PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Disability Pension Among Young Women In Sweden, With Special Emphasis On Family Structure: A Dynamic Cohort Study
AUTHORS	Birgitta Floderus, Maud Hagman, Gunnar Aronsson, Klas Gustafsson, Staffan Marklund and Anders Wikman

VERSION 1 - REVIEW

REVIEWER	Peter Westerholm Professor emeritus Uppsala University Occupational and Environmental Medicine Sweden Competing interests: None
REVIEW RETURNED	21/01/2012

GENERAL COMMENTS	1) In "Conclusions" of Abstract, last sentence, you state that high numbers of young women leaving working life may be counteracted by extended gender equality and/or reducing working hours of fathers. This seems a fair assumption and a most reasonable hypothesis. There are, however, no data in the present article to corroborate this view. 2) Under "METHODS" - "Exposures" - last sentence reference is made to close relation between sickness absence and being granted DP. This is the reason givne for excluding sickness absence from multivariate analyses of family structure. Why? The rationale of this exclusion needs a better explanation. 3)The analyses using the SAS MPHREG macro instrument are not easy to grasp in their entirety by many readers. It certainly sounds most appealing that adjustments may be made for research subjects changes of family status or education over the years. Does this imply, then, that in such changes, research subjects assume a new risk category status and, from this moment on, the risk calculus is made based on time in this new risk and going on from there until a new change takes place. Ending it up with a pooling of Hazard expectation. Adding a adjustment for age and calendar year. Is this correct? Present draft text not easy to follow for many readers on this point. 3) Under "statistical methods" - ref the five-year follow-up the reversed causation is given as motive for requirement of no registered sickness absence during three years preceding exposure classification. The rationale is not clear to me. Could admission of subjects with sickness absence during this time period affect exposure variables family composition or education in a reverse causation? I shall, as I believe many readers, appreciate to have point clarified. Was this excluding criterion examined with regard to consequences?

REVIEWER	Sturla Gjesdal, MD PhD Porfessor Dpt of Public Health and Primary Helath Care, University of Bergen, Norway
REVIEW RETURNED	02/02/2012

THE STUDY	Mixing DP before and after 30 years not appriopriate. Impact of sickness absence totally different research question, should be left out. The role of family structure on DP after 30 years should be the resarch question.
GENERAL COMMENTS	**Disability pension (DP) among young women in Sweden, with special emphasis on family structure** **and sickness absences: a dynamic cohort study** Important and interesting study on mechanisms behind women's increasing use of social insurance benefits in some European countries. If one believes that social insurance is linked to ill-health, the study also sheds light on mechanisms behind women's increased morbidity – why "women are sicker". And thus the study investigates the "double burden" theory. The dataset used is also very comprehensive. However I have a few major objections as to the design, and some minor comments to the manuscript. **1.** Judging on behalf of the society that a person is unable to take income-producing work for the rest of her/his life (caused by disease or injury) is a very serious decision both for society and the individual. Especially for young persons. Traditionally below 30 years of age, inborn diseases and handicaps like cognitive disabilities, serious mental diseases with early debut, like schizophrenia and addiction, and accidents are the most usual causes for DP. The incidence/prevalence among young men are often as high as for women. Within this age group (<30) the factors studied in this paper are less relevant and should be studied separately from the young/middle age group (30-45). The female overrepresentation of DP in the Nordic countries is especially important in this group, whereas the gender distribution becomes more even in the older age groups (46-60). **This study should be limited to the 30-45 age- group, where the impact related to family structure probably is very relevant.** 2. The second part of the study concerns the association between sickness absence with social security benefits and subsequent DP. The close relation between these two social security programs in Sweden is well known and also ascertained by the authors. Both programs cover the whole Swedish population (of working age) and require work incapacity because of sickness or injuries. Sickness absence benefits could be granted for very long periods (>many years) but when made permanent it was transformed into DP. This association (that sickness absence "track record") predicts later DP has been shown in previous Swedish studies, and should not be included in this paper. This association is not interesting for an international readership, and hardly for the Swedes themselves (?). Consequently table 4 should be left out 3. All analyses are done with 1 and 5 year follow-up. Most recent similar studies (by Kivimaki and others) use another strategy: Events during the first year follow-up is left out (wash out period) since the distance from baseline is so short (1-365 days). In this study, it is not quite clear whether 5-year follow up also includes events in the 1-year follow-up. My advice is to leave out the 1-year follow up from all analyses and tables, and stick to 5-year. It might be interesting to see the results with and without the

	events in the first year after baseline (with or without wash out). Perhaps, presentation of results of analyses with and without wash-out might be relevant. 4. As mentioned by the authors, the main analytical challenge is the rapid change of values for several independent variables over time: having children, cohabiting, being single etc might change often. Also education, employment status and so on. Normally this is handled by means of time-dependent variables in Cox regression. However here the SAS MPHREG macro was employed. There is 4 references to support this method (one own paper by the authors themselves, and two papers related to leiomyoma incidence in the US). Not being a trained statistician myself I am not convinced of the value of this method which needs a professional statistical assessment before publishing. Minor comments: 5. Abstract: Needs updating after revision of analyses 6. Intro: Generally needs more exact information. A table showing prevalence of DP in Sweden and other OECED countries, in different age groups and gender, had been of interest, to illustrate international relevance. The same goes for the increase of DP in Sweden since 1990, more exact figures are needed. The downward trend after 2004 is not evident among women below 30, what about men and women 30-45? 7. "Incidence of DP among women with psychiatric diagnoses tripled"- what does this mean and which age groups were involved here? 8. Study population – dynamic cohort (also table 1 and table 2): It is not quite clear which birth cohorts were included and for how long they were followed up. In first part of table 1 and 2, data on personyears and incidences are related to age groups (at baseline?). The oldest cohort (born in 1960), were 33 years in 1993, and 38 years after 5 years. The youngest cohort (1979) was 20 years at inclusion in 1999 and followed until 24 years in 2003. The first cohort included at 20 years seems to be 1973. **Different birth cohorts are studied at different ages and in different time periods**. If the study only included cases of DP from age 30 and only 5 (6)-years follow-up, inclusion at 25 years would have been more relevant, with birth cohort 1973 as the youngest (25 years in 1998, 30 in 2003). I would have liked to see person-years and incidences for each birth cohort. 9. Sector of employment (possible confounder) is not commented later is spite of interesting results, table 1+2 10. Country of birth: Detailed results (specific countries) in table 1 might be left out in spite of some interesting facts, since they are collapsed into few groups in the analyses and table 2.

VERSION 1 – AUTHOR RESPONSE

Reviewer: Peter Westerholm
Professor emeritus
Uppsala University
Occupational and Environmental Medicine
Sweden

Competing interests: None

Thank you for a careful review!

1) In "Conclusions" of Abstract, last sentence, you state that high numbers of young women leaving working life may be counteracted by extended gender equality and/or reducing working hours of fathers. This seems a fair assumption and a most reasonable hypothesis. There are, however, no data in the present article to corroborate this view.
We agree, and the last sentence of the Conclusions has been changed to: “It remains to be analyzed to what extent the high numbers of young women exiting from working life may be counteracted by a) extended gender equality, b) fewer work hours among fathers and mothers of young children, and c) by financial support to lone women with children.”
The reason why we want to keep the sentence is to hold back interpretations implying that increased gender inequality by reduced participation in working life for women should be a solution.

2) Under "METHODS" - "Exposures" - last sentence reference is made to close relation between sickness absence and being granted DP. This is the reason givne for excluding sickness absence from multivariate analyses of family structure. Why? The rationale of this exclusion needs a better explanation.

Sickness absence may to a considerable extent be part of the pathway between family structure and DP. Family structure is also associated with sickness absence, according to previous findings (ref 11).
However, Sickness absence has now been removed from the study, as suggested by reviewer 2, with one exception: we still use the variable as a tool for increasing health comparability between exposed and unexposed at start of follow up (see “healthy mother effect” below).

3)The analyses using the SAS MPHREG macro instrument are not easy to grasp in their entirety by many readers. It certainly sounds most appealing that adjustments may be made for research subjects changes of family status or education over the years. Does this imply, then, that in such changes, research subjects assume a new risk category status and, from this moment on, the risk calculus is made based on time in this new risk and going on from there until a new change takes place. Ending it up with a pooling of Hazard expectation. Adding a adjustment for age and calendar year. Is this correct? Present draft text not easy to follow for many readers on this point.
This is correct in theory! However, this description gives the impression that the calculus was based on a continuous time line, while in fact time was cut into equal time intervals (years).
We have changed the text to:
” The program has been used in other studies11, 13, 14 and is analogous to the proportional hazard regression with time dependent repeated measurements with a counting process style of input. The difference from a traditional Cox proportional hazard regression is that the calculus was not based on the individuals’ continuous time lines. Instead, an individual data record was created for each year in which the participant was at risk of receiving a DP, which allowed the individuals to change risk category status each year. With this method, all of an individual’s changes regarding, for example, family structure or level of education, were accounted for across time. The risk categories of a certain year were linked to DP/no DP in the subsequent year.”

3) Under "statistical methods" - ref the five-year follow-up the reversed causation is given as motive for requirement of no registered sickness absence during three years preceding exposure classification. The rationale is not clear to me. Could admission of subjects with sickness absence during this time period affect exposure variables family composition or education in a reverse causation? I shall, as I believe many readers, appreciate to have point clarified. Was this excluding criterion examined with regard to consequences?
We have deleted the expression reversed causation.
The assumption is that sickness absence is a marker of an impaired health status that to some extent is negatively correlated with having children, implying that women with children on average would be at decreased risk of going on DP compared to women without children from the very beginning, which

would yield a selection bias.
In previous studies we have found that women without health problems (no longstanding illness or handicap) have a higher probability of having children, which may be called a “healthy mother effect”, analogous to the well-known “healthy worker effect”. A “healthy mother effect” would mask a true association between having children and risk of DP. Thus, we tried to make all participants “healthy” at start by the requirement of no sickness absence during the three-year period before their base-line year. This does not mean that we have succeeded to erase the selection bias, but hopefully we have reduced it. This restriction of the study base should also have lead to exclusion of women with inborn severe diseases or handicaps (see answer to reviewer 2!).
We did examine the consequences by analyzing the data with and without the restriction in the five-year follow up (see Table 3), and the hazard ratios for having children were somewhat higher when the criterion was added, both among lone and cohabiting women, which gives some support to the assumption of a selection bias.

Reviewer: Sturla Gjesdal, MD PhD
Porfessor Dpt of Public Health and Primary Helath Care, University of Bergen, Norway

Thank you for a careful review!

Mixing DP before and after 30 years not appriopriate. Impact of sickness absence totally different research question, should be left out. The role of family structure on DP after 30 years should be the resarch question.
All results on sickness absence have been deleted.
The main objective was to study DP in young women (young mothers) to provide some knowledge about the impact of family structure on the incidence of DP. Therefore we cannot agree with the reviewer that DP before age 30 should be excluded.
The main argument for excluding DP before age 30 is that “inborn diseases and handicaps like cognitive disabilities, serious mental diseases with early debut, like schizophrenia ….” are common causes of DP in young persons. The influence from this type of illness was reduced in different ways in the current study:
• It was a main reason for excluding women 16-19 years of age from the study. Many women with the mentioned health problems should be granted a disability pension before 20 years of age.
• This type of health problems should be negatively correlated with employment, due to reduced work capacity, and this was part of the explanation for stratifying the analyses on employed/not employed women.
• The restriction of the study base in the five-year follow up to comprise “healthy” women, with no sickness absence exceeding 14 days during the three years preceding start of follow up, should also lead to exclusion of women with severe health problems.
• The results show that family structure is associated with DP. It may well be that the associations between family structure and DP would be stronger with a different age range. However, the aim was to study young women in the age range 20-40 years, and we want to refrain from a new analysis that was not decided a priori.

Important and interesting study on mechanisms behind women’s increasing use of social insurance benefits in some European countries. If one believes that social insurance is linked to ill-health, the study also sheds light on mechanisms behind women’s increased morbidity – why “women are sicker”. And thus the study investigates the “double burden” theory. The dataset used is also very comprehensive.
We believe that use of social insurance in terms of disability pension (and sickness absence) is linked to ill-health, and that the study may shed some light on mechanisms behind women´s morbidity and the “double burden” theory. However, the aim did not include questions on gender differences, why

“women are sicker” (than men?).

However I have a few major objections as to the design, and some minor comments to the manuscript.

1. Judging on behalf of the society that a person is unable to take income-producing work for the rest of her/his life (caused by disease or injury) is a very serious decision both for society and the individual. Especially for young persons. Traditionally below 30 years of age, inborn diseases and handicaps like cognitive disabilities, serious mental diseases with early debut, like schizophrenia and addiction, and accidents are the most usual causes for DP. The incidence/prevalence among young men are often as high as for women. Within this age group (<30) the factors studied in this paper are less relevant and should be studied separately from the young/middle age group (30-45). The female over-representation of DP in the Nordic countries is especially important in this group, whereas the gender distribution becomes more even in the older age groups (46-60). This study should be limited to the 30-45 age- group, where the impact related to family structure probably is very relevant.
See above!

2. The second part of the study concerns the association between sickness absence with social security benefits and subsequent DP. The close relation between these two social security programs in Sweden is well known and also ascertained by the authors. Both programs cover the whole Swedish population (of working age) and require work incapacity because of sickness or injuries. Sickness absence benefits could be granted for very long periods (>many years) but when made permanent it was transformed into DP. This association (that sickness absence “track record”) predicts later DP has been shown in previous Swedish studies, and should not be included in this paper. This association is not interesting for an international readership, and hardly for the Swedes themselves (?). Consequently table 4 should be left out
Results on sickness absence and Table 4 have been left out.

3. All analyses are done with 1 and 5 year follow-up. Most recent similar studies (by Kivimaki and others) use another strategy: Events during the first year follow-up is left out (wash out period) since the distance from baseline is so short (1-365 days). In this study, it is not quite clear whether 5-year follow up also includes events in the 1-year follow-up.
The usage of a “wash out period” should depend on the research question. To us it is somewhat unclear what it is that should be “washed out”.
In the one-year follow up the aim was to explore the conditions just before going on DP, conditions that may contribute to the decision of granting a DP. No wash out period was used.
In the five-year follow up we analyzed the long-term prediction, and then we used a five-year “wash out” period.

My advice is to leave out the 1-year follow up from all analyses and tables, and stick to 5-year.

See above! We prefer to keep the one-year follow up because it gives a complementary picture of the relation between family structure and DP. The one-year follow up also provides a detailed control of exposure classification and confounding by the annual assessment of the individuals’ risk categorization which has not been done before.

It might be interesting to see the results with and without the events in the first year after baseline (with or without wash out). Perhaps, presentation of results of analyses with and without wash-out might be relevant.
See above!

4. As mentioned by the authors, the main analytical challenge is the rapid change of values for

several independent variables over time: having children, cohabiting, being single etc might change often. Also education, employment status and so on. Normally this is handled by means of time-dependent variables in Cox regression. However here the SAS MPHREG macro was employed. There is 4 references to support this method (one own paper by the authors themselves, and two papers related to leiomyoma incidence in the US). Not being a trained statistician myself I am not convinced of the value of this method which needs a professional statistical assessment before publishing.

The SAS MPHREG macro is analogous to Cox proportional hazard regression with time-dependent repeated measurements with a counting process style of input. However, we find this macro more convenient to use, because it provides a more detailed report of the basic data on which the regression is based, and we think that the programming – the statements can be written in a more perspicuous way. We have long experience of this and other methods of regression analyses.

Minor comments:

5. Abstract: Needs updating after revision of analyses
Done!

6. Intro: Generally needs more exact information. A table showing prevalence of DP in Sweden and other OECED countries, in different age groups and gender, had been of interest, to illustrate international relevance. The same goes for the increase of DP in Sweden since 1990, more exact figures are needed. The downward trend after 2004 is not evident among women below 30, what about men and women 30-45?

The aim was not to focus on gender differences. We have tried to illustrate the international relevance of the study by adding a sentence on the increased proportion of women among individuals going on DP.

7. "Incidence of DP among women with psychiatric diagnoses tripled"- what does this mean and which age groups were involved here?
We have changed the text to: "The incidence of DP with psychiatric diagnoses was 15 per 104 in 1998 in Swedish women aged 16 to 64, and raised to 53 per 104 in 2004. Men showed an increase from 14 to 30 per 104 during the same period.4"

8. Study population – dynamic cohort (also table 1 and table 2): It is not quite clear which birth cohorts were included and for how long they were followed up.
The birth cohorts comprised 1960-79. The risk categorization was started in 1993 or the year of entry into the cohort, provided that the woman had reached the age of 20. Follow up was discontinued at the year of DP, emigration, death, or end of 2003, whichever came first. This has been clarified.

In first part of table 1 and 2, data on person-years and incidences are related to age groups (at baseline?).
No, the person years refer to number of individuals passing a certain age during follow up. In the Tables, "Age group" was changed to "Ages during follow-up".

The oldest cohort (born in 1960), were 33 years in 1993, and 38 years after 5 years.
Yes
The youngest cohort (1979) was 20 years at inclusion in 1999 and followed until 24 years in 2003.

Yes
The first cohort included at 20 years seems to be 1973.
Yes, all who were born in 1973 and survived to 1993 had reached the age of 20 in 1993.

Different birth cohorts are studied at different ages and in different time periods. If the study only included cases of DP from age 30 and only 5 (6)-years follow-up, inclusion at 25 years would have been more relevant, with birth cohort 1973 as the youngest (25 years in 1998, 30 in 2003). I would have liked to see person-years and incidences for each birth cohort.

See above! We want to keep women aged 20-30, and also the one-year follow-up.

9. Sector of employment (possible confounder) is not commented later is spite of interesting results, table 1+2

We agree that the result of sector of employment is interesting, and this applies also to other results shown in Table 1. However, we have avoided further comments on the relations between the potential confounders and DP in the Discussion section to keep the focus on the exposure factor – i.e. family structure.

10. Country of birth: Detailed results (specific countries) in table 1 might be left out in spite of some interesting facts, since they are collapsed into few groups in the analyses and table 2
We do not think that the formation of broader groups in the multivariate analyses motivates deletion of the more detailed results presented in Table 1.

VERSION 2 – REVIEW

REVIEWER	Sturla Gjesdal MD PhD Professor Dpt of Public Health and Primary Health Care, University of Bergen I have no competing interests
REVIEW RETURNED	26/03/2012

GENERAL COMMENTS	Disability pension among young women in Sweden, with special emphasis on family structure: A dynamic cohort study Bmjopen-2012-000840.R1. I will again thank for the opportunity of reviewing this important study. The paper is now somewhat revised and improved. However there are still important weaknesses which make a major revision necessary before publication There is a large literature concerning the role of motherhood, with respect to women's health and related to that, the use of social insurance benefits like disability pension (DP). Some studies have found that the combination of work and taking care of children ("the double burden") might create or contribute to, health problems among women. Musculoskeletal and minor psychiatric disorders/ common mental disorders (CMD) are the most likely consequences. Among some of the women exposed to the "double burden" the health problems become chronic and do not respond to treatment or rehabilitation. In some countries these women will end up with a permanent DP. It is reasonable to think that this process takes a rather long time. Other studies have refuted the "double burden" theory. The health consequences of motherhood is a main topic of the present study, however this literature is not reviewed in the introduction (refs 1-11), except for the references of the authors' own

	studies. The relevant literature is not mentioned in the discussion section either (comparison with previous literature). The relation between health and civil status is another central topic in social epidemiology and social insurance research. In the paper there are only two references from a vast literature in this area (refs 5, 16). Previously the main categories have been "married","" divorced/ separated" and "never married". In the present study the authors use a more" modern" categorization: Cohabiting or lone/single. It is not explained how the non-married in the first group are identified. Furthermore those who cohabit "without children in common" are defined as "lone" (page 9, line 8-10). It is not explained what "children in common" means. This definition of lone/single makes it difficult to interpret the results. However, the introduction gives the impression that the aim of this study is to explain an increase in DP among young women in Sweden, and especially among women below the age of 30. Intro line 8-40 deals with these Swedish trends, however without giving any figures. There is information on the incidence of DP with psychiatric diagnoses among Swedish women and men between 16-64 years, which is less relevant for this study. According to the authors, in Sweden women below 30 years increasingly are judged as permanently disabled because of musculoskeletal and minor psychiatric disorders, in addition to the more serious disorders that traditionally have been the reason for permanent DP in this age group. The references 1-5 deals with official statistics concerning DP, however without documenting that the Swedish trends among young women below 30 is similar in North America or other Western European countries This reduces the generalization of the study. Study population: The birth cohorts 1960-1979 were included, however with different follow-up periods (different number of person years). According to table 1 nearly 60% of the person years at risk concerned women below 30 years and nearly 30% in the age 31-35. So indeed this is a study of very young women. A separate analysis for those above 30 years at baseline would be of some help Exposure: The main variable was "family structure": a combination of having children or not, and partner status. The problems related to the cohabiting variable are mentioned before. Potential confounders were other variables previously known as predictors of DP (table 1 and 2). Statistics: The study applied a modified proportional hazards analysis. This is now explained better than in the first version of the paper. However, the references used (11-14) still are not sufficient. Follow-up: The authors still want to include the one-year (1-365 days) follow-up in addition to a five-year follow-up. There are no explanations for this decision in the paper. One should expect that the main explanatory variables in question (family structure) to exert their effect over a rather long time, and the effects should not be observed immediately after baseline. In fact other prospective studies of predictors of DP have excluded events in the first year after baseline (wash out). In addition, both the results- and discussion sections become quite difficult to read because of the mixing of results from one-year and five-year follow-up. The long-term (5-year) results clearly are most relevant with respect to causal implications.

	Results: Page 13-14 deal with the effects of the “potential confounders”, known previously to be predictors of DP. In addition the results are shown in table 1 and 2 of 3 (univariate and multivariate effects of the confounders). Since these effects are known previously, it takes up too much space All these results are based on the one-year follow-up. Why? Discussion: I prefer the recommended structure of this section: Main findings, strengths and limitations, comparison with other literature and conclusion. Now this section appears unfinished The conclusion on page 19 highlights the negative effects of being a lone mother at a young age and the study seems to verify the negative health consequences of motherhood: Both lone mothers in work (double burden), and lone mothers without work clearly have an increased risk of DP. Among those cohabiting and working, women with children (also double burden) had a slightly increased risk of DP compared to those cohabiting without children (those married without children, a very small group). Lone women without children and working had not increased risk of DP. However, those without both work and children are worst off (might be reverse causation?)

VERSION 2 – AUTHOR RESPONSE

Reviewer: Sturla Gjesdal MD PhD Professor
Dpt of Public Health and Primary Health Care, University of Bergen

I have no competing interests

Disability pension among young women in Sweden, with special emphasis on family structure: A dynamic cohort study Bmjopen-2012-000840.R1.

I will again thank for the opportunity of reviewing this important study. The paper is now somewhat revised and improved. However there are still important weaknesses which make a major revision necessary before publication

There is a large literature concerning the role of motherhood, with respect to women’s health and related to that, the use of social insurance benefits like disability pension (DP). Some studies have found that the combination of work and taking care of children (“the double burden”) might create or contribute to, health problems among women. Musculoskeletal and minor psychiatric disorders/ common mental disorders (CMD) are the most likely consequences. Among some of the women exposed to the “double burden” the health problems become chronic and do not respond to treatment or rehabilitation. In some countries these women will end up with a permanent DP. It is reasonable to think that this process takes a rather long time. Other studies have refuted the “double burden” theory. The health consequences of motherhood is a main topic of the present study, however this literature is not reviewed in the introduction (refs 1-11), except for the references of the authors’ own studies. The relevant literature is not mentioned in the discussion section either (comparison with previous literature).

We are aware of the vast literature on the “double burden” hypothesis (we have referred to that literature in our previous studies on self-reported health and medically certified sickness absence). Much of the literature is too far out from the focus of this study. However, we have now included some of the most relevant and recent articles. Still, we want to point out that there is no previous study with a focus on family structure, work and DP in a population sample of women.

The relation between health and civil status is another central topic in social epidemiology and social insurance research. In the paper there are only two references from a vast literature in this area (refs 5, 16). Previously the main categories have been “married”,”” divorced/ separated” and “never married”. In the present study the authors use a more” modern” categorization: Cohabiting or lone/single. It is not explained how the non-married in the first group are identified. Furthermore those who cohabit “without children in common” are defined as “lone” (page 9, line 8-10). It is not explained what “children in common” means. This definition of lone/single makes it difficult to interpret the results.

The definition of cohabiting has been elaborated in the methods section, and the potential effect of the coding should be conservative (work against the hypothesis).

However, the introduction gives the impression that the aim of this study is to explain an increase in DP among young women in Sweden, and especially among women below the age of 30. Intro line 8-40 deals with these Swedish trends, however without giving any figures. There is information on the incidence of DP with psychiatric diagnoses among Swedish women and men between 16-64 years, which is less relevant for this study.

The introduction was meant to stress the importance of a focus on young women because of the remarkable increase in DP incidence in this group. We have included a figure to clarify this further.

Figure 1. New cases of disability pension among women 20-29 and 30-39 years of age, due to mental diagnoses (ICD-10: F00-F99), musculoskeletal diagnoses (ICD-10: M00-M99) and diagnoses of the nervous system (ICD-10: G00-G99). Sweden 1971-2005. Data source: The Swedish Social Insurance Agency (reference 4). (Differences in ICD coding during the time period were harmonized).

According to the authors, in Sweden women below 30 years increasingly are judged as permanently disabled because of musculoskeletal and minor psychiatric disorders, in addition to the more serious disorders that traditionally have been the reason for permanent DP in this age group.

The references 1-5 deals with official statistics concerning DP, however without documenting that the Swedish trends among young women below 30 is similar in North America or other Western European countries This reduces the generalization of the study.

There is no evidence of a similar trend in the United States or Canada, and for European countries other than the Nordic countries it is only the Slovak Republic and Belgium (1999-2007) that could be mentioned according to Figure 2.A1.4 (ref 3).

Study population: The birth cohorts 1960-1979 were included, however with different follow-up periods (different number of person years). According to table 1 nearly 60% of the person years at risk concerned women below 30 years and nearly 30% in the age 31-35. So indeed this is a study of very young women. A separate analysis for those above 30 years at baseline would be of some help

These groups have shown similar patterns of DP incidence, and we see no reason why a distinction should be made.

Exposure: The main variable was “family structure”: a combination of having children or not, and partner status. The problems related to the cohabiting variable are mentioned before. Potential confounders were other variables previously known as predictors of DP (table 1 and 2).

We have commented on the coding.

Statistics: The study applied a modified proportional hazards analysis. This is now explained better than in the first version of the paper. However, the references used (11-14) still are not sufficient.

We have added one more reference.

Follow-up: The authors still want to include the one-year (1-365 days) follow-up in addition to a five-year follow-up. There are no explanations for this decision in the paper.

We have elaborated the meaning of the one-year follow up.

One should expect that the main explanatory variables in question (family structure) to exert their effect over a rather long time, and the effects should not be observed immediately after baseline. In fact other prospective studies of predictors of DP have excluded events in the first year after baseline (wash out).

See answer to the previous review! In the five-year follow up a five-year "wash out" was used.

In addition, both the results- and discussion sections become quite difficult to read because of the mixing of results from one-year and five-year follow-up. The long-term (5-year) results clearly are most relevant with respect to causal implications.

We have now clarified the differences between the one- and five-year follow up results.

Results: Page 13-14 deal with the effects of the "potential confounders", known previously to be predictors of DP. In addition the results are shown in table 1 and 2 of 3 (univariate and multivariate effects of the confounders). Since these effects are known previously, it takes up too much space All these results are based on the one-year follow-up. Why?

The tables (1 and 2) were included to provide an overview of potential confounding in the study group, and to introduce the stratification on employment which caused us to use different models for employed and not employed women. Since other researchers may have an interest in comparing previous results with the outcome in this study group, we have kept Table 1 but deleted Table 2 to keep a better balance.

Discussion: I prefer the recommended structure of this section: Main findings, strengths and limitations, comparison with other literature and conclusion. Now this section appears unfinished

We have rewritten the discussion. As there are no previous studies similar to this one, it is hard to make comparisons to other studies.

The conclusion on page 19 highlights the negative effects of being a lone mother at a young age and the study seems to verify the negative health consequences of motherhood: Both lone mothers in work (double burden), and lone mothers without work clearly have an increased risk of DP. Among those cohabiting and working, women with children (also double burden) had a slightly increased risk of DP compared to those cohabiting without children (those married without children, a very small group). Lone women without children and working had not increased risk of DP.
However, those without both work and children are worst off (might be reverse causation?)

Yes, (and the potential of reverse causation or selection bias was already pointed out.)