PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Work and non-work stressors, psychological distress and obesity: Evidence from a 14-year study on Canadian workers
AUTHORS	Blanc, Marie-Eve; Marchand, Alain; Beauregard, Nancy

VERSION 1 - REVIEW

REVIEWER	Wei-Qing Chen Department of Biosatistics and Epidemiology School of Public health Sun Yat-sen Univeristy Guangzhou China
REVIEW RETURNED	02-Sep-2014

GENERAL COMMENTS	(1) Generally, the manuscript is a little longer and will be compressed. The title did not well match the contents in Discussion. The title mainly focused on the mediation role, but the most contents of Discussion were spent on the associations between the significant variables and obesity. (2) Theoretically, both mental health and obesity are effects of work stress, the former is acute effect and the latter is chronic effects. Although, some evidences indicated that association between mental health and obesity, is what its biological mechanisms? In this paper, the biological mechanism has not been introduced and discussed. (3) In statistics, the final models in this paper were not the best model, and there are many insignificant variables included in the model, which may be over adjusted for and decrease the precision of estimation. This limitation will be addressed in Discussion. (4) Although associations between several characteristics and obesity were statistically significant, the strength of these associations was low (from 0.85-1.56), thus, and the causal relationship between them can not determined. This limitation will be also addressed. (5) To help readers to read and understand the results more clearly, correlations between different work, non-work characteristics and mental health will be presented, which may help to observe multicollinearity.

REVIEWER	Reiner Rugulies National Research Centre of the Working Environment, Copenhagen, Denmark
REVIEW RETURNED	22-Oct-2014

GENERAL COMMENTS	Point #2: I answered "no", because the conclusion in the abstract "preventing obesity in the workplace requires monitoring the level of decision authority" (because it increases work stress) to which women must adapt" is not supported by the results. The results do not show that low decision authority increases work stress, so the parenthesis seems to be misplaced. The results show an association of low decision authority with obesity among women, when other risk factors were adjusted. But also in this case, it remains unclear why monitoring should be an effective intervention and should be recommended. Further, in the conclusion in the abstract the authors write that psychological distress “seems to promote the experience of obesity”, which is a rather awkward wording. I suggest to simply write “it predicted risk of obesity”. Point #7: I answered “no”, because for examining mediation the authors need to establish that the predictor variables (work factors, non-work factors, individual factors) not only predict the outcome, but also predict the mediator. This is not done. Point #11: I answered “no”, partly for the reason explained in point #2 above. In addition, I would like to ask the authors to be more cautious with terms (such as “effect”) that seem to indicate that this study has established clear causal relations. After all, this is an observational study, so caution regarding unmeasured confounding and other biases affecting causal inference is needed. In your paper, you examine whether poor mental health, measured by a scale on psychological distress, mediates the association of various work- and non-work-related factors with risk of obesity. You found that most work and non-work related factors did not predict obesity. Psychological distress also did not predict obesity when stratified by sex, but did predict obesity in the non-stratified sample. There were no indications that psychological distress might have mediated the association of work- and non-work-related factors with obesity. The strengths of your study are the national sample, the repeated measurements and the focus on both work and non-work-related factors and also individual factors in the aetiology of obesity. This said, I have the following comments on your paper: GENERAL 1) In the title you address mental health as a potential mediator, but in your study you call your mediator psychological distress. I suggest that your are consistent and use the term psychological distress also in the title. Introduction: 2) The introduction is rather long and discusses several previous studies on risk factors for obesity rather detailed. Try to summarize the existing knowledge in a more concise way.

3) Page 8: It seems that there are only very few prospective observational studies that have examined whether mental health problems predict obesity and whether obesity predicts mental health problems (your references #19, #32, #33). Thus, your conclusion that “a causal relationship between mental health problems and obesity, both direct and inverse, is emerging from the literature” seems to be an exaggeration. Also, the expression “direct and inverse” is confusing as “inverse” is not the antonym to “direct”. I suppose you mean that there is increasing evidence that mental health problems predict obesity and that also obesity predicts mental health problems?

4) You write “Knowledge gained so far clearly indicates that psychosocial risks arising in both work and non-work living environments contribute to the development of mental health problems”. “Clearly indicates” is a strong statement, please back this up with more references, including from the psychiatric literature. Right now, you are only providing a reference to your own work. With regard to the contribution of workplace factors to risk of mental health, I would ask you to be more cautious. Whether or not work contributes to poor mental health and to psychiatric disorders is discussed controversially in the literature, which is partly due to the methodological challenges (in particular the challenge of reporting bias). This needs to be addressed here.

METHODS

5) Page 10, you write: “Recent research claimed to view mental health as a continuum”. No, this is not this simple. It has actually been controversially discussed, over a long time, to what extent mental health in general and specific mental health problems, such as depressive symptoms, should be viewed on a continuum or not. So, a more differentiated view on the discussion in the literature is needed.

6) In your description of the workplace factors, it is not clear whether you used the different factors (skill utilization, decision authority etc.) as continuous variables or as categorical variables. Please make this clear.

7) Data analysis. Because your aim is to examine whether the prospective association of various factors with risk of obesity was mediated by psychological distress, I had expected that you would analyse whether the factors predict psychological distress (which is necessary, otherwise psychological distress could not be a mediator). But I could not find these analyses. Please add them to the paper.

RESULTS

8) You present your results first stratified by sex and then for men and women combined. Why? Either you believe it makes sense to stratify by sex, then you should state your rational for doing so and should stick with this (i.e. not conducting the analyses with men or women combined) or you believe that it makes most sense to analyse men and women in a combined sample, while adjusting for sex, then do this and stick with this.

9) You focus too much on statistical significance, which is, of course, strongly influenced by sample size. Take for example psychological distress. The point estimate for men (table 2), for women (table 3) and both sexes combined (table 4) is very similar and also the

	confidence intervals are very similar. It just happens that the confidence intervals are a little bit more narrow and do not include unity in the combined analyses, whereas in the sex-stratified analyses they are a little bit wider and just include unity (with a lower confidence interval bound of 0.99). Seems to me that the association of distress with obesity is the same in all three analyses and the difference in statistical significance is driven by sample size. 10) It seems to me that all variables in the tables are adjusted for each other. Is this correct? If this is so, that means that you have adjusted the different workplace factors for each other. Please discuss in the discussion section the advantages and disadvantages of doing so. For example, I find it problematic to adjust psychological demands for working hours, since long working hours are probably an indicator for high psychological demands. 11) Why did you analyse psychological demands, decision authority, skill utilization and social support separately and why did you not combine them into measures of job strain and iso-strain? Please explain. DISCUSSION 12) In the discussion section you repeat your most important result and relate them to the literature. This is fine. However, I would like to see a more elaborated discussion why you believe that your core hypotheses were rejected in this paper. In the introduction you wrote that there is emerging support for a causal association of poor mental health and obesity. However, in your study, the association seems to be rather weak. This is an unexpected finding, any explanations? Also, contrary to your hypothesis psychological distress was not a mediator. Again, what is your explanation for this unexpected finding? 13) Regarding limitations of your study. All variables in your study, exposures, endpoints and the potential mediator were self-reported. Please discuss critically the advantages and disadvantages of self-reported data, in particular the issue of reporting bias when studying the association of self-reported work environment factors with a measure of self-reported psychological health. 14) A quite surprising result of your study was that a higher level of physical excercise was associated with an increased risk of obesity. Very puzzling. Any explanations? You write: "Since we did not, however, estimate any survival model (e.g., from studies of incident cases) it could be that this is actually an instance of inverse causality." I do not understand this. As I had understood you had excluded everyone with obesity at baseline and you are analysing the prospective association of various predictors plus the mediator psychological distress with new caseness of obesity. As predictors and the mediator are assessed among non-obesed participants, I do not see the case for reverse causation. But if there is the possibility of reverse causation, i.e. that obesity has affected the level of physical activity, then you have a severe problem in your study. Because then, it would also be possible that obesity has affected the other predictors, for example decision authority. Please clarify.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name Wei-Qing Chen

Thank you very much Dr. Wei-Qing for your helpful review.

This paper aimed to evaluate whether mental health mediated the work stress causing obesity. In this study, a representative longitudinal data was used to solve this assumption, suitable scales were correctly used. But, no strong biological mechanisms support this hypothesis, and the paper does not add any new knowledge in this field.

(1) Generally, the manuscript is a little longer and will be compressed. The title did not well match the contents in Discussion. The title mainly focused on the mediation role, but the most contents of Discussion were spent on the associations between the significant variables and obesity.

The title has been modified according to this comment. The revised manuscript has also been compressed.

(2) Theoretically, both mental health and obesity are effects of work stress, the former is acute effect and the latter is chronic effects. Although, some evidence indicated that association between mental health and obesity, is what its biological mechanisms? In this paper, the biological mechanism has not been introduced and discussed.

This study is about social mechanisms linking work and non-work stressors, psychological distress and obesity. Therefore, the biological mechanism was not an element under review here. We concede that psychobiological pathways triggered by social stressors may be at play. For instance, Gariepy and al. (2010) cited in our revised version noted in their systematic review of anxiety disorders and obesity that dysregulation in the HPA-axis, a known indicator for the stress mechanism in the human body, may lead to an increase in appetite and weight gain. Nevertheless, our study with its social epidemiological take on obesity and mental health has the unique advantage of relying on a large nationally representative sample of Canadian workers. The unique design and target of the NPHS cohort emphasized population health determinants measured by self-reports rather than objective biological markers of health outcomes.

(3) In statistics, the final models in this paper were not the best model, and there are many insignificant variables included in the model, which may be over adjusted for and decrease the precision of estimation. This limitation will be addressed in Discussion.

The main objective of our statistical analyses was to provide a strong adjustment of variables in the models. Considering the sample size, power was not an issue. We however address this comment at p.22 of the revised manuscript

(4) Although associations between several characteristics and obesity were statistically significant, the strength of these associations was low (from 0.85-1.56), thus, and the causal relationship between them can not determined. This limitation will be also addressed.

Please refer to the comment at p.22 of the revised manuscript

(5) To help readers to read and understand the results more clearly, correlations between different

work, non-work characteristics and mental health will be presented, which may help to observe multicollinearity.

A correlation matrix is now presented in the revised manuscript

Again, thank you very much Dr. Wei-Qing for your helpful review.

Reviewer: 2
Reviewer Name Reiner Rugulies

Thank you very much Dr. Reiner Rugulies for your helpful review

Point #2: I answered "no", because the conclusion in the abstract "preventing obesity in the workplace requires monitoring the level of decision authority" (because it increases work stress) to which women must adapt" is not supported by the results. The results do not show that low decision authority increases work stress, so the parenthesis seems to be misplaced. The results show an association of low decision authority with obesity among women, when other risk factors were adjusted. But also in this case, it remains unclear why monitoring should be an effective intervention and should be recommended.

The abstract has been modified in this revision of the manuscript

Further, in the conclusion in the abstract the authors write that psychological distress “seems to promote the experience of obesity”, which is a rather awkward wording. I suggest to simply write “it predicted risk of obesity”.

The abstract has been modified according to this comment

Point #7: I answered “no”, because for examining mediation the authors need to establish that the predictor variables (work factors, non-work factors, individual factors) not only predict the outcome, but also predict the mediator. This is not done.

We have provided and discussed a correlation matrix in the revised manuscript

Point #11: I answered “no”, partly for the reason explained in point #2 above. In addition, I would like to ask the authors to be more cautious with terms (such as “effect”) that seem to indicate that this study has established clear causal relations. After all, this is an observational study, so caution regarding unmeasured confounding and other biases affecting causal inference is needed.

The manuscript have been revised to be more cautious when describing the relationships between variables

I recommend that the statistical analyses are checked by a biostatistician.

Dear Authors,
In your paper, you examine whether poor mental health, measured by a scale on psychological distress, mediates the association of various work- and non-work-related factors with risk of obesity. You found that most work and non-work related factors did not predict obesity.

Psychological distress also did not predict obesity when stratified by sex, but did predict obesity in the non-stratified sample. There were no indications that psychological distress might have mediated the association of work- and non-work-related factors with obesity.

The strengths of your study are the national sample, the repeated measurements and the focus on both work and non-work-related factors and also individual factors in the aetiology of obesity. This said, I have the following comments on your paper:

GENERAL
1) In the title you address mental health as a potential mediator, but in your study you call your mediator psychological distress. I suggest that your are consistent and use the term psychological distress also in the title.

The title has been modified according to this comment

Introduction:
2) The introduction is rather long and discusses several previous studies on risk factors for obesity rather detailed. Try to summarize the existing knowledge in a more concise way.

The introduction has been revised to make it more concise.

3) Page 8: It seems that there are only very few prospective observational studies that have examined whether mental health problems predict obesity and whether obesity predicts mental health problems (your references #19, #32, #33). Thus, your conclusion that “a causal relationship between mental health problems and obesity, both direct and inverse, is emerging from the literature” seems to be an exaggeration. Also, the expression “direct and inverse” is confusing as “inverse” is not the antonym to “direct”. I suppose you mean that there is increasing evidence that mental health problems predict obesity and that also obesity predicts mental health problems?

This sentence was confusing. The revised manuscript has been modified according to your suggestion. Change appears at p.7

4) You write “Knowledge gained so far clearly indicates that psychosocial risks arising in both work and non-work living environments contribute to the development of mental health problems”. “Clearly indicates” is a strong statement, please back this up with more references, including from the psychiatric literature. Right now, you are only providing a reference to your own work. With regard to the contribution of workplace factors to risk of mental health, I would ask you to be more cautious. Whether or not work contributes to poor mental health and to psychiatric disorders is discussed controversially in the literature, which is partly due to the methodological challenges (in particular the challenge of reporting bias). This needs to be addressed here.

The sentence has been modified and supported with a recent study. Changes appear at p.8 of the revised manuscript

METHODS
5) Page 10, you write: “Recent research claimed to view mental health as a continuum”. No, this is not this simple. It has actually been controversially discussed, over a long time, to what extent mental health in general and specific mental health problems, such as depressive symptoms, should be viewed on a continuum or not. So, a more differentiated view on the discussion in the literature is needed.

This is an important comment. Our view on psychological distress as a prepathological expression of mental health problems in the workforce is in line with that of Wheaton (2007). This is position is substantiated by conceptual and methodological grounds. Conceptually, psychological distress is conceived as a pre-pathological mental state to be distinguished from clinical psychopathology (i.e., severe depression, bipolar disorders, etc.)(Dorenwhend et al., 1980). Caseness on psychopathology, as argued by proponents like Wheaton, is a binary reality. Alternatively, conceiving psychological distress as a continuum of non-specific pre-pathological symptoms allows us to assess the full variation in the working population psychological distress can take in association with the risk for obesity. Methodologically, we sought to expand our appreciation of the distress-obesity association beyond caseness on both outcomes, which would by convention be equated to the upper quintile of the distribution of psychological distress. As we mentioned in our discussion, some studies have successfully found a risk-pattern association between psychopathology (e.g., depression) and obesity, whereas our results are more mitigated in that sense. Nevertheless, we felt here that an important contribution needed to be made by investigating psychological distress as a continuum before moving on, in future studies, to a more targeted part of its distribution (upper quintile associated with heightened symptoms).

We succinctly referred to this line of reasoning in the text to clarify to our readership our position.

Dohrenwend, B.P., Shrout, P.E., Egri, G., & Mendelsohn, F.S. (1980). Nonspecific psychological distress and other dimensions of psychopathology. Archives of General Psychiatry, 37, 1229-1236.

Wheaton, Blair. (2007). The twain meet: distress, disorder and the continuing conundrum of categories (comment on Horwitz). Health (London, England : 1997), 11(3), 303-319.

Changes appear at p.9 of the revised manuscript

6) In your description of the workplace factors, it is not clear whether you used the different factors (skill utilization, decision authority etc.) as continuous variables or as categorical variables. Please make this clear.

Variables are used as continuous. We have added this precision at p.9 of the revised manuscript.

7) Data analysis. Because your aim is to examine whether the prospective association of various factors with risk of obesity was mediated by psychological distress, I had expected that you would analyse whether the factors predict psychological distress (which is necessary, otherwise psychological distress could not be a mediator). But I could not find these analyses. Please add them to the paper.

A correlation matrix is now added and discussed at p.9 of to the revised manuscript.

RESULTS

8) You present your results first stratified by sex and then for men and women combined. Why? Either you believe it makes sense to stratify by sex, then you should state your rational for doing so and should stick with this (i.e. not conducting the analyses with men or women combined) or you believe that it makes most sense to analyse men and women in a combined sample, while adjusting for sex, then do this and stick with this.

Combined and stratified analyses are there to examine if results are the same for both sexes. However, the way we presented the results may have leaded to some confusion. In this revision of the manuscript, the results section has been modified to present first the combined analysis, and then

stratified by sex. The analysis sequence is presented at pp.10-11 and the results section is modified accordingly.

9) You focus too much on statistical significance, which is, of course, strongly influenced by sample size. Take for example psychological distress. The point estimate for men (table 2), for women (table 3) and both sexes combined (table 4) is very similar and also the confidence intervals are very similar. It just happens that the confidence intervals are a little bit more narrow and do not include unity in the combined analyses, whereas in the sex-stratified analyses they are a little bit wider and just include unity (with a lower confidence interval bound of 0.99). Seems to me that the association of distress with obesity is the same in all three analyses and the difference in statistical significance is driven by sample size.

You are right, and we added in the revision the relationship between psychological distress and obesity was not moderated by gender. Change appears at p.17 and p.21 of the revised manuscript.

10) It seems to me that all variables in the tables are adjusted for each other. Is this correct? If this is so, that means that you have adjusted the different workplace factors for each other. Please discuss in the discussion section the advantages and disadvantages of doing so.
For example, I find it problematic to adjust psychological demands for working hours, since long working hours are probably an indicator for high psychological demands.

You are right and it's a standard approach we have used in many of our previous studies. We are a little bit surprised by this comment, because we are interested in obtaining net effects of variables in models. How one can be sure that skill utilisation associate with obesity if the effect of psychological demands is not adjusted for? By considering correlations between work variables, we thus end up with specific effects, and we do not see the relevance of discussing this in the discussion as it is a standard procedure.

11) Why did you analyse psychological demands, decision authority, skill utilization and social support separately and why did you not combine them into measures of job strain and iso-strain? Please explain.

Simply because the interactions between control-demands or control-demands-support are rarely empirically supported. We have examined this question in depth in the past with the same NPHS cohort in relation with psychological distress (for instance, Marchand et al., 2005) and could not find any synergetic effect between work factors on psychological distress. These past findings guided our choice to directly test here for a mediation effect of psychological distress on the work factors-obesity association using a continuous distribution of the work factors. We agree though with your comment as emphasized by our literature review (see p.5) that synergetic effects of work factors on obesity were evidenced elsewhere, but these studies only tested a direct pathway between the work environment and obesity.

Marchand, A., Demers, A., & Durand, P. (2005). Do occupation and work conditions really matter? A longitudinal analysis of psychological distress experiences among Canadian workers. Sociology of Health and Illness, 27(5), 602-627.

DISCUSSION
12) In the discussion section you repeat your most important result and relate them to the literature. This is fine. However, I would like to see a more elaborated discussion why you believe that your core hypotheses were rejected in this paper. In the introduction you wrote that there is emerging support for a causal association of poor mental health and obesity. However, in your study, the association

seems to be rather weak. This is an unexpected finding, any explanations? Also, contrary to your hypothesis psychological distress was not a mediator. Again, what is your explanation for this unexpected finding?

Throughout the discussion, we have provided more explanation. Additions specific to psychological distress appear at p.21 and p.22 of the revised manuscript

13) Regarding limitations of your study. All variables in your study, exposures, endpoints and the potential mediator were self-reported. Please discuss critically the advantages and disadvantages of self-reported data, in particular the issue of reporting bias when studying the association of self-reported work environment factors with a measure of self-reported psychological health.

We have added a discussion of limits yielded by self-reported measures. Changes appear at p.22 of the revised manuscript

14) A quite surprising result of your study was that a higher level of physical excercise was associated with an increased risk of obesity. Very puzzling. Any explanations? You write: "Since we did not, however, estimate any survival model (e.g., from studies of incident cases) it could be that this is actually an instance of inverse causality." I do not understand this. As I had understood you had excluded everyone with obesity at baseline and you are analysing the prospective association of various predictors plus the mediator psychological distress with new caseness of obesity. As predictors and the mediator are assessed among non-obesed participants, I do not see the case for reverse causation. But if there is the possibility of reverse causation, i.e. that obesity has affected the level of physical activity, then you have a severe problem in your study. Because then, it would also be possible that obesity has affected the other predictors, for example decision authority. Please clarify.

We have to apologise here because since the revision of the manuscript, we have identified a problem in an Excel file we use to convert regression coefficients to odds ratios with their 95%CI corrected for design effects. Only continuous variables were affected. Results have been modified accordingly in the revised manuscript. We have now physical activity, psychological distress and skill utilization negatively related to obesity risk.

Again, thank you very much Dr. Reiner Rugulies for your helpful review.

VERSION 2 – REVIEW

REVIEWER	Reiner Rugulies National Research Centre for the Working Environment, Denmark
REVIEW RETURNED	13-Dec-2014

GENERAL COMMENTS	Checklist: Point 2: No. See my comments #2 and #3 below. Point 11: No. See my comments #1 and #6 below. Point 15: No. See my comment #11 below. Dear Authors, You replied well and comprehensively to my remarks. However, the changes you made evoke new questions: 1) The biggest chance you made was not one that was requested by the reviewers, but a change you made because you “identified a problem in an Excel file”. In the first submission you found that a)

high psychological distress was associated with an increase in obesity risk (as hypothesized) and that high physical activity was associated with an increased risk (quite surprising). Now, after you identified some problems in the data and have corrected them, you report that high psychological distress is associated with a decreased risk of obesity (surprising) and that high physical activity is associated with a decreased risk (as one would expect).

I am glad to see that high physical activity no longer appears as a risk factor of obesity in your study, this finding has puzzled me quite a bit. But now we have the rather strange result that psychological distress no longer predicts an increased risk of obesity, but, to the contrary, it predicts a decreased risk of obesity. In other words a high level of psychological distress seems to be protective against becoming obese. A strange and interesting result. And contrary to your assumption that psychological distress might be a meditator for the living conditions – obesity association. Because for being a mediator, psychological distress needs to be a risk factor for obesity. But it is not in your study.

So, the surprising result regarding psychological distress needs to be discussed comprehensively in your discussion section. But you are not doing this. Why? You need to make clear in the discussion that because of the apparent protective effect of high psychological distress with regard to obesity, you hypothesis of mediation is clearly refuted. And you need to discuss why and through which mechanism high psychological distress might protect against obesity. And also, how do you explain the puzzling finding that one measure of stress (child-related stress or child-related strain, you use both expressions in the paper) predicts obesity, whereas psychological distress (which likely has something to do with stress) protects against obesity. How do you explain this? Or might this even indicate that there is still something wrong with the data?

Assuming that everything is fine with the data, I would like to ask you to thoroughly discuss the unexpected finding on psychological distress and obesity risk in the discussion section of your paper.

ABSTRACT
2) In the abstract you write in the result section: “Living as a couple, child-related stress, psychotropic drug use, hypertension and physical activities were obesity risk factors”. Shouldn’t it be “being physical inactive”, instead of “physical activities”? And you should mention under results also that psychological distress seems to protect against obesity.

3) In the abstract under conclusion you write: “In addition, although psychological distress is not mediating the contribution of work factors, it seems to decrease the obesity risk”. Consider to reword this:” In addition, psychological distress did not mediate the contribution of work factors and actually seems, contrary to expectation, to decrease the obesity risk”.

RESULTS
4) In table 3 (both genders combined), psychological distress is not associated with obesity when adjusting for individual variables (model 2), but is associated with decreased risk of obesity after further adjustment for work and non-work factors. In the light of the unexpected protective effect of psychological distress, it would be

	interesting to know, how psychological distress is associated with risk of obesity, when only adjusted for age and sex. Please provide this information in the text. 5) Page 45: line 14, you write with regard to the analyses among women: “Model 2 shows that psychological distress is not statistically significant and does not modify the associations with work factors”. Instead of “modify” (which would indicate interaction), I assume you wanted to say “mediate”? Please check the manuscript, if you have mixed up the terms also at other places. DISCUSSION 6) As said above, you need to more clearly address the surprising result regarding the apparent protective effect of psychological distress. 7) Page 48, you write: “This may be a sign that decision authority makes women more active in their job required them to stay in good shape to coping with challenges coming from the workplace.” This might be an explanation, another explanation is that high decision authority can be also regarded as an indicator of high socioeconomic position. And socioeconomic position is usually clearly inversely related to obesity. Please discuss this. 8) Page 50, lines 34-38. You write: “However, as psychological distress and work and non-work variables were reported, some reverse causality might have biases the association with psychological distress and thus could explain why we failed to find significant psychological distress mediation”. No, I disagree. You never had a chance to find mediation by psychological distress, because psychological distress was not risk factor for obesity in your study. GENERAL REMARKS 9) You are using psychological distress and mental health interchangeable at some parts of the manuscript. This might confuse readers, therefore consider using only one term. 10) You use both the terms “child-related strain” and “child-related stress”. Since stress and strain are used as different concepts in several stress theories, I suggest not to mix up the terms in your paper. 11) It seems that the newly added text (the text marked in yellow) has not the same standard of academic English as the other parts.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 2
Reviewer Name Reiner Rugulies
Institution and Country National Research Centre for the Working Environment, Denmark
Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below
Checklist:
Point 2: No. See my comments #2 and #3 below.
Point 11: No. See my comments #1 and #6 below.
Point 15: No. See my comment #11 below.

Dear Authors,

You replied well and comprehensively to my remarks. However, the changes you made evoke new questions:

1) The biggest chance you made was not one that was requested by the reviewers, but a change you made because you "identified a problem in an Excel file". In the first submission you found that a) high psychological distress was associated with an increase in obesity risk (as hypothesized) and that high physical activity was associated with an increased risk (quite surprising). Now, after you identified some problems in the data and have corrected them, you report that high psychological distress is associated with a decreased risk of obesity (surprising) and that high physical activity is associated with a decreased risk (as one would expect).

I am glad to see that high physical activity no longer appears as a risk factor of obesity in your study, this finding has puzzled me quite a bit. But now we have the rather strange result that psychological distress no longer predicts an increased risk of obesity, but, to the contrary, it predicts a decreased risk of obesity. In other words a high level of psychological distress seems to be protective against becoming obese. A strange and interesting result. And contrary to your assumption that psychological distress might be a meditator for the living conditions – obesity association. Because for being a mediator, psychological distress needs to be a risk factor for obesity. But it is not in your study.

So, the surprising result regarding psychological distress needs to be discussed comprehensively in your discussion section. But you are not doing this. Why? You need to make clear in the discussion that because of the apparent protective effect of high psychological distress with regard to obesity, you hypothesis of mediation is clearly refuted. And you need to discuss why and through which mechanism high psychological distress might protect against obesity. And also, how do you explain the puzzling finding that one measure of stress (child-related stress or child-related strain, you use both expressions in the paper) predicts obesity, whereas psychological distress (which likely has something to do with stress) protects against obesity. How do you explain this? Or might this even indicate that there is still something wrong with the data?

Assuming that everything is fine with the data, I would like to ask you to thoroughly discuss the unexpected finding on psychological distress and obesity risk in the discussion section of your paper.

We would like to cordially thank again Dr.Rugulies for his kind help and informative comments on our manuscript.

The multilevel regression results obtain with the Mlwin software were valid to begin with, it is only with

the transfer to *xls file that an error was detected and promptly corrected. Results reported in our Tables now were verified and are trustworthy.

We would like to address now the substantive comments that were raised by incidental changes in results limited only the continuous variables in our models. First, the directionality of the association between psychological distress and obesity was indeed discussed at p. 21 of our manuscript (last paragraph beginning with “Finally”). Our interpretation of psychological distress emerging as a protective factor for obesity was based on cumulative knowledge reported by five separate systematic reviews. Two possible explanations were highlighted for future research. Documenting the causal pathways between mental health and obesity using different stages in psychopathology (prepathological indicators such as distress, pathological indicators such as depression), remains a pivotal issue. Unfortunately, no meta-analyses were found that could support a quantitative evaluation of the magnitude of the causal association between different stages in psychopathology (psychological distress, depression) and obesity in community samples. That being said, we expanded our rationale exploring biological pathways between psychopathology and obesity which might add further guidance as to why more severe mental health problems might better associate with obesity relative to milder afflictions such as psychological distress (p.22). We also reported on results from a 2014 meta-analysis showing an inverse relation between distress and obesity in a sample of cross-sectional studies. The second explanation provided pertained to the specific temporal sequence examined in the paper between psychological distress and obesity. Indeed, a reverse association where obesity predicted psychological distress could have been at play. Again, we provided substantive grounds to account for that possibility which exceeded though the scope of the paper.

Addressing the comment made with regards to the child-related strains emerging as risk factors for obesity, we further expanded on the interpretation of that result on p. 20. Also, we now use the expression child-related strains throughout the manuscript

ABSTRACT

2) In the abstract you write in the result section: “Living as a couple, child-related stress, psychotropic drug use, hypertension and physical activities were obesity risk factors”. Shouldn’t it be “being physical inactive”, instead of “physical activities”? And you should mention under results also that psychological distress seems to protect against obesity.

The abstract has been modified writing now: being physical inactive

3) In the abstract under conclusion you write: “In addition, although psychological distress is not mediating the contribution of work factors, it seems to decrease the obesity risk”. Consider to reword this:” In addition, psychological distress did not mediate the contribution of work factors and actually seems, contrary to expectation, to decrease the obesity risk”.

The abstract has been modified according to your suggestion.

RESULTS

4) In table 3 (both genders combined), psychological distress is not associated with obesity when adjusting for individual variables (model 2), but is associated with decreased risk of obesity after further adjustment for work and non-work factors. In the light of the unexpected protective effect of psychological distress, it would be interesting to know, how psychological distress is associated with risk of obesity, when only adjusted for age and sex. Please provide this information in the text.

We have run an analysis to evaluate how psychological is associated with obesity when only adjusted

for age and gender. The results reveal that it is not associated with obesity when only adjusted for gender and age (OR=1.01 95%CI=0.99-1.04). This analysis is now added at p.14 of the revised manuscript.

5) Page 45: line 14, you write with regard to the analyses among women: “Model 2 shows that psychological distress is not statistically significant and does not modify the associations with work factors”. Instead of “modify” (which would indicate interaction), I assume you wanted to say “mediate”? Please check the manuscript, if you have mixed up the terms also at other places.

Correction done at p.17 of this revision. The overall manuscript has been checked according to your comment.

DISCUSSION

6) As said above, you need to more clearly address the surprising result regarding the apparent protective effect of psychological distress.

Correction done at p.21 of this revision.

7) Page 48, you write: “This may be a sign that decision authority makes women more active in their job required them to stay in good shape to coping with challenges coming from the workplace.” This might be an explanation, another explanation is that high decision authority can be also regarded as an indicator of high socioeconomic position. And socioeconomic position is usually clearly inversely related to obesity. Please discuss this.

We have integrated this possible other explanation. Change appears at p.20 of the revised manuscript

8) Page 50, lines 34-38. You write: “However, as psychological distress and work and non-work variables were reported, some reverse causality might have biases the association with psychological distress and thus could explain why we failed to find significant psychological distress mediation”. No, I disagree. You never had a chance to find mediation by psychological distress, because psychological distress was not risk factor for obesity in your study.

You are right, and we have deleted this sentence in the revised manuscript.

GENERAL REMARKS

9) You are using psychological distress and mental health interchangeable at some parts of the manuscript. This might confuse readers, therefore consider using only one term.

We are uncomfortable and thus in disagreement with this comment. Our study is based on psychological distress as an indicator of mental health. In the literature on obesity and mental health, one could find indicators like common mental disorders (Kivimaki et al, 2009), mood disorders (McElroy et al., 2004), anxiety disorders (Gariepy et al, 2010), generalized anxiety(Block et al., 2010), anxiety-depression (Nishitani et Sakakibara, 2007), and so on. Therefore, using the term mental health helps in grouping different manifestation of mental health symptoms under a general terminology. When we refer to our own study, we use psychological distress.

10) You use both the terms “child-related strain” and “child-related stress”. Since stress and strain are used as different concepts in several stress theories, I suggest not to mix up the terms in your paper.

We now use the expression child-related strains throughout the manuscript

11) It seems that the newly added text (the text marked in yellow) has not the same standard of academic English as the other parts.

The overall manuscript has been revised by a native English speaking colleague

VERSION 2 – REVIEW

REVIEWER	Reiner Rugulies National Research Centre for the Working Environment, Denmark
REVIEW RETURNED	01-Feb-2015

GENERAL COMMENTS	Abstract, result section 1) First sentence: Write “decision authority was associated” instead of “decision authority associated” 2) Add the finding that high psychological distress was associated with a lower obesity risk to the result section of the abstract, right now you are only reporting this finding in the conclusion section of the abstract. I suggest you simply write: “…hypertension, being physically inactive and low psychological distress were obesity risk factors…” Introduction 3) Page 5, line 12: You write “Studies of the role of the ERI model obesity yielded mixed results”. Either delete “obesity” or write “…of the role of the ERI model in obesity risk…” or something similar. Results 4) Page 12, next to last line from the bottom, you write: “which is an indication that psychological could therefore”, this should be “psychological distress”. Discussion 5) Page 19, line 8, you write: “how psychological distress and obesity associated when they”, this should be “were associated” Conclusion 6) To summarize your findings, you write the following on page 23: “The results of this study suggests that mental health (i.e. psychological distress), non-work (i.e. living in couple, child-related strains) and individual factors (i.e. physical activity, hypertension and psychotropic medication) are predictive factors for obesity for both men and women, while work factors (i.e. decision authority) only contribute to additionally explain women’ obesity.” Please make clear here, in which direction the associations are, that is make the following changes: “The results of this study suggests that GOOD mental health (i.e. LOW psychological distress), non-work (i.e. living in couple, child-related strains) and individual factors (i.e. physical INactivity, hypertension and psychotropic medication) are predictive factors for obesity for both men and women, while work factors (i.e. LOW decision authority) only contribute to additionally explain women’s obesity.” 7) On page 24 you have a long sentence at the end of the conclusion section starting with “Our results suggest that workplaces

	can become powerful targets to promote healthy life habits and mental health, with…". I suggest that you delete the whole sentence. I do not think it is justified to write that "workplaces can become powerful targets for promoting healthy life habits" based on a single (and rather small) association of low decision authority and obesity risk in women. And you have not prospectively investigated the association of workplace factors with mental health. 8) General: You often write that xy decreased (or increased) obesity risk. With regard to the statistical analyses this is correct but many researchers would prefer to refrain from language that indicate causal inference in an observational (i.e. non-experimental) study and would prefer more cautious wording such as "xy was associated with a decreased (or increased) obesity risk". Of course, this change in wording increases word count and you could argue that it is not necessary, since it is clear that your study is observational. I leave it up to you (and the editor) whether to change or not change the wording.

VERSION 2 – AUTHOR RESPONSE

Reviewer Name Reiner Rugulies
Institution and Country National Research Centre for the Working Environment, Copenhagen, Denmark
Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below Dear Authors,

Thanks for the revision. I only have a couple of minor things left:

We would like to cordially thank again Dr.Rugulies for his kind help and informative comments on our manuscript.

Abstract, result section
1) First sentence: Write "decision authority was associated" instead of "decision authority associated"
The abstract has been modified according to your suggestion.

2) Add the finding that high psychological distress was associated with a lower obesity risk to the result section of the abstract, right now you are only reporting this finding in the conclusion section of the abstract. I suggest you simply write: "…hypertension, being physically inactive and low psychological distress were obesity risk factors…"
The abstract has been modified according to your suggestion.

Introduction
3) Page 5, line 12: You write "Studies of the role of the ERI model obesity yielded mixed results". Either delete "obesity" or write "…of the role of the ERI model in obesity risk…" or something similar.
Correction done at p.5 of this revision.

Results
4) Page 12, next to last line from the bottom, you write: "which is an indication that psychological

could therefore", this should be "psychological distress".
Yes you are right. Correction done at p.12 of this revision.

Discussion
5) Page 19, line 8, you write: "how psychological distress and obesity associated when they", this should be "were associated"
Correction done at p.19 of this revision.

Conclusion
6) To summarize your findings, you write the following on page 23: "The results of this study suggests that mental health (i.e. psychological distress), non-work (i.e. living in couple, child-related strains) and individual factors (i.e. physical activity, hypertension and psychotropic medication) are predictive factors for obesity for both men and women, while work factors (i.e. decision authority) only contribute to additionally explain women' obesity." Please make clear here, in which direction the associations are, that is make the following changes:
"The results of this study suggests that GOOD mental health (i.e. LOW psychological distress), non-work (i.e. living in couple, child-related strains) and individual factors (i.e. physical INactivity, hypertension and psychotropic medication) are predictive factors for obesity for both men and women, while work factors (i.e. LOW decision authority) only contribute to additionally explain women's obesity."
The sentence has been modified according to your suggestion. Correction done at p.23 of this revision.

7) On page 24 you have a long sentence at the end of the conclusion section starting with "Our results suggest that workplaces can become powerful targets to promote healthy life habits and mental health, with…". I suggest that you delete the whole sentence. I do not think it is justified to write that "workplaces can become powerful targets for promoting healthy life habits" based on a single (and rather small) association of low decision authority and obesity risk in women. And you have not prospectively investigated the association of workplace factors with mental health.
Yes you are right, this sentence has been deleted according to your suggestion. Correction done at p.24 of this revision.

8) General: You often write that xy decreased (or increased) obesity risk. With regard to the statistical analyses this is correct but many researchers would prefer to refrain from language that indicate causal inference in an observational (i.e. non-experimental) study and would prefer more cautious wording such as "xy was associated with a decreased (or increased) obesity risk". Of course, this change in wording increases word count and you could argue that it is not necessary, since it is clear that your study is observational. I leave it up to you (and the editor) whether to change or not change the wording.

Given the disciplinary perspective of the journal anchored in public health and acknowledging the terminology used in similar initiatives on obesity research in other well-known large epidemiological cohorts (e.g., Whitehall, Gazel), we propose to rally to the current suggestion. The manuscript has been modified accordingly.