Exploring the Effects of Secondary Coverage on Medicare Spending for the Elderly

A report by Direct Research, LLC, for the Medicare Payment Advisory Commission

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EXECUTIVE SUMMARY

This research examines the effects of Medicare beneficiaries' secondary insurance on their use of Part A and Part B services. It is an update and extension of a June 2009 MedPAC Contractor's Report (Hogan 2009).

We adopt the same methods as the earlier report, pooling multiple years of Medicare Current Beneficiary Survey (MCBS) data on elderly community-resident beneficiaries. We then compare service use and spending of fee-for-service beneficiaries with and without private secondary coverage, adjusting for sociodemographics and health status. The main extensions of the prior report are in refinements and tests of the methods, and in looking at mortality and changes in health status.

Part I: Update of Prior Analysis of Coverage and Spending

The first section replicates the three key findings of the 2009 report. Results using newer information (2006 to 2008 data) parallel those in the original report (2002 to 2005 data).

- Secondary coverage is associated with significantly higher service use (after adjustment for sociodemographic factors and health status).
- This is primarily due to beneficiaries with first-dollar or near-first-dollar coverage (defined here as paying under 5% of Part B costs in excess of $1000, for individuals with at least $1000 in Part B spending).
- This varies by type and place of service. Secondary coverage has little effect on emergency care (urgent or emergent hospital admissions, emergency visits, ambulance services). By contrast, secondary coverage was associated with much higher use of preventive care, elective hospital admissions, medical specialists, endoscopies, and (new in this analysis) joint replacements.

Several modest variations in methods had no material effect on results. These include the elimination of individuals with possibly questionable survey data (decedents, MCBS "ghosts", persons with proxy interviews); inclusion of measures of possibly confounding factors (drug coverage, health behaviors including smoking and obesity); and alternative approaches for identifying those with near-first-dollar coverage.

The only tested variation in methods that mattered materially was choice of risk adjuster. As noted in the 2009 report, a standard CMS risk adjuster (based on diagnoses from claims data) misinterprets lower service use of the Medicare-only population as low risk (good health). Substituting self-reported disease prevalence for the claims-based risk adjuster increased the estimated impact of secondary coverage on spending.

Finally, we drilled down by type of service, comparing three populations: First-dollar coverage, less-than-first-dollar coverage, and no coverage (Medicare only). The latter two populations had similar, low, aggregate spending, but a different mix of services. Compared to the Medicare-only group, the less-than-first-dollar group did not stint on

screening and preventive services, office visits, and certain types of specialist visits, such as eye exams.

Part II: Analysis of Changes in Health Status and Changes in Coverage

The second part of this research addresses impact on health status. When coinsurance reduces service use, does that result in more rapid deterioration of health status? If so, for whom, and by how much?

The 2009 report was a study of spending, and did not address impacts on health status. We noted a substantially higher annual mortality rate in the Medicare-only (no-secondary-coverage) population, ruled out some purely technical explanations, and suggested that a larger sample was needed to investigate the issue.

Here, we find that the Medicare-only population has a higher one-year risk of mortality, institutionalization, and (for the non-institutionalized) movement to Medicaid coverage. We do not, however, find statistically significant differences in deterioration of self-reported health status or limits on activities of daily living (ADLs).

By contrast, for beneficiaries with private secondary insurance, we find no difference in outcomes between those with near-first-dollar coverage and others. Lower spending appears associated with higher health risk for the Medicare-only population, but not for the population with partial (not-first-dollar) secondary coverage.

We hypothesized two plausible explanations for this, based on potentially observable factors. (A third choice – that some unobserved characteristic of the Medicare-only population results in earlier death – is possible, but not useful from a research perspective).

One hypothesis is dynamic, relating to changes in coverage: Perhaps some individuals pass through Medicare-only coverage as part of a pre-existing downward trajectory. If so, that would be interpreted in the cross-section as showing that Medicare-only status causes more rapid deterioration of health.

One piece of evidence suggesting this is the rapid turnover of the Medicare-only population. Looking back one year, 13 percent of the Medicare-only population had some secondary coverage in the prior year. (By contrast, 2 percent of those with private coverage had anything else in the prior year.) Looking back ten years (via the Health and Retirement Study), *over 70 percent had some secondary coverage in the tenth year prior*. More than half had private secondary coverage. Loss of private coverage was moderately correlated with low 2010 income. This shows a population in flux, and suggests that some individuals may arrive at Medicare-only status through some type of adverse life event.

A second hypothesis takes the use and outcomes findings at face value, and assumes that poor outcomes are the consequence of inappropriately low service use in the Medicare-

only population. If true, this broadly suggests a "sweet spot" in Medicare benefits design. Under this interpretation, the Medicare-only population has low cost and low quality, the first-dollar-coverage population has high cost and high quality, and the population with some (but not first-dollar) secondary coverage has both low cost and high quality. Coincidentally or not, despite low spending, that population did not stint on screening and preventive services or office visits. If validated by further research, this suggests that modest coinsurance or copayment may be a reasonable model for Medicare benefit design.

1 INTRODUCTION AND BACKGROUND

1.1 Background of this report

Most Medicare beneficiaries have some form of secondary insurance that pays part or all of the deductible and coinsurance liabilities incurred on Medicare-covered services. Excluding Medicare Advantage beneficiaries, about 90 percent of non-institutionalized Medicare fee-for-service population had some form of secondary coverage in 2007 (calculated from MedPAC 2011a).

Research and practice have long suggested that deductibles and coinsurance under Medicare Parts A and B reduce the use of services. This was the conclusion of separate analyses by the Physician Payment Review Commission (PPRC) and Congressional Budget Office (CBO) staffs (PPRC 1997; Christensen and Shinogle, 1997). Using different data sources and adjusting for health status and sociodemographic factors, these studies found substantially higher use of services for beneficiaries with secondary coverage. Both analyses suggested that most of the difference in spending was for Part B services, not for hospitalizations.

In 2008, MedPAC commissioned a re-analysis of this issue (Hogan 2009). A (then) recently published work attributed much of the estimated secondary insurance effect to the improper analysis of individuals with Veterans' Administration (VA) coverage (Lemieux and Chovan, 2008). The MedPAC-sponsored analysis studied the VA issue in depth, and continued to find large effects on spending after excluding VA-covered enrollees.

The MedPAC-sponsored research uncovered two new pieces of information. The study showed that:

- Essentially the entire secondary insurance effect occurred for individuals with near-first-dollar coverage ("free care"). Those paying 5 percent or more of total Part B costs showed no increase in spending relative to the population without secondary coverage.
- The impact of secondary coverage varied widely by type of service. There was no effect on emergency care, but large impacts on preventive care, scheduled hospital admissions, medical specialists, and minor procedures.

In early 2012, MedPAC commissioned this follow-up study with two goals in mind. First, this study should validate and update the prior findings on coverage and spending. Do the prior findings change materially when more recent data or modestly different analytical methods are used?

Second, this study should look for any effects on health status. Does the population without secondary coverage show signs of more rapid deterioration in health? We know they use substantially fewer services, including services that physicians judge to be

necessary care (MedPAC 2003). But does this result in a measurably more rapid deterioration of health?

1.2 Outline of the analysis

Section 2 of the paper replicates the three main findings of the original report. Except where noted, the methods used are identical to the preceding 2009 study. Accordingly, the methods are presented in abbreviated form, and the reader is referred back to the 2009 report if more detail is required.

Section 3 of the paper looks at the issue of the deterioration of health status. Realistically, with the MCBS, we can only look at an individual for two years at a time, using the overlaps across the annual MCBS panels. Accordingly, we supplement the MCBS analysis with analysis from the Health and Retirement Survey, which now has a nearly-20-year follow-up of one cohort of individuals as they neared and entered retirement.

Section 4 of the paper provides a high-level summary of the results. Rather than repeat the detailed findings of the executive summary, it is more of a retrospective summary of what appears to have been learned with this analysis.

2 UPDATE OF PRIOR ANALYSIS OF COVERAGE AND SPENDING

In this section we replicate the three key results of the prior report (Hogan 2009). These include the following:

- Regression-adjusted estimate of secondary insurance effect (prior report table 4).
- First-dollar effect (prior report tables 7 and 8).
- Type of service breakouts (Tables 9 and 10).

The previous report summarized the important demographic differences between the Medicare-only population and those with secondary coverage. They tend to be poorer, less educated, less likely to be married, more likely to be working aged. That background information will not be repeated here.

We begin with a condensed summary of methods. Readers should refer to the 2009 report if more detail on methods is required. We then address the three main findings in turn.

2.1 Methods for this analysis

In broad outline, we compare beneficiaries who do and do not have secondary insurance coverage, using regression analysis to account for confounding factors such as health status, income, education, and disability. The only change in methods for this analysis is the addition of three more years of MCBS Cost and Use files (2003 to 2008). The additional data will allow us to see how volatile the findings are as we move across separate MCBS samples, and will let us calculate one set of more stable (and precise) estimates pooling all the available data.

2.1.1 Population

This analysis is restricted to Medicare beneficiaries on the MCBS who meet the following criteria.

- Elderly (age 65 or older).
- Not institutionalized.
- Enrolled in both Part A and Part B for every month enrolled.
- Not enrolled in Medicare Advantage at any time during the year.
- Answering the MCBS questions on secondary insurance status.
- Without evidence of VA-paid services during the year.

We also exclude Medicaid from most analysis because Medicaid coverage may occur through spend-down. In general, we pool Medicaid beneficiaries in the analyses, separately identify them by insurance status, but do not report the Medicaid results.

2.1.3 Secondary insurance status

We adopted MedPAC's approach to defining secondary insurance categories. This assigns a person to whichever secondary coverage state accounted for the largest fraction of the months observed for that person. For example, the "Medicare-only" (no secondary coverage) category will include beneficiaries who had no listed secondary coverage for the plurality of months of Medicare enrollment. In every analysis, the Medicare-only population is the smallest population studied, and sets the limit on the statistical precision of the analysis.

2.1.4 Measures of spending

In the 2009 initial analysis, we used both Medicare's claims-reported spending, and an enhanced measure on the MCBS that included events reported by beneficiaries, but not shown in claims data. This avoided criticism regarding services used with other payers (e.g., VA) potentially substituting for Medicare. Results from the prior study did not differ materially based on source of spending data. For this analysis, we rely on spending data as reported by Medicare. We use beneficiary-reported spending only for the out-of-pocket cost measures.

2.1.5 Health status and other covariates

Health status. We use several survey-reported measures of health status. These are more-or-less standard among MCBS users and are not controversial. These include self-reported health status (from excellent to poor) and counts of limitations of activities of daily living (ADLs). We also include a flag for working aged (as CMS does in its Medicare Advantage risk adjustment), and in some cases, a flag to identify decedents (due to their much higher costs in the year of death, all other things held equal).

We also use a claims-based health status measure based on CMS's Hierarchical Condition Category (HCC) model. Two factors make the use of an HCC model a more controversial choice.

First, reliance on diagnoses from claims means that the risk measure is censored for the Medicare-only population. No claims means no diagnosis information, and reduced claims volume may mean reduced information. And, in fact, the Medicare-only population appears much "healthier" than others on this claims-based measure *only*. The large discrepancy between the claims-based HCC measure and other measures of health status suggests that the bias is material.

The decision to include or exclude the HCCs as risk adjusters is a judgment call. The greater accuracy of claims-based adjusters is desirable, but the bias should lead to a conservative (small) estimate of the impact of insurance on use. Regression analysis will likely attribute some portion of the lower spending to "better health", as measured from

the HCCs based on claims. For this analysis, we will discuss one set of results without the HCC measures, to demonstrate a range of estimates.

Second, there is a minor technical issue in the use of the stock CMS HCC model. The HCC model should be run "prospectively", using current-year diagnoses to predict next-year costs. But the MCBS forces us to use it "concurrently" – we risk adjust current-year spending with current-year diagnoses. This means that the HCCs will capture a very large portion of spending variation, but that the actual HCC categories will not have been optimized for use in a concurrent fashion.

Other factors: We also include sociodemographic factors. These are generally uncontroversial, and include, age, sex, race, urban/rural location, marital status, educational attainment, and family income (calculated per person for married couples). In the 2009 analysis, we found that inclusion of dummies for the state of residence (fixed effects for states) had no material effect on the results, and that is not repeated here.

2.2 Regression-adjusted estimate of secondary insurance effect

Background: The 2009 report showed that beneficiaries with secondary coverage had substantially higher Medicare spending than those without such coverage. However, those with and without secondary coverage also had different levels of health status, income, education, and other factors. Spending remained higher, for the populations with secondary coverage, even after adjusting for those factors via regression analysis.

Results: Table 1 shows that Medicare continues to spend substantially more per person for beneficiaries who have secondary coverage. The columns at the right show spending relative to the population with no secondary coverage ("Medicare Only"). Comparing the more recent data (2006-2008) to the earlier findings (2003-2005), the qualitative differences remain the same (highest for Part B, highest for Medigap), while the magnitudes are somewhat smaller.

None of the changes in the ratios shown in Table 1 is statistically significant. This does not mean that there was no trend, merely that any underlying trend was indistinguishable from random variation, given the precision of the estimates calculated from the MCBS.

Indirectly, Table 1 illustrated the value and limitations of pooling additional years of MCBS data. Three additional years of data should result in generally more stable (lower variance) estimates. Findings arising by chance in the prior analysis may well disappear in the current analysis. However, subtle trends will remain undetectable, given the variation in the data and the statistical power available from the MCBS sample. Here, the drop from (e.g.) 59 percent to 50 percent higher spending does not even approach statistical significance. Given the limits on the statistical precision of the MCBS estimates, the best we can say is that the more recent era looks roughly the same as the original 2003-2005 data.

Table 1: Unadjusted Differences in Medicare Outlays by Beneficiaries' Secondary Coverage							
Secondary Coverage	**Persons in Sample**	**Medicare Program Payments**			**Ratio to "Medicare Only" (No Secondary Coverage)**		
		Total	**Part A**	**Part B**	**Total**	**Part A**	**Part B**
2003-2005							
1:Medicare Only	1,493	$ 4,015	$ 2,335	$ 1,680	1.00	1.00	1.00
2:Employer Sponsored	8,519	$ 5,935	$ 2,976	$ 2,959	1.48	1.27	1.76
3:Employer + Individual	1,374	$ 5,515	$ 2,495	$ 3,019	1.37	1.07	1.80
4:Individual purchase	6,475	$ 6,368	$ 3,149	$ 3,220	1.59	1.35	1.92
2006-2008							
1:Medicare Only	1,192	$ 5,126	$ 2,846	$ 2,280	1.00	1.00	1.00
2:Employer Sponsored	8,058	$ 6,774	$ 3,190	$ 3,585	1.32	1.12	1.57
3:Employer + Individual	1,426	$ 6,765	$ 3,044	$ 3,722	1.32	1.07	1.63
4:Individual purchase	5,189	$ 7,692	$ 3,823	$ 3,869	1.50	1.34	1.70
All years pooled							
1:Medicare Only	2,685	$ 4,535	$ 2,574	$ 1,961	1.00	1.00	1.00
2:Employer Sponsored	16,577	$ 6,354	$ 3,083	$ 3,271	1.40	1.20	1.67
3:Employer + Individual	2,800	$ 6,155	$ 2,776	$ 3,379	1.36	1.08	1.72
4:Individual purchase	11,664	$ 6,977	$ 3,459	$ 3,519	1.54	1.34	1.79
Source: Analysis of MCBS Cost and Use files, 2003-2008							

Table 2 shows the regression-adjusted estimate of the effect of secondary insurance on Medicare spending. Behind this analysis is an ordinary least squares regression predicting spending based on secondary insurance and a large number of covariates capturing sociodemographic and heath status information, including:

- Secondary insurance coverage.
- Age, race, sex, marital status
- Income and education
- Self-reported health status and total activities of daily living (ADL) limitations.
- Whether currently working or has a military-service-related disability.
- Decedent during the current year.
- Presence of roughly 70 diseases (HCC categories).

Paralleling Table 1, the regression-adjusted results from the most recent period (2005-2008) are similar to the earlier period, but smaller in magnitude. As before, secondary insurance has no statistically significant effect on Part A spending, but a large and statistically significant effect on Part B. And, as with Table 1, changes in the coefficients between the earlier and later periods are not statistically significantly different from zero. For example, for total spending, the estimated effect of individual purchase insurance dropped from 33 percent to 23 percent. While this *looks* large, the corresponding t-value is 1.1. For Part B spending, the drop from 54 percent to 39 percent has a t-value of 1.6. We do not have enough statistical power to be able to distinguish any true downward

trend from chance variation. As with Table 1, the best we can say is that the later period (2006-2008) appears qualitatively similar to the earlier period (2003-2005).

Table 2: Regression-Adjusted Increase in Medicare Spending Associated With Secondary Insurance, Total and Parts A and B						
Elderly Medicare Fee-for-Service Beneficiaries With No VA Use, 2003-2008 Pooled						
Update of 2009 report, Table 5.						
	Total		**Part A**		**Part B**	
	Spending	P-value	Spending	P-value	Spending	P-value
2003-2005 Pooled (1503 Medicare-only observations)						
Memo: Spending, Medicare Only	$4,015		$2,335		$1,680	
Percent increase associated with:						
Employer sponsored	17%	*	9%		30%	***
Employer + Individual	25%	*	9%		48%	***
Individual Purchase	33%	***	18%		54%	***
2005-2008 pooled (1182 Medicare-only observations)						
Memo: Spending, Medicare Only	$ 5,126		$ 2,846		$ 2,280	
Percent increase associated with:						
Employer sponsored	12%		-2%		28%	***
Employer + Individual	19%	*	6%		36%	***
Individual Purchase	23%	***	9%		39%	***
2003-2008 pooled (2685 Medicare-only observations)						
Memo: Spending, Medicare Only	$ 4,535		$ 2,574		$ 1,961	
Percent increase associated with:						
Employer sponsored	14%	**	4%		28%	***
Employer + Individual	22%	***	9%		40%	***
Individual Purchase	27%	***	13%		45%	***
Source: Analysis of 2003-2008 MCBS Cost and Use files. Notes: * = p < 0.05, ** = p < 0.01, *** = p < 0.001						

Table 3 shows the same type of regression-adjusted figures for seven different claim types as summarized on the MCBS. The most consistent effect of secondary coverage is on the use of physicians' services, where spending is elevated for all three types of secondary coverage.[1]

Table 3: Regression-Adjusted Increase in Medicare Spending Associated With Secondary Insurance, by Type of Claim			
Elderly Medicare Fee-for-Service Beneficiaries With No VA Use, 2003-2008 Pooled			
	Mean for Medicare Only Population	% difference associated with secondary insurance	P-value
Inpatient reimbursement	$ 2,111		
Employer sponsored		4%	
Employer + Individual		9%	
Individual Purchase		15%	
Skilled Nursing Facility	$ 152		
Employer sponsored		28%	
Employer + Individual		55%	*
Individual Purchase		60%	***
Hospital outpatient	$ 483		
Employer sponsored		21%	*
Employer + Individual		17%	
Individual Purchase		39%	***
Physician/supplier	$ 1,118		
Employer sponsored		41%	***
Employer + Individual		65%	***
Individual Purchase		60%	***
Durable Medical Equipment	$ 170		
Employer sponsored		11%	
Employer + Individual		31%	
Individual Purchase		38%	***
Hospice	$ 181		
Employer sponsored		-22%	
Employer + Individual		-21%	
Individual Purchase		-35%	
Home Health	$ 321		
Employer sponsored		-6%	
Employer + Individual		-25%	
Individual Purchase		-14%	
Source: Analysis of 2003-2008 MCBS Cost and Use files.			
Notes: * = p < 0.05, ** = p < 0.01, *** = p < 0.001			

[1] Possibly worth noting, differences are consistently negative for home health and hospice. These services have no beneficiary cost-sharing requirements. (Note that decedent status is included as a regressor, so the regression should adjust for differences in mortality rates and their effect on hospice use.)

Variations in methods. As discussed in the 2009 report, the inclusion of the HCC-based risk adjusters is questionable. The use of disease prevalence information gathered from diagnoses on claims is a generally accepted method of risk adjustment. Here, however, the low service use of the Medicare-only population (particularly the large fraction with no claims during the year) appears to bias the HCC-based risk adjuster downward. Table 4 contrasts the HCC community model risk score to self-reported health status measures. Based on self-reported data, the other populations appear somewhat healthier than the Medicare-only population (Table 4, bottom line, positive values). But based on the HCC model (only), those populations appear substantially less healthy. The censoring from lack of claims pushes the HCC score down for the Medicare-only population, making it appear healthier.

Table 4: Comparison of Several Risk Adjusters, 2003-2008 Data Pooled					
Secondary Coverage	Cases	HCC community model score	% with self-reported health excellent or very good	% with self reported health fair or poor	Mean number of ADL limitations
Risk Scores					
1:Medicare Only	2,685	0.90	48%	22%	0.534
2:Employer Sponsored	16,577	1.08	48%	18%	0.503
3:Employer + Individual	2,800	1.01	58%	12%	0.401
4:Individual purchase	11,664	1.09	48%	19%	0.563
Percent "Healthier" than Medicare Only					
1:Medicare Only		-	-	-	-
2:Employer Sponsored		-19%	1%	16%	6%
3:Employer + Individual		-12%	21%	44%	33%
4:Individual purchase		-21%	1%	14%	-5%
Simple Average		**-17%**	**8%**	**25%**	**11%**
Source: Analysis of MCBS Cost and Use Files, 2003-2008 pooled.					

Table 4B compares the claims-based prevalence measure to the corresponding self-reported data from the MCBS. For some (but not all) diseases captured by HCCs, we can match some collection of HCC categories to an MCBS question on the same disease. Note, however, that the MCSB question is typically lifetime prevalence ("ever had"), while the HCCs capture one-year prevalence (essentially, treatment). We do not expect to see the same levels of prevalence for the HCC and survey-reported data.

The first interesting finding from Table 4B is the right-hand column. That column shows the extent to which claims and survey data differ in characterizing the relative prevalence of disease. Positive values in that column show that claims data exaggerate the good health of the Medicare-only population, while negative values show the reverse. Claims data appear to exaggerate differences in disease prevalence across these populations, for

many common diseases. On average, the HCCs appear to provide a biased measure of prevalence in this case. Compared to self-reported data, the HCCs show a lower burden of disease in the Medicare-only population, compare to the population with secondary coverage. Plausibly, that is an artifact of having fewer claims from the Medicare-only population.

The second interesting finding is on the bottom line of Table 4B: Both claims and self-reported data show *lower* disease prevalence in the Medicare-only population. This runs contrary to the other health status indicators. Based on self-reported health and functional status, the Medicare-only population appears to have below-average health. But based on the burden of disease, as measured by the count of conditions present) is lower, the reverse is true.

A third interesting finding is in the sort order of Table 4B, from smallest to largest discrepancy between the self-reports and the claims data. Because the HCCs are typically triggered by physician claims for treatment of the condition, the sorted table is a guide to the types of conditions for which the Medicare-only population is less likely to receive treatment in a year (relative to the population with secondary coverage). Unsurprisingly, heart attack, stroke, hip fracture are near the top of the table, with minimal discrepancies between the self-reported and claims-based data. These are conditions requiring immediate, emergency attention. Rheumatoid arthritis, coronary artery disease (excluding heart attack), and cancer appear at the bottom.[2] These are conditions for which lack of follow-up care may have no immediate consequences, or where effective treatments involve relatively expensive therapies. These are the conditions for which the Medicare-only population is less likely to receive any treatment during the year, relative to the survey-based prevalence.

[2] To understand this interpretation, the reader needs to know that follow-up checkups for cancer, or long post-cancer treatments (such as hormone-suppression therapy), may appear on claims with a cancer diagnosis. Thus the claims-based cancer prevalence in the HCCs is a mix of acute treatment and long-term followup. For some cancers (e.g., prostate), in any year, the follow-up cases may outnumber the incident (acute-care) cases.

Table 4B: Comparison of Disease Prevalence, Survey-Reported ("Ever Had") Versus HCC (Diagnosis Appears on Claims)							
	Self-Report (Survey)			Claims Data (HCCs)			Self-Report Diff. Less Claims Data Diff.**
	Medicare only	Some Secondary	% Diff*	Medicare only	Some Secondary	% Diff*	
Hip fracture/dislocation this year	0.005	0.007	-43%	0.008	0.011	-39%	-4%
COPD	0.128	0.147	-15%	0.108	0.121	-11%	-3%
Stroke	0.103	0.108	-5%	0.033	0.034	-3%	-2%
Cardiac rhythm disorders	0.148	0.217	-47%	0.084	0.124	-47%	0%
Heart attack this year	0.018	0.020	-8%	0.011	0.012	-11%	3%
Psychiatric condition	0.121	0.130	-7%	0.019	0.023	-21%	14%
Diabetes	0.192	0.193	-1%	0.181	0.208	-15%	14%
Congestive heart failure	0.058	0.070	-20%	0.083	0.112	-36%	16%
Cancer excluding skin cancer	0.135	0.199	-47%	0.074	0.129	-74%	27%
Coronary artery disease	0.164	0.199	-21%	0.054	0.083	-54%	33%
Rheumatoid arthritis	0.098	0.110	-12%	0.023	0.044	-89%	77%
Median of the column of data	0.121	0.130	-14.7%	0.054	0.083	-36.0%	13.8%
Mean of the column of data	0.106	0.127	-20.5%	0.062	0.082	-36.3%	15.8%
Source: Analysis of MCBS cost and use files, 2003-2008 pooled							
Notes: *Negative values indicate that the Medicare-only population appears healthier than those with secondary coverage. ** Positive values indicate that claims data exaggerate the good health of the Medicare-only population compared to self-reported disease prevalence.							

We re-ran the regression analysis to show the effect of the HCCs on our estimates (Table 4C). With HCCs – which may exaggerate differences in health status – the estimated insurance impact on spending is 20 percent. Using the self-reported disease flags, by contrast, the estimated impact is 37 percent. Ignoring prevalence of disease entirely results in an estimated impact of 49 percent. Arguably, the plausible range of estimates lies in the interval bounded by the HCC and self-reported prevalence estimates. The inclusion of the HCCs likely results in a somewhat conservative estimate of the impact of insurance.

Table 4C: Effect of Different Measures of Prevalence of Diseases						
Regression-Adjusted Increase in Medicare Spending Associated With Secondary Insurance, Total Spending (A plus B)						
	Using HCCs		Using Survey Self-Reported		Using None	
Memo: Spending, Medicare Only	$ 4,535		$ 4,535		$ 4,535	
Percent increase associated with:						
Employer sponsored	14%	**	31%	***	44%	***
Employer + Individual	22%	***	41%	***	54%	***
Individual Purchase	27%	***	45%	***	56%	***
All secondary insurance combined	20%	***	37%	***	49%	***
Source: Analysis of MCBS 2003-2005 cost and use files, pooled.						
Notes: '* = p < .05, ** = p < 0.01, *** = p < .001						

Finally, we performed two other sets of robustness checks on the analysis. We included or excluded persons and measures whose use or exclusion might plausibly confound the analysis. None of these had any material effect on the results. First, we re-ran the estimates eliminating individuals with potentially questionable survey data. These include MCBS "ghosts" (new enrollees who were not actually surveyed, but receive proxy data from other surveyed individuals), and persons with proxy interviews (typically, individuals not mentally competent or interviews on behalf of beneficiaries who have died). Inclusion or exclusion had no material effect on the results.

Second, we added variables for factors that might plausibly confound the results. These included drug coverage (which may affect Part A and B spending, and is highly correlated with other secondary coverage). This had a negligible impact on the estimated effects of coverage for A and B services.[3] This also included measures of health

[3] We were concerned with the omitted variables bias, in the sense that failure to measure drug coverage might have affected the estimated impacts of Part A/Part B secondary coverage. The coverage variables are highly correlated, and spillovers from drug to acute care spending have been reported in the literature. We found no significant issue. Inclusion of a separate measure for drug coverage had essentially no effect on the estimated effect of A/B secondary coverage, and drug coverage was associated with a small and insignificant reduction in total A and B spending. By contrast, when we predicted *prescription drug spending* as a function of coverage, the reverse was not true. Secondary insurance for A/B services had a strong, positive impact on drug spending, and omission of the A/B secondary coverage variable resulted in a near-doubling of the estimated impact of drug coverage in isolation. The upshot is that, in this cross-section at least, drug coverage appeared a minor factor in determining A/B spending (conditional on health, sociodemographics, and other coverage), but secondary coverage for A and B services appeared to be a major driver of drug costs (conditional on those other factors). Plausibly, beneficiaries who are deterred from seeing their physicians for reasons of cost simply do not obtain (or do not continue to fill) as many prescriptions as others, regardless of drug coverage. This relationship (between out-of-pocket costs for visits and drug spending) was observed empirically in at least one setting (Cecil et al, 2006).

behaviors: current and former smoking status, and overweight and obesity. Again, these had little material impact on the estimated effects of insurance on spending.[4]

2.3 Effect of Near-First-Dollar Coverage (2009 report tables 7 and 8)

Background. The 2009 report showed that nearly all the impact of secondary insurance occurred for individuals who appeared to have essentially first-dollar coverage. For practical purposes, this was defined as beneficiaries who reported paying less than 5 percent of total cost of Part-B-covered spending. In aggregate, those who reported paying more than 5 percent of Part B costs had spending that was not significantly different from the Medicare-only (no secondary coverage) population.

This analysis has one major caveat. The MCBS does not explicitly gather information on the depth of insurance coverage. Instead, the only information we have is the *observed* depth of coverage, that is, the fraction of spending reported as paid out-of-pocket. This information is "censored" in the statistical sense. It is reliably available only for beneficiaries with significant Part B spending. For a variety of reasons, restricting the analysis to individuals with spending above some level will understate the true impact of depth of coverage on spending. For example, differences in average spending that result from the higher fraction of Medicare-only with very low spending will be eliminated once we restrict the analysis solely to individuals with significant spending. The regression coefficients are biased toward showing minimal differences across populations.

Second, even for non-zero spending, we are concerned that persons with total spending below the Part B deductible might have high apparent out-of-pocket costs (even if fully covered above the deductible), leading to a spurious correlation between observed depth of coverage and spending. For this reason, we have to limit the analysis to individuals with spending substantially above the Part B deductible.[5]

The upshot is that we first select individuals with substantial Part B spending, then identify those that appear to have first-dollar or nearly first-dollar coverage. For example, seeing no reported out-of-pocket cost for $1000 in Part B spending is a reasonable indication of complete coverage. The main caveat is that, by taking only those individuals above some spending level, we underestimate the effect of secondary

[4] Smoking and obesity affected costs and outcomes in quite different ways. Obesity in isolation is strongly predictive of higher costs, but obesity in a regression that includes self-reported health status had no predictive power. Apparently, obese individuals who report themselves in poor health have no higher cost than others in poor health. Second, current smoker status behaved counter-intuitively. It was associated with lower current spending, conditional on self-reported health and other factors in the regression. Apparently, it flags individuals who remain healthy enough to continue smoking. Smoking was, however, always associated with substantially higher likelihood of death during the year. So, while it was not important (or counterintuitive) for analysis of costs, it may be significant for analysis of mortality rates.

[5] We did not identify any standard statistical approach that had been applied to this situation. Models for left-censored data (e.g., Tobit) deal with the censoring of the dependent variable (in this case, the fact that spending is never below zero). In this case, by contrast, not only is the dependent variable left-censored, the main predictive variable (depth of coverage) is both censored (for zero spending), biased (for low levels of spending), and biased in a way that is strongly correlated with level of spending.

coverage on spending. For this reason, we are primarily looking to see whether there is some dose-response relationship (that those with first-dollar coverage have higher spending), and will not make much out of the magnitude of the difference.

Results. Table 5 shows that, within each secondary coverage category, beneficiaries with apparent near-first-dollar coverage have much higher spending than others. In each case, there is no statistically significant difference between those who have secondary insurance but paid at least 5 percent of Part B spending, and the Medicare-only population. This is after risk adjustment (as done above), and holds true both for all beneficiaries, and for beneficiaries with more than $1000 in total Part B spending.

Table 5: Part B Spending by Insurance and Revealed Out-of-Pocket Share							
Regression-adjusted Part B spending relative to Medicare-only Individuals							
Formerly Table 8, 2009 report.							
		All Individuals Regardless of Spending			Individuals with at Least $1000 in Part B Spending.		
Percent of Part B Spending Out-of-Pocket		% of Persons	Increase rel. to Mcr.-only	P value	% of Persons	Increase rel. to Mcr.-only	P value
2003-2005							
Employer-Sponsored							
	No Part B	5%	N/A		N/A	N/A	
	<5%	50%	68%	***	61%	24%	***
	>5%	45%	0%		39%	-6%	
Employer+Individual							
	No Part B	1%	N/A		N/A	N/A	
	<5%	52%	77%	***	61%	21%	***
	>5%	46%	23%		39%	6%	
Individual Purchase							
	No Part B	3%	N/A		N/A	N/A	
	<5%	63%	85%	***	75%	30%	***
	>5%	34%	12%		25%	4%	
2006-2008							
Employer-Sponsored							
	No Part B	4%	N/A		N/A	N/A	
	<5%	52%	51%	***	60%	17%	***
	>5%	44%	8%		40%	-4%	
Employer+Individual							
	No Part B	1%	N/A		N/A	N/A	
	<5%	50%	64%	***	56%	18%	***
	>5%	48%	11%		44%	-1%	
Individual Purchase							
	No Part B	3%	N/A		N/A	N/A	
	<5%	65%	65%	***	76%	22%	***
	>5%	32%	-2%		24%	-3%	
All Years Pooled							
Employer-Sponsored							
	No Part B	4%	N/A		N/A	N/A	
	<5%	51%	58%	***	60%	19%	***
	>5%	45%	3%		40%	-5%	
Employer+Individual							
	No Part B	1%	N/A	***	N/A	N/A	
	<5%	51%	69%	***	59%	20%	***
	>5%	47%	14%		41%	-1%	
Individual Purchase							
	No Part B	3%	N/A	***	N/A	N/A	
	<5%	64%	73%	***	76%	24%	***
	>5%	33%	2%		24%	-4%	
Notes: N/A no relevant data. By construction, these individuals have zero Part B spending. Coefficients should not be used to provide a quantitative estimate of the elasticity of spending with respect to coverage. See text for discussion of statistical bias issues.							
* = p < .05, ** = p< .01, *** = p < .001							
Source: Analysis of 2003-2008 MCBS files, pooled							

These findings suggest that, to a reasonable approximation, the entire effect of secondary insurance on spending is attributable to those who have first-dollar or nearly-first-dollar coverage. By contrast, beneficiaries with secondary coverage who paid at least 5 percent of Part B costs had spending not significantly different from the Medicare-only population.

Variation in methods. As noted above, the use of observed average coverage is far from ideal. This section briefly discusses the theoretical issue, and shows some alternative methods for separating those with near-first-dollar coverage.

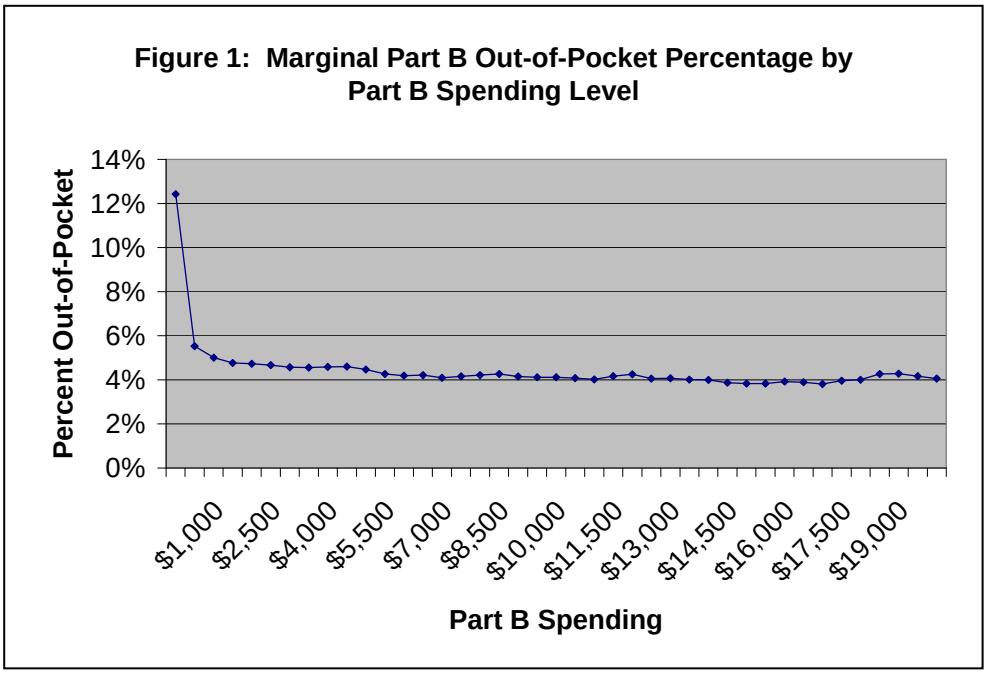
When the details of the insurance policy are known, actuaries calculate average coverage for each policy for some known distribution of spending. Applying the same spending to every policy ensures that the actuarial value of the policy is independent of the spending of the particular person who observed to own that policy. For example, actuarial value can be assigned to policies of individuals who had no spending in the year.

In our case, we do not know the details of every policy. (We sometimes have the plan letter for Medigap, but we need to apply the same methodology to all cases.) We would like to sort the policies into depth-of-coverage categories in some way that does not bias the results.

The issue is that we could have two individuals with identical or very similar coverage, but place them into different categories based on their observed spending. For example, if plan does not cover the Part B deductible, beneficiaries spending $500 and $5000 would have average copayment shares of 20% and 2%, respectively. This would place them into different categories (despite having the same underlying coverage) and worse, would place the low-spender into the low-coverage category (creating a spurious negative correlation between depth of insurance coverage and spending).

First, we redid the analysis with successively higher spending cutoffs, demonstrating that the results are robust to the choice of spending cutoff. In the main analysis, we dropped individuals with less than $1000 in Part B spending. In this re-analysis, we raised the spending threshold in $1000 increments. Up to $5,000, results remained for all three types of secondary coverage (that is, only individuals with near-first-dollar coverage had spending that was statistically significantly different from the Medicare-only population.) Up to $9000, it persisted for employer sponsored and Medigap (but not for those with both, a fairly small group). At those levels, we are unlikely to have mis-classified beneficiaries based on modest spending early in the year.

Second, we plotted the coinsurance percentage for $500 increments in spending (Figure 1). The effect of the deductible is clearly observable there, but there is little aggregate variation in coinsurance percentage above the deductible. Thus, if there is a potential for distorting the relationship between coinsurance and cost, it would come from including costs near the deductible. (The terminal point on the line is for all Part B in excess of $20,000).



This suggests an alternative approach of categorizing beneficiaries coinsurance rates *excluding the first $500 or $1000 in Part B spending*. This avoids having initial high coinsurance rates affect the categorization of beneficiaries.

This approach has both advantages and drawbacks. The main advantage is that it will not spuriously assign low-spending individuals to the high out-of-pocket category, merely due to the deductible. The main disadvantage is that if the deductible in fact suppresses spending, this method will ignore that (by failing to distinguish beneficiaries who do and do not pay the deductible).

Finally, we note that quirks of the data create some small, spurious deviations in measured out-of-pocket costs. We only included claims with some Medicare-covered Part B service. Some claims, however, also include non-covered costs (potentially adding to out-of-pocket costs), and some claims include bad debt (subtracting from out-of-pocket costs). Each of these sources (non-covered, bad debt) amount to several percent of total Part B spending.

Table 5B shows this revised approach, for three choices of spending thresholds. The results are qualitatively similar to the base case in Table 5: Only those with near-first-dollar coverage have spending that is statistically significantly higher than the Medicare-only population. The magnitudes, however, are smaller than in the original analysis. That could occur due to some mix of a) bias in the original analysis, or b) true suppression of spending by the deductible. Either way, eliminating the effects of the deductible (both on measured out-of-pocket percents, and on spending (via ignoring it for purposes of classifying individuals) results in the same conclusion as before. Regardless of type of coverage, only those with first-dollar or near-first-dollar coverage have Part B

spending that is higher and statistically significantly different from the Medicare-only population.

Table 5B: Variations on the Analysis of Near-First-Dollar Coverage						
			Using All Claims With Any Medicare Part B (includes non-covered services)		Using Claims With Only Medicare Part B (no non-covered services)	
	Average Out-of-Pocket Percent		**Total Part B Increase Rel. to Medicare-Only Spending**	**P value**	**Total Part B Increase Rel. to Medicare-Only Spending**	**P value**
All individuals, all spending included in calculation						
	Employer sponsored					
	< 5%		20%	***	21%	***
	> 5%		-4%		-6%	
	Employer+Individual					
	< 5%		21%	***	20%	***
	> 5%		0%		-1%	
	Individual Purchase					
	< 5%		25%	***	24%	***
	> 5%		-3%		-5%	
At least $500 Part B, ignore first $500 in coins. calculation						
	Employer sponsored					
	< 5%		18%	***	18%	***
	> 5%		2%		-1%	
	Employer+Individual					
	< 5%		21%	***	21%	***
	> 5%		9%		6%	
	Individual Purchase					
	< 5%		24%	***	23%	***
	> 5%		9%		9%	
At least $1000 Part B, ignore first $500 in coins. calculation						
	Employer sponsored					
	< 5%		15%	***	16%	***
	> 5%		1%		-1%	
	Employer+Individual					
	< 5%		15%	***	16%	***
	> 5%		6%		4%	
	Individual Purchase					
	< 5%		21%	***	20%	***
	> 5%		7%		8%	
At least $1000 Part B, ignore first $1000 in coins. calculation						
	Employer sponsored					
	< 5%		14%	***	15%	***
	> 5%		3%		0%	
	Employer+Individual					
	< 5%		15%	***	17%	***
	> 5%		7%		3%	
	Individual Purchase					
	< 5%		21%	***	20%	***
	> 5%		8%		9%	
Source: Analysis of 2003-2008 MCBS files, pooled						
Notes: *** = p < .001, ** = p < .01, * = p < .05						

We considered but did not model yet a third possible variant of this exercise. We could rely on only the spending *below* the cutoff for calculation of average coinsurance rates. That would use the exact same information for each beneficiary, but would ignore the bulk of the information regarding coverage (because all spending above the cutoff would be ignored for purposes of classifying beneficiaries). On net, while the use of identical information for all is appealing, the heavy reliance on the deductible portion of the curve for categorizing individuals seems a significant drawback. This third variation was not modeled.

2.4 Type of service breakouts (Tables 9 and 10)

Background: The tables 9 and 10 of the 2009 report demonstrated that the effect of secondary insurance varies widely across services. Using the same regression adjustment framework as the prior sections, we found no difference in use of emergency care (emergency visits, emergency or urgent hospitalizations, or ambulance transports), but larger differences for preventive care, scheduled admissions, and minor procedures.

Throughout this analysis, bear in mind that some services tend to come as part of a larger episode, while others can either be thought of as determining an episode (such as major surgery) or typically being stand-alone care (like screening and preventive services). Some services may be affected by secondary coverage because they are typically part of an episode that is affected by such coverage.

Results. Throughout this analysis, we struggle with the small number of observations for any given type of services. Medicare beneficiaries receive many significant services at rates of one service per hundred or per thousand beneficiaries. To increase sample sizes, we combined the three separate private secondary insurance categories into one. Pooling all private secondary insurance helps with this issue, as does looking at broader categories of services when possible.

We applied a second screen to address the issue of small sample size. Results are not presented for a spending category if fewer than 30 persons had such spending in the Medicare-only group, or of the services accounts for less than $10 per capita in spending. With a large number of services studied, these screens help suppress findings that are most likely "flukes" of the particular sample and set of claims examined, rather than more permanent aspects of secondary coverage.

While this does guard against making inferences based on a handful of persons, it also has the effect of eliminating almost all analysis of specific surgical procedures. Almost no major surgical procedures are common enough to exceed that cutoff for the Medicare-only beneficiary group. This means that the analysis is effectively restricted to looking at broader classes of procedures and services.

We used the following categorizations of spending to explore which services are more or less strongly affected by secondary insurance status:

- Physicians' services by site of service.
- Physicians' services by broad specialty category.
- Part B (physician and hospital outpatient) spending within broad Berenson-Eggers Type of Service (BETOS) categories.
- Hospital admissions by emergency, urgent, and elective status. Part B spending for all preventive services combined.

Table 6 shows regression-adjusted differences in per-capita spending between those with no secondary insurance and those with private insurance. The columns show the per-capita spending for those with no insurance, and the percent increase in spending associated with ownership of private secondary insurance.

With the exception of the inpatient data, there was little change in the estimated effect of secondary insurance. Pooled across the entire sample, the effect of secondary coverage was highest for office-based care (as opposed to inpatient services), medical specialists (as opposed to primary care physicians), and elective (as opposed to emergency or urgent) hospitalizations. In addition, those with secondary coverage were much more likely to receive preventive care.

The inpatient services results from the later period (2006-2008) do not match those of the earlier period (2003-2005). The pooled results (all years) provide results that are qualitatively similar to the original analysis, but smaller in magnitude.

Table 6: Medicare Spending by Category, Sorted By Estimated Impact of Private Secondary Insurance										
Update of 2009 report, Table 9										
	2003-2005				**2006-2008**			**All years pooled**		
	Medicare only mean	**% incr. With supp. Ins.**	**P**		**Medicare only mean**	**% incr. With supp. Ins.**	**P**	**Medicare only mean**	**% incr. With supp. Ins.**	**P**
Carrier claims by place of service										
All other sites	$ 127	23%	*		$ 174	9%		$ 149	13%	
Hospital inpatient	$ 281	32%	**		$ 289	10%		$ 285	21%	**
Hospital OPD	$ 261	33%	***		$ 275	40%	***	$ 267	36%	***
Office	$ 643	75%	***		$ 803	67%	***	$ 718	70%	***
Carrier claims by specialty (non-physician omitted)										
Generalists	$ 315	36%	***		$ 391	10%		$ 351	23%	***
Radiologists	$ 119	30%			$ 117	65%	**	$ 118	45%	***
Surgeons	$ 329	50%	***		$ 305	72%	***	$ 318	61%	***
Medical specialists	$ 341	89%	***		$ 482	54%	***	$ 407	67%	***
Inpatient claims by admission type										
Emergency	$ 1,221	-6%			$ 1,257	-23%	*	$ 1,238	-14%	
Urgent	$ 405	6%			$ 384	23%		$ 395	19%	
Elective	$ 405	90%	***		$ 624	24%		$ 508	52%	***
Preventive services (carrier and OPD claims combined)										
Payments	$ 21	97%	***		$ 25	91%	***	$ 23	93%	***
% with some service	37%	60%	***		$ 0.4	47%	***	38%	54%	***
Source: Analysis of 2003-2008 MCBS Cost and Use files.										
Notes: * = p < 0.05, ** = p < 0.01, *** = p < 0.001										

Table 7 shows carrier and hospital OPD payments by aggregations of Berenson Eggers Type of Service codes (BETOS). With few exceptions, the revised analysis shows much the same pattern of service use differences as the original 2009 study. First, presence of secondary insurance has little apparent effect on emergency care services, including ambulance services and emergency room visits. Second, presence of secondary insurance has a large effect on use of specialists' services, minor procedures, and endoscopy. New this year, the pooled results show a very large impact on major orthopedic procedures.

Table 7: Medicare Spending Sorted by Estimated Impact of Private Secondary Insurance										
Update of 2009 report, Table 10										
	2003-2005				2006-2008			All years pooled		
	Medicare only mean	% incr. With supp. Ins.	P		Medicare only mean	% incr. With supp. Ins.	P	Medicare only mean	% incr. With supp. Ins.	P
Ambulance (O1A)	$ 77	-21%			$ 70	10%		$ 74	-8%	
Emergency Visits (M3)	$ 58	0%			$ 66	-3%		$ 62	-2%	
Home/Nursing Home Visits (M4)	!	!	!		$ 14	-14%		$ 11	1%	
Hospital Visits (M2)	$ 125	28%	*		$ 142	-7%		$ 133	10%	
Major Proc, Cardiovasc (P2)	$ 74	30%			$ 89	11%		$ 81	18%	
Consultations (M6)	$ 65	34%	***		$ 74	23%	***	$ 69	28%	***
Clinical Lab Tests (T1)	$ 113	40%	***		$ 167	26%	***	$ 138	30%	***
Eye procedures (P4)	$ 115	14%			$ 85	66%	***	$ 101	35%	**
Tests other than clin lab (T2)	$ 44	32%	**		$ 55	41%	***	$ 49	35%	***
Chiropractic (O1B)	$ 15	30%			$ 18	42%		$ 16	36%	
Imaging, Advanced (I2)	$ 78	62%	***		$ 125	27%	*	$ 100	39%	***
Office Visits (M1)	$ 244	45%	***		$ 281	40%	***	$ 261	42%	***
Ambulatory Procedures (P5)	$ 62	52%	***		$ 78	42%	*	$ 69	47%	***
Anesthsia (P0)	$ 30	45%	***		$ 30	55%	***	$ 30	49%	***
Oncology (P7)	!	!	!		$ 43	104%		$ 52	49%	
Imaging, Standard (I1)	$ 92	54%	***		$ 102	51%	***	$ 97	51%	***
Major Procs, Various (P1)	$ 32	53%	*		$ 39	64%	*	$ 35	55%	**
Imaging, Echography (I3)	$ 50	56%	***		$ 57	61%	***	$ 53	57%	***
Chemotherapy Drugs (O1D)	!	!	!		!	!	!	$ 42	58%	
Imaging, Procedure (I4)	$ 15	72%	*		$ 16	48%		$ 16	61%	*
Specialist Visits (M5)	$ 57	78%	***		$ 64	65%	***	$ 60	70%	***
Minor procedures (P6)	$ 93	89%	***		$ 118	56%	***	$ 105	71%	***
Endoscopy (P8)	$ 54	100%	***		$ 71	77%	***	$ 62	87%	***
Drugs Excl Chemo (O1E)	$ 31	331%	***		$ 129	49%		$ 77	108%	***
Major Proc, Orthopedic (P3)	!	!	!		!	!	!	$ 19	121%	***
Source: Analysis of 2003-2008 MCBS Cost and Use files.										
Notes: * = p < 0.05, ** = p < 0.01, *** = p < 0.001, ! = fewer than 30 cases or $10 in that year										

2.5 Type of service impacts and first-dollar coverage

In this section, we take the prior two analyses to their logical conclusion, showing the impact of near-first-dollar coverage by type of service. In general, do those with *incomplete* (not first-dollar) secondary coverage forego the same types of services as those with *no* secondary coverage (the Medicare-only population)? Restated, does paying *some significant part* of the coinsurance generate the same service-mix effects as paying *all* of the coinsurance? If not, what services appear to be the major exceptions?

In part, this section can be viewed as a further test of the methods. In a prior section, we observed no (statistically significant) difference in *aggregate Part B spending* between those who paid more than 5 percent of Part B costs and those who paid full Medicare coinsurance. If the effect of coinsurance is uniform, we expect to see no difference in the detailed spending by type-of-service as well. In that sense, this is a further test of the method. We are testing whether only coinsurance matters for determining spending behavior.

One complicating factor is that we can only measure coverage for persons who have a significant amount of spending. As in the prior section, we restrict the analysis to individuals with at least $1000 in Part B spending, and categorize their average Part B out-of-pocket percentage ignoring the first $1000. As with the first-dollar analysis above, this will tend reduce the observed difference across populations. Whether or not it will introduce any other artifacts in the data is difficult to say.[6]

In the tables below, we have two sets of columns for the population with secondary coverage. The set of the left is for those with near-first-dollar coverage (out-of-pocket costs below 5 percent of Part B spending). The set on the right is for others (where out-of-pocket exceeds 5 percent). In both cases, the data show the difference in spending between those with secondary coverage and the Medicare-only population.

If the entire effect of secondary insurance were due to first-dollar coverage (and if the restriction of the analysis to those spending at least $1000 does not distort the prior results), then we would expect to see the following:

- The left-hand column (near-first-dollar coverage) should look very much like Tables 6 and 7 above.
- The right hand column (significant out-of-pocket payments) should consist of nothing but small and statistically insignificant differences.

[6] E.g., The $1000 Part B cutoff differentially excludes individuals who had no major health care event, such as a hospitalization, during the year. To the extent that hospitalization drives the observed mix of Part B services, and beneficiaries have little or no control over Part B services provided while hospitalized, we may observe different service use effects in this population than in the whole population.

Results. Table 6B is the analog of Table 6 above, but split by the presence of first-dollar coverage (and restricted to those with at least $1000 in Part B spending). In general, the left-hand columns are a good qualitative match for Table 6. In general, the right hand columns show smaller coefficients than the left, indicating some effect of coinsurance. But there remain several instances where the partial-coinsurance population uses more care than the full-coinsurance (Medicare-only) population.

The first-dollar-coverage population essentially matches the expectation. As in Table 6, service use (relative to the Medicare-only population) is most elevated for:

- Specialty: Medical specialists and surgeons, but not for generalists.
- Site: Office-based care more than inpatient.
- Inpatient: Elective admissions but not emergency or urgent.
- Preventive: Preventive services use is much higher.

The partial-coinsurance population does not completely match expectation that there would be no difference between this population and the Medicare-only population. For the population paying at least 5 percent of Part B out-of-pocket:

- Specialty: Modestly more care for medical specialists and surgeons.
- Site: Somewhat more care in the physician office.
- Inpatient: Fewer emergency admissions, no difference in elective admissions.
- Preventive: Preventive services use is as high as the first-dollar-coverage population.

Arguably the most inconsistent finding is that there is no reduction in preventive care use in the partial-coverage population. For this population (paying more than 5 percent of coinsurance, spending more than $1000 in Part B services), coinsurance was no deterrent to obtaining preventive care. [7]

[7] To test the possibility that their secondary coverage paid completely for preventive care, we matched the Part B claims to the survey event records and calculated mean coinsurance levels for preventive services and all other services (results not shown). The individuals faced essentially the same coinsurance rates for preventive as for all other care.

Table 6B: Medicare Spending by Category, Sorted By Estimated Impact of Near-First-Dollar Coverage						
Comparing those with and without near-first-dollar coverage to the Medicare-only population						
Restricted to individuals with at least $1000 in Part B spending, coverage category is based on spending in excess of $1000.						
			Near-first-dollar (<5% out-of-pocket)		**All others (>5% out-of-pocket)**	
	Medicare only mean		**% incr. With supp. Ins.**	**P**	**% incr. With supp. Ins.**	**P**
Carrier claims by place of service						
All other sites	$	335	3%		-1%	
Hospital OPD	$	644	6%		-4%	
Hospital inpatient	$	719	16%	**	3%	
Office	$	1,523	45%	***	26%	***
Carrier claims by specialty (non-physician omitted)						
Generalists	$	723	7%		0%	
Radiologists	$	315	26%	*	8%	
Surgeons	$	745	31%	***	13%	*
Medical specialists	$	957	47%	***	26%	**
Inpatient claims by admission type						
emergency	$	2,986	-14%		-24%	**
urgent	$	951	22%		6%	
elective	$	1,252	44%	***	19%	
Preventive services (carrier and OPD claims combined)						
Preventive services payments	$	39	41%	***	38%	***
Fraction with any preventive service		0.52	29%	***	30%	***
Source: Analysis of 2003-2008 MCBS Cost and Use files.						
Notes: * = p < 0.05, ** = p < 0.01, *** = p < 0.001						

Results were also somewhat mixed for the summary of physician spending by BETOS. In broad outline, results were mainly as expected. Services strongly and weakly affected by near-first-dollar coverage largely paralleled those of Table 7. Impact of less-generous coverage was, on average, smaller than that of first-dollar coverage. But there were significant exceptions.

First, with services ordered from smallest to largest effect of first-dollar coverage, the listing of Table 7A largely parallels that of Table 7. For example, there is no increase in emergency services (top of table), but a large increase in use of specialist visits. Second, with some exceptions, the estimated effect of secondary coverage is larger and more likely to be statistically significant for near-first-dollar coverage than for less-generous secondary coverage.

That said, several lines stand out for unexpectedly large or small spending in the less-generously-covered secondary insurance population:

- Rates of flu vaccine were unaffected by coinsurance (consistent with the findings on preventive care above).
- Echography use was unaffected by coinsurance. (Not shown, drill down by three-character BETOS code shows that echocardiography accounted for the bulk of the differences across insured populations.)
- Specialist visits were unaffected. (In Medicare, dollar-weighted, specialist visits consist mostly of eye examinations.)
- Office visits appear unaffected.
- There was significantly less spending on emergency care (consistent with the lower spending for emergency hospitalizations).
- Use of major cardiovascular procedures was lower; use of minor procedures and endoscopy was higher.

A plausible characterization of this pattern of spending is that it largely extends the preventive care finding from the prior table. By BETOS category, the partial-coverage population continues to get flu shots, have the cardiac status monitored, obtain eye exams, and get office visits. At the same time, these individuals spend less on emergency care and on major cardiovascular procedures.

Table 7B: Medicare Physician Spending by BETOS Category, Sorted By Estimated Impact of Near-First-Dollar Coverage						
Comparing those with and without near-first-dollar coverage to the Medicare-only population						
Restricted to individuals with at least $1000 in Part B spending, coverage category is based on spending in excess of $1000.						
			Near-first-dollar (<5% out-of-pocket)		**All others (>5% out-of-pocket)**	
	Medicare only mean		**% incr. With supp. Ins.**	**P**	**% incr. With supp. Ins.**	**P**
Ambulance (O1A)	$	180	-12%		-10%	
Emergency Visits (M3)	$	141	-12%	*	-19%	***
Eye procedures (P4)	$	254	-3%		-8%	
Home/Nursing Home Visits (M4)	$	25	2%		6%	
Major Procedures, Cardiovascular (P2)	$	208	10%		-34%	*
Consultations (M6)	$	166	11%	**	4%	
Clinical Lab Tests (T1)	$	282	12%	*	2%	
Hospital Visits (M2)	$	333	12%		0%	
Tests other than clinical lab tests (T2)	$	111	13%		9%	
Chiropractic (O1B)	$	27	15%		9%	
Ambulatory Procedures (P5)	$	163	16%		5%	
Imaging, Advanced (I2)	$	241	17%	*	0%	
Imaging, Standard (I1)	$	219	19%	***	9%	
Office Visits (M1)	$	486	22%	***	14%	***
Anesthsia (P0)	$	76	22%	***	7%	
Imaging, Echography (I3)	$	119	28%	***	26%	**
Major Procedures, Various (P1)	$	90	31%		9%	
Oncology (P7)	$	132	33%		-11%	
Influenze Immunization (O1G)	$	12	34%	***	34%	***
Imaging, Procedure (I4)	$	40	35%		20%	
Specialist Visits (M5)	$	119	38%	***	42%	***
Minor procedures (P6)	$	248	42%	***	21%	*
Endoscopy (P8)	$	147	44%	***	27%	*
Chemotherapy Drugs (O1D)	$	108	45%		-10%	
Major Procedures, Orthopedic (P3)	$	50	74%	**	51%	
Drugs Excluding Chemotherapy (O1E)	$	196	93%	***	22%	
Source: Analysis of 2003-2008 MCBS Cost and Use files.						
Notes: * = p < 0.05, ** = p < 0.01, *** = p < 0.001						

2.6 Summary and interpretation of service-use findings

Most of the results from this section have been long established in the research literature. Since the 1980s, studies have found higher total spending by those with secondary coverage, concentrated in Part B services (e.g., Link, Long, and Settle 1980; McCall et al, 1991; Grana and Stuart, 1996; Christensen and Shinogle, 1997; Khandker and McCormack, 1999). Even details such as the effect of first-dollar coverage have been noted previously (McCall et al., 1991). Estimated magnitudes were similar to the effect sizes shown here.[8]

The large effect of secondary coverage on screening and preventive services has repeatedly received attention (e.g, Blustein, 1995; Carrasquillo et al. 2001; Schneider et al., 2008). These studies tend to show about half as much preventive care in the population lacking secondary coverage, consistent with the results shown here. Similarly, significant cost sharing was associated with lower rates of mammography use in Medicare Advantage (Trivedi et al., 2008).

Here, drilling down into the data, we showed that the largest increase associated with secondary coverage was for physicians' services. There was minimal effect on hospital spending. Spending for home health and hospice (service categories with no coinsurance requirement) was consistently lower in the population with secondary insurance, but the difference was not statistically significant.

In terms of methods, we looked carefully at the HCC risk adjuster. HCCs were intended to be robust to differences in utilization because they merely flag the presence of any diagnosis on any hospital or physician (non-imaging, non-test) claim. But they did not appear robust here. Comparison to self-reported disease prevalence data suggests that the low HCCs score for the Medicare-only population is partially a reflection of lower disease prevalence, and partially an artifact of having fewer claims reported. Secondarily, the HCC to self-report comparison provided a rough guide to conditions for which the Medicare-only beneficiary is less likely than others to obtain some treatment during the year.

This issue with HCCs affects the measured impact of secondary coverage. Including HCCs in the regression gave a relatively modest estimated impact of secondary coverage on spending. Excluding the HCCs resulted in a substantially larger estimated impact.

[8] This is not to imply that the literature is uniform. Studies that fail to find this result appear to fall into two general categories. One consists of studies using multi-stage or simultaneous-equations statistical techniques, in an attempt to correct for unobservable selection bias. Results from such studies vary widely. A second genre consists of "natural experiments", typically using a sample of convenience (e.g., a health care plan for the elderly) where an increase or decrease in coinsurance is observed, and cost changes are tracked pre- and post-change (e.g., Trivedi et al, 2010). As published, these tend to show cost increases accompanying Part B coinsurance increases, via large indirect "spillovers" to Part A spending. Both of those findings – that Part B coinsurance raises costs, and that the effects occur largely through Part A spending – run contrary to the long-standing finding of lower total costs, and equal Part A costs, for those without secondary coverage (compared to those with such coverage).

Substituting self-reported disease prevalence for HCCs gives an estimated impact between those values.

The HCC finding is relevant to further work in this area. Medicare claims now identify beneficiaries with secondary payer claim crossover arrangements in place.[9] Most insurers with significant Medicare secondary coverage business have such agreements. This makes it possible to identify substantially all beneficiaries with secondary coverage, in Medicare claims files, opening the possibility of studying service mix in greater detail via claims data. Such an analysis would, however, rely entirely on claims and other administrative data. In particular, the sole option for risk adjustment is likely the HCC model or some similar approach. This suggests caution in using the HCC model, and suggests that it may be worthwhile to reconfigure the claims-based risk adjuster to be less sensitive to claims volume, possibly by restricting it to services known to be relatively insensitive to the presence of secondary coverage.

We also showed that the spending increase with secondary coverage is associated with near-first-dollar coverage (defined here as paying less than 5% of the total cost of Part B services.) We flagged "near-first-dollar coverage" several different ways, aiming to eliminate the effects of paying the Part B deductible. Each time, we found that increased spending was associated with first-dollar coverage and not with less comprehensive secondary insurance.

By type of service, we validated the results of the 2009 report. Emergency care (ambulance, emergency visits, emergency admissions) seemed insensitive to the level of secondary coverage. This effect had been observed previously in a detailed study of emergency care in a single area (Ho et al, 2002). By contrast, elective admissions, minor procedures, endoscopies, and (new in this analysis) major orthopedic procedures (primarily joint replacements) appeared sensitive to the level of secondary coverage.

When we combined the detailed type-of-service analysis and the first-dollar-coverage analysis, the results were mixed. In broad outline, those with limited secondary coverage (paying 5% or more of Part B costs) showed smaller (but non-zero) increases in service use than the first-dollar population. There were, however, significant exceptions. The limited-coverage population did not reduce their use of Medicare screening and preventive services. Further, they did not reduce their use of certain physicians' services likely indicative of secondary prevention and monitoring of specific conditions, including specialist visits (likely eye exams), echography (likely monitoring of heart conditions), and office visits in general.

[9] These are arrangements whereby the Medicare carriers, for a fee, forward clean electronic claims directly to the beneficiary's secondary payer. This largely avoids the requirement that providers submit (and payers process) a second claim for collection of coinsurance and deductible amounts.

3 CHANGES IN HEALTH STATUS AND CHANGES IN COVERAGE

In this section, we ask whether lower use of Part A and Part B services in the Medicare-only population results in faster declines in health status. We also check whether having some secondary coverage, but paying at least 5 percent of costs, has a similar impact on health status. Finally, we look at changes in health insurance over time, particularly for the Medicare-only population.

Two general sets of comments frame this analysis. There is little directly relevant research literature, and this is a difficult task, both conceptually and technically.

A first comment for framing this analysis is that most of the literature in the area of insurance and health status deals with the uninsured, or with Medicare drug coverage. By contrast, we focus on secondary coverage of Part A/Part B services, and the effects of reduced Part A and B service use on health. Evidence from an uninsured population may not be relevant to a relatively well-insured one (Medicare beneficiaries). Results by type-of-service above show that it is not plausible to assume that impact of coinsurance on one type of service (in this case, prescription drugs) would be relevant to another (Part A or Part B services).

There is a reasonably large body of evidence showing that lack of secondary coverage affects the process of care. At the minimum, it appears to reduce use of services generally accepted as beneficial. This includes preventive care as noted above, as well as a broader set of "necessary care" indicators from the Access to Care for the Elderly Project (MedPAC, 2003). These change in process of care suggest a decline in health status is plausible, but they do not directly demonstrate it.

Two published studies show higher mortality rates for beneficiaries with less complete coverage. Doescher et al. (2000) used the 1987 NMES, looking at five-year mortality rates for Medicare beneficiaries with private secondary coverage. They found that those with the least comprehensive secondary coverage were about 40 percent more likely to die than those with the most comprehensive secondary coverage. Porell and Mitilades (2001) pooled six years of MCBS data, looked at transitions in functional status and mortality. They found that having secondary insurance was associated with roughly one-third lower risk of developing an ADL limitation during the year, and, among the elderly with ADL limitations, one-third lower risk of dying.

In our 2009 report, we noted that the mortality rate of the population with secondary coverage was about one-third lower than the Medicare-only population (Hogan 2009, page 22). At that time, we suggested that more data were needed to determine whether or not the higher mortality of the Medicare-only population could be attributed to their lower service use. [10]

[10] Oddly, these studies all show mortality differences for *secondary* coverage that are roughly as large as those found between *uninsured and insured* individuals. For example, one study of the near-elderly found a one-third higher risk of mortality (over a decade) for those uninsured in the baseline year (Baker et al., 2000).

A second general comment for framing this analysis is that it is inherently difficult to show the link from coverage to worsening health outcomes in this situation. This occurs for both conceptual reasons (having to do with the timing of health care and health status changes), and for several purely technical reasons (having to do with changes in coverage over time, and limitations on the available data).

Below, we list the main challenges to estimating these effects correctly. These mainly serve as general threats to the validity of the conclusions. We can only directly address a few of these issues in the analysis.

First, the link from coverage to spending is contemporaneous, with a clear mechanism. The link from service use to health status occurs with some lag, and often without clear evidence of a mechanism. For example, failure to obtain screening and preventive services will have an effect only when (e.g.) cancers develop. The contemporaneous impact of coverage on spending means that we can plausibly study spending by looking solely at the current year's insurance coverage. By contrast, if the impact of coverage on health occurs over time, changes in coverage over time may affect the results.

Second, as shown below, coverage frequently changes over time. The need to track health status changes for more than a year at a time requires that we understand how coverage changes between baseline and final periods of observation.

Third, in Medicare, in some cases we know that changes in health status *cause* changes in insurance coverage. Medicaid spend-down is the clear example. Beneficiaries can obtain Medicaid coverage in most states by "spending down" to the required poverty level. (This provision is the reason that Medicaid beneficiaries are not separately considered in this analysis.) Every year, a portion of the sickest beneficiaries will transition to Medicaid coverage and be lost to this analysis. In effect, this truncates the observed illness distribution for the near-poverty population with no or private insurance. (Once sufficiently frail, coverage transfers to Medicaid).

Fourth, removal of institutionalized beneficiaries from the MCBS analysis excludes more than one-third of deaths. The main outcome of interest in this section is mortality. In general, we must exclude the institutionalized because we do not know their secondary insurance coverage. They account for just 6 percent of MCBS 65+ enrollees, but 35 percent of 65+ deaths. Even minor differences in timing and extent of institutionalization could affect measured mortality rates. For example, if those with secondary insurance are more likely to move to assisted living earlier in their lives, this will depress the measured mortality rate for the community-resident secondary-coverage population studied here. This is also likely to skews the mix of deaths observed, as diseases leading to substantial disability (and institutionalization) prior to death (e.g., congestive heart failure, but particularly Alzheimer's disease and other dementias) are likely under-represented relative to others (heart attack, vascular incident, cancer).

Fifth, decedents in a year are more likely to have their surveys answered by some proxy (spouse, child, or caretaker). If some proxy respondents do not know the decedent's secondary coverage or give pro-forma "no" answers, then such decedents will end up assigned to the Medicare-only population. That is, death may cause assignment to the Medicare-only category due to missing secondary coverage information from proxy respondents.

Sixth, coverage for Part A and Part B services is highly correlated with prescription drug coverage, but studies seldom attempt to estimate the separate effects of coverages. Numerous studies show a strong and immediate effect of drug coverage on compliance with drug therapies. For example, entering the "doughnut hole" in current Part D coverage immediately reduces compliance with prescription orders (Polanski et al., 2011). While the link from non-compliance to outcome is less clear, this suggests that a conservative estimate of the effect of Part A/Part B secondary coverage should include a separate measure of drug coverage as well. Absent that, the substantial correlation between A/B secondary coverage and drug coverage in the historical data means that the A/B coverage variable will capture both the direct effect of A/B coinsurance, and some of the effect of drug coverage.

Seventh, MCBS sample sizes are generally too small to show a link between specific services and outcomes. Analysis would be stronger if we could link specific health outcomes to the use of related services, for example, cancer morbidity to cancer screening. Small sample size largely precludes this. For the entire 6 year period, for the Medicare-only population, we observe the following counts of inpatient admissions: Heart attack, 25; stroke, 24; cancer, 35; congestive heart failure, 180. For the entire period, we have 141 deaths in the Medicare-only population. We will be hard-pressed to get stable estimates for aggregate measures such as overall health status and mortality.

Eighth, the estimated impact of coverage on health, conditional on health status in the base year, should get larger when longer time spans are considered. Inclusion of base year health status as a risk adjuster means that we only capture deterioration in health beyond the base year. Studies looking at longer time periods should see larger proportionate impacts. This means that results from the MCBS (looking at one-year changes) may not be comparable to other estimates.

Methods and results. For this analysis, we pool the six years of MCBS data, taking individuals who were in two successive years of the file.[11] We look for more or less rapid deterioration of aggregate measures of health status in the Medicare-only population. The aggregate measures of health status are:

- Self-reported health status
- Count of ADL limitations

[11] By construction, this analysis excludes decedents from the first year of data, and drops all MCBS "ghosts" (proxies for new enrollees). In addition, for certain results, this analysis also drops all individuals institutionalized in the second year. Because decedents and institutionalized account for such a large share of spending, this analysis necessarily provides a somewhat distorted view of (e.g.) year-to-year changes in spending.

- Institutionalization
- Transition to Medicaid coverage (which may reflect health, or income, or both)
- Death

In addition, we test several variations in methods. Once, with no constraints on spending, contrasting the Medicare-only population to each of the secondary coverage populations. Then again, for those with at least $1000 in Part B spending, contrasting the Medicare-only population to the secondary coverage population split by near-first-dollar coverage (those with out-of-pocket costs under 5% of Part B) and others. We also test the inclusion of a separate drug coverage variable in the regression.

Following Riley (2000), we include baseline health status and ADL limitations as a series of binary variables for each level of health status, and predict the final year health status or ADL limitations. This is necessary due to the bounded nature of those variables – individuals with excellent health in the base year can go no higher on the health scale, those with poor health in the base year can go no lower.

In principle, the health status and ADL variables should be run as ordered ordinal variables. For this analysis, we simply group them into binary values (e.g., health fair or poor), which avoids the ordering issue entirely. This way, we can run all five outcomes (health, functional status, institutionalization, Medicaid coverage, and death) in a reasonably comparable format.

Statistical tests in this section are only approximate, for three reasons. First, contrary to possible best practice, we continue to use the cross-sectional MCBS weights, rather than try to adapt the longitudinal weights to this pooled approach. (The longitudinal weight files were not available for the earliest MCBS years, and there is no obvious way to use them when pooling this many years in any case). Second, the SAS procedure used here (Logistic) does not account for MCBS design effects. Statistical significance levels will therefore be modestly overstated relative to true levels of significance. Third, in theory, differences in mortality in the base year impose differential censoring on the final-year outcomes. The results in this simple two-year framework are modestly less exact than could be had (in theory) by estimating a more complete model (e.g., a proportional hazards model) accounting for the censoring.

Finally, for mortality (only) we can run the one-year mortality rate in the standard cross-section of data. This is to some degree redundant with the second-year mortality estimate. But it is worth doing separately due to the larger number of observations available, and a sample that is more nearly representative of the entire Medicare population.

Results. Tables 8A and 8B show the unadjusted mean values of the variables of interest. Because this unadjusted table in fact captures most of the phenomena found in the regression-adjusted results, we describe it at length.[12]

[12] Some technical notes follow: While we know mortality rate and institutional status for all, we do not capture insurance or health status of the institutionalized. In addition to the count of persons, we therefore

The Medicare-only (no secondary coverage) population is substantially more likely to die, become institutionalized, or enroll in Medicaid in the second year (Y2 here), compared to the population with secondary coverage (Table 8A). That is true for all beneficiaries and for those with at least $1000 in base-year Part B spending (the population for whom we can identify depth of secondary coverage).

Table 8A highlights the substantial instability of Medicare-only coverage. In a single year of aging, 7 percent of the Medicare-only population switched to Medicaid coverage, and 5 percent was lost via death. A further 3 percent became institutionalized (at which point secondary coverage is no longer known

This large one-year outflow from the Medicare-only population implies one of two outcomes. Either the Medicare-only population rapidly shrinks (as a percent of the total) as the cohort ages, or a significant number of individuals move *into* the Medicare-only category each year, replacing those who left. We eliminated the first possibility by tabulating Medicare-only as a fraction of population by age (not shown). The proportion falls slowly with the age of the cohort. Individuals must be continuously moving into Medicare-only status to replace the ones lost to Medicaid coverage and death.

The upshot is that Medicare-only coverage status appears highly unstable. While the total number of Medicare-only beneficiaries is roughly constant from year to year, beneath the total is a rapid flow of individuals out of and into Medicare-only coverage.

The instability of Medicare-only coverage does not appear to have been addressed in prior analyses of secondary coverage. This will be addressed in more detail in the next section.

Finally, we note from the bottom section of Table 8A that first-dollar coverage appears to have no particular beneficial effect, relative to less comprehensive secondary coverage. Within the secondary-coverage population, those with first-dollar coverage have a slightly *higher* rate of mortality (4.6 percent versus 3.8 percent) and institutionalization (2.1 percent versus 1.7 percent) than others.

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also show a second, lower count of individuals for whom we have full information in the second year. We do not show the change in spending between years, because by definition the Year 1 population excludes decedents, while the Year 2 population includes them. In addition, by construction, the MCBS "ghosts" drop from the overlapping-panels analysis. Statistical tests are not provided here, but are shown on the regression-adjusted numbers in a subsequent table. (But in fact, large differences in this table are statistically significant in the regression analysis, and small differences are not).

Table 8A: Persons in MCBS for Two Years, Second Year Characteristics by First Year Insurance Status, No Regression Adjustment					
	Cases		Percent of Population, Y2		
	Total	Not institut-ionalized	Died	Institutionalized	Medicaid
			Y2	Y2	Y2
All beneficiaries					
No secondary coverage	1,340	1,292	5.2%	3.1%	7.1%
Private secondary coverage	16,004	15,669	3.5%	1.7%	0.4%
Empl_spons	8,522	8,378	3.3%	1.4%	0.3%
Emp+Indiv	1,470	1,454	3.0%	0.9%	0.3%
Individual	6,012	5,837	4.0%	2.4%	0.6%
Beneficiaries with at least $1000 in Part B (base year)					
No secondary coverage	584	562	7.8%	3.5%	7.7%
Private secondary coverage	10,214	9,963	4.4%	2.0%	0.4%
First dollar (<5% of B paid)	8,229	8,023	4.6%	2.1%	0.4%
Not first dollar (>5% of B)	1,985	1,940	3.8%	1.7%	0.5%
Private coverage by type					
Empl_spons	5,362	5,250	4.1%	1.7%	0.3%
First dollar (<5% of B paid)	4,010	3,924	4.5%	1.8%	0.3%
Not first dollar (>5% of B)	1,352	1,326	3.1%	1.5%	0.4%
Emp+Indiv	988	974	3.9%	1.2%	0.2%
First dollar (<5% of B paid)	725	717	4.1%	1.0%	0.3%
Not first dollar (>5% of B)	263	257	3.6%	1.7%	0.0%
Individual	3,864	3,739	5.0%	2.7%	0.7%
First dollar (<5% of B paid)	3,494	3,382	4.8%	2.7%	0.6%
Not first dollar (>5% of B)	370	357	6.6%	2.8%	1.5%
Source: Analysis of 2003-2008 MCBS Cost and Use files.					

Table 8B looks at various health status measures, in a manner parallel to 8A. Here, we show the first and second year values, then the difference. Unlike Table 8A, differences are uniformly small with no consistent direction. The Medicare-only population shows a slightly larger decrease in the population with best self-reported health status, but also a slightly smaller increase in the population in worst heath. Not shown, results for restrictions on Activities of Daily Living showed a similar pattern of small and inconsistent differences across the groups.

Table 8B: Persons in MCBS for Two Years, Change in Health Status Measures

	Health Excellent/Vgood			Health Fair/Poor		
	Y1	Y2	**Diff**	Y1	Y2	**Diff**
All beneficiaries						
No secondary coverage	48.9%	46.4%	-2.5%	20.8%	22.5%	1.6%
Private secondary coverage	50.5%	48.8%	-1.7%	16.3%	18.2%	1.9%
Empl_spons	49.7%	47.9%	-1.8%	16.4%	19.1%	2.6%
Emp+Indiv	58.2%	56.5%	-1.6%	10.9%	12.3%	1.4%
Individual	49.6%	48.0%	-1.6%	17.6%	18.6%	1.0%
Beneficiaries with at least $1000 in Part B (base year)						
No secondary coverage	36.6%	36.7%	0.1%	31.2%	31.0%	-0.2%
Private secondary coverage	42.1%	41.8%	-0.4%	21.2%	23.0%	1.8%
First dollar (<5% of B paid)	41.2%	41.2%	0.0%	21.9%	23.5%	1.6%
Not first dollar (>5% of B)	46.1%	44.3%	-1.7%	18.4%	20.8%	2.4%
Private coverage by type						
Empl_spons	41.8%	40.7%	-1.1%	21.1%	23.6%	2.5%
First dollar (<5% of B paid)	40.8%	40.1%	-0.7%	22.1%	24.3%	2.2%
Not first dollar (>5% of B)	44.5%	42.3%	-2.2%	18.5%	21.9%	3.4%
Emp+Indiv	49.8%	50.6%	0.8%	14.9%	15.7%	0.8%
First dollar (<5% of B paid)	47.8%	51.0%	3.2%	15.6%	15.5%	-0.1%
Not first dollar (>5% of B)	55.5%	49.5%	-6.0%	13.0%	16.3%	3.4%
Individual	40.6%	40.9%	0.3%	23.1%	24.0%	0.9%
First dollar (<5% of B paid)	40.1%	40.2%	0.0%	23.2%	24.4%	1.3%
Not first dollar (>5% of B)	45.0%	48.3%	3.3%	22.4%	20.3%	-2.1%

Source: Analysis of 2003-2008 MCBS Cost and Use files.

Table 9 shows regression-adjusted estimates of the same outcome and health status measures. For this analysis, we predict the likelihood of the (binary) outcome in the second year, conditional on demographics, health, risk adjustment factors, and insurance coverage in the first year.[13] We ran the estimates twice – once using ordinary least squares (with proper correction for MCBS design effects), once in a logistic regression. In both cases, we converted the coefficient (or the odds ratio) to show the percent change in each population, relative to the Medicare-only population.

The regression-adjusted results parallel the simple means in the earlier tables (Table 9), with large differences in outcomes (death, institutionalization) and no statistically significant differences in health status trends. Beneficiaries with no secondary coverage appear substantially more likely to die, become institutionalized, and obtain Medicare coverage. However, none of the changes in the health status measures was statistically significantly different between the Medicare-only and secondary-coverage populations.[14] Looking at the lower portion of the table, there appears to be essentially no difference in outcomes for the population with near-first-dollar coverage and the remainder of the population with private secondary coverage.

[13] For this analysis, we also added past and current smoking (and obesity), as smoking is known to be (and in this regressions, is) a significant determinant of mortality.

[14] Detailed technical notes follow: We ran this using both OLS (correctly accounting for MCBS design effects) and logistic (where such a correction was not available in the version of SAS used). In addition, logistic regression sometimes would not converge when the HCC categories were included in the regression, so the self-reported disease prevalence variables were used instead of the HCC categories. In the final strictly cross-sectional regression explaining mortality, much of the explanatory was attributed to the drug coverage variable, not the A/B coverage. This is the principal reason that the cross-sectional coefficient is small.

Table 9: Persons in MCBS for Two Years, Change in Health Status Measures by Secondary Coverage										
Regression-adjusted estimates using ordinary least squares and logistic regression										
All Beneficiaries										
		Ordinary Least Squares					**Logistic**			
Outcome	Mean, Mcr-only Pop	Impact of private supplemental	P value				Impact of private supplemental	P value		
Died	5.2%	-31%	**				-28%	*		
Institutionalized	3.1%	-44%	**				-42%	***		
Entered Medicaid	7.8%	-89%	***				-92%	***		
Health Excellent/Very Good	46.2%	0%					2%			
Health Fair/Poor	22.9%	-6%					-14%			
No ADL limitations	73.3%	2%					20%			
Three or more ADL limitations	7.4%	1%					-2%			
Memo: Died, one-year cross-section	4.3%	-15%					-13%	*		
Beneficiaries with > $1000 Part B spending										
	Mean, Mcr-only Pop	Impact of near-first-dollar coverage	P value	Impact of other secondary coverage	P value		Impact of near-first-dollar coverage	P value	Impact of other secondary coverage	P value
Died	7.8%	-30%	*	-29%	*		-26%		-32%	
Institutionalized	3.5%	-32%		-33%			-29%		-34%	
Entered Medicaid	7.7%	-90%	***	-88%	***		-94%	***	-91%	***
Health Excellent/Very Good	36.7%	0%		-1%			-1%		2%	
Health Fair/Poor	31.0%	-5%		-5%			-10%		-12%	
No ADL limitations	66.4%	2%		1%			16%		13%	
Three or more ADL limitations	11.0%	0%		3%			-1%		2%	
Died, one-year cross-section	6.9%	-23%	*	-17%			-23%		-20%	
Source: Analysis of 2003-2008 MCBS Cost and Use files.										
Notes: * = p < 0.05, ** = p < 0.01, *** = p < 0.001										

One puzzling aspect of the results is the apparent disconnect between the self-reported health status variables and the outcomes, with large effects on outcomes and no statistically significant effects on health status. One possibility is that these factors simply change more slowly than the outcomes, or that, given the timing of the MCBS survey rounds, the survey-based data may be less sensitive to single-year changes. For example, deaths will be recorded in administrative data regardless of date, but declining functional and health status prior to death will only be obtained if the health status survey component is delivered near the time of death.

Alternatively, possibly some dynamic aspect of the Medicare only population may drive this discrepancy. Note that, in this single year, more than 12 percent of the Year 1 Medicare-only population was lost to death and Medicaid enrollment in Year 2. Yet, Medicare-only as a fraction of total enrollment has been roughly constant for many years, and the Medicare-only population does not rapidly diminish with the aging of beneficiary cohorts.[15] Clearly, there must be roughly an equally large annual flow *into* the Medicare-only cohort. Are some significant number driven to Medicare-only status as part of some negative life event?

Medicare-only (no secondary coverage) status changes significantly from in one year (Table 10). In this MCBS sample, restricted to non-institutionalized individuals alive in two consecutive years (ignoring death and institutionalization), we cross-tabulate insurance status from prior to current year, and current to subsequent year, then averaged.

For Medicare, 12 to 13 percent of non-institutionalized survivors change coverage every year. Reading across the top block of lines, roughly 5 percent of Medicare-only enrollees move to private coverage, 8 percent to Medicaid. Reading down the bottom block of lines, roughly 7 percent had private coverage in the prior year, roughly 5 percent had Medicaid. On net, ignoring institutionalization and death, there is a flow of patients from private coverage, through Medicare-only coverage, into Medicaid coverage.

Private coverage, by contrast, is stable. In any year, 98 percent of those in private coverage had such coverage the prior year, and 98 percent will retain such coverage into the next year (Table 10).

[15] Medicare-only accounted for 10 percent of the 65-69 cohort, but still accounts for 6 percent of the 85+ cohort.

Table 10: Approximate Patient Flows, One Year, MCBS (Ignores Death and Institutionalization)				
	Destination			
	1:Medicare only	2:Private	3:Medicaid	Sum
1:Medicare only	87%	5%	8%	100%
2:Private	1%	98%	1%	100%
3:Medicaid	4%	5%	91%	100%
	Source			
	1:Medicare only	2:Private	3:Medicaid	
1:Medicare only	88%	1%	7%	
2:Private	7%	98%	6%	
3:Medicaid	5%	1%	87%	
Sum	100%	100%	100%	
Source: Analysis of 2003-2008 MCBS cost and use files, pooled.				

This one-year view cannot determine whether there is some large core of stable Medicare-only enrollees (so that the same 12 to 13 percent cycles back and forth across coverages). Or, whether most of what we observe as the Medicare-only population is, in fact, only temporarily in that status.

To answer that question, we turned to the Health and Retirement Study.[16] The HRS is a long-term longitudinal study of a cohort of individuals who were nearing retirement in 1992. Surveys occur roughly every two years, starting in 1992. Although not specifically designed to track Medicare secondary coverage, the HRS has enough information in most years to construct a secondary coverage variable roughly equivalent to the (more detailed) MCBS information. It is not possible, however, to distinguish individual-purchase Medigap from other insurance coverage in most years.

Table 11 shows individuals who were 1) interviewed in both 2010 and 2000, 2) enrolled in Medicare in 2010 and in 2000 (10-year Medicare survivors), 3) had no secondary coverage in 2010, 4) not institutionalized in 2010, and 5) were not working in 2000, if their sole source of private coverage was reported as some type of employer sponsored plan. (The latter condition is to avoid confusing Medicare working aged (with Medicare as the secondary payer) with employer-sponsored retirement coverage).

Half the population observed to have no secondary coverage in 2010 had private secondary insurance in 2000 (Table 11). Only 29 percent had no secondary coverage in 2000, meaning that 71 percent of the population started with some secondary coverage in

[16] Accessible at http://hrsonline.isr.umich.edu/.

2000. Over the decade, under 13 percent of the final Medicare-only population had never had any secondary coverage.[17]

Table 11: The Year 2000 Secondary Coverage of Non-Institutionalized Medicare Beneficiaries With No Secondary Coverage in 2010.	
Year 2000 Secondary Coverage	**% of population**
Private	50%
None	29%
Medicare HMO	15%
Medicaid/Government (Inc. Military)	6%
Memo: Percent who never had any secondary coverage shown on any biannual survey, 2000 to 2010	13%
Source: Calculations from Health and Retirement Survey "Fat" files, 2000 to 2010.	
Notes: Includes individuals who were Medicare-enrolled in 2010 and 2000, were not institutionalized in 2010, and who were not employed in 2000 if the sole source of secondary coverage was employer-sponsored coverage.	

By focusing on community-resident survivors only, Table 11 likely understates the total turnover of the Medicare-only population. Even with that, it shows the substantial instability of the Medicare-only coverage status.

Using the HRS, we looked prospectively from 2000 forward to determine whether the transition from private coverage to Medicare only was associated with income and wealth in 2010. We found a small and consistent effect of income, with wealthier individuals have a modestly (and statistically significantly) lower chance of transitioning to Medicare-only status.

Unfortunately, we simply do not have enough sample size to determine whether individuals moving into Medicare-only status are at elevated risk of death. Table 12 tabulates all persons on the pooled file who were Medicare-only in the base year, in the MCBS and not institutionalized in the prior year, by type of secondary coverage in the prior year. Although the mortality rate of those leaving private coverage is elevated, the difference is not statistically significant. We would need a far larger sample of individuals to resolve this issue.

[17] It is "less than" 13 percent because individuals are only interviewed every two years, and not every individual participates in every interview. This is therefore an upper bound on the fraction of persons who never had secondary insurance over the period.

Table 12: Insufficient Evidence on Insurance Transitions and Mortality Rate

Individuals with Medicare-only coverage in base year, by secondary coverage in prior year.

	Persons in Sample	Mortality Rate	Difference	T-statistic	P value
Medicare only	1079	4.5%	0.0%		
Private secondary	79	10.4%	5.9%	1.69	0.09
Medicaid	62	4.9%	0.4%	0.15	0.88

Source: Analysis of MCBS 2003-2008 cost and use files.

4 BRIEF SUMMARY AND POLICY DISCUSSION

This section provides a brief, high-level overview what has been accomplished in this research. Details on specific findings, by contrast, are given in the executive summary at the front of the report.

The first goal of this analysis was to validate the earlier (2009) study, using more data, and testing reasonable variations on the methods. By and large, results here appear quite similar to, but somewhat smaller in magnitude than, the original 2009 study results.

In part, the modestly smaller magnitudes are due to the incoming MCBS data. The most recent three years of files in fact show a smaller difference in spending by secondary insurance status. Pooling those years into the analysis reduced most measured impacts across-the-board. These reductions were not, however, statistically significant in and of themselves. That is, there was no statistically significant trend in the finding, just findings that were modestly smaller.

In part, however, some of the less extreme findings were the result of changes in methods. In particular, we revised the way in which "near first dollar" coverage was defined. In both the 2009 study and this analysis, we restricted the analysis to individuals with $1000 or more in Part B spending, to provide some information on average secondary insurance generosity. In this study, however, we tossed out the first $1000 in spending before calculating out-of-pocket percentages. All things considered, this was probably a more conservative approach, because it eliminated any chance that deductibles for low-spending individuals would influence assignment to insurance class. (At the price of ignoring any use-deterring effects that deductibles might have.) Having done that, we still found statistically significantly higher spending only in the near-first-dollar group, but spending differences across insurance coverage ("near-first-dollar" and other) became less extreme.

Except for the awkwardness of having to restrict the analysis to those with substantial Part B spending, we would have preferred to structure this, from the outset, as a study of three populations: Medicare only, less-than-first-dollar secondary insurance, and first-dollar coverage. In the end, we seem to have achieved much of that anyway, by piecing together the individual results.

Briefly: The Medicare-only population has both low cost and apparently poor outcomes. The first-dollar-coverage has high cost and good outcomes. And the less-than-first-dollar population appears to hit the "sweet spot" of benefit design: Low cost and good outcomes. Supporting that conclusion, we found that the less-than-first-dollar population did not stint on screening and preventive care, office visits, or certain types of specialty care likely associated with monitoring of chronic conditions (eye exams, echocardiography).

How likely is it, that this represents a real finding related to benefit design, and not idiosyncrasies of these populations? One of the interesting findings from the quality

analysis is that most of the individuals we observe in the Medicare-only population used to be members of the private secondary coverage population. This makes it more difficult to argue that some unique and unobservable protective factor is associated with private coverage. If so, it must disappear when those individuals lose the coverage and become Medicare-only.

On the issue of deterioration of health, we see two large caveats. The first is that the excess mortality in the Medicare-only population seems disproportionate. As noted in the paper, the level of excess mortality is as large as has been measured for the near-elderly uninsured. Is it plausible that coinsurance in Medicare can cause as much harm as lack of insurance in the near-elderly? Further, those outcomes were disproportionate to the observed rates of deterioration of self-reported health status and restrictions on ADLs. The data make it appear that those individuals were not getting sicker very quickly, just dying at a much faster rate.

The second is the rapid turnover of the Medicare-only population. This seems to be genuinely new information. Historically, the percentage of enrollees without secondary coverage has been stable, and that percentage varies in reasonable ways (e.g., by age cohort). There was no obvious reason to suspect that the cross-sectional stability was completely misleading. Conceptually, we have been discussing the Medicare-only segment as if that were another stable form of coverage. But in fact, the stable year-to-year counts are the product of large volumes of individuals moving into and out of Medicare-only status each year.

This raises the possibility that we may be misreading the excess mortality observed in the cross-sectional data. We clearly would like to know why these individuals switch coverage. We saw some hints from the MCBS that individuals losing coverage tend to have elevated mortality, but would need substantially larger sample sizes to be able to pin that down. We worry that, possibly, some individuals may pass through Medicare-only status as part of a pre-existing downward arc toward institutionalization and death. Perhaps cognitive and sensory deficits lead to an increased likelihood of failure to pay premiums? Perhaps an adverse health care event or increasing disability leads to loss of employment and income? We can guess at plausible scenarios, but at this point we do not have any data source that would allow this to be quantified adequately.

This is an important point because it affects the final interpretation of the results. We have two results that are separable. First, the near-first-dollar and other secondary coverage groups have essentially identical outcomes. This suggests relatively little risk in discouraging first-dollar coverage over secondary coverage that retains moderate beneficiary cost sharing. Modest cost sharing appears to hold the promise of cost reduction without worse outcomes.

Appendix A Evaluating a Two-Stage Statistical Approach

One issue related to the relationship between insurance coverage and health care spending is potential endogeneity. The possibility of endogeneity or biased selection complicates our ability to measure the effect of insurance coverage. That is, the extent to which Medicare beneficiaries choose secondary coverage based on their (expected) health care use, in ways not accounted for by demographics, income, education, and health status measures. For example, if beneficiaries who have an unobservable preference for more intensive medical intervention tend to purchase first-dollar coverage, one-stage regressions would overstate the impact of out-of-pocket costs on spending. If frequent enough and expensive enough, single-stage regression analysis would materially overstate the effect of insurance on cost.[18]

Two factors probably limit the influence of endogeneity in this situation. Mainly, the analysis already accounts for the major influences that drive spending, and secondarily, there are significant institutional constraints that limit beneficiaries' ability to choose their coverage in response to health.

First, we have already accounted for income, education, demographics, and detailed information on health status. In particular, the HCC risk adjuster accounts for the presence or absence of almost all conditions that significantly affect costs. This implies that endogeneity should only matter to the extent that individuals correctly anticipate they a) would want more intensive treatment once ill, or b) would want more diagnostic and preventive services unrelated to presence of a specific disease.[19]

Second, endogeneity is an issue only to the extent that beneficiaries correctly anticipate their health care use (conditional on presence of a disease) and can freely choose the depth of their coverage. But a sizeable fraction of Medicare fee-for-service elderly face constraints on choice or change of insurance. At the start of the study period, roughly 60 percent of all private employers offered a single health insurance plan.[20] Currently, 55 percent of large private employers are reported to offer only one plan.[21] This suggests that a significant fraction of those with employer-sponsored coverage had either no or limited choice of secondary insurance options at time of retirement. Similarly, Medicare beneficiaries had a single open-enrollment period (with unrestricted choice of plan) at time of retirement, and virtually all would have been screened for health conditions if

[18] Because the concern here is that we not overstate the effect of first-dollar coverage, we ignore the alternative hypothesis of risk aversion, that beneficiaries who avoid financial risks (and so purchase first-dollar coverage) may also tend to avoid also avoid health risks (and so have inexplicably lower medical expenses).

[19] Recall from Table 4 that *healthier* individuals purchased secondary coverage. So any bias from endogeneity has to arise because they want to spend more on health care for whatever illnesses they may or may not have.

[20] Strouse, R, Community Tracking Study, 2003 Employer Followback Pilot Study, Center for Studying Health Systems Change Technical Publication 52 (CSHCS, Washington, DC: October 2003).

[21] 2013 Survey of Employer-Provided Health and Other Benefits, Savitz, Inc., Philadelphia, PA, accessible at www.savitz.com/docs/2013_SurveyReport.pdf

attempting to switch later to more extensive coverage.[22] The resulting financial penalties are expressly designed to limit adverse selection from beneficiaries responding to worsening health by purchasing more extensive Medigap coverage.[23] The situation here is materially different from the canonical case where individuals freely choose annually among multiple health insurance options.

In this analysis, we used the novel approach of regression analysis at a detailed level (e.g., by type of claim, physician specialty, type of admission, type of service, depth of insurance coverage). This helped rule out some alternative explanations of the observed differences in spending. For example, prior to this analysis, one could plausible suggest that an unobserved difference in health status was the root cause of higher spending for those with secondary coverage. Now, by contrast, one would have to assert that there was some prevalent, significant, and unobserved difference in health status *that only affects discretionary care.* That seems to lack face validity. And so this approach shifts the plausible explanations of higher spending from "perhaps they are sicker" to "perhaps they would want to spend more on health care in any case." This does not settle the issue, but it serves to reduce the number of plausible alternative explanations of the results.

Similarly, the finding of a dose-response relationship between depth of coverage and spending further narrows the debate. Prior to this analysis, one could plausibly suggest that individuals decided to obtain secondary coverage or not based on accurate knowledge that they would want to spend more on health care. Now, by contrast, a correct statement of that alternative explanation is more convoluted. Something like "Individuals who knew they wanted to spend more on health did not merely purchase secondary coverage, they purchase first-dollar coverage, while others either purchased less-than-first-dollar or no coverage, which then made no difference in terms of total health care spending." Again, this does not settle the issue, but it serves to make the alternative explanation of the facts less plausible.

The standard statistical approach when faced with potential endogeneity is to use a two-stage estimation process such as instrumental variables. The first stage of that approach is to predict each individual's choice of insurance coverage based on factors that are thought to be uncorrelated with unexplained variation cost. (Those factors are termed the "instruments" for insurance coverage.) In the second stage, the regression analysis showing the effect of insurance on cost uses the predicted (instead of actual) insurance choice. If all maintained hypotheses are true and sample size is large enough, that should give a (statistically) unbiased estimate of the effect of secondary insurance on cost.

The instrumental variables (IV) method has several practical drawbacks. It is statistically inefficient in the sense that the resulting estimator has higher variance than the estimate

[22] US GAO, MEDIGAP INSURANCE: Plans Are Widely Available but Have Limited Benefits and May Have High Costs, report GAO-01-941 (GAO, Washington, DC: July 2001), page 12.
[23] GAO, op. cit.

using actual plan choice.[24] In practical terms, we have to observe a larger effect on cost in order for that effect to be judged statistically significant. Second, the IV estimate retains some bias in a finite sample, and can remain strongly biased even in fairly large samples.[25] In both cases, the more poorly we predict choice of insurance, the worse these problems become. Finally, IV estimators can suffer from various types of specification bias. Statistical theory requires that we have at least one instrument (a variable used to predict plan choice that is not used to predict cost). Beyond that, the number and choice of instruments is at the discretion of the analyst, and seemingly arbitrary choices can materially affect the resulting estimates.

In this particular case, we believe that the drawbacks of IV estimation substantially outweigh the benefits. This occurs because we are working with a small sample (the MCBS) with an endogenous variable that is hard both hard to predict, and for which many plausible predictors (e.g., income, race) have already been used to predict costs. Between the small sample and the difficult prediction task, the IV estimator developed below performs poorly, and we judge that it does not provide any useful additional information. With this concern in mind, we rejected using a two-stage statistical approach.

Methods and results

Starting from the data used in the main analysis, we limited the file to those with Medigap or no secondary coverage. Thus, the analysis is an analysis of the decision to purchase Medigap or not.

We added the following variables to be used in constructing the instrument for choice of insurance. Each of these was correlated with actual Medigap purchase at $p < .05$, so individually they satisfy the requirement that they be correlated with actual insurance purchase.

- State average Medigap insurance premiums.[26]
- A variable derived from MCBS, showing the beneficiary's ownership of any type of specialty insurance coverage, for example, dental insurance or long-term-care insurance.
- The average rate of Medigap purchase in the beneficiary's state of residence.
- A flag for rural location.

The rationale for each is straightforward. Economic theory suggests that higher premiums should deter Medigap purchase. The specialty insurance purchase identifies

[24] This is explained clearly on page 12 of Instrumental Variables, Nathaniel Beck, Department of Politics, NYU, New York, NY 10012, accessible at www.nyu.edu/classes/nbeck/q2/iv2slss.pdf; or in greater depthat
[25] For discussion in a social science context see Bloom, H, Zhu,P "Finite Sample Bias from Instrumental Variables, Analysis in Randomized Trials", MDRC August 2010, accessible at: http://www.mdrc.org/publication/finite-sample-bias-instrumental-variables-analysis-randomized-trials.
[26] Variation and Trends in Medigap Premiums, ASPE REPORT, December 2011, By: Steven Sheingold, Adele Shartzer, and Dan Ly, accessible at http://aspe.hhs.gov/health/reports/2011/medigappremiums/index.shtml#_ftnref7

beneficiaries with a demonstrated willingness to purchase coverage. And the state-average takeup rate (presumably) captures regional variation in such items as availability of plans, or possibly just captures small-area variation in insurance purchase propensity. The rural flag captures the known lower propensity to obtain insurance in rural as opposed to urban areas.

We added these variables to a linear probability model (an ordinary least squares regression) predicting purchase of Medigap insurance. The model also included the remaining variables in the cost regression.

The results are summarized in Table A-1. The concordance figure was calculated as would be done for a logistic model – a predicted probability of purchase over 50 percent was counted as a predicted purchase, and concordance is the fraction of predicted and actual Medigap purchase pairs that agreed.

Table A-1: Predicting Medigap Purchase, Linear Probability Model			
Coefficients for instrumental variables			
	Coefficent	T-statistic	P value
State Medigap premium	-5.8E-05	-3.48	0.0005
State average Medigap takeup	0.77517	17.03	<.0001
Owns specialty insurance	0.03923	2.87	0.0041
Rural location	0.00199	0.29	0.7738
Memo: Adj. R-squared	0.174		
Memo: Concordance	0.829		
Note: Regression also includes all variables from cost regressions.			

By all signs, this would seem to be a reasonable instrument for Medigap purchase. Explanatory power is adequate, three of the four selected variables used to identify the instrument are statistically significant predictors of choice, the signs of the coefficients appear plausible, and the model in fact predicts the correct choice nearly 83 percent of the time.[27]

We next asked what would happen if we dropped the four identifying variables from the regression. This is an important question to ask, because when the instrument is used in the cost equation, the resulting coefficient will reflect only the independent variation of the instrument – the variation not captured by a linear combination of the other variables in the cost regression.

The results of that are shown in Table A-2. In fact, (a linear combination of) the factors already in the cost equation capture virtually all of the predictable component of choice. When converted to a 0/1 predicted Medigap purchase variable, the two sets of predicted values disagree on just 1.5 percent of cases (their concordance is 98.5 percent).

[27] Simply predicting that everyone would purchase Medigap would yield a concordance of 80 percent.

Table A-2: Predicting Medigap Purchase, Linear Probability Model	
Using only the variables from the cost equation.	
Memo: Adjusted R-squared	0.157
Memo: Concordance	0.828
Memo: Concordance between full instrument and instrument without identifying variables.	0.985

In effect, the predicted insurance choice appears nearly multicollinear with the remaining variables in the cost equation. In this situation (near-multicollinearity), we should expect the resulting coefficient in the cost regression to a) take on large values, b) have large standard errors, and c) be sensitive to changes in specification. And that is exactly what we observe.

Table A-3 shows three variations on the cost regression. Using actual Medigap ownership, the regression suggests that Medigap raises spending by about $1200 per person, after accounting for all other factors in the regression. The coefficient is estimated with good precision (small standard deviation) and so is statistically significant at $p < 0.0001$. Using the statistical instrument for choice, by contrast, the regression coefficient suggests Medigap ownership *reduces* Medicare costs by more than $2000. The standard error is now 10 times larger than before, so this is not statistically significant. Finally, the instrument estimated without the market-average takeup rate suggest that Medigap *adds* more than $7,000 to cost, but with such large standard errors that the result is not statistically significant.

The standard errors on both IV estimators are so large that we would have to observe a very large difference in spending in order for that difference to be judged statistically significant. This IV estimator is unable to detect spending differences that would be considered important from a policy perspective.

Table A-3: Predicting Total (Part A plus Part B) Cost Using Actual Insurance Choice and Statistical Instrument for Choice				
Model	Estimated Impact of Insurance	Standard error	t Value	p value
Actual insurance purchase	$ 1,230	$ 221	5.56	<.0001
Statistical instrument as described	$ (2,467)	$ 2,074	-1.19	0.2348
Statistical instrument, exclude state average Medigap takeup	$ 7,427	$ 7,530	0.99	0.3243

In some sense, all we have proven is that *we* could not come up with a good statistical instrument for Medigap choice, and that *our* instrument is not robust. The variables were plausible – Medigap price, beneficiary propensity to purchase insurance, and small-area variation in Medigap takeup rates. The choice regression itself looked reasonable in isolation. But in the end, our instrument failed because it was nearly multicollinear with the pre-existing variables in the cost equation. It had too little independent variation, and the resulting coefficients had the expected large standard errors and extreme sensitivity to changes in specification.

In that narrow sense, this appendix explains why we did not use a two-stage statistical technique. That approach is superior in theory, but not in practice, in this case.

Based on our experience, we speculate that one puzzling aspect of the published literature on insurance coverage and cost may be an artifact of methodology. Single-stage estimates (such as those in the body of this report) almost uniformly show that insurance coverage increases health care spending. By contrast, two-stage estimates vary widely. One explanation for the lack of convergence of the two-stage studies is that the true effect of health insurance coverage actually does vary substantially from year to year or situation to situation. An alternative explanation is that there is an unrecognized lack of reliability of the instrumental variables method in this situation, and the reported variation in estimated impact of coverage is an artifact of methodology.

In summary, there is no one methodology that is theoretically superior in this case. The one-stage risk-adjusted regressions used here may or may not suffer from significant endogeneity bias. A two-stage instrumental variables approach may or may not suffer from the problems of near multicollinearity and the resulting high variance and lack of robustness. It comes down to a practical choice. Our best attempt at an IV estimator was incapable of detecting even very large impacts of insurance on cost. On net, we believe that the IV approach adds no useful information in this instance.

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