made? Can we consider that attendance over 80% is associated with health benefits?

We have inserted an explanation and reference for the 80% cut off. The section now reads: ‘Failure was determined as attendance at <80% of scheduled sessions. The level was based on adherence data from an earlier study [9].’

“Although they are continuous in nature….practice.” The argument is receivable. However, because the authors are interested in predictors, an analysis with continuous variable could have been more powerful.

We accept that retaining blood pressure as a continuous variable delivers more statistically powerful analysis. However, the loss of information created by the use of categories was weighed against the benefit of discussing the variable in terms that are practically meaningful, familiar and commonly applied in policy and practice. The text has been amended in the ‘Exposure measures’ section to acknowledge this trade off. Further, of the four continuous variables in our unadjusted analysis in table 1 (now table 2) - age, index of multiple deprivation, body mass index and blood pressure - only blood pressure was treated exclusively as categorical. Age was the only variable significantly associated with exercise adherence and was treated as a continuous variable in the regression model.

Because the level of physical activity and the level of adherence are different between men and women, an analysis by gender would be interesting. Some results have shown that men were more likely to attend exercise sessions despite lower referral rates.

Gender differences have been reported in exercise referral schemes although not consistently. We did not undertake detailed analysis as our data showed no difference. Indeed, gender returned one of the least significant p values of all variables tested in table 2. A possible explanation for this is offered in the section ‘Comparison with existing literature’.

Do some interactions have been tested (with gender or social conditions)?

We did not initially report this because (unreported) analysis prior to fitting the regression model did not find any significant interactions, leaving us to focus on the main effects. However, we acknowledge that given the results from some other studies (e.g. gender differences in adherence), it merits mention. Accordingly we have revised part of the text in the ‘Data analysis’ to include this.

Discussion: was the intervention appropriate for individuals who get referred? A discussion on appropriate referral could be added.

This has been included in the last paragraph of the discussion and a reference added.

It seems that a contradiction appears in the literature: results showed the positive effect on physical activity levels on the short-term but a low adherence and ineffective effects in increasing activity level in the long term and a higher adherence?

We are not clear what this comment means and so have not been able to respond adequately. If it is alluding to the fact that adherence rates drop over the long term, that is correct, although the decline is variable and we have mentioned this in both the introduction and discussion sections. Our study had a higher adherence rate than most other studies but still adherence was lower in the longer term compared to the short term as illustrated by the 13th and 20-26th week data.

A reference should be added to illustrate the 1st paragraph.

The text has been revised and references added

In the last phrase of the 1st paragraph what does mean “them”
“Them” refers to social factors such as gender, ethnicity and deprivation. The word has been replaced with “these factors” to clarify the sentence.

p13. In the study limitations, the authors should add a limitation. The level of physical activity at baseline was not reported. It can be suggested that adherence to the exercise intervention may be higher in participants more active at baseline? If these data are available, it would be interesting to look at this point.

This has been added to the text in the limitations section.

Minor essential revisions

p4. Add a point at the end of the 1st phrase, after [1,2] .

A full stop has been added.

Discretionary revisions

In the background of the abstract the authors refer to “…the maintenance stage”. Does this stage correspond to the stage of change from Prochaska? If yes, it would be interesting to introduce these stages in the paper.

A table has been added illustrating the stages of change (table 1).

p7. Medical referral category was collected and 4 were identified in the paper. How many categories exist in total?

There were 6 categories altogether (shown in figure 1). Specific mention was made of the largest four because they accounted for 90% of the cohort.

p8. The authors mentioned 7 deprivation domains. What are these domains?

These have been added to the relevant sentence.

Reviewer: Afroditi Stathi

Major Compulsory Revisions

1. There seems to be a mistake in the regression data. An OR of 1.02 does not represent a tenfold increase in odds of attendance. The authors need to check these analyses.

Thank you for drawing our attention to this. The statement has been corrected to indicate that the OR of 1.02 represents a 21.8% increase in odds of attendance for every 10 year increase in age.

2. Explain why the duration of the programme ranges from 20 to 26 weeks. Can you describe the type and duration of individual activities within the programme? i.e. what was the content of the programme? Do these factors affect programme attendance?

Documents we accessed during the study indicated that the duration of the scheme was decided by the commissioners using the best available understanding and models of good practice at the time. We did not at the time seek more specific information about the reasons behind their decision. We have now expanded the description of the activities in the setting section. While our interest was in biosocial factors, the discussion section readily acknowledges that wider influences, including the programme design factors highlighted, also influence attendance.

3. What is the frequency of programme sessions (i.e. once/twice a week?). What is the total number of sessions that participants could attend? Does that differ depending on the type of chosen activities? That would provide more meaningful data than simply the percentage (%) of session attendance.
The frequency and total number of sessions varied depending on the exercise plan agreed at enrolment. Attendance rates were calculated on an individual basis. On this basis and for the purpose of measurement we believe a percentage of total sessions attended remains a relevant and practical indicator.

5. The authors highlight inadequate participant profiling as a major weakness of many exercise of referral schemes. In this study, people with orthopaedic and cardiovascular conditions had lower rates of attendance. Do these people differ from participants in other groups in other characteristics? Could the authors provide such data? What possible confounders were taken into account in the analyses? Indeed, were predictors entered into a multi-variate model or a series of univariate models? – this is not clear in the description of the analysis.

The analysis is described in the Data analysis section. The process was in two stages – the 2nd stage being the logistic regression model where we entered all the factors that were associated with adherence on univariable analysis (first stage). Binomial logistic regression was used because the outcome variable was a dependent variable with 2 levels. The regression model takes possible confounders into account. The results are shown in table 2. We believe the extra text we have included in the section now clarifies the steps in our analysis.

6. The authors say in the introduction that most programmes report an 80% drop-out. Williams et al (2007) stress the low uptake rates in exercise on referral programmes. Here we are modelling short-term attendance with the starting point being attendance at session 1. What was the drop out between referral and attendance at session 1? What was the attendance at the gym or leisure centre after the exercise of referral programme finished? The terminology and place of this data in the overall model of a) uptake then b) programme attendance and c) maintenance of an active lifestyle needs to be made much clearer.

There are certainly important questions around the earlier phases of exercise schemes (initial referral, uptake and attendance) but these have been a primary focus of attention in most other studies for reasons discussed in our introduction section. Our interest was in the latter/maintenance phase (which we have referred to as ‘longer term adherence’). Indeed, much of our discussion attempts to compare early and late phase influences on adherence. We hope the text revisions we have made in the abstract and introduction in response to comments from the 2nd reviewer now better clarify this. Also, although we did not explicitly state it, we provided information in figure 1 on the study cohort which showed that 14 people did not start (i.e. take up the referral). This corresponds to a drop out of 1.3%.

We mentioned in our introduction that because of poor record keeping and the use of separate data systems by many schemes, it is often not possible to accurately track subsequent participants’ attendance at the gym or leisure centre after completing the exercise referral programme. This scheme was no different to others in that respect. We also made the point that it is questionable whether this is a truly valid measure of exercise adherence. The issue was again highlighted in the study limitations but we have added further text in the limitations section to emphasise this.

6. This exercise programme targeted particularly Black and Minority Ethnic People. Their proportion in the programme (37% compared to the 29% in the general population of Greenwich) demonstrates that the programme was quite successful in its target. What were the programme characteristics/strategies that not only attracted Black and Minority Ethnic people but helped them to adhere to the programme at comparable levels with White people could be very useful information and perhaps a direction for further research?

Although recruitment of BME participants to the scheme was successful, once within it they did not show better adherence rates than White participants (see ethnicity analysis in table 2). So while we acknowledge that this is an interesting feature, we feel the cause relates more to the external context of the scheme and beyond the objective of our paper.

7. Page 13, second paragraph. The authors have stressed that the adherence rates presented in this paper refer only to participation in structured exercise. That is a very good point as there is no information on
whether this programme triggers changes in everyday life, motivating people to become more active in
general during the programme or after its completion. Therefore, we can not even speculate that this could
be the case so the authors could probably omit the final sentence in Page 13, Paragraph two “while it is
highly likely…real world”.

The statement has been removed and the text in the section revised to highlight additional limitations.

The authors could add to their discussion that in future studies we need to address some of the following
issues: a) Do exercise on referral programmes with a fixed duration actively prepare participants for long
term maintenance of physically active lifestyle? If yes, what are the successful strategies for helping people
stay active in the long term? b) Can we monitor activity levels and patterns after programme completion to
examine the long term effects of participation in exercise on referral programmes?

Extra text has been added in the conclusion section around this.

8. The definition “longer term adherence” is highly misleading and does not imply maintenance to an active
lifestyle – the timeframe for this that would be recognised by most experts would be much longer than six
months. Even if we use the 6 month period as a cut-off point, the programme as a whole does not meet that
criterion as the different activities range from 20-26 weeks. “Adherence” as a term would be sufficient.

We appreciate the objection to this but would like to argue that the term should be retained for the
following reasons:

a. Longer term adherence was used to reflect the unusually long duration of participation in the
scheme and helps to compare and contrast in relation to other short term schemes. It therefore
gives sense to our comparisons to short term exercise behaviour.

b. The term is a descriptive one for the purpose of distinguishing the different phases of the overall
programme – ‘early’, ‘longer term’ and ‘very long term’ – and is consistent with the terminology
used by the National Institute for Health and Clinical Excellence to categorise the periods of
effectiveness of exercise referral schemes.

NICE (2006). A rapid review of the effectiveness of exercise referral schemes to promote
physical activity in adults. NICE Public Health Collaborating Centre – Physical activity Final

NICE (2006). A rapid review of the effectiveness of exercise referral schemes to promote
physical activity in adults. NICE Public Health Collaborating Centre – Physical activity Final

We have clearly defined in the paper what the term means and this is in line with the
recommendation of the British Heart Foundation ERS toolkit that terms used should be clearly
defined in order to provide more accurate and meaningful evaluation data.

BHF. A toolkit for the design, implementation and evaluation of exercise referral schemes.
Guidance for referring health professionals, exercise referral professionals and exercise

d. The timeframe by which ‘long’ has been defined in the past has not enjoyed common
understanding; but it is for that very reason that resources such as the NICE document referenced
above offer a valuable basis for unifying differing perspectives.

e. Prochaska and DiClemente’s stages of change model represents a continuum with coterminal
(and invariably overlapping) stages. Hence the cut off points between periods will be widely
understood as approximate rather than exact points in time. In this regard, we would argue that
reasonable equivalence can be made between the 20-26 weeks of the scheme and the 6 months
cut off.

f. The model acknowledges that behavioural change occurs in a cyclical process that involves both
progress and periodic relapse through all the stages. But in successful behavioural change, while
relapses occur, individuals never remain within the earlier stage to which they have regressed, but
spiral upwards, until they eventually reach a state where most of their time is spent in the
maintenance stage. This perhaps answers the reviewer’s comment that ‘the timeframe for this
........... would be much longer than six months’