Reviewer's report

Title: Development of mental disorders one year after exposure to psychosocial stressors in primary care patients with a physical complaint; a cohort study

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Reviewer: Susanne Knappe

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Comments to the authors

In its current form, the manuscript reads interesting, but at the same time lacks preciseness in terms and statistical analyses that substantially diminish the quality of this work. I therefore recommend the paper for “major compulsory revisions”. I have outlined shortcomings (from my point of view) to the authors below.

Abstract

1. The term principal mental disorders is used here for the first time. The term is introduced later in the manuscript, and I would appreciate if the term was already explained in the abstract (or replaced by i.e. incident mental disorders, or incident anxiety, depressive, somatoform disorders) .

2. Similarly, it does not yet get clear what is meant by “bother a lot” – i.e. subjective distress?

3. I think a word is missing in this sentence: “Patients who are bothered a lot by a stressor are therefore 2.5 times (CI95% 1.5; 4.0) more likely to experience a mental disorder at one year later.”

4. Notion of a putative causal association is tempting, but not justified by data quality and analyses.

Background

5. The section starts out with a major argument on the prevalence of mental disorders. Thus, I would suggest to present some numbers (which in turn would facilitate to follow power calculations. In addition, what about substance use disorders as a substantial burden to mental health?

6. “Many patients, at the onset of a mental disorder, first attend their general practitioner (GP) with a somatic complaint with or without underlying organic pathologies [4] putting the GP first in line to explore psychosocial distress as is expected by the patients [5, 6]. – Sentence is not clear to me.

7. Epidemiological studies and causality: I understand that the failure to demonstrate causality is one of the major problems in (mental health) research; nonetheless, most studies (irrespective of epidemiological, clinical, observational,
naturalistic etc studies) are not adequately designed to examine putative causality (see. Kraemer et al. for further information). Similarly, neither is this study. I felt it would be more appropriate to refrain from any causal conclusions in the manuscript.

8. .. to explore the onset..” what do you mean by onset: first onset, incidence ??

Methods

9. Patients spontaneously reporting physical complaints. I speculated you mean to examine patients with acute physical complaints / symptoms ?

10. Comment on the refusion rate at baseline and drop out rate from baseline to follow-up.

11. In lieu of case definition, I was a bit confused by inclusion criteria for somatofom disorders: when patients are required to have a history of chronic somatization – how can they be diagnosed at follow up for the first time? Then “chronic” would refer to a maximum of 1 year, right? Also, the DSM requires onset before the age of 30 – so how many cases were excluded already at baseline and what was incidence rate for somatoform disorders (and for anxiety and depressive disorders as well) at follow-up?

12. Description of follow-up assessment needs some refinement: what was the mean interval between baseline and follow up (also min, max?). was the number of consultation within this interval assessed?

13. Information on the psychometric properties, number of items, validity of the PHQ…etc is needed. Then, also the respective discussion section becomes clearer.

14. Did the GP had access to PHQ data, i.e. in terms of need for depression treatment?

15. Linearity of association between number of stressor events and mental disorders is questionable with regard to ceiling effects (i.e. increase of relative risks decreases with number of events)

Results

16. Patients with complaints had a higher risk for mental disorders one year later. This should be clearly acknowledged in presentation of results as prospectivity of associations was ensured by design and in part also by the analyses plan.

17. Throughout reading, I felt that a number of putative covariables were not assessed, or at least, not included in the analyses: findings may also be affected by the number of physical complaints, duration of physical complaints, illness severity, number of events, number of diagnoses according to PHQ, comorbidity with other mental disorders such as bipolar or substance use disorders or somatic disorders such as diabetes, hypertension etc. This substantially affected reviewing the manuscript and I am highly interested in how the authors handled this.

18. Also, do you have any data on onset of physical complaints, duration etc as well as onset of principal mental disorders. The latter appear to first manifest
during the follow-up period, but in few cases, they may have already been present at time of first assessment – albeit in minor manifestation.

19. Additional analyses on the role of gender and psychological treatment are intuitively clear; however they came out of the blue and the rational/hypotheses were not clear to me.

Discussion

20. The argument on the study sample as a subset of patients rarely seen in primary care needs some refinement, as some readers may not follow the author at first glance. That is, information on the core incidence period of the disorders considered here is needed, as well as information of the age distribution of the sample.

21. I do not agree that subsuming three disorders under “principal mental disorders” also suggest that they have common biological underpinnings. This is too far.

22. A further limitation may be that physical complaints may be in fact attributable to mental disorders (i.e. false positive)

Tables

23. Table 1 and 2: base rates for individuals exposed – not exposed to stressors are needed. Similarly, it is not ultimatively clear, to what base rates percentages refer to.

24. Table 1: p –value in rows is redundant; subjective health status – can you please verify why a substantial proportion of participants reported excellent or very good health, albeit they attended a primary care setting for acute physical complaints? Maybe this information referred to another time period a priori? This should be indicated then in the tab.

25. Table 3: initial exposure to a stressor - “initial” comes out of the blue and is not justified by your assessment strategy. It is likely that the majority of respondents experiences similar stressors in the period before the study.

26. Table 4: indicate what is meant by psychiatric comorbidites.

Level of interest: An article whose findings are important to those with closely related research interests

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.