PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Educational disparities in the intention to quit smoking among male smokers in China: a cross-sectional survey on the explanations provided by the Theory of Planned Behaviour
AUTHORS	Huang, Xinyuan; Droomers, Mariel; Fu, Wenjie; Yang, Yong; Li, Hong; Zheng, P

VERSION 1 - REVIEW

REVIEWER	Jiang Yuan Tobacco Control Office, China CDC China
REVIEW RETURNED	22-Feb-2016

GENERAL COMMENTS	This research is very important, especial for CHines tobacco control, 3 million smoker, understanding quit behavior. One comment:just like auther mentioned, somke free policy is different in three cities. Could add the city factor to know it the SF policy can affect the quit behavior.

REVIEWER	Dr. Nihaya Daoud Department of Public Health, Ben-Gurion University of the Negev.
REVIEW RETURNED	15-Mar-2016

GENERAL COMMENTS	This study repeats previous research that was conducted in the Netherlands by the first author et.al.: Droomers M, Schrijvers CT, Mackenbach JP. Educational differences in theintention to stop smoking: explanations based on the Theory of Planned Behaviour. Eur J Public Health 2004;14(2):194-8. This fact was not mentioned directly in the introduction, methods or discussion. Since the study methods are "not new" but the study setting is different, the authors could make an effort to explain why they repeated the same study in China. Was they aim to study the applicability of the theory of planned behavior to the social context of China? As for the mediation analysis, for a variable to be explanatory it should meet three conditions: First the independent variable should predict the dependent variable, second the independent variable should be associated with the possible explanatory variables, and third the possible mediator should reduce the associations between the independent and dependent variable when it is introduced in regression models.

Please see: Baron RM, Kenny DA. The moderator-mediator variable distinction in social psychological research: conceptual, strategic, and statistical considerations. J Pers Soc Psychol. 1986 Dec;51(6):1173-82

One of these conditions (regarding the association with education) was not answered in this study, but the researchers continued to conduct mediation analysis. While the results were interesting, we don't know if this is a "real association" or it is an 'artifact' confounded by other factors that were not considered in this study such as smoking related variables, nicotine dependence, number of years of smoking, other smokers in the family etc.

REVIEWER	Michael Parks Minnesota Department of Health, United States of America
REVIEW RETURNED	25-Mar-2016

GENERAL COMMENTS	<u>Summary:</u> This study investigates the intention to quit smoking among Chinese males, with a focus on how levels of education relate to intentions to quit. More specifically, the study examined how constructs purported by the Theory of Planned Behavior explain the relationship between education and intentions to quit smoking (i.e., perceived social norms, self-efficacy or behavioral control). The sample consisted of male smokers from three main cities in China – Shanghai, Nanning, and Mudanjiang) – with the final analytic sample comprised of 3,676 male smokers aged 18 or older. Results show no bivariate association between education and intentions to quit smoking. However, education was related to attitudes toward smoking cessation, perceived social norms associated with smoking cessation, and self-efficacy in regards to cessation. Bootstrapping techniques were used to examine mediation, and results show that perceived social norms mediated the non-relationship between education and intentions to quit smoking. No other dimension of the Theory of Planned Behavior helped to explain the education-intention to quit non-relationship. In sum, a primary conclusion was that aspects of the Theory of Planned Behavior can help explain social determinants of smoking-related behavior, which has potential to guide intervention and prevention efforts focused on disparities in smoking within China. <u>General Comments:</u> Smoking and smoking cessation in vulnerable populations is undoubtedly an important subject. Arguably since the now commonly cited 2006 article from Jha et al. in *Lancet* (among other works, such as work from Kenneth Warner, and other scholars who study the social determinants of health), we have known that smoking is a strong determinant of health disparities associated with income, and that increasing smoking cessation in low-income populations has potential to dramatically decrease gaps in health outcomes demarcated by income and poverty. I agree with

statements in the current manuscript that this subject matter is worthwhile. As a reviewer, I concluded that the subject matter is quite important (i.e., disparities in smoking, smoking cessation in vulnerable populations, and how they contribute to health disparities within nations experiencing high prevalence rates of smoking, such as China [see Ng et al., 2014]).

I think the major strengths of the current manuscript were: (1) it reports interesting data, (2) there are interesting results showing how the Theory of Planned Behavior (TPB) may help explain social disparities in smoking-related behavior and health in China, and (3) it is an interesting finding that social norms can be a target for prevention efforts in countries like China, as this type of prevention work could potentially draw from a growing body of literature in countries such as the U.S. on how to utilize social networks within prevention science.

However, as a reader I concluded that manuscript consists of a few weaknesses that challenge the quality of the current manuscript and the analysis. The main issues that I noticed are: (1) the framing of the introduction could be stronger, (2) the description of the data was minimal and more critical information is needed, (3) the measure of "socioeconomic disparities" is not necessarily rooted in literature, (4) we are missing key descriptive statistics on the main independent and dependent variables, (5) there is clustering within cities that is not accounted for in the analysis, (6) it is debatable whether one should conduct mediation analyses on a non-significant and non-robust bivariate relationship, (7) the general presentation and writing style of the manuscript was relatively difficult to follow as a reader in some sections. I will offer more detailed comments below.

Introduction

1. I think the introduction could include more specific details on smoking rates in China and how smoking-related behaviors and health are distributed across education or income in China. For example, the recent article in *JAMA* from Ng et al. (see Ng et al., 2014) shows that China currently has one of the highest rates of smoking in the world, and this fact can frame the importance of the current study. I also think that the paper could frame the purpose of the paper to be more directed at how TBP might help to explain social disparities in smoking and smoking-related behaviors (i.e., intentions to quit) in China. As it is currently written, the reader is not explicitly given this research question, although I think it is the main purpose of the paper and the analysis.
2. I think it is important to note that the term "socioeconomic disparities" is not necessarily rooted in past literature within the manuscript. There are differences among the constructs of low-income, low-SES, "disadvantaged," low-education, etc. This may appear to be nitpicking, but I think it is important to clarify. For instance, in the manuscript's introduction there is mention of

inequalities associated with SES as well as disparities associated with education and even occupational categories. These terms are used almost interchangeably in the introduction, but seminal work on the social determinants of health (e.g., Berkman & Kawachi, 2000) is rooted in classic sociological theory and research. Sociologists for centuries have debated the meaning of poverty, social stratification, "social class," and how to measure these phenomena. Social science has continued to demonstrate that there are differences among the phenomena of income, education, "class," occupational prestige/groupings, etc. For example, SES is typically a scale that includes income *and* education (and sometimes, occupational prestige). But this continues to be debated in the literature. The central point is that the choice of operationalization and choice of wording is important when examining socioeconomic disparities – particularly when the analyses only include levels of education like in the current manuscript. Literature has noted that certain aspects of education can influence health behavior in a unique fashion compared to other potential sources of disparities (e.g., income).

Methods

3. On page 4 there is critical information missing in regards to the data and sampling techniques used. There appears to be two different sample techniques used to generate the single sample. Is this correct? We need more information on the distribution of the sample across these two techniques. Are there more details on the community sampling technique used to generate the sample of retirees?

4. We are not given critical descriptive information on the sample, the main independent variable, and the main dependent variable. How many current smokers were daily smokers versus not daily smokers? What was the distribution of the intention to quit smoking variables (both at one month and six months)?

5. We see that the data are individuals clustered within cities. This may require random-effects or multilevel regression models that account for clustering within cities (Raudenbush, & Bryk, 2002). At a minimum we might need to know how bad the clustering is (i.e., how the main independent and dependent variables [i.e., TPB variables and intention to quit] vary across the three cities). Adjusting the standard errors for the clustering could be sufficient in order to arrive at appropriate significance tests. However, as of right now there is no adjusting for the fact the data come from three different cities with different sociodemographic profiles within the main set of analyses.

6. The article from Hayes (2009) is cited within the text as a justification for using mediation analyses even in the absence of a non-significant and non-existent bivariate relationship. Hayes offers reasonable justification for doing such analyses, however it is still debatable how acceptable it is to conduct mediation analyses on non-robust bivariate relationship. That is, in the current case there is no association between education and

intentions to quit smoking.

7. As noted, the measure of socioeconomic status is only a measure of education. The phrasing of the measure should probably state that the analysis considers education and not a scale of SES, as the measure does not include a scale or index of SES that includes other constructs such as income or occupational prestige.

Conclusion

8. The discussion and conclusion section was interesting and intriguing to read. There were parts that were also relatively hard to follow as a reader because of the writing style and wording. The findings on the role of social norms is interesting as it indicates that social network approaches in prevention science identified in US-based research could be applicable in a cross-national context such as China (see Gest et al., 2011).

Specific Comments:

Page 4, line 31: "officer" is missing a "c."

Page 4, line 39: Exclude should be past tense.

Page 6, line 33: To which confounders does this refer?

References
1. Berkman, L. F., & Kawachi, I. (2000). *Social epidemiology*. New York, NY: Oxford University Press.
2. Ng, M., Freeman, M. K., Fleming, T. D., Robinson, M., Dwyer-Lindgren, L., Thomson, B., …, Gakidou, E. (2014). Smoking prevalence and cigarette consumption in 187 countries, 1980–2012. *Journal of the American Medical Association*, *311*(2), 183–192.
3. Raudenbush, S. W., & Bryk, A. S. (2002). *Hierarchical linear models: Applications and data analysis methods*. Newbury Park, CA: Sage.
4. Gest, S. D., Osgood, D. W., Feinberg, M. E., Bierman, K. L., & Moody, J. (2011). Strengthening prevention program theories and evaluations: Contributions from social network analysis. *Prevention Science*, *12*, 349–360.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Jiang Yuan
Institution and CountryL Tobacco Control Office, China CDC, China
Competing Interests: None

This research is very important, especial for CHines tobacco control, 3 million smoker, understanding quit behavior.

We want to thank the reviewer for pointing out the importance of the results of our survey for the

further development of Chinese tobacco control. We have added a similar statement to the introduction to stress this need for and use of our results (second paragraph, last sentence).

One comment: just like auther mentioned, somke free policy is different in three cities. Could add the city factor to know it the SF policy can affect the quit behavior.

We agree with the reviewer that it would be very interesting to study if smoke free policies affect the quitting behaviour. In the current study we do however focus on individual determinants of smoking cessation behaviour and have no intention to evaluate the impact of different policies. We decided to study individual determinants of the smoking cessation process in these three different cities in order to account for different phases in the smoking epidemic. We hypothesized that different policies might have led to differences in the components of the Theory of Planned Behaviour, such as the subjective norm or personal attitude, thereby maximizing the variation in determinants in our study.
Last, we are not able to study the quit behaviour do only have information about the intention of these smokers to quit. We did not follow-up their actual behaviour changes.

Reviewer: 2
Reviewer Name: Dr. Nihaya Daoud
Institution and Country: Department of Public Health, Ben-Gurion University of the Negev, Israel.
Competing Interests: None declared

This study repeats previous research that was conducted in the Netherlands by the first author et.al.: Droomers M, Schrijvers CT, Mackenbach JP. Educational differences in theintention to stop smoking: explanations based on the Theory of Planned Behaviour. Eur J Public Health 2004;14(2):194-8.

This fact was not mentioned directly in the introduction, methods or discussion. Since the study methods are "not new" but the study setting is different, the authors could make an effort to explain why they repeated the same study in China. Was they aim to study the applicability of the theory of planned behavior to the social context of China?
The reviewer correctly refers to earlier work of the first author. We did at first not include a reference to this earlier work, because this work did not start as a copy of the earlier work in the first place. Like the reviewer suspected, the Chinese study is indeed an attempt to study the impact of the components of the Theory of Planned Behaviour (TPB) on smoking cessation in China. We did not intend to test the applicability of the TPB within the social context of China as such, but wanted to study the potential impact of its components in order to inform the development of(more) effective strategies to promote smoking cessation. Furthermore, this Chinese study included culturally appropriate measures of the components of TPB and is also in that regard not a copy of earlier work. As suggested by the reviewer, we have added a reference to the earlier work and its results to the introduction to illustrate the usefulness of the TPB to inform the development of intervention strategies targeting educational differences in smoking cessation (fifth paragraph, last sentence) as well as the discussion to stress the similarity of the results (second paragraph, fist sentence).

As for the mediation analysis, for a variable to be explanatory it should meet three conditions: First the independent variable should predict the dependent variable, second the independent variable should be associated with the possible explanatory variables, and third the possible mediator should reduce the associations between the independent and dependent variable when it is introduced in regression models. Please see: Baron RM, Kenny DA. The moderator-mediator variable distinction in social psychological research: conceptual, strategic, and statistical considerations. J Pers Soc Psychol. 1986 Dec;51(6):1173-82

One of these conditions (regarding the association with education) was not answered in this study, but the researchers continued to conduct mediation analysis. While the results were interesting, we

don't know if this is a "real association" or it is an 'artifact' confounded by other factors that were not considered in this study such as smoking related variables, nicotine dependence, number of years of smoking, other smokers in the family etc.

We appreciate that the reviewer pointed out that we did not sufficiently justify the use of mediation analyses in the absence of an association between educational level and the intention. We have explained our reasons to do so in more detail in the result section (fifth paragraph, second and third sentence). We are familiar with the Baron and Kenny methodology, but we are also aware of the criticisms, such as expressed by Hayes AF. Beyond Baron and Kenny: Statistical Mediation Analysis in the New Millennium. Communication Monographs 2009;76(4):408-420. We refered to the latter as the basis for our decision to perform mediation analysis. We would also like to refer here to comment number 6 of reviewer 3 who acknowledges that "Hayes offers reasonable justification for doing such analyses" .

Reviewer: 3
Reviewer Name: Michael Parks
Institution and Country: Minnesota Department of Health, United States of America
Competing Interests: None declared.

Comments for authors:
Summary:
This study investigates the intention to quit smoking among Chinese males, with a focus on how levels of education relate to intentions to quit. More specifically, the study examined how constructs purported by the Theory of Planned Behavior explain the relationship between education and intentions to quit smoking (i.e., perceived social norms, self-efficacy or behavioral control). The sample consisted of male smokers from three main cities in China – Shanghai, Nanning, and Mudanjiang) – with the final analytic sample comprised of 3,676 male smokers aged 18 or older. Results show no bivariate association between education and intentions to quit smoking. However, education was related to attitudes toward smoking cessation, perceived social norms associated with smoking cessation, and self-efficacy in regards to cessation. Bootstrapping techniques were used to examine mediation, and results show that perceived social norms mediated the non-relationship between education and intentions to quit smoking. No other dimension of the Theory of Planned Behavior helped to explain the education-intention to quit non-relationship. In sum, a primary conclusion was that aspects of the Theory of Planned Behavior can help explain social determinants of smoking-related behavior, which has potential to guide intervention and prevention efforts focused on disparities in smoking within China.
General Comments:
Smoking and smoking cessation in vulnerable populations is undoubtedly an important subject. Arguably since the now commonly cited 2006 article from Jha et al. in Lancet (among other works, such as work from Kenneth Warner, and other scholars who study the social determinants of health), we have known that smoking is a strong determinant of health disparities associated with income, and that increasing smoking cessation in low-income populations has potential to dramatically decrease gaps in health outcomes demarcated by income and poverty. I agree with statements in the current manuscript that this subject matter is worthwhile. As a reviewer, I concluded that the subject matter is quite important (i.e., disparities in smoking, smoking cessation in vulnerable populations, and how they contribute to health disparities within nations experiencing high prevalence rates of smoking, such as China [see Ng et al., 2014]).
Based on the general comments of the reviewer we decided to more explicitly acknowledge and state the contribution of smoking to socioeconomic health disparities in the introduction (first paragraph, 3-4th sentences) in order to emphasize the relevance of our study.
I think the major strengths of the current manuscript were: (1) it reports interesting data, (2) there are interesting results showing how the Theory of Planned Behavior (TPB) may help explain social disparities in smoking-related behavior and health in China, and (3) it is an interesting finding that

social norms can be a target for prevention efforts in countries like China, as this type of prevention work could potentially draw from a growing body of literature in countries such as the U.S. on how to utilize social networks within prevention science.

However, as a reader I concluded that manuscript consists of a few weaknesses that challenge the quality of the current manuscript and the analysis. The main issues that I noticed are: (1) the framing of the introduction could be stronger, (2) the description of the data was minimal and more critical information is needed, (3) the measure of "socioeconomic disparities" is not necessarily rooted in literature, (4) we are missing key descriptive statistics on the main independent and dependent variables, (5) there is clustering within cities that is not accounted for in the analysis, (6) it is debatable whether one should conduct mediation analyses on a non-significant and non-robust bivariate relationship, (7) the general presentation and writing style of the manuscript was relatively difficult to follow as a reader in some sections. I will offer more detailed comments below.

Introduction

1. I think the introduction could include more specific details on smoking rates in China and how smoking-related behaviors and health are distributed across education or income in China. For example, the recent article in JAMA from Ng et al. (see Ng et al., 2014) shows that China currently has one of the highest rates of smoking in the world, and this fact can frame the importance of the current study. I also think that the paper could frame the purpose of the paper to be more directed at how TBP might help to explain social disparities in smoking and smoking-related behaviors (i.e., intentions to quit) in China. As it is currently written, the reader is not explicitly given this research question, although I think it is the main purpose of the paper and the analysis.

We have added more details about the general smoking rates in China, using the reference that was so kindly suggested by the reviewer among others (see introduction, second paragraph, fourth sentence and further).

2. I think it is important to note that the term "socioeconomic disparities" is not necessarily rooted in past literature within the manuscript. There are differences among the constructs of low-income, low-SES, "disadvantaged," low-education, etc. This may appear to be nitpicking, but I think it is important to clarify. For instance, in the manuscript's introduction there is mention of inequalities associated with SES as well as disparities associated with education and even occupational categories. These terms are used almost interchangeably in the introduction, but seminal work on the social determinants of health (e.g., Berkman & Kawachi, 2000) is rooted in classic sociological theory and research. Sociologists for centuries have debated the meaning of poverty, social stratification, "social class," and how to measure these phenomena. Social science has continued to demonstrate that there are differences among the phenomena of income, education, "class," occupational prestige/groupings, etc. For example, SES is typically a scale that includes income and education (and sometimes, occupational prestige). But this continues to be debated in the literature. The central point is that the choice of operationalization and choice of wording is important when examining socioeconomic disparities – particularly when the analyses only include levels of education like in the current manuscript. Literature has noted that certain aspects of education can influence health behavior in a unique fashion compared to other potential sources of disparities (e.g., income).

We completely agree with the reviewer and apologize for our sloppiness. We have adapted the wording used throughout the manuscript so it reflects the fact that we are studying and reporting about educational differences in this paper.

Methods

3. On page 4 there is critical information missing in regards to the data and sampling techniques used. There appears to be two different sample techniques used to generate the single sample. Is this correct? We need more information on the distribution of the sample across these two techniques. Are there more details on the community sampling technique used to generate the sample of retirees?

The reviewer is correct in assuming that we used two different sampling strategies. We have more explicitly described this process in the Method, study population section (second paragraph).

4. We are not given critical descriptive information on the sample, the main independent variable, and

the main dependent variable. How many current smokers were daily smokers versus not daily smokers? What was the distribution of the intention to quit smoking variables (both at one month and six months)?

We thank the reviewer for pointing out this omission. We have added the descriptive information about daily smoking and the intention to quit smoking to table 2 in the Result section. These results were described in the first two paragraphs of the result section.

5. We see that the data are individuals clustered within cities. This may require random-effects or multilevel regression models that account for clustering within cities (Raudenbush, & Bryk, 2002). At a minimum we might need to know how bad the clustering is (i.e., how the main independent and dependent variables [i.e., TPB variables and intention to quit] vary across the three cities). Adjusting the standard errors for the clustering could be sufficient in order to arrive at appropriate significance tests. However, as of right now there is no adjusting for the fact the data come from three different cities with different sociodemographic profiles within the main set of analyses.

We did test if clustering of the intention to quit smoking occurred between males living within the same cities. We used MLWin to perform multilevel logistic regression analyses with individual smokers on level 1 and cities on level 2. The empty model revealed that there was no statistically significant difference between cities in the intention to quit smoking with 1 month (p=0.319) and 6 months (p=0.281). The Intra Class Correlation (ICC) or variance in the intention attributable to the city level was estimated to amount 1.1%. We have added this information to the manuscript in the Method, analyses section, 2nd-3rd sentence.

For the reviewer's information we here would like to further explain how we did estimate the ICC or clustering of individuals within the cities, because when analyzing a binary outcome instead of a continuous one, there is no simple calculation of the variance partition coefficient (VPC). The VPC is, for a two-level random intercept model, the proportion of total residual variance which is attributable to level 2 or cities, i.e, $\sigma^2_{u0}=(\sigma^2_{u0} + \sigma^2_e)$. The VPC is equal to the intra-unit correlation, which is the correlation between two level 1 units in the same level 2 unit. In the case of binary and other discrete response models, there is no single VPC measure since the level 1 variance is a function of the mean, which depends on the values of the explanatory variables in the model. The VPC (and therefor the ICC) has to be estimated. One alternative VPC measure is obtained if the logistic model is cast in the form of a linear threshold model. The VPC can then be computed as $\sigma^2_{u0}/(\sigma^2_{u0} + 3:29)$. Using such a threshold representation of our model, we obtained a VPC of 0.038/0.038+3.290 = 0.0114 (1 month intention) and 0.037/0.037+3.290 = 0.0111 (6 months intention).This means that 1.1% of the variation in the intention to quit smoking can be attributed to the city level.

6. The article from Hayes (2009) is cited within the text as a justification for using mediation analyses even in the absence of a non-significant and non-existent bivariate relationship. Hayes offers reasonable justification for doing such analyses, however it is still debatable how acceptable it is to conduct mediation analyses on non-robust bivariate relationship. That is, in the current case there is no association between education and intentions to quit smoking.

We agree with the reviewer that Hayes offers reasonable justification for doing mediation analysis even in the absence of an association between the independent and dependent variable. Because of the comments of this reviewer and reviewer 2, we decided to describe our reasons to do so in more detail in the result section (fifth paragraph, second and third sentence).

7. As noted, the measure of socioeconomic status is only a measure of education. The phrasing of the measure should probably state that the analysis considers education and not a scale of SES, as the measure does not include a scale or index of SES that includes other constructs such as income or occupational prestige.

We would like to refer to our answer to this reviewer's earlier comment on this matter (comment number 2).

Conclusion

8. The discussion and conclusion section was interesting and intriguing to read. There were parts that were also relatively hard to follow as a reader because of the writing style and wording. The findings on the role of social norms is interesting as it indicates that social network approaches in prevention

science identified in US-based research could be applicable in a cross-national context such as China (see Gest et al., 2011).

We thank the reviewer for suggesting the intriguing reference about the use of social network approaches in public health interventions. We have added this to our discussion (last paragraph, last sentence). We have asked an English text corrector to correct the writing style and wording of the manuscript.

Specific Comments:

Page 4, line 31: "officer" is missing a "c."
We have corrected this word.
Page 4, line 39: Exclude should be past tense.
This word has been deleted when adjusting this paragraph about the sampling procedure.
Page 6, line 33: To which confounders does this refer?
We were referring to the confounders included in the multivariate logistic regressions mentioned one paragraph (10 lines) before. We have decided to mention the confounders in detail again to avoid this kind of confusion for the reader. We have also added information on the confounders included in the reported analyses to the tables.

References
1. Berkman, L. F., & Kawachi, I. (2000). Social epidemiology. New York, NY: Oxford University Press.
2. Ng, M., Freeman, M. K., Fleming, T. D., Robinson, M., Dwyer-Lindgren, L., Thomson, B., …, Gakidou, E. (2014). Smoking prevalence and cigarette consumption in 187 countries, 1980–2012. Journal of the American Medical Association, 311(2), 183–192.
3. Raudenbush, S. W., & Bryk, A. S. (2002). Hierarchical linear models: Applications and data analysis methods. Newbury Park, CA: Sage.
4. Gest, S. D., Osgood, D. W., Feinberg, M. E., Bierman, K. L., & Moody, J. (2011). Strengthening prevention program theories and evaluations: Contributions from social network analysis. Prevention Science, 12, 349–360.

VERSION 2 – REVIEW

REVIEWER	Dr Victoria Allgar University of York, UK
REVIEW RETURNED	02-Aug-2016

GENERAL COMMENTS	The methods section does not give detail on how respondents were approached - recruitment rates, consent rates, and how the survey was completed - postal, face to face, online? A STROBE checklist should be included along with a flow-chart of recruitment.

VERSION 2 – AUTHOR RESPONSE

Reviewer Name: Dr Victoria Allgar
Institution and Country: University of York, UK
Competing Interests: None declared

The methods section does not give detail on how respondents were approached - recruitment rates, consent rates, and how the survey was completed - postal, face to face, online? A STROBE checklist should be included along with a flow-chart of recruitment.

We have added more detail information about the recruitment process in (see paragraphs in page 5-

6, marked yellow). We also made a flowchart of recruitment and will upload it as a supplementary file with the STROBE checklist.

VERSION 3 – REVIEW

REVIEWER	Dr Victoria Allgar University of York, England
REVIEW RETURNED	05-Sep-2016

GENERAL COMMENTS	The authors have addressed the previous comments.