Author's response to reviews

Title: What do we know about the non-work determinants of occupational mental health? A systematic review of longitudinal studies

Authors:

   Nancy Beauregard (nancy.beauregard.2@umontreal.ca)
   Alain Marchand (alain.marchand@umontreal.ca)
   Marie-Ève Blanc (marie-eve.blanc@umontreal.ca)

Version: 2 Date: 1 April 2011

Author's response to reviews: see over
March 31st, 2011

BMC Public Health
Jigisha Patel MRCP, PhD
Series Editor

Dear Dr Patel,

The present document addresses comments raised by the recent evaluation of the manuscript MS: 6834217924988574 *What do we know about the non-work determinants of occupational mental health? A systematic review of longitudinal studies* submitted to the BMC Public Health.

First and foremost, we would like to take the opportunity given here to warmly thank reviewers for the manuscript for their highly insightful and substantive comments. The latter have relevantly led to significant refinements of the article whose details are presented point-by-point below, per reviewer.

We sincerely hope these modifications answered with clarity and succinctness previous interrogations regarding the conceptual and methodological underpinnings of this systematic review. We remain at your entire disposal for further precisions and are fully committed towards the delivery of this contribution to the BMC Public Health readership to the best of its academic potential for knowledge advancement on workers’ mental health issues.

Thank you for your time and consideration.

Sincerely,

Nancy Beauregard, PhD (corresponding author)
Postdoctoral fellow
School of Industrial Relations, University of Montreal
University of Montreal Research Institute in Public Health
Montreal, Quebec
Canada
E-mail: nancy.beauregard.2@umontreal.ca

Alain Marchand, PhD
Associate Professor
School of Industrial Relations, University of Montreal
University of Montreal Research Institute in Public Health
Montreal, Quebec
Canada
E-mail: alain.marchand@umontreal.ca
Marie-Eve Blanc, PhD
Project Coordinator
University of Montreal Research Institute in Public Health
Montreal, Quebec
Canada
E-mail: marie-eve.blanc@umontreal.ca
Reviewer 1.

1.1. Major revision
1.1.1. Inclusion criteria for studies

As it stands, I don’t agree with your view that different analysis based on the same population sample is ‘independent’ in terms of this review. You may want to make your case for this clearer (either in your response and/or in the text), but I think at the least you need to state in the text how many different cohorts have been included in the review (you nearly do this at the bottom of page 10 but not quite), and refer to this and the risks of duplication in your discussion limitation section.

This comment rightfully points to the ambivalent use of the terms independent and non-independent studies in the following sentences (p. 9):

“We considered studies derived from the same cohorts but having different endpoints, different work and non-work exposures, or different mental health outcomes as independent. In the presence of multiple non-independent studies and studies relying on multiple analytical strategies, we selected the study yielding the highest NOS score. “

From an epidemiological standpoint, we fully agree with the position that only a certain number of independent studies were covered by our systematic review. In fact, among the 13 studies reviewed, only 6 of these were stricto sensu based on independently designed longitudinal cohorts [1-6]. Of the 7 remaining studies, 2 independently designed longitudinal cohorts, namely the National Population Health Survey [7-10], and the Whitehall study [11-13] were involved.

Concretely, the ambiguity raised may be adequately resolved by a) the explicit statement in the text regarding common origins of the studies derived from a single cohort and the shared partial data overlap (p.10); b) the explicit statement in the targeted paragraph that these studies were examined separately to alleviate further confusion (p. 9-10); c) a brief discussion of the implications of the methodological choice to include partial data overlap between studies derived from a given cohort (p.17); and d) a synthesis of the extent of the overlap in Table 2a. These solutions have for underlying rationale the need to validly maintain such studies in the analysis the reasons for which we will further discuss.

The methodological choice to integrate studies with partial data overlap from a single cohort was initially based on the high heterogeneity observed in the causal dynamics investigated. For clarity purposes, our inclusion criteria for studies cumulatively required: a) different endpoints; and b) a combination of substantively different work and non-work exposures, and different mental health outcomes. The Table 2a below presents the main distinctive features of these studies. This table was create to facilitate the current discussion.

From Table 2a, studies were ranked according to their methodological and conceptual quality in descending order. Additionally, we emphasized in bold characters analytical elements from studies derived from a same prospective cohort of workers that were specific to each study examined. For example, using the highest quality study from the
NHPS as reference[7], the study by Smith and associates was distinctive in its use of a combined measure of decision latitude to assess work exposure, and of different chronic stressors to assess non-work exposure. The same rationale was adopted for the Whitehall study, whereas compared to Stansfeld and associates[13], the study conducted by Furher and associates[12] was distinctive in its level of comprehensiveness of the network evaluated (e.g., extension to all nominated persons in measurement) and the use of an alternative indicator for mental health.

One of the main observations stemming from Table 2a resides in the appreciable non-overlapping dynamics singularly examined by studies from a single cohort, be the NPHS or the Whitehall study. Accordingly, and given the exploratory nature of our initial research question, we felt that the richness of these contributions needed to be reflected in our analyses. In the past, other narrative syntheses have distinguished for analytical purposes studies from a single prospective cohort based on different endpoints or on different research questions [14]. Clarifying such issue is of central importance to minimize duplication bias and overestimation of the results in systematic reviews [15]. In our review, two studies matched stricto sensu the criterion for non-independent samples. These studies were considered as such since they used the same data sets and predictors but differed in the operationalization of the outcome [7, 16]. We resolved this potential duplication bias by selecting only the study with the highest NOS score for further analysis [7].

One possible limitation associated with our methodological choice to allow for partial data overlap between studies derived from a common cohort could have yielded conflated results. This might have been especially true given the fact that our critical appraisal tool was partly based on consistency of the findings (i.e., with a 75% threshold for significant findings in the anticipated direction). Yet, different results were observed across studies within cohorts for single indicators.

For instance, in the case of the 4 NPHS studies, 2 of them reported no significant effect in time for household income based on different endpoints and operationalizations of psychological distress [7, 8], whereas 2 others reported a significant effect with again different endpoints but different outcomes [9, 10]. As with the Whitehall study II, a similar trend was evidenced. Contrasting Stansfeld and associates’ contribution with that of Fuhrer and associates yields substantive design variations relative to endpoints, outcomes, as well as in comprehensiveness of work and non-work exposures. We can tentatively hypothesize that distinctive causal dynamics at play potentially associated in such design variations might have accounted for notable differences in results, namely: a) mixed findings reported for the effects of combined community and network-level indicators (i.e., significant results in one case but not the other); b) gender differences; and c) differences in the magnitude of effect sizes detected for combined network and family-level indicators.

Again, we estimated that the exploratory nature of our research question needed to translate a balance between the level of comprehensiveness necessitated to allow for such an investigation to be conducted, and the level of restrictiveness in studies inclusion criteria necessitated to rigorously contain a conflation bias in the results. This
was best achieved in our view by allowing multiple studies from single cohorts to be considered for evaluation following stringent criteria at different stages of our methodology (i.e., cumulative criteria for studies inclusion, critical appraisal).

1.2. Minor revisions
1.2.1. Abstract
You describe this as a ‘qualitative’ systematic review – which may mislead readers into thinking you reviewed qualitative evidence. It would be clearer if you use the term ‘narrative synthesis’ as you do on page 8.

Recommendation accepted. Please refer to p.2 for revision.

1.2.2. Inclusion criteria for studies
You have had to make some arbitrary decisions (e.g only including studies of 200 workers – page 6). Even if they are unavoidable, I think the text should briefly draw attention to this arbitrary element.

Recommendation accepted. Please refer to p.6 for revision for the detailed modifications proposed.

1.2.3. Search strategy
To our knowledge, this review represents the first attempt at defining the non-work domain”. Page 7. I suggest you delete this as the following paragraph is about literature searching not defining the non-work domain – and I know (from personal involvement) at least one other review search that also had a non-work focus (BMC Public Health 2008, 8:239 doi:10.1186/1471-2458-8-239 – but if you delete that sentence, I see no need to reference). By the way, I think your search strategy is a real improvement on the one I developed for that.

Recommendation accepted. Please refer to p.7 for revision.

1.2.4. Terminology
‘whereas subjective indicators comprised subjective appraisals…’ – page 9 – could you expand a little on this explanation as at the moment it is rather circular and difficult to understand.

Recommendation accepted. Please refer to p.9 for revision. Redundancy in the use of the term subjective was addressed by substituting the latter by the expression workers’ appraisals.

1.2.5. Critical appraisal
There are several issues here that need to be either changed or better explained
a) I think you need to emphasise that ‘high quality studies’ as you define them are only high relative to the other included studies because the distinction is based on their score relative to the mean score of all the studies – so use the term ‘relatively high’. If the NOS
The expression referring to the relatively high methodological quality of included studies was introduced in the paragraphs presenting a) parameters for strength of evidence assessment (p. 9); and b) descriptive statistics of the included studies (p. 11). We also explicitly mentioned that the maximum score on the NOS scale was of 9, and the reasons explaining why each of the highest ranking studies (n=3) on this scale departed from that score (p.11).

b) Consistency of findings is based on 75% consistency. I found this idea interesting – but I think you will have to class cases where there is only one study for a particular outcome/level as something like ‘consistency not established’ –and therefore not be considered consistent. Have you done this? If so, make it clearer. If not, I think you need to.

c) …or a mixture of high and low quality studies independently from the strength of the association” – I didn’t understand this, could you have another go at wording

Clarifications were made (p. 9-10) regarding the presentation of the assessment criteria for the strength of evidence. Appendix IV Decision process for strength of evidence assessment was added to guide readers through the application of this assessment.

1.3. Discretionary

I would have been interested to see some discussion as to whether there was any evidence that results tended to differ by gender – e.g. are women more likely to be influenced by non-work factors than men?

This comment is highly relevant, especially given the fact that several epidemiological studies both in the general and in the workforce populations have reported significant gender-based differences in prevalence and explanatory dynamics for mental health [9, 17]. In all, 8 out the 13 included studies used gender as a stratification variable, which further limited the possibility to refine our results around this specific issue given the nature of the critical appraisal adopted. Nevertheless, the quality of the reported data could indeed make the initiative feasible statistically speaking[18] and constitute a complementary contribution to the present one.
<table>
<thead>
<tr>
<th>Study</th>
<th>Appraisal</th>
<th>Endpoints</th>
<th>Work exposure</th>
<th>Non-work exposure</th>
<th>Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>NPHS</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Marchand et al., 2005</td>
<td>6,7</td>
<td>T1-T4</td>
<td>Skill utilization, decision authority, job demands, physical demands, social support at work, job insecurity, work hours, work schedule</td>
<td>Societal occupational structure, mean occupational income, social support (someone), marital status, couple strains, household income, children aged 0-5 yo at home, children aged 6-11 yo at home, children aged 12-24 yo at home, children strains</td>
<td>Psychological distress (binary) (WHO-CIDI)</td>
</tr>
<tr>
<td>Smith et al., 2008</td>
<td>5,6</td>
<td>T1-T3</td>
<td>Decision latitude</td>
<td>Nb. of chronic stressors (friends, neighbourhood), Nb. of chronic stressors (partner, child, parent), chronic exposure to financial stress, household income</td>
<td>Psychological distress (continuous) (WHO-CIDI)</td>
</tr>
<tr>
<td>Shields, 2002</td>
<td>5,6</td>
<td>T1-T3</td>
<td>Self-employment status, weekend shifts, job strain</td>
<td>Occupational status, marital status, couple strains, children aged 0-12yo at home, household income</td>
<td>Psychological distress (continuous) (WHO-CIDI)</td>
</tr>
<tr>
<td>Shields, 1999</td>
<td>4,7</td>
<td>T1-T2</td>
<td>Self-employment status, rotating shift, work hours, job strain</td>
<td>Occupational status, marital status, household income, children aged 0-12yo at home</td>
<td>Major depressive episodes (WHO-CIDI)</td>
</tr>
<tr>
<td><strong>Whitehall</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Stansfeld et al., 1998</td>
<td>6,5</td>
<td>T1-T3</td>
<td>Decision latitude, job demands, social support at work, effort-reward imbalance</td>
<td>Social network index (friends, relatives; church, social clubs), confiding support, negative aspects of close relationships (closest nominated person)</td>
<td>Mental health functioning (SF-36, binary)</td>
</tr>
<tr>
<td>Fuhrer et al., 1999</td>
<td>6,4</td>
<td>T1-T2</td>
<td>Social support at work</td>
<td>Social network index (friends, relatives; church, social clubs), confiding support, practical support, negative aspects of close relationships (all close nominated persons)</td>
<td>Psychological distress (GHQ-30, binary)</td>
</tr>
<tr>
<td>Griffin et al., 2002</td>
<td>4,5</td>
<td>T3+T5</td>
<td>Decision latitude</td>
<td>Caregiving status (relative), marital status, number of children, home control.</td>
<td>Depression (GHQ-30, binary)</td>
</tr>
</tbody>
</table>

*Note.* Emphasis was added to demonstrate specific indicators pertaining to each study relative to other studies from a common cohort.
Reviewer 2.

2. Revisions

2.1. Theoretical background

According to authors (page 5) “in the interests of conceptual clarity we have borrowed from our past work and that of Berkman and Glass on social integration to designate these other life environments as the non-work domain. Accordingly, the non-work domain can be posited to shaped mental health through causally and dynamically intertwined mechanisms at three levels of analysis”. They then lay out macrosocial, meso-social, and family levels. I’d like to know where the individual is in all this? In particular there is a long literature on the role of stressful personal life events (losses etc) in a person’s life on mental health. This seems like a fourth dimension that is missing in their model. I’m not suggesting that they add this back in but that at least in their selection of this 3 level approach they address the issue of why the individual level is not in their model. As well, the selection of their 3 level approach is based on their work and Berkman and Glass. This seems a bit narrow. Is there a bigger set of academic supports they can bring to bear on this (more refs).

This insightful comment led to an important expansion the authors’ own published work in the Theoretical background section (p.4-5). Accordingly, should we pursue the systemic analogy of the non-work domain, we view all the micro-, meso- and macrosocial structures (i.e., family, networks, community/society) as distinctive features of the non-work domain. According to the sociological theory of agency-structure from which our work borrowed[19, 20], features of the social structures are to be analytically distinguished from that of individual agents. Rather, individual agents’ or workers’ attributes encompass the "reflectiveness, rationality, creativity, demography, affect, the body, biology, representations, perceptions, motivations, habits, and attitudes"[7]. Because the aim of our study was to delineate the specific contribution of the non-work domain on workers’ mental health, we did not expand our inquiry to workers’ individual attributes beyond documenting their inclusion in studies’ adjustment strategy (see Table 1- Description of included longitudinal cohort and case-control studies). The clarification sought by this comment was integrated in the revised version of the article.

As highlighted, we also referred to a recent systematic meta-review on key psychosocial risk factors in community settings[21]. In their theoretical background, Egan and associates[21] borrowed notably from Putnam’s work on social capital and MacIntyre’s work on neighbourhoods as social structures of importance for population health, key references which were also targeted by Berkman and Greenglass’ framework[22] used as a pivotal anchor point in our article. Egan and associates[21] also adopted a similar systemic approach of the psychosocial environment or of the non-work domain as we seek to characterize it. Accordingly, the analytical levels considered either the microsocial structure of the family (i.e., “home”) or a combined meso/macrostructural structure of the community (i.e., encompassing such references as "friends", "neighborhoods").

Our approach to the non-work domain as a three-level systemic structure thus offers a complementary reading of these key contributions. It also provides additional theoretical and empirical support for the relevance of untangling the
micro-, meso- and macrosocial structures, the latter of which refers to distinctive psychosocial pathways of interest for population health in our article (p.4-5).

2.2. Search strategy
Under “Search Strategy” The sentence “To our knowledge this review represents the first attempt at defining the non-work domain”. I don’t think these authors actually mean this. This is the first attempt perhaps, at a systematic review of occupational literature to determine the extent to which the non-work domain is utilized effectively in investigations of occupational mental health it is not the “first attempt at defining the non-work domain”.

Recommendation accepted. We suppressed the sentence to alleviate any ambiguity (p.7).

2.3. Recommendations
But recommendations felt a bit weak. Recommendation 1 is that future longitudinal research consider both work and non-work determinants of occupational mental health. The authors point out that this is important because we need to accumulate more studies of this issue in order to build the evidence base. Ok, but surely, based on this major review of these types of studies the authors have something stronger than “do more studies” to say. For example, based on their review they must have some opinions on what type of approach, conceptually one might take to better incorporate non-work domain into these types of studies.

Again, recommendation number 2 is that “better consideration about conceptualization and operationalization” of non-work domain is needed. This is obvious. The authors have undertaken a major systematic review. Surely they have more to say about how one might conceptualize and operationalize in this way?? I think the authors must be much bolder in their recommendations especially as they have content knowledge from their own research and this fine study to offer.

Our recommendations primarily sought to open the discussion to all researchers from the field of occupational mental health about the necessity to expand current investigations of the social aetiology of mental health problems most frequently investigated in the workforce. To achieve this, a rigorous demonstration of such gaps needed to be proposed at the conceptual and operational level.

At the conceptual level, we connected key contributions (i.e., work by Marchand, Berkman, Egan and their associates) from diverse academic disciplines such as sociology, psychology and epidemiology to support the view that the social aetiology of workers’ mental health could relevantly be defined, understood, and researched as an integrated topic. In doing so, we provided theoretically-driven guidelines aiming at better identifying the specific nature, pathways, and analytical levels from which the non-work domain comes to affect workers’ mental health.

The originality and contribution of our study lies in its demonstration of important underinvestigated areas in research, supported by the application of a novel conceptual critical assessment tool. While we did not provide for novel ways to reconceptualise key indicators (e.g., subjective appraisal of children strains[7]), we did emphasize the potential relevance of increasing research efforts in specific areas by relatively
positioning and comparing most relevant indicators, pathways, and analytical levels one with another.

In sum, the driving idea underlying our study was to provide a structured map from which extent knowledge on non-work determinants of workers’ mental health could be benchmarked. Future efforts seeking to refine the substantive content of any of the investigated indicators, pathways or analytical levels are undoubtedly of critical importance for research advancement. We hope that our contribution humbly offered some basis for discussion as to where such research needs are most pressing in that matter.
Reviewer 3.

3.1. Major revisions

3.1.1. Terminology

The term 'occupational mental health' needs to be defined - or perhaps substituted by other terms. This also apply to the title and abstract. It is stated that the review examine the relative contribution of non-work determinants in predicting occupational mental health. There is no such condition 'occupational mental health'. I assume the authors are interested in the relative contributions of work- and non-work determinants of 'mental health' - which by definition implies working people (currently or earlier). I suggest that this is clarified throughout the paper.

Recommendation accepted. We replaced throughout the article any reference to the term *occupational mental health* by workers’ mental health or similar expressions.

3.1.2. Inclusion criteria for studies: population

Why should sampling be community based? Does this mean that sampling of populations based upon workplaces are not included (which does not seem reasonable)?

For clarity purposes, we provided the additional information in the text (p.7): "Firstly, we opted for community-based as opposed to clinical-based sampling of workers to ensure that selected workers were not followed for other concurrent medical conditions implicating potential reverse causation effects of mental health on workers’ assessment of their work and non-work exposures[23]."

3.1.3. Inclusion criteria for studies: outcomes and exposures

The authors define three non-work domains and three outcomes - how did these become part of the eligibility criteria?

- Was it a request that each included study was measuring all non-work and work dimensions?

And what about the three considered outcomes? In the latter case: exactly which?

This information may be in the appendices but needs to be explicitly defined in the man in text.

We’ll address first the part of this highly relevant comment pertaining to the nature of the non-work exposure considered for eligibility criteria. For clarity purposes, we first lay the definitional basis of the non-work domain in the Theoretical background section of our study. Following key integrative contributions from multiple disciplinary background (Marchand et al., sociology; Berkman and Glass, sociology, psychology; and in the revised version, Egan et al., population health and epidemiology), we first defined the non-work domain as "the non-work domain can be posited to shape workers’ mental health through causally and dynamically intertwined mechanisms at three levels of analysis: 1) the macrosocial level of community or society (e.g., culture, socioeconomic factors); 2) the mesosocial level of networks (e.g., social network structures, characteristics of network ties); and 3) the microsocial level of the family unit (e.g., marital and parental relationships) ".

This definition was then directly transposed into our bibliometric search strategy with keywords referring to specific indicators associated to each micro-, meso- and
macrosocial level comprised in our definition. A manual confirmatory search strategy omitting these keywords was also conducted to account for the possible non-reporting of factors of interest either in the indexing of the article, its title or abstract (e.g., marital status is a good example). The connection between the Theoretical background and the Methodology section was established with the sentence: "Drawing from our framework, we defined non-work exposure from the levels of analysis (e.g., family, networks, community, society) that describe the non-work domain[7, 22]. " (p.6).

At the minimum, any study cumulatively satisfying eligibility criteria listed at p-6-7 needed to fit one last criteria for work and non-work exposures, which we clarified at p.7: "Fourthly, a multivariate evaluation of work and non-work exposures with reports of their respective effect sizes was required". Hypothetically, any given study including only one work and non-work factors matching all eligibility criteria could have been selected.

The second part of the comment deals with the determination of mental health outcomes considered by the systematic review. Essentially, we opted for a definition of workers’ mental health that would translate commonly investigated mental disorders in the literature[24]. This was stated in our methodology: "The definition of mental health put forth encompassed three widely investigated outcomes: psychological distress, depression, and burnout" (p.6). Further, our definition of mental health focused only on mental disorders leading to potentially pathological yet reversible effects on health[25]. A recent meta-analysis on the effects of the psychosocial work environment on mental health alternatively integrated irreversible effects (i.e., suicide) in its definition of workers’ mental health[26]. This distinction was maintained in the bibliometric search strategy fully reported in the Appendix I. We also provided readers with parameters considered in the elaboration of the bibliometric search strategy, referring to past validated strategies used by previous systematic reviews on the outcomes: "We elaborated an electronic search strategy that included indexed and free terms in keeping with similar research on outcomes[26, 27] ".

3.1.4. Critical appraisal

Appraisal and synthesis of the evidence: it is not entirely clear when the authors describe quality rating of the individual studies and when they outline the principles for the evaluation of the evidence across included studies.

Criteria 1 and 3 for 'evidence rating', page 9, are not defined clearly: 1) what is meant by a NOS score > mean NOS score?? 3) OR=0.75 is not corresponing to OR=2.0?

How was the strength criterion applied when several studies where available? Should all studies report OR ratios above 2.0 or below 0.75??

As I understand the criteria, the evidence was considered strong if one high quality study reported a significant association in the anticipated direction with a relative risk of >2.0 (or<0.75). This is hardly reasonable?

Clarifications were made (p. 9-10) relative to the presentation of the criteria for assessment of the strength of evidence. Appendix IV- Decision process for
establishing the strength of evidence was added to guide readers through the application of this assessment with concrete examples.

Specific details for the interpretation of the assessment of the strength of evidence were also added. For instance, criteria 1 now states "adequacy of methodological quality (relatively high methodological quality set at NOS score > mean NOS score) " (p.10). The mean NOS score refers to the average score obtained using the Newcastle-Ottawa scale to determine the methodological study quality. On p.12, we provided the descriptive statistics ($M=5.5, SD=1.2$) allowing for the identification of relatively high-vs low-quality studies. For parsimony purposes, we chose to report this information in Table 3- Summary of the strength of association by simply referring to high-quality studies as NOS score $\geq 6$, and low-quality studies as NOS score $<6$.

As with the criteria 3 relative to the magnitude of the reported associations, we followed the method applied by other authors [26] to determine strong associations acting as either a risk factor ($OR \geq 2.0$) or a protective factor ($OR \leq 0.75$) for workers’ mental health. Given the fact that our study sought to reflect current diversity in the operationalization of mental health outcomes varying from continuous to binary outcomes[28], studies based on continuous operationalizations were accounted for in the evaluation of criteria 2 for consistency of the findings, but not integrated for the strength of association. This certainly is a trade-off of our evaluation system, confining studies based on continuous operationalizations at the best to evidence of moderate strength only. Nevertheless, this shortcoming appears in our view compensated by the inclusiveness of our rating system allowing for a greater diversity in outcome measurement to be detected and better reflected in the systematic review.

3.1.5. Results
Is it reasonable not to distinguish between the outcomes psychological distress and depression. These conditions are not assumed to have the same determinants. If so they should be treated separately (cf Table 3).

This comment relevantly addresses the fair level of heterogeneity characterizing measurement issues about workers’ mental health in the literature. This heterogeneity is well illustrated by the Table 1- Description of included longitudinal cohort and case-control studies. To date, extensive psychometric evaluations still report mitigated results in ascertaining clear differences between screening instruments such as the GHQ or the SCL-90[29]. In their meta-analysis, Stansfeld and Candy[26] reported conclusive moderate effects of psychosocial work factors on common mental health disorders without further distinction between psychological distress and depression. Nevertheless, we have indeed shown in a recent cross-sectional study on Canadian police officers differences in psychosocial pathways linking the work environment according to specific mental health outcomes [24]. Results demonstrated a greater explained variance for burnout (MBI-16) as opposed to psychological distress (GHQ-12) or even depression measured by a screening instrument with a high sensitivity for clinical depression (Beck Depression Inventory-21) by the models integrating work and non-work factors. While we denoted some common pathways (e.g., psychological demands) between the three investigated outcomes, differences were also evidenced (e.g., decision
latitude predicted only burnout). Hence, the extent to which such heterogeneity appears maintained in the explanation of differential pathways for mental health outcomes certainly warrants further investigation from prospective studies, with a particular attention given to the assessment of multiple outcomes (e.g., psychological distress, depression, and burnout) in a single working population.

3.2. Minor revisions

3.2.1. Table 2

Table 2: not easy to understand. There is a need to define the terms (analytical breadth etc) and explain meaning and range of stars. The TOTAL looks as numbers with decimals even they are not. Please, clarify.

Recommendation partially accepted, a semicolon for clarity replaced the coma (please refer to p.42 for revision). For parsimony purposes, we chose to redirect the readers to the Appendix II where full details of the scoring systems are presented, adding to the description provided in the main text (p.11). We view the relevance of reintegrating such information for the benefits of the readers as suggested; the only constraint may be at this stage space limitation.

3.2.2. Table 3

Table 3: an error: NOS>6: 0/5=40% positive.

Recommendation accepted. Please refer to p.42 for revision.
Editor’s comments

1) Further consideration of your manuscript is conditional on improvement of the English used.

We certify that the English revision has been conducted as described in the Acknowledgements (p.20).

2) Please can you ensure that your manuscript adheres to MOOSE guidelines

Please refer to Appendix III for the MOOSE checklist.

3) Please can you ensure that the Background section of your Abstract needs context information.

The relative contribution of this systematic review was put into context with regard to the general literature on the work environment as a social determinant of workers’ mental health in the Background section of the abstract (p. 2).
References


