Reviewer's report

**Title:** The effects of managed care on potentially preventable hospitalizations among adult Medicaid recipients

**Version:** 2  
**Date:** 27 February 2014

**Reviewer:** Michel Boudreaux

**Reviewer's report:**

The manuscript addresses an important and clearly defined set of questions about preventable hospitalizations among adult Medicaid beneficiaries in Florida. The authors compare the share of hospital discharges that are for ambulatory care sensitive conditions between the fee-for-service and managed care products in Florida Medicaid. They consider whether differences vary by race, county level demographic characteristics and county-level market factors. The paper addresses an important set of questions and comes to an unexpected finding, but I have major concerns.

**Major Compulsory Revisions:**

1. The sample is inadequately described (pg 6). Please describe the eligibility pathways for adult Medicaid beneficiaries in Florida. Are they parents, SSI beneficiaries, childless-adults? Describe the mix (particularly if it varies by ffs vs managed care).

2. The major limitation of this study is that participation in managed care versus FFS is (presumably) not random and the effects they estimate could be biased by omitted variables. The authors acknowledge this in the last sentence of the limitations section (p19) and on page 17, but more attention needs to be paid to this problem earlier in the paper. Please provide a fuller description of how a Medicaid enrollee ends up in managed care versus FFS. Consider if there is some study design that would help you control for selection into managed care. The study has value, as currently constructed, but if the authors wish to simply compare FFS to managed care they should be very careful that the language they use does not imply causality. For example, the use of “effect” in the title is not justified (the title should also indicate the focus on Florida). Please mention unobserved selection earlier in the manuscript (analysis section), and discuss in the limitations section what direction they think the bias goes. The discussion of selection on page 17 is useful, but it should be applied to the entire study, not just the spill-over effects. Given the study suggests that managed care is associated with an increased share of discharges that are ACSC (which is counter-intuitive), the manuscript needs to take on the unobserved selection issue head on and heavily qualify the findings as reflecting correlation and not causation.

3. Please do not conflate discharges for ACSC and access to primary care. While it is appropriate to theoretically link the two (this is done appropriately on page
15), they are not necessarily the same. Isn’t it possible that a patient accesses primary care, does not follow medical advice, and then needs to be admitted for an ACSC? Language like that in first sentence of last paragraph of page 16 should be refined. I am specifically concerned that the HMO plans could be doing their jobs in that they are holding down total hospitalizations, but there is certain type of patient that will get admitted regardless of their managed care/ffs status. This might show up as a larger share of ACSC in managed care (if the patient type is correlated with the ACSC definition) and there is nothing that generic “access to primary care” can do about it.

4. Please clarify text in the spill-over effects section (p16). The first sentence says there is not spill over. The fourth sentence says HMO penetration increases the odds of ACSC hospitalization for FFS. Where are you drawing your conclusion for no spillover effects?

5. Please include some measure of sampling error (e.g. confidence interval) in Figure 1. And describe if the predicted probabilities are obtained by setting covariates at their means or by averaging marginal effects. Also consider the limitations of estimating standard errors in non-linear models with interaction terms, i.e.: HSR 47(1pt1): 255-274. Consider clustering your standard errors at the county or hospital level.

6. Please provide more detail on why managed care should theoretically improve access to care in the context of Florida Medicaid (page 3). Do FFS enrollees pay a co-pay for primary care?

7. Please clarify your dependent variable. On page 7 you start by saying you have count data. The last sentence says you have a binary 0/1 variable. Please also be clear and consistent through-out the text that you are measuring the share of discharges that are ACSC, not the rate of discharges where the denominator is the at-risk population. The discussion on top of page 15 is helpful, but this needs to be brought up in the methods section.

8. Discuss your results in the context of Basu, 2007 (whom the authors cite) who found a negative correlation between managed care and ACSC discharges in Florida Medicare.

9. On page 3 you say 2/3rds of Medicaid enrollees are on managed care, but table 1 says 23% of discharges are managed care. Is this difference because the admission rate for managed care is so much lower?

10. Please report the unadjusted OR of ACSC for managed care vs FFS (in the text is fine) so the reader can guage how the covariates are impacting the results.

11. On page 6 please provide more information on missing values: how frequent was missing data and did you delete more ffs vs managed care?

12. On page 4 you suggest that difference by urban/rural are about the number of providers per capita. But you control for that and still find an effect by urban/rural. Please discuss if you have an alternative pathway in mind or if you think there is measurement error in your ARF provider count (you don’t observe Medicaid participating physicians?). On page 4, also provide more detail on why
market concentration should affect your outcome in the context of Medicaid in Florida.

13. For covariates like age and per capital providers consider creating categorical variables because it is hard to interpret magnitudes for a one-unit change for those types of variables.

Minor essential revisions: none at this time.

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 I have no competing interests.