PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	EFFECTIVENESS OF A TARGETED LIFESTYLE INTERVENTION IN PRIMARY CARE ON DIET AND PHYSICAL ACTIVITY AMONG SOUTH ASIANS AT RISK OF DIABETES: 2-YEAR RESULTS OF A RANDOMISED CONTROLLED TRIAL IN THE NETHERLANDS
AUTHORS	Vlaar, Everlina; Nierkens, Vera; Nicolaou, Mary; Middelkoop, Barend; Busschers, Wim; Stronks, Karien; van Valkengoed, Irene

VERSION 1 - REVIEW

REVIEWER	Namratha Kandula Northwestern University Chicago, IL USA
REVIEW RETURNED	27-Apr-2016

GENERAL COMMENTS	**Review** The purpose of this paper is to describe the effectiveness of a lifestyle intervention on diet, physical activity, and socio-cognitive outcomes among South Asians who are at high risk for type 2 diabetes living in the Netherlands. **Major comments** This paper would make a much stronger contribution to the literature if it included a well-developed conceptual framework related to intervention development and factors related to behavior change in South Asians, a more sophisticated description of potential motivators of behavior change in South Asians, and greater details about process evaluation measures. These factors could help the audience understand where gaps might have existed in the intervention delivery, acceptance, and targeted behaviors and socio-cognitive features. At this stage, there is a growing body of evidence that culturally adapted lifestyle interventions among South Asians migrants in the West have resulted in only very modest or insignificant changes in clinically relevant outcomes and behaviors. The authors needs to strengthen their rationale for why their findings are impactful and what we can learn from these findings. For example: 1. **Bhurji, et al. Improving management of type 2 diabetes in South Asian patients: a systematic review of intervention studies**. BMJ

	Open 2016;**6**:4 e008986 doi:10.1136/bmjopen-2015-008986 2. Morrison, et al. 2014. "Understanding experiences of participating in a weight loss lifestyle intervention trial: a qualitative evaluation of South Asians at high risk of diabetes." BMJ Open 3. Brown T, Smith S, Bhopal R, Kasim A, Summerbell C. Diet and physical activity interventions to prevent or treat obesity in South Asian children and adults: a systematic review and meta-analysis. Int J Environ Res Public Health. 2015 Jan **Other comments** *Introduction* Would be helpful to have more detail about what "deep" structures of culture the intervention addressed and how these deep structures related to the socio-cognitive outcomes measures. Even though the authors state they have described this before, it is important context for the current paper. Did any of the formative work point to acculturation, stress, or discrimination and factors that influence behavior and behavior change? These have been found to be important in other immigrant groups and also South Asians in other Western countries. *Methods* What language was the intervention developed in? Was there cultural and linguistic concordance between the dieticians and the participants? *Outcomes* Conceptual framework underdeveloped: sociocognitive factors are based on a Western model of health behavior change. Why did the research team focus on causal beliefs, perceived control, social support and self-efficacy? These are heavily influenced by Western attitudes to individual behavior change and ignore the collective nature of many cultures. In addition, there is no discussion of how the intervention components and counseling addressed these specific factors: causal beliefs, perceived control, social support and self-efficacy. Did the intervention address these specifically and if so, which components of the intervention were aimed at changing these socio-cognitive factors? *Analysis/Results* Table 1. We have no information on migration status, number of years in Netherlands, and language of participants. Also, do they have information on income? Confidence intervals have superseded p-values and these should be

	presented in the primary analysis and tables. Table 2 and 3 should be simplified- they are hard to read and confusing because 2 time points and so many rows. I would suggest moving baseline values of the main outcomes of interest to Table 1 and removing p-values in Table 1. Tables 2 and 3, can simply present the change in outcomes by intervention group at each time point. *Discussion* Discussion needs to be more robust. Authors could provide a more detailed discussion of immigration factors unique to South Asians in Netherlands, such as acculturation, language, and discrimination and how these may play a role in health and health behavior change. Authors should consider and discuss if using a more community-engaged approach throughout the research process and during the trial would have resulted in better outcomes. Was the community engaged in this process in any way? The high dropout suggests that the intervention had very limited reach and may not have been culturally acceptable. Community involvement could help some of these issues. Authors should discuss group-level barriers to change since their intervention was largely targeted to individual-level factors. Are their group-level barriers that could or should be addressed? *Minor comments* 2 typos p. 6, line 14 p 18, line 50

REVIEWER	Mary Beth Weber Assistant Professor of Global health Emory University USA
REVIEW RETURNED	04-May-2016

GENERAL COMMENTS	Overall, this is a clearly written and interesting manuscript. As the authors state, this is a high-risk group where successful interventions are desperately needed. I do have a few concerns I would like to see addressed: 1. Background: In the final paragraph, the second to last sentence beginning with "In that intervention…" What intervention are you

	talking about? Do you mean "In THIS intervention?" 2. Methods: A: First paragraph: there is a typo/sentence fragment in the second sentence ("The primary outcomThe term…."). B. Please clarify why you did not include OGTTs from April to October 2010. C. While I applaud the inclusion of family members in the intervention and understand why they needed to be in the same group, this affects randomization. Please discuss this issue specifically. For example, how many family members were included? When you remove them from the analysis, what effect does it have? Are they evenly distributed in each study arm? D. To better understand the type of nutrition data collected, please include some of the study questions used. Ideally, I would love to see all the questions as a supplemental table. 3. Results: A. Given what we know about health behaviors, can you please provide some data on changes in health behaviors at year 1? Where the changes made but not sustained? Or did people not make these changes at all? Obviously the ramifications of these are different. B. Tables 2 and 3: the data is not self-explanatory to me. I only really understood the tables after reading the text. Ideally, tables should stand-alone. I suggest either adding some explanatory footnotes or perhaps adding a row showing the percentage who had no change for each measure. C. Figure 1: can you please show the n lost to follow-up and missing data by study arm instead of aggregated?

REVIEWER	J. Anthony Michigan State University, United States
REVIEW RETURNED	26-May-2016

GENERAL COMMENTS	The PLOS One publication described much of the study details repeated here, and showed null outcomes in bio parameters such as weight gain. The article is focused on potential mediators such as lifestyle change. The analysis approach is not optimal for several reasons. One reason is substantial loss to followup, most likely non-ignorable. Such loss might be associated with the rigor of the life change plan and not completely at random -- I.e., an extreme value of the negative change parameter described in the findings. What is needed is an analysis plan appropriate for an "encouragement" design where participants can be encouraged to engage in an intervention, but he tx is not "administered"'as in a placebo versus active vaccine. In addition, the response variable focus on change from baseline may undercut validity of estimates because unreliability of baseline confounds unreliability of followup assays. In sum, an admirable explication of some of the processes that account for why the intervention did not achieve its primary goals. More qualitative than quantitative, and the result is useful for those who wish examples to fill in details already covered as generalities in the Sohal et al. (2015) systematic review. A more specialized journal such as Diabetes Care or Prevention Science might be especially interested in qualitative aspects of this work. More rigor in the quantitative aspect is needed before definitive evidence is achieved.

REVIEWER	Richard McNally Institute of Health & Society, Newcastle University, UK
REVIEW RETURNED	26-May-2016

GENERAL COMMENTS	GENERAL The statistical aspects need some improvement, particularly with regard to the description of what was done. SPECIFIC 1. Abstract. State statistical methods used. 2. Methods, Study Population, Line 14. Incomplete / muddled sentences. 3. Social-cognitive determinants of behaviour change. Define the measure of consistency used (alpha). 4. Statistical Analysis. More detail is required throughout. For example, in the first paragraph define the intervals given after the means. In the second paragraph describe how each of these methods or tests was applied to the data. Was normality assessed? If so, how? How were continuous variables analysed? How were categorical variables analysed? Some more clarity and careful explanation is required.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Namratha Kandula
Northwestern University , Chicago, IL , USA

Review
The purpose of this paper is to describe the effectiveness of a lifestyle intervention on diet, physical activity, and socio-cognitive outcomes among South Asians who are at high risk for type 2 diabetes living in the Netherlands.

Major comments
This paper would make a much stronger contribution to the literature if it included a well-developed conceptual framework related to intervention development and factors related to behavior change in South Asians, a more sophisticated description of potential motivators of behavior change in South Asians, and greater details about process evaluation measures. These factors could help the audience understand where gaps might have existed in the intervention delivery, acceptance, and targeted behaviors and socio-cognitive features. At this stage, there is a growing body of evidence that culturally adapted lifestyle interventions among South Asians migrants in the West have resulted in only very modest or insignificant changes in clinically relevant outcomes and behaviors. The authors needs to strengthen their rationale for why their findings are impactful and what we can learn from these findings.
For example:
1.
Bhurji, et al. Improving management of type 2 diabetes in South Asian patients: a systematic review of intervention studies. BMJ Open 2016;6:4 e008986 doi:10.1136/bmjopen-2015008986
2.
Morrison, et al. 2014. “Understanding experiences of participating in a weight loss lifestyle intervention trial: a qualitative evaluation of South Asians at high risk of diabetes.” BMJ Open
3.
Brown T, Smith S, Bhopal R, Kasim A, Summerbell C. Diet and physical activity interventions to prevent or treat obesity in South Asian children and adults: a systematic review and metaanalysis. Int J Environ Res Public Health. 2015 Jan

Reaction: *Indeed, so far, studies that have culturally adapted existing interventions to prevent T2D to South Asian migrants have shown modest changes in biomedical outcomes. Despite these disappointing results, these studies can provide very useful insights into what worked and what did not work. These and related questions are the focus of the newly established Eurodhyan collaboration (eurodhyan.eu) , in which researchers of the DHIAAN study take part.*
We agree with the reviewer that this requires a careful analysis of the results in view of, first, the theoretical base of the intervention, and second, the results of the process evaluation. We have now strengthened the theoretical base of the intervention in the revised version of our paper. We have presented a detailed description of the theoretical base of this intervention elsewhere (Nicolaou et al. 2013). We already referred to those papers, but have now included a summary of this information in the current paper in the data and methods section (p. 9-11).
Moreover, we carried out a process evaluation. The results of this part of the study accompanying the RCT has been published in the doctoral thesis of the PhD student working on this project (Linda Vlaar. Feasibility and effectiveness of a culturally targeted diabetes prevention program among South Asians in Dutch primary care. The DH!AAN-study. University of Amsterdam, PhD thesis 2014.) Some of these results had already been incorporated in the discussion section, but we have now extended this analysis, in order to increase the understanding as to why the intervention was not effective (p. 23-24).

Other comments
Introduction
Would be helpful to have more detail about what "deep" structures of culture the intervention addressed and how these deep structures related to the socio-cognitive outcomes measures. Even though the authors state they have described this before, it is important context for the current paper.

Reaction: *Thank you for this suggestion. We thought that the most logical place for giving more detail on cultural adaptations would be in the method section. On p. 9, we have now specified the specific goals of the intervention, which relate to the deep structure of culture. Creating the conviction that healthy eating can also be tasty is an example of this. This relates to specific socio-cognitive outcomes such as attitudes and self-efficacy.*

Did any of the formative work point to acculturation, stress, or discrimination and factors that influence behavior and behavior change? These have been found to be important in other immigrant groups and also South Asians in other Western countries.

Reaction: *We are indeed aware of literature pointing at the importance of acculturative stress and discrimination for the health of South Asian population. In our formative work for the present study, this appeared not to be a very important factor in relation to type 2 diabetes and the underlying behaviours. The limited role of discrimination has been confirmed in a recent paper published by our group in relation to the increased risk of metabolic syndrome in South Asian immigrants (Ikram et al. Psychosomatic Medicine 2016). However, the individual dietary counselling with the motivational interviewing technique could address these issue if important for the individual's behavior. We have added a comment to the methods section (p.8). Additionally, the intervention addressed stress due to social pressure through the involvement of family.*

Methods
What language was the intervention developed in? Was there cultural and linguistic concordance between the dieticians and the participants?

Reaction: *The intervention was developed in the Dutch language. South Asian Surinamese do have a good proficiency in Dutch (as Suriname is a former colony of the Netherlands). This is further illustrated –as now mentioned in the methods- by the finding that only two opted to be interviewed in Sarnami (Surinamese dialect based on North Indian dialects) at baseline.*
The six dieticians in the study were familiar with the South Asian Surinamese culture; three were South Asian Surinamese origin and three had extensive experience working within the South Asian community. This information has now been added in the method section (p. 9).

Outcomes
Conceptual framework underdeveloped: sociocognitive factors are based on a Western model of health behavior change. Why did the research team focus on causal beliefs, perceived control, social

support and self-efficacy? These are heavily influenced by Western attitudes to individual behavior change and ignore the collective nature of many cultures.

Reaction: *In previous work, similar attitudinal and self-efficacy patterns were found in the Surinamese population as in the general population, albeit for smoking (Nierkens et al. Prev Med 2006). So we had no reason to assume that the basic structure of previous interventions based on the sociocognitive models would not apply to Surinamese. But, the reviewer is of course right in saying that the e.g. the beliefs underlying these constructs might differ between immigrant and European origin populations. That is exactly the reason that the deep structure adaptations have been made, as explained in the methods section. One of the elements that address the collective character of the culture is the attention to the mobilization of social support by involving participant's families in the intervention.*

In addition, there is no discussion of how the intervention components and counseling addressed these specific factors: causal beliefs, perceived control, social support and self-efficacy. Did the intervention address these specifically and if so, which components of the intervention were aimed at changing these socio-cognitive factors?

Reaction: *As indicated above, in the method section (p. 9), we have now elaborated on the main socio-cognitive factors addressed in the culturally adapted intervention (e.g. appropriate risk perception), and the way the different components of the intervention (e.g. information materials, family sessions etc.) aimed to change these factors. For a more extensive description of this, we refer to our paper on the development of the intervention (Nicolaou et al. 2013). In addition, we mention (e.g. on p.8) that the counseling was based on motivational interviewing in which the individual beliefs are addressed.*

Analysis/Results
Table 1. We have no information on migration status, number of years in Netherlands, and language of participants. Also, do they have information on income?

Reaction: *We have added information on country of birth, years of residence in the Netherlands, having paid work, and net family income to Table 1. We have also added information on the preferred language for the interview to the methods section (participants were offered a choice between Dutch and Sarnami; all but two opted for Dutch).*

Confidence intervals have superseded p-values and these should be presented in the primary analysis and tables.

Reaction: *We have removed the p-values from Table 1. Where relevant, we have also changed the reporting from mean (SE) to mean (95%-confidence interval).*

Table 2 and 3 should be simplified- they are hard to read and confusing because 2 time points and so many rows. I would suggest moving baseline values of the main outcomes of interest to Table 1 and removing p-values in Table 1.
Tables 2 and 3, can simply present the change in outcomes by intervention group at each time point.

Reaction: *As suggested, we have moved the baseline values to Table 1 to simplify Tables 2 and 3. Changes in means are now reported with 95%-confidence intervals.*

Discussion
Discussion needs to be more robust. Authors could provide a more detailed discussion of immigration factors unique to South Asians in Netherlands, such as acculturation, language, and discrimination and how these may play a role in health and health behavior change.

Reaction: *As requested, we have detailed the discussion. As indicated in our reply to a previous point, discrimination and acculturative stress does not seem to be very important in shaping the behaviours underlying the risk of diabetes mellitus. We did, however, extended information on the background of the South Asian Surinamese population in the Netherlands in order for the reader to judge the generalizability of the results and to be better equipped to interpret the outcome (Introduction p. 5, and discussion p. 23).*

Authors should consider and discuss if using a more community-engaged approach throughout the research process and during the trial would have resulted in better outcomes. Was the community engaged in this process in any way? The high dropout suggests that the intervention had very limited reach and may not have been culturally acceptable. Community involvement could help some of these issues.

Reaction: *Thank you for this very valid suggestion. We have now explicitly referred to this strategy (p.22). A more community engaged approach could indeed be promising, particularly if it e.g. targets a larger part of the community.*
However, we should mention that the present study builds on a longstanding relationship of the Municipal Health Service the Hague (led by B. Middelkoop) with the South Asian community. In collaboration, they have developed and carried out multiple health interventions. Moreover, in the current study, we did try to engage the community in the pilot-stage of the project. We held focus group sessions, invited an advisory board, consulted professionals of South Asian origin, and tested information materials among potential participants. However, this appeared insufficient. Nevertheless, we also know from other studies that although community members are engaged, reach and participation can still be low (e.g. Nierkens et al. Nicotine Tob Res. 2013;15:112-20.).

Authors should discuss group-level barriers to change since their intervention was largely targeted to individual-level factors. Are their group-level barriers that could or should be addressed?

Reaction: *Although the intervention targeted the individual, also group-level barriers were targeted. Now that we have given more detail on the intervention and the conceptual model underlying this, we hope it will become clear for the reviewer and reader. In addition, in the discussion section (p. 23) we have emphasized possible next steps, such as the importance (and possible difficulty of) changes in the environment (including the social norm), so as to make the easy choice the healthy choice.*

Minor comments
2 typos : p. 6, line 14 , p 18, line 50

Reaction: *These typos have been corrected.*

Reviewer: 2

Mary Beth Weber
Assistant Professor of Global health
Emory University
USA

Please leave your comments for the authors below
Overall, this is a clearly written and interesting manuscript. As the authors state, this is a high-risk group where successful interventions are desperately needed.

I do have a few concerns I would like to see addressed:

1. Background: In the final paragraph, the second to last sentence beginning with "In that intervention…" What intervention are you talking about? Do you mean "In THIS intervention?"

Reaction: *We were indeed referring to the intervention in the current paper. We have changed the text to 'In this intervention…'.*

2. Methods:
A: First paragraph: there is a typo/sentence fragment in the second sentence ("The primary outcomThe term….").

Reaction: *This has been corrected.*

B. Please clarify why you did not include OGTTs from April to October 2010.

Reaction: *We extended the recruitment period of the trial [Changes were reported in Vlaar et al. 2012, and referred to in the trial registration]. After the extension, the OGTT was dropped from the protocol for practical reasons. Due to the shorter duration of a screening with a single measurement, a greater number of people could be screened within the available time. We have clarified this in the text.*

C. While I applaud the inclusion of family members in the intervention and understand why they needed to be in the same group, this affects randomization. Please discuss this issue specifically. For example, how many family members were included? When you remove them from the analysis, what effect does it have? Are they evenly distributed in each study arm?

Reaction: *We included 18 people with family members in the intervention group and 11 in the control group. Removal of people with family members from the analysis does not affect the interpretation of the results. To briefly illustrate:*

	INT	*CON*	*p-value T2*
Mean moderate-to-vigorous activity (min/week)	*11.7 (-126.2-149.5)*	*18.0 (-120.5-156.6)*	*0.969*
Mean total activity (min/week)	*-94.1 (-277.5-89.4)*	*72.3 (-121.9-266.5)*	*0.525*
Whole wheat: almost exclusively (%)			
Participants with positive change (%)	*21 (14.3)*	*16 (11.6)*	*0.667*
Participants with negative change (%)	*6 (4.1)*	*4 (2.9)*	
Meal pattern: 3 meals/day at a regular times (%)			
Participants with positive change (%)	*30 (20.5)*	*25 (18.2)*	*0.359*
Participants with negative change (%)	*10 (6.8)*	*16 (11.7)*	

We have added this information to the Methods section as follows:
Furthermore, as only 29 people with family members in the study had follow-up data available (intervention n=18, control n=11), no multilevel analysis was performed on family data. Analysis of reported total physical activity, meal pattern and whole wheat consumption after exclusion of all people with family members in the study showed similar results to the analysis in the full population (data not shown).

D. To better understand the type of nutrition data collected, please include some of the study questions used. Ideally, I would love to see all the questions as a supplemental table.

Reaction: *We have copied the questions to a supplement (New supplement 1) and have added a reference under Methods. Because the questions are all in Dutch, we have translated some questions (concerning brown rice) below:*
- Do you have an elevated risk of diabetes if you eat much white rice? Yes/No/Do not know
- How many times per week do you eat rice? 6 – 7 times per week/ 3 – 5 times per week/1 – 2 times per week < 1 times per week / (almost) never
- What type of rice do you eat? Always white rice/ Mostly white rice, sometimes brown rice/ As often white rice as brown rice/ Mostly brown rice, sometimes white rice/ Always brown rice/ I (almost) never eat rice
-I find the recommendation on eating on eating brown rice… very important/important/ not important and not unimportant/ unimportant/totally unimportant (Question asked after brief listing of the national dietary guidelines by interviewer.)
- I find eating brown rice… pleasurable / a little pleasurable/ not pleasurable and not unpleasurable/ a little unpleasurable/ unpleasurable/ not applicable
- Are you encouraged by close family members to more often eat brown rice instead of white rice? Yes, very often/yes, often/ yes, sometimes/no, almost never/ no, never/ not applicable
- To what extent could you manage to eat brown rice instead of white rice every day from today, if you wished to change this aspect of your diet? Definitely manage/ Likely manage / Maybe manage or

maybe not/ Likely not manage/ Definitely not manage (Please note that translation combines the question and the interviewer instruction)
- I plan to eat brown rice instead of white rice… Yes, already have since 6 months or longer/ Yes, since recently (less than 6 months)/ No, but planning to shortly (less than 1 month)/ No, but planning to within next 6 months/ No, and not planning to (Please note that translation combines the question and the interviewer instruction)

3. Results:
A. Given what we know about health behaviors, can you please provide some data on changes in health behaviors at year 1? Where the changes made but not sustained? Or did people not make these changes at all? Obviously the ramifications of these are different.

Reaction: *The changes after 1 year are shown in Tables 2 and 3. We hope that the changes in the lay out of the tables have made the presentation of these results more clear. In addition, we have now added a marking for significance of changes after 1 year. We unfortunately do not have information on changes at other time points during the first year.*

B. Tables 2 and 3: the data is not self-explanatory to me. I only really understood the tables after reading the text. Ideally, tables should stand-alone. I suggest either adding some explanatory footnotes or perhaps adding a row showing the percentage who had no change for each measure.

Reaction: *As requested, we have simplified the layout of the tables by moving the baseline values to Table1 (please also see our response to reviewer 1 - Namratha Kandula). As suggested, we have also added an explanatory sentence to the footnote: The remaining participants had not changed their dietary intake or physical activity.*

C. Figure 1: can you please show the n lost to follow-up and missing data by study arm instead of aggregated?

Reaction: *We have added this to Figure 1.*

Reviewer: 3

J. Anthony
Michigan State University, United States

Please leave your comments for the authors below

The PLOS One publication described much of the study details repeated here, and showed null outcomes in bio parameters such as weight gain. The article is focused on potential mediators such as lifestyle change. The analysis approach is not optimal for several reasons. One reason is substantial loss to followup, most likely non-ignorable. Such loss might be associated with the rigor of the life change plan and not completely at random -- I.e., an extreme value of the negative change parameter described in the findings.

Reaction: *We agree with the reviewer that loss to follow-up poses a serious problem and likely non-ignorable. Therefore, we had already attempted to quantify drop out and to assess the potential effects on our conclusions. In the limitation section of our study, we have now explicitly stated that it is likely that drop-outs had a poor experience in the intervention. We are, however, hesitant to ascribe the high percentage "negative changes" to selection effects. Unfortunately, recent discussions within our European collaboration (Eurodhyan.eu) have revealed that in other trials (with attrition <5%) a substantial proportion of people also experienced negative changes during the trial period. For instance Bhopal et al. report a weight loss distribution in the intervention group 1.13 kg (SD 4.12), which suggests weight gain in a substantial proportion of the population.*

What is needed is an analysis plan appropriate for an "encouragement" design where participants can be encouraged to engage in an intervention, but he tx is not "administered"'as in a placebo versus active vaccine.
Reaction: *We very much appreciate the suggestion to use an encouragement design. However, this design does not seem applicable to our study where we compare an intensive lifestyle intervention,*

carried out by specially trained health professionals (experimental condition) to a control intervention. The key aspect in the encouragement design seems to be the prompt for randomly-selected individuals to an already-available service. Lifestyle interventions such as ours are not standard care in the Netherlands, however. The service was only available to intervention group participants , but not to control group participants.

Nevertheless, we have attempted to incorporate a more general point following from the concerns of the reviewer, namely the finding that the intervention in its current form is not acceptable enough for the target group, despite likely positive selection due tolow response and high drop-out. Moreover, the findings show that even the best scenario –i.e. in the group that can be expected to have had the best intervention experience- there is no significant effectiveness. We have now mentioned more general implications for acceptability on p. 23, and in the summary of strengths and limitations p.3. We have also discussed that the intervention addressed barriers that had been identified (see addition to methods p.9-10), but perhaps these are too strong to be addressed with a single intervention (see addition p.24 discussion). We kindly refer to the text for more detail.

In addition, the response variable focus on change from baseline may undercut validity of estimates because unreliability of baseline confounds unreliability of followup assays.

Reaction: *We have thoroughly discussed the limitations of the use of selfreported data in the limitations section of our paper. We have discussed the choice between an analysis of change relative to baseline and an analysis of the difference with our statistician and co-author Busschers, who checked the data and advised to report as is presented in the tables.*

In sum, an admirable explication of some of the processes that account for why the intervention did not achieve its primary goals. More qualitative than quantitative, and the result is useful for those who wish examples to fill in details already covered as generalities in the Sohal et al. (2015) systematic review. A more specialized journal such as Diabetes Care or Prevention Science might be especially interested in qualitative aspects of this work. More rigor in the quantitative aspect is needed before definitive evidence is achieved.

Reaction:
The paper by Sohal et al. 'Barriers and Facilitators for Type-2 Diabetes Management in South Asians' focuses on diabetes management rather than prevention. We believe that our paper adds to rather than replicates the work summarized in that paper. Our work extends some of the generalities to the prevention of diabetes; e.g. we note that several of the named barriers and facilitators overlap with aspects addressed in our intervention. In addition, we analysed changes in health behaviours. As Sohal et al. did not report on the changes in behavior, we unfortunately were not able to compare differences or similarities related to this aspect.

We are grateful to the reviewer for the suggestion to extend the discussion as to why the intervention did not work, which actually corresponds with the comments of the first reviewer. We have done so in the revised version, and hope this meets the expectation of both reviewers.

Reviewer: 4

Richard McNally
Institute of Health & Society, Newcastle University, UK

Please leave your comments for the authors below
GENERAL
The statistical aspects need some improvement, particularly with regard to the description of what was done.

SPECIFIC
1. Abstract. State statistical methods used.

Reaction: *The statistical methods were added to the abstract.*

2. Methods, Study Population, Line 14. Incomplete / muddled sentences.

Reaction: *We have corrected this.*

3. Social-cognitive determinants of behaviour change. Define the measure of consistency used (alpha).
Reaction: *We used Cronbach's alpha to estimate the internal consistency of items within the scale. This was clarified in the text.*

4. Statistical Analysis. More detail is required throughout. For example, in the first paragraph define the intervals given after the means. In the second paragraph describe how each of these methods or tests was applied to the data. Was normality assessed? If so, how? How were continuous variables analysed? How were categorical variables analysed? Some more clarity and careful explanation is required.

Reaction: *We have detailed the description of the statistical analysis (p. 15-17).*

VERSION 2 – REVIEW

REVIEWER	Mary Beth Weber Emory University, USA
REVIEW RETURNED	15-Jul-2016

GENERAL COMMENTS	The authors addressed all comments thoroughly; the changes have improved the manuscript.

REVIEWER	Richard McNally Institute of Health & Society, Newcastle University, UK
REVIEW RETURNED	Institute of Health & Society, Newcastle University, UK

GENERAL COMMENTS	The authors have addressed all of my concerns.