

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Sickness absence and disability pension before and after first childbirth and in nulliparous women: longitudinal analyses of three cohorts in Sweden
<b>AUTHORS</b>	Björkenstam, Charlotte; Orellana, Cecilia; Laszlo, Krisztina; Svedberg, Pia; Voss, Margaretha; Lidwall, Ulrik; Lindfors, Petra; Alexanderson, Kristina

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Jaana Halonen Finnish Institute of Occupational Health, Finland
<b>REVIEW RETURNED</b>	22-May-2019

<b>GENERAL COMMENTS</b>	<p>Sickness absence and disability pension in relation to first childbirth: three cohorts of women in Sweden</p> <p>bmjopen-2019-031593</p> <p>This is a nice descriptive paper on disability pension and SA comparing women who gave birth and who did not. The findings clearly suggest that there is selection into not giving birth among those with DP, which is expected; if you're not well, you're not likely to get pregnant. And perhaps there is selection also among those who decide to have another child, as those with more than one delivery had the lowest numbers of SA days suggesting they are healthier overall than the other groups. It would be interesting to see if SA varied between the three groups when those with DP were excluded. Then, healthy women would be compared to each other based on their delivery status and the results for SA might look different.</p> <p>Abstract</p> <ol style="list-style-type: none"><li>1. Conclusions should not only repeat the results. Here the authors could mention the possible selection regarding those with DP somehow, as well as the reliance to medically certified SA (there might be more short-term SA among those with small children). On page 14 the selection has been nicely described.</li></ol> <p>Introduction</p> <ol style="list-style-type: none"><li>1. Page 5. I believe that in vitro fertilization may affect also SA prevalence before the pregnancy as the treatments may require SA.</li><li>2. Page 5, Please clarify the comparison group in: "combination of paid and unpaid work is one reason why women have higher levels of SA." Higher than men?</li><li>3. Page 5. What is the length of SA that was examined in reference 23? Did it include also short-term SA?</li></ol>
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	<p>4. Page 5, line 38. Perhaps remove word “Nevertheless” as the following sentence starts with “However” and two such sentences do not link well together.</p> <p>Results</p> <p>1. Table 1 might read better if it only included percentages for the characteristics when the total N for each group is given on the first row.</p> <p>2. Table 2 is large and a bit difficult to read. It might look nicer as a figure with years on the x-axis and percentages on the y-axis and B0, B1 and B1+ as the grouping variables. As there are many groups and sub-groups, perhaps divide information into 3 panels: total; DP recipients; SA recipients. This would also show some trends, if any.</p> <p>3. Figure 1 is missing unit for the y-axis.</p> <p>Discussion</p> <p>1. Page 15, line 10 the paragraph beginning with: “In our study, women aged 30 years...” only repeats the results. It should be linked to the literature or its meaning should be discussed. Otherwise it could be omitted.</p> <p>2. Page 15. There is sentence: “... had fewer days of both SA and DP up until Y+2 when the levels became very similar, as compared to the other groups. The most plausible explanation for this is SA due to their subsequent pregnancy.” What does “this” refer to in the latter sentence? That the levels of SA became similar in the B1+ and other groups in Y+2? Might it read better if you said something like: “We found that women having a subsequent childbirth during the follow-up (B1+), had fewer days of both SA and DP at Y+1 than B0 and B1, but from Y+2 the levels were similar to the other groups, possibly due to the new pregnancy.”</p> <p>3. Page 15. Please discuss the possible reasons for: “the levels of SA/DP combined increased in a graded manner from Cohort1995 to Cohort2000 and was highest in Cohort2005.”</p> <p>4. Page 16. The lack of short sickness absence spells was mentioned. I think this needs some further discussion as small children are known to be vulnerable to many infections that the parents may also caught. While these flus and stomach flus may not last many days, they may be re-current among parents of small children. In addition, is it possible in Sweden to call in sick for the sickness a child? If so, this may also add to the absence days even if the mother herself was not sick.</p> <p>5. Page 16, the last sentence suggests that “more research with longer follow-up periods after childbirth is needed”. What would you expect to see with longer follow-ups? I don’t know if the health consequences of pregnancy itself last for many years, so wouldn’t it be the same if adults with and without children were compared? Perhaps consider omitting the last sentence.</p> <p>6. I would like to see selection included in the conclusions section as there is no possibility to estimate the causality of the findings.</p>
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<b>REVIEWER</b>	Peter Noone Occupational health service HSE DNE Ireland
<b>REVIEW RETURNED</b>	16-Jun-2019

<b>GENERAL COMMENTS</b>	interesting findings be interesting to look at Short term absence <15 days
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## VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Jaana Halonen

Institution and Country: Finnish Institute of Occupational Health, Finland

Please state any competing interests or state 'None declared': None declared

This is a nice descriptive paper on disability pension and SA comparing women who gave birth and who did not. The findings clearly suggest that there is selection into not giving birth among those with DP, which is expected; if you're not well, you're not likely to get pregnant. And perhaps there is selection also among those who decide to have another child, as those with more than one delivery had the lowest numbers of SA days suggesting they are healthier overall than the other groups. It would be interesting to see if SA varied between the three groups when those with DP were excluded. Then, healthy women would be compared to each other based on their delivery status and the results for SA might look different.

Response: Thank you for this suggestion for conducting one or several other studies. The aim of this study was not to investigate associations of morbidity with SA and/or DP in our three exposure groups. For such a study we would have needed data on e.g., previous morbidity, which would be another study. Moreover, we do not consider SA and DP to be good measures of morbidity. Instead, SA and DP are good measures of social consequences of morbidity, in terms of not being able to work part- or full-time. Actually, most people with different diagnoses are not on SA or DP – also some of them having, what in medicine, is considered severe diagnoses.

Another reason for not excluding the women on DP is that some were on DP for part-time; this could be as low as 25% of full-time during one month during a year (if granted DP for 25% in December). Moreover, the criteria for being granted DP has varied somewhat over the years, as well as how soon in a SA spell the Social Insurance Agency assessed if an individual fulfilled the criteria for DP. This is one of the reasons why we combined SA and DP days in order to increase comparability between the three cohorts.

As shown in Figure 1, some women in all three childbirth groups were on DP already three years before T0. We, in this first exploratory study of these aspects, aimed to use data on all women in a country fulfilling the inclusion criteria, not a sample, in order to get an overall picture of these aspects. One of the strengths of our study is that we include both SA and DP – most of the few previous studies focus only on SA (that is, biasing the outcome through excluding those on DP which actually could be considered as permanent SA). Here we show that also DP is of importance.

However, from your comment we realize that we have not been careful enough when discussing a possible health selection – we now clearer state that other type of data to study that are needed (page 14). We have also added some text about the importance of including both SA and DP days in the analyses (pages 15 and 17).

Comment:

Abstract

1. Conclusions should not only repeat the results. Here the authors could mention the possible selection regarding those with DP somehow, as well as the reliance to medically certified SA (there might be more short-term SA among those with small children). On page 14 the selection has been nicely described.

Response:

We now mention in the conclusion of the abstract the possible health selection into childbirth among those with DP and as a limitation that we did not have information in SA spells  $\leq 14$  days (page 3).

Comment:

Introduction

1. Page 5. I believe that in vitro fertilization may affect also SA prevalence before the pregnancy as the treatments may require SA.

Response:

On page 5 we now mention that women with in vitro fertilization may have increased SA both in the months preceding conception and during pregnancy. This is also covered by us including 12 months before the birth date (T0) and through following women up regarding new births as long as 43 weeks after the third year after T0.

Comment:

2. Page 5, Please clarify the comparison group in: "combination of paid and unpaid work is one reason why women have higher levels of SA." Higher than men?

Response:

We now clarify that the combination of paid and unpaid work might be one reason why women have higher levels of SA and DP than men.

Comment:

3. Page 5. What is the length of SA that was examined in reference 23? Did it include also short-term SA?

Response:

We now clarify that the study in reference 23 only examined disability pension (not SA). We include this reference as it shows that also family situation, in terms of being single as opposed to married/cohabitating and in terms of more children is associated with DP.

Comment:

4. Page 5, line 38. Perhaps remove word "Nevertheless" as the following sentence starts with "However" and two such sentences do not link well together.

Response:

We have revised the text as suggested.

Comment:

Results

1. Table 1 might read better if it only included percentages for the characteristics when the total N for each group is given on the first row.

Response:

We now, as you suggest, present the total N corresponding to each cohort and childbirth group in the first row and percentages when cross-tabulating exposure with the covariates.

Comment:

2. Table 2 is large and a bit difficult to read. It might look nicer as a figure with years on the x-axis and percentages on the y-axis and B0, B1 and B1+ as the grouping variables. As there are many groups and sub-groups, perhaps divide information into 3 panels: total; DP recipients; SA recipients. This would also show some trends, if any.

Response:

Thank you for this suggestion. We have tried different ways of presenting this in figures however, not managed to find a good way for this. We are aware that the table is complex but we prefer to keep it as it is.

Comment:

3. Figure 1 is missing unit for the y-axis.

Response:

We have now added the unit for the Y-axis in Figure 1.

Comment:

Discussion

1. Page 15, line 10 the paragraph beginning with: "In our study, women aged 30 years..." only repeats the results. It should be linked to the literature or its meaning should be discussed. Otherwise it could be omitted.

Response:

Thank you for pointing this out. We have now added that these findings indicate that the hypothesis that having children leads to higher levels of SA can be questioned; we provide also a reference (pages 14 and 15).

Comment:

2. Page 15. There is sentence: "... had fewer days of both SA and DP up until Y+2 when the levels became very similar, as compared to the other groups. The most plausible explanation for this is SA due to their subsequent pregnancy." What does "this" refer to in the latter sentence? That the levels of SA became similar in the B1+ and other groups in Y+2? Might it read better if you said something like: "We found that women having a subsequent childbirth during the follow-up (B1+), had fewer days of both SA and DP at Y+1 than B0 and B1, but from Y+2 the levels were similar to the other groups, possibly due to the new pregnancy."

Response:

Thank you for this suggestion, we have now revised the text accordingly.

Comment:

3. Page 15. Please discuss the possible reasons for: “the levels of SA/DP combined increased in a graded manner from Cohort1995 to Cohort2000 and was highest in Cohort2005.”

Response:

We now acknowledge that our data did not allow to investigate the reasons for these time trends; we have provide several potential explanations for the increase in SA/DP during the study period and added some references to some of these (pages 15 and 16). We hope that others, from our results, get inspired to investigate this further (or that we get the possibility to conduct such studies).

Comment:

4. Page 16. The lack of short sickness absence spells was mentioned. I think this needs some further discussion as small children are known to be vulnerable to many infections that the parents may also caught. While these flus and stomach flus may not last many days, they may be re-current among parents of small children. In addition, is it possible in Sweden to call in sick for the sickness a child? If so, this may also add to the absence days even if the mother herself was not sick.

Response:

Thank you for making us aware of that we did not include information on the possibilities to stay at home with sick children. For each child a parent can stay home up to 60 days/year with benefits at the same level as SA-benefits (however, also getting benefits for the first day, which is not possible during a SA spell). This annual amount of days can be prolonged in the case of severe disease of the child (e.g. cancer) that demand a parent to be present for longer time. Also, it is likely that both the parent and the child/children have the same short-term infection and thus choose to use child-caring benefits instead of SA.

We have now included information on the child-caring benefits in the Methods section (page 7) and extended the discussion about the study limitation regarding the short SA spells according to the reviewer’s comment (page 16).

Comment:

5. Page 16, the last sentence suggests that “more research with longer follow-up periods after childbirth is needed”. What would you expect to see with longer follow-ups? I don’t know if the health consequences of pregnancy itself last for many years, so wouldn’t it be the same if adults with and without children were compared? Perhaps consider omitting the last sentence.

Response:

We have now omitted this sentence.

Comment:

6. I would like to see selection included in the conclusions section as there is no possibility to estimate the causality of the findings.

Response:

We now mention that the findings that the number of combined SA and DP net days were generally lower among women giving birth than among those not giving birth (except for the year before

childbirth), are suggestive of a health selection into pregnancy (page 17). This is an exploratory descriptive study, we had no aim to study causality of findings that we did not even know that would occur.

Reviewer: 2

Reviewer Name: Peter Noone

Institution and Country: Occupational health service

HSE DNE

Ireland

Comment:

interesting findings be interesting to look at Short term absence <15 days.

Response:

We agree, however, unfortunately, we have no information from registers on such SA spells.

Nevertheless, SA spells shorter than 14 days represent only a small proportion of the total SA days.

We are instead glad that we could include all more serious SA spells (serious in terms of duration) as well as all DP days; the latter is often missing in these types of studies. This is now pointed out in the discussion section (pages 15 and 17).

#### VERSION 2 – REVIEW

<b>REVIEWER</b>	Jaana Halonen Finnish Institute of Occupational Health, Finland
<b>REVIEW RETURNED</b>	18-Jul-2019

<b>GENERAL COMMENTS</b>	<p>The Authors have responded well to my prior comments and I find the paper has improved.</p> <p>One additional thing came to my mind; how different are the numbers of work participation days between those who gave birth and those who did not? I mean, often a mother giving birth is on maternity/parental leave for several months and thus not "at risk" of SA or DP, especially during the the first year after giving birth. How did you take this into account?</p> <p>If this was not taken into account, I would think that the results for means SA/DP days are not comparable between these groups. Importantly, those with more than one childbirth may not have worked much during the three-year follow-up period and therefore they seem to have the observed lower levels of SA and DP.</p> <p>This is actually supported by results in Table 2 where the proportions of those with 0-30 or 30-90 SA days are rather equal between the groups Y-3 to Y-1, but increasing by increasing number of given births in later years Y+1 (SA 0-30 days: B0 41% , B1 62%, B1+ 68%); Y+2 (B0 43%, B1 44%, B1+ 50%) and Y+3 (B0 44%, B1 45%, B1+ 51%).</p> <p>The possible bias due to maternity/parental leave, if not taken into account, should be at least discussed in the paper. If this has been considered, please clarify in the text. It might be best to compare the SA (and DP) days/ working days or working months in the three groups.</p>
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## VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Jaana Halonen

Institution and Country: Finnish Institute of Occupational Health, Finland

Please state any competing interests or state 'None declared': None declared

Reviewer comment:

Authors have responded well to my prior comments and I find the paper has improved.

One additional thing came to my mind; how different are the numbers of work participation days between those who gave birth and those who did not? I mean, often a mother giving birth is on maternity/parental leave for several months and thus not "at risk" of SA or DP, especially during the first year after giving birth. How did you take this into account?

If this was not taken into account, I would think that the results for means SA/DP days are not comparable between these groups. Importantly, those with more than one childbirth may not have worked much during the three-year follow-up period and therefore they seem to have the observed lower levels of SA and DP.

Authors response: Thank you for making us aware of the fact that we had not been clear enough about this issue. In Sweden, women with income from parental leave benefits are covered within the same public SA insurance system as those with income from work or unemployment benefits. Students are also covered to some extent. Thus, women on parental leave are also at risk for SA due to e.g., mental disorders, injuries during delivery or other health issues unrelated to childbirth. They can be on SA, but they may not use parental benefits during that period. During the corresponding period someone else cares for the child, often the other parent, who then can receive parental benefits. As stated in the manuscript, all individuals aged 19-64 years, irrespective of income from work, unemployment, or parental leave, are covered by the DP insurance system. Women on full or partial DP before giving birth remain on DP also after giving birth. We now clarify these aspects on pages 6 and 7 in the manuscript.

Please note that this is a first exploratory study regarding levels of SA and DP among all women aged 18-39 years. We were interested in SA and DP in women who 1) did not give birth, 2) gave birth to one child, and 3) gave birth to more than one child during follow-up, respectively. Unfortunately, we have no information on the working days during any of the studied years. The questions you raise are indeed of great interest; however, they have other aims than our study focusing on SA and DP and not on paid work, and they would necessitate other type of data than what we have access to.

Reviewer comment:

This is actually supported by results in Table 2 where the proportions of those with 0-30 or 30-90 SA days are rather equal between the groups Y-3 to Y-1, but increasing by increasing number of given births in later years Y+1 (SA 0-30 days: B0 41% , B1 62%, B1+ 68%); Y+2 (B0 43%, B1 44%, B1+ 50%) and Y+3 (B0 44%, B1 45%, B1+ 51%).

Authors response: Please note that the percentages from Table 2 that you refer to concern those who had some SA, that is, not to all in the groups.

We expected that women giving birth would have less SA in the year following the date of giving birth due to being on parental leave, a hypothesis confirmed by our main findings presented in Figure 2.



This is indeed why we chose a three-year follow-up. As SA usually increases much during pregnancy, the women in B1+ would be rather expected to have higher levels of SA in Y+2 and Y+3, that is, when the next pregnancy occurred. To account for a possible new pregnancy in Y+3, we followed the women with regard to childbirth also after the end of the three-year follow-up. As can be seen in Table 2 (column 6) the B1+ group had higher rates of at least one SA spell >14 days during Y+2 and Y+3 than the other two groups, but not in terms of mean combined SA and DP days (Figure 1 and 2). One of the important methodological aspects of this study is that we included both SA and DP, both in terms of frequencies, percentages, and mean net days.

Reviewer comment:

The possible bias due to maternity/parental leave, if not taken into account, should be at least discussed in the paper. If this has been considered, please clarify in the text. It might be best to compare the SA (and DP) days/ working days or working months in the three groups.

Authors response. We have now extended the discussion regarding this aspect in the revised manuscript (pages 6, 7 and 15). We mention that women on parental leave with health problems that hinders them to care for a child can have SA benefits, while someone else cares for the child. Regarding your suggestion of using another outcome measure, this was not the aim of our study and, as mentioned earlier, we do not have information on working days during the study years. We acknowledge among the study's limitations that we did not have information on whether, and if so how much the women in the three groups had days of paid work (page 17).

### VERSION 3 – REVIEW

<b>REVIEWER</b>	Jaana Halonen Finnish Institute of Occupational Health
<b>REVIEW RETURNED</b>	10-Aug-2019
<b>GENERAL COMMENTS</b>	The Authors have addressed my additional points in the article. I have no further comments.