Reviewer’s report

Title: Is Expanding Medicare Coverage Cost-Effective?

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Reviewer: Adam Atherly

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General

My main criticism of the manuscript is that the key variable is endogenous. Indeed, virtually every paper published in the past decade examining the link between supplemental insurance and Medicare expenditures has been an attempt to improve on previous attempts to deal with the endogenity issue. While the authors acknowledge that this is an issue for their work, they argue cannot be dealt with because:

- They lack adequate instruments
- The complex sample design of their data

And that it is not a fatal flaw because:

- The results are consistent with RAND
- The impact is mitigated because they are trying to estimate the effect on the entire Medicare population
- They are estimating a ratio

First, I don’t agree with their characterization of RAND in their letter, in particular that RAND varied to copayment from 95% to 0% with a maximum dollar expenditures equal to 5, 10 or 15 percent of total family income (up to $1000), which seems fairly dramatic. Also, RAND isn’t applicable to the elderly population, so appealing to RAND for support doesn’t seem compelling.

The idea that the impact of the endogenity is mitigated because the authors are trying to estimate the effect on the entire population also seems suspect. The predicted benefit depends on the estimated coefficients; if the betas are biased, then the predictions are biased, regardless of the sample over which the prediction is performed.

Finally, the ratio argument suggests that the ratio of two biased coefficients, if they are biased in the same direction, is less biased than the coefficients by themselves. This statement is generally correct, but the biased ratio can lead to incorrect conclusions even if the coefficients are biased in the same direction. It will be entire a question of magnitude; if the true ratio is 1 QALY per $40,000 in spending, and the bias doubles the numerator and quadruples the denominator, we get an estimate of 1 QALY per $80,000, below the usual acceptance threshold. Conversely, if the numerator is quadrupled and the denominator doubled, we get an estimate of 1 QALY per $20,000, easily within the acceptance region. So I still believe that, even with a ratio, we cannot be sure of the true benefits of the increased spending. However, if the direction of the bias is consistent, the ratio generally would help.

But what is the direction of bias? For expenditures, most argue that adverse selection into supplemental plans leads to a positive bias (unobserved variables correlated with higher
expenditures and supplemental insurance), although in my empirical work I’ve consistently found evidence of a negative bias, possible due to differences in risk aversion being correlated with preventive health behaviors. For the employer based supplements, there is almost certainly a negative bias (the same life habits that lead an individual to work in the same job for a (typically) large employer for many years are also associated lower health expenditures). So I don’t think one can be sure that the bias isn’t negative and driving the results.

As an example, I took the 1998 Medicare Current Beneficiary Survey and produced the following table of the percent of Medicare beneficiaries who currently smoke (which is included in the estimated model), have a usual source of care, and possess and examine at least monthly the CMS produced publication â€œMedicare and Youâ€”:

<table>
<thead>
<tr>
<th>Plan Type</th>
<th>Percent Currently Have a Usual</th>
<th>Has and looks</th>
</tr>
</thead>
<tbody>
<tr>
<td>HMO</td>
<td>12.58</td>
<td>96.31</td>
</tr>
<tr>
<td>Employer Supplement</td>
<td>9.74</td>
<td>94.19</td>
</tr>
<tr>
<td>Medigap Plan</td>
<td>10.01</td>
<td>94.98</td>
</tr>
<tr>
<td>Medicare Only</td>
<td>23.60</td>
<td>71.22</td>
</tr>
</tbody>
</table>

We see here a consistent pattern that the Medicare only group has the worst health habits, is the most disconnected from the health care system and has the worst information on Medicare. The smoking habits are controlled for and one could argue that the usual source of care is one of the dependent variables, but combined the three variables suggest that the reference group for this study is really quite different from those with private insurance, in ways measured and unmeasured. And all of these variables are associated with shorter life expectancy or poorer health outcomes for those without supplemental insurance but with different expected effects on expenditures.

Fundamentally, I am not convinced the results of this paper are casual. Although the authors demonstrate the association between expenditures and supplemental insurance and between life expectancy and supplemental insurance, I lack confidence that this is a casual association without stronger evidence. Put it another way: in Table 2, we see that those with supplemental insurance have 1.6 more office visits per year and use nearly three more prescription drugs annually. This leads to a nearly one year increase in life expectancy? Or Table 3, showing that increasing spending by around 10% leads to that same gain? Most studies find that the marginal effect of health care spending is near zero; this one suggests that it’s quite dramatic.

I’ve been a bit slow in this review because the paper is well written, interesting and does an excellent job of the CE analysis and Markov modeling and deals with the expenditure regressions competently. However, I remain unconvinced that reducing Medicare cost sharing would yield anything like the increase in life expectancy suggested by this article.

The authors have also done a reasonable job addressing most of my other comments.

What next?: Reject because scientifically unsound

Level of interest: An article of importance in its field
Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:
I declare that I have no competing interests