Author’s response to reviews

Title: Multi-level, cross-sectional study of workplace social capital and smoking among Japanese employees

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Author’s response to reviews: see over
Professor Melissa Norton, Editor-in-Chief  
BMC Public Health  
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Dear Prof. Norton,

We are sending herewith a revised manuscript entitled “Multi-level, cross-sectional study of workplace social capital and smoking among Japanese employees” (3154041854005108) by E Suzuki, et al. which we would like to submit for the publication in BMC Public Health as a research article.

The manuscript reports independent influences of individual-level and company-level social capital at work on employees’ smoking status using multilevel logistic regression analysis. We believe that the manuscript, if it is published in BMC Public Health, is interesting to the readers, since there have been only limited number of studies on workplace social capital and smoking although workers throughout the global economy are spending increasing amounts of time in the work-place as opposed to socializing with their neighbors. To our knowledge, this is the first study examining the relationship between workplace social capital and smoking among private sector workers.

We take public responsibility for its validity and final presentation as an original publication. We declare that there are no conflicts of interest. We state here that this is an original work, which has not been published in any journals, either in whole or in part, or is not under review elsewhere. This manuscript follows the Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) recommendations.

We would be grateful if the article could be reviewed and considered for publication in BMC Public Health.

Yours sincerely,

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Dear Editor:

Thank you very much for your thoughtful review and positive evaluation of our article. We have revised our article following your helpful comments. Our responses are described in normal font following the editor’s comments in boldface.

1. The 65.1% in the abstract is unclear, what is this referring to?

Response:
Of the total of 1,800 subjects in 60 companies, 1,171 employees (65.1%) were identified from 46 companies. We modified the sentence as follows:

(Page 3 line 11)
Of the total of 1,800 subjects in 60 companies, 1,171 (men/women; 834/337) employees (65.1%) were identified from 46 companies in Okayama in 2007.

2. Remove ‘marginal’ results from the abstract.

Response:
In accordance with your suggestion, we deleted the explanation.

3. I think too much emphasis is placed on a nonsignificant interaction on age as there was no overall affect of age and you made multiple comparisons and did not correct for this. I would suggest removing this from the abstract and discussion.

Response:
In accordance with your suggestion, we deleted the sentences from the abstract and discussion.

4. Page 17, since this is a cross-sectional study you can’t say what effect moving to a company might have on changing smoking.

Response:
In accordance with your suggestion, we modified the sentence as follows:

(Page 18 line 7)
In the present study, the MOR of the empty model was 1.54, indicating that if we randomly select a person in a company with a higher probability of smoking, his/her odds of smoking is (in median) 1.54 times higher than that of a person in a company with a lower probability of smoking.

5. Finally, we ask that you comment on the ethical approval obtained for your study in your revised manuscript. Experimental research that is reported in the manuscript must have been performed with the approval of an appropriate ethics committee. Research carried out on humans must be in compliance with the Helsinki Declaration (http://www.wma.net/e/policy/b3.htm), and any experimental research on animals must follow internationally recognized guidelines. A statement to this effect must appear in the Methods section of the manuscript, including the name of the body which gave approval, with a reference number where appropriate.

Response:
We added a sentence as follows:

(Page 10 line 17)
The study was reviewed and approved by the Office of Human Research at the Harvard School of Public Health.
Reviewer 1
Dear Dr. Aida:

Thank you very much for your thoughtful review and positive evaluation of our article. We have revised our article following your helpful comment. Our response is described in normal font following the reviewer’s comment in boldface.

1. **Some studies showed the association between neighborhood-level socioeconomic status and health behavior** (e.g. Int J Public Health. 2009;54(3):183-92.). Additional adjustment for company-level socioeconomic status might be showing a more precise picture of the association between company-level social capital and smoking.

**Response:**
Unfortunately, the robust assessment of company-level socioeconomic status was not available in the present study. By acknowledging Adams et al. (2010), we added this limitation in Discussion section as follows:

(Page 19 line 17)
Further, the company-level socioeconomic status was not available, which might partly explain the association between company-level trust and smoking [46]. If company-level socioeconomic status was also associated with company-level reciprocity, however, we would have observed the association between company-level reciprocity and smoking, which contradicts with the present finding.
Reviewer 2
Dear Dr. Kim:

Thank you very much for your thoughtful and thorough review and positive evaluation of our article. We have revised our manuscript in response to your suggestions. Our responses are described in normal font following reviewer’s comments in boldface.

1. Abstract: The authors stated beyond what they found from the present study in conclusions. Smoking and smoking cessation are totally different concepts although they are related. Authors did not investigate smoking cessation and thus, the statement “The present study suggest...” seems to go beyond what they have found. Particularly, the suggestion was contradicting to the finding, that is, age was associated with higher odds of smoking in companies with high reciprocity.

Response:
In accordance with your comment, we modified the concluding sentence in the Abstract as follows:

(Page 4 line 14)
Further studies are warranted to examine the possible link between company-level trust and smoking cessation in the Japanese workplace.

In addition, we also modified the concluding sentence of Discussion as follows:

(Page 20 line 14)
Further studies are warranted to examine the possible link between company-level trust and smoking cessation in the Japanese workplace.

2. Background: The authors used interchangeably the following concepts: social capital, social resources (trust and reciprocity), social cohesion, and social support (p. 16, the second line from the top). Yet, there was no review of theoretical literature explicating the relationships among these variables.

Response:
In accordance with your comment, we added some explanations on these concepts as follows:

(Page 5 line 3)
Within public health research, the literature has evolved around two distinct conceptions
of social capital, one that emphasizes processes and outcomes associated with social cohesion (e.g., trust, solidarity, norms), and the other that emphasizes resources accessed through social networks [4]. The social cohesion definition tends to emphasize social capital as a group attribute and to analyze it as a contextual influence on individual health [1]. By contrast, the network theory of social capital defines the concept in terms of resources (e.g., social support), and it employs methods of sociometric analysis, i.e., either ego-centered network mapping or whole network analysis [5]. These two approaches are not mutually exclusive [4, 6].

3. The authors stated “a growing body of empirical research…” on page 5, yet, the literature cited a book written by the authors. It could have been helpful if they provided data-based research articles such as the work by Giordano GN and Lindström M (2010) and other major contributors in the field. There was no reference provided after the statement “theory would predict that this would be…” on p. 6. Although research questions were not clearly defined, they could be easily elicited from the statement of first three lines on page 7.

Response:
In accordance with your comment, we additionally acknowledged some empirical studies, including Giordano and Lindström (2010), as follows:

(Page 5 line 18)
In support of these assertions, a growing body of empirical research has suggested the beneficial effects of social capital on a range of health outcomes [2], including mortality [8, 9], physical health (e.g., cardiovascular disease) [10, 11], and mental health [12].

(Page 6 line 3)
Individual-level social capital has been found to be inversely associated with daily smoking [13, 14], and aspects of social capital, such as social participation [15-17], have been also associated with smoking cessation.

(Page 6 line 15)
On the other hand, if the goal is to introduce a health-promoting innovation – such as a smoking cessation intervention – theory would predict that this would be facilitated by stronger trust and reciprocity within the group [7, 21].

In accordance with your comment, we modified the sentence explaining the research question of the present study as follows:
Accordingly, in the present study we sought to examine the association between workplace social capital (both at individual- and company-levels) and smoking status among Japanese private sector employees.

4. Methods: This section has some problems. No power calculation was provided with regard to sample size. It is not clear whether the effect of intraclass correlation coefficient was taken into consideration for the nested sample. It could have been much better if the authors had weighted the number of employees selected from each company based on its size (the 3 strata). The companies were stratified into three categories but no consideration was taken for this in sampling. Furthermore, company size might have had an association with the key variables trust and reciprocity but this was not taken into consideration in data analysis.

Response:
We totally agree with your comment. As we had explained in Methods section, this study was based on a representative survey of employees in Okayama prefecture, which was conducted by the local prefectural government. Due to some issues including budget, the prefectural government could not employ the weighted sampling procedure. Regarding the possible confounding due to company size, we had already conducted the supplementary analysis by additionally adjusting for company size, which showed no substantial differences. We added a sentence to explain this result as follows:

As a supplementary analysis, we added a smoking restriction policy and a company size as company-level ordinal variables.

When we further added a company-level variable of smoking restriction policy and a company size, no substantial changes were observed (data not shown).

5. Results: In general data appeared to be sound with one exception. The authors stated that 46 companies participated out of 60 selected (20 companies from 3 strata) but no descriptions were provided as to how those 14 companies that opted out were similar to or different from the companies participated in the study. Furthermore given a wide difference in the response rate of employees (36.7% to 100%) among the companies, it could have been helpful if median response rate
instead of mean response rate had been provided.

Response:
Among the 60 companies (20 companies from 3 strata), we obtained the data from 46 companies (16, 15, and 15 companies from each stratum, respectively.) Although the non-respondent data were not available, we assume that they are similar to the companies included in the present study. Regarding the response rate of employees among the companies, the median response rate was 90.0%. We modified the sentences as follows:

(Page 13 line 2)
Of the total of 1,800 subjects, questionnaires were returned from 1,218 employees (67.6%) from 46 companies (16, 15, and 15 from each stratum). The mean response rate among these 46 companies was 83.1% (median; 90.0%, range from 36.7% to 100%).

6. Discussion: The authors discussed the study in comparison with the study of smoking cessation with Finnish public sector employees. Yet, the present study was about smoking, whereas the other study investigated smoking cessation. Thus, I wonder whether the comparison was adequate. Given that smoking has been the social norm, Asian men may be more likely to smoke if they have a high level of trust and reciprocity, whereas smoking cessation seems to be related to individual decision or family norm against smoking. Furthermore, the difference in findings could have been interpreted in light of cultural difference between individualistic and collectivistic societies.

Response:
We totally agree that smoking status and smoking cessation are different health outcomes, and careful comparison is indeed warranted. We revised the manuscript with careful attention to this issue. Further, we also agree that the interpretation based on cultural differences would be of great interest. In accordance with your suggestion, we modified the sentences as follows:

(Page 15 line 17)
These studies examined different health behaviors (i.e., current smoking versus smoking cessation), which makes it difficult to compare their findings. Indeed, since this is only the second study on this topic, the discrepancy could be a chance finding. On the other hand, it is possible that the difference between the results of the Finnish study and our Japanese study may reflect broader cultural differences between western individualism versus Asian conformism [35]. Thus, given that smoking has been the social norm Asian men may be more likely to smoke if they have a high level of trust and reciprocity,
whereas smoking cessation in Finland may be related to individual decisions to quit.

7. Given that an empty model had more explanatory power (a higher odds ratio) than model 1 and 2 did, the association between social capital and smoking might have been driven by unmeasured company characteristics such as company size and other factors including length of employment, and the mode of hiring (kinship/referral vs. official entrance exam). The discussion provided for the interpretation of study findings seems somewhat lacking in explanation and alternative interpretations could have been possible. The second paragraph on page 15 is not well balanced with the finding that age interacted with the relationship between reciprocity and smoking.

Response:
Thank you for your thoughtful comment. We agree that alternative interpretations on the present findings are warranted. As we explained above, when we additionally adjusted for company size, no substantial differences were observed. Unfortunately, other individual-level factors such as length of employment and the mode of hiring were not available in the present study although we think that age would be a good proxy of length of employment of the subjects. Further, it is less likely that the workers’ smoking status can vary according to the mode of hiring (at least) in Japan. In accordance with your comment, we modified the sentences as follows:

(Page 18 line 15)
Since the relatively large variance at the company-level could also suggest the possibility that the association between company-level trust and smoking might have been driven by unmeasured company characteristics, future studies are warranted to examine these alternative explanations.

In accordance with the editor’s comment, we deleted the explanation on cross-level interaction between company-level reciprocity and age from Discussion. In the revised manuscript, we focus on company-level trust in the formerly second paragraph on page 15 (the second paragraph on page 16 in the revised manuscript).

8. Conclusions: Study limitations were clearly and adequately stated. Overall the writing is clear.

Response:
Thank you very much for your positive evaluation for our manuscript.
We thank all the reviewers again for their helpful comments, which we feel have improved our manuscript. We hope that with these modifications, our paper can now be accepted for publication.

Sincerely,