Reviewer's report

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

Version: 1 Date: 19 January 2004

Reviewer: Derek Angus

Reviewer's report:

General
This study explores the important topic of variation in care and outcome by race. The authors focus on community-acquired pneumonia, which they argue is less open to differences in patient preferences, and therefore more indicative of delivery decisions. They also restrict their analysis to patients under a single insurer (Medicare), thus limiting potential confounding of racial variation by variation in insurance status. They find that blacks are less likely to receive some processes of care associated with better outcome, yet are paradoxically more likely to survive. The study is thoughtfully conducted, the authors acknowledge several potential limitations, and provide generally insightful interpretation. I do have a few concerns, however, as listed below.

Discretionary Revisions (which the author can choose to ignore)

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
Please provide actual raw rates of processes of care and outcomes in addition to odds ratios.
Please clarify where the 30day mortality information was obtained.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
There is no accounting for clustering by hospital. The authors argue that blacks are more likely to seek care at hospitals providing better quality of care. I think recent work by Skinner et al would counter that argument. And, blacks almost certainly seek care at a different mix of hospitals from whites in Pennsylvania, given the large geographic variation in habitus by race in that state. Some sort of clustering or nesting by hospital therefore seems essential. 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?

There is no discussion of sample size. 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.

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. There is no information provided on model performance. Furthermore, covariates with relatively large co-efficients may have been excluded simply because of inadequate sample or events.
As discussed above, there are no actual rates, only odds ratios. What was the mortality rate? What was the compliance with each of the processes of care? Both overall and by racial group?

The authors have information on severity of illness, process of care, and outcome. Yet, they never report whether process of care was associated with outcome after adjusting for severity of illness. 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.

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?

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.

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.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I conduct research in the field of critical illness, including severe community-acquired pneumonia. My research is funded by the NIH and by several commercial companies. I have also received consulting fees and speaker’s honoraria for work related to severe community-acquired pneumonia. However, I have not received any reimbursement for work specifically looking at variation in care processes by race.