PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Socioeconomic position, symptoms of depression, and subsequent mental health care treatment: a Danish register-based six-month follow-up study on a population survey.
AUTHORS	Packness, Aake Halling, Anders; Hastrup, Lene; Simonsen, Erik; Wehberg, Sonja; Waldorff, Frans

VERSION 1 – REVIEW

REVIEWER	Darrell Hudson Washington University in St. Louis, USA
REVIEW RETURNED	29-Dec-2017

GENERAL COMMENTS	This paper examined mental health utilization and whether SEP, defined by education, affected diagnosis and treatment of depression. The strength of the study is use of the Danish National Health Service Register for Primary Care to assess mental health utilization rather than relying only on self-report. The authors also adjusted for patients receiving current depression treatment. However, there were some major issues with the paper that must be addressed. Was education the only SEP indicator? I would recommend re-labeling SEP as education if there are no other indicators available. Also, I might consider using more intuitive education categories rather than short, long, etc. Or perhaps just create numbered levels. Explain how data were collected. For instance, how was the MDI was administered? Provide clearer information about the dependent variable as well as the additional variables that were adjusted for in the analysis (e.g. age, gender, marital status). How were each of these measured and how were the data obtained? Were number of visits (e.g. GP visits) adjusted for in the analysis? Even though current treatment/ diagnosis of depression is adjusted for in the analysis, I am not certain that this is adequate enough to disentangle potential effects. For instance, anti-depressants have physiological effects that must "wash out." This will not occur by simply adjusting for this influence in the analysis. Why was the highest level of education combined? I am not convinced by the authors' rationale to focus only on education. The remarkable piece, to me, of the findings was that depression needs were addressed by providers and there were no differences by education. The piece about expectation about educational differences (e.g. increasing sample size) seems slightly overstated/ incongruent with what the data are actually saying. Especially since there are a variety of factors, such as stigma, that are not studied in this analysis. Some of the authors' claims are unsupported by their own data. For

	instance, the discussion about the CIDI and estimating prevalence rates is superfluous to what the authors have found. There is no discussion of theoretical or methodological issues about estimating national prevalence rates in the introduction. And there are problems with the data that are presented, so I would recommend sticking with characterizing the data being analyzed. Another major factor (re: need and no use) would be the social causation perspective (e.g. Dohrenwend) which suggest that low SES predicts poor mental health. While the researchers don't have longitudinal data stretching back decades, there seems that it would worthwhile in discussing important explanations.

REVIEWER	Amber Gum, Ph.D. University of South Florida, USA
REVIEW RETURNED	27-Feb-2018

GENERAL COMMENTS	This manuscript describes analyses predicting mental health care treatment for adults with symptoms of depression, according to educational level. Although there are some strengths of the study (prospective, register-based data on service use), there are several concerns related to the writing and presentation of results. 1. In the title and many places, the authors use causal language, such as "impact," "determining," or "influencing." The data are correlational; as such, this language should be revised. 2. The title and much of the writing is misleading in its use of SEP; only a variable of education is used as a predictor. It is recommended that the authors use education in the title and elsewhere, and then they may discuss how education is related to other aspects of SEP. 3. This reviewer found the entire manuscript difficult to read, with key points and terms not stated clearly. There were awkward phrases throughout (e.g., in Abstract, outcomes of different units are mixed together; please note this is only one example). Variables were not clearly described, and results were not presented or discussed in a clear, straightforward manner in the abstract or main body of the text. 4. Introduction: there is a large body of research on service use by depressed adults, including many studies that document disparities by education and other aspects of SEP. This research is largely omitted from the manuscript. 5. Methods: Explain current treatment more clearly. Why was four months prior to the index date chosen? 6. Methods: Explain the outcome variables more clearly. Explain that the data were used to create 5 binary outcome variables, from the index date through what ending time point? 7. Results: Explain in more detail why the sample was reduced by the 1,627. 8. Results: Explain in more detail about the representativeness by education level (second paragraph of the Results; where do these data come from?). 9. Results: In the third paragraph, who is being compared? 10. Results: It seems Table 1 should present inferential statistics, if differences are being discussed in the text.

	11. Results and Discussion: There are many results presented, and the authors seem to choose only some of them to present in the abstract and discussion. I realize that not all results can be discussed in detail, but the overall picture seems incomplete. Specifically, the authors seem to focus on the fact that more highly educated individuals use more specialized services, which is an important health disparity issue, to be sure, although it should be placed in the context that those with more education and no/few symptoms were less likely to receive services at all. While this is the case, many other results seem to suggest fairly reasonable service delivery overall, with no differences in overall use by educational level for those with more moderate-severe symptoms (Table 2) and those with more severe symptoms receiving a higher level of treatment (Table 4). Table 3 suggests that those with less education are more likely to be offered new GP consult beginning after the index date. 12. Results and Discussion: It seems inaccurate to describe those with no/few symptoms of depression as being in "no need" – at least without explaining in more detail. It seems that these could have been individuals who had depression more than 4 months before the index date, whose depression improved, and who were on maintenance treatment. Could this be the case? If not, why not? Explain more. 13. Discussion: The authors could shorten the description of the findings in this section, and discuss policy implications in greater detail. It seems that there is some good news (see comment #11 above) and that some aspects of service delivery and policy are working (e.g., training and reimbursing GPs for basic counseling, perhaps; also that those with lower education are receiving more services overall (Tables 2 and 3). There does seem to be a disparity in terms of access to specialty services, and one implication of this might be integrating more brief behavioral interventions into the GP's setting that extend beyond the GP's brief counseling. There are many references regarding this trend that could be mentioned here.

VERSION 1 – AUTHOR RESPONSE

Reviewer #1

This paper examined mental health utilization and whether SEP, defined by education, affected diagnosis and treatment of depression. The strength of the study is use of the Danish National Health Service Register for Primary Care to assess mental health utilization rather than relying only on self-report. The authors also adjusted for patients receiving current depression treatment. However, there were some major issues with the paper that must be addressed.

1: Was education the only SEP indicator? I would recommend re-labeling SEP as education if there are no other indicators available.
We initially used education for simplicity. We do agree it would have been more correct to use the term education instead of SEP. But since we did have access to data on income as well, we conducted additional analyses using income as a variable. We found almost the same outcomes, except for contact of specialized services by respondents with none/few symptoms.

We have added income in the analysis and made major changes to address SEP as a multifaceted concept. When income and education have the same association with an outcome, we address it as an association with the SEP; otherwise the association is addressed by the variable in question (income/education).

2: Also, I might consider using more intuitive education categories rather than short, long, etc. Or perhaps just create numbered levels.
We have changed the terms of education into *no postsecondary education*, *1-3 years of postsecondary education*, and *3+ years postsecondary education*. We hope this is more understandable.

3: Explain how data were collected. For instance, how was the MDI was administered?
We have adjusted the text to clarity, adding: "Data from the self-administered GESUS questionnaire was used in the present study." Later in the text we also added: "Data on all independent variables came from GESUS."

4: Provide clearer information about the dependent variable as well as the additional variables that were adjusted for in the analysis (e.g. age, gender, marital status). How were each of these measured and how were the data obtained?
We have adjusted the text. We have added: "Data on dependable variables was drawn from national registers."

We also numbered the stepwise hierarchy of treatment intensity to be more explicit in the Method section under *Dependent variables* and *Statistical analysis from #0 - #7.*

5: Were number of visits (e.g. GP visits) adjusted for in the analysis?
The adjustment was only for contacting a psychologist or psychiatrist, or prescription of antidepressants four months prior to index date. For income we have adjusted for cohabitation, too. This is clarified in the manuscript.

6: Even though current treatment/ diagnosis of depression is adjusted for in the analysis, I am not certain that this is adequate enough to disentangle potential effects. For instance, anti-depressants have physiological effects that must "wash out." This will not occur by simply adjusting for this influence in the analysis.
We are not sure we fully understand this remark; however, we would not expect a "wash out" effect to differ between socioeconomic groups.

7: Why was the highest level of education combined?
Originally we had five educational levels, as seen in Table 1. We combined them in order to gain power for the statistical analysis and create groups of approximately equal sizes.

8: I am not convinced by the authors' rationale to focus only on education.
We accept this critique fully. Please refer to the answer in our first response.

9: The remarkable piece, to me, of the findings was that depression needs were addressed by providers and there were no differences by education. The piece about expectation about educational differences (e.g. increasing sample size) seems slightly overstated/ incongruent with what the data are actually saying. Especially since there are a variety of factors, such as stigma, that are not studied in this analysis.
We agree and have removed that part.

10: Some of the authors' claims are unsupported by their own data. For instance, the discussion about the CIDI and estimating prevalence rates is superfluous to what the authors have found. There is no discussion of theoretical or methodological issues about estimating national prevalence rates in the introduction. And there are problems with the data that are presented, so I would recommend sticking with characterizing the data being analyzed.
We agree and have consequently removed that part and focused on our own data.

11: Another major factor (re: need and no use) would be the social causation perspective (e.g. Dohrenwend) which suggest that low SES predicts poor mental health. While the researchers don't have longitudinal data stretching back decades, there seems that it would worthwhile in discussing important explanations.
We address this in the discussion and added the following sentence: "Similar findings were shown in another Australian study, where low SEP was associated with higher prescription rates not attributable to higher rates of depression. The most plausible reason for this association is that depressive disorders are more prevalent in this group and antidepressants are the first choice of treatment, or that antidepressants are more commonly used as analgesic medications in this group, as chronic pain is more common for persons in low SEP."

Reviewer #2

This manuscript describes analyses predicting mental health care treatment for adults with symptoms of depression, according to educational level. Although there are some strengths of the study (prospective, register-based data on service use), there are several concerns related to the writing and presentation of results.

1. In the title and many places, the authors use causal language, such as "impact," "determining," or "influencing." The data are correlational; as such, this language should be revised.
We agree with this and have revised the language accordingly.

2. The title and much of the writing is misleading in its use of SEP; only a variable of education is used as a predictor. It is recommended that the authors use education in the title and elsewhere, and then they may discuss how education is related to other aspects of SEP.
Please see comment above to reviewer #1, comment #1.

3. This reviewer found the entire manuscript difficult to read, with key points and terms not stated clearly. There were awkward phrases throughout (e.g., in Abstract, outcomes of different units are mixed together; please note this is only one example). Variables were not clearly described, and results were not presented or discussed in a clear, straightforward manner in the abstract or main body of the text.
We have tried to be more explicit in defining and presenting terms and definitions, and the manuscript has been proofread by a professional before resubmitting.

4. Introduction: there is a large body of research on service use by depressed adults, including many studies that document disparities by education and other aspects of SEP. This research is largely omitted from the manuscript.
There are several studies on depression and health service use or depression and SEP but there are not many studies on SEP, depression, and type of health services used – at least not recently. Additionally, the studies are either on use of health care services or antidepressants, but not combined. We have added the following (with references): "As for depression and anxiety disorders, some studies have found access to specialist care to be reflective of clinical need, with little inequity in SEP, whereas others report specialized mental health services are not provided to persons in low SEP according to their need, or that higher SEP is associated with more use of specialized mental health services."

5. Methods: Explain current treatment more clearly. Why were four months prior to the index date chosen? We have adjusted the text: "The period of four months was chosen assuming active treatment would include a treatment contact or renewed prescription within every three to four months, at least."

6. Methods: Explain the outcome variables more clearly. Explain that the data were used to create 5 binary outcome variables, from the index date through what ending time point?
We have adjusted the text and added under objectives: "The objective was to examine if the severity of symptoms of depression (need) was associated with the mental health care treatment received,

independently of SEP, in both type and frequency of treatments, and highest gained treatment level within six months following a symptom score received in a survey study."

Under *Method* we have added: "*Design*: A six-month follow-up study on respondents with symptoms of depression, combining survey data with register data on mental health care treatment."

7. Results: Explain in more detail why the sample was reduced by the 1,627.
It was a practical decision, because data for the year 2009 were not available.

We have adjusted the text to clarify this issue: "Data from national registers covered the years 2010 – 2014 in order to fit a timeframe of four months prior to index date; however, the sample was reduced to include only respondents entering the GESUS study from May 2010, due to lack of data availability from 2009. The period of four months was chosen assuming active treatment would include a treatment contact or renewed prescription within every three to four months, at least."

8. Results: Explain in more detail about the representativeness by education level (second paragraph of the Results; where do these data come from?).
We have adjusted the text to clarify. We performed a comparison with data from Statistics Denmark on the population in the municipality of Næstved to describe the sample as somewhat better-off than the population they were sampled from.

9. Results: In the third paragraph, who is being compared?
Thank you for mentioning this. It is not relevant, we have removed this text.

10. Results: It seems Table 1 should present inferential statistics, if differences are being discussed in the text.
Thank you for mentioning this. The section has been removed.

11. Results and Discussion: There are many results presented, and the authors seem to choose only some of them to present in the abstract and discussion. I realize that not all results can be discussed in detail, but the overall picture seems incomplete. Specifically, the authors seem to focus on the fact that more highly educated individuals use more specialized services, which is an important health disparity issue, to be sure, although it should be placed in the context that those with more education and no/few symptoms were less likely to receive services at all. While this is the case, many other results seem to suggest fairly reasonable service delivery overall, with no differences in overall use by educational level for those with more moderate-severe symptoms (Table 2) and those with more severe symptoms receiving a higher level of treatment (Table 4). Table 3 suggests that those with less education are more likely to be offered new GP consult beginning after the index date.
We have re-written this section to focus on these issues, as suggested.

12. Results and Discussion: It seems inaccurate to describe those with no/few symptoms of depression as being in "no need" – at least without explaining in more detail. It seems that these could have been individuals who had depression more than 4 months before the index date, whose depression improved, and who were on maintenance treatment. Could this be the case? If not, why not? Explain more.
The issue is addressed in the revised text. We did adjust for *present treatment*, so maintenance treatment is not a likely scenario, but it could be recurrent depression (and treatment) which would be more common in low SEP where depression is more prevalent. We suggest antidepressants used as analgesic treatment could be an explanation too.

13. Discussion: The authors could shorten the description of the findings in this section, and discuss policy implications in greater detail. It seems that there is some good news (see comment #11 above) and that some aspects of service delivery and policy are working (e.g., training and reimbursing GPs for basic counseling, perhaps; also that those with lower education are receiving more services overall (Tables 2 and 3). There does seem to be a disparity in terms of access to specialty services, and one implication of this might be integrating more brief behavioral interventions into the GP's setting that

extend beyond the GP's brief counseling. There are many references regarding this trend that could be mentioned here.
We have rewritten this section to address this issue.

VERSION 2 – REVIEW

REVIEWER	Amber Gum, Ph.D. University of South Florida, United States
REVIEW RETURNED	25-Apr-2018

GENERAL COMMENTS	The manuscript is significantly improved, and the authors seem to have responded to the reviewers' comments.

REVIEWER	Darrell Hudson Washington University in St. Louis, USA
REVIEW RETURNED	17-May-2018

GENERAL COMMENTS	Strengths of this study include the combination of survey data with register data on mental health service use. This approach eliminated recall bias from respondents. The researchers also used multiple indicators of SEP. Another strength is the study was conducted in a country with universal health care (although mental health care is only partially covered). Why was there a separate sampling procedure for those aged ≥ 30 versus those between 20-30? The writing is not as clear as it can be in a number of places throughout the paper and should be edited for both clarity and to improve efficiency. For instance, page 9 line 19-23. This sentence is long and confusing. Similarly, the results section needs to be reorganized to more clearly delineate findings. The tables tell the story but the text is confusing. The key takeaways, although drawn from a unique set of data, are not novel. The authors should also cover the seminal work of Dohrenwend and colleagues as well as Muntaner on SEP and mental health.

VERSION 2 – AUTHOR RESPONSE

Response to the remarks by reviewer 1

1) Why was there a separate sampling procedure for those aged ≥ 30 versus those between 20-30? Why only ¼ of the population in the age group 20-29 were invited is not mentioned explicitly in the description of GESUS, but the focus is on multi-morbidity which is not frequent among young people. We have added the aim of the GESUS, as it was stated by the initiators.

2) The writing is not as clear as it can be in a number of places throughout the paper and should be edited for both clarity and to improve efficiency. For instance, page 9 line 19-23. This sentence is long and confusing. We have rephrased throughout the manuscript. Please look at the manuscript.

3) Similarly, the results section needs to be reorganized to more clearly delineate findings. The tables tell the story but the text is confusing. We have rearranged, rephrased and specified the description of the results presented in table 1, 2 and 3, and hope to have succeeded to do this in a more logic manner.

4) The key takeaways, although drawn from a unique set of data, are not novel. We investigated our aim using a novel combination of unique data. The result of the analysis was in accordance with our main hypothesis, however we could not know that beforehand. We believe that our results are important and give insight into how the GP's are working as gatekeepers in a health system as the Danish.

5) The authors should also cover the seminal work of Dohrenwend and colleagues as well as Muntaner on SEP and mental health. Thank you for mentioning these authors. We have now referred to Dohrenwend and Muntaner's work on social causation in the introduction.