Reviewer's report

Title: Incidence and prognosis of non-specific chest pain among patients recruited from consecutive series of hospitalizations for acute coronary syndrome

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Reviewer: mohammed justin zaman

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Review for BMC Medicine on: Incidence and prognosis of non-specific chest pain among patients recruited from consecutive series of hospitalizations for acute coronary syndrome

The question is of considerable interest, and well-defined. However, as I write below, they cannot claim to be studying incidence. It is a relatively novel area.

Major Compulsory Revisions

1. Abstract methods are not clear – they should clearly state this is a systematic review or literature search.

2. Introduction: “The premise of the review is that obtaining such information is necessary in order to optimize the management of NSCP patients” - please clarify what ‘information’ you mean.

3. In the methods, the sections ‘Risk of bias in individual studies’ and ‘Risk of bias across studies’ are best transferred to the discussion. The methods are otherwise well-described and the supplementary files useful. The data are sound but more should made of their heterogeneity – for instance, meta-analysing these data would lead to a rather high I² value. The manuscript adheres to the relevant standards for reporting.

4. The authors refer to ‘incidence’ throughout – strictly this is not correct – as these are not cohort studies examining first presentation of chest pain. The authors are actually taking prevalent cases and assessing their prognosis.

5. The methods superficially discussed the papers, and I think some of the important facts and numbers about prognosis should be brought into the results section. The manuscript is organized in such a manner that it is thus a little illogical or not easily accessible to the reader – I would suggest bringing out more of the results into the results, and not leaving it all to the discussion where no new findings should be presented (e.g. – it is not good writing to start the discussion with the line “Among consecutive patients hospitalized for chest pain in order to rule out ACS, the minimum incidence of NSCP was 18% and the maximum 82%.”). In particular, the comparative prognosis between those with ACS and those with NSCP should be brought out. Accordingly, I think table 1 needs to be more filled-out with these figures. Bias is discussed well.

6. In the conclusions, the authors write that “Due to co-existing CHD in nearly 40% of these patients, their prognosis is not necessarily benign” – that is of
course true due to their co-existing CHD, but such patients understandably might be more concerned about pain that is musculoskeletal and that might not worry others without a history of CHD. This needs to be discussed in more detail along with the excellent statement made earlier that “gastrointestinal, psychiatric and musculoskeletal disorders may be managed and hopefully be of value to the patients’ quality of life.” I and other have shown that a high-risk group (South Asians) with atypical pain should have their risk factors managed properly as opposed to worrying about their pain [Zaman MJ, Junghans C, Sekhri S, Chen R, Feder G, Timmis A and Hemingway H. Presentation of stable angina pectoris among women and South Asian people. CMAJ 2008; 179(7):659-667] and they should read this and consider citing it. The authors of this paper need to discuss the importance of reassurance and appropriate non-cardiac management to the patient with a cardiac history but who genuinely may now have non-cardiac pain.

7. I do not really like the title – I would suggest ‘Prevalence and prognosis of non-specific chest pain among patients hospitalized for acute coronary syndrome - a systematic literature search’ – or something like that.

8. There is a good paper here somewhere, and a lot of hard work seems to have gone into the systematic review. However, the authors have not done that justice in the discussion. They need to bring out the figures more – separating the background information on the studies from the discussion of NSCP misses a trick. The reader needs to be clear how important NSCP is to prognosis compared to those with ACS, or indeed to those who have an ACS fully treated and discharged. I am not clear how much having NSCP means over and above having a history of CHD. Better justification is needed for the arguments based on existing data and the clarity and coherence of the paper needs to be improved.

Minor Essential Revisions

1. The writing is acceptable though needs some minor tweaking.

**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