Author's response to reviews

Title: The role of paternal psychosocial work condition on mental health of their children: A case-control study

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Author's response to reviews: see over
BMC Editorial Team

September 12, 2007

Dear Editor,

We are re-submitting the manuscript #6691101541290657. Following the suggestion of one of the reviewers, the manuscript title is now “Paternal psychosocial work conditions and mental health outcomes: a case-control study”.

We found the reviewers comments helpful and we believe the revised manuscript is a stronger one thanks to the suggested changes. We also believe that several of the comments were due to lack of clarity in the methods section which we hope to have satisfactorily improved.

In what follows is a detailed account of our responses to the reviewers’ comments. Please note that our responses have been added in the text in bold letters. We look forward to receiving news of your editorial decision on this revised and improved manuscript.

Sincerely,

Stefania Maggi.
Reviewer's report
Title: The role of paternal psychosocial work condition on mental health of their children: A case-control study
Version: 1 Date: 3 April 2007
Reviewer: Johannes Siegrist

Reviewer's report:
General
This study explores associations of paternal working conditions with their children's onset of a variety of mental disorders in a life-course perspective, using expert ratings of workplaces and administrative data based on ICD 9-diagnoses. The large cohort of sawmill workers and of their children, the careful study design, the variety of diagnoses under study, and the statistical analyses conducted that include additional independent variables contribute to the strengths of this study. As a main finding, authors demonstrate some effects of psychosocial work stress, as measured by components of the demand-control model, on children's risk of mental disorder, depending on type of diagnosis and age at onset.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. The Introduction exclusively addresses work-related factors, but the Results and Discussion sections are additionally concerned with ethnicity, marital status, parental health etc. Authors should justify the additional selection of these variables in terms of content, i.e. their significance for children's health, in the Introduction, rather than mentioning them by the way on p. 7.

We have added in the introduction a discussion on the importance of such variables thus providing an explicit rationale for including ethnicity, marital status, and paternal mental health in the analysis.

2. A general problem concerns the way results of the study are presented in Tables 2 to 4 as well as their interpretation. For instance, in Table 2 one would expect a total of 100 regressions if each one of the 5 work stress categories was tested for each type of diagnosis (5) in each age category (4). Actually, 5 out of 100 estimates are statistically significant, without considering the issue of multiple testing. Thus, authors should be more careful in interpreting the robustness of their findings.

We apologize for the lack of clarity in the methods section that led this reviewer to misinterpret the modeling procedure. In fact, we did not run 100 models but 28: 4 for the children’s cohort; 8 for the adolescent cohort; 8 for the young adult cohort; and 8 for the adult cohort. While the issue of multiple testing still applies, the results are much stronger than those generated by 100 tests. Nonetheless, we have made some changes to the language throughout the manuscript to ‘tone down’ what was perceived as over confidence in the robustness of the findings.
Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. On p.4, authors discuss the role of father’s work experience in children's mental development, without referring to the classical studies of Melvin Kohn who demonstrated strong effects of work complexity and autonomy on children's mental states.

We thank the reviewer for introducing us to the work of Melvin Kohn. We have taken care to read most of Melvin Kohn’s publications that directly related to children (e.g., Class, Occupation and Orientation- 1969; Social Class & Parental Values – 1959; as well as later works) but failed to find the connection between work complexity and autonomy in fathers with mental health outcomes in children. Our reading of Melvin Kohn’s work is that it is concerned with structure of work life as a predictor of child-rearing values, such that, for example, higher job status fathers prefer consideration, curiosity, responsibility, and self control in their children; and low status fathers prefer good manners, neatness, honesty, and obedience.

If we have missed important publications by this author, may this reviewer direct to the appropriate source so that we can integrate Kohn’s work in our manuscript?

2. On p.6, components of the demand-control model rather than the model itself (interaction term, quadrants etc.) are introduced. Authors might explain why the model as such was not tested.

The purpose of this study is to explore whether there are aspects of paternal psychosocial work conditions that are associated with mental health of their children rather that test the demand/control model per se. In a similar study, we found that there are associations between suicidal behaviours of the children and specific dimensions of the demand/control model relative to paternal work psychosocial conditions (Ostry et al. 2007, BMC Public Health). To be consistent with and extend our line of research, we have opted for a similar approach in this manuscript.

3. On p.8f. it is pointed out that each individual is classified once per age category. Given substantial co-morbidity and chronicity in mental health, it may be that a substantial number of children appear twice or more in the analyses. The possible bias of this fact should be stressed by authors.

On page 11 the possibility of having same participants represented more than one is stated. On page 16 the potential for bias due to this issue is discussed as a limitation.

4. On p. 13f, authors discuss some findings as if they were derived from a true longitudinal investigation ("differential impact on each developmental age group"; "play a more important role from adolescence through adulthood").
The sentence now reads: Third, we found that the association between paternal psychosocial work conditions and mental health outcomes could be different depending on the age cohort (e.g., whether it was the children’s or the adult’s cohort).

5. In discussing limitations, the many null findings are not mentioned.

See response to point 2 under ‘major revisions’

6. The conclusions both in the Abstract and text are overstated, given the above mentioned problems.

Abstract and conclusions now read: This study provides support to the tenet that being exposed to paternal work stress during childhood can have long lasting effects on the mental health of individuals.

Discretionary Revisions (which the author can choose to ignore)
1. The title sounds somehow strange (The role...on...).

The title now reads: Paternal psychosocial work conditions and mental health outcomes: a case-control study

2. In the Abstract and on p. 14, age 16 is emphasized, but the cut between age groups is 14.

The age 16 for the exposure variables has been de-emphasized in the abstract and the main text

3. p.8, line 12: should be: "health outcomes of the...."

Done

4. p.13, line 11: should be: " health in that it suggests"

Done

Figure 1: Authors should mention that probabilities of being diagnosed are not identical across the different age groups (see p. 8f).

I am not sure I understand this comment. Maybe the reviewer can explain further what he means?

What next? Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests: I declare that I have no competing interests
Reviewer's report

Title: The role of paternal psychosocial work condition on mental health of their children: A case-control study

Version: 1 Date: 4 July 2007

Reviewer: Jim van Os

Reviewer's report:

General
This is a well designed manuscript addressing the impact of early influences on mental health and development from childhood into adulthood with a special focus on effects of paternal psychosocial working conditions. Interesting is the identification of sub-dimensions of these conditions that are most probable to increase the risk for developing psychopathology from childhood into adulthood. However, there are some problems that need to be addressed.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

-1. Methods:

a. diagnostic categories for children 14 years old and younger: in contrast to adults, children may not be capable of experiencing or reporting the symptoms thought to be representative of for example major depressive disorder. Hence, the pattern of manifested symptoms is likely to vary according to age and stage of development of the child and young adolescent. This is probably related to concept and language development. As a consequence, children who are diagnosed as having a depression at one point of time can be diagnosed as psychotic at another point of time; the psychotic disorder being the persistence of the depression. Therefore, there will be some overlap between the onset and persistence of a disorder. Furthermore, in some cases a it is not possible to give a psychiatric diagnosis at all or the diagnosis has to be postponed. There is also the complexity of comorbidity. The authors should make the inclusion criteria of the diagnostic categories more explicit. Also, some of the above mentioned topics have to be addressed in the discussion of the paper.

We agree with the reviewer that the phenomenology of mental health is very likely to be different among children and that there are important challenges in identifying such conditions in the children’s population. We realize that this becomes an important issue especially when attempting to generate some prevalence rates and compare them across age groups. This could also be a problem when attempting to make comparisons between age groups as to what predictors are associated with specific conditions since the participants who may have the condition but have not been diagnosed could enter the pool of controls. A possible consequence is the potential for diminished strength of association due to the ‘spurious’ control group. In a sense, this could lead to more conservative results than those desired. Therefore, rather than impacting on the probability of obtaining significant
findings, this issue may have affected our results by generating more non-significant associations than expected with a ‘pure’ control group.

As per the problem of onset versus persistence and comorbidity (which was also raised by reviewer 1) on page 11 the possibility of having same participants represented more than one is stated. On page 16 the potential for bias due to this issue is discussed as a limitation.

b. There is no diagnostic category “hyperactivity”. Why is that? This is the main diagnosis in children and omitting these cases may explain the findings.

The ICD9 diagnostic for hyperactivity (ICD9 314) is a childhood specific diagnostic called Hyperkinetic Syndrome of Childhood. The focus of the manuscript is to document possible effects of paternal stress in different age cohorts. Therefore, we did not analyze mental health conditions that could be diagnosed in one age group only. However, this comment, in conjunction with point 1.a, made us reflect as to whether it was appropriate to present results for psychotic based disorders for which we had sufficient cases in one or two age groups (depending on the specific condition) and for which there was no diagnostic code in childhood. We have come to the conclusion that the manuscript would improve in focus and clarity if we showed results that allowed for comparisons between at least three of age cohorts investigated and did remove results and portions of discussion that relate to the psychotic conditions.

c. Age categories: it is unclear to the reader why the authors chose for the present age categories.

The age categories has been informed by a combination of how the ICD9 codes are split for children and adults (14 and under have separate diagnostics that are specific to children; 15 and older are all grouped in the ‘adult’ population), and by general cut-offs of broad developmental phases. Rationale for age categorization is explained on pages 6-7.

d. Independent variables: please describe more explicitly in the methods what kind of independent variables are used in the statistical analyses and present these also in the tables (see also point 2). How did you (re)code the variables? What was the reference category? What categorical variables were recoded into dummies?

See pages 11-12.

e. Statistical analysis: A number of statistical comparisons is presented in the paper, but these are not related to any stated hypothesis. Please provide a framework of a priori hypotheses within which statistics can be used properly. Please give a description of the models.

See pages 11-12.

f. First paragraph under the heading “mental health outcomes of the children’s
cohort*: what kind of mental health services are available through the BCLHDB? Please describe these services more explicitly.

See pages 9-10

g. How representative is the sample? (minority culture).

According to Census 2001 there are approximately 10% of Chinese 3% of Sikh living in British Columbia. More than 90% of these live in metropolitan areas such as Vancouver and Victoria. The proportion of Chinese participants in our sample (approximately 1% or less of the general population) is representative of the BC population. However, the Sikh participants are overrepresented in our sample with approximately 13% of the participants versus the expected 3% in the general BC population.
See page 13 for added text on this point.

2. Results:

a. I recommend restructuring the tables, presenting significant as well as non-significant results and including p-values. Possibly, the authors performed stepwise regression analyses (although this was not described in the methods). In that case the authors should redo the analyses, deciding themselves based on their hypothesis, what variables should be included, rather than letting the computer decide for them. Possibly, ethnicity is a categorical variable entered in the analyses using dummies (although this is not described in the methods). Deleting one of the dummies from the model and leaving another one in the analysis is not correct.

We hope that with more details in the method it will become clear what process we used for the analysis. We apologize for lack of clarity in the first version of manuscript. We did not do stepwise analysis but rather used the approach that the reviewer was suggesting. Also, we have provided a complete table of results, but it is rather long occupying 24 pages, which is the reason we opted for a shortened version in the first place. However, we agree with the reviewer that it is important to show the complete table, but perhaps it will be at the discretion of the editor?

c. It is sometimes unclear to the reader what conclusions can be drawn from the tables. For example, “paternal work stress” (=duration of employment?) was significantly associated with depression among adolescents but not among children, young adults, and adults” (line 15, page 12). “Work stress was more strongly associated with psychotic disorders and alcohol and drug related disorders in adulthood than it was in adolescence and childhood” (abstract): to my knowledge there is no diagnostic category ‘psychosis’ in the population of children in the present paper.

The ambiguous wording has been fixed throughout the text (e.g. see abstract).
d. The presentation of the results has to be complemented. Line 5, page 12: Chinese origin also functioned as a protective factor for neurotic disorders in young adults and adults.

Done. See page 13

3. Discussion

a. Please give some clinical implications.

See page 17

b. Please mention that most of the ratings were made years before the child, adolescent and adult were referred to health services.

See page 14

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

The authors should also present odds ratios, confidence intervals and p-values throughout the results section.

Done

The first paragraph under “paternal psychosocial work conditions, mental health and socio-demographics” can better be placed in the background section.

Done

Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests: I declare that I have no competing interests