PEER REVIEW HISTORY

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ARTICLE DETAILS

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<td>AUTHORS</td>
<td>Eguchi, Hisashi; Tsutsumi, Akizumi; Inoue, Akiomi; Hikichi, Hiroyuki; Kawachi, Ichiro</td>
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VERSION 1 – REVIEW

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GENERAL COMMENTS

This study is interesting because it is one of the first that deals with the relationship between social capital and stress in a longitudinal way. The study can be improved with more references to the literature.


Review of the manuscript: “Association of workplace social capital with psychological distress: Results from a longitudinal multilevel analysis of the J-HOPE study”

The manuscript is based on an interesting dataset of Japanese employees, and investigates the associations between social capital and psychological distress. However, there are some major issues which need to be addressed by the authors.

Major issues
1. Statistical and theoretical model. Even though the authors investigate the associations between workplace social capital (WSC) and distress, the formulation of the statistical model as such implies additional hypotheses which the authors did not explicitly considered. The authors simply called “covariates” a set of highly relevant variables related to occupational exposure, health states, and health-related behaviour. For instance, weekly working hours is not simply a “covariate” which can or cannot be included in the analysis; it rather represents workload. Hence, weekly working hours is rather an occupational exposure leading to increasing levels of psychological distress. Similarly, occupation may also be regarded as a proxy variable of work-related hazards, and may also be considered in the web of causation leading from exposure to distress symptoms.

On the other hand, the authors consider body mass index in their analysis. Why should this matter for the levels of psychological distress? Do the authors have empirical evidence suggesting that higher levels of BMI lead to higher work-related distress? The set of health variables poses also serious problems. The authors consider “alcohol drinking” as a mere “covariate”. Nonetheless, it is known that alcohol consumption may serve as a sort of distress coping strategy under certain circumstances. Hence, “alcohol consumption” is more likely to be the “consequence” of distress rather than the “cause” (right-hand side of the equation). Regarding the variable “chronic diseases” it is impossible for the reader to assess what this variable should correspond to. What kind of “chronic disease” has been here collected, diabetes, cardiovascular diseases, chronic depression? Moreover, it is highly unlikely that for an employed population about 77% report suffering from a chronic condition (Table 1). Thus, I doubt the validity of the variable “chronic diseases”.

In general, it seems that the authors do not have clear thoughts about the causality implications of the variables included in the analysis. The major problem is that those implications may have real effects on the estimates, despite the authors being unaware of the tacit implications of their models. The authors should define their statistical models on sound (i.e. empirically based) theoretical
assumptions, and provide thus sound theoretical arguments for (1) variable selection and (2) the expected direction of associations.

2. Multilevel modelling. It seems that the authors included uni-level WSC in the fixed effects part of the model. I think, the variable should be handled in the random-effects part of the model (i.e. at the second level of variation), since it contributes to variation at that level (and not on the individual one). On the other hand, the estimates of unit-level variation are practically insignificant (e.g. 0.13, VPC workplace 0.1%). Hence, I don't believe the authors' conclusion holds, namely: “the longitudinal analysis showed that uni-level WSC was associated with mental health across 3 years, above and beyond an individual's own perceptions” (p. 16). I'm not convinced that a 0.1% explained variance contributes “above and beyond” individual's own perceptions. I don't know which test the authors performed on page 15, (line 236), but it seems that the reported p-value is small only because of the large sample size. The authors should double check the model specification and perform adequate test of goodness of fit regarding the relevance of unit-level variation. As far as I can observe the models require only variation at the individual- and time level (50% and 49%, respectively, see Table 3).

3. Conclusions. Given that (1) unit-level variation is practically insignificant, and that (2) the authors did not investigate the effects of “work unit” social activities on distress, there is no empirical evidence whatsoever supporting the authors' recommendation: “To prevent mental health problems in subordinates, work unit managers should have a role in boosting WSC, such as organizing athletic competitions within the company and social activities (e.g., weekend corporate retreats and cherry-blossom viewing picnic parties)” (p. 26). This conclusion is no supported by the study, and should be removed. In addition, the study did not investigate the role of managers, and, consequently, no statements can be done regarding their role in the aetiology of mental health.

Minor revisions.
1. Please specify which type of ICC is being reported (ICC(1)), and perform adequate statistical tests for the null hypothesis ICC = 0 (pages 13 and 14), see (McGraw und Wong 1996).
2. Please provide in Table 2 the within-group agreement index for each group.
3. I don't understand why the authors state: “individual-level WCS was significantly (and modestly) related to psychological distress” (p. 16, lines 245f). The coefficient indicates reductions of 0.84 standard deviation (SD) units of WSC. Hence, one unit SD reduction on the WSC scale would lead to reductions of 0.84*SD(distress). According to Table 1 the distress reduction would be around 3.7 (~ 15% from total range 0-24). I don't think these are “modest” but rather “medium” or “large” reductions.
4. Please supply the raw estimates of the models for the non-imputed dataset in order to compare the extent to which the results between imputed and non-imputed models differ. This should be done after addressing the major issues stated above.
5. The authors should consider whether the ratio of the JCQ scales is adequate. I believe this is not necessary since it is simply a multiplicative model (i.e. demands x latitude). A ratio of variables requires that both scales are metrical (which they're not, they are ordinal).
References

REVIEWER
Gabriele Giorgi
European University of Rome, Italy

REVIEW RETURNED
15-May-2018

GENERAL COMMENTS
The manuscript describes the hypothesis, study, and methods of an original research. The language, specifically the grammar, is of a good quality.

Abstract and keywords are discreet both in terms of appropriateness of context and the purpose of study.

The introduction (including its subsections) is well written and more than satisfactory in terms of appropriateness of context and the purpose of study. The length is adequate and the analysis of the literature can be improved, especially in view of the importance in the field of Occupational Health Psychology with particular attention to peculiar workplaces, as manufactures ones. In this way, I advise you to consider and also comment some recent publications on the topic. For example, I suggest you to refer to the following publications:


You could also consider a brief discussion regarding the potential additional effects of the economic crisis in the dynamics of workplace bullying. Indeed, new trends in the current period need to be addressed. It is a very current topic and I suggest you to refer to the following publications:


Methods section appears of a more than satisfactory quality. However, you should go deeper into this section and, in addition, explain better the reasons why you have chosen to use these research methods. This makes the whole paper more understandable to the readers. Add also any possible consideration about ethics statement in the Methods body text. The statistical methodologies that you have used are well illustrated.

Results section is, as a whole, of a more than satisfactory quality. However, you should better connect the text with tables. In other words, you should more thoroughly explain the contents of the table and the figure before making reference to them in the body text.

The discussion section, although methodologically correct and well written, is just enough. You should carefully compare your various findings with the field literature. I suggest you to go deeper into the study's limitations. Finally, I suggest you to use a final paragraph to sum up your findings. Otherwise, you should also carefully explain what is the specific contribution that your findings brings to literature and knowledge in this area.

REVIEWER
Marcello Bertotti
Institute for Health and Human Development, University of East London, London, UK

REVIEW RETURNED
21-May-2018

GENERAL COMMENTS
although the article is unique in its design (longitudinal, unit, individuals etc), it is really difficult to see how it moves the field forward. It is important to assess whether an association between WSC and psychological distress exists, but this article does not adequately confront with the components making up WSC (e.g. reciprocity, trust, informal social control, collective efficacy) which would be of considerable value in understanding what to do in moving the field forward. So my recommendation is for major revisions and much more clarity on the concept of WSC.

Abstract:
is it workplace social capital or workplace bonding social capital. These are not the same thing. Please clarify as there are different types of social capital

Introduction:
the definition of SC includes the term 'resources'. There is no discussion of what these resources may be apart from a short consideration on p.24. i.e. reciprocity and mutual support; collective efficacy etc. These should be discussed in the intro.
The discussion of WSC may need improving. WSC is not defined, it is not clear whether it is bonding WSC or others types of social capital. I think there is a need to clarify this point.
There is no specification of clear objectives as suggested by STROBE statement

Methods:
Research questions are missing; it would be important to specify the research questions for this article.
Methods do not seem to include any details about the questions asked as part of Working Social Capital. 6 items instrument to measure bonding social capital is mentioned but no details or reference is given to the actual content of the questions. There is a conceptual point about using the mean of individual responses as a measure of WSC. although this has been used widely in the literature, isn't social capital meant to add up to more than the sum of its parts? Using the mean as a measure only tell us an average group level. As there is no details about the content of the 6 items questions which make up WSC, it is difficult to really assess this.

Under independent variable description (P.10) 'each item is scored...' seems to be repeated.

Conclusions:
The conclusion is that WSC is associated with improvement in mental health among employees. Whilst this is an important conclusion, it does not really tell us what aspect of WSC may be important. is it reciprocity and mutual support, is it informal social control or is it collective efficacy? As a result, the argument for supporting athletic competitions or social activities is therefore not necessarily justified by the evidence presented.

Limitations:
what if reverse causality applies? i.e. people with lower mental health problems are also more likely or willing or capable of accessing the resources provided by the network. Perhaps worth considering this in the limitations or explain how it may have been addressed in the study.

VERSIO 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Nicola Magnavita
Institution and Country: Università Cattolica del Sacro Cuore, Roma, Italy

This study is interesting because it is one of the first that deals with the relationship between social capital and stress in a longitudinal way.


Thank you for pointing us towards useful references about workplace social capital and social support in the workplace. The concepts of workplace social support and workplace social capital are indeed related. For example, a workplace with high social cohesion and solidarity (i.e. high social capital) is also likely to be a workplace where employees generally receive social support from co-workers and supervisors. We agree that it is important to draw a clear distinction between these two concepts. Workplace social support is a resource that individual workers can access. However, even in the same workplace, there may be inequalities in receipt of social support, i.e., some workers will receive more than others. Workplace social capital, however, is a property of the workplace, not the individual. Our multi-level analysis aggregates workers’ perceptions about cohesion and solidarity up to the work unit level. Workplace social capital is a group-level concept which is distinct from individual report of social support. We have therefore added more about this to the discussion section:

P27, Line 313: The concepts of workplace social support and WSC are related. For example, a workplace with high social cohesion and solidarity (i.e. high social capital) is likely to be one where employees receive social support from their co-workers and supervisors. There are, however, some significant differences between the concepts. Workplace social support is a resource that individual workers can access. Even in the same workplace, there may be inequalities in receipt of social support, i.e., some workers will receive more than others. WSC, however, is a property of the workplace, not the individual. In our multi-level analysis, we aggregated workers’ perceptions about cohesion and solidarity up to the work unit level. WSC is therefore a group-level concept and distinct from individual reports of social support.

We have also added several new references on workplace social capital, as you suggested:


Reviewer: 2
Reviewer Name: Diego Montano
Institution and Country: Clinic for Psychosomatic Medicine and Psychotherapy, Ulm University, Germany

The manuscript is based on an interesting dataset of Japanese employees, and investigates the associations between social capital and psychological distress. However, there are some major issues which need to be addressed by the authors.

Thank you for your positive evaluation of our study design. We have tried to revise our manuscript to reflect your useful suggestions.
Major issues
1. Statistical and theoretical model. Even though the authors investigate the associations between workplace social capital (WSC) and distress, the formulation of the statistical model as such implies additional hypotheses which the authors did not explicitly considered. The authors simply called “covariates” a set of highly relevant variables related to occupational exposure, health states, and health-related behaviour. For instance, weekly working hours is not simply a “covariate” which can or cannot be included in the analysis; it rather represents workload. Hence, weekly working hours is rather an occupational exposure leading to increasing levels of psychological distress.

We agree that with your assessment of the nature of weekly working hours as occupational exposure that can result in increasing levels of psychological distress. Working hours may also affect workplace social capital by causing strain/tension among co-workers as well as between employees and supervisors. They can therefore be considered a common cause (i.e. confounder) of the association between social capital and distress. We have therefore adjusted for working hours in our model.

Similarly, occupation may also be regarded as a proxy variable of work-related hazards, and may also be considered in the web of causation leading from exposure to distress symptoms.

Again, we agree that occupation can also be considered a common cause of social capital and distress, i.e. that the type of occupation may both determine levels of social capital in the workplace and be a predictor of distress. (In other words, social capital does not determine the type of occupation; rather it is the other way around). We have therefore also controlled for occupation.

On the other hand, the authors consider body mass index in their analysis. Why should this matter for the levels of psychological distress? Do the authors have empirical evidence suggesting that higher levels of BMI lead to higher work-related distress?

We believe that high body mass index (overweight and obesity) may be an underlying cause of discrimination and bullying in the workplace, and hence may be a predictor of distress.

The set of health variables poses also serious problems. The authors consider “alcohol drinking” as a mere “covariate”. Nonetheless, it is known that alcohol consumption may serve as a sort of distress coping strategy under certain circumstances. Hence, “alcohol consumption” is more likely to be the “consequence” of distress rather than the “cause” (right-hand side of the equation).

We agree with your assessment. Alcohol consumption can be a downstream consequence of distress. However, it can be also conceptualized as a cause of workplace social capital and distress. For example, many workplaces in Japan encourage after-hours drinking to build social cohesion within the company. This may be a coping strategy to prevent the build-up of tensions and distress in the workplace.

Regarding the variable “chronic diseases” it is impossible for the reader to assess what this variable should correspond to. What kind of “chronic disease” has been here collected, diabetes, cardiovascular diseases, chronic depression? Moreover, it is highly unlikely that for an employed population about 77% report suffering from a chronic condition (Table 1). Thus, I doubt the validity of the variable “chronic diseases”.

We apologize for this; it was a typing error. 77% was the percentage of the employees without a chronic disease. We have now corrected this.

In general, it seems that the authors do not have clear thoughts about the causality implications of the variables included in the analysis. The major problem is that those implications may have real effects
on the estimates, despite the authors being unaware of the tacit implications of their models. The authors should define their statistical models on sound (i.e. empirically based) theoretical assumptions, and provide thus sound theoretical arguments for (1) variable selection and (2) the expected direction of associations.

Thank you for useful comments about the choice and explanation about covariates. As you pointed out, our explanation for including each covariate was clearly not enough. We have therefore reviewed the appropriateness of each covariate to confirm that they are all suitable based on previous studies, and added suitable references for each.

Page 11, Line 161: The demographic and lifestyle characteristics measured were based on previous studies about the risk factor for psychological distress were sex, age, educational,21 BMI,22 job strain,23 occupation,24 employment contract,25 weekly working hours,26 annual family income,27 chronic medical illness,28 smoking status,29 frequency of alcohol drinking,30 and physical exercise.31

2. Multilevel modelling. It seems that the authors included unit-level WSC in the fixed effects part of the model. I think, the variable should be handled in the random-effects part of the model (i.e. at the second level of variation), since it contributes to variation at that level (and not on the individual one). On the other hand, the estimates of unit-level variation are practically insignificant (e.g. 0.13, VPC workplace 0.1%). Hence, I don't believe the authors' conclusion holds, namely: "the longitudinal analysis showed that unit-level WSC was associated with mental health across 3 years, above and beyond an individual's own perceptions" (p. 16). I'm not convinced that a 0.1% explained variance contributes "above and beyond" individual's own perceptions. I don't know which test the authors performed on page 15, (line 236), but it seems that the reported p-value is small only because of the large sample size. The authors should double check the model specification and perform adequate test of goodness of fit regarding the relevance of unit-level variation. As far as I can observe the models require only variation at the individual- and time level (50% and 49%, respectively, see Table 3).

Thank you for your suggestions about our analysis. In model 2, in line with your suggestions, we have moved unit-level workplace social capital to the random-effects part. Second, we added more discussion about the estimates of unit-level variation. We also removed “above and beyond” from the conclusion. Third, we confirmed the goodness of fit for the relevance of unit-level variations. The revised sections of text now read:

Page 15, Line 228: The longitudinal analysis was performed with time (at level 1), individuals (at level 2) and work unit (at level 3). Three models were run, with the cumulative measure of K6 regressed on work units in the empty model (Model 0), with individual-level WSC and all individual-level variables included in Model 1, with a full model including all individual- and work unit-level variables and work unit-level WSC and workplace size included in Model 1 as random-effects parts (Model 2). We standardized all explanatory variables before inclusion in the multilevel analysis.

Table 3:

Page 26, Line 302: In our crude analyses, the ICC for social capital was only 0.1 %, indicating that a substantial proportion of the variance of individual social capital is between work-units. This is quite low compared with the previous studies.4 39 42 43 The reason for this discrepancy may be the number of work units and the repeated-measures design. Smaller number of individual employees per work units may show larger ICCs (number of participants/number of work units) such as 32,053/2,182,4 9,524/1,522,39 and 2,043/260.42 No previous studies have used the repeated
measures of psychological distress. The repeated measures may decrease ICC, which indicates the amount of variation in psychological distress between workplaces. The VPC at the work unit level was 6.9% which was in line with previous studies.

Page 5, Line 50: We found that unit-level workplace social capital had an impact on employees' psychological distress.

Page 17, Line 271: In summary, the longitudinal analysis showed that unit-level WSC was associated with mental health over a 3-year period.

3. Conclusions. Given that (1) unit-level variation is practically insignificant, and that (2) the authors did not investigate the effects of “work unit” social activities on distress, there is no empirical evidence whatsoever supporting the authors' recommendation: “To prevent mental health problems in subordinates, work unit managers should have a role in boosting WSC, such as organizing athletic competitions within the company and social activities (e.g., weekend corporate retreats and cherry-blossom viewing picnic parties)” (p. 26). This conclusion is no supported by the study, and should be removed. In addition, the study did not investigate the role of managers, and, consequently, no statements can be done regarding their role in the aetiology of mental health.

In line with your previous comments, we have now included the unit-level workplace social capital in the random-effects part of the model. The result showed that the work unit variance partition coefficient was 6.9%. In the model, unit-level variation is significant. We therefore believe that it is legitimate to draw out this policy implication. As you pointed out, we did not investigate the effect of manager behaviors and work unit social activities. However, previous community-based intervention studies suggested that these activities may strengthen workplace social capital. We have therefore revised the section of the discussion on this topic:

Page 28 Line 340: Unit-level variation of WSC was significant. In the workplace, managers may play an important role in boosting unit-level WSC. Previous community-based intervention studies suggested that work unit social activities may strengthen WSC. Examples of interventions to promote WSC include scheduling athletic competitions (undokai) within the company, and social activities such as weekend corporate retreats (shain-ryoko) and cherry-blossom viewing picnic parties (hanami).

Minor revisions.
1. Please specify which type of ICC is being reported (ICC(1)), and perform adequate statistical tests for the null hypothesis ICC = 0 (pages 13 and 14), see (McGraw und Wong 1996).

Thank you for these suggestions. We have now specified the type of ICC and performed suitable statistical tests.

Page 14, Line 215: We estimated a null model that included only a random intercept and allowed us to estimate the intraclass correlation coefficient (ICC(1)). The ICC(1) was 4.0% (P < 0.001) in the first wave, 3.5% (P < 0.001) in the second wave, and 4.0% (p < 0.001) in the third wave, indicating significant variance in individual WSC between work units.

2. Please provide in Table 2 the within-group agreement index for each group.

We have added the minimum and maximum values of within-group agreement index (rwg) for each group to Table 2.
3. I don’t understand why the authors state: “individual-level WCS was significantly (and modestly) related to psychological distress” (p. 16, lines 245f). The coefficient indicates reductions of 0.84 standard deviation (SD) units of WSC. Hence, one unit SD reduction on the WSC scale would lead to reductions of 0.84*SD(distress). According to Table 1 the distress reduction would be around 3.7 (~15% from total range 0-24). I don’t think these are “modest” but rather “medium” or “large” reductions.

In line with your comment, we have revised this section to read:

Page 3, Line 37: In the full model, each standard deviation increase in unit-level WSC was associated with 0.69 point improvement in K6 scores (95% confidence interval: −1.12 to −0.26).

Page 17, Line 265: In Model 2, the VPC at the work unit level was 6.9%. Unit-level WSC was significantly related to change in psychological distress (coefficient = −0.69; 95% CI −1.12 to −0.26). This equated to an average improvement in K6 scores of roughly 0.69 points over 3 years for every standard deviation change in unit-level WSC. Similarly, individual-level WSC was significantly related to psychological distress (coefficient = −0.84; 95% CI −0.94 to −0.74). In summary, the longitudinal analysis showed that unit-level WSC was associated with mental health over a 3-years period.

4. Please supply the raw estimates of the models for the non-imputed dataset in order to compare the extent to which the results between imputed and non-imputed models differ. This should be done after addressing the major issues stated above.

We have added the raw estimates to Table 3, but there is not much difference between the imputed and non-imputed datasets.

5. The authors should consider whether the ratio of the JCQ scales is adequate. I believe this is not necessary since it is simply a multiplicative model (i.e. demands x latitude). A ratio of variables requires that both scales are metrical (which they’re not, they are ordinal).

We have discussed this, and concluded that the ratio of the JCQ is adequate, based on previous studies.

Reviewer: 3
Reviewer Name: Gabriele Giorgi
Institution and Country: European University of Rome, Italy

The manuscript describes the hypothesis, study, and methods of an original research. The language, specifically the grammar, is of a good quality. Abstract and keywords are discreet both in terms of appropriateness of context and the purpose of study.

1. The introduction (including its subsections) is well written and more than satisfactory in terms of appropriateness of context and the purpose of study. The length is adequate and the analysis of the literature can be improved, especially in view of the importance in the field of Occupational Health Psychology with particular attention to peculiar workplaces, as manufactures ones. In this way, I advise you to consider and also comment some recent publications on the topic. For example, I suggest you to refer to the following publications:


You could also consider a brief discussion regarding the potential additional effects of the economic crisis in the dynamics of workplace bullying. Indeed, new trends in the current period need to be addressed. It is a very current topic and I suggest you to refer to the following publications:


Thank you for your useful comments on potential new variables and issues. We agree that workplace bullying and the economic crisis are important issues. We have added a brief discussion of these to the section on our study limitations, citing the studies you recommended:

Page 29, Line 358: Fifth, there may be other social and economic factors that should have been considered. Workplace bullying plays a significant role in mediating the association between psychosocial factors and psychological distress. Economic crises may also have a potential additional negative impact on workers' mental health. In Japan, suicide as a result of psychological distress was a significant public health concern for working-aged men after the “bubble economy” collapsed. These social and economic conditions in Japan may therefore affect the association between organizational psychosocial factors such as WSC and individual mental health.

2. Methods section appears of a more than satisfactory quality. However, you should go deeper into this section and, in addition, explain better the reasons why you have chosen to use these research methods. This makes the whole paper more understandable to the readers. Add also any possible consideration about ethics statement in the Methods body text. The statistical methodologies that you have used are well illustrated.

We have revised the manuscript as follows:

Page 7, Line 81: In this study, we constructed a multilevel analysis using three levels (repeated measurements of psychological distress nested within individual employees, then work units) to evaluate the impact of a change in unit-level WSC on individual workers' psychological distress. We
used panel data from three waves of the Japanese Study of Health, Occupation, and Psychosocial Factors Related Equity (J-HOPE), an occupational cohort study on social class and health in Japan that involved 21 work units.

Page 7, Line 91: Written consent was obtained from participants. The study aims and protocol were reviewed by the Research Ethics Committee of the Graduate School of Medicine and Faculty of Medicine, The University of Tokyo (No. 2772), the Kitasato University Medical Ethics Organization (B12-103), and the Ethics Committee of the University of Occupational and Environmental Health, Japan (No. 10-004).

3. Results section is, as a whole, of a more than satisfactory quality. However, you should better connect the text with tables. In other words, you should more thoroughly explain the contents of the table and the figure before making reference to them in the body text.

We have added more explanation of the tables to this section. It now reads:

Page 16, Line 249: Table 1 summarizes the participants’ characteristics. The majority of the participants were male (71.2%), and the mean age was 38.8 years (SD=10.9) (table 1). The largest occupational group was manual workers. About 80% of our sample were employed full-time and about 30% worked 41–50 hours per week in the time between the first and third surveys.

Table 2 shows the workplace characteristics. The mean unit-level social capital was 16.3 (SD = 2.8) to 17.1 (SD = 0.5 and 0.8) in the first survey, 16.1 (SD=2.1) to 17.3 (SD=0.2) in the second survey and 16.4 (SD=0.0) to 17.4 (SD=1.0) in the third survey.

Table 3 shows the three-level hierarchical regression results. The null model indicated a significant amount of variation in psychological distress between workplaces (ICC = 0.1%, P < 0.001). Random effects in the initial empty model showed that 50.5% of the average variation in psychological distress was attributed to clustering by individuals over time, while 0.1% was attributed to clustering by work units over time. In Model 1, individual-level WSC showed a significant association with psychological distress (coefficient = −0.84; 95% confidence interval [CI] −0.94 to −0.74). In Model 2, the VPC at the work unit level was 6.9%. Unit-level WSC was significantly related to change in psychological distress (coefficient = −0.69; 95% CI −1.12 to −0.26). This equated to an average improvement in K6 scores of roughly 0.69 points over 3 years for every standard deviation change in unit-level WSC. Similarly, individual-level WSC was significantly related to psychological distress (coefficient = −0.84; 95% CI −0.94 to −0.74). In summary, the longitudinal analysis showed that unit-level WSC was associated with mental health over a 3-years period.

4. The discussion section, although methodologically correct and well written, is just enough. You should carefully compare your various findings with the field literature. I suggest you to go deeper into the study's limitations.

We have added an additional paragraph to the discussion, and also added two new limitations. The new parts read:

Page 26, Line 302: In our crude analyses, the ICC for social capital was only 0.1 %, indicating that a substantial proportion of the variance of individual social capital is between work-units. This is quite low compared with the previous studies.4 39 42 43 The reason for this discrepancy may be the number of work units and the repeated-measures design. Smaller number of individual employees per work units may show larger ICCs (number of participants/number of work units) such as 32,053/2,182,4 9,524/1,522,39 and 2,043/260.42 No previous studies have used the repeated measures of psychological distress.4 39 42 43 The repeated measures may decrease ICC, which indicates the amount of variation in psychological distress between workplaces. The VPC at the work unit level was 6.9% which was in line with previous studies. 4 39 42 43
Page 29, Line 358: Fifth, there may be other social and economic factors that should have been considered. Workplace bullying plays a significant role in mediating the association between psychosocial factors and psychological distress. Economic crises may also have a potential additional negative impact on workers’ mental health. In Japan, suicide as a result of psychological distress was a significant public health concern for working-aged men after the “bubble economy” collapsed. These social and economic conditions in Japan may therefore affect the association between organizational psychosocial factors such as WSC and individual mental health. A previous study tested both traditional and reverse causality, because chronic stress has been reported to decrease some cognitive functions (notably indicative reasoning and vocabulary) in the working-age population. Future research should also consider reverse causality to examine the association between workplace social capital and psychological distress more comprehensively.

Sixth, our study only tested traditional causality (i.e., from workplace social capital to psychological distress). A previous study tested both traditional and reverse causality, because chronic stress has been reported to decrease some cognitive functions (notably indicative reasoning and vocabulary) in the working-age population. Future research should also consider reverse causality to examine the association between workplace social capital and psychological distress more comprehensively.

5. Finally, I suggest you to use a final paragraph to sum up your findings. Otherwise, you should also carefully explain what is the specific contribution that your findings brings to literature and knowledge in this area.

We have added a new concluding paragraph to the section on limitations, to sum up our study contribution:

Page 30, Line 374: Our study has several strengths, including the large sample of Japanese employees, and the use of a new statistical method, a multilevel analysis using three levels (repeated measurements of psychological distress nested within individual employees, then work units). Based on these analyses, our study provides a new research insight into the contextual effect of WSC on employees’ mental health.

Reviewer: 4
Reviewer Name: Marcello Bertotti

Although the article is unique in its design (longitudinal, unit, individuals etc), it is really difficult to see how it moves the field forward. It is important to assess whether an association between WSC and psychological distress exists, but this article does not adequately confront with the components making up WSC (e.g. reciprocity, trust, informal social control, collective efficacy) which would be of considerable value in understanding what to do in moving the field forward. So my recommendation is for major revisions and much more clarity on the concept of WSC.

1. Is it workplace social capital or workplace bonding social capital? These are not the same thing. Please clarify as there are different types of social capital.

Thank you for your useful comments. The study mainly measured bonding workplace social capital. Bridging workplace social capital would have included items explicitly asking about trust between employees and supervisors, or trust between workers and managers, i.e. vertical trust and solidarity. To clarify this point, we have added more to the methods section as follows.

Page 10, Line 146: The items in the measure are: “People keep each other informed about work-related issues in the work unit”, “We have a ‘we are together’ attitude”, “People feel understood and accepted by each other”, “In our workplace, there is an atmosphere of helping each other”, “In our workplace, we trust each other”, and “Our workplace is a place of laughter and smiles”. The WSC
scale has acceptable reliability and validity, described in detail elsewhere. In brief, the scale includes items relevant to bonding WSC to measure the network, trust, and reciprocity aspects of the concepts.

2. The definition of SC includes the term 'resources'. There is no discussion of what these resources may be apart from a short consideration on p.24. i.e. reciprocity and mutual support; collective efficacy etc. These should be discussed in the intro.

We have added a new paragraph to the introduction to discuss the "resources" inherent in workplace social capital:

Page 6, Line 65: The pathways linking social capital to health outcomes vary by level of analysis. In this paper, we have focused on social capital as a group-level construct. Group-level WSC can bring benefits to individuals, probably through increased emotional support and respect from coworkers, which can reduce psychophysiological stress responses to physically strenuous jobs.

3. The discussion of WSC may need improving. WSC is not defined, it is not clear whether it is bonding WSC or others types of social capital. I think there is a need to clarify this point.

As we mentioned above, we have added more information about the nature of workplace social capital in this study to the methods section.

Page 10, Line 146: The items in the measure are: "People keep each other informed about work-related issues in the work unit", "We have a 'we are together' attitude", "People feel understood and accepted by each other", "In our workplace, there is an atmosphere of helping each other", "In our workplace, we trust each other", and "Our workplace is a place of laughter and smiles". The WSC scale has acceptable reliability and validity, described in detail elsewhere. In brief, the scale includes items relevant to bonding WSC to measure the network, trust, and reciprocity aspects of the concepts.

4. There is no specification of clear objectives as suggested by STROBE statement

We have revised the manuscript to make the objectives clearer:

Page 7, Line 81: In this study, we constructed a multilevel analysis using three levels (repeated measurements of psychological distress nested within individual employees, then work units) to evaluate the impact of a change in unit-level WSC on individual workers' psychological distress. We used panel data from three waves of the Japanese Study of Health, Occupation, and Psychosocial Factors Related Equity (J-HOPE), an occupational cohort study on social class and health in Japan that involved 21 work units.

5. Research questions are missing: it would be important to specify the research questions for this article.

We have added more to the introduction to clarify our research questions and study objectives:

Page 6, Line 65: The pathways linking social capital to health outcomes vary by level of analysis. In this paper, we have focused on social capital as a group-level construct. Group-level WSC can bring benefits to individuals, probably through increased emotional support and respect from coworkers, which can reduce psychophysiological stress responses to physically strenuous jobs.
Page 7, Line 81: In this study, we constructed a multilevel analysis using three levels (repeated measurements of psychological distress nested within individual employees, then work units) to evaluate the impact of a change in unit-level WSC on individual workers’ psychological distress. We used panel data from three waves of the Japanese Study of Health, Occupation, and Psychosocial Factors Related Equity (J-HOPE), an occupational cohort study on social class and health in Japan that involved 21 work units.

6. Methods do not seem to include any details about the questions asked as part of Working Social Capital. 6 items instrument to measure bonding social capital is mentioned but no details or reference is given to the actual content of the questions.

We have added details of the questions used to measure workplace social capital:

Page 10, Line 146: The items in the measure are: “People keep each other informed about work-related issues in the work unit”, “We have a ‘we are together’ attitude”, “People feel understood and accepted by each other”, “In our workplace, there is an atmosphere of helping each other”, “In our workplace, we trust each other”, and “Our workplace is a place of laughter and smiles”.

7. There is a conceptual point about using the mean of individual responses as a measure of WSC. although this has been used widely in the literature, isn’t social capital meant to add up to more than the sum of its parts? Using the mean as a measure only tell us an average group level. As there is no details about the content of the 6 items questions which make up WSC, it is difficult to really assess this.

As you say, aggregating individual responses to the work unit level has been widely used in previous studies, and that is therefore the approach we decided to use. We think, however, that the resulting average “work unit social capital” represents something above and beyond individual perceptions about the workplace. For example, even if one individual expresses low trust of co-workers, that person may be working in a unit where everyone else expresses high trust. In that case, the individual may benefit from a positive spillover effect, and this is tested by our multi-level analysis. We also confirmed the appropriateness of using the average as unit-level workplace social capital using rwg and Cronbach’s α.

We have added more information about the calculation of workplace social capital and details of the way it is measured:

Page 10, line 139: WSC was the main independent variable of interest. To assess this, we used a validated six-item instrument to measure bonding workplace social capital, with each item scored on a four-point Likert scale: 1: strongly disagree, 2: disagree, 3: agree, 4: strongly agree. The responses were summed, resulting in individual WSC scores from 6–24 with higher scores indicating higher WSC. The internal consistency of the scale was acceptably high in each survey, with Cronbach’s α coefficients for the six-item WSC scale of 0.89 in the first wave, 0.90 in the second wave, and 0.90 in the third wave. The items in the measure are: “People keep each other informed about work-related issues in the work unit”, “We have a ‘we are together’ attitude”, “People feel understood and accepted by each other”, “In our workplace, there is an atmosphere of helping each other”, “In our workplace, we trust each other”, and “Our workplace is a place of laughter and smiles”. The WSC scale has acceptable reliability and validity, described in detail elsewhere.20 In brief, the scale includes items relevant to bonding WSC to measure the network, trust, and reciprocity aspects of the concepts.

8. Under independent variable description (P.10) ’each item is scored...’ seems to be repeated.

We apologize for these mistakes. We have revised this section to remove repetition.
9. The conclusion is that WSC is associated with improvement in mental health among employees. Whilst this is an important conclusion, it does not really tell us what aspect of WSC may be important. Is it reciprocity and mutual support, is it informal social control or is it collective efficacy? As a result, the argument for supporting athletic competitions or social activities is therefore not necessarily justified by the evidence presented.

In line with this comment and one from another reviewer on the same issue, we have added a new section to the conclusion of our manuscript, and this section now reads:

Page 30, Line 382: This prospective study adds to previous research by showing that WSC is associated with improvement in mental health among employees. WSC appears to have a contextual effect on employees’ mental health. We recommend that unit-level WSC is considered alongside other known contextual influences on the mental health of workers. To prevent mental health problems in subordinates, work unit managers might have a role in boosting WSC, such as organizing athletic competitions within the company and social activities (e.g., weekend corporate retreats and cherry-blossom viewing picnic parties).

10. What if reverse causality applies? i.e. people with lower mental health problems are also more likely or willing or capable of accessing the resources provided by the network. Perhaps worth considering this in the limitations or explain how it may have been addressed in the study.

Thank you for this useful comment. We have added a further limitation about reverse causality to the limitations section.

Page 30, Line 366: Sixth, our study only tested traditional causality (i.e., from workplace social capital to psychological distress). A previous study tested both traditional and reverse causality, because chronic stress has been reported to decrease some cognitive functions (notably indicative reasoning and vocabulary) in the working-age population.56 Future research should also consider reverse causality to examine the association between workplace social capital and psychological distress more comprehensively.

**VERSION 2 – REVIEW**

| REVIEWER         | Nicola Magnavita  
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**GENERAL COMMENTS**
The authors have only partially accepted my suggestions for the improvement of the manuscript

| REVIEWER         | Marcello Bertotti  
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**GENERAL COMMENTS**
Only one further change in the limitations section suggested and following from authors point 10 in their revised manuscript comments: 10. What if reverse causality applies? You argue in your revised manuscript that you have tested traditional causality. I think your argument here is incorrect because with this design you do not demonstrate causality. You would need a more longitudinal study with a control group to claim causality. I would invite you to
Reviewer: 4

Reviewer Name: Marcello Bertotti

Institution and Country: Institute for Health and Human Development, University of East London, UK

1. Only one further change in the limitations section suggested and following from authors point 10 in their revised manuscript comments:

10. What if reverse causality applies? You argue in your revised manuscript that you have tested traditional causality. I think your argument here is incorrect because with this design you do not demonstrate causality. You would need a more longitudinal study with a control group to claim causality. I would invite you to rephrase this section of your limitations and use a more tentative language. E.g. ‘there may be a causal relationship between WSC and psychological distress on the basis of our study’.

Thank you for your useful comments. According to your comment on our argument about “causality”, we revised the sentence as follow.

P30, Line 366: Sixth, we cannot reject the possibility of “reverse causation” between WSC and psychological distress on the basis of our study design, since the changes in exposure and outcome were assessed simultaneously.