

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Educational differences in psychological distress? Results from a population-based sample of men and women in Sweden in 2012
AUTHORS	Molarius, Anu; Granström, Fredrik

VERSION 1 – REVIEW

REVIEWER	Sakari Suominen University of Turku, Finland University of Skövde, Sweden
REVIEW RETURNED	16-Jan-2018

GENERAL COMMENTS	<p>This is a paper of an important topic, namely educational differences of mental distress in a great sample from Mid-Sweden. However, as the manuscript now stands a continuous problem throughout the paper is that the relation, both the theoretical, i.e. conceptual and empirical, between mental distress and mental health is not clarified. The authors write as if these two concepts would be synonymous. both theoretically and empirically which they surely are not and this relation should be further elaborated and problemized in all chapters of the manuscript, including the Abstract, although there naturally only in a few words.</p> <p>In the limitations of the study I missed some reflections on the representativeness of their data in relation to Sweden and maybe also in relation to a broader context.</p> <p>On page 4 the authors write 'Mental health problems may thus have a mediating role in the association between education and self-rated health. Therefore it is important to examine educational differences in mental health.' I do not find that this in an optimal way justifies this study. Hence, as a consequence of what I have previously said I would rather instead of these two sentences write something like 'A corresponding social gradient in Sweden concerning mental health has also been found. Since mental distress can be understood as a proxy for mental health and could also be reflected in self-rated health, it is important to examine educational differences in mental health.' As far as I can see, the mediating role of mental health is not studied here.</p> <p>Minor comments I would not mention the term causal in the limitations at all since even in a prospective setting it would not be possible to say anything about causality, I would recommend to replace 'causal' with e.g. 'direction of influence between explanatory and outcome variables'. I would prefer the expression '.....lower response rates....' than</p>
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	<p>'.....higher non-response rates....' which is used at least twice. In the third sentence of the Discussion I would add the word respectively, 'The associations between educational level, economic difficulties and psychological distress, respectively, were rather similar in men and in women even though the prevalence of psychological distress was higher among women.</p> <p>Moreover, I would add to the caption of Table 1 the specification of the statistical models applied, i.e. multivariate logistic regression models and also describe in a more detailed way the Model 1 and 2 although a reader can understand the way the models differ from each other.</p>
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REVIEWER	<p>Caroline S. Duchaine 1-Laval University, Department of Social and Preventive Medicine, Quebec city, Canada 2-CHU de Québec-Université Laval Research Center, Population Health and Optimal Health Practices Unit, Quebec city, Canada</p>
REVIEW RETURNED	18-Jan-2018

GENERAL COMMENTS	<p>1. Is the research question or study objective clearly defined?</p> <p>Major comment</p> <p>#1: Contribution of the study to the current knowledge</p> <p>First, the contribution of the study to the current knowledge seems weak and not fully justified. The authors state that social inequalities in mental health are well documented and that a longitudinal study with the same objective was already done in Sweden. Thus, it is not clear what a cross-sectional study with the same objective and on a similar population could add to the state of knowledge (page 10, line 28-30):</p> <p>“Similar results on the lack of association between low educational level and psychological distress have, however, been reported from a longitudinal study in Sweden (13).”</p> <p>13. Kosidou K, Dalman C, Lundberg M, Hallqvist J, Isacsson G, Magnusson C. Socioeconomic status and risk of psychological distress and depression in the Stockholm Public Health Cohort: a population-based study. <i>J Affect Disord</i> 2011;134:160-7.</p> <p>One of the contributions of this study could be the use of two indicators of socioeconomic status (SES), namely education and economic difficulties, with the addition of the evaluation on gender difference in social inequalities in mental health. This point could be elaborated further and supported with references to reinforce the contribution of the study.</p> <p>Second, the authors justify the study of social inequalities in mental health because mental health problems could mediate the relationship between SES and self-rated health (SRH). This does not seem appropriate. SRH was not evaluating in this study and no analysis of mediation was done. I suggest removing this paragraph from the introduction. (Page 4, line 34-43):</p> <p>“In Sweden, there are large educational differences in self-rated health. Persons with low educational level have approximately twice as often poor self-rated health as persons with high educational level (5, 10). Mental health problems may thus have a mediating role in the association between education and self-rated health. Therefore it</p>
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	<p>is important to examine educational differences in mental health.”</p> <p>#2: The objective concerning the association between economic difficulties and psychological distress was not clearly defined.</p> <p>The objective on the association between education and psychological distress was clearly defined but this was not the case for the objective on the association between economic difficulties and psychological distress. This objective seems exploratory or secondary to the authors, but the rationale of this choice was not clear (page 5, line 5): “Also, the role of economic difficulties was investigated.”</p> <p>If this objective is important, it should be mentioned clearly and a literature review regarding this association should be summarized in the introduction in order to show the contribution of this objective.</p> <p>2. Is the abstract accurate, balanced and complete?</p> <p>The abstract was accurate and balanced, but incomplete as it lacked the objective about economic difficulties.</p> <p>3. Is the study design appropriate to answer the research question?</p> <p>A longitudinal study would be more appropriate. However, the authors were aware of this limit and do not pretend to study a causal relationship. If the objective of this study was only descriptive, a cross-sectional study seems appropriate.</p> <p>4. Are the methods described sufficiently to allow the study to be repeated? Yes</p> <p>5. Are research ethics (e.g. participant consent, ethics approval) addressed appropriately? Yes</p> <p>6. Are the outcomes clearly defined? Yes</p> <p>7. If statistics are used are they appropriate and described fully?</p> <p>Major comment</p> <p>#1 : The use of a Poisson robust model instead of logistic regression should be considered</p> <p>The measure of frequency of the outcome used in this study was the prevalence. Thus a Poisson robust model (generalized estimated equation with log link and Poisson distribution) could be more appropriate. Using this model, you would obtain prevalence ratios, which are more intuitive and easier to interpret than odds ratios. Furthermore, these models are in general more conservative, especially when the health problems are frequent.</p> <p>#2 : Few potential confounders were considered</p> <p>Only age and gender were considered as potential confounders in</p>
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	<p>the analyses. Are other variables available? For example, history of mental health problems, lifestyle habits, social support, etc. If possible, add more potential confounders with the assurance that there were no intermediate factors in order to avoid over adjustment. Furthermore, it was not clear if the models in Table 1 are mutually adjusted (i.e. model on education was adjusted for economic difficulties and model on economic difficulties was adjusted for education?). The following sentence needs clarification (page 6, lines 17-24): “Since the prevalence of mental health symptoms is higher in younger age groups (8) multivariate logistic regression models adjusting for age (in 10-year groups), and in a second model also for economic difficulties, were carried out.”</p> <p>Minor comment:</p> <p>Chi-squared statistics test with its p-value were not relevant in the descriptive analyses. As these are crude analyses, they are subject to confounding bias. Furthermore, as the sample size was large, detectable difference with this test should be small and non-clinically significant. I suggest removing these p-values in Table 1.</p> <p>8. Are the references up-to-date and appropriate?</p> <p>The references seem appropriate and up-to-date but this question is difficult to answer without doing a systematic literature review on the subject. As mentioned in point #1, there is a lack of literature review on the economic difficulties / psychological distress relationship.</p> <p>9. Do the results address the research question or objective?</p> <p>Results are brief and consist of only one table. I would appreciate at least one descriptive table of the study population. We do not know the age distribution of the population. As this variable is included in the analyses, it would be important to know its distribution. Moreover, as discussed in comment #7, there were very few potential confounders included in this study. If there are other variables available, please add it in a descriptive table in order to provide a better description of the study population.</p> <p>10. Are they presented clearly?</p> <p>Please add footnotes in Table 1 in order to identify the variables included in each model. Tables should be as self-explanatory as possible.</p> <p>11. Are the discussion and conclusions justified by the results?</p> <p>Major comment:</p> <p>I would appreciate a more in-depth discussion on the difference observed in the results between the two SES. Why are the associations stronger with economic difficulties than with education? Could it not be a reverse causal relationship? What is the implication for future research?</p> <p>Minor comment:</p> <p>As mentioned in comment #1, a discussion on the mediating effect of mental health problems in the relationship between SES and SRH</p>
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was irrelevant. I suggest removing this part from the discussion (Page 11, lines 13-30):

“Mental health problems may have a mediating role in the association between education and self-rated health. In this study, no inverse association between educational level and psychological distress was however found. On the basis of the results of this and other studies (8, 13), it is therefore improbable that mental health problems would explain, or even have a significant contribution to, educational inequalities in self-rated health, and even more general to health inequalities in Sweden. This applies to men and women alike. The notion that it is improbable that mental health problems would explain educational inequalities in self-rated health is further supported by the fact that psychosocial factors have a weaker contribution to educational inequalities in self-rated health than material factors (17).”

12. Are the study limitations discussed adequately?

First, a low participation rate among subjects with a low level of education and with psychological distress does not necessarily underestimate the effect. Considering DAGs theory, overestimation is also likely (1, 2). The following statement should be revised (Page 10 line 17-22):

“As people with poor health and/or low education often have higher non-response rates, this may lead to underestimation of educational differences in mental health.”

Please refer to:

1. Hernan MA, Hernandez-Diaz S, Robins JM. A structural approach to selection bias. *Epidemiology* (Cambridge, Mass). 2004 Sep;15(5):615-625.
2. VanderWeele TJ, Hernan MA. Results on differential and dependent measurement error of the exposure and the outcome using signed directed acyclic graphs. *American journal of epidemiology*. 2012 Jun 15;175(12):1303-1310.

Second, potential residual confounding were not addressed in the limits section. As the models were only adjusted for age and gender, residual confounding is likely to arise.

Finally, common method bias is likely in the association between economic difficulties and psychological distress as the two variables were self-reported at the same moment.

13. Is the supplementary reporting complete (e.g. trial registration; funding details; CONSORT, STROBE or PRISMA checklist)?

N/A

14. To the best of your knowledge is the paper free from concerns over publication ethics (e.g. plagiarism, redundant publication, undeclared conflicts of interest)?

Yes

15. Is the standard of written English acceptable for publication?

Yes

16. Does this paper require specialist statistical review?

Yes, see comment #7.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

This is a paper of an important topic, namely educational differences of mental distress in a great sample from Mid-Sweden. However, as the manuscript now stands a continuous problem throughout the paper is that the relation, both the theoretical, i.e. conceptual and empirical, between mental distress and mental health is not clarified. The authors write as if these two concepts would be synonymous. both theoretically and empirically which they surely are not and this relation should be further elaborated and problemized in all chapters of the manuscript, including the Abstract, although there naturally only in a few words.

- We have clarified in the background the concepts of mental health, mental health problems and more severe mental health disorders. We have deleted the concept common mental disorders as using too many different concepts is confusing. We have described that we use psychological distress (using GHQ-12) to measure less severe mental health problems, but that this indicator is also predictive of more severe mental disorders (p. 4-5). In addition, we have included a paragraph in the discussion (p. 14) where we clarify that previous studies that have used several measures of mental health problems have found similar associations between socioeconomic position and mental health problems irrespective of the indicator of mental health problems and even irrespective of severity.

In the limitations of the study I missed some reflections on the representativeness of their data in relation to Sweden and maybe also in relation to a broader context.

- We have addressed the representativeness of the data on p. 15 where we explain that the study area comprises about 1 million inhabitants in Sweden and that the prevalence of psychological distress was similar to the national average in Sweden.

On page 4 the authors write 'Mental health problems may thus have a mediating role in the association between education and self-rated health. Therefore it is important to examine educational differences in mental health.' I do not find that this in an optimal way justifies this study. Hence, as a consequence of what I have previously said I would rather instead of these two sentences write something like 'A corresponding social gradient in Sweden concerning mental health has also been found. Since mental distress can be understood as a proxy for mental health and could also be reflected in self-rated health, it is important to examine educational differences in mental health.' As far as I can see, the mediating role of mental health is not studied here.

- We agree that we did not analyse the mediating role of mental health problems in explaining educational differences in self-rated health in the study. Therefore, we have excluded this paragraph from the background. We have, however, kept the comment in the discussion (p. 15). Since mental health problems are strongly associated with self-rated health, educational differences in them might explain educational differences in SRH. But this would require an inverse association between educational level and mental health problems and since no inverse association was found between education and psychological distress in this study, the mediating role is unlikely. (See also reply to reviewer 2).

Minor comments

I would not mention the term causal in the limitations at all since even in a prospective setting it would not be possible to say anything about causality, I would recommend to replace 'causal' with e.g. 'direction of influence between explanatory and outcome variables'.

- We have now replaced “causal” with the “direction of the association”. We have also added some references from longitudinal studies that indicate that the main direction is from education and economic difficulties to mental health problems (p. 13).

I would prefer the expression '.....lower response rates....' than '.....higher non-response rates....' which is used at least twice.

- We agree and have replaced these with 'lower response rates'.

In the third sentence of the Discussion I would add the word respectively, 'The associations between educational level, economic difficulties and psychological distress, respectively, were rather similar in men and in women even though the prevalence of psychological distress was higher among women.

- We have added the word respectively (p. 12).

Moreover, I would add to the caption of Table 1 the specification of the statistical models applied, i.e. multivariate logistic regression models and also describe in a more detailed way the Model 1 and 2 although a reader can understand the way the models differ from each other.

- We have described the statistical method used and the models in more detail (please note that the former Table 1 is now Table 2).

Reviewer: 2

Please leave your comments for the authors below

1. Is the research question or study objective clearly defined?

Major comment

#1: Contribution of the study to the current knowledge

First, the contribution of the study to the current knowledge seems weak and not fully justified. The authors state that social inequalities in mental health are well documented and that a longitudinal study with the same objective was already done in Sweden. Thus, it is not clear what a cross-sectional study with the same objective and on a similar population could add to the state of knowledge (page 10, line 28-30):

“Similar results on the lack of association between low educational level and psychological distress have, however, been reported from a longitudinal study in Sweden (13).”

13. Kosidou K, Dalman C, Lundberg M, Hallqvist J, Isacson G, Magnusson C. Socioeconomic status and risk of psychological distress and depression in the Stockholm Public Health Cohort: a population-based study. *J Affect Disord* 2011;134:160-7.

- We have now added an elaboration to the background on what the previous studies in Sweden have investigated and what our study can add to the state of knowledge (p. 5).

One of the contributions of this study could be the use of two indicators of socioeconomic status (SES), namely education and economic difficulties, with the addition of the evaluation on gender difference in social inequalities in mental health. This point could be elaborated further and supported with references to reinforce the contribution of the study.

- We agree and have lifted the role of economic difficulties in the objective of the study. We have elaborated the description of these two indicators and added references from the relevant literature (pp. 4-5). Also the evaluation of gender differences was added to the aim.

Second, the authors justify the study of social inequalities in mental health because mental health problems could mediate the relationship between SES and self-rated health (SRH). This does not seem appropriate. SRH was not evaluating in this study and no analysis of mediation was done. I suggest removing this paragraph from the introduction. (Page 4, line 34-43):

“In Sweden, there are large educational differences in self-rated health. Persons with low educational level have approximately twice as often poor self-rated health as persons with high educational level (5, 10). Mental health problems may thus have a mediating role in the association between education and self-rated health. Therefore it is important to examine educational differences in mental health.”

- We agree that we did not do any explicit analysis of mediation of mental health problems in explaining educational differences in self-rated health in the study. Therefore, we have excluded this paragraph from the background. We have, however, kept the comment in the discussion (p. 15). Since mental health problems are strongly associated with self-rated health, educational differences in them might explain educational differences in SRH. But this would require an inverse association between educational level and mental health problems and since no inverse association was found between education and psychological distress in this study, the mediating role is unlikely. (See also reply to reviewer 1).

#2: The objective concerning the association between economic difficulties and psychological distress was not clearly defined.

The objective on the association between education and psychological distress was clearly defined but this was not the case for the objective on the association between economic difficulties and psychological distress. This objective seems exploratory or secondary to the authors, but the rationale of this choice was not clear (page 5, line 5):

“Also, the role of economic difficulties was investigated.”

If this objective is important, it should be mentioned clearly and a literature review regarding this association should be summarized in the introduction in order to show the contribution of this objective.

- As mentioned earlier, we have lifted the role of economic difficulties in the objective of the study. A short literature review concerning this point was added (pp. 4-5). We have also extended the discussion of the findings regarding the association between economic difficulties and mental health problems (pp. 12-13).

2. Is the abstract accurate, balanced and complete?

The abstract was accurate and balanced, but incomplete as it lacked the objective about economic difficulties.

- The role of economic difficulties was added to the objective of the study in the abstract.

3. Is the study design appropriate to answer the research question?

A longitudinal study would be more appropriate. However, the authors were aware of this limit and do not pretend to study a causal relationship. If the objective of this study was only descriptive, a cross-sectional study seems appropriate.

4. Are the methods described sufficiently to allow the study to be repeated?

Yes

5. Are research ethics (e.g. participant consent, ethics approval) addressed appropriately?

Yes

6. Are the outcomes clearly defined?

Yes

7. If statistics are used are they appropriate and described fully?

Major comment

#1 : The use a Poisson robust model instead of logistic regression should be considered

The measure of frequency of the outcome used in this study was the prevalence. Thus a Poisson robust model (generalized estimated equation with log link and Poisson distribution) could be more appropriate. Using this model, you would obtain prevalence ratios, which are more intuitive and easier to interpret than odds ratios. Furthermore, these models are in general more conservative, especially when the health problems are frequent.

- We agree that Poisson robust models would be more appropriate when studying prevalences. However, since our main aim was not to measure the size of the associations between educational level, economic difficulties and psychological distress but to analyse if they are associated and since logistic regression models are more known and widely used, we decided not to replace the logistic regression models.

#2 : Few potential confounders were considered

Only age and gender were considered as potential confounders in the analyses. Are other variables available? For example, history of mental health problems, lifestyle habits, social support, etc. If possible, add more potential confounders with the assurance that there were no intermediate factors in order to avoid over adjustment. Furthermore, it was not clear if the models in Table 1 are mutually adjusted (i.e. model on education was adjusted for economic difficulties and model on economic difficulties was adjusted for education?). The following sentence needs clarification (page 6, lines 17-24):

“Since the prevalence of mental health symptoms is higher in younger age groups (8) multivariate logistic regression models adjusting for age (in 10-year groups), and in a second model also for economic difficulties, were carried out.”

- After careful consideration, we have now added two confounders, employment status and social support, and revised the abstract, methods, tables, results and discussion correspondingly. We did not have data on history of mental health problems and adjusting for example for physical activity had no impact on the results. Adding the two confounders changed the results to some extent, the odds ratio between economic difficulties and psychological distress was somewhat attenuated but remained strong and the odds ratio for low educational level became statistically significantly below

zero. We have added a discussion on these limitations and on how to interpret the results to the discussion section (pp. 13-14).

- We have also clarified in the methods section and in Table 2 (former Table 1) the models applied.

Minor comment:

Chi-squared statistics test with its p-value were not relevant in the descriptive analyses. As these are crude analyses, they are subject to confounding bias. Furthermore, as the sample size was large, detectable difference with this test should be small and non-clinically significant. I suggest removing these p-values in Table 1.

- We agree and have deleted the p-values.

8. Are the references up-to-date and appropriate?

The references seem appropriate and up-to-date but this question is difficult to answer without doing a systematic literature review on the subject. As mentioned in point #1, there is a lack of literature review on the economic difficulties / psychological distress relationship.

- A literature review concerning this point was added (pp. 4-5) (see also our comment to points #1-2).

9. Do the results address the research question or objective?

Results are brief and consist of only one table. I would appreciate at least one descriptive table of the study population. We do not know the age distribution of the population. As this variable is included in the analyses, it would be important to know its distribution. Moreover, as discussed in comment #7, there were very few potential confounders included in this study. If there are other variables available, please add it in a descriptive table in order to provide a better description of the study population.

- We have added a new Table 1 to describe the study population and the new confounders.

10. Are they presented clearly?

Please add footnotes in Table 1 in order to identify the variables included in each model. Tables should be as self-explanatory as possible.

- We have described the statistical method used and the models in more detail (please note that the former Table 1 is now Table 2).

11. Are the discussion and conclusions justified by the results?

Major comment:

I would appreciate a more in-depth discussion on the difference observed in the results between the two SES. Why are the associations stronger with economic difficulties than with education? Could it not be a reverse causal relationship? What is the implication for future research?

- We have added a discussion on the difference between the two measures of SES, their proximity and why the results may differ (pp. 12-13). We have also added some references from longitudinal studies that indicate that the main direction is from education and economic difficulties to mental health problems (13). We have also added a reflection on future studies (p. 16).

Minor comment:

As mentioned in comment #1, a discussion on the mediating effect of mental health problems in the relationship between SES and SRH was irrelevant. I suggest removing this part from the discussion (Page 11, lines 13-30):

“Mental health problems may have a mediating role in the association between education and self-rated health. In this study, no inverse association between educational level and psychological distress was however found. On the basis of the results of this and other studies (8, 13), it is therefore improbable that mental health problems would explain, or even have a significant contribution to, educational inequalities in self-rated health, and even more general to health inequalities in Sweden. This applies to men and women alike. The notion that it is improbable that mental health problems would explain educational inequalities in self-rated health is further supported by the fact that psychosocial factors have a weaker contribution to educational inequalities in self-rated health than material factors (17).”

- See our reply to comment #1.

12. Are the study limitations discussed adequately?

First, a low participation rate among subjects with a low level of education and with psychological distress does not necessarily underestimate the effect. Considering DAGs theory, overestimation is also likely (1, 2). The following statement should be revised (Page 10 line 17-22):

“As people with poor health and/or low education often have higher non-response rates, this may lead to underestimation of educational differences in mental health.”

Please refer to:

1. Hernan MA, Hernandez-Diaz S, Robins JM. A structural approach to selection bias. *Epidemiology* (Cambridge, Mass). 2004 Sep;15(5):615-625.
2. VanderWeele TJ, Hernan MA. Results on differential and dependent measurement error of the exposure and the outcome using signed directed acyclic graphs. *American journal of epidemiology*. 2012 Jun 15;175(12):1303-1310.

- We agree and have added this comment, including reference 1, to the discussion (p. 13).

Second, potential residual confounding were not addressed in the limits section. As the models were only adjusted for age and gender, residual confounding is likely to arise.

- As mentioned above, we have added two potential confounders to the analysis. We have also added a comment on the possibility of further residual confounding to the discussion (p. 14).

Finally, common method bias is likely in the association between economic difficulties and psychological distress as the two variables were self-reported at the same moment.

- A comment on possible common method bias was added to the limitations mentioned in the discussion (p. 14)

13. Is the supplementary reporting complete (e.g. trial registration; funding details; CONSORT, STROBE or PRISMA checklist)?

N/A

14. To the best of your knowledge is the paper free from concerns over publication ethics (e.g. plagiarism, redundant publication, undeclared conflicts of interest)?

Yes

15. Is the standard of written English acceptable for publication?

Yes

16. Does this paper require specialist statistical review?

Yes, see comment #7.

- See our reply to comment #7.

VERSION 2 – REVIEW

REVIEWER	Sakari Suominen University of Turku, Department of Public Health, Finland University of Skövde, School of Health and Education, Sweden
REVIEW RETURNED	27-Mar-2018

GENERAL COMMENTS	The authors have sufficiently responded to my comments and according to my opinion, also to the comments made by the other reviewer.
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REVIEWER	Caroline S. Duchaine CHU de Québec-Université Laval Research Center Population Health and Optimal Health Practices Unit Laval University, Department of social and preventive medicine, Quebec city, Canada
REVIEW RETURNED	28-Mar-2018

GENERAL COMMENTS	All my previous comments have been taken into account by the authors in this revised version. The main limitation of this study is the cross-sectional design. However, the association between economic difficulties and mental health problems was rarely evaluated in prospective studies, so this paper bring some contribution to the state of knowledge.
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