Author's response to reviews

Title: Declining mortality following acute myocardial infarction in the Department of Veterans Affairs Health Care System

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Version: 2 Date: 20 August 2009

Author's response to reviews:

August 18, 2009

Dr. J Appleford
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Dear Dr. Appleford,

Once again, I would like to express my sincerest appreciate for your intervention regarding the manuscript referenced above. We have revised the abstract as you suggested. We are pleased that reviewers were generally so positive and are delighted to respond their specific suggestions as detailed below.
Reviewer Michel Burnier had no specific comments or suggestions related to revisions.

Reviewer James Rohrer made the following comments:

1. Linkage to the Dartmouth Atlas service areas is mentioned, as is area demographic characteristics. However, the analysis presented appears not to use that information. If it was not used, don't mention it in this paper. If it was used, then the tables should include findings related to those data. I hope that adjustment for 'hospital' characteristics does not refer to the community characteristics, since community characteristics reflect an entirely different dynamic than hospital characteristics.

As described in the manuscript, we did use the Dartmouth Atlas as in Dartmouth Atlas to map each VA and non-VA hospital to one of 306 distinct hospital referral regions for tertiary health services for the purpose of restricting comparisons to identical geographic areas. The data from this analysis are presented in Table 6. We are unsure to what adjustment for “hospital characteristics” the reviewer refers but presume he means adjustment for “hospital effect” as a random effect.

As described in the manuscript, we “included in our statistical models random intercepts for hospitals. Thus, the exponentiated value of the VA hospital indicator represents the odds of death in the ‘average’ VA hospital relative to the ‘average’ private sector hospital. We did not, however, perform any adjustments for community characteristics.

2. The Medicare group appears to have more CHF patients, yet I see no mention of adjustment for a diagnosis of CHF in the results tables.

As described, we applied the Elixhauser methodology which included all of the covariates listed in Table 4 and thus included heart failure and history of AMI.

3. The narrative mentions that the percent with previous heart attacks was known, but none of the tables mention adjustment for previous heart attack. Instead, they mention 'history of heart failure', which presumably means CHF. The results tables should list which variables were included in the model to adjust for patient differences so as to eliminate the concern that perhaps the data analyst was confused about the difference between history of MI and history of CHF. Presumably history of MI and current CHF should both have been included as risk adjustment variables.

As indicated in the response to #2, the variables listed in Table 4 were all candidates for entry into the model. The text has been revised to indicate this.

Once again, we are delighted to resubmit this paper and appreciate your kind assistance.

Warmest regards,
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