Author's response to reviews

Title: Is Expanding Medicare Coverage Cost-Effective?

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Author's response to reviews:

Dear BMC Editorial Team,

We have responded to reviewer comments to our submission to BMC Medicine. The reviewer comments were very insightful, but we were not able to implement some of them for the technical reasons discussed below. We hope that you will nonetheless find the paper to be important, compelling, and worthy of publication in BMC Health Services Research.

Policymakers critically need data on the incremental cost-effectiveness of supplemental insurance for Medicare recipients. Due to the high costs and ethical concerns of repeating the Rand Health Insurance Experiment (HIE), it is not likely that a randomized controlled trial will ever become available that outlines the costs and effectiveness of supplemental medical insurance. Nonetheless, given the plethora of medical advances made in the 20 years since the HIE, it is also likely that the extra care provided by private supplemental insurance is life saving. It is therefore necessary to rely upon large national health surveys linked to mortality data to estimate these effects. The paper should stimulate a good deal of interest and discussion within the health services research community, and should provide useful and critically needed data for policymakers.

Sincerely,

Peter Franks
Peter Muennig
Marthe Gold

Reviewer comments and responses from BMC Medicine reviews.

Major compulsory revisions

Comment 1. This paper addresses an interesting and important question: is there a health benefit from the extra services used by Medicare beneficiaries with supplementary coverage. But I am not convinced that the authors have demonstrated that there is, in fact, a gain in life expectancy from supplemental insurance. The paper, in my view, rests fundamentally on the regression relating QALYs and private supplemental insurance. If this regression is flawed, than the Markov models and all other analysis that flows from the regression models is also flawed. In the regression, the authors have demonstrated an association between private supplemental insurance and QALYs, but not a causation because private supplemental insurance is endogenous to the model, i.e., is correlated with the error term in the regression.

Endogeneity is almost a reflexive criticism by economists, and I recognize that it is an easy criticism to make and often an overblown concern. However, in this case, I believe that it is critical to the analysis attempted by the authors. The authors are comparing expenditures and QALYs in two groups, those with private supplemental insurance and those with Medicare only, which are very different on almost every measurable dimension. The private supplemental insurance group is healthier, wealthier, better educated, younger, has fewer chronic illnesses, is more likely to be married and non-minority than the Medicare only group. In short, the private supplemental insurance group is “healthier” on virtually every dimension one can measure. Indeed, the Landerman (1998) cited by the authors even found that the Medicare only group were more likely to be overweight, cook with fat and to live in inadequate housing or in an unsafe neighborhood than the private supplemental insurance group. So to attribute differences in QALY (or expenditures) to private
supplemental insurance, rather than unmeasured covariates, seems implausible.

This problem is exasperated by the relative oddity of the Medicare only group. This group is quite unusual; only around 10% of the Medicare population has Medicare coverage alone. The typical member of this group is not poor enough for Medicaid, not wealthy enough to buy a supplement and lives somewhere without access to a decent HMO. Extrapolating from this unusual subgroup is particularly problematic. The authors do address this problem in the limitations and in the Technical Appendix, but the solutions seem quite unsatisfactory. This problem is not necessarily fatal to the analysis, but does present a major analytical challenge. There are various approaches that are available, including a Heckman Selection Correction or a propensity model. Many studies of expenditures have addressed the endogeneity issue in some way, including Atherly (2002), Ettner (1997), McCall (1991), Taylor (1988), Wolfe and Goddeeris (1991) and Cartwright, et al. (1992). Given the large amount of work on this problem, and the multiple approaches to solving it in the published literature, the authors should be able to deal with the endogeneity issue more directly.

Responses.

1. We appreciate the reviewer's thoughtful comments--particularly those addressing the central problem in our analysis--that of endogeneity of insurance status in the models we used. The reviewer notes that our attempts to address this are unsatisfactory--and we agree with this comment, at least in a general sense. The extent to which unobserved variables account for observed relationships in any observational studies is unknowable. External validation with a randomized trial is rarely available (but see below). While techniques to address this problem have been developed--particularly in econometrics--the extent to which they would be helpful in our current analyses is uncertain. There are also a number of barriers to effectively implementing such methods with the current data.

The instrumental variable (IV) approach depends on finding a suitable instrument (a variable related to the key independent variable of interest, but not theoretically related to the outcome). The reviewer cites a number of studies, including his own work, that have used this approach. Some have suggested that employer (ESI) vs. individually chosen (Medigap) supplemental insurance may be a useful instrument--since ESI occurs as a result of one's job and does not involve choice. In Atherly (2002), the reviewer criticizes this logic, since factors determining employment choice may also affect risk aversion and health status (more health oriented people may seek jobs with lower health risks and better insurance, including ESI coverage). However, he goes on to use a geographic instrument as an alternative. In the example he gives, a secretary in a poor, rural area is less likely to have supplemental insurance than an identical secretary in a suburban area were government is the main employer. But are these secretaries identical? Just as one may choose a job depending on its health insurance offerings, one may also choose to work and live in an area that reflects one’s underlying (i.e. unmeasured) values, including many that bear on health orientation. Thus the extent to which this instrument solves the problem, rather than moving the bias from one place to another, is unknown.

It should also be noted that the relative impact of adverse selection or favorable selection depends on separating out the relative effects of ESI and Medigap (as noted by Atherly 2002 and others)--but these details are not available in the NHIS dataset we used. We used NHIS as it is the most recently publicly available data that allows calculations of QALYs. Does this limitation mean the findings are of no interest? Obviously we think that despite these limitations an initial exploration of the value of expanding health insurance for the elderly is useful (see also below)

There are, besides, a number of logistical issues that make implementation of an IV approach more difficult and questionable. The national survey data we used do not allow implementation of geographic type instruments since the required information is not provided due to confidentiality concerns. Another critical problem is the complex structure of the surveys themselves--using complexly clustered and weighted data. Unless appropriate software is used the parameter estimates will be biased, and the standard errors will typically be too conservative. While STATA has limited capacity for simultaneously implementing IV regression and addressing survey design issues, SUDAAN (which is needed for the survival analyses) does not. Thus one is faced with a tradeoff between possible bias due to not using an IV approach vs. bias due to ignoring the survey design.

Summarizing, while we acknowledge that endogeneity is a potential problem, we are unclear that the proposed solutions clearly help, or can be effectively implemented in this case. One is left with alternative thought experiments to examine the problem of how much bias is likely to be present, and, more importantly, how will the likely bias affect the Incremental Cost-Effectiveness Ratios (ICER). First, the RAND Health Insurance Experiment (HIE). We briefly discuss the relevance of the HIE. Essentially, the RAND HIE found evidence that reduced copayments produced some small health benefits, associated with increased utilization. Given that the experiment varied the amount of co-payment very little, and the experiment was conducted before many of the important recent gains in the effectiveness of medicine, we consider that our findings are quite consistent.
Second, much of the debate on endogeneity centers on the relative importance of moral hazard, and adverse or favorable selection. Our analysis essentially considers the impact expanding Medicare to make it a universal program. Under such a scenario, people would not have the option of opting in (if they thought it good value) or out (if they thought it bad value). Such a strategy is likely to reduce the net consequences of these endogeneity issues.

Third, we think the consequences of moral hazard/adverse/favorable selection are likely to have less impact on the ratio of health effects/costs (the ICER) than they would on the numerator or denominator alone. This is because any of these factors is likely to have effects on both health and expenditures. This assumption depends on the net effectiveness of medical care--that is people who use more care (up to a point of optimal utilization, which is not yet exceeded by those who already have expanded coverage--which we use as the reference group) generate higher health care costs, but have better health care outcomes. We cite, as an example, published work on medical skepticism. After adjustment, people who are more skeptical are less likely to have insurance, and also (allowing for the insurance effect and other potential confounders) have lower utilization. They are also less healthy (after adjustment) and more likely to die. In other words, they have less benefits (numerator) and less costs (denominator).

We have expanded on our discussion about why we think our approach is a useful beginning to addressing the question of whether expanding Medicare for the elderly cost-effective, with more focused exposition of some of these endogeneity issues. We have also expanded our technical report to explain in more detail why we did not adopt the approach suggested by the reviewer.

Given that the reader accepts the underlying assumption that Medicare is good for the health rather than bad or neutral, the question essentially boils down to one of magnitude. Specifically, it is a question of whether the bounds of demographic differences that we might not have adequately controlled for exceed those of the sensitivity analysis. Given a willingness to pay threshold of $50,000 - $60,000/QALY (the upper range within which most necessary medical interventions fall) though, the sensitivity analysis was quite robust.

2. The authors need to include more detail on the sample in the body of the paper. Although everything necessary is included in the technical appendix, there needs to be more detail in the paper itself. In particular, (and at a minimum) the sample size and exclusion criteria need to be included.

We have added the details: sample size, numbers dying, and exclusion criteria, to the paper

Minor Revisions

1. The discussion of the viewpoint taken by the study should be expanded. This is a particularly important point in this paper because most Medicare beneficiaries already have additional coverage. So expanding Medicare benefits would primarily serve to substitute private financing for public financing. From a societal perspective, this is irrelevant. However, from a government perspective, this would likely be a very inefficient use of resources, if one accepts the estimates in the paper. This point should be highlighted, along with an estimate of the welfare loss associated with the increase in taxation necessary to finance such an expansion in Medicare.

There are formidable challenges in calculating payments from a governmental perspective. Unanswered questions include: What would the change in efficiency be? How would overhead costs change? Should profits be excluded? If so how? What about implicit productivity valuations in HRQL scores (many people in the US do continue to work after 65)?


We have corrected this statement as suggested by the reviewer.

3. The text suggests on Page -3- that "some are likely to forgo care due to this expense". This should be clarified. Do you mean that they will forgo buying insurance or that they will decide not to buy insurance and therefore implicitly reduce the use of services?
We have clarified this, thanks.

4. Page -6- states that employment status is a variable in the model. Individuals who are employed and have health insurance through their employer have Medicare coverage as secondary (supplemental coverage). Generally such persons are dropped from the analysis because their relationship with Medicare is so different.

This mis-statement is left over from a parallel analysis that was done on persons under the age of 65. It is an error and does not apply to those > 65. It has been corrected.