Author's response to reviews

Title: Discrepancies Between The Medical Record and The Reports Of Patients With Acute Coronary Syndrome Regarding Important Aspects Of The Medical History.

Authors:

Chete M Eze-Nliam (cezenli1@jhmi.edu)
Kellie Cain (khirt1@jhmi.edu)
Kasey Bond (kbond7@jhmi.edu)
Keith Forlenza (kforlenza@loyola.edu)
Rachel Jankowski (rljankowski@loyola.edu)
Gina Magyar-Russell (gmagyar1@jhmi.edu)
Gayane Yenokyan (gyenokya@jhsph.edu)
Roy C Ziegelstein (rziegel2@jhmi.edu)

Version: 3 Date: 7 March 2012

Author's response to reviews: see over
Dear Reviewers,

Thank you for reviewing our manuscript and giving us very helpful feedback. Please kindly see our responses to your comments below.

REVIEWER 1

1. **Title**: I wonder if the subtitle “Discrepancies in Medical History” isn’t just redundant to main title.

   On the title page of the revised text, we make it clearer that “Discrepancies in Medical History” is intended to indicate a “Running Title” and should not be part of the title of the manuscript. We apologize for causing confusion on this point.

Abstract and throughout paper:

1. *The authors should specify up front that they analyzed data from a total of 62 respondents.*

   This is now clearly indicated in the Abstract (page 3, lines 5 to 6) and also in more detail in the Results section (page 8, lines 124 to 127)
2. The Results section in the Abstract seems to suggest that education level contributed to agreement ORs for all respondents. It looks to me as if this variable was only significant for those respondents with some college in your unadjusted model. Can the authors report this more clearly for readers in abstract?

   Thank you for noting this. We have now revised the Abstract as suggested to indicate this more clearly (page 3, lines 14 to 15). We have also reported it more clearly in the Results section (page 9, lines 159 to 163) of the revised manuscript.

3. What does “improved understanding” mean in the Conclusion section of the abstract. I never seem to hear how this might be obtained anywhere in the later parts of the paper. Please clarify.

   The reviewer is correct that our work highlights the prevalence of potentially important discrepancies, but does not identify how more accurate health information can be obtained. The Abstract has been revised to indicate that this should be a focus of future research (page 3, lines 18 to 19).

Introduction

1. The authors really must define what ACS is for your readers not familiar with this term, or how it may be different/similar to AMI.

   The terms “acute myocardial infarction” and “unstable angina pectoris” are often referred to by the more general term “acute coronary syndrome” or ACS, since these conditions both arise from rupture of an atherosclerotic plaque and thrombosis of a coronary artery. This is now defined in the Introduction section (page 4, lines 33 to 35) of the revised manuscript.
2. The authors should probably cite the Corser, et al., 2008 paper in BMC-HSR that compared the same thing you were examining for 500-some ACS patients.

This article is now cited in the Discussion section (page 9, lines 169 to 171) of the revised text.

3. I am unclear whether the self-report items used to collect self-report data offered any type of lay explanatory phrases associated with them. For instance, Katz, et al. (1996) very specifically provided self-report respondents with interpretive phrases re: more complex terms contained in the Charlson Comorbidity index.

Yes, the interviewer attempted to clarify items in the questionnaire by using lay terms. This is stated in the manuscript in the Methods section (page 5, lines 69 to 72): “To facilitate comprehension of the terms on the HHQ, non-medical terms were used to describe these conditions where appropriate; for instance patients were told angina was analogous to chest pain and arrhythmia to an irregular heartbeat.”

**Methods**

1. I am unclear on Page 5 whether the authors are only speaking about Troponin levels when they mention “elevated cardiac enzymes” as an inclusion criterion.

   This has been clarified in the revised manuscript in the Methods section (page 5, lines 52 to 53). Only cardiac troponin I was used as an inclusion criterion.

2. On Page 6, I am unclear whether the electronic medical records were audited using a particular chart audit “protocol” or “manual” of any sort. Please specify for readers who may be interested in replicating your methods.
We did not use a specific manual to audit electronic medical records. The protocol used is specified in the Methods section on page 6, lines 78 to 98 and on page 7, lines 99-105.

3. I would suggest that the authors define and discuss the “gold standard” decision that they attempted to avoid making earlier in area of Page 6. I don’t really see this term used until Page 11 of the current draft.

The term “gold standard” is now defined in the Discussion section (page 10, lines 198 to 200) of the revised manuscript.

4. Please define MMSE is on Page 7 of current draft for readers unfamiliar with what this is.

We are sorry that this did not seem clear in the original text. MMSE is defined when first used in the text on page 6, line 72 as the “mini-mental state examination.” The test is discussed on page 7, lines 73-75 and the original paper by Folstein, et al. (ref 16) is cited.

Results

1. The possible reasons for sources of discordant reports on Page 9 could be made clearer to readers. I personally got lost trying to interpret what factors the authors suggest might have been at play.

This section has been simplified and re-written on page 9, lines 151 to 157. We hope that it is clearer.
2. On Page 10, I have a problem with citing Reference 21 in this paper since these respondents responded primarily/entirely about symptoms. The reference 22 seems appropriate, and I suppose the authors could cite Corser, et al, 2008 again.

Thank you for this suggestion. We deleted the original Reference 21 and cited Corser et al., BMC-HSR 2008 as suggested.

3. Note that use of the term “myocardial infarction” on Page 10 needs to be defined or changed to ACS.

We addressed this issue as you recommended earlier. Thank you.

Conclusions

1. This section really needs to be expanded with some “drumbeat” and “take home messages” for your readers.

We thank the reviewer for this suggestion. We have revised the Conclusions section (page 12, lines 238-246) to make the following points or “take home messages,” namely that: (1) significant discrepancies were observed between the medical record and patient self-report for 13 health conditions of importance to the care of ACS patients; (2) discrepancies of the type documented here may be important to the care of any patient; (3) since important treatment decisions in ACS patients are made based in part on information from the initial clinical history, the findings reported here are of great potential significance for these patients in particular; and (4) there is an urgent need for research that identifies the most effective and efficient means to obtain accurate health information.
Editorial Notes:

1. Please split up some of these especially long paragraphs.

According to your recommendation, this has been done as much as is feasible.

2. The authors might consider removing some of the many abbreviations since some don’t really appear to be used very often.

Thank you for this suggestion. The abbreviations GEE, COPD, CVA, CVD, and CHF have all been removed except if they are used in tables.

REVIEWER 2

The odds ratio of agreement in individuals with at least some college education is reported to be significantly different, however, according to the Table 4 the P-value is 0.053

We thank the reviewer for this comment. As shown in Table 4, the unadjusted odds ratio of agreement between individuals with at least some college education and those without was 1.45 with 95% CI of 1.03 to 2.01 and a p value of 0.034. After adjusting for age, gender, race and mini-mental state examination scores, the odds ratio was 1.42 with 95% CI of 1.00 to 2.01 and a p value of 0.053. The reviewer is correct that only the unadjusted odds ratio is statistically significant, and that the adjusted OR is only marginally so. This is now stated explicitly in the Results section (page 9, lines 161 to 163) of the revised text.