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ARTICLE DETAILS

TITLE (PROVISIONAL)	Major Health-Related Behaviors and Mental Well-being in the General Population: The Health Survey for England
AUTHORS	Stranges, Saverio; Samaraweera, Preshila; Taggart, Frances; Kandala, Ngianga-Bakwin; Stewart-Brown, Sarah

VERSION 1 - REVIEW

REVIEWER	Vibeke Koushede The National Institute of Public Health University of Southern Denmark
REVIEW RETURNED	23-Jun-2014

GENERAL COMMENTS	Brief summary: The background for the study is that little is known about major behavioral risk factors impact on mental well-being in the general population. The authors examine behavioral correlates of high and low mental wellbeing using data from the Health Survey for England. The authors conclude that fruit and vegetable consumption is the health-related behavior most consistently associated with low and high mental wellbeing in both sexes, that smoking is also strongly correlated but that alcohol intake and obesity are not associated with high mental wellbeing. Methods: The authors write that the Health Survey for England (HSE) is an annual survey of a nationally representative population sample of England. In Denmark we have seen a decreasing response rate in our national health surveys over time and unfortunately they appear to becoming less and less representative of the entire population see e.g. http://www.ncbi.nlm.nih.gov/pubmed/22786925. Also the Supplementary Table 1 of the comparison of selected lifestyle characteristics between participants with and without WEMWBS data show a statistically significant different distribution between the two groups - I propose the authors touch on these issues of generalizability in the discussion. Is there a reason why the authors choose to call: "employed, unemployed seeking work, retired, economically inactive" for economic status rather than employment status? And what does economically inactive cover? In relation to this - Underneath the fully adjusted models it says adjusted for socio-demographic variables (age, sex, marital status, education, employment income and ethnicity) without a "," between employment and income but as I read it in the methods section the

models are adjusted for what I would call employment status and household income. (Also, here the word economic status is not used). Is this correct?

Results - discussion:
BMI - In Denmark leading experts in obesity research are currently suggesting that BMI is not an appropriate measure if the aim is to examine health, as it only takes weight related to height into account but doesn´t account for the actual distribution of fat. An individual may have a high BMI yet a healthy percentage of body fat or have a low BMI with a harmful percentage of body fat. As I read the results in the unadjusted models the OR for low MWB are highest among not only the obese but also the underweight in a u-shaped manner – as the authors also point out they are in sex-stratified analyses among women. BMI is a commonly used measure in health surveys yet perhaps this comment might be a point for discussion regarding the results of e.g. overweight and high MWB.

Smoking: Lower odds ratios of high mental wellbeing were found among ex-smokers (0.79, 95% CI 0.69-0.90) – could this be due to underlying illness resulting in smoking cessation?

I have the following considerations in relation to alcohol intake and mental well-being:
Table one shows a statistical significant different distribution of all four health-related behaviors in relation to well-being. Looking at alcohol there are e.g. fewer individuals with low MWB who drink sensibly compared those with medium or high MWB, and there are fewer individuals with high MWB who have a harmful drinking behavior compared to those with medium and low MWB.
I believe that one would generally not expect a linear trend in relation to alcohol intake and health but rather a u-shaped tendency – which also appears to be the case in relation to well-being – therefore I suggest leaving out the p value for linear trend in table 2 and 3. I am uncertain if changing the reference group to be the sensible drinkers rather than never-drinkers might alter the statistical significance off the results related to alcohol but I am not a statistician.

Also looking at the results I wonder if the mechanisms or underlying characteristics behind never drinkers and ex-drinkers among individuals with low MWB and those with high MWB might differ, do the authors have any thoughts on this?.

There is as I see it a u-shaped association between alcohol intake and low MWB which is statistically significant in table 2 model 2 adjusted for age and sex.

In the models in table 3 there similarly appears to be an association between alcohol intake and high mental wellbeing albeit not statistically significant. The results suggest that individuals with a harmful drinking pattern are the least likely to have high odds of high MWB compared to the other groups and this result is also statistically significant in model 2.

In the supplementary tables 2 and 3 there also appears to be a u-shaped association between OR for low mental wellbeing and alcohol intake among women and men in the unadjusted models, and among women in the adjusted model. Similarly there appears to be an almost u-shaped pattern in the unadjusted model of sex-

stratified analyses, OR for high mental wellbeing (supplementary table 3) related to alcohol intake among men and women - with harmful drinkers having the lowest OR of high MWB compared to the other groups.

Most health behaviors are patterned by social determinants and many of these determinants as well as health-behaviors are strongly correlated with one another e.g. education, income, and employment, smoking and alcohol – perhaps the non-significant results in model 3 reflect some sort of over- adjustment? I suggest the authors perhaps consider this aspect e.g. in the discussion. Causal graphs (Directed Acyclic Graphs) are increasingly used to address this issue see e.g. Pearl J. Causality: models, reasoning and inference. Cambridge: Cambridge University Press, 2000.

Conclusion:
In the abstract the authors write that little is known about major behavioral risk factors impact on mental well-being in the general population, and in the introduction that they sought to quantify cross-sectional associations between these behavioral risk factors and mental wellbeing in this large representative sample of the English adult population.

From the wording and choice of outcome it is apparent that the authors believe health behavior to have a causal effect on well-being. And in conclusion the authors suggest that their findings highlight the potential role of fruit and vegetable intake as a driver not just of physical but also of mental wellbeing in the general population.

I am aware of the strong associations between mental health and health behavior. I also understand how e.g. physical activity might influence well-being, or how smoking due to withdrawal symptoms between cigarettes might affect well-being. Yet how a high fruit and veg intake may result in high MWB in itself is not clear to me unless it is due to a correlation e.g. with physical activity (as the authors themselves touch upon) or reverse causality. The authors point to a few recent studies in the discussion that suggest a causal pathway between fruit and veg intake and well-being, yet these studies may similarly be affected by problems related to confounding and reverse causality. I would be interested to hear more about the authors' thoughts of the underlying mechanism between fruit and veg intake and mental well-being.

Also I know that a high intake of fruit and veg is recommended yet there is to date no systematic evidence to rule out with certainty that accumulative pesticide and herbicide residue is not harmful although the doses are small – This is merely a personal reflection.

The authors point themselves to limitations of the study related to causality - I would soften the conclusion – perhaps rather write that fruit and vegetable consumption is the health-related behavior most consistently associated with low and high mental wellbeing and may play a potential as a driver not just of physical but also of mental wellbeing in the general population – future studies should be carried out to address this topic. It is a very interesting study regardless of findings related to fruit and veg as the other findings support the notion that results related to low MWB and high MWB do not merely mirror each other, and that mental health and well-being should not be considered a continuum.

	Minor comments: Under the method section health behavior it says Data on physical activity related were not collected in either the 2010 or 2011 surveys it seems the word related should be deleted. There is a "d" missing in table 2 model 1 in the word unadjusted. Also there is part of a number missing in model 2, same table, under the result for <1 portion/day fruit and veg. Again thank you for giving me the opportunity to comment on this interesting study.

REVIEWER	Mark S. Kaplan, Dr.P.H. Professor of Social Welfare UCLA Luskin School of Public Affairs USA
REVIEW RETURNED	04-Jul-2014

GENERAL COMMENTS	As the authors correctly note, "mental wellbeing now assumes an important place in mental health and public health policy." Although it remains an important public health issue, the evidence on the behavioral correlates of mental wellbeing is limited. Psychiatric pathology has an enormous population health impact. Therefore increasing our understanding of the role of mental wellbeing in behavioral health is critically important for public health. The correlational analysis is based on a large nationally representative (although sampling procedure were not included) database with extensive information on lifestyle habits. The primary aim involves association between behavioral risk factors and mental wellbeing. Overall, the study is well conceived, with a clear aim. The findings shed important light on our understanding of the health behavior correlates of mental health. and may contribute to future clinical and public health interventions. The biggest concerns involve the absence of a physical activity measure. Also, the authors should discuss how fruit and vegetable consumption might also represent a complex set of highly correlated dietary exposures, including fish, whole grains, etc. In terms of the methodology, because of the absence of well-established cut-points (e.g., caseness), the authors might want to use the continuous version of the WEMWBS. However, if they use a categorical outcome, I recommend using a multinomial regression rather than two logistic models or adjust for multiple comparisons. Apparently, income should include a "missing" category in the model. The manuscript has a few typos and the use of the term "average" rather that "middle range." Finally, here are some recent review articles that could be cited in the study. Lai, Jun S., et al. "A systematic review and meta-analysis of dietary patterns and depression in community-dwelling adults." The American journal of clinical nutrition (2014): ajcn-069880. Rooney, Ciara, Michelle C. McKinley, and Jayne V. Woodside. "The potential role of fruit and vegetables in aspects of psychological well-being: a review of the literature and future directions." Proceedings of the Nutrition Society 72.04 (2013): 420-432. Quirk, Shae E., et al. "The association between diet quality, dietary patterns and depression in adults: a systematic review." BMC psychiatry 13.1 (2013): 175.

	Sanhueza, C., Ryan, L., & Foxcroft, D. R. (2013). Diet and the risk of unipolar depression in adults: systematic review of cohort studies. Journal of Human Nutrition and Dietetics, 26(1), 56-70.

VERSION 1 – AUTHOR RESPONSE

RESPONSE TO REVIEWER 1
Thank you for giving me the opportunity to review this very well written paper on a highly relevant public health topic. The background, methods and results are all clear and well described.
Reply: Thank you for very kind remarks about our manuscript.
Methods: The authors write that the Health Survey for England (HSE) is an annual survey of a nationally representative population sample of England. In Denmark we have seen a decreasing response rate in our national health surveys over time and unfortunately they appear to becoming less and less representative of the entire population... Also the Supplementary Table 1 of the comparison of selected lifestyle characteristics between participants with and without WEMWBS data shows a statistically significant different distribution between the two groups - I propose the authors touch on these issues of generalizability in the discussion.
Reply: The reviewer makes an excellent point. In the methods section we have already quoted the household response rate which was 66% in both years. In the discussion we have added a sentence drawing readers' attention to this and also referring to the percentage of WEMWBS complete responders in our sample and commenting on the generalizability. However we have edited Supplementary Table 1 because a missing values category had been included in error. When these are excluded there are fewer significant differences in the distribution of the selected variables between responders and non-responders. (The differences were because there is an expected correlation between non-response for WEMWBS questions and non-response for other questions).
Is there a reason why the authors choose to call: "employed, unemployed seeking work, retired, and economically inactive" for economic status rather than employment status? And what does economically inactive cover?
Reply: This was a derived variable compiled by the Health Survey for England (HSE). It is a "social classification system that attempts to classify groups on the basis of employment relations, based on characteristics such as career prospects, autonomy, mode of payment and period of notice" unquote HSE. "Economically inactive" covers people who are not full time students. We have used the terminology employment status in the revised manuscript.
In relation to this - Underneath the fully adjusted models it says adjusted for socio-demographic variables (age, sex, marital status, education, employment income and ethnicity) without a "," between employment and income but as I read it in the methods section the models are adjusted for what I would call employment status and household income... Is this correct?
Reply: Yes, this is correct, i.e. the models were adjusted for employment status and household income indeed. We have clarified this issue in the revised manuscript and revised tables.

Results - discussion: BMI - In Denmark leading experts in obesity research are currently suggesting that BMI is not an appropriate measure if the aim is to examine health, as it only takes weight related to height into account but doesn´t account for the actual distribution of fat. An individual may have a high BMI yet a healthy percentage of body fat or have a low BMI with a harmful percentage of body fat. As I read the results in the unadjusted models the OR for low MWB are highest among not only the obese but also the underweight in a u-shaped manner – as the authors also point out they are in sex-stratified analyses among women. BMI is a commonly used measure in health surveys yet perhaps this comment might be a point for discussion regarding the results of e.g. overweight and high MWB.
Reply: These are two additional, excellent points made by this reviewer. We have altered the discussion to include these two points, and included an additional reference on the important and

independent role of body fat distribution, above and beyond relative weight, on health outcomes. Waist to hip ratio is also included in the HSE data and we could have used this; however, these measurements can be inaccurate so the convention is still to use body mass index where no direct fat percentage measures are available.

Smoking: Lower odds ratios of high mental wellbeing were found among ex-smokers (0.79, 95% CI 0.69-0.90) – could this be due to underlying illness resulting in smoking cessation?
Reply: this is a plausible explanation for this result, and we have included a statement in the discussion to acknowledge this issue.

I have the following considerations in relation to alcohol intake and mental well-being…Looking at alcohol there are e.g. fewer individuals with low MWB who drink sensibly compared those with medium or high MWB, and there are fewer individuals with high MWB who have a harmful drinking behavior compared to those with medium and low MWB. I believe that one would generally not expect a linear trend in relation to alcohol intake and health but rather a u-shaped tendency – which also appears to be the case in relation to well-being – therefore I suggest leaving out the p value for linear trend in table 2 and 3. I am uncertain if changing the reference group to be the sensible drinkers rather than never-drinkers might alter the statistical significance off the results related to alcohol but I am not a statistician.
Reply: Following the reviewer's suggestion, we have acknowledged the well-known U-shaped relationship between alcohol and health in the discussion of the revised paper. It is a credible explanation that moderate alcohol users are people who have an overall healthy social life and this would explain the relationship with wellbeing. We have left the analysis with the reference category "no alcohol", since this is the conventional format. For consistency across lifestyle variables, we would also prefer to leave the p-values for linear trend in tables 2 and 3.

Also looking at the results I wonder if the mechanisms or underlying characteristics behind never drinkers and ex-drinkers among individuals with low MWB and those with high MWB might differ, do the authors have any thoughts on this?
Reply: Never drinkers are likely to be different from ex-drinkers. There will be cultural, social and/or religious reasons for never drinking, but ex-drinking is more likely to be a-health related behaviour. The mechanisms linking these behaviours with low or high mental wellbeing are uncertain; we feel potential explanation would be overly speculative at this time.

Most health behaviours are patterned by social determinants and many of these determinants as well as health-behaviors are strongly correlated with one another e.g. education, income, and employment, smoking and alcohol – perhaps the non-significant results in model 3 reflect some sort of over- adjustment? I suggest the authors perhaps consider this aspect e.g. in the discussion. Causal graphs (Directed Acyclic Graphs) are increasingly used to address this issue see e.g. Pearl J. Causality: models, reasoning and inference. Cambridge: Cambridge University Press, 2000.
Reply: the reviewer is right; many of these social determinants as well as health-behaviours are strongly correlated. We took the above fact into account in our model building. Our strategy consisted in showing first the independent relationship of each individual health-behaviour (what we termed Model 1: unadjusted relationship) then moving to partial adjustment (Model 2), which consisted of excluding the effects of age and sex. Finally, we made the full adjustment in Model 3 to resolve the issue of multiple confounders. By showing and moving from the independent relationship to the partial and full adjustment, we believe that the readers would appreciate magnitude of each health-behaviour. We also agree with the reviewer that causal graphs can be used to address the above issue. However, in our case, causal graphs were not required, because we were able to address this issue with straightforward methods such as correlation and use of co-linearity test to exclude correlated determinants to come up with a model which is parsimonious.

Conclusion: I am aware of the strong associations between mental health and health behavior. I also understand how e.g. physical activity might influence well-being, or how smoking due to withdrawal symptoms between cigarettes might affect well-being. Yet how a high fruit and veg intake may result in high MWB in itself is not clear to me unless it is due to a correlation e.g. with physical activity (as the authors themselves touch upon) or reverse causality. The authors point to a few recent studies in the discussion that suggest a causal pathway between fruit and veg intake and well-being, yet these studies may similarly be affected by problems related to confounding and reverse causality. I would be interested to hear more about the authors' thoughts of the underlying mechanism between fruit and veg intake and mental well-being.

Also I know that a high intake of fruit and veg is recommended yet there is to date no systematic evidence to rule out with certainty that accumulative pesticide and herbicide residue is not harmful although the doses are small – This is merely a personal reflection.

The authors point themselves to limitations of the study related to causality - I would soften the conclusion – perhaps rather write that fruit and vegetable consumption is the health-related behaviour most consistently associated with low and high mental wellbeing and may play a potential as a driver not just of physical but also of mental wellbeing in the general population – future studies should be carried out to address this topic. It is a very interesting study regardless of findings related to fruit and veg as the other findings support the notion that results related to low MWB and high MWB do not merely mirror each other, and that mental health and well-being should not be considered a continuum.

Reply: We agree with the reviewer on these issues. The highly correlated nature of the variables included and the real possibility of reverse causality in cross sectional studies preclude firm conclusions about this. However the most consistent associations with wellbeing appear to be with fruit and vegetable consumption and it is possible that there is a causal link although as the reviewer points out there is not an obvious mechanism. Hence, following the reviewer's suggestion, we have included a statement in the discussion to acknowledge this issue, and have softened our concluding remarks in the revised manuscript.

Minor comments:
Under the method section health behavior it says Data on physical activity related were not collected in either the 2010 or 2011 surveys it seems the word related should be deleted.
There is a "d" missing in table 2 model 1 in the word unadjusted. Also there is part of a number missing in model 2, same table, under the result for <1 portion/day fruit and veg.
Reply: Thank you for pointing out these errors – We have made the corrections.

RESPONSE TO REVIEWER 2
As the authors correctly note, "mental wellbeing now assumes an important place in mental health and public health policy." Although it remains an important public health issue, the evidence on the behavioral correlates of mental wellbeing is limited. Psychiatric pathology has an enormous population health impact. Therefore increasing our understanding of the role of mental wellbeing in behavioral health is critically important for public health. The correlational analysis is based on a large nationally representative (although sampling procedure were not included) database with extensive information on lifestyle habits. The primary aim involves association between behavioral risk factors and mental wellbeing. Overall, the study is well conceived, with a clear aim. The findings shed important light on our understanding of the health behavior correlates of mental health and may contribute to future clinical and public health interventions.
Reply: Thank you for very kind remarks about our manuscript.

The biggest concerns involve the absence of a physical activity measure.
Reply: We have acknowledged this as a limitation of our study.

Also, the authors should discuss how fruit and vegetable consumption might also represent a complex

set of highly correlated dietary exposures, including fish, whole grains, etc.
Reply: We have addressed this in our responses to reviewer one, and have included a statement in the discussion to acknowledge this issue.

In terms of the methodology, because of the absence of well-established cut-points (e.g., caseness), the authors might want to use the continuous version of the WEMWBS. However, if they use a categorical outcome, I recommend using a multinomial regression rather than two logistic models or adjust for multiple comparisons.
Reply: The reviewer makes an important methodological point here. The use of continuous version of the WEMWBS is methodologically sound compared to its categorization especially in the absence of well-established cut-points. This later approach, we agree with the reviewer, suffers from loss of information. However, we chose the binary outcome instead of the continuous WEMWBS values, due to interpretability reasons since with the binary outcome one can estimate the likelihood of mental well-being in the sample population, while accounting for a number of potential covariates. Again, the reviewer is right in mentioning that an alternative to the binary logistic regression we used here is a multinomial logistic regression. However, we prefer the use of ordinary logistic regression because the multinomial regression will change the original research questions to a very different one. One problem with using logistic regression instead of multinomial regression is that each analysis is potentially run on a different sample. The other problem is that without constraining the logistic models, we can end up with the probability of choosing all possible outcome categories greater than 1. However, this is not the case in our sample. To avoid the above drawbacks, we have a priori stratified our analysis in clearly defined and distinct groups: low mental well-being as compared to middle-range mental well-being and high well-being as compared to middle-range well-being.

Apparently, income should include a "missing" category in the model.
Reply: Yes, that is correct.
The manuscript has a few typos and the use of the term "average" rather that "middle range."
Reply: Thank you for pointing out these errors – We have made the corrections.
Finally, here are some recent review articles that could be cited in the study.
Reply: We have included a few of the suggested references (i.e. those concerning psychological well-being rather than mental illness) in the revised manuscript.

VERSION 2 – REVIEW

REVIEWER	Vibeke Koushede The Danish National Institute of Public Health University of Southern Denmark Denmark
REVIEW RETURNED	05-Aug-2014

GENERAL COMMENTS	Thank you for your thorough revisions and comments. Congratulations on an interesting, relevant and nicely written paper.