Author's response to reviews

Title: Racial Variations in Processes of Care for Patients with Community-Acquired Pneumonia

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Version: 3 Date: 30 Mar 2004

PDF covering letter
March 25, 2004

Re: Racial Variations in Processes of Care for Patients with Community-Acquired Pneumonia

Dear Editor:

Thank you for your review of our above-referenced article. We have revised the paper in response to the comments of the reviewer. Attached to this letter are our responses to the reviewer’s comments and a description of how and where the manuscript has been modified based on their remarks.

In closing, on behalf of the co-authors of this paper, I want to thank you for the review of our work. I believe that the article has improved based on the peer-review process. We hope that you deem the revised version suitable for publication in the BMC Health Services Research.

Sincerely yours,

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1. There is no accounting for clustering by hospital. Why could the authors not match on hospital, or at least hospital type, in addition to age and gender, when generating their match, for example?

Unfortunately we were not able to take clustering into account by hospital due to the absence of this data in the dataset that was provided. As part of the agreement under which this data was obtained the information for hospital was removed and the data de-identified so that we are unable to account for clustering by hospital. Although this is a limitation we do not feel that it significantly changes the validity of our findings that black patients were less likely to receive certain processes of care than white patients.

2. I think the sample is quite small, which is surprising and a pity, given that they were selecting patients from over 100 hospitals. There are several consequences of the small sample size. First, the authors conclude prompt antibiotics are given less often in blacks, with an odds ratio of 0.6. Yet, cultures are also less common by point estimate (0.7), but the confidence intervals overlap 1. Therefore, the authors state there is no difference. One could argue that 0.6 and 0.7 are both pretty low, and it would have been nice to have been adequately powered to state whether 0.7 was 'real' or not.

We agree that the small number of black patients is a significant limitation. However our study design and data analysis plan were designed so as to allow us to draw valid conclusions from our data set. By using a case matching methodology, and by performing multiple resamplings, we feel that we have strengthened our results.

3. The second problem of the small sample size relates to the regression modeling. The authors conducted quite sophisticated models, considering a large set of potential predictor variables. With such a limited sample size, one wonders how robust the models were.

Unfortunately we are unable to assess the robustness of our analysis using traditional techniques such as model cross validation on a new independent sample or by randomly subdividing our current sample into a training and test samples due to our small sample size. However we do have quasi-replication, at least in the white sample, by the multiple sampling that we performed.

4. What was the mortality rate? What was the compliance with each of the processes of care? Both overall and by racial group?

Information such as the overall and race-specific mortality rates, and compliance with processes of care, have been added to the paper as suggested by the reviewer. In the univariate analysis, mortality at 30-days was 7.8% for whites and 5.8% for blacks (p=0.3), and 82.1% of whites received antibiotics within 8 hours as compared to 75.7% of blacks (p=0.04). Regarding blood cultures performance, 96.4% of white and 97.1% of blacks had blood cultures obtained within 24 hours, and 84.8% of whites and 77.8% of blacks had blood cultures obtained prior to antibiotics (p=0.03). Oxygenation saturation was
assessed within 4 hours of 88.9% of whites and 93.9% of blacks (p=0.03).

5. **When stating that blacks received worse care but had better outcome, surely one would want to explore this paradox by asking if the process measure was indeed associated with outcome? If it wasn't, then one would question whether: a.) the process really does help, b.) the study was inadequately powered to find clinically important associations between process and outcome, or c.) there was some problem either with severity adjustment or with measurement of the care process.**

We agree with the reviewer that it is important to assess whether processes of care are associated with outcomes. Previous, much larger, studies as cited in the manuscript have described an association between pneumonia-related processes of care including time to initial antibiotic dose, obtaining blood cultures prior to antibiotics, and using antimicrobial therapy that is guideline-concordant. Our study did not have adequate sample size to be able to answer this question.

6. **Blacks had worse PSI scores, and had more PSI underlying disease. But there is no exploration of what caused the higher PSI scores. Was it just because of more underlying disease? Or, was acute physiology also worse?**

To assess what factors were significantly associated with either being black or white we created a multivariable logistic regression model with race as the dependent variables and the individual factors of the PSI as the independent variables. The only factors that were significantly associated with being in the black group were having a history of malignant neoplasm, having a respiratory rate > 30 breaths per minute, and having a Hematocrit < 30%.

7. **In particular, arterial oxygenation must be more closely examined. First, blacks were more likely to have arterial oxygenation ascertained. When oxygenation is not assessed, it is assumed to be normal. If there was a systematic bias in this measurement, then that assumption might not be equally true in both racial groups. Rather, instances of occult hypoxia would occur at a higher frequency in whites. If true, then whites were sicker than appreciated, possibly explaining the difference in the 30 day severity-adjusted mortality.**

It is true that a systematic bias in the measurement of arterial oxygenation might bias our findings however we find it unlikely that there is a factor that causes medical personnel to more often assess arterial oxygenation in black as compared to whites. We find it much more likely that this difference was due to medical personnel perceiving black patients as having higher severity of illness which is often associated with increased rates of assessment of arterial oxygenation.

8. **Another oxygenation problem is that pulse oximetry is much more common than arterial blood gas measurement - yet pulse oximetry calibrates differently with blood oxygenation depending on skin color. If this calibration was not accounted for, then the PSIs could be calculated with systematic measurement bias by race.**

We are unable to adjust for potential bias in pulse oximetry since this is a retrospective
study. However recent work, including the articles by Adler et al (Academic Emergency Medicine 1998:5;965-7) and Bothma et al (South African Medical Journal. 1996:86(5 Suppl):594-6) questions the idea that pulse oximetry does not perform as well in those with increase pigmentation as compared to those with lighter pigmentation. Therefore we feel that it is unlikely that this would systematically bias the results of our study.