Reviewer's report

Title: Comorbidity, age, race and stage at diagnosis in colorectal cancer: a retrospective, parallel analysis of two health systems

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Reviewer: Wayne Kendal

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Review of “Comorbidity, age race and stage at diagnosis in colorectal cancer: a retrospective, parallel analysis of two health systems”, by Zafar et al.

The authors present a retrospective comparison of two cohorts of individuals with colorectal cancer. The first cohort was drawn from individuals enrolled in the Cancer Care Outcomes Research and Surveillance Consortium, who were diagnosed and treated at 15 Veterans Administration (VA) hospitals. The second cohort was drawn from community and academic Fee For Service (FFS) practices in the Southeastern United States. This cohort consisted of an admixture of patients diagnosed with metastatic colorectal cancer (CRC) between January 2003 and June 2006 with other patients diagnosed with non-metastatic colorectal cancer diagnosed prior to 2003.

The hypothesis to be examined was “that patients with greater comorbidity, older age, and white race would be more likely to be diagnosed with early-stage CRC, due to more frequent contact with the healthcare system”. They thus examined for associations between stage at diagnosis and comorbidity, age and race using logistic regression analysis.

Since “in the VA cohort, higher comorbidity was associated with earlier stage at diagnosis” whereas “no such significant relationship was identified in the FFS cohort”, the authors concluded that “higher comorbidity may lead to earlier stage of CRC diagnosis, perhaps due to increased interactions with the healthcare system due to comorbidity”.

First, I would like to state that this is an interesting and plausible hypothesis. The relationship between comorbidity and cancer is poorly understood and warrants study, as within the present analysis. I find the measurements of comorbidity by the Charlson scale within the two different cohorts of interest, as well as the regression analyses provided. However, the authors have not provided convincing enough data to support their hypothesis. The two cohorts comprise two very different populations, and it is conceivable that some unmeasured confounder or bias may have lead to the results obtained rather than the proposed disparity in health care interactions. In order to solidify their arguments it would have been useful to provide measurements as to the degree and quality of access to healthcare for individuals from the two cohorts prior to their diagnosis of CRC. There may have been inequities in the staging tests
performed between the two cohorts that may have contributed to the different proportions of metastatic disease within the two cohorts at diagnosis. As well, since the FFS cohort was drawn from two subpopulations, one with metastatic disease at diagnosis and the other without, there may have been some bias introduced by the selection methods.

Granted, it is difficult to control for all potential confounders and biases that may potentially affect retrospective cohort analyses. However, the conclusion provided by the authors seems somewhat overstated, given the lack of further supportive data and the possibility that there may be other explanations for the observed associations. I would like to see this study published, but would suggest to the authors that they consider some modification to their conclusions.

Wayne S. Kendal

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.