Reviewer’s report

Title: High prevalence of lack of knowledge of symptoms of acute myocardial infarction in Pakistan and its contribution to delayed presentation to the hospital

Version: Date: 13 March 2007

Reviewer: Sharon McKinley

Reviewer’s report:

General

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

The manuscript reports on an interesting research project from Pakistan on the problem of delayed presentation for treatment of symptoms of acute myocardial infarction (AMI), now more commonly referred to as acute coronary syndrome (ACS) and some of the predictors of delayed patient presentation after symptom onset. The authors make a cogent case for the study based on the increasing prevalence of coronary heart disease and heart attack in South Asia and the benefits of early reperfusion therapy, but do not inform the reader of the availability of thrombolytic or percutaneous reperfusion therapies in Karachi. Although there are other clear benefits of early hospital treatment for heart attack, e.g. detection and treatment of life threatening arrhythmic and ischaemic complications, the rationale for the study would be more coherent if the reader knew that patients who presented early were likely to receive thrombolytic therapy.

There are some questions about the conduct of the study that need to be addressed:

1. Should the sentences on p 6-7 re recruitment read “Every patient admitted to the hospital with a first AMI and surviving the initial 24 hours was screened for eligibility for the study. Eligible patients were approached by a trained medical research officer to seek informed consent, and enrolled in the study after obtaining informed consent.” The present description of enrollment does not make sense.

2. The response rate was exceptionally high at 100% of patients approached. Was there any ethics committee of other institutional review process carried out to independently assess the procedure by which patients were approached and informed consent sought? Was there an approved written or verbal patient information statement that patients received before being asked to give informed consent?

3. What was the content of the interview that the patients participated in? Some of this is suggested in the results section and the tables and figures, but it is not clear if patients were presented with a list of possible reasons for delay or asked an open-ended question, or whether the few number of symptoms of heart attack that are reported were the only ones that patients were asked about. This last point is important as the authors’ main inference from the results they report is that knowledge of heart attack symptoms needs to be improved to decrease delay times, yet the number of symptoms they appear to have asked patients about is quite limited (i.e. no arm, shoulder or jaw pain/discomfort, no nausea, no sweating, no shortness of breath, no fatigue). The significance of some of these symptoms has become more evident since 2003 when data were collected for the study reported in the manuscript. However there appear to be limitations to what the patients were asked to respond to as symptoms and reasons for delayed hospital presentation. If so these limitations need to be more fully acknowledged in the discussion of the results.

4. The authors state in the methods that a p value of <0.05 was considered statistically significant in the analyses, yet they report (correctly) 95% confidence intervals for the ORs in the results (not p values). There should be consistency on the parameters stated for inferring statistical significance. More importantly, how do the authors justify their inferential statements in several places – the abstract, page 10, page 11, page 13, page 16 – about an interaction between higher education levels and tobacco smoking on delay times when this does not meet their a priori level of statistical significance, and has a tenuous logical link for which no other empirical support is offered.

5. How were continuous variables such as age and VAS pain scores categorised for the cross tabulation
and regression analyses, especially pain levels which were found to be significant?

6. Why is the sensitivity analysis not described in the statistical methods, and some of the data introduced in the discussion not reported in the results?

In the discussion there needs to be reference to the body of research that shows that efforts to change knowledge do not necessarily lead to change in behaviour, particularly the REACT trial in the context of reducing delay in responding to heart attack symptoms.

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

There are quite a few English expression and presentation corrections that would be warranted if the methods issues described above are satisfactorily addressed by the authors.

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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 whose findings are important to those with closely related research interests

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

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests