Author's response to reviews

Title: Is Expanding Medicare Coverage Cost-Effective?

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Version: 7 Date: 4 February 2005

Author's response to reviews: see over
Dear Editor,

Thank you for the opportunity to address the reviewers’ comments on our manuscript. We provide an overview of our responses to the comments of both reviewers below. We hope the manuscript continues to be of interest to your journal. In general, as requested by the reviewers, we have been more forthcoming in acknowledging the limitations of our analysis—the essentially irresolvable problem, in an observational study, of endogeneity. However, we think that, despite the limitations, the analysis provides a useful beginning framework to explore the relationships between the potential costs of expanding Medicare, and the potential health benefits. We hope that our methods are transparent enough to allow readers and policy makers to make useful inferences about the consequences of potential biases and missing variables, in turn, enabling better studies to be conducted in the future.

Sincerely,

Peter Franks
Peter Muennig
Marthe Gold
1. 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 endogeneity 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 the amount of co-payment very little" 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. We agree with the reviewer’s argument that in this study, as in all observational studies, the essential problem is adequately adjusting for endogeneity, or within an epidemiological framework, adjusting for confounding. We agree that RAND study is not entirely a relevant case, for several reasons, including that it applies to a younger age-
group (who don’t have Medicare), and an earlier time period (when there were less effective therapies available). Given that it is an RCT that shows mortality reductions for a subset of patients, it could be interpreted as evidence that improved health outcomes is not simply a matter of confounding. However, in view of the reviewer’s concerns and those of Dr. Virgo, we have dropped reference to this study. We have also employed stronger language to ensure that the reader understands that our findings are not definitive. (For example, see p. 13, 1st paragraph, last sentence, changes throughout the discussion.)

2. 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. Again, we agree—endogenous effects are not mitigated because we are looking at a population, and we misframed this issue. We do believe, though, (and there is some evidence to support this) that a voluntary, partial expansion of Medicare (or any insurance plan) would likely increase the risk of endogenous effects. This is because persons will select plans that they perceive to be optimal from their personal perspective, and because those persons are likely to have more information about their own morbidity than would be available to
insurers, their care is likely to be relatively expensive, with possibly
more marginal benefits in terms of health. In framing our analysis
from a population perspective, we anticipate that, to some extent,
this problem would be mitigated (though not eliminating the
endogeneity problem), as all people, regardless of health risks (or
asymmetric information between patient and insurer), would be
insured.

3. 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 entirely 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 "Medicare and You": Plan Type Percent Currently Have a Usual Has and looks Smoking Source of Care at "Medicare and You" Book at least monthly Medicare HMO 12.58 96.31 28.77 Employer Supplement 9.74 94.19 32.47 Medigap Plan 10.01 94.98 29.58 Medicare Only 23.60 71.22 18.26 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. **We agree that the use of a ratio doesn’t eliminate the bias—on balance, however, we think that the use of a ratio will more likely reduce the bias (since those without insurance have worse health habits, and likely would have worse outcomes even if insured, but they are also less likely to**
utilize health care even if they have insurance, so that their costs may not be as high as predicted based on some pent up previously unmet demand). We agree this is a matter of debate, and short of a randomized trial one is left with speculation, and interpretation of flawed/limited observational studies. As suggested by Professor Virgo, we now include discussion of how the use of a ratio does not eliminate bias (See bottom of p. 12).

Our purpose of including Table 2 (showing the differences in utilization) was not to provide a comprehensive itemization of how differences in utilization may explain the differences in outcomes, but simply to illustrate that the differences in expenditures are also associated with differences in the amount of care given. This is not intended to be a comprehensive list of differences, but merely illustrative of some of the less ambiguous differences. We also acknowledge that the marginal effect of spending on health has been found to be much smaller in most studies than we observed in our study. We think there are a couple of explanations for that. First, most studies date from an earlier period, when the availability of significantly effective therapies was quite limited. One simple example of this is the relatively recent availability of statins, which are expensive, and thus less available to those without supplemental insurance, but which have been found to have widespread and profound effects on mortality (including cardiovascular, and
cerebrovascular—two of the major killers in this age-group).

Second, most studies of the marginal effects of spending on health have looked at the entire age-range of population. Evidence from a variety of sources suggests that as the risks of morbidity increase with age, so the absolute benefits of effective interventions that reduce those risks also increase. In other words, the elderly benefit more from many given effective therapies than do younger persons. We have conducted a similar analysis on younger persons (comparing no insurance with any insurance) and found significantly smaller absolute benefits (though comparable hazard ratios).

We agree that these issues can be argued either way—and that robust evidence of causality is absent from our study. However, our study represents the first attempt to link the potential costs of expanding health insurance coverage to the potential health effects—and thus provides a beginning estimate of the value of such an intervention. We acknowledge and welcome the critiques—which we believe will lead over time to improved estimates, or ranges of estimates of the effects—as Dr Virgo suggests. However, we think our analysis provides a useful starting framework for beginning to explore this issue. We have modified our discussion to reflect our goals and these uncertainties more clearly and forcefully (See, especially, pp. 12-13).
Katherine Virgo

1. I have reviewed the manuscript, the authors' response to the reviewer's initial comments, and the reviewer's final round of comments. I agree with most of the reviewer's comments, though I do not agree that the paper should be rejected. The paper could still be published, as it is unlikely that any one single paper will adequately answer the question at hand. Each well-written paper on the topic contributes incrementally to the achievement of the final goal. The caveat is that the authors would have to be willing to expand the discussion section of the paper to include a more complete discussion of the limitations. If they are not willing, then I would also recommend rejection. In other words, from the authors' original letter responding to comment #1 (why the endogeneity issue was not dealt with more directly), the authors should compose a summary to add to the limitations section. Specifically, they should eliminate the reference to RAND and add a comment to the section regarding the ratio of health effects/costs explaining that a biased ratio can still lead to incorrect conclusions (the direction of the bias is generally assumed to be positive, but can be negative in the case of employer-based supplements). We fully agree with this approach, and have added relevant text to the limitations section of the discussion. Please see p. 12, final paragraph, and p. 13 top. We composed a summary, have eliminated the reference to the RAND study, and have added discussion relating to the use of a ratio.
2. The authors should also add to the limitations section a statement that, though their paper has shown an association between expenditures and supplemental insurance and between life expectancy and supplemental insurance, stronger evidence is needed to demonstrate a causal relationship. **We have done so. Please see p. 13, top.**

3. The authors should then proceed to tell us what the next steps in the research should be. What other variables should be examined that were not examined in the current manuscript? I agree with the reviewer that 1.6 additional office visits and 3 more prescription drugs are unlikely to increase life expectancy by almost 1 year. Further research is needed to ensure that some other factor(s) are not responsible. **We have added a discussion of possible next steps, primarily further studies using different datasets and different approaches. Specifically, we have discussed these issues within an econometric framework using propensity scores, and instrumental variables, if suitable instruments can be found.** Within an epidemiological framework, we have expanded the list of potential confounders. Other studies might explore some potential ‘natural experiments,’ such as comparing outcomes in those who have supplemental insurance ‘involuntarily’ as a result of prior employment with those with choice, and the health and cost implications of increases in copayments, or of losing access to supplemental insurance completely. (Please see p. 12, bottom.)
4. Minor comments: From page 7, move "Hazard ratio estimates were unstable...(see technical appendix)." to the results section. **We have done so. (Please see the first paragraph of the Results section.)**

5. Page 9: Explain why the 70 year olds were less cost effective than the 75 year olds since this is the only exception to the downward trend. **We have done so. (Please see last paragraph of p. 10.)**

6. Table 3: Add the word, "Observed" as a header over the private supplemental insurance and Medicare-only columns. **We have done so.**

7. Page 17, Figure 1: Should follow Table 5, not precede it. This figure could even be eliminated as there are already 5 tables in the paper. If deleted, summarize in an additional sentence on page 9 the range of the findings. **We moved Table 5 to before Figure 1.**