PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Site-specific cancer mortality inequalities by employment and occupational groups: a cohort study among Belgian adults, 2001-2011
AUTHORS	Vanthomme, Katrien; Van den Borre, Laura; Vandenheede, Hadewijch; Hagedoorn, Paulien; Gadeyne, Sylvie

VERSION 1 – REVIEW

REVIEWER	Marlen Toch-Marquardt Institute of Political Science and Sociology, Norwegian University of Science and Technology, Norway
REVIEW RETURNED	20-Dec-2016

GENERAL COMMENTS	This is a very interesting article analysing occupational status inequalities in site specific cancer in the Belgian population, and examines whether occupational status is a fundamental cause of these inequalities. The authors present a very thoroughly prepared analysis and a well written paper. However, the manuscript could benefit from some changes and clarifications. Revise and resubmit would be my suggestion. The comments are presented in chronological order. Abstract: - Please check the consistency of the use of SEP or SE (Objective and Conclusion part). - The Results part could be more precise. Article summary: - First paragraph, third line the authors write: "…, they may represent different aspects,…". Please verify what these aspects are. Data and Methods: - Design and study population: Please check the correct use of tenses. - Design and study population: In the analysis the authors used persons which were between the ages of 25 to 65 years at the census in 1991. Since information on cancer mortality stems from the 2001 and 2011 register data, we are looking at a time lag of 10 respectively 20 years. Please explain how these persons were followed exactly and what happened to persons who had entered retirement age at the start of the analysis? The same might be true for the occupational status variable. Is there

	information of occupational status in the 2011 register data? (Paragraph 'Statistical analysis') - Variables, first paragraph, first line: A 'the' needs to be added in front of "total cancer mortality". - Variables, first paragraph after the table, line 43: check spelling of 'retirees'. - Variables, first paragraph after the table: The authors use a category of 'unemployed' in the analysis. I am wondering, since it is a fairly large group, if this group could be split up into unemployed which are actively looking for a job and inactive unemployed? These two groups of unemployed might have a quite different risk of cancer mortality, and since the authors took care to include as many different groups as possible into the analysis, this distinction might add some more breadth to it. - Statistical analysis: As a reader I would be interested to see the sensitivity analyses. Is it possible to publish these as an online supplement? As a reviewer, however, I need to see these analyses, in order to get a look at the whole picture. - Statistical analysis: The authors use ASMR as a measure of absolute inequalities. The analysis would be more complete if another measure of absolute inequalities, e.g. rate difference, would be incorporated. Results: - Description of the study population: Please check the use of correct tenses. - Relative cancer mortality inequality by employment status, second paragraph: Please check correct use of commas. Discussion and Conclusion: - Theoretical considerations on the main findings, first paragraph: Please check correct use of plural s. - Theoretical considerations on the main findings, third paragraph: Please note that the authors Toch-Marquardt et al. (6) did not include agricultural workers into their analysis. - Theoretical considerations on the main findings, third paragraph: Please check your discussion for redundancies. In line 6-9 and further down in line 43-45 you are using the exact same explanation. - Theoretical considerations on the main findings, fourth paragraph: The authors make use of the acronym FCT throughout the entire manuscript, why change to Link and Phelan in this paragraph? General Remarks: - In the title the authors ask if occupational status is a fundamental cause of cancer mortality. However, this question is left unanswered in the conclusion. The manuscript would gain from a short statement about this issue in the conclusion.

REVIEWER	Enrique Regidor Universidad Complutense de Madrid Spain
REVIEW RETURNED	11-Feb-2017

GENERAL COMMENTS	This paper presents a lot of ideas, but such ideas are not presented in a clear and understandable way. Nor are those ideas linked to another in a reasonable way. First I point out something related with an incorrect comment from the authors in the introduction. It is not demonstrated that the

	socioeconomic position is a fundamental cause of the inequalities in health, observed in the rich countries in the last decades. What is documented is that there are inequalities in health. And several theories try to explain them. The theory of fundamental causes is one of them. In the introduction the authors propose two theoretical frameworks whose joint approach throughout the article they fails to link well. On the one hand the authors try to contrast the theory of the fundamental causes with the mortality by diverse localizations of cancer. It is an interesting idea and sufficiently argued based on a multitude of previous works. And, on the other hand, the authors intend to evaluate the relation of the occupation with the mortality by diverse localizations of cancer, after eliminating the effect of other socioeconomic variables in that relation. This second idea is not sufficiently argued. Nor the relation of this idea with the contrast of the theory of fundamental causes is well argued. On the other hand, in the methodology there are some limitations. If the authors intend to study the relation of occupation with cancer mortality, why do they include the unemployed and others (incapacitated, as they point out in the discussion)? The selection bias due to health in both is clear. Do the authors really believe that the gap in the magnitude of the ratio of mortality rates in the unemployed to the magnitude of the ratio of mortality rates in the employed should be due to lifestyles or to less access to the health system? The explanation based on this would mean something incredible and practically impossible: that the prevalence of risk behaviours is very, very high among the unemployed and that the unemployed have little access to the health system. For this reason, the best option is to include the analysis only to people with occupation. Obviously the authors should also have excluded housewives. The authors say that they calculate absolute and relative measures of inequalities in cancer mortality. This is incorrect. The authors calculate a measure of frequency (mortality rates) and a relative measure of inequality in cancer mortality (rates ratio). The authors have not calculated an absolute measure of inequality in cancer mortality (rates difference). In the results section it is necessary to remind the authors that the title of their paper appears the word occupation, whereas in results the employment situation appears. I reiterate my previous comment. On the other hand, based on the first two paragraphs on page 10, it is difficult to get an idea of the main findings that have been found. In the discussion they do not address a crucial aspect and that it is an important limitation of the study. Many subjects will have died between 1991 and 2001, so that the subjects remaining in 2001 will be the healthiest. Therefore, there will be a strong selection bias in mortality in these subjects in the period 2001-2011 and, therefore, the probability of finding differences according to occupation (or according to any other variables) is lower. On the other hand, as I pointed out above, the approach to the two objectives is not resolved and the discussion is unclear. For example, the authors speculate on a multitude of issues, particularly on page 12, but they do not fully state whether their findings support or do not support the theory of fundamental causes. Indeed, in the light of their findings (in case their findings were correct and did not have the limitation I noted earlier), it should be concluded that the theory of fundamental causes is discredited.

REVIEWER	Hisashi Eguchi Department of Public Health, Kitasato University School of Medicine
REVIEW RETURNED	16-Feb-2017

GENERAL COMMENTS	Altogether, the article was written well. I think that this manuscript is very interesting and significant. I think that the authors want to encourage policy makers to be interested in the socioeconomic inequalities in cancer mortality by publishing this paper. However, there are some issues that have to be addressed before any publication might be considered: 1. You might mention about the ethical approval. Since this study is secondary data analysis, I think the ethical approval would be exempted. 2. This paper is based on the Belgian people. About the results, what is the difference between this results and other countries (e.g., USA and UK)? Could you discuss it? From the generalizability of this results, reviewer considers that comparison between the countries is important. 3. P12, 1st paragraph: the authors discuss the effect of smoking on the cancer incidence. Can you present the smoking rate by occupation? If you presented it, the discussion would deepen more. And recently, a non-smoking area in the public space is spreading. I'm not sure the situation in Belgium. Do these kinds of environmental changes affect the occupational exposure that you discussed? 4. The choice of using managers and professional as the reference category is not well justified. It is difficult to interpret the results for other occupations when compared to managers and professionals, which is a very broad category and represents a group of workers who may also have relevant exposures that place them at higher risk of one or more of the evaluated cancers. This is not addressed at all in the Discussion or interpretation of the findings. Did you consider alternative approaches such as comparing the occupation-specific risks of death from these cancers to the overall population, or using alternative reference groups? How do the age-adjusted death rates from these cancers in Belgium overall compare to the age-adjusted mortality rates calculated in the paper for specific occupations? 5. How did the participants choose their occupation? By interview, or questionnaire? How do you think about the misclassification of the occupation? Does the misclassification affect the result? Please discuss it. 6. Dividing the cancers into two groups "preventable" and "non-preventable" is a unique idea. You seem not to mention about the effect of the cancer screening. I am not familiar with the cancer screening system in Belgium. Policy makers might be interested in the effect of the cancer screening on the cancer mortality by occupation. Could you discuss it? 7. Abstract Objective: Please indicate the full spell of SEP before using it. 8. Introduction 2nd paragraph: Please indicate the full spell of SE before using it.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1: Marlen Toch-Marquardt - Institution and Country: Institute of Political Science and Sociology, Norwegian University of Science and Technology, Norway

This is a very interesting article analysing occupational status inequalities in site specific cancer in the Belgian population, and examines whether occupational status is a fundamental cause of these inequalities. The authors present a very thoroughly prepared analysis and a well written paper. However, the manuscript could benefit from some changes and clarifications. Revise and resubmit would be my suggestion.

The comments are presented in chronological order.
Abstract:
- Please check the consistency of the use of SEP or SE (Objective and Conclusion part).
We have replaced the abbreviations by the full spelling.

- The Results part could be more precise.
We have amended this part.

Article summary:
- First paragraph, third line the authors write: "…, they may represent different aspects,…".
Please verify what these aspects are.
This sentence has been removed from the article summary (see before).

Data and Methods:
- Design and study population: Please check the correct use of tenses.
We checked and corrected the use of tenses.

- Design and study population: In the analysis the authors used persons which were between the ages of 25 to 65 years at the census in 1991. Since information on cancer mortality stems from the 2001 and 2011 register data, we are looking at a time lag of 10 respectively 20 years. Please explain how these persons were followed exactly and what happened to persons who had entered retirement age at the start of the analysis? The same might be true for the occupational status variable. Is there information of occupational status in the 2011 register data? (Paragraph 'Statistical analysis')
The starting point of our analysis is the 2001 census because we have a mortality follow-up for 2001-2011. We selected all Belgians that were alive at the 2001 census and who were supposed to be working ten years earlier (at the 1991 census), based on age (25-65 years). For these individuals, we have added information on employment and occupation, stemming from the 1991 census. This was the most recent detailed information on occupation, because in the census of 2001, this kind of information is only available for a sample of 20%. We do not believe that the time lag is a problem since cancer is a disease that shows a considerable lag time between exposure and disease onset. The final study population ranges from 35-85 years.

We do not take into account whether they are still employed or retired at the 2001 census, because we really want to study the association between occupation (only available in 1991) and cancer mortality.

- Variables, first paragraph, first line: A 'the' needs to be added in front of "total cancer mortality".
This has been corrected.

- Variables, first paragraph after the table, line 43: check spelling of 'retirees'.
This sentence has been removed.

- Variables, first paragraph after the table: The authors use a category of ‘unemployed’ in the analysis. I am wondering, since it is a fairly large group, if this group could be split up into unemployed which are actively looking for a job and inactive unemployed? These two groups of unemployed might have a quite different risk of cancer mortality, and since the authors took care to include as many different groups as possible into the analysis, this distinction might add some more breadth to it.
Thank you for this remark. We have made two important adaptations. 1) We split up the analyses: inequalities by employment group and by occupational categories. 2) For the analyses by employment group, we distinguish between four groups: employed; unemployed and looking for a job; unemployed and not looking for a job; and disabled. By doing this, we indeed split up the large group of unemployed as you suggested.

- Statistical analysis: As a reader I would be interested to see the sensitivity analyses. Is it possible to publish these as an online supplement? As a reviewer, however, I need to see these analyses, in order to get a look at the whole picture.
We have added the tables of the sensitivity analyses as a supplementary file.

- Statistical analysis: The authors use ASMR as a measure of absolute inequalities. The analysis would be more complete if another measure of absolute inequalities, e.g. rate difference, would be incorporated.
We agree that this is the best way to measure absolute inequalities. We decided to look at the overlap of the confidence intervals to see which groups have lower or higher mortality compared with other groups. We decided not to calculate a mortality rate difference because we believe that it is difficult to consider employment and occupational group as ordinal variables. Therefore, we did not calculate mortality rate differences, yet discussed the differences in ASMRs between the different groups in the text.

Results:
- Description of the study population: Please check the use of correct tenses.
Ok, this has been checked.

- Relative cancer mortality inequality by employment status, second paragraph: Please check correct use of commas.
We have checked and corrected this use of commas.

Discussion and Conclusion:
- Theoretical considerations on the main findings, first paragraph: Please check correct use of plural s.
We have checked this paragraph.

- Theoretical considerations on the main findings, third paragraph: Please note that the authors Toch-Marquardt et al. (6) did not include agricultural workers into their analysis.
Thank you for this remark. We use this reference to discuss the discrepancy between manual and non-manual, therefore we do not consider this as a problem.

- Theoretical considerations on the main findings, third paragraph: Please check your discussion for redundancies. In line 6-9 and further down in line 43-45 you are using the exact same explanation.
The redundant sentences have been removed.

- Theoretical considerations on the main findings, fourth paragraph: The authors make use of the acronym FCT throughout the entire manuscript, why change to Link and Phelan in this

paragraph?
We have changed this as proposed by the reviewer.

General Remarks:
- In the title the authors ask if occupational status is a fundamental cause of cancer mortality. However, this question is left unanswered in the conclusion. The manuscript would gain from a short statement about this issue in the conclusion.
We have adapted the title, which does not longer include the question but stresses the inclusion of both employment and occupation. We also have adapted the study design, as recommended by one of the reviewers. We now use the FCT more as a theory that explains the existence of health inequalities, rather than asking whether employment and occupation are fundamental causes of health inequalities (see further).

Reviewer 2: Enrique Regidor - Institution and Country: Universidad Complutense de Madrid, Spain
This paper presents a lot of ideas, but such ideas are not presented in a clear and understandable way. Nor are those ideas linked to another in a reasonable way.

First I point out something related with an incorrect comment from the authors in the introduction. It is not demonstrated that the socioeconomic position is a fundamental cause of the inequalities in health, observed in the rich countries in the last decades. What is documented is that there are inequalities in health. And several theories try to explain them. The theory of fundamental causes is one of them.
Thank you for this remark. We have adapted these sentences.

In the introduction the authors propose two theoretical frameworks whose joint approach throughout the article they fails to link well. On the one hand the authors try to contrast the theory of the fundamental causes with the mortality by diverse localizations of cancer. It is an interesting idea and sufficiently argued based on a multitude of previous works. And, on the other hand, the authors intend to evaluate the relation of the occupation with the mortality by diverse localizations of cancer, after eliminating the effect of other socioeconomic variables in that relation. This second idea is not sufficiently argued. Nor the relation of this idea with the contrast of the theory of fundamental causes is well argued.
Thank you for this remark. We have changed the study design to respond to your remark. With this paper, we want to study the net contribution of employment and occupational group to site-specific cancer mortality. We think this is important as 1) previous research has shown that different SEP indicators tap into different pathways between SEP and health; 2) most studies have focused on the association between education or income/housing and cancer mortality, while the association with employment/occupation has not yet been well documented, certainly not for Belgium. We decided to account for educational level and housing conditions (two other important SEP indicators that have been studied thoroughly), to assess the real contribution of employment and occupation to cancer mortality.

On the other hand, in the methodology there are some limitations. If the authors intend to study the relation of occupation with cancer mortality, why do they include the unemployed and others (incapacitated, as they point out in the discussion)? The selection bias due to health in both is clear. Do the authors really believe that the gap in the magnitude of the ratio of mortality rates in the unemployed to the magnitude of the ratio of mortality rates in the employed should be due to lifestyles or to less access to the health system? The explanation based on this would mean something incredible and practically impossible: that the prevalence of risk behaviours is very, very high among the unemployed and that the unemployed have little access to the health system. For this reason, the best option is to include the analysis only to people with occupation. Obviously the authors should

also have excluded housewives.
We have now altered the study design and divided it in two research aims: 1) to assess the association between employment group and cancer mortality, and 2) within the employed group: to assess the association between occupational group and cancer mortality. We hope that by doing this, we can meet your requests. By splitting this up, we want to account for the selection bias. We acknowledge that the results of the analysis by employment group is for a large part due to a health selection bias, which we also mention in the text. Although this is partly due to a selection bias, we still believe that it is important to document these inequalities, for example for policy measures such as financial security for the unemployed.

The authors say that they calculate absolute and relative measures of inequalities in cancer mortality. This is incorrect. The authors calculate a measure of frequency (mortality rates) and a relative measure of inequality in cancer mortality (rates ratio). The authors have not calculated an absolute measure of inequality in cancer mortality (rates difference).
Thank you for this remark. As mentioned earlier in a response to a comment of the previous reviewer, we acknowledge that using mortality rate differences are the best way to measure absolute inequalities. However, we decided not to calculate the mortality rate differences because we believe that it is difficult to consider employment and occupational group as ordinal variables. Therefore, we decided to look at the overlap of the confidence intervals of the ASMRs to see which groups have lower or higher mortality compared with other groups and we simply discussed the differences in ASMRs between the different employment and occupational groups in the text.

In the results section it is necessary to remind the authors that the title of their paper appears the word occupation, whereas in results the employment situation appears. I reiterate my previous comment.
Since we decided to divide the research question into two subquestions (one on inequalities by employment group and one by occupational group), we believe that this issue is no longer problematic. Furthermore, we also changed the title of the paper.

On the other hand, based on the first two paragraphs on page 10, it is difficult to get an idea of the main findings that have been found.
We have adapted the results section based on the two separate analyses.

In the discussion they do not address a crucial aspect and that it is an important limitation of the study. Many subjects will have died between 1991 and 2001, so that the subjects remaining in 2001 will be the healthiest. Therefore, there will be a strong selection bias in mortality in these subjects in the period 2001-2011 and, therefore, the probability of finding differences according to occupation (or according to any other variables) is lower.
Thank you for this remark. We believe that this is not a big issue since the starting point is the census of 2001. We have selected our study population based on being alive at this census and having reached the economically active age range ten years before, i.e. being 35-74 years at the 2001 census.

For these people, we have a follow-up period of 11 years (2001-2011) and we use the retrospective information on employment and occupation from 10 years earlier (census 1991) as a measure of SEP. We consider this time lag not as a problem because cancer is a typical disease with a time lag between exposure and disease onset, for instance in the case of lung cancer (which accounts for a large part of mortality and inequality).

On the other hand, as I pointed out above, the approach to the two objectives is not resolved and the discussion is unclear. For example, the authors speculate on a multitude of issues, particularly on page 12, but they do not fully state whether their findings support or do not support the theory of fundamental causes. Indeed, in the light of their findings (in case their findings were correct and did

not have the limitation I noted earlier), it should be concluded that the theory of fundamental causes is discredited.
Because we split the research aims in two sub-aims, we believe that the story is clearer now. Additionally, in the last part, we tried to be more conclusive about the findings.

Reviewer 3: Hisashi Eguchi - Institution and Country: Department of Public Health, Kitasato University School of Medicine

Altogether, the article was written well. I think that this manuscript is very interesting and significant. I think that the authors want to encourage policy makers to be interested in the socioeconomic inequalities in cancer mortality by publishing this paper. However, there are some issues that have to be addressed before any publication might be considered:

1. You might mention about the ethical approval. Since this study is secondary data analysis, I think the ethical approval would be exempted.
We have added a sentence on the ethical approval at the end of the text.

2. This paper is based on the Belgian people. About the results, what is the difference between this results and other countries (e.g., USA and UK)? Could you discuss it? From the generalizability of this results, reviewer considers that comparison between the countries is important.
Thank you for this remark. We agree that international comparison is important. In the discussion section, and mainly in the paragraph on theoretical considerations on the main findings, we compared our findings with those from similar studies in other European countries. Yet, a large difference is in the methods that are used. The way we operationalized occupation in relation to cancer mortality has not been often done (hence the importance of this study). Most studies use large categories of occupation or focus on the relation between a specific occupational exposure and a specific cancer site. Therefore, it is difficult to really compare our findings with other studies.

3. P12, 1st paragraph: the authors discuss the effect of smoking on the cancer incidence. Can you present the smoking rate by occupation? If you presented it, the discussion would deepen more. And recently, a non-smoking area in the public space is spreading. I'm not sure the situation in Belgium. Do these kinds of environmental changes affect the occupational exposure that you discussed?
Thank you for this remark. Unfortunately, the data we use are administrative data, hence we do not dispose of data on health behaviors such as smoking, as mentioned in the methodological issues section. However, in the future, we hope to establish a linkage between the data used in this study and data of the Belgian Health Interview Study, which will allow us to study the contribution of smoking more in depth, rather than making assumptions about it. Concerning the ban on smoking in public areas, in Belgium a ban on smoking in public areas and at work was implemented in 2010.

Consequently, this is not yet reflected in our results because of 1) the follow-up time of 2001-2011 and 2) the time lag between smoking exposure and disease onset (lung cancer) of at least two to three decades.

4. The choice of using managers and professional as the reference category is not well justified. It is difficult to interpret the results for other occupations when compared to managers and professionals, which is a very broad category and represents a group of workers who may also have relevant exposures that place them at higher risk of one or more of the evaluated cancers. This is not addressed at all in the Discussion or interpretation of the findings. Did you consider alternative approaches such as comparing the occupation-specific risks of death from these cancers to the

overall population, or using alternative reference groups? How do the age-adjusted death rates from these cancers in Belgium overall compare to the age-adjusted mortality rates calculated in the paper for specific occupations?
We have now split the analyses in inequalities by employment group and among the employed also inequalities by occupational group. By doing this, we want to enhance the interpretability of the analyses.
Concerning the inequalities by employment group we choose the employed as a reference category; for the inequalities by occupational group we choose managers and professionals as a reference category. These choices have been made based on these (practical) issues: 1) these are large groups, which is recommended to choose as a reference category; 2) we assumed lower cancer mortality for these groups, therefore choosing these groups would be easier to interpret (resulting in mortality rate ratios larger than one).

5. How did the participants choose their occupation? By interview, or questionnaire? How do you think about the misclassification of the occupation? Does the misclassification affect the result? Please discuss it.
The employment and occupational group of the participants is derived from the census of 1991, which is an administrative questionnaire covering the entire population. This census contains detailed information on the occupation itself, which was afterwards classified by the Belgian National Institute for Statistics in categories using the International Standard Classification of Occupations. We used broad categories, based on the 1-digit categories from the ISCO classification. Therefore, we assume that the misclassification bias will be very limited.

6. Dividing the cancers into two groups "preventable" and "non-preventable" is a unique idea. You seem not to mention about the effect of the cancer screening. I am not familiar with the cancer screening system in Belgium. Policy makers might be interested in the effect of the cancer screening on the cancer mortality by occupation. Could you discuss it?
Thank you for this interesting remark. This was indeed an issue that did not come up enough in the previous version. In this revised version we have emphasized that the inequalities are especially large for the cancer sites that are associated with adverse health behaviours such as smoking or alcohol use, and therefore will be more explained by these kind of factors as compared to cancer screening.

7. Abstract Objective: Please indicate the full spell of SEP before using it.
We have replaced the abbreviation by the full spelling.

8. Introduction 2nd paragraph: Please indicate the full spell of SE before using it.
The full spell of SE was mentioned in the first paragraph.

VERSION 2 – REVIEW

REVIEWER	Marlen Toch-Marquardt Department of Public Health and Nursing, Norwegian University of Science and Technology, Norway
REVIEW RETURNED	09-Jun-2017

GENERAL COMMENTS	I would like to congratulate the authors to this very much improved manuscript. The new design of the study highlights very well the importance of employment status and occupational status. I agree with all the changes that were made in the manuscript. I would like to recommend this article to be accepted for publication, however, there are to things that need to be taken care of first.

	Introduction: - Line 13: Please spell out SEP one time, before using the abbreviation. Discussion and Conclusion: - Paragraph 3, lines 10-15: large parts of these lines clearly cite Toch-Marquardt et al. 2014 (please see: Introduction, paragraph 3 in named article), however, this is only made clear in the first of three sentences this applies to. Please check the correctness of the citation.

REVIEWER	Hisashi Eguchi Department of Public Health, Kitasato University School of Medicine
REVIEW RETURNED	20-Jun-2017

GENERAL COMMENTS	Thank you for your dedication toward the revision of the paper. Your paper has been improved by the revision. However, I want to ask you to consider some points. 1. In the abstract and introduction section, you still use “SEP” without explaining of the full spelling “socioeconomic position.” Please add the explanation like following; socioeconomic position (SEP). 2. You explained you could not use the data on health behaviors. This is the important limitation of this study. The previous study pointed out it. Please refer the following link. https://www.cdc.gov/mmwr/preview/mmwrhtml/mm6038a2.htm. Please discuss it. 3. In the comments to the reviewers, you explained why you chose “managers and professionals” as a reference category. Please add the mention about the reason in the method section.

VERSION 2 – AUTHOR RESPONSE

Reviewer 1: Marlen Toch-Marquardt - Institution and Country: Institute of Political Science and Sociology, Norwegian University of Science and Technology, Norway

I would like to congratulate the authors to this very much improved manuscript. The new design of the study highlights very well the importance of employment status and occupational status.
I agree with all the changes that were made in the manuscript. I would like to recommend this article to be accepted for publication, however, there are two things that need to be taken care of first.

Introduction:
- Line 13: Please spell out SEP one time, before using the abbreviation.
We have replaced the abbreviation by the full spelling.

Discussion and Conclusion:
- Paragraph 3, lines 10-15: large parts of these lines clearly cite Toch-Marquardt et al. 2014 (please see: Introduction, paragraph 3 in named article), however, this is only made clear in the first of three sentences this applies to. Please check the correctness of the citation.

We have repeated the reference at the end of the referred text.

Reviewer 3: Hisashi Eguchi - Institution and Country: Department of Public Health, Kitasato University School of Medicine
Thank you for your dedication toward the revision of the paper. Your paper has been improved by the revision. However, I want to ask you to consider some points.

1. In the abstract and introduction section, you still use “SEP” without explaining of the full spelling “socioeconomic position.” Please add the explanation like following; socioeconomic position (SEP).
We have replaced the abbreviations by the full spelling.

2. You explained you could not use the data on health behaviors. This is the important limitation of this study. The previous study pointed out it. Please refer the following link. https://www.cdc.gov/mmwr/preview/mmwrhtml/mm6038a2.htm. Please discuss it.
Thank you for this reference. We have added a sentence reporting highest smoking rates among manual workers and people working in the catering industry, which is in line with our findings (see p. 14, lines 15-17).

3. In the comments to the reviewers, you explained why you chose “managers and professionals” as a reference category. Please add the mention about the reason in the method section.
We added the following mentioning in the text, see page 4, lines 20-23: We choose the employed as a reference category and among the employed the managers and professionals. These choices have been made because of the size of these groups, and because we assume lower cancer mortality for these groups, which facilitates the interpretation.

VERSION 3 – REVIEW

REVIEWER	Marlen Toch-Marquardt Norwegian University of Science and Technology, Norway
REVIEW RETURNED	22-Aug-2017

GENERAL COMMENTS	I agree with all the changes that were made in the manuscript. I would like to recommend this article to be accepted for publication. Kind regards.

REVIEWER	Hisashi Eguchi Department of Public Health, Kitasato University School of Medicine
REVIEW RETURNED	23-Aug-2017

GENERAL COMMENTS	Thank you for your effort and time to the revision. My concerns in the original draft resolved. Therefore, I think this draft can go to the next step for publication.