

NURSING RESEARCH REVIEW FORM

1st Review

Manuscript # 2006/036
Manuscript title: Symptom clusters in acute myocardial infarction: A secondary data analysis
Manuscript type: Regular
Number of text pgs.: 14
Number of figures: 1
Number of tables: 4
Reviewer: Mark Parshall, PhD, RN
Stat reviewer assigned?: No -- do you recommend a stat reviewer? No

Please evaluate the following with these choices: (1) adequate, (2) inadequate (describe in written review) or (3) not appropriate (describe in written review)

Problem statement: 1
Attention to relevant literature: 1
Theoretical framework: 3 (see review)
Research design: 1 (however see comment 1 in review)
Data analysis: 2 (see comment 2 in review)
Discussion of results: 2 (see comment 3 in review)
Organization: 1
Writing style: 1

Please rate the following topics 1-5 (with 5 being the highest rating):

Value of topic: 4
Probable reader interest in topic: 4
Importance of present contribution to nursing: 4
Priority of topic for publication: 4
Rank this manuscript for its value: 4

Reviewer's Recommendation (please type "X" after your choice):

Accept without revisions
Accept with revisions XX
Maybe accept with revisions
Do not accept

**Please provide a comprehensive and integrated review of this manuscript.
Be sure to present a balanced view of the manuscript's strengths and weaknesses.**

MS # NR2006/036: Symptom clusters in acute myocardial infarction

This is an interesting investigation that has implications for developing a more nuanced understanding of symptom presentations among patients with acute myocardial infarction (AMI). The study is a secondary analysis of data from nine cross-sectional observational studies conducted primarily in the United States (with one study from the United Kingdom). As with much exploratory research, there is not an explicit conceptual framework. That is not a problem *per se* (i.e., the work itself is a step in that direction), but it would be helpful for the authors to make an explicit statement that their approach is somewhat atheoretical and data driven. A potential issue with the Background section is the emphasis on how better understanding of symptom clusters might facilitate earlier recognition of AMI symptoms by members of the public and possibly enhance case identification by professionals. Because the sample consists exclusively of persons in whom an AMI was diagnosed, it is not suitable for making judgments of how discriminating any particular symptom or cluster is (see 3b below).

A strength of the study is a relatively large aggregate sample ($N = 1073$). A novel aspect of potential interest to the readership of this journal is the use of latent class analysis (LCA), a technique rarely used in published nursing research (of a mere 23 hits in CINAHL, only one was in a core nursing journal). The advantage of LCA compared with cluster analysis is that LCA has more clearly defined methods for identification of an adequate model or solution (i.e., somewhat less 'art' is involved). On the other hand, LCA is still an inductive, exploratory method, so results at this stage must be considered provisional hypotheses in need of subsequent support in independent (ideally prospective) samples.

In general, the manuscript is clearly written. The authors provide a credible, if not compelling, argument for a model consisting of 5 symptom clusters (hence patients who could be classified accordingly) consistent with an AMI. The main issues that concern this reviewer are: (1) insufficient information on the source studies with respect to how they were identified, when they were conducted or published, and in terms of case definition for AMI; (2) some of the data analysis could be presented more clearly and in ways that could enhance clinical interpretability; (3) inattention to several substantial methodological limitations.

- (1) More should be said about the source studies (e.g., how they were identified, when they were conducted and published, and whether any were unpublished studies). Those things are de rigueur in systematic reviews and meta-analyses. Because definitions of AMI have evolved over the years, I do think it is important at least to indicate in what years the studies were conducted.

Under study eligibility requirements (p. 6, statement [b], lines 18-19), some clarification is needed. The statement indicates that AMI had to have been validated by *both* serum cardiac markers and ECG changes in the source studies. That implies that patients with non ST-elevation MI might have been excluded from source studies. Was that the case? Also, were any of the source studies of a vintage that predated troponin testing? In statement (d) (lines 20-22) some clarification would also be helpful. Did a study have to include specific questions for every one of the mentioned symptoms? Or is this just a catalog of all symptoms across all the included studies? The former is a far more restrictive eligibility criterion for a potential source study than the latter, but the former also makes the count and percentage data in Table 2 more internally valid, whereas the latter might have enhanced external validity by increasing the number of potentially eligible studies.

It would be helpful in Table 2 to order the symptoms by percentage of occurrence (i.e., move neck or jaw pain to below indigestion). I note in passing that if all symptoms were inquired about in all the included studies, the percentages in Table 2 amount to the Sensitivity (true positive rate) of the symptom for AMI.

- (2) In general, the description of LCA methods is clear. The case for the 5 cluster model is more persuasive in terms of being better than 4 clusters, but a little weaker in justifying that 5 was

better than 6 or more. In particular, the model fit data in Table 3 are a little opaque without considerable reference to what is stated on pp. 8-9 (footnoting of abbreviations in the table and a more explanatory caption would help). The authors correctly state that, in contrast to the other fit statistics, the BIC is more indicative of a 4 cluster model.¹ But it is evident in Table 3 that the other three indices are still decreasing at cluster 6. The authors note (on p. 8, lines 22-23) that the decrease at that point was “minimal (<2% change in each)” which sounds reasonable, if a little ad hoc in relation to their statement, “As the model fit improves, the absolute values of the fit statistics decline” (p. 8, lines 20-21). They also note that the difference in L^2 was statistically significant between 4- and 5-cluster models (p.8, line 23-p.9, line 1). If the difference was no longer significant between 5- and 6-cluster models, it would be helpful and more persuasive for the authors to state that explicitly (i.e., it would help justify the implication that minimal decreases are tantamount to no longer declining).

In my opinion, it would be helpful (space permitting—the editor’s judgment) to include another table that sorts the symptom labels by clusters (in rows) by high, medium, or low conditional probabilities (as defined by the authors on p. 8, lines 1-4), e.g.,

Cluster	High	Medium	Low
1	Chest Pain Weakness Shoulder / Arm / Hand Pain	Sweating Fatigue SoB Nausea / Vomiting Dizziness / Lightheadedness	Back Pain Neck/Jaw Pain Indigestion Abdominal Pain

....
Etc.

I realize that the Figure is an attempt to depict this visually, but it’s a little difficult to grasp because it is a busy figure and, as in Table 4, the ordering of symptoms is invariant while the conditional probabilities vary from cluster to cluster.

(3) With respect to strengths and limitations, I note the following

- a. P. 15 lines 6-7, the statement about the generalizability of the findings is arguable because it depends to a considerable degree on the sampling designs of the source studies which are opaque to the reader.
- b. The authors correctly note a general limitation of secondary analysis (p. 15, lines 14-20); one is always hostage to the methods and operational definitions of the source studies. A further limitation is that the sample consists only of cases of confirmed MI. Depending on when the source studies were conducted, there may well have been differences in how the diagnosis was ascertained (e.g., the advent of troponin testing altered case definition of MI and treatment algorithms quite substantially). Even if the serum biomarker in all source studies was a cardiac troponin, limiting inclusion to cases of confirmed AMI provides only a partial picture of symptom presentations consistent with AMI. Assuming that all symptoms in Table 2 were sought in all included studies, the percentages in Table 2 amount to sensitivities only; there are no data from which data on specificity could be extracted, hence there is insufficient information on which to base any kind of clinical discrimination between AMI and anything not an AMI (e.g., unstable angina, PE, aortic dissection, or symptoms of non-cardiopulmonary origin). That’s not a fatal flaw by any means (one has to start somewhere, and considering the potentially devastating costs of missed diagnosis, a sensitive set of screening criteria is as good a place as any to start), but it is an important limitation when inclusion in a data set is based on a confirmed diagnosis.

Minor comments.

¹ Loken (2004) notes that the BIC “is known to be conservative” (p. 634) relative to other fit indices.

In the Background, it seems like the study by Ryan and Zerwic (2004, cited on p. 5, lines 4-6) has some direct relevance and comparability to the present study. A short paragraph in the Discussion comparing similarities and differences in findings might be enlightening if space constraints permit.

On p. 11, I imagine that the repeated η^2 should be χ^2 (use lower case c and change to symbol font for Greek letter chi).

On p. 13, line 18, the antecedent for "These findings....." is unclear, and it is not clear how the findings discussed in the previous paragraph relate to sex.

Some of the reference citations use abbreviations of journal names. Under APA guidelines, full journal names should be used (excluding an initial "The".)

Reference

Loken, E. (2004). Using latent class analysis to model temperament types. *Multivariate Behavioral Research*, 39(4), 625-652.